CRU CORRESPONDENCE
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458. 1104855751.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: t.osborn@uea.ac.uk,k.briffa@uea.ac.uk
Subject: Fwd: Re: Fw: Rutherford et al. [2004]
Date: Tue, 04 Jan 2005 11:22:31 +0000

FYI.
Just look at the attachment. Don't refer to it or send it on to anybody yet. I guess you could refer to it in the IPCC Chapter - you will have to some day !
Cheers
Phil

X-Sender: mem6u@multiproxy.evsc.virginia.edu
X-Mailer: QUALCOMM Windows Eudora Version 6.1.1.1
Date: Thu, 30 Dec 2004 09:22:02 -0500
To: Phil Jones <p.jones@uea.ac.uk>
From: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fw: Rutherford et al. [2004]
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
X-UEA-MailScanner-SpamScore: s
Phil,
I would immediately delete anything you receive from this fraud.
You've probably seen now the paper by Wahl and Ammann which independently exposes
McIntyre and McKitrick for what it is--pure crap. Of course, we've already done this on
"RealClimate", but Wahl and Ammann is peer-reviewed and independent of us. I've attached
it in case you haven't seen (please don't pass it along to others yet). It should be in
press shortly. Meanwhile, I would NOT RESPOND to this guy. As you know, only bad things
can come of that. The last thing this guy cares about is honest debate--he is funded by
the same people as Singer, Michaels, etc...
Other than this distraction, I hope you're enjoying the holidays too...
talk to you soon,
mike
At 09:02 AM 12/30/2004, you wrote:

Mike,
FYI. Just in for an hour or so today as still off until Jan 4.
Not replied to this - too much else with IPCC etc. Not read this in detail - just printed it off.
Have a good New Year's Eve.
Cheers
Phil

From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Fw: Rutherford et al. [2004]
Date: Wed, 29 Dec 2004 10:08:18 -0500
X-Mailer: Microsoft Outlook Express 6.00.2800.1158
X-UEA-MailScanner-Information: Please contact the ISP for more information

X-UEA-MailScanner: Found to be clean
Dear Phil,

I have noticed the following statements in Rutherford et al [2004], in which you are a co-author. As compared with some of your co-authors, I get the impression that, while you feel very strongly about your views, you are also concerned with getting to the bottom of matters and are less concerned with scoring meaningless debating points. In this spirit, I draw your attention to some incorrect statements in Rutherford et al. [2004] concerning our material. There is really a quite serious problem with the PC methods in MBH98 and the comments made in Rutherford et al [2004] are really quite misleading. For the reasons set out below, I request that these comments be removed from the manuscript.

Regards, Steve McIntyre

----- Original Message -----
From: [1]Steve McIntyre
To: [2]David Randall
Cc: [3]Scott Rutherford ; [4]Paul Kushner ; [5]Cindy Carrick ; [6]Ross McKitrick
Sent: Tuesday, December 28, 2004 1:48 PM
Subject: Rutherford et al. [2004]
Dear Dr. Randall,

Recently, at the website [7]www.realclimate.org, Michael Mann publicized a submission by Rutherford et al. to Journal of Climate, entitled Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season, and Target Domain. This paper contains some untrue statements and mischaracterizations regarding criticisms we (McIntyre and McKitrick) made of Mann et al. (1998) [MBH98] in a 2003 paper and subsequent exchanges under the auspices of Nature. We are writing to request that these untrue statements be removed from the paper before any further processing of the document by Journal of Climate takes place.

First, Rutherford et al. states that McIntyre and McKitrick [2003] used an incorrect version of the Mann et al. (1998) proxy indicator dataset. The history of this matter is summarized below (all relevant emails and other documentation are available at [8]http://www.climate2003.com/file.issues.htm .

In April 2003, we requested from Mann the FTP location of the dataset used in MBH98. Mann advised me that he was unable to recall the location of this dataset and referred the request to Rutherford. Rutherford eventually directed us to a file (pcproxy.txt)

located at a URL at Manns FTP site. In using this data file, we noticed numerous problems with it, not least with the principal component series. We sought specific confirmation from Mann that this dataset was the one used in MBH98; Mann said that he was too busy to respond to this or any other inquiry. Because of the many problems in this data set, we undertook a complete new re-collation of the data, using the list of data sources in the SI to MBH98 and using original archived versions wherever possible.
After publication of McIntyre and McKitrick [2003], Mann said that dataset at his FTP site to which we had been referred was an incorrect version of the data and that this version had been prepared especially for me; through a blog, he provided a new URL which he now claimed to contain the correct data set. The file creation date of the incorrect version was in 2002, long prior to my first request for data, clearly disproving his assertion that it was prepared in response to my request. Mann and/or Rutherford then deleted this incorrect version with its date evidence from his FTP site.

It is false and misleading for Rutherford et al. to now allege that we used the wrong dataset. We used the dataset they directed us to at their FTP site. More importantly, for our analysis, to avoid the problems with the principal component series, we re-collated the tree ring data identified in MBH98 from ITRDB archives, calculated fresh principal component series; in addition, we re-collated other proxy data from archived versions wherever possible. Thus, our own calculations were not affected by the errors in the supplied file as we did NOT use the incorrect version in our calculations. To suggest otherwise, as is done in Rutherford et al [2004], is highly misleading. To date, no source code or other evidence has been provided to fully demonstrate that the incorrect version (now deleted) did not infect some of Manns and Rutherfords other work.
In this respect, we note that the now deleted file pcproxy.txt occurs in a legend in a graphic at Rutherfords website, indicating possible use elsewhere by Rutherford of the incorrect version.

Accordingly, we request that the above claim be removed from the manuscript.

Secondly, Rutherford et al. [2004] argues that the difference between MBH98 results and MM03 results occurs because of our misunderstanding of a stepwise procedure in MBH98 for the calculation of principal component series for tree ring networks. Again, this claim is misleading on its face. While our 2003 paper did not implement the (then undisclosed) stepwise procedure, as soon as this matter was raised in subsequent correspondence in

November 2003, we implemented it and we continued to observe the discrepancies in principal component series and final results. The current manuscript ignores a refereed exchange at Nature in which we specifically clarified (in response to a reviewers question) that we had obtained such results while using the exact stepwise procedure described in MBH98. Mann is aware of this refereed exchange.

The reason for the difference between our results and MBH98 results is primarily due to the fact that the tree ring principal component series in MBH98 cannot be replicated using a conventional principal components method. The MBH98 principal component series can only be replicated by standardizing on a short segment a procedure nowhere mentioned in MBH98 and only recently acknowledged in the SI to the Corrigendum of Mann et al. [Nature 2004] in response to our concerns on the subject expressed to Nature. In effect, MBH98 did not use a conventional centered PC calculation, but used an uncentered PC calculation on de-centered data. The impact of this method is the subject of ongoing controversy, which is well-known to the authors, but the existence of the method in MBH98 is no longer in doubt. In discussions of PC calculations in 2004 exchanged with the authors through Nature, we implemented the stepwise procedures of MBH98 referred to in the present manuscript and demonstrated that important differences remain even with stepwise procedures, as long as the uncentered and decentered methods of MBH98 are used. The differences in PC series resulting from using centered and uncentered series has been fully agreed to by all parties in the Nature exchange, although the parties continue to disagree on the ultimate effect on final NH temperature calculations.

Accordingly, the discussion in Rutherford et al. [2004] is very incomplete and misleading in this respect. While we recognize that Mann et al. have argued that they can salvage MBH98-type results using alternative methodologies (e.g. increasing the number of PC series used in the 1400-1450 period), these salvage efforts are themselves a matter of controversy and do not validate the claims being put forward in the Rutherford et al. paper.

Accordingly we ask that this claim also be deleted from the manuscript.

Regards,
Stephen McIntyre and Ross McKitrick

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[9]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\Wahl_MBH_Recreation_JClimLett_Nov22.pdf"

References

1. mailto:stephen.mcintyre@utoronto.ca
2. mailto:randall@atmos.colostate.edu
3. mailto:srutherford@rwu.edu
4. mailto:j.climate@atmosp.physics.utoronto.ca
5. mailto:cindy@atmos.colostate.edu
6. mailto:rmckitri@uoguelph.ca
7. http://www.realclimate.org/
8. http://www.climate2003.com/file.issues.htm
9. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Jonathan Overpeck <jto@u.arizona.edu>
To: k.briffa@uea.ac.uk
Subject: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Tue, 4 Jan 2005 21:52:47 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov

Hi Keith - Happy new year. Hopefully, you had a good holiday. I've had a chance to read
your section and hopefully you've had a chance to read what I sent just before the
holidays. The purpose of this email is to help get a focus on the finish line (just a few
days away) and to get a dialog going that will hopefully help you finish section 6.3.2.1.
If you'd like to talk on the phone, just let me know.

Please see my email from right before xmas holidays for original comments. Plus, here are
the new ones from both me and David Rind:

0) as leader of this KEY section, we need you to take the lead integrating everything you think should be integrated, editing and boiling it down to just ca 4 pages of final text (e.g., 8 pages of typed text plus figs). This means cutting some material (e.g., forcings and simulations) and perhaps moving glacier record (MUCH boiled down) to a box. See below.

00) note that we can also perhaps move some of the details to the appendix (although we won't write this until after the current ZOD crunch, save an outline of what you might want in there).

1) I like your figure ideas, with the comments:

1a) I don't think you need figure 1d - the SH recons are sketchy since not much data, and it might be better to just discuss in a sentence or three. Any space saved is good too. Not sure about your proposed 1e - have to see it, I guess.

1b) Figure 2 looks interesting. I'm trying to get the latest Arctic recon from Konrad Hughen - it is quite robust and a significant multi-proxy update. Should be published in time, though not sure thing since he's still hot on including his (our) AO recon which is more sketchy

1c) I think we can save space and improve organization if we DO NOT include Fig 3. However, this is open for debate - see David's comments below.

2) I agree with David's comments in general - so see them below. The prickly issue is where to put the forcings and simulated changes. I am close to having the prose from the radiation chapter, including the latest Lean and Co's view on solar - this will make many of the existing simulations involving inferred past solar forcing suspect (I will send in a day or so I hope). This means that we might be best saving space and downplaying this work some. I'm not sure, but wanted to debate it with you. Also, Chap 9 will have simulations in spades, so we can save space by letting them do it. Also, as David points out, we can focus on it elsewhere in our chapter more concisely - leaving you to focus on the VERY important obs record of temp and other changes. Can you tell, I'm still not 100% sure? I'll send another email to you and others about this in a bit.

3) Your section is too long and needs to be condensed. Thus, you need to think through what's most important and what's less so. For example, we need to figure out how to condense the glacier record of change. David thinks it should be a separate section that cuts across time scales (i.e., Holocene and last 2000 years). Perhaps we should try to make it into a box - 3 to 5 short paragraphs and a figure or two. Either way we have to really wack it. What do you think - you and I should be on the same page with Eystein before discussing w/ Olga perhaps. Or you can discuss with her - you're the lead on this section.

4) you're doing an impressive job! Lots to keep track of.

Next, here is what David has offered. Take it all with a grain of salt, but I have read it
and he has many good points. On the structural or any other points, I'm happy to discuss on
the phone, or you can just debate with him and me on email.
******* From David Rind 1/4/05 ****************
6.3 Understanding Past Climate System Change (forcing and response)
6.3.1 Introduction (0.5 pages)
6.3.2 The Current Interglacial
6.3.2.1 Last 2000 years (4 pages)
Figure 1 should be of the last 2000 years, with appropriate caveats, not just since 1860
(which will undoubtedly be in other chapters).

pp. 8-18: The biggest problem with what appears here is in the handling of the greater
variability found in some reconstructions, and the whole discussion of the 'hockey stick'.
The tone is defensive, and worse, it both minimizes and avoids the problems. We should
clearly say (e.g., page 12 middle paragraph) that there are substantial uncertainties that
remain concerning the degree of variability - warming prior to 12K BP, and cooling during
the LIA, due primarily to the use of paleo-indicators of uncertain applicability, and the
lack of global (especially tropical) data. Attempting to avoid such statements will just
cause more problems.
In addition, some of the comments are probably wrong - the warm-season bias (p.12) should
if anything produce less variability, since warm seasons (at least in GCMs) feature smaller
climate changes than cold seasons. The discussion of uncertainties in tree ring
reconstructions should be direct, not referred to other references - it's important for
this document. How the long-term growth is factored in/out should be mentioned as a prime
problem. The lack of tropical data - a few corals prior to 1700 - has got to be discussed.
The primary criticism of McIntyre and McKitrick, which has gotten a lot of play on the
Internet, is that Mann et al. transformed each tree ring prior to calculating PCs by
subtracting the 1902-1980 mean, rather than using the length of the full time series (e.g.,
1400-1980), as is generally done. M&M claim that when they used that procedure with a red
noise spectrum, it always resulted in a 'hockey stick'. Is this true? If so, it constitutes
a devastating criticism of the approach; if not, it should be refuted. While IPCC cannot be
expected to respond to every criticism a priori, this one has gotten such publicity it
would be foolhardy to avoid it.
In addition, there are other valid criticisms to the PC approach. Assuming that the PC
structure stays the same was acknowledged in the Mann et al paper as somewhat risky, given
the possibility of altered climate forcing (e.g., solar). Attempting to reconstruct
tropical temperatures using high latitude PCs assumes that the PCs are influenced only by

global scale processes. In a paper we now have in review in JGR, and in other papers
already published, it is shown that high latitude climate changes can directly affect the
local expression of the modes of variability (NAO in particular). So attempting to fill in
data at other locations from PCs that could have local influences may not work well; at the
least, it has large uncertainties associated with it.
The section from p.18-20 - simulations of temperature change over the last millennium ,
including regional expressions - should not be in this section. It is covered in the
modeling section (several different times), and will undoubtedly be in other chapters as
well. And the first paragraph on p. 19 is not right - only by using different forcings have
models been able to get similar responses (which does not constitute good agreement). The
discussion in the first paragraph of p. 20 is not right - the dynamic response is almost
entirely in winter, which would not have affected the 'warm season bias'
paleoreconstructions used to prove it. It also conflicts with ocean data (Gerard Bond,
personal communication). Anyway, it's part of the section that should be dropped.
pp. 20-28: The glacial variations should be summarized in a coherentglobal picture.
Variations as a function of time should be noted - not just lumped together between 1400
and 1850 - for example, it should be noted where glaciers advanced during the 17th century
and retreated during the 19th century, for that is important in understanding possible
causes for the Little Ice Age (as well as the validity of the 'hockey stick'). The
discussion on the bottom of p.25-27 as to the causes of the variations is inappropriate
and should be dropped - note if solar forcing is suspect, every paragraph that relates
observed changes to solar forcing will be equally suspect (e.g., see also p. 44, first
paragraph).
Bottom of p. 27: Greene et al. (GRL, 26, 1909-1912, 1999) did an analysis of 52 glaciated
areas from 30-60N and found that the highest correlation between their ELA variations in
the last 40 years was with summer season freezing height and winter season precip. The warm
season freezing height was by far more important. Therefore, the relationship of glacier
variations to NAO changes (which are important only in winter), as discussed in this
paragraph, while perhaps valid for a period of time in southern Norway, is not generally
applicable.

p. 34-36 on forcings: note that this is redundant to what is discussed in several later
sections (e.g., 6.5.2); and other chapters), and that is true of forcing in general for the
whole of section 6.2. I would strongly suggest dropping forcing from section 6.3.2.1, at
least, and perhaps giving it its own number, or referring to othersubsections for

it. It
has a different flavor from the responses, and the section is already very big. Forcing
does need to be discussed in the paleoclimate chapter, for reasons of climate sensitivity
and explaining observations, but that is what Chapter 6.5 is about.
(In summary - 6.3.2.1 already is taking on one controversy - paleotemperatures, which is
needs to do better, It should not have to deal with the forcing problems as well, and
especially not in an off-handed way.)
Specific comments: p. 36: 6 ppm corresponds to a temperature response of 0.3 to 0.6°K using
the IPCC sensitivity range.
p. 36, last paragraph: one could equally well conclude that the reconstructions are showing
temperature changes that are too small. This is the essence of the problem with the last
2000 years: if the reconstructions are right, either there was no solar forcing, or climate
sensitivity is very low. If the real world had more variability, either there was solar
forcing, or climate sensitivity is high (as is internal variability). I've tried to say
this in the climate sensitivity sub-chapter.
pp. 37-41: obviously a lot of overlap, but it shouldn't be hard to combine these.
p. 39, first paragraph: but can the models fully explain what is thought to have happened?
Quantification is important here, because many of the same climate/veg models are being
used to assess future changes in vegetation.
p. 42 - first full paragraph: what are the implications of the methane drop without a CO2
drop?
p. 43, middle paragraph: obviously should mention solar-orbital forcing in this paragraph.
p. 44, first paragraph: again, assuming a solar forcing
p. 45, first paragraph: overlap with pp. 20-28.
Second paragraph: overlap with p.39, last full paragraph
p. 52 - repeat of p. 43.
******* END From David Rind 1/4/05 ****************

Thanks! Cheers, peck

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Jonathan T. Overpeck
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: Re: the Arctic paper and IPCC
Date: Wed, 5 Jan 2005 11:15:53 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Hi Keith - great (!) to hear from you - hope you had a good holiday.
Your reward (ha) is the attached paper and comment below from Konrad.
He can supply data if needed for a synthetic figure, but we can add
this later once the Science paper he mentions (w/ us a co-authors
among millions, I assume) gets vetted more. Your call.

I'm still not convinced about the AO recon, and am worried about the
late 20th century "coolness" in the proxy recon that's not in the
instrumental, but it's a nice piece of work in any case.

Now, for all the issues you raise on other stuff in your email, I'll
address to you and that crowd.

thanks, Peck

>X-Sieve: CMU Sieve 2.2
>Date: Wed, 05 Jan 2005 10:53:56 -0500
>From: Konrad Hughen <khughen@whoi.edu>
>Organization: WHOI
>X-Accept-Language: en-us, en
>To: Jonathan Overpeck <jto@u.arizona.edu>
>Subject: Re: the Arctic paper and IPCC
>X-Virus-Status: No
>X-Virus-Scanned: by amavisd-new at email.arizona.edu
>
>Hey Peck,
>
>Here's a pdf of a draft of Peter's methods paper. The figures will
>be what goes into the Science paper. I've sent the whole thing to
>help explain the figs, but let me know if you guys have questions.
>Also, I have a movie of reconstructed Arctic temp through time. Too
>big to attach but I'll try and get it to you somehow. Pretty cool.
>We're planning to include the movie and supplemental figs
>("robustness" tests, etc.) into the new website Matt's working on.
>
>Good to talk yesterday. I'll get a CV to you today.
>
>-Konrad
>

--
Jonathan T. Overpeck
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Wed, 5 Jan 2005 12:24:47 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov, Fortunat Joos <joos@climate.unibe.ch>, joos <joos@climate.unibe.ch>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>
Hi Keith and Co - I think David likes a good debates, so the main thing is to consider his comments and respond appropriately. Although the first priority has to be on the ZOD text and display items, maybe you can go back over his comments AFTER the looming deadline and further discuss things with David and others. For now, just work away.

The biggest issue is how to handle forcing and simulations - i.e., where to put different pieces in the chapter. Eystein and I will help the team work through this. More soon, but for now just proceed as you have been proceeding. There is real merit to the concept that your section is about how climate varied over the last 2ka, and what caused these variations. The flip side is that we need to get a clear vision of how this differs from what goes into the other sections. Eystein and I will work more on this asap.

Your plan re: glaciers is good. That's a tough one, but it has to be boiled WAY down. Moreover, my gut is to focus on the extent to which these complicated natural archives (e.g., complicated by ppt change) support or do not support the other proxy evidence/conclusions. This is why I was thinking we might think about a box, and to include the Lonnie perspective in it - e.g., glaciers are now melting everywhere (almost - we know why they are not in those places) in a manner unprecedented in the last xxxx years. Make sense? See what Olga says, and if needbe, I can help focus that stuff more.

Thanks! Peck

>Hi Peck (et al)
>I am considering comments (including David's) re last 2000 years -
>some are valid = some are not . Will try to chop out bits but we
>need this consensus re the forcing and responses bit - I am for
>keeping the forcings in as much as they relate to the specific model
>runs done - and results for last 1000 years as I suspect that they
>will not be covered in the same way elsewhere . David makes couple
>good points - but extent to which forcings different (or
>implementation) perhaps need addressing here. The basic agreement I
>mean is that the recent warming is generally unprecedented in these
>simulations.
>It will take time and input from the tropical ice core /coral people
>to do the regional stuff well . I think the glaciological stuff is a
>real problem - other than just showing recent glacial states (also

>covered elsewhere) - of course difficult to interpret any past
>records without modelling responses (as in borehole data), but this
>requires considerable space . My executive decision would be to ask
>Olga to try to write a couple of papragraphs on limits of
>interpretation for inferring precisely timed global temperature
>changes? What do others think? I only heaved Olga's stuff in at
>last moment rather than not include it - but of course it needs
>considerable shortening. The discussion of tree-ring stuff is
>problematic because it requires papers to be published eg direct
>criticism of Esper et al. We surely do not want to waste space HERE
>going into this esoteric topic? All points on seasonality , I agree
>with , but the explicit stuff on M+M re hockey stick - where is
>this? ie the bit about normalisation base affecting redness in
>reconstructions - sounds nonsense to me ?
>
>I have to consider the comments in detail but am happy for hard
>direction re space and focus. If concensus is no forcings and model
>results here fine with me - Peck and Eystein to rule
>Keith
>

--
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Professor, Department of Atmospheric Sciences

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http://www.ispe.arizona.edu/
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Parker, David (Met Office)" <david.parker@metoffice.gov.uk>, Neil Plummer
<n.plummer@bom.gov.au>
Subject: RE: Fwd: Monthly CLIMATbulletins
Date: Thu Jan 6 08:54:58 2005
Cc: "Thomas C Peterson" <Thomas.C.Peterson@noaa.gov>

    Neil,
      Just to reiterate David's points, I'm hoping that IPCC will stick with
1961-90.
    The issue of confusing users/media with new anomalies from a
    different base period is the key one in my mind. Arguments about
    the 1990s being better observed than the 1960s don't hold too much
    water with me.
    There is some discussion of going to 1981-2000 to help the modelling
    chapters. If we do this it will be a bit of a bodge as it will be hard to do
    things properly for the surface temp and precip as we'd lose loads of
    stations with long records that would then have incomplete normals.

If we do we will likely achieve it by rezeroing series and maps in an ad hoc way.
  There won't be any move by IPCC to go for 1971-2000, as it won't help with satellite series or the models. 1981-2000 helps with MSU series and the much better Reanalyses and also globally-complete SST.
  20 years (1981-2000) isn't 30 years, but the rationale for 30 years isn't that compelling. The original argument was for 35 years around 1900 because Bruckner found 35 cycles in some west Russian lakes (hence periods like 1881-1915). This went to 30 as it easier to compute.
   Personally I don't want to change the base period till after I retire !
Cheers
Phil
At 09:22 05/01/2005, Parker, David (Met Office) wrote:

> Neil
> There is a preference in the atmospheric observations chapter of IPCC AR4 to stay with the 1961-1990 normals. This is partly because a change of normals confuses users, e.g. anomalies will seem less positive than before if we change to newer normals, so the impression of global warming will be muted. Also we may wish to wait till there are 30 years of satellite data, i.e until we can compute 1981-2010 normals, which will then be globally complete for some parameters like sea surface temperature.
> Regards
> David
> On Tue, 2005-01-04 at 21:58, Neil Plummer wrote:
> > Hi Hama, Tom
> > (and David, Blair)
> > Re: the issue of using the 1971-2000 normals in CLIMAT rather than
> > 1961-1990 normals.
> >
> > Happy New Year!
> > I have copied the relevant text from CCl XIII below, which provides
> > reasons for staying with the 1961-90 standard.
> > My initial recommendation is the same as Tom's, i.e. stay with the
> > standard for now.
> >
> > I think there are two main factors to consider here - capability and
> > demand. While there are clearly advantages with widespread use of
> > normals derived using the later period there must be the capacity to
> > do so.
> >
> > Perhaps in the lead-up to CCl-XIV, OPAG 2 can find out the extent of
> > the support for the change among users of CLIMAT and OPAG 1 can find
> > out more about capabilities. (Note, however, that this is not strictly
> > on issue for OPAG 1 according to the ToRs for the ICT and any of the
> > ETs. Happy to assist though).
> >
> > We may use the climate working groups in the Regional Associations to
> > assist with surveying members capabilities and could do the same
> > regarding the demand question though I think Tom's CCl/CLIVAR ET is
> > best placed to give that guidance.
> >
> > *** David, Blair - Interested in your thoughts on this matter.
> >
> > Cheers
> > Neil
> > ---
> > From CCl XIII ...
> >
> > 6.1.2 The Commission noted with satisfaction that

> the 19611990 Standard Normals were now complete
>
> and expressed its appreciation to NCDC for assembling
>
> the data as well as to those Members who had contributed
>
> data. It further noted that the 19611990
>
> Standard Normals would remain in use for global purposes
>
> until the next Standard Normals for the period
>
> 19912020 were completed.
>
> 6.1.3 The Commission noted that, in addition to the
>
> 1961 to 1990 WMO Standard Normals, many countries
>
> had produced climatic normals using the 1971 to 2000
>
> period. The Commission also noted the discussion held
>
> among Members on whether the standard 30-year normals
>
> should be accompanied by normals calculated over
>
> a more current period or a shorter period to reflect
>
> recent climate variability. The Commission noted the
>
> usefulness of periods other than the contiguous 30-year
>
> period for certain analyses below the global scale.
>
> However it decided to maintain the Climatological
>
> Standard Normals process, as it provided a common reference
>
> period for climate research and monitoring
>
> worldwide.
>
>
>
> Neil Plummer
>
> Senior Climatologist
>
> National Climate Centre
>
> Bureau of Meteorology
>
> 700 Collins Street, Melbourne, VIC 3001, Australia
>
> Tel +61 3 9669 4714; Fax: +61 3 9669 4725; Mobile 0419 117865
>
> Email n.plummer@bom.gov.au
>
>
>
>

> _____
> From: Thomas C Peterson [[1]mailto:Thomas.C.Peterson@noaa.gov]
> Sent: Tuesday, 4 January 2005 1:11 AM
> To: H Kontongomde
> Cc: Hans Teunissen; Neil Plummer
> Subject: Re: Fwd: Monthly CLIMATbulletins
>
>
> Thanks for responding, Hama. I agree with you on both
> points. I wonder how many countries produced 71-2000
> Normals? I'll cc Neil Plummer on this as the ET on Observing
> Requirements and Standards for Climate is under his
> leadership.
> Regards,
> Tom
>
> H Kontongomde wrote:
> > Dear Tom and Hans,
> >
> > Happy New Year! I apologize for responding so late. I was on annual
> > leave since 13 December. The question of which "Normal" between
> > 1961-1990 and 1971-2000 is now frequently asked by many WMO Members.
> > Depending on the practical use of the normal, one of the two Normal can
> > be preffered to the other. However, the policy for CLIMAT messages is
> > to use the 1961-1990 Normals and until CCl change the standard, I would
> > also recommend that our colleagues of Turkey continue to use these 61-90
> > normals. This allows spatial comparisons for the entire globe, because,
> > not all countries have their 1971-2000 averages ready for use.
> >
> > However, I think it is time that the CCl Expert Team on Observing
> > Requirements and Standards for Climate clarifies the problem in
> > explaining why the 61-90 Normals should continue to be the standard or
> > why it is time to change.
> >
> > I will respond to our colleagues of Turkey.
> >
> > Best regards,
> >
> > Hama Kontongomde
> >
> >
> >
> > > > > Hans Teunissen 1/3/2005 12:16:00 PM >>>
> > > > >
> > Thanks for those suggestions, Tom. I'm not sure if your two questions
> > below were meant to be different (is a word 'change' missing from the
> > first?), but I think I get the gist from the answers. Re the CLIMAT code
> > official standards, I don't think Dick (or GCOS) is really the right
> > person to go to. That would be Hama, or, it seems, OSY (Sasha Karpov)
> > since they arranged the publication of TD-1188. Is that right, Hama? And

> > are you OK to use Tom's suggestion in the reply to Turkey?
> >
> > Hans.
> >
> >
> > > > > "Thomas C Peterson" <Thomas.C.Peterson@noaa.gov> 17.12.04 19:58:42
> > > > >
> > > > >
> > Dear Hans & Hama,
> >
> > As you may remember, I was just in Turkey in October interacting with
> > many people in their climate group. They have a pretty good team.
> >
> > The question as I understand it is not the reliability of their data
> > that are transmitted (e.g., for December 2004) but for the section of
> > the CLIMAT code which shows anomalies to a base period or what quintile
> > the precipitation falls in. Turkey indicates that they think their
> > 1971-2000 Normals are more reliable than their 1961-1990 Normals. I
> > would agree with them that they are probably correct in that. I believe
> > the same could be said about the US Normals.
> >
> > However, as I recall, not all countries redo their Normals every 10
> > years. Many only redo them every 30 years, which, I believe is the WMO
> > Standard. So for this WMO coded transmission (CLIMAT) I expect that
> > they specify the 1961-1990 Normals.
> >
> > 1. Would it make a difference in climate monitoring? Yes for those
> > users who make use of the anomaly values it could make a big difference.
> > More important, probably, than reliability is that the climate changes
> > over a decade and taking 1961-1970 out and substituting in 1991-2000 to
> > the base period calculation may make a big difference in some cases.
> >
> > 2. Would it make a difference in climate monitoring? Probably not as
> > most climate monitoring groups don't use the reported anomalies each
> > month but rather take the observations and use them with Normals they
> > already have in a different file.
> >
> > In sum, if my memory was correct on the coding, I would recommend that
> > they continue to use the official standard even if they have something
> > better out there because it has the potential for making a significant
> > difference and it is important that all groups follow the official
> > standard.
> >

> > Does this sound reasonable? I'm not an expert in the CLIMAT code, so
> > you might want to check with Dick about official standards for CLIMAT
> > before you answer.
> >
> > Regards,
> >
> > Tom
> >
> > Hans Teunissen wrote:
> > Hama: This one looks like it's definitely a concern for CCl/WCD. From
> > theGCOS side, it seems just an issue of what's to be in the GSN archive
> > -1971 to 2000 (reliable) or 1961 to 1990 (possibly unreliable). My
> > votewould be for the former, but I don't know what CCl policy would be.
> > Tom,do you agree re the GSN archive? (I see 6 stations for Turkey are
> > inthere now, some with very long records; not sure what implication
> > ofthis proposal really would be for those...are you?) Or would you
> > preferto try to salvage some of the older data there (at NCDC)? Could
> > you letus know? I then suggest that Hama respond for the WMO/CCl
> > 'system'. Doesthat sound OK? I'll be away from tomorrow until 3 January.
> > Best wishes for the Holidays and the New Year, Hans.
> >
> > ==Dr.
> > Hans W. Teunissen
> > Tel:+41.22.730.8086Global Climate Observing System (GCOS) Fax:
> > +41.22.730.8052c/o World Meteorological Organization
> > E-mail:HTeunissen@wmo.int7 bis, Ave. de la PaixCP 2300, CH-1211
> > Geneva
> > 2Switzerland==========
> >
> >
> > Subject:
> > Fwd: Monthly CLIMATbulletinsFrom:
> > "Alexander Karpov" <AKarpov@wmo.int>Date:
> > Fri, 17 Dec 2004 11:52:43 +0100To:
> > "Hans Teunissen" <HTeunissen@wmo.int>
> > Dear Hans,As per attached query, I am kindly relying on your expertise
> > how to best navigate the solisitor.Best regards,Sasha *zden Dokuyucu
> > <odokuyucu@meteor.gov.tr> 17/12/04 08:58:21 >>> Dear
> > colleagues,First of all I want to say that, I find out your e-mail
> > addresses from the Web site of WMO. Please excuse me if this question
> > doesn't concern you. But if you know who concern this matter, could you
> > forward him/her this mail to get answer. I will be very gladif you pay
> > attention me.Thanks. We are a group of people who has been working in
> > the division of Climate Section,which is the sub departmentof
> > Agricultural Meteorology in Turkish State Meteorological Service. This
> > department is responsible for collecting all climatedata from the
> > observing stations, recording and transmitting them via the

> > telecommunication system to the data collectingcentre and archiving them
> > properly. This division is also responsible for transmitting monthly
> > CLIMAT bulletins to the WMO's relevant service. On behalf of Turkey, we
> > consider the climate data, which iclude the period of between 1971 and
> > 2000 years, are more trustworty because of the development in
> > technological, telecommuniational and training fields. Our experiences
> > are supporting this situation. We want to ask you, does it any effect on
> > global monitoring system, if we use the period of years 1971-2000
> > instead of 1961-1990in transmitting monthly CLIMAT REPORTS.We would be
> > very pleasure if you could get us more information.Yours Sincerely.
> > Ozden DOKUYUCUEngineerAgricultural Meteorology and Climatology Analysis
> > DepartmentTurkish State Meteorological ServiceP.O. Box: 401 Ankara,
> > TurkeyTelephone :+90-312-3022446Fax
> > :+90-312-3612371e-mail : odokuyucu@meteor.gov.tr
> > -- Thomas C. Peterson, Ph.D.Climate Analysis BranchNational Climatic
> > Data Center151 Patton AvenueAsheville, NC 28801Voice:
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> >

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References

1. mailto:Thomas.C.Peterson@noaa.gov

463. 1105024270.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: solomina@gol.ru
Subject: Fwd: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Thu Jan 6 10:11:10 2005
Cc: jto@u.arizona.edu,Eystein Jansen <eystein.jansen@geo.uib.no>

Olga
am sending this to get you in this loop re the discussion for slimming down the 2000 year
section Basically , IN THIS BIT - the decision is to reduce the glacier evidence to a very
much smaller piece , coached in the sense of how the glacier evidence is problematic for
interpreting precise and quantitative indications of the extent of regional or Hemispheric
Warmth (and even cold) - issues of translating tongue position or volume into specific

temperature and precipitation forcing . Hence , I am having to remove the stuff you sent
and am asking if you could consider trying to write a brief section dealing with the issues
I raise ? I also attach some initial comments by David Rind (on the full first draft of the
chapter sent round by Eystein) for consideration Sorry about this - but presumable (as you
suggested earlier) some of this can go in the 10K bit. You can shout at me (and the others)
later!
cheers
Keith

X-Sender: jto@jto.inbox.email.arizona.edu
Date: Wed, 5 Jan 2005 12:24:47 -0700
To: Keith Briffa <k.briffa@uea.ac.uk>
From: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov,
Fortunat Joos <joos@climate.unibe.ch>, joos <joos@climate.unibe.ch>,
"Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
X-Virus-Scanned: by amavisd-new at email.arizona.edu
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
Hi Keith and Co - I think David likes a good debates, so the main thing is to consider
his comments and respond appropriately. Although the first priority has to be on the ZOD
text and display items, maybe you can go back over his comments AFTER the looming
deadline and further discuss things with David and others. For now, just work away.
The biggest issue is how to handle forcing and simulations - i.e., where to put
different pieces in the chapter. Eystein and I will help the team work through this.
More soon, but for now just proceed as you have been proceeding. There is real merit to
the concept that your section is about how climate varied over the last 2ka, and what
caused these variations. The flip side is that we need to get a clear vision of how this
differs from what goes into the other sections. Eystein and I will work more on this
asap.
Your plan re: glaciers is good. That's a tough one, but it has to be boiled WAY down.
Moreover, my gut is to focus on the extent to which these complicated natural archives
(e.g., complicated by ppt change) support or do not support the other proxy
evidence/conclusions. This is why I was thinking we might think about a box, and to
include the Lonnie perspective in it - e.g., glaciers are now melting everywhere (almost
- we know why they are not in those places) in a manner unprecedented in the last xxxx
years. Make sense? See what Olga says, and if needbe, I can help focus that stuff more.
Thanks! Peck

Hi Peck (et al)
I am considering comments (including David's) re last 2000 years - some are valid =

some are not . Will try to chop out bits but we need this consensus re the forcing and
responses bit - I am for keeping the forcings in as much as they relate to the specific
model runs done - and results for last 1000 years as I suspect that they will not be
covered in the same way elsewhere . David makes couple good points - but extent to which
forcings different (or implementation) perhaps need addressing here. The basic agreement
I mean is that the recent warming is generally unprecedented in these simulations.
It will take time and input from the tropical ice core /coral people to do the regional
stuff well . I think the glaciological stuff is a real problem - other than just showing
recent glacial states (also covered elsewhere) - of course difficult to interpret any
past records without modelling responses (as in borehole data), but this requires
considerable space . My executive decision would be to ask Olga to try to write a couple
of papragraphs on limits of interpretation for inferring precisely timed global
temperature changes? What do others think? I only heaved Olga's stuff in at last moment
rather than not include it - but of course it needs considerable shortening. The
discussion of tree-ring stuff is problematic because it requires papers to be published
eg direct criticism of Esper et al. We surely do not want to waste space HERE going into
this esoteric topic? All points on seasonality , I agree with , but the explicit stuff
on M+M re hockey stick - where is this? ie the bit about normalisation base affecting
redness in reconstructions - sounds nonsense to me ?
I have to consider the comments in detail but am happy for hard direction re space and
focus. If concensus is no forcings and model results here fine with me - Peck and
Eystein to rule
Keith

--
Jonathan T. Overpeck
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Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences
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[1]http://www.geo.arizona.edu/
[2]http://www.ispe.arizona.edu/

--
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Phone: +44-1603-593909
Fax: +44-1603-507784
[3]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.geo.arizona.edu/
2. http://www.ispe.arizona.edu/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

464. 1105042411.txt
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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Susan.Solomon@noaa.gov, Susan Solomon <Susan.Solomon@noaa.gov>, Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Re: After the FOD
Date: Thu Jan 6 15:13:31 2005
Cc: martin.manning@noaa.gov

Susan,
Thanks for the quick reply. Kevin might have thoughts, but I'll give it some thought
over the next few months. It isn't crucial till well after our second meeting.
Kevin can relay our thoughts on references next week, and we can come up with specific suggestions here if these need to be discussed with WG2 and WG3 before all the second lead author meetings. I know we can reduce our number of references with more work, but I suspect we will be requested at the time of the FOD and SOD (and maybe the ZOD) to consider many others. A lot of NMSs, University Depts. and Research Institutes measure success as seeing their work cited by IPCC ! I reviewed KNMI this time last year and they did exactly this. This shouldn't be a measure, but we will likely be under pressure to cite many more papers for this reason.
Cheers
Phil
At 13:58 06/01/2005, Susan Solomon wrote:

Phil,
Happy new year to you too. It's good to hear that your chapter is progressing well. I'll see Kevin next week at the AMS meeting and perhaps we can discuss its high points, along with the more basic issue of references, etc.
You've raised a number of concerns that are always an issue not only for IPCC but also for other assessments and even for our own individual key papers at times. But you have made no suggestions as to how to deal with them.
Could you please let me know if you have any suggestions to put forward?
Thanks,
Susan
>> Susan,
> Happy New Year !
> I'm working hard on the Chapter that Kevin has put sterling efforts on over
> the Christmas break. It'll be with you by Jan 14, hopefully earlier.
>
> I've been talking to Keith Briffa here and there is a lot of email
>traffic
> from the skeptics about the last 1K years. Also Senator Inhofe's speech
> from Jan 4 is doing the rounds.
>
> I know you've probably thought all this through, but there will be
> a number of key issues in AR4. Likely candidates that I'm aware of
> are the MSU issue (where we seem to be making some progress)

> and the last 1K years (where we might be but as this is about paleo
> it does take time).
>
> Well the issue is, once the FOD goes out to all -in say Sept/Oct 05 -
> what will stop the drafts getting onto web sites, in the media etc - and
> the whole thing blowing up then instead of being properly aired in 2007.
> I know we won't have an SPM, but those that want will say - they are
> only referring to papers that endorse their views and they are not
> referring to scientists with contrary ones. AR4 will get a bad press
> only half way through it's development.
>
> I know you will have phrases like 'draft only' and 'not for
distribution'
> but can we really police this.
>
> Once the ZOD is in, Kevin and me will be sending you some ideas
> about referencing - formats, abbreviations, smaller fonts etc. We currently
> have about 3 times what we allowed for (7 pages of 70).
>
> Cheers
> Phil
>
>
>
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>
>

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465. 1105282939.txt
##########

From: "olgasolomina" <olgasolomina@yandex.ru>
To: k.briffa@uea.ac.uk
Subject: IPCC glaciers
Date: Sun, 9 Jan 2005 10:02:19 +0300 (MSK)
Reply-to: olgasolomina@yandex.ru
Cc: jto@u.arizona.edu, eystein.jansen@geo.uib.no, Valerie.Masson@cea.fr,
ricardo@lab.cricyt.edu.ar

Hi Keith,

May I have your part of the text (2ka) to have a look, please. As far as I
understand we decided to have glacier fluctuations separately in a frame. In this

case, shall we keep glacier variations in the Holocene or we will extract it to place in this frame? I will contact Georg Kaser (ch 04)to see what they already have to comment on glacier/climate links. They must have treated this problem already. Besides it is more natural to concider it using the instrumental data. In this case we will deal with the paleo problem only, i.e. the dating of moraines, the errased traces of old advances, the use of lacustrine deposits to reconstruct the glacier erosion (size), the reconstruction of former ELAs, the sizes of retreated glacier etc. Shall we discuss the accumulation reconstructed from the ice cores or it will be just the problem of glacier front variations?
Another possibility is to have a common frame with the ch 04: How glaciers reflect climate and what they say about the climate in the Holocene (last 2ka).

I need the answers before I begin.

Please notice the change of my e-mail address. I will check both addresses a while, but have to move to a new one olgasolomina@yandex.ru

Regards,
olga

466. 1105386027.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: [Wg1-ar4-ch06] comments to 6.3.2.1 (mainly for Keith)
Date: Mon, 10 Jan 2005 14:40:27 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, cddhr@giss.nasa.gov, rahmstorf@pik-potsdam.de, joos <joos@climate.unibe.ch>

<x-flowed>
I agree; Keith should have the room, and section 6.5.8 should be compatible - has Fortunat followed the discussion between David/Stefan. Can you guys (David, Stefan, Keith, and Fortunat) ensure this?

Thanks, Peck

>Hi,
>interesting discussion on an important topic. If space is the
>limiting factor we may have to evaluate whether to cut back on less
>central issues elswhere in the chapter. We will to a large extent be
>judged on how we tackle the hockey stick, sensitivity, unprecedented
>20th century warming isuues in view of palaeo, and if a slight
>expansion is what it takes to do this properly, then I am
>sympathetic to that (without having heard Peck on the issue).
>Cheers,
>Eystein
>
>
>
>At 16:32 +0000 10-01-05, Keith Briffa wrote:
>>thanks David
>>have to say that it is very difficult to say much in the minimal
>>space - and we really need a page to discuss the problems in the
>>reconstruction and and interpretation of the various forcings in
>>different models - I am just going to put this down in an over
>>abbreviated way and ask for specific corrections for you and Stefan
>>et al. The detail perhaps depends on what the final Figure looks
>>like and Tim is trying to put it together but lots of weird and
>>interesting stuff / questions arise as we do - especially relating

>>to past estimates of solar irradiance used by different people. At
>>15:29 10/01/2005, David Rind wrote:
>>>(I tried to send this earlier and it got hung up; apologies if it
>>>eventually gets through and you get a second version.)
>>>
>>>Well, yes and no. If the mismatch between suggested forcing, model
>>>sensitivity, and suggested response for the LIA suggests the
>>>forcing is overestimated (in particular the solar forcing), then
>>>it makes an earlier warm period less likely, with little
>>>implication for future warming. If it suggests climate sensitivity
>>>is really much lower, then it says nothing about the earlier warm
>>>period (could still have been driven by solar forcing), but
>>>suggests future warming is overestimated. If however it implies
>>>the reconstructions are underestimating past climate changes, then
>>>it suggests the earlier warm period may well have been warmer than
>>>indicated (driven by variability, if nothing else) while
>>>suggesting future climate changes will be large.
>>>
>>>This is the essence of the problem.
>>>
>>>David
>>>
>>>
>>>
>>>
>>>
>>>At 9:28 AM +0000 1/10/05, Keith Briffa wrote:
>>>>THanks Stefan
>>>>At 21:13 07/01/2005, Stefan Rahmstorf wrote:
>>>>>Keith,
>>>>>
>>>>>some comments added in the text for the past millennium, plus I
>>>>>wrote some extra sentences on the implications of the dispute
>>>>>(repeated below).
>>>>>Hope it is useful,
>>>>>Stefan
>>>>>
>>>>>>Note that the major differences between the proxy
>>>>>>reconstructions and between the model simulations for the past
>>>>>>millennium occur for the cool periods in the 17th-19th
>>>>>>Centuries; none of these reconstructions or models suggests
>>>>>>that there was a warmer period than the late 20th Century in
>>>>>>the record.
>>>>>>
>>>>>>A larger amplitude of preindustrial natural climate variability
>>>>>>does not imply a smaller anthropogenic contribution to 20th
>>>>>>Century warming (which is estimated from 20th Century data, see
>>>>>>Chapter XXX on attribution), nor does it imply a smaller
>>>>>>sensitivity of climate to CO2, or a lesser projected warming
>>>>>>for the future.
>>>>>
>>>>>
>>>>>--
>>>>>Stefan Rahmstorf
>>>>><http://www.ozean-klima.de>www.ozean-klima.de
>>>>>www.realclimate.org
>>>>>_____
>>>>>Wg1-ar4-ch06 mailing list
>>>>>Wg1-ar4-ch06@joss.ucar.edu
>>>>>http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
>>>>
>>>>--
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>>>>_____
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>>>>Wg1-ar4-ch06@joss.ucar.edu
>>>>http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
>>>_____
>>>Wg1-ar4-ch06 mailing list
>>>Wg1-ar4-ch06@joss.ucar.edu
>>>http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
>>
>>--
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>>http://www.cru.uea.ac.uk/cru/people/briffa/
>>_____
>>Wg1-ar4-ch06 mailing list
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>>http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
>
>
>--
>_____
>Eystein Jansen
>Professor/Director
>Bjerknes Centre for Climate Research and
>Dep. of Earth Science, Univ. of Bergen
>Allégaten 55
>N-5007 Bergen
>NORWAY
>e-mail: eystein.jansen@geo.uib.no Phone: +47-55-583491 -
>Home: +47-55-910661
>Fax: +47-55-584330
>-----------------------
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>More info at: www.bjerknes.uib.no/mcts
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>_____
>Wg1-ar4-ch06 mailing list
>Wg1-ar4-ch06@joss.ucar.edu
>http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06

--
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</x-flowed>

467. 1105395606.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Valerie.Masson@cea.fr
Subject: Re: Glaciers Ch 6
Date: Mon, 10 Jan 2005 17:20:06 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, trond.dokken@bjerknes.uib.no, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>
V - well said. Eystein and I will be working on your Holo section - more tomorrow. thx, Peck

>2 comments
>
>- the various NH T reconstr use polar records : to my knowledge only
>use of melt index that itself does not calibrate properly in Mann's
>reconstruction. I sent you Keith winter d18O from Vinther 2003 which
>provides a reconstruction of NAO changes (I think this is the more
>detailed calibration study for Greenland isotopes).
>On a decadal time scale calibration studies for Antarctica (Vostok
>and Law Dome, inland vs coastal sites) using available instr records
>(50 years) show correct decadal scale temperature signals. Even at
>places with subannual resolution like Law Dome I think that you
>cannot use the isotopes on a yearly basis but only decadal scale.
>
>- tropical glaciers : works conducted here on Andean ice cores
>together with modelling of isotopes in a GCM all showed a consistent
>decadal variability on the 20th century, most of which interpreted
>to be related to precip change (see for instance Hoffmann et al,
>Science, "Taking the pulse of the tropical water cycle", Science,
>2003). For more ancient past periods it is thought that part of the
>signal is due to T (and vertical lapse rate change), part to
>precip.I would not like to cosign any text claiming for a T
>reconstruction based on Andean ice cores.
>
>
>Keith Briffa wrote:
>
>>I agree with suggestion - there is the problem of the isotopic
>>analyses from tropical (and to some extent polar) ice cores still .
>>I am not happy simply to show these in a Figure relating to the
>>large-scale temperature changes - because we are not sure of the
>>extent to which they can be interpreted as such . The various NH
>>reconstructions use some polar isotope records but looking at plots
>>of the tropical records throws up some strange behavior over the
>>last 2000 years . I am not happy to write about these as Valerie
>>and Olga are better qualified and because I would like to see more
>>formal calibration against even short temperature records . I have

>>therefore , not as yet explicitly said anything about these
>>tropical records. I will sendthe latest text and latest draft
>>FIgure 1 later today
>>
>>At 10:03 09/01/2005, Jansen@geo.uib.no wrote:
>>
>>>Dear Olga,
>>>My suggestion would be, and I believe this is echoed by Peck, is
>>>that the box
>>>we produce comes in the overall Holocene sub-chapter, thus to avoid
>>>repetition. The figure should mainly give syntheses of the glacier extent
>>>variations through the Holocene, if possible, or a fraction of it
>>>if data only
>>>exists e.g. for the last few millennia, for those regions where there is a
>>>reliable data set. Then with text explaining what we think drove these
>>>variations. I think it should be a box in Ch6, and could also include the
>>>recent trends I have just talked with Atle and he is able to contribute
>>>curves for Scandinavia and the Alps into a figure before the end of the week
>>>(in a couple of days). He feels putting something together for North America
>>>and perhaps New Zealand is feasible, but he cannot do this before the ZOD
>>>deadline. Perhaps you might be able? If we get something for the
>>>tropics from
>>>Lonnie and Ellen and what you have, I will be able to put this together in a
>>>figure for the box via assistance here. We can in such a figure leave space
>>>open for curves we anticipate including for the First Draft.
>>>It might be a good idea to in this figure also include the recent,
>>>instrumental evidence for the same regions, akin to what will be in Ch4, and
>>>of course, in the next iteration come back to possible joint Ch4
>>>and 6 figure.
>>>
>>>How does this sound?
>>>
>>>Cheers,
>>>Eystein
>>
>>
>>--
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468. 1105462633.txt
##########

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Valerie.Masson@cea.fr, masson@dsm-mail.saclay.cea.fr
Subject: Re: Urgent - pls respond FAST
Date: Tue, 11 Jan 2005 11:57:13 +0100
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Jean-Claude Duplessy <Jean-Claude.Duplessy@lsce.cnrs-gif.fr>, raynaud@lgge.obs.ujf-grenoble.fr, cddhr@giss.nasa.gov, rahmstorf@pik-potsdam.de, dolago@uonbi.ac.ke, Keith Briffa <k.briffa@uea.ac.uk>, Fortunat Joos <joos@climate.unibe.ch>

<x-flowed>
Valerie,
Thanks for putting together the chaper so well. I think it is quite comprehensive now. I have made a few changes in the enclosed document and also added a comment(pops up if you mark the yellow field).
I tend to like the questions, and think it highlights the relevance elements of the chapter. The missing references I have suggested, we can take care of in the final editorial process from our side.
As for figures one figure showing the evidence for Holocene warrmt and the abrupt character of the 5-4ka cooling, perhaps with a low latitude data set that shows another evolution would be good to have, as you indicate, but we cannot bombard the chapter with wiggly lines, so the most characteristic exampes would be best.
If you need high lat.ocean data I can provide, or perhaps NorthGrip O-18 is best?
Cheers,
Eystein

Cheers,
Eystein

At 11:13 +0100 11-01-05, Valerie Masson-Delmotte wrote:
>Valerie Masson-Delmotte wrote:
>
>>I tried the question style for the Holocene
>>section... Any feedback would be appreciated
>>together with missing references (Fortunat).
>>Valerie.
>>
>>Jonathan Overpeck wrote:
>>
>>>Hi all leads and seconds of our Chap 6.5
>>>Synthesis sections. Fortunat came up with a
>>>interesting way to highlight what's important
>>>and why in his section 6.5.3, and Eystein and

>>>I would like feedback from you - particularly
>>>the leads - on whether this approach would
>>>work for each of your subsections.
>>>
>>>He used a question and answer style. If people
>>>do not like this then the question at the
>>>beginning of the paragraphs can of course be
>>>easily dropped and replaced by a statement.
>>>BUT, what do you say about using this
>>>convention throughout 6.5??? Note that some
>>>sections might have much more text per unit
>>>question.
>>>
>>>Please respond asap. Thanks, Peck and Eystein
>>
>>
>>
>
>
>
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Attachment Converted: "c:\eudora\attach\Holocene-VMD3_ej_com.doc"

469. 1105543270.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: derzhang@cma.gov.cn
Subject: Re: Re: [Wg1-ar4-ch06] URGENT - Deadline approaching
Date: Wed, 12 Jan 2005 10:21:10 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, r.ramesh@prl.ernet.in,
dolago@uonbi.ac.ke, Jean-Claude Duplessy <Jean-Claude.Duplessy@lsce.cnrs-gif.fr>,
Keith Briffa <k.briffa@uea.ac.uk>

   Hi Prof. Zhang: thanks for your email and good to hear about your book. I will
send the
   reference file to the LAs for them to incorporate as appropriate. You will also
be editing
   the ZOD when it's complete, or of specific sections before then if you ask the
appropriate

leader of a section of interest (see previous listserv email with this list in case you
don't remember from Italy).

Regarding 6.5.9, I will cc this to Dan and Ramesh so you can coordinate with them
directly. This is the process we have adopted for all subsections so we don't waste time
with the CLA's having to relay messages. Go direct...

I will also CC to Jean-Claude and Keith, so they make sure they have checked your input.

Many thanks, Peck

Å@Dear Peck:
...

As regards Section 6.5.9 I shall do my utmost to help Den and Ramesh. But the assistance is to come only after I have read through their draft . Only in that way can
I form an ideal "it must be relevant to policy makers" . I have been accustomed to
write about scientific facts. Now I am confronted with a new problem how to serve the
purpose of another style. Otherwise my suggestions would be of no use.

I had sent paragraphs to Jean-Claunde for Section 6.2.2 and to Keith Briffa for
section 6.3.2.1. last Nav..

With best wishes,

--

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470. 1105556495.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: where I am !!!! !
Date: Wed Jan 12 14:01:35 2005

Eystein
in theory - it is supposed to be finished. I would just remove the two sections I suggested
(or certainly move the regional simulation stuff into Ricardo's section. How does

end note
cope with references that are not published?
Keith
At 13:26 12/01/2005, you wrote:

Hi Keith,
I am in transit back to Bergen where there is a strong storm at present, but just a
query to ask what you think a a realistic time fframe for your part. I will be reding
through it on the way. If you have problems getting the references in, this is
something we can help with, if you just write i text author name, year and paper, then
we download from the ISI base and enter into End Note here, just to help you
prioritising the text and figures.
Thanks for all your efforts. This is a critical part of the Chapter and the most complex
and it seems to progress well, despite the strains.
Cheers,
Eystein

Basically , I need to send this to you to because there comes a point when I am just not
able to read it objectively.
I would really like you both - and David and Stefan (I am ccing to them only) to look
at it . Obviously it has grown too much, but the information in here is in my opinion
all important.
I suggest removing the regional simulations stuff from the end (as David said earlier!)
but feel this should be somewhere - also (sorry Eystein) perhaps the ocean section
should go? I have dropped the proposed Figure 2 _ after wasting a lot of time on it -
there are too many problems with getting and understanding data - and then making any
sensible conclusion on the basis of it. We really must have the two Figures left though
- or some variants (these need borehole curves including and some way of indicating
envelope of uncertainty around all reconstructions - perhaps as gray shading of
different darkness depending on how may confidence limits overlap).
I would really appreciate a dispassionate look by all of you at the conclusions drawn
after the the desciption of both Figures - in the light of the discussion we had about
interpreting these Figures. I am really happy if you and David and Stefan (and
Fortunat?) consider what is worth and not worth trying to say re the implications of
these Figures, beyond the TAR. I can not tell if what I am saying is balanced (I know
Esper reconstruction is very hairy and ECHO-G run has much too great long-term
variability - but no evidence PUBLISHED to support this - yet at least). Is what I say
about the implications of the reconstructions banal?
I have been battling with teaching today and fucked up course scheduling by the
administration that has outraged some students. Tomorrow I must take daughter back for
new term in Cambridge - and now must work on proposal for Russian who leaves Thursday
and needs to submit before then.
Do have a look and trim , cross reference as needed. The nightmare with these

references
continues also and I will have to get someone to help out here - incidentally
our
secretary has gone absent for a month . I will be back in hopefully by tomorrow
afternoon . The conclusions (bullets?) should be very brief - but can not see
them yet -
suggestions welcome
I can try to do something for the methods but would rather you just told me
exactly what
is needed. I will then work on this Thursday and likely happy to accept what
you say re
this text. I know I have not contributed to the discussing on other sections -
very
frustrating - but must wait til after ZOD . Sorry
Keith
--
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[3]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.bjerknes.uib.no/mcts
3. http://www.cru.uea.ac.uk/cru/people/briffa/

471. 1105566936.txt
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, peltier@atmosp.physics.utoronto.ca, rahmstorf@pik-potsdam.de, cddhr@giss.nasa.gov
Subject: Urgent - FINAL review/edits of 6.5.8 Sensitivity
Date: Wed, 12 Jan 2005 16:55:36 -0700
Cc: raynaud@lgge.obs.ujf-grenoble.fr, Jean-Claude Duplessy <Jean-Claude.Duplessy@lsce.cnrs-gif.fr>

Hi all on the list above... Some of you have received this already straight from David, but
some other key people have not. Eystein and I would appreciate it very much if you would
please read/comment/and edit the attached section 6.5.8 (Sensitivity) NO LATER THAN
THURSDAY NOON, Eastern time (6PM GMT).

Please send responses to all on the address list ABOVE, plus Peck.

Thanks, Peck

> X-Sieve: CMU Sieve 2.2
> X-Sender: drind@4dmail.giss.nasa.gov
> Date: Wed, 12 Jan 2005 13:29:53 -0500
> To: joos <joos@climate.unibe.ch>
> From: David Rind <drind@giss.nasa.gov>
> Subject: Re: Fwd: Re: Fwd: 6.5.8 Sensitivity
> Cc: David Rind <drind@giss.nasa.gov>,
> Jonathan Overpeck <jto@u.arizona.edu>,
> Dominique Raynaud <raynaud@lgge.obs.ujf-grenoble.fr>,
> Eystein Jansen <eystein.jansen@geo.uib.no>,
> trond.dokken@bjerknes.uib.no, peltier@atmosp.physics.utoronto.ca,
> Jean-Claude Duplessy <Jean-Claude.Duplessy@lsce.cnrs-gif.fr>,
> rahmstorf@pik-potsdam.de, cddhr@giss.nasa.gov
> X-Virus-Scanned: amavisd-new at email.arizona.edu
> X-Spam-Status: No, hits=-2.272 required=7 tests=BAYES_00, HTML_20_30,
> HTML_MESSAGE, MIME_SUSPECT_NAME
> X-Spam-Level:
>
> Dear Fortunat (and others),
>
> Here is the revised section 6.5.8. I've put in most of your changes (and also most of
> those suggested by Stefan, particularly with regards to clarifying the sign of the
> radiative forcing). Most importantly, I've removed the table - I agree it seems to imply
> a solidity that is really not there. The one thing I have not done is condense it
> greatly (of course!). The real reason for going into such detail, rather than just
> saying, "well, the forcing and response are uncertain, so we can't conclude anything",
> is I think it's important to show that paleoclimate scientists have gone to some effort
> to try to deduce climate sensitivity from the paleorecord, the parameter that is
> probably of most interest to IPCC. In that respect the details are important, as are the
> magnitudes of uncertainty represented in the different studies. Obviously, at any point
> in the proceedings the section can be shortened, but I thought it useful to

start with
this level of quantification, and show paleoclimate has this similarity with the rest of
IPCC in addition to more qualitative concepts.

I've responded to your individual comments below.

At 6:15 PM +0100 1/11/05, joos wrote:

Dear David,
Here my comments on the updated climate sensitivity section. Please apologize if I formualate my comments straight away, but I need to leave very soon. Many of my comments might have to do with presentation.
Your main conclusions in paragraph f are fine.
My view is that it would be ideal to address the issue from a probabilistic view point. this is of course not always possible.
1) Maunder Minimum section:
Several studies using Monte Carlo approaches show that almost any climate sensitivity is posssible when taking into account uncertainties in radiative forcing input data as well as observational records over the 20 century as constraints. See the Paris report for more information.
The uncertainty does not only arise from indirect aerosol effect, but also form the whole range of forcing agents that all have an uncertainty attached. E.g. Reto Knutti did some evaluation of his results where he assumed that the aerosol forcing is exactly know (No error) -> even then climate sensititivity remains unconstraint. Clearly, uncertainty is growing when going further back in time than the last century as done here. Then, the numbers provided in the table are useless, as you now state in the last sentence of the revised text.
2) Other sections:
I think similar concerns also hold for the other sections. For example, the LGM global cooling is very uncertain. I have just heard yesterday a

talk by Ralph Schneider who showed how different SST reconstructions (Alkenone, Cd/Ca, MAT, radiolare etc) disagree. global SST cooling might be anywhere between 0 and 4 K or so. Of course, CLIMAP and the recent GLAMAP update provide a reasonable estimate. However, the point is that uncertainies are huge.
The table is a very focused and stand alone thing for the reader. It gives the impression that climate sensitivity for different period can be well evaluated. However, this is not the case.
3) My conclusion:
- The table should be dropped. I have quite a strong feeling here, as it seems to me that the number in the table are very hard to defend and should not be made prominent.

The table and reference to it has been dropped.

- The whole section should be condensed considerably. Your main conclusions in paragraph f are fine.

Well, removing the table will shorten this section!

Further comments:
1) section d) 1. para: solar forcing reduction estimate range up to

0.65% for MM e.g. Reid, 97 and Bard et al.

Correction made, and reference added (and I also corrected the numbers as Stefan
suggested, although the upper number is actually larger given the Reid estimate).

2) section d, last para equilibrium
The statement that transient effects are not important is very hard to defend:
2a) The warming and forcing up to today is considered. Certainly, we are now far from equilibrium (a lag of 30 years or so).
2b) the volcanic forcing is very pulse like and I do not see how the equilibrium concept holds here. It can only be evaluated in a transient way.
3c) The MM is probably not in equilibrium climate, as solar forcing has likely varied over the MM as indicated by radiocarbon, althoug sunspots were not present

I've removed the word "transient" but I have justified the equilibrium aspect of the sentence with a reference (we investigated that issue by running from 1500 through the Maunder Minimum, and seeing what the prior changes in solar forcing did to the Maunder Minimum cooling - the effect, as noted in the reference, was small in our model).

3) section b) end of 1. para: How should such a 'general climate sensitivity' be defined?

For now I've simply suggested what should also be factored in; I don't know that it's our place to come up with a new definition per se, although if IPCC is interested, we could try!

4), section c) Somewhat a mix of model and observations. end of 2 para:
It is not clear which forcing was operating in these different models (at least it is not stated in the text) and hence one can not directly imply a climate sensitivity in the way done here. For this the forcing that went into the model simulations must be known.

I looked at each of the references and saw what forcing they actually used - they were all very similar except for one which used current orbital parameters (not really important). This comment is now included.

Hope this is useful and looking foreward to further debate the issue.

Thanks for the comments!

David

ps - Jonathan, the attached Endnote library includes the references we discussed yesterday, as well as all the ones relevant for this section.

--

//
//

--

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472. 1105588673.txt
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: methods - section 6.2.2
Date: Wed, 12 Jan 2005 22:57:53 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, joos <joos@climate.unibe.ch>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>

<x-flowed>
Hi all: Keith and Tim asked for specific requests in terms of what you could do for section 6.2.2. I'm hoping Valerie and Fortunat have already made enough progress that they can ask, but here's my take:

1. you have lots of methodology material in your 6.3.2.1, and this is good. It would be good to refer to this from the earlier, more general 6.2.2

2. the goal of 6.2.2 is to give the reader more confidence in paleo and to get them to read on with confidence that what they read will be of use

3. I suspect that the format V and F will be working around will be one that can first highlight chronological issues (that we can date some proxies very well, and that's what we focus on in this chapter primarily). It would be good to have the usual comforting comments about tree rings and other annual proxies.

4. The, it would be good to have the basics on how proxies reflect climate, and how we know we understand the relationship. That it is useful even if the proxy is responding to things other than climate. Seasonality, etc. Include brief overview of calibration, verification. you know the drill.

5. keep it short and not too detailed. Use lots of references - including to the most recent stuff.

6. I'm sure we'll end up modifying/improving later after we figure out what to do with the appendix

7. Need to work fast, very fast, but hopefully V and F have made real progress already.

Thanks!! Peck

--
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473. 1105588681.txt
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: Comments on 6.3.2.1
Date: Wed, 12 Jan 2005 22:58:01 -0700
Cc: rahmstorf@pik-potsdam.de, drind@giss.nasa.gov, Eystein Jansen <eystein.jansen@geo.uib.no>, joos <joos@climate.unibe.ch>

<x-flowed>
Keith, Tim (and friends- please read below and provide your comments THURS too) - just finished reading your draft and my primary reaction is one of great relief and admiration. You've done an excellent job. I'm sure things will look different in the end, but for the ZOD, this lays things out just fine.

That said, here are comments. More are in the attached draft w/ track changes

1. still need to see the figs - ok to state what still has to be done (as you have)
2. regarding the ocean section, I think some of it should stay in - both as a placeholder for other relevant stuff, and because it is important. See attached. It would be good if EYSTEIN would look at my comments for this section and provide the needed minor help - we need the punchline/bullet - how does the 20th century compare with the previous part of the record (you say it shows the warming, but then don't go the next step.
3. THIS IS THE ONLY COMMENT THAT WILL TAKE MORE THAN A FEW MINUTES - can we get THE word on the MWP in before hydro? Heck, I'd even support a small (smaller than the other ones) box. There is lots of debate about the MWP,. and we need to weigh in. Was it global, hemispheric, regional only (e.g., Europe and N. Atlantic - can then refer back to it in ocean section)? Was it one synchronous warm event or a bunch of shorter regionally asynchronous events? Warmer than 20th? Late 20th? (think you answered this, but need to nail it!). Cite the cast of papers you've already discussed, plus Bradley et al Science 03.
4. what you say is balanced, and it's ok to note in the text where you anticipate serious improvement w/ more published paper support - e.g., Esper (you're doing a paper on this, no?) and ECHO-G.
5. have to have boreholes on Figs too - that would be more important now than uncertainty estimates around all recons - the latter is

harder, but in any case, say what you intend to add after ZOD.
6. see text - minor edits
7. I can make draft bullets from what you sent

Guys - it was worth the wait. Hope you can take advantage of the relatively minor edits required and help some with other sections as asked for.
--
Jonathan T. Overpeck
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Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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474. 1105627987.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Peck your comments...
Date: Thu, 13 Jan 2005 09:53:07 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Hi Keith -

1) ok on the refs - send tomorrow
2) glad you're keen for the box - it can't be too long - maybe shot for ca. 400 words? After the ZOD is done, I'm sure we can tune to the correct balance of info. A fig is ok if it's compelling. The box will either be 6.1 or 6.2 depending on whether you refer to it in your section before or after the glacier box. I'm guessing it'll be 6.1 and come first, but it's your call. Think of a title for the box - something like "Box 6.1: The Medieval Warm Period" or maybe something more catchy. Can't be too glib.
3) glad you have some borehole in there. Of course, you'll be at the front of the line for dealing with the grief we get no matter what choice we make. So the key is to go with what can be best justified. Your section has this nice balance already.

Thanks for getting Tim (and you as time permits) to work on those other sections - VERY important too. But, your section is the most important.

thx, Peck

>...are really welcome. Am now incorporating them , plus doing some
>editorial bits - though will wait on Eystein to send replacement
>ocean bit . Having to get one of my people to do the references but
>not likely these will arrive til tomorrow. The main point to discuss

>is your comment on the MWP . I like the idea of a box. This IS
>sufficiently important to warrant it - in the context that most
>people say "it was warm/warmer than now then so disproves anthro
>effect - we should address this explicitly. I will have a go - but
>need to know how many words and Figure(s) allowed. We can simply
>just refer to this box in a couple of places in existing text. Sorry
>about Figures - now got some (2) borehole lines in (but may need
>more - reluctant to use Huang and Pollack original though because
>obviously much too cold on basis of simple regional averaging
>biases. Will send latest version (without box on MWP) tonight my
>time.
>Keith
>
>--
>Professor Keith Briffa,
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>University of East Anglia
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>
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--
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Professor, Department of Atmospheric Sciences

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</x-flowed>

475. 1105653626.txt
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##########

From: David Rind <drind@giss.nasa.gov>
To: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
Subject: Re: 6.5.8 revisions
Date: Thu, 13 Jan 2005 17:00:26 -0500
Cc: David Rind <drind@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Jonathan
Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen
<eystein.jansen@geo.uib.no>, FortunatJoos@email.arizona.edu

   Here are my responses to Stefan's comments. While I could have made each of these
points in
   the document itself, it is already sufficiently long that Jonathan had me cut it
before
   most of you guys saw it.

   At 8:53 PM +0100 1/13/05, Stefan Rahmstorf wrote:

Hi folks,
on the topic of climate sensitivity. I just lost a long mail on it due to a software
crash, so sorry if I'm brief now.

I think it makes no sense for the purpose of the IPCC to discuss a climate sensitivity
to orbital forcing - if such a thing can be defined at all. The first-order idea of
orbital forcing is that in annual global mean it is almost zero - and in any case the
large effect orbital forcing has on climate has very little to do with its global mean
value. Hence, we'll confuse people by discussing it in this way, and even citing numbers
for it. For the purpose of IPCC, I think climate sensitvity should refer to climate
sensitivity wrt. greenhouse gases.

The point here is that climate can be forced by other factors than simply a global, annual
average radiation change, which is the metric now being used. The orbital forcing induced
changes are wonderful examples of this, hence the paleoclimate chapter is a perfect place
to discuss it. Variations in seasonal and latitudinal forcing clearly have had a major
impact on climate, including forcing of ice ages, yet the annual average radiative change
is small. The importance of this with respect to IPCC is that other climate forcings can
also affect the seasonal and latitudinal distribution of radiation - aerosols, land surface
changes, and even solar radiation (considering cloud cover distributions) - hence they too
may have a disproportionate influence compared to their annual global average magnitude.
What is said in this subsection is simply that this one metric clearly fails with respect
to the major variations in paleoclimate, and as a general rule, there should be room for an
expanded concept (which may then have utility for current and future climate forcing as
well).

Also, it is questionable to discuss climate sensitivity for uncoupled models, especially
for glacial times - Ganopolski et al. (Nature 1998) have shown that glacial climate
looks very different with mixed layer ocean vs. coupled. I think for a 2007 IPCC report
we shouldn't be discussing old uncoupled runs when coupled model results are available.
(And it is a little odd that the above paper, the first coupled model simulation of
glacial climate, cited over 150 times so far, is ignored here in the discussion of the
last glacial maximum - if you do a search on the Google Scholar engine for the key words
"Last Glacial Maximum", you'll find it's the second-most cited paper on this topic after
the Petit et al. Vostok data paper.)

In fact, most if not all of climate sensitivity measurements have been done for what Stefan calls "uncoupled models", atmospheric models coupled to mixed layer ocean models. The results from all prior IPCC reports give sensitivities from precisely these types of models - for the basic reason that almost no one has ever run a coupled model for 2CO2 to equilibrium. The other disadvantage of coupled models in this regard is that their control run, if simulated long enough, often does not reproduce the current climate in important respects - one is then getting a climate sensitivity with respect to something far removed from the current climate, so what good is it? The fact that models coupled to a dynamic ocean and those coupled to mixed layer oceans may get different responses - and one can see from the numbers that the responses are actually fairly similar in general - can be related to the ocean dynamics changes; as the text notes, that is considered a feedback in this subsection, and therefore an appropriate part of the climate sensitivity calculation.

I still think it makes no sense to say that climate sensitivity depends on the sign of the forcing. Talking about greenhouse gases: whether you will do an experiment going from 280 ppm to 300 ppm, or the other way round from 300 ppm to 280 ppm, should give you the same climate sensitivity. Perhaps you mean that going from 280 to 300 will give a different result compared to going from 280 to 260, but then you're really comparing different mean climates. I think this "directionality" of climate sensitivity is not a good concept.

It's not the forcing per se that's the issue here, it's the feedbacks that potentially can alter the climate sensitivity to the sign of the forcing.

It has been suggested in the past that climate sensitivity is larger to cooling perturbations then to warming ones, and we ourselves have found that result in some earlier model runs. The standard reason given is that with a cooling climate perturbation, sea ice can expand further equatorward, to cover a broader area, and intersect more solar radiation - therefore providing a more positive feedback to the cooling. In a warming climate, the sea ice retreats and intersects less radiation - but the sunlight-weighted area is smaller in the regions it is retreating to, so its positive feedback to the warming is not as large.

However - water vapor works the opposite way. Given the exponential dependence of water vapor on temperature, in a warming climate the added temperature would allow for a greater

water vapor change (increase) than would occur with a cooling climate of the same magnitude. Hence the water vapor feedback should be greater in a warming climate.

So the answer is - nobody knows. Jim Hansen did a survey of people at GISS recently to see what the general opinion was for a paper he's working on (and sending around). Since paleoclimates have suffered both positive and negative forcings (in the examples given in this section), and since we don't know the answer to this question, we can't really say whether the sign of the forcing is important or not. So I've left it as an open question, with the possibility that it might matter.

> Relating forcing to response, the sensitivity from the models is then on the order of 0.6°C/ Wm-2 (or higher, depending on the model used); the sensitivity from the observations, if taken at face value, would be considerably less.

> I still don't understand how you get this conclusion. This would mean: if you take models with those estimated forcings and run them, they should show a big mismatch with the proxy data. As far as I can tell from the diagram by Mike Mann attached, combining models and data, only the Von Storch simulation (not shown on this one) does show such a mismatch. (And that uses 1.5 times the Lean solar forcing.)

If you look at the various model simulations done for this time period, the only way the models can reproduce the "observed" cooling relative to the present is by using only a subset of the forcings. When you use all the forcings, you get a much higher number. You can do the math yourself: with a "best-guess" radiative forcing change of 2.4Wm**-2, models with a sensitivity of 0.6C/Wm**-2 will get a temperature change of some 1.5C, which over the course of 300 years shows up in GCMs. For example: Cubasch et al (1997), using just solar forcing in the ECHAM 3 model came up with cooling of 0.5C; if you add a reasonable response to the approximately 1.5-2 W/m**2 forcing from trace gases plus aerosols, you get an additional 1C cooling (given the sensitivity stated above). Counteracting that could be land surface changes - but counteracting that are undoubtedly the reduced pre-industrial tropospheric ozone, plus any additional volcanic cooling (a la Crowley). So assuming those sort of cancel, we have a 1.5C cooling for the MM time period from solar plus anthropogenic, similar to what we get in the GISS model (as noted in our 2004 paper). That can be compared with the Mann et al reconstruction - and you can see from your figure that for the 1700 time period relative to the 1990s, the cooling is about 0.5C. Similarly, Fischer-Bruns et al. (2002) with the ECHAM 4 model, using solar forcing of -0.1% for the MM, and volcanic forcing greater than today (like Crowley) got a cooling of 1.2C. The

Zorita et al study also got a large magnitude cooling when using all the forcings. BTW,
neither ECHAM 3 nor ECHAM 4 has a large climate sensitivity - it is of the order of
0.6C/Wm-2, as referred to in the comment above. Note that none of these models are shown in
your accompanying figure, and all are GCM studies.

How did the Crowley and Bauer studies that are shown in the figure (using EB or EMIC
models) get the smaller cooling magnitudes indicated there? Only by using a subset of the
forcings - Crowley basically threw out the solar changes (and had a lower sensitivity
model), Bauer et al. used a large aerosol effect and still needed a large deforestation
warming to bring her results in line with the Mann et al. reconstruction (in fact, it was
done specifically for that reason). None of these runs used the tropospheric ozone
reduction that we have evidence did occur. My impression is that these studies took the
observations as given and were asking the question of what forcings would be needed to
reproduce them. That is an interesting question, but it obviously does not validate the
observations.

The specific comment you refer to above relates to the discussion in the previous
paragraphs, which detail the radiative forcings and all the different model responses. It
is a fair representation of the current status, however unsettling that is. But in the
current incarnation of this subsection, we do not use it to imply a low climate sensitivity
- we simply say that given the uncertainties in forcing and response, we cannot use this
time period to better understand climate sensitivity. And I think that's accurate.

David

--

//
//

476. 1105661016.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: jto@u.arizona.edu,David Rind <drind@giss.nasa.gov>,
joos@climate.unibe.ch,Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: near final 6.3.2.1
Date: Thu Jan 13 19:03:36 2005

Guys
here is the latest draft of 6.3.2.1 (only waiting on slight edits on ocean bit from Eystein
and ENDNOTE reffs to be sorted. Have agreed with Peck and Eystein to do a Medieval Warm Box

tomorrow and insert a sentence or two on lack of info for SH .Figures of course need work -
particularly sorting out how to represent uncertainty around all reconstructions in Fig 1
and represent totality ion Fig 2d. Also some forcing data still missing - may have to wait
til after ZOD (will also need to put in other borehole curve(s) but data not to hand).
Having virus troubles with by email (and our system randomly blocking some files) - sorry
so don't know whether David has seen this at all (re his comments on Figures - which are
now embedded as GIFs and attached separately as 2 files in case go wrong again.
As I type just got Stefan's message and comments and Goose paper- will look at tonight and
incorporate tomorrow.
David - I know it is received wisdom that volcanos only force climate for 1 to 2 years -
but in our SOAP transient models this is not the case where several large eruptions occur
(co- incidentally often in sunspot minima periods - see the actual magnitude of radiative
forcing in Figure 2 (and these effects are directly transmitted as continually propagating
coolings in ocean in HADCM3 and ECHO-G for up to decades i believe. Anyway - I am happy
with your conclusions and agree that these are not "negative". I would rather just pick a
cool period and not label it as MM (or late MM) as this is a solar
definition as such should be defined according to solar proxy data (and hence choice of
shorter period seems unsupported). If you just say a date range without the label , I think
it avoids the issue.
Sorry for garbled writing but rushing - I like your bit (in case this did not come across)
thanks all for now
Keith

--
Professor Keith Briffa,
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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

477. 1105661725.txt
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From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: comments on Briffa, last millennium
Date: Thu, 13 Jan 2005 19:15:25 +0100
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen

<eystein.jansen@geo.uib.no>

<x-flowed>
Dear Keith,

you've done a great job on the touchy subject of the last millennium,
which is central to our whole chapter.
My comments to that are threefold:
(1) If you could shorten the text somewhat, it could become more powerful
(2) Some small edits & comments are in the attached doc
(3) I propose some improvements to the figures as follows.
- Fig 1a the land temps seem to go off plot, temperature scale needs to
be extended
- we need a break between panels a and the rest, since it's a different
time scale on the x axis
- Fig 1c also has one curve going off the top
- Panels 1b-d might run the time axis up to 2010 or so, else the
important rise at the end is hidden in the tick-marks and less obvious
than it should be
- the legends need to say what the baseline period (zero line of y-axis)
is (hard to find this in the axis label)
- this baseline should be the same for all curves, i.e. 1961-1990. Fig
2d says 1901-1960 - it's not ideal to have a different one, as compared
to Fig 1. Also, is it true? Surely the Storch curve is not shown
relative to this baseline, it's way above it. Aligning it like this
could lead to the dangerous misunderstanding that Storch suggests a much
warmer medieval time compared to everyone else, which of course is not
the case.

I hope this helps.

Cheers, Stefan

--
Stefan Rahmstorf
www.ozean-klima.de
www.realclimate.org

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Attachment Converted: "c:\eudora\attach\goosse_et_al_2005.pdf"

478. 1105663624.txt
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From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Box 6.1: The Medieval Warm Period
Date: Thu, 13 Jan 2005 19:47:04 +0100
Cc: t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen
<eystein.jansen@geo.uib.no>, drind@giss.nasa.gov, Valerie Masson-Delmotte
<Valerie.Masson@cea.fr>, joos <joos@climate.unibe.ch>

<x-flowed>
Hi friends,

good idea for a box. Just want to make sure you're aware of the attached
paper by Goosse et al., which may be helpful in illustrating what we all

know, but what here is shown in a citeable way: local climate variations
are dominated by internal variability (redistribution of heat), only
very large scale averages can be expected to reflect the global forcings
(GHG, solar) over the past millennium.

Stefan

--
Stefan Rahmstorf
www.ozean-klima.de
www.realclimate.org

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479. 1105667593.txt
##

From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: David Rind <drind@giss.nasa.gov>
Subject: Re: 6.5.8 revisions
Date: Thu, 13 Jan 2005 20:53:13 +0100
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Keith
Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>,
FortunatJoos@email.arizona.edu

   Hi folks,
   on the topic of climate sensitivity. I just lost a long mail on it due to a
software crash,
   so sorry if I'm brief now.
   I think it makes no sense for the purpose of the IPCC to discuss a climate
sensitivity to
   orbital forcing - if such a thing can be defined at all. The first-order idea of
orbital
   forcing is that in annual global mean it is almost zero - and in any case the
large effect
   orbital forcing has on climate has very little to do with its global mean value.
Hence,
   we'll confuse people by discussing it in this way, and even citing numbers for
it. For the
   purpose of IPCC, I think climate sensitvity should refer to climate sensitivity
wrt.
   greenhouse gases.
   Also, it is questionable to discuss climate sensitivity for uncoupled models,
especially
   for glacial times - Ganopolski et al. (Nature 1998) have shown that glacial
climate looks
   very different with mixed layer ocean vs. coupled. I think for a 2007 IPCC
report we
   shouldn't be discussing old uncoupled runs when coupled model results are
available. (And
   it is a little odd that the above paper, the first coupled model simulation of
glacial
   climate, cited over 150 times so far, is ignored here in the discussion of the
last glacial
   maximum - if you do a search on the Google Scholar engine for the key words "Last
Glacial
   Maximum", you'll find it's the second-most cited paper on this topic after the
Petit et al.

Vostok data paper.)
I still think it makes no sense to say that climate sensitivity depends on the sign of the
forcing. Talking about greenhouse gases: whether you will do an experiment going from 280
ppm to 300 ppm, or the other way round from 300 ppm to 280 ppm, should give you the same
climate sensitivity. Perhaps you mean that going from 280 to 300 will give a different
result compared to going from 280 to 260, but then you're really comparing different mean
climates. I think this "directionality" of climate sensitivity is not a good concept.

Relating forcing to response, the sensitivity from the models is then on the order of
0.6°C/ Wm^-2 (or higher, depending on the model used); the sensitivity from the
observations, if taken at face value, would be considerably less.

I still don't understand how you get this conclusion. This would mean: if you take models
with those estimated forcings and run them, they should show a big mismatch with the proxy
data. As far as I can tell from the diagram by Mike Mann attached, combining models and
data, only the Von Storch simulation (not shown on this one) does show such a mismatch.
(And that uses 1.5 times the Lean solar forcing.)
Stefan
--
Stefan Rahmstorf
[1]www.ozean-klima.de
[2]www.realclimate.org

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documents\eudora\attach\millennium.jpg"

References

1. http://www.ozean-klima.de/
2. http://www.realclimate.org/

480. 1105670738.txt
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: the new "warm period myths" box
Date: Thu, 13 Jan 2005 21:45:38 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Valerie Masson-Delmotte
<Valerie.Masson@cea.fr>

<x-flowed>
Hi Keith and Tim - since you're off the 6.2.2 hook until Eystein
hangs you back up on it, you have more time to focus on that new Box.
In reading Valerie's Holocene section, I get the sense that I'm not
the only one who would like to deal a mortal blow to the misuse of
supposed warm period terms and myths in the literature. The sceptics
and uninformed love to cite these periods as natural analogs for
current warming too - pure rubbish.

So, pls DO try hard to follow up on my advice provided in previous email. No need to go into details on any but the MWP, but good to mention the others in the same dismissive effort. "Holocene Thermal Maximum" is another one that should only be used with care, and with the explicit knowledge that it was a time-transgressive event totally unlike the recent global warming.

Thanks for doing this on - if you have a cool figure idea, include it.

Best, peck
--
Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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481. 1105723247.txt
##########

From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: David Rind <drind@giss.nasa.gov>
Subject: Re: 6.5.8 revisions
Date: Fri, 14 Jan 2005 12:20:47 +0100
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, FortunatJoos@email.arizona.edu

> Hi David,
> thanks for the detailed response. I'll try to be brief.
> On the orbital forcing you write:
>
> > The point here is that climate can be forced by other factors than simply a global,
> > annual average radiation change, which is the metric now being used.
>
> I think we all agree on this point. My concern is only about how to present it in the
> section. I think that giving a climate sensitivity wrt. global mean orbital forcing is
> confusing to the uninitiated, e.g. your statement in the section:
>
> > This high climate sensitivity (2°C/ Wm^-2) is occurring in an atmospheric model
> > (ECHAM-1) whose sensitivity to doubled CO[2] is about 0.6°C/Wm^-2.
>
> I really think we should not give a number like 2°C/ Wm^-2 as "climate sensitivity" to
> global-mean orbital forcing and contrast it to that to doubled CO2. It gives out the
> message to people that climate sensitivity is all over the place and ill defined.

That's
not the case. Climate sensitivity is a well-defined concept for a globally uniform forcing
like CO2 forcing, but nobody expects any clear relation between the global mean part of
orbital forcing and the climate response.
On uncoupled models:
I agree that for 2xCO2 runs, you will get very similar climate sensitivity with uncoupled
and coupled models, because there is no large change in ocean heat transport between
equilibrium 1x and 2x CO2 states (as confirmed by doing this in coupled models). The mixed
layer boundary condition used in the uncoupled models simply assumes a fixed, prescribed
ocean heat transport, which turns out to be a valid approximation in this case.
My concern was and is specific to the discussion for LGM climate, where this is not a valid
approximation, as we know both from proxy data and from model results that ocean
circulation and heat transport was very likely quite different in the LGM compared to
today. In our Nature 98 LGM simulation, we get 50% difference in the response of the
Northern Hemisphere mean temperature, between the uncoupled "mixed layer" experiment and
the one that includes the ocean model. 50% is a first-order difference, and hence I think
that all the evidence we have today, points to the "constant heat transport" approximation
breaking down when applied to the LGM. The IPCC report should not draw conclusions about
climate sensitivity from LGM experiments that have made this approximation, as I think
those would be hard to defend. I must say I'm starting to get a little concerned about the
chapter discussing 1980s papers for no other apparent reason then them being authored by
Rind, while leaving out important more recent, widely recognised advances in the field.
I attach the Schneider et al. paper I announced earlier, submitted to Science today and
arguable the most comprehense study on deriving climate sensitivity from LGM data
constraints that has been done so far.
On the directionality of the cimate sensitivity:
of course I understand the reasons, the ice feedback and water vapor feedback etc., I've
written about those myself in the past - again this is only a difference in how best to
present the same, undisputed facts. You make the argument that when going to a colder
climate, sensitivity is different from when going to a warmer climate. That is undisputed.
But that in my view has nothing to do with the "direction" of the experiment, but with the
fact that sensitivity in a colder climate is different from sensitivity in a warmer
climate. I explained with the ppm example because I thought that's simple. A
"directionality" would be, if going from 280 to 300 ppm would give a different equilibrium
response compared to going from 300 to 280. But that's not what you're talking about. Your
talking about going from 280 to 260 (say), as compared to going from 280 to 300. That of

course gives different results, because the difference 280-260 applies to a colder climate
than the difference 300-280 (no matter in which "direction" you derive this).
Stefan
--
Stefan Rahmstorf
[1]www.ozean-klima.de
[2]www.realclimate.org

Attachment Converted: "c:\eudora\attach\schneider_jan05.pdf"

References

1. http://www.ozean-klima.de/
2. http://www.realclimate.org/

482. 1105730627.txt
##########

From: David Rind <drind@giss.nasa.gov>
To: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
Subject: Re: 6.5.8 on climate sensitivity and last millennium
Date: Fri, 14 Jan 2005 14:23:47 -0500
Cc: David Rind <drind@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, FortunatJoos@email.arizona.edu

Here are my responses to the comments concerning 6.5.8d. With respect to Stefan's main
concern: I too am sensitive to the possible mis-use of words that appear in a cavalier
manner in the text. I think the way to avoid that is to be as precise as possible about
what is being said. I also feel that hand-waving should be minimized - just because there
are uncertainties, does not mean IPCC will throw up its hands. Thus the attempt to quantify
these numbers are precisely as they will be done in other IPCC chapters. Again, the
responses are in red, and the text alterations (or in this case, some entire text) are in
blue.

I'm not working on this topic myself so I'm by no means an expert. But I am still quite
concerned with the wording in 6.5.8 on the last millennium.
First, to avoid misunderstandings, I would like to suggest again to describe forcings
and climate changes going forward in time, rather than going backwards in time. Even
colleagues here that I discuss it with misunderstand the present version with backwards
reasoning - it leads to phrases like "deforestation warming" (used by David in his last
mail), although deforestation caused cooling - backwards in time you can see this as a
warming, but should you call it "afforestation warming" if you look back in time? I
suggest to use the physical, forwards, time arrow in the discussion.

In all the other sections of 6.5.8 we discuss the temperature change and the

radiative
forcing relative to the present - when it was colder than the present, the temperatures
were indicated to be colder, and the radiative forcing more negative. To alter that for
this section alone would cause added confusion. I have therefore in each case tried to make
it perfectly clear what is being said. In particular, I agree that in the case of
deforestation the terminology does become confusing so the text has been changed to be more
communicative; it now reads,

Warming of 0.35°C due to the existence of vegetation and forests that have since been cut
down was found by (Bauer et al., 2003) ...

The section states:

If one takes mid-range estimates of solar and anthropogenic forcings, and assumes that
volcanic, tropospheric ozone and land albedo changes cancel out, the resulting radiative
forcing change is ~-2.4 Wm-2.

I don't think we should give a "mid-range" of the forcing like this; the assumption that
ozone, land albedo and volcanic changes cancel is hard to justify in any case. For the
forcing we need to give a range in my opinion, not one number. If we give a range, it
will become clear that the forcing is too uncertain for drawing conclusions on climate
sensitivity from this time period.

The problem with giving a maximum range for this time period is the same as giving one for
the 20th century - the inclusion of the potential indirect effects of aerosols means you
can wipe out all climate forcing entirely. It becomes a 'reductio aud absurdum'. The issue
in particular for the Late Maunder Minimum time period, and the specific reason for
including it, is that it potentially says something about SOLAR forcing.

In writing this section, we are not simply doing a core dump of everything people have
done, we are supposed to use our brains to assess the likely situation. Having already
provided the range of uncertainty,we can give a 'best estimate' for the various forcings
that we can use in a meaningful way if we are careful - and which show the importance of
the uncertainty in the solar forcing. I do agree that what existed in the text especially
for the third paragraph needed improvement. Therefore, after several talks with people
here, I've altered (especially) the first and third paragraphs accordingly. Rather than
just stating the conclusion that climate sensitivity can't be well defined, the paragraphs
now show quantitatively that is the case. The specificity, I believe, gives people a real
feeling for the uncertainties, and in the way it is done here, especially the

uncertainty
in the solar forcing and actual climate response. (This rewrite obviates the need for a
direct response to several of Stefan's other comments.)

(d)Last 1000 years

We concentrate here on the Late Maunder Minimum time period in which sunspots were
generally missing (approximately 1675-1715), but outside of the estimated solar irradiance
change, the discussion is applicable for the pre-industrial climate in general. The primary
forcings relative to today are (1) a decrease in various greenhouse gases, with a forcing
of approximately -2.4±0.25 Wm-2 (not including tropospheric ozone changes); (2) reduced
tropospheric sulfate aerosols, whose direct effect is estimated by IPCC (2001) as +0.4±0.3
Wm-2 with an indirect effect ranging from +0.5 to +2 Wm-2 (3) a solar forcing reduction
estimated as ranging from -0.12 to -1.56 Wm-2 (0.05% to 0.65%) ((Hoyt and Schatten,
1993);(Lean, 2000);(Foukal and Milano, 2001); (Reid, 1997)); and (4) volcanic aerosol
forcing either similar to today ((Robertson, 2001)), lower than today ((Robock and Free,
1996)), or higher ((Crowley, 2000)). Large uncertainties therefore exist for all of the
forcings except the trace gas values (again excluding tropospheric ozone). The cooling
effects are offset to small degree by land albedo changes, estimated to contribute +0.4
Wm-2 ((Hansen et al., 1998)). Reduced tropospheric ozone has been estimated to cause an
additional forcing of -0.3 to -0.8Wm-2 (Mickley et al., 2001), while increased
stratospheric ozone produced a positive forcing of -0.09 to -0.25 Wm-2 (IPCC, 2001). If one
takes the most widely used or mid-range estimates of solar (-0.5 Wm-2 from (Lean, 2000))
and anthropogenic forcings (-2.4Wm-2 from reduced trace gases, other than tropospheric
ozone; +0.5 Wm-2 from reduced sulfate aerosols), land albedo changes (0.4 Wm-2), decreased
tropospheric ozone (-0.35 Wm-2 (IPCC, 2001)) and increased stratospheric ozone (+0.15 Wm-2
(IPCC, 2001)), the net radiative forcing for this time is estimated as -2.2 Wm-2. [For this
exercise we ignore the effects of volcanoes, the indirect effects of sulfate aerosols, and
the effects of carbon and organic aerosols.]. Including these additional components (except
for volcanic aerosols for which even the sign of the change is not well know), Hansen
(personal communication) calculates a value close to -2 Wm-2.
How cold was this time period? Different reconstructions (Fig. X1) provide different
estimates of cooling, ranging from -0.45°C ((Mann et al., 1999), annual value for the
Northern Hemisphere), to about -0.7°C ((Esper et al., 2002)for 20-90°N in the growing
season, and (Briffa and Osborne, 2002)(from borehole temperature records). Model studies

(Fig. X2) for this time period have generally employed significant solar reductions (-0.2% to -0.4%), which by themselves have resulted in cooling of about -0.5°C ((Cubasch et al., 1997);(Bauer et al., 2003);(Rind et al., 2004)). Utilizing a forcing of -1.5 to -2 Wm-2 from the combined influence of preindustrial trace gases and aerosols results in additional cooling of about -1 to -1.5°C ((Fischer-Bruns et al., 2002);(Rind et al., 2004);(Zorita et al., 2004)). If volcanic aerosols were actually more extensive during this time period, then additional cooling would arise from this factor as well (on the order of -0.4°C found by (Hegerl et al., 2003)using the (Crowley, 2000)reconstruction). Warming of 0.35°C due to the existence of vegetation and forests that have since been cut down was found by (Bauer et al., 2003), on the same order but of opposite sign to the tropospheric ozone forcing (Mickley et al., 2004). Adding these effects from model simulations together produces a total cooling on the order of -1 to -1.5°C or greater, significantly larger than any of the paleo-estimates. For the ~50 year time period associated with the Maunder Minimum, without large forcing trends, the model results are essentially in radiative balance, and while the influence of past solar variations could still be in acting, in at least one study they were shown to be unimportant (Rind et al., 2004).

The climate sensitivity from the GCMs used for these studies in on the order of 0.6°C/ Wm-2 (or higher, depending on the model used). To calculate the sensitivity from the observations, we first use the estimated forcing of -2.2 Wm-2 and recognize that ~0.85 Wm-2 of this is unresolved (Hansen, personal communication) due to the rapid trace gas changes of the last few decades. Therefore, only 1.35 Wm-2 of the radiative forcing should have been expressed in the system. Were this to have resulted in a temperature change of about -0.5°C (as in the Mann et al reconstruction), it would imply a climate sensitivity of 0.37 Wm-2, i.e. at the low end of the IPCC range for doubled CO2 response. Using the higher estimated cooling of -0.7°C results in a climate sensitivity of 0.52 Wm-2. Alternatively, if the uncertain solar forcing change was at the estimated minimal value (-0.12 Wm-2), then the radiative forcing change would be reduced accordingly, and climate sensitivity for the two reconstructions increases to 0.5 Wm-2 and 0.7 Wm-2 (near 3°C for doubled CO2) respectively, for the different temperature reconstructions. This exercise can be carried on ad infinitum; considering the actual uncertainty in many of the forcings, and in the actual temperature response of the climate system, we conclude that we cannot properly constrain climate sensitivity for this time period (and to some extent the results are similar for other preindustrial time periods compared to the present).

As an aside: if one uses the minimal estimate of solar forcing in the example presented,
one gets a range of temperature response to 2xCO2 of 2-3°C, not too much different from
that concluded in the paper Stefan just sent around (which was 2.5 to 3C).

Then you state the Mann et al. data are 0.5 ºC below the 1990s in the Maunder Minimum. I
can see they are 0.4 ºC below the reference level (I believe this is 1961-1990). The
mean of the 1990s is 0.3 ºC above this level (I calculated this from the Jones data) -
so I find that the Mann data are in fact 0.7 ºC below the 1990s in the MM. The
difference between model expectation for 2.4 W/m2 and the actual found in the Mann data
is almost gone then. Add to that the possibility that the Mann data may somewhat
understimate the variability, and I do not see any significant discrepancy between
models and data, which we should mention and which we could defend as real - even for
"best guess" sensitivity and forcing, let alone considering the uncertainty in those.

The easiest way to see this is to note that the Mann et al reconstruction has the late
1600s slightly warmer than the late 1800s. It is widely acknowledged that the late 1800s
were 0.6C colder than today (taking into account the heat island effect) (and the radiative
forcings, a la IPCC 2001, are all with respect to the 1990s.) That puts the late 1600s at
less than 0.6C colder, close to the value indicated in the text.

David

--

//
//

483. 1105978592.txt
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##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>,
oyvind.paasche@bjerknes.uib.no
Subject: Keith's box
Date: Mon, 17 Jan 2005 11:16:32 -0700

<x-flowed>
Hi all - attached is Keith's MWP box w/ my edits. It reads just great
- much like a big hammer. Nice job.

Please insert after Eystein has had his say. thx, Peck
--
Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

Mail and Fedex Address:

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fax: +1 520 792-8795
http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
</x-flowed>

Attachment Converted: "c:\eudora\attach\MWP-KRBjto.doc"

484. 1106322460.txt
##########

From: Malcolm Hughes <mhughes@ltrr.arizona.edu>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fwd: Your concerns with 2004GL021750 McIntyre
Date: Fri, 21 Jan 2005 10:47:40 -0700
Cc: Tom Wigley <wigley@cgd.ucar.edu>, rbradley@geo.umass.edu, t.osborn@uea.ac.uk, wigley@ucar.edu, phil Jones <p.jones@uea.ac.uk>, keith Briffa <k.briffa@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>
Michael E. Mann wrote:

> Hi Malcolm,
>
> This assumes that the editor/s in question would act in good faith.
> I'm not convinced of this.
>
> I don't believe a response in GRL is warranted in any case. The MM
> claims in question are debunked in other papers that are in press and
> in review elsewhere. I'm not sure that GRL can be seen as an honest
> broker in these debates anymore, and it is probably best to do an end
> run around GRL now where possible. They have published far too many
> deeply flawed contrarian papers in the past year or so. There is no
> possible excuse for them publishing all 3 Douglass papers and the Soon
> et al paper. These were all pure crap.
>
> There appears to be a more fundamental problem w/ GRL now,
> unfortunately...
>
> Mike
>
> At 08:47 PM 1/20/2005, mhughes@ltrr.arizona.edu wrote:
>
>> Mike - I found this sentence in the reply from the GRL
>> Editor-in-Chief to be
>> interesting:
>> "As this manuscript was not written as a Comment, but rather as
>> a full-up scientific manuscript, you would not in general be asked to
>> look it over."
>> Does it not then follow that if you were to challenge their "work" in
>> a "full-
>> up scientific manuscript", but not as a "Comment" it, too, should be
>> reviewed
>> without reference to MM?

>> Maybe the editor-in-chief should be asked if this is the case, or simply
>> challenged by a submission?
>> Cheers, Malcolm
>> Quoting "Michael E. Mann" <mann@virginia.edu>:
>>
>> >
>> >
>> > Thanks Tom,
>> >
>> >
>> > Yeah, basically this is just a heads up to people that something
>> might be
>> > up here. What a shame that would be. It's one thing to lose "Climate
>> > Research". We can't afford to lose GRL. I think it would be
>> > useful if people begin to record their experiences w/ both Saiers and
>> > potentially Mackwell (I don't know him--he would seem to be
>> complicit w/
>> > what is going on here).
>> >
>> >
>> > If there is a clear body of evidence that something is amiss, it
>> could be
>> > taken through the proper channels. I don't that the entire AGU
>> hierarchy
>> > has yet been compromised!
>> >
>> >
>> > The GRL article simply parrots the rejected Nature comment--little
>> > substantial difference that I can see at all.
>> >
>> >
>> > Will keep you all posted of any relevant developments,
>> >
>> >
>> > mike
>> >
>> >
>> > At 04:30 PM 1/20/2005, Tom Wigley wrote:
>> >
>> > Mike,
>> >
>> >
>> >
>> > This is truly awful. GRL has gone downhill rapidly in recent years.
>> > I
>> >
>> > think the decline began before Saiers. I have had some unhelpful
>> >
>> > dealings with him recently with regard to a paper Sarah and I have
>> >
>> > on glaciers -- it was well received by the referees, and so is in
>> > the
>> >
>> > publication pipeline. However, I got the impression that Saiers was
>> >
>> > trying to keep it from being published.
>> >
>> >
>> > Proving bad behavior here is very difficult. If you think that
>> > Saiers
>> >
>> > is in the greenhouse skeptics camp, then, if we can find
>> > documentary

>> >
>> > evidence of this, we could go through official AGU channels to get
>> >
>> > him ousted. Even this would be difficult.
>> >
>> >
>> > How different is the GRL paper from the Nature paper? Did the
>> >
>> > authors counter any of the criticisms? My experience with Douglass
>> >
>> > is that the identical (bar format changes) paper to one previously
>> >
>> > rejected was submitted to GRL.
>> >
>> >
>> > Tom.
>> >
>> > ===============
>> >
>> >
>> > Michael E. Mann wrote:
>> >
>> > Dear All,
>> >
>> >
>> > Just a heads up. Apparently, the contrarians now have an
>> > "in" with GRL. This guy Saiers has a prior connection w/ the
>> > University of Virginia Dept. of Environmental Sciences that causes me
>> > some unease.
>> >
>> >
>> > I think we now know how the various Douglass et al papers w/
>> Michaels and
>> > Singer, the Soon et al paper, and now this one have gotten published in
>> > GRL,
>> >
>> >
>> > Mike
>> >
>> >
>> >
>> >
>> > Subject: Your concerns with
>> > 2004GL021750 McIntyre
>> >
>> > Date: Thu, 20 Jan 2005 14:42:12 -0600
>> >
>> > X-MS-Has-Attach:
>> >
>> > X-MS-TNEF-Correlator:
>> >
>> > Thread-Topic: Your concerns with 2004GL021750 McIntyre
>> >
>> > Thread-Index: AcT/MITTfwM54m4OS32mJvW4BluE+A==
>> >
>> > From: "Mackwell, Stephen"
>> > <mackwell@lpi.usra.edu>
>> >
>> > To:
>> > <mann@virginia.edu>
>> >
>> > Cc: <cjr@egs.uct.ac.za>,
>> > <james.saiers@yale.edu>

>> >
>> > X-OriginalArrivalTime: 20 Jan 2005 20:42:12.0740 (UTC)
>> > FILETIME=[84F55440:01C4FF30]
>> >
>> > X-UVA-Virus-Scanned: by amavisd-new at fork7.mail.virginia.edu
>> >
>> > X-MIME-Autoconverted: from base64 to 8bit by
>> multiproxy.evsc.Virginia.EDU
>> > id j0KKgLO11138
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>> >
>> > Dear Prof. Mann
>> >
>> > In your recent email to Chris Reason, you laid out your concerns that I
>> > presume were the reason for your phone call to me last week. I have
>> > reviewed the manuscript by McIntyre, as well as the reviews. The editor
>> > in this case was Prof. James Saiers. He did note initially that the
>> > manuscript did challenge published work, and so felt the need for an
>> > extensive and thorough review. For that reason, he requested
>> reviews from
>> > 3 knowledgable scientists. All three reviews recommended
>> > publication.
>> >
>> > While I do agree that this manuscript does challenge (somewhat
>> > aggresively) some of your past work, I do not feel that it takes a
>> > particularly harsh tone. On the other hand, I can understand your
>> > reaction. As this manuscript was not written as a Comment, but
>> rather as
>> > a full-up scientific manuscript, you would not in general be asked to
>> > look it over. And I am satisfied by the credentials of the reviewers.
>> > Thus, I do not feel that we have sufficient reason to interfere in the
>> > timely publication of this work.
>> >
>> > However, you are perfectly in your rights to write a Comment, in which
>> > you challenge the authors' arguments and assertions. Should you
>> elect to
>> > do this, your Comment would be provided to them and they would be
>> offered
>> > the chance to write a Reply. Both Comment and Reply would then be
>> > reviewed and published together (if they survived the review process).
>> > Comments are limited to the equivalent of 2 journal pages.
>> >
>> > Regards
>> >
>> > Steve Mackwell
>> >
>> > Editor in Chief, GRL
>> >
>> >
>> >
>> > _____
>> >
>> >
>> > Professor Michael E. Mann
>> >
>> >   Department
>> > of Environmental Sciences, Clark Hall
>> >
>> >
>> > University of Virginia
>> >
>> >
>> > Charlottesville, VA 22903

>> >
>> > _____
>> >
>> > e-mail:
>> > mann@virginia.edu
>> > Phone: (434) 924-7770   FAX: (434) 982-2137
>> >
>> >
>> > http://www.evsc.virginia.edu/faculty/people/mann.shtml
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>   University of Virginia
>   Charlottesville, VA 22903
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> e-mail: mann@virginia.edu   Phone: (434) 924-7770   FAX: (434) 982-2137
>   http://www.evsc.virginia.edu/faculty/people/mann.shtml
>
Hi Mike - of course we shouldn't make that assumption. If the issues are
being dealt with elsewhere in the peer-reviewed literature soon (in time
for IPCC to be aware of them) then there would be no reason for a
riposte in GRL. Even so, it might be worth putting the hypothetical case
to the Editor-in-Chief to test his response. Cheers, Malcolm
</x-flowed>

485. 1106338806.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: FOIA
Date: Fri Jan 21 15:20:06 2005
Cc: Ben Santer <santer1@llnl.gov>

    Tom,

I'll look at what you've said over the weekend re CCSP. I don't know the other panel members. I've not heard any more about it since agreeing a week ago.
As for FOIA Sarah isn't technically employed by UEA and she will likely be paid by Manchester Metropolitan University.
I wouldn't worry about the code. If FOIA does ever get used by anyone, there is also IPR to consider as well. Data is covered by all the agreements we sign with people, so I will be hiding behind them. I'll be passing any requests onto the person at UEA who has been given a post to deal with them.
Cheers
Phil

At 14:35 21/01/2005, Tom Wigley wrote:

Phil,
Thanks for the quick reply.
The leaflet appeared so general, but it was prepared by UEA so they may have simplified things. From their wording, computer code would be covered by the FOIA. My concern was if Sarah is/was still employed by UEA. I guess she could claim that she had only written one tenth of the code and release every tenth line.
Sorry I won't see you, but I will not come up to Norwich until Monday.
Let me fill you in a bit (confidentially). You probably know the panel members. We were concerned that the chair would be a strong person. It is Jerry Mahlman -- about the best possible choice. Richard Smith is the statistician -- also excellent. Dave Randall, too -- very good. As token skeptic there is Dick Lindzen -- but at least he is a smart guy and he does listen. He may raise his paper with Gianitsis that purports to show low climate sensitivity from volcanoes. I will attach our paper that proves otherwise, in press in JGR.
Preparing the report has been a good and bad experience. I think I had the worst task with the Exec. Summ. -- it tied up most of my time for the past 3 months. The good has been the positive interactions between most of the people -- a really excellent bunch. I have been very impressed by Carl Mears and John Lanzante.
At meetings, John Christy has been quite good -- and there were good and positive interactions between John and Roy and the RSS gang that helped clarify a lot. Outside the meeting, in the email world, he has been more of a pain. He has made a lot of useful suggestions for the ExSumm -- but he keeps accusing the AOGCMers of faking their models (not quite as bluntly as this). In the emails there are some very useful exchanges from Jerry Meehl, Ramaswamy and Ben detailing the AOGCM development process. We will be writing a BAMS article on this in the summer -- much of what happens in model development is unknown to the rest of the community. The 'faking' idea prompted me to write a tongue in cheek note -- also attached. As far as I know, John will not raise this particular issue in his dissentin views.
To accommodate dissenting views, the report will have a "dissenters' appendix", with responses. You will get this at some stage -- the deadline for dissenters to produce is Jan 31, and we will not finish our rebuttals until mid Feb. The dissenters are John C, and (far worse) Roger Pielke Sr. All of the rest of us disagree with these persons' dissenting views. Roger has been extremely difficult -- but the details are too complex to put in an email. On the other hand he has made a number of useful contributions to the ExSumm and other chapters. Suffice to say that he has some strange ideas (often to do with the effects of landuse change) that are interesting but still, in my view, speculative -- but testable.
We have yet to see the dissents -- and it would not be ethical for me to say any more than I have already.

Best wishes,
Tom.
Phil Jones wrote:

Tom,
I hope the VTT panel doesn't prove a meeting too many at this time. It is currently scheduled for Feb 23-25 and I only get back from an 8 day workshop in Pune on Feb 20.
The IPCC Chapter with Kevin is now with WGI in Boulder. We did put you down as one of our potential reviewers. Don't know whether you'll have time or whether WGI will select you - regional balance etc.
Next week I'll be in Reading and Exeter, so won'be be in CRU. Have to be at an RMS Awards meeting then something on Reanalysis, then I have to collect some data from the archives in Exeter for a small project we have. It is easier for me to get this than explain to someone how to do it. So I'll miss you - not back till Thursday night.
On the FOI Act there is a little leaflet we have all been sent. It doesn't really clarify what we might have to do re programs or data. Like all things in Britain we will only find out when the first person or organization asks. I wouldn't tell anybody about the FOI Act in Britain. I don't think UEA really knows what's involved.
As you're no longer an employee I would use this argument if anything comes along. I think it is supposed to mainly apply to issues of personal information - references for jobs etc.
Sorry I'll miss you next week. If you're in on Sunday perhaps you could come round to our new house in Wicklewood. Phone number is still the same as 01953 605643. Keith and Sarah know where it is even if they did get lost the first time they came.
Cheers
Phil
At 02:59 21/01/2005, you wrote:

Phil,
Tom Karl told me you will be on the VTT review panel. This is very good news.
Unfortunately I will not be at the meeting on the 23rd -- I will be in midair half way across the Pacific to spend a couple of weeks in Adelaide.
I got a brochure on the FOI Act from UEA. Does this mean that, if someone asks for a computer program we have to give it out?? Can you check this for me (and Sarah).
I will be at CRU next Mon, Tue, Wed in case Sarah did not tell you.
Thanks,
Tom.

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia

Norwich Email p.jones@uea.ac.uk
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University of East Anglia
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NR4 7TJ
UK

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486. 1106346062.txt
##########

From: "PJ Valdes, Geographical Sciences" <P.J.Valdes@bristol.ac.uk>
To: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: EU
Date: Fri, 21 Jan 2005 17:21:02 -0000
Cc: oyvind.paasche@geo.uib.no

<x-flowed>
Keith,

It is purely a matter of resources, and since Simon will be doing the millenial stuff with the Hadley model within IMPRINT, and I think that probably my resources will be best focussed in some of the other work packages. But it is possible and I will try to do it if the opportunity arises.

Cheers
Paul

--On 21 January 2005 17:12 +0000 Keith Briffa <k.briffa@uea.ac.uk> wrote:

> Great Paul
> but I still do not see , if we do get funded, why you can not do some
> runs (in keeping with the wider hemisphere isotope records) that fit with
> your wishes within IMPRINT.
>
> At 15:16 21/01/2005, PJ Valdes, Geographical Sciences wrote:
>> Keith and Eystein,
>>
>> Thanks for your comments. Without modelling MILLENNIUM is a very much
>> weaker project. I admit that I am attracted to doing something with them
>> because I have wanted to get more involved in the last 1000 years, and
>> it would be a good opportunity to run our new isotope enabled version
>> of the Hadley model.
>>
>> However, IMPRINT is a much stronger project overall and and I also
>> prefer the broader range of timescales offered by IMPRINT (although
>> whether we have ended up being too broad is another issue). Given this
>> and the other things discussed, I will decline the offer from Danny
>> Carroll
>>
>> Best Wishes
>> Paul
>>
>> --On 20 January 2005 22:24 +0100 Eystein Jansen

>> <eystein.jansen@geo.uib.no> wrote:
>>
>>> Hi Keith and Paul,
>>>
>>> I think Millennium might be a problem, but if the project does not
>>> employ a hierarchy of models and have a comprehensive modelling
>>> component it is hard to see how it fits the work program of the call.
>>> We disussed this kind of situation in one of our first meetings and
>>> agreed that we on an institutional basis should not be involved in
>>> competing projects, and I think we need to re-emphasise this agreement
>>> in our London meeting. I also gave Valerie the same opinion as some of
>>> the people in her lab had been asked to join the McCarroll proposal
>>> This said, it is clear that we have work to do with Imprint, we need to
>>> scrutinize budgets and the size of the partnership, look at how we best
>>> focus the science and give enough funds to the critical aspects. I do
>>> hope that the Imprint partners remain loyal to the project and that we
>>> keep it as intended: the best paleoscientists in Europe joined
>>> together. Best regards,
>>> Eystein
>>>
>>>
>>>
>>>
>>> At 13:31 +0000 20-01-05, Keith Briffa wrote:
>>>> Paul
>>>> there is no doubt that Danny's project presents
>>>> something of a problem for us. As far as I
>>>> understand ,yes, it and IMPRINT are the only two
>>>> contenders. I know (confidentially) that they
>>>> have been criticised for not having any
>>>> modelling . Danny approached Hans von Storch
>>>> (and presumably others) , but Hans decided not
>>>> to go with them . At the outset of our
>>>> deliberations regarding IMPRINT , we did discuss
>>>> the possibility that we would impose an
>>>> exclusivity clause on participants - asking them
>>>> to agree not to subscribe to any other project
>>>> (I think Rick Battarbee had been involved in
>>>> another project that did this) . Hence at least
>>>> several of us , in the early (HOLCLIM) stage
>>>> agreed to this - but it was never reinstituted
>>>> after the project expanded to its present size.
>>>> Personally , I worry that we are too large and
>>>> possibly could be seen as not focused enough -
>>>> but this is then hard to square with the recent
>>>> referees' comments suggesting our geographic
>>>> scope was too narrow! On paper , I believe the
>>>> whole formulation and partnership of IMPRINT is
>>>> superior to MILLENNIUM , but that did not stop
>>>> me being interested when Danny asked me, some
>>>> time ago , if I would also them. Like you , I do
>>>> not wish to cut off possible fingers in possible
>>>> pies - but I felt that I could not be formally
>>>> included in both .
>>>> The problem is that one has no idea which way
>>>> the anonymous referees will view the judging
>>>> criteria. Surely , in terms of scientific scope
>>>> , our project is superior (though how well it
>>>> ever works and how well we integrate in practise
>>>> is any ones bet).
>>>> The bottom line as I see it is that as only one
>>>> project can be funded , MILLENNIUM should still
>>>> be seen as competition - with you as part of it

>>>> , it would be much stronger competition.
>>>> As for the funding - I know things are
>>>> ill-defined at best at present. I do not think
>>>> anything should be seen as rigid - though we
>>>> certainly have too large a group .
>>>>
>>>> Don't know if this helps
>>>> Keith
>>>>
>>>> At 12:47 20/01/2005, you wrote:
>>>>> Keith,
>>>>>
>>>>> I've just tried to phone you but you were not in your office.
>>>>>
>>>>> I have been contacted by Danny Carroll and
>>>>> invited to join his EU project MILLENNIUM. I
>>>>> gather that this project has also passed the
>>>>> first hurdle and, according to Danny, there are
>>>>> only two such projects so I assume that
>>>>> MILLENNIUM is directly competing against
>>>>> IMPRINT.
>>>>>
>>>>> The modelling he wants me to do is different to
>>>>> anything I will be doing for IMPRINT so there
>>>>> is no scientific reason why I shouldn't say yes
>>>>> to him, and of course it would also allow me to
>>>>> keep a foot in both camps! However there are
>>>>> clear political/strategic issues to consider
>>>>> and I rate IMPRINT higher on my agenda, even
>>>>> though (judging from the IMPRINT indictative
>>>>> money which was very low for Bristol despite
>>>>> having Colin, Sandy and myself involved) it
>>>>> seems likely that the IMPRINT resources will be
>>>>> very limited.
>>>>>
>>>>> Before I respond to him, I wanted to know if
>>>>> you (or anyone else at UEA) are involved in
>>>>> MILLENNIUM. From what I can see, it is very
>>>>> close to your interests. If you are not, was
>>>>> this because you wanted to focus entirely on
>>>>> IMPRINT.
>>>>>
>>>>> Don't misinterpret this email. As I said, I do
>>>>> see IMPRINT higher than MILLENNIUM. However, I
>>>>> would just like more info before deciding how
>>>>> best to respond to Danny.
>>>>>
>>>>> Cheers
>>>>> Paul
>>>>>
>>>>> --
>>>>> Prof. Paul Valdes   Tel: +44 (0) 117 3317 222
>>>>> School of Geographical Sciences Fax: +44 (0) 117 928 7878
>>>>> University of Bristol   Email: P.J.Valdes@bristol.ac.uk
>>>>> University Road   Http: www.bridge.bris.ac.uk
>>>>> Bristol BS8 1SS
>>>>> --
>>>>
>>>> --
>>>> Professor Keith Briffa,
>>>> Climatic Research Unit
>>>> University of East Anglia
>>>> Norwich, NR4 7TJ, U.K.

>>>>
>>>> Phone: +44-1603-593909
>>>> Fax: +44-1603-507784
>>>>
>>>> http://www.cru.uea.ac.uk/cru/people/briffa/
>>>
>>>
>>> --
>>> _____
>>> Eystein Jansen
>>> Professor/Director
>>> Bjerknes Centre for Climate Research and
>>> Dep. of Earth Science, Univ. of Bergen
>>> Allégaten 55
>>> N-5007 Bergen
>>> NORWAY
>>> e-mail: eystein.jansen@geo.uib.no Phone: +47-55-583491 - Home:
>>> +47-55-910661 Fax: +47-55-584330
>>> ----------------------
>>> The Bjerknes Training site offers 3-12 months fellowships to PhD
>>> students More info at: www.bjerknes.uib.no/mcts
>>> --
>>> -- ---
>>>
>>
>>
>>
>> --
>> Prof. Paul Valdes   Tel: +44 (0) 117 3317 222
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>
> --
> Professor Keith Briffa,
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>
>

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Bristol BS8 1SS

</x-flowed>

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##
##########

From: "Stephen Juggins" <Stephen.Juggins@newcastle.ac.uk>
To: "Eystein Jansen" <eystein.jansen@geo.uib.no>, <imprint-ssc@bjerknes.uib.no>
Subject: Imprint vs. Millennium
Date: Fri, 28 Jan 2005 12:53:52 -0000
Cc: <oyvind.paasche@geo.uib.no>, "Erick Larson" <Erick.Larson@fa.uib.no>

Hi Eystein

I received these comments below from our research office. This outlines the Newcastle approach.

In one case at least it is clear that the idea that groups would not join another consortium as agreed by the ssc had not been passed on to partners outside those discussions. To apply this retrospectively could be seen as unfair - this is obviously how Millennium interpret it. One option that would avoid a split and limit any wider damage or bad feeling would be to get partners to sign a confidentiality agreement now. This would restrict or stop the flow of information between consortia, which, after all, is the main cause for concern.

Cheers, Steve

-----Original Message-----
From: Alan Tuck [mailto:Alan.Tuck@newcastle.ac.uk]
Sent: 28 January 2005 11:40
To: Tony Stevenson
Subject: RE: Question on ethics

Sharp practice certainly. Not necessarily unethical I would have thought.

In a number of cases we have been asked by coordinators to sign up to an exclusitivity agreement whereby we will not take part in other consortia who are applying under the same call.

However, we have resisted this saying that we cannot restrict the activities of other academics on the campus, although we have been prepared to sign up to such an agreement that would limit the activities of the particular PI and his/her immediate research group. That way, all of those involved are fully aware of the commitment and its implications. Of course, if they are not happy about this we would not sign up but that in turn would probably mean exclusion from the consortium.

Additionally, and this applies to any collaboration during the preparatory stage, we would recommend that a confidentiality agreement were put in place; this at least would limit the onward transmission of information that could help another grouping.

In this instance I guess that we are where we are.

As it was not established at the outset that a party could only be involved with one group it may be difficult to move to that position now, not so much because of issues with the other Coordinator but more importantly because it could jeopardise ongoing relationships with fellow collaborators who would be made to choose sides. There again, as these are the probably the very parties who have operated as split personalities there is the question of working with them again.

In any event, it may still be sensible to try to implement a confidentiality agreement so that access to information is restricted and not used to help the other consortium's cause.

Of course, there is the other option of possibly joining forces. The result could be an even stronger application.

Alan

Steve Juggins
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University of Newcastle Tel: +44 (0)191 222 8799
Newcastle upon Tyne Fax: +44 (0)191 222 5421
NE1 7RU, UK Mobile: +44 07740054905
http://www.campus.ncl.ac.uk/staff/Stephen.Juggins/

> -----Original Message-----
> From: Tett, Simon [mailto:simon.tett@metoffice.gov.uk]
> Sent: 28 January 2005 09:23
> To: Michael Diepenbroek; simon.tett@metoffice.gov.uk; Eystein
> Jansen; imprint-ssc@bjerknes.uib.no
> Cc: oyvind.paasche@geo.uib.no; Erick Larson
> Subject: RE: [Fwd: URGENT]
>
> One issue to stress in the proposal is that we are trying to
> build a new community. One that units parts of the broad
> paleo community with (part of) the climate modelling community.
> Simon
>
> Dr Simon Tett Managing Scientist, Data development and applications.
> Met Office Hadley Centre (Reading Unit)
> Meteorology Building, University of Reading Reading RG6 6BB
> Tel: +44 (0)118 378 5614 Fax +44 (0)118 378 5615
> Mobex: +44-(0)1392 886886
> E-mail: simon.tett@metoffice.gov.uk http://www.metoffice.gov.uk
> Global climate data sets are available from http://www.hadobs.org
>
>
> -----Original Message-----
> From: Michael Diepenbroek [mailto:mdiepenbroek@pangaea.de]
> Sent: 27 January 2005 17:21
> To: simon.tett@metoffice.gov.uk; 'Eystein Jansen';
> imprint-ssc@bjerknes.uib.no
> Cc: oyvind.paasche@geo.uib.no; 'Erick Larson'
> Subject: AW: [Fwd: URGENT]
>
>
> Simon, a forced merge could definitely happen if the
> commission feels that it is worth to have a paleo IP. The
> other outcome could be that they get the impression that the
> community is devived and thus this IP might fail to have the
> wanted impact. The result could be that there is no IP in the
> end. Michael
>
> Dr. Michael Diepenbroek
> WDC-MARE / PANGAEA - www.pangaea.de
> _____
> MARUM - Institute for Marine Environmental Sciences
> University Bremen
> POP 330 440
> 28359 Bremen
> Phone ++49 421 218-7765, Fax ++49 421 218-9570
> IP Phone ++49 421 57 282 970
> e-mail mdiepenbroek@pangaea.de

> > -----Ursprüngliche Nachricht-----
> > Von: Tett, Simon [mailto:simon.tett@metoffice.gov.uk]
> > Gesendet: Donnerstag, 27. Januar 2005 15:20
> > An: Eystein Jansen; imprint-ssc@bjerknes.uib.no
> > Cc: oyvind.paasche@geo.uib.no; Erick Larson
> > Betreff: RE: [Fwd: URGENT]
> >
> >
> > Hi Eystein,
> >   1) Institutions (assuming they are sufficiently
> controlling)
> > should not be involved in two proposals. It feels unethical
> to me -- a
> > lot of time and effort goes into putting the proposal together.
> Someone
> > doing this is trying to benefit without being sufficiently
> committed.
> >
> > 2) You are right -- we are including this as a condition of
> being part
> > of the Imprint partnership. Institutions could choose to
> drop out of
> > Imprint or Millennium. Note we do need to be somewhat
> pragmatic. There
> > are institutions that we really need.
> >
> > 3) It is only bullying if we have a greater degree of power than
> > Millennium and use that power to punish. For example it would be
> > bullying if I said I would never work with anyone involved in
> > Millennium. As nobody is saying such a thing I think it
> would be crazy
> > to say we are bullying...
> >
> > 4) I talked to my director. He supports my position but notes some
> > nuances. For example if the two projects were competing for
> the same
> > call but had some very different foci. His example was hot
> spots. You
> > could have one proposal about East Europe and another about
> the Med.
> > Their would not be such a direct clash there.
> >
> > to summarise. I think our position should be "you can only
> be in one
> > competing project. Please choose which one.".
> >
> > Eystein it might be worth you taking to Danny -- if only to smooth
> > things over. One possible outcome of the two proposals
> going in is a
> > forced merge. If that happens we need to have reasonable
> relationships.
> >
> > Simon
> >
> > Dr Simon Tett Managing Scientist, Data development and
> applications.
> > Met Office   Hadley Centre (Reading Unit)
> > Meteorology Building, University of Reading Reading RG6 6BB
> > Tel: +44 (0)118 378 5614 Fax +44 (0)118 378 5615
> > Mobex: +44-(0)1392 886886

> > E-mail: simon.tett@metoffice.gov.uk   http://www.metoffice.gov.uk
> > Global climate data sets are available from http://www.hadobs.org
> >
> >
> > -----Original Message-----
> > From: Eystein Jansen [mailto:eystein.jansen@geo.uib.no]
> > Sent: 27 January 2005 12:18
> > To: imprint-ssc@bjerknes.uib.no
> > Cc: oyvind.paasche@geo.uib.no; Erick Larson
> > Subject: [Fwd: URGENT]
> >
> >
> > FYI, see below what happened after Valerie said
> > that LSCE was not going to participate
> > inMillennium.
> > My opinion is as follows:
> >   We should do as planned.
> > We will ask people to choose which project to be
> > part of. My opinion is that it is not ethical to
> > participate in two competing proposals for the
> > same topic. This creates concerns about
> > confidentiality and concerns that proprietary
> > information might be transferred between
> > projects.
> > Most people would see that this is not a good
> > position to be in and see that it creates
> > conflicts of interest.
> > We cannot force anybody to withdraw, but we have
> > the right to decide who is part of our project
> > and the responsible person at each institution
> > have the right to choose whether the institution
> > joins a bid or not.
> > This is not bullying, and we have come across
> > this problem because we have found out about this
> > in our own partner institutions, which of course
> > needs to know which projects they are part of.
> > I donÂ´t think we should force this, it is not
> > worth it, but we should make our point clear, and
> > try to convince those concerns that it is best to
> > choose.
> >
> > Any comments are appreciated.
> >
> > Eystein
> >
> >
> > >Envelope-to: Jansen@geo.uib.no
> > >Date: Thu, 27 Jan 2005 12:52:04 +0100
> > >From: Valerie Masson-Delmotte <Valerie.Masson@cea.fr>
> > >Reply-To: Valerie.Masson@cea.fr
> > >Organization: LSCE
> > >X-Accept-Language: en-us, en
> > >To: Jansen@geo.uib.no
> > >Subject: [Fwd: URGENT]
> > >X-Miltered: at dsm-mail with ID 41F8D587.000 by
> > >Joe's j-chkmail (http://j-chkmail.ensmp.fr)!
> > >X-checked-clean: by exiscan on alf
> > >X-Scanner: 275dbee6d499691adc2db0ba5dbafa18
> > http://tjinfo.uib.no/virus.html
> > >X-UiB-SpamFlag: NO UIB: 1.1 hits, 11.0 required
> > >X-UiB-SpamReport: spamassassin found;
> > >   0.1 -- hvorfor herfra?
> > >   0.2 -- HTML included in message

> > >   0.9 -- Message is 40% to 50% HTML
> > >
> > >Dear Eystein,
> > >
> > >You may have thought that I was more diplomatic
> > >than I really am. Sorry about this trouble and
> > >wishing that it would create no more trouble.
> > >Valerie.
> > >
> > >
> > >Return-Path: <D.McCarroll@swansea.ac.uk>
> > >Received: from nenuphar.saclay.cea.fr (nenuphar.saclay.cea.fr
> > [132.166.192.7])
> > >   by dsm-mail.saclay.cea.fr
> > >(8.12.11/jtpda-5.4) with ESMTP id j0RBlUBU030794
> > >   for <masson@lsce.saclay.cea.fr>; Thu, 27 Jan
> 2005 12:47:30
> > +0100
> > >Received: from araneus.saclay.cea.fr (araneus.saclay.cea.fr
> > [132.166.192.110])
> > >   by nenuphar.saclay.cea.fr
> > >(8.12.10/8.12.10/CEAnet-internes.4.0) with ESMTP
> > >id j0RBlV99004140
> > >   for <masson@lsce.saclay.cea.fr>; Thu, 27 Jan 2005 12:47:31
> +0100
> > (MET)
> > >Received: from sainfoin.extra.cea.fr (unverified) by
> > araneus.saclay.cea.fr
> > > (Content Technologies SMTPRS 4.3.17) with ESMTP
> > >id
> > ><T6ec09f0a1284a6c06e548@araneus.saclay.cea.fr>;
> > > Thu, 27 Jan 2005 12:47:30 +0100
> > >Received: from mhs.swan.ac.uk (mhs.swan.ac.uk [137.44.1.33])
> > >   by sainfoin.extra.cea.fr
> > >(8.12.10/8.12.10/CEAnet-Internet.4.0) with ESMTP
> > >id j0RBlSab008971;
> > >   Thu, 27 Jan 2005 12:47:30 +0100 (MET)
> > >Received: from [137.44.41.18] (helo=ccs-mail1.singleton.swan.ac.uk)
> > >   by mhs.swan.ac.uk with esmtp (Exim 4.43)
> > >   id 1Cu87R-0003P8-PD; Thu, 27 Jan 2005 11:47:25 +0000
> > >Received: by ccs-mail1.singleton.swan.ac.uk with
> > >Internet Mail Service (5.5.2656.59)
> > >   id <DJ8KFQ1Y>; Thu, 27 Jan 2005 11:46:50 -0000
> > >Message-ID:
> > ><840186FCFC231A4980595D19685DDE4A0129CB6D@lsntex3.clyne.swan.ac.u
> > k>
> > >From: "McCarroll D." <D.McCarroll@swansea.ac.uk>
> > >To: William Austin <wena@st-andrews.ac.uk>,
> > >   Anders Rindby
> > >   <anders@coxsys.se>,
> > >   "Andreas J. Kirchhefer"
> > >   <Andreas.Kirchhefer@ib.uit.no>,
> > >   Andreas Luecke <a.luecke@fz-juelich.de>,
> > >   Barbara Wohlfarth <barbara@geo.su.se>,
> > >   Brazdil Rudolf
> > >   <brazdil@sci.muni.cz>,
> > >   Brigitta Ammann <Brigitta.Ammann@ips.unibe.ch>,
> > >   Christian Bigler <christian.bigler@eg.umu.se>,
> > >   Christian Kamenik
> > >   <christian.kamenik@ips.unibe.ch>,
> > >   "Davies Siwan."
> > >   <Siwan.Davies@swansea.ac.uk>,
> > >   Emilia Gutierrez <emgutierrez@ub.edu>,

> > > "Froyd C." <C.Froyd@swansea.ac.uk>,
> > > "Gagen M.H."
> > > <M.H.Gagen@swansea.ac.uk>,
> > > Gerd Helle <g.helle@fz-juelich.de>,
> > > Gudrun Larsen <glare@raunvis.hi.is>,
> > > gunhild rosqvis
> > > <gunhild.rosqvist@natgeo.su.se>,
> > > Hakan Grudd <hakan.grudd@dendrolab.se>,
> > > Hogne Jungner <hogne.jungner@helsinki.fi>,
> > > "J.D.Scourse "
> > > <oss048@bangor.ac.uk>, Jan Esper <esper@wsl.ch>,
> > > Jan Heinemeier
> > > <jh@phys.au.dk>,
> > > Jean-Louis EDOUARD <jean-louis.edouard@univ.u-3mrs.fr>,
> > > John Waterhouse <j.s.waterhouse@apu.ac.uk>,
> > > Jon Eiriksson
> > > <jeir@rhi.hi.is>,
> > > Karen Luise Knudsen <Karenluise.knudsen@geo.au.dk>,
> > > Kerstin Treydte <kerstin.treydte@wsl.ch>, Laia
> > <laiandreu@ub.edu>,
> > > "Leng, Melanie J " <mjl@nigl.nerc.ac.uk>,
> > > "Loader N.J."
> > > <N.J.Loader@swansea.ac.uk>,
> > > "Lotter, prof. dr. A.F."
> > > <A.F.Lotter@bio.uu.nl>,
> > > Margit Schwikowski <margit.schwikowski@psi.ch>,
> > > Markus Leuenberger <leuenberger@climate.unibe.ch>,
> > > Martin Grosjean
> > > <Grosjean@giub.unibe.ch>,
> > > "McCarroll D." <D.McCarroll@swansea.ac.uk>,
> > > Michael Friedrich <michaelf@uni-hohenheim.de>,
> > > Michel Stievenard
> > > <misti@dsm-mail.saclay.cea.fr>,
> > > moira mcmanus
> > > <moira.mcmanus@dmtechnology.co.uk>,
> > > "Niklaus E. Zimmermann "
> > > <nez@wsl.ch>,
> > > OCTAVI PLANELLS CARVAJAL <octaviplanells@ub.edu>,
> > > Paul Dennis <paul.dennis@dmtechnology.co.uk>,
> > > Risto Jalkanen
> > > <Risto.Jalkanen@metla.fi>,
> > > Rob Wilson <rjwilson_dendro@blueyonder.co.uk>,
> > > "Robertson I." <I.Robertson@swansea.ac.uk>,
> > > Saurer Matthias
> > > <matthias.saurer@psi.ch>,
> > > sheila hicks <sheila.hicks@oulu.fi>,
> > > "stefan.Wastegard" <stefan.Wastegard@geo.su.se>,
> > > Tatjana Bottger
> > > <tatjana.boettger@ufz.de>,
> > > Tom Levanic <tom.levanic@gozdis.si>, Tom Levanic
> > <tomle999@volja.net>,
> > > Tomasz Goslar <goslar@radiocarbon.pl>, Ulf Buentgen
> > <buentgen@wsl.ch>,
> > > Valerie Daux <Valerie.Daux@cea.fr>,
> > > Valerie Masson-Delmotte <masson@dsm-mail.saclay.cea.fr>
> > >Subject: URGENT
> > >Date: Thu, 27 Jan 2005 11:46:42 -0000
> > >MIME-Version: 1.0
> > >X-Mailer: Internet Mail Service (5.5.2656.59)
> > >Content-Type: multipart/alternative;
> > >   boundary="----_=_NextPart_001_01C50465.A49F468B"
> > >X-SA-Exim-Mail-From: D.McCarroll@swansea.ac.uk
> > >X-Miltered: at dsm-mail with ID 41F8D4D2.001 by

> > >Joe's j-chkmail (http://j-chkmail.ensmp.fr)!
> > >X-Spam-Checker-Version: SpamAssassin 2.64 (2004-01-11) on
> > dsm-mail.cea.fr
> > >X-Spam-Level: **
> > >X-Spam-Status: No, hits=2.8 required=4.0 tests=BAYES_44,HTML_60_70,
> > >   HTML_MESSAGE,NIGERIAN_SUBJECT1 autolearn=no version=2.64
> > >
> > >27th January
> > >
> > >Dear Millennium partners
> > >
> > >I have been informed by one of our partners that
> > >the other IP proposal (IMPRINT) has decided that
> institutions should
> > >not be in both applications (IMPRINT and MILLENNIUM) and that they
> > >want Millennium partners to choose either one or the
> > >other. I am advised that they may issue a
> > >dictate to this effect very soon.
> > >
> > >It is my view that they have absolutely no right
> > >to do this. The Millennium application is
> > >confidential, and they have no right to ask
> > >anyone if they are part of the proposal or not.
> > >They certainly have no right to dictate that an
> > >institution can only be part of one proposal.
> > >
> > >I suggest that if any of you are contacted by
> > >IMPRINT and asked about Millennium you either
> > >ignore the message or politely tell them that EU
> > >proposals are confidential. They should not be
> > >allowed to bully anyone in this way or to
> > >undermine our project.
> > >
> > >Personally I think that there is absolutely no
> > >problem with institutions or even individuals
> > >being in both projects. The aim of an Integrated
> > >Project is to bring together the best
> > >scientists, so it is not a surprise that the
> > >best scientists appear in more than one
> > >application. If they are forced to choose then
> > >it inevitably means that some of the best groups
> > >will not get funded. That is not in the
> > >interests of the EU or of science.
> > >
> > >I will contact the leaders of IMPRINT today and
> > >try to encourage them to re-think this strategy.
> > > It is not necessary to make the community
> > >divide in this way. If they go ahead I will
> > >immediately contact the Commission and make a
> > >formal complaint.
> > >
> > >Apart from this small problem everything is
> > >going very well and we are on target to produce
> > >a very strong proposal which is realistically
> > >funded. I think that is why we are having this
> > >problem with IMPRINT!
> > >
> > >
> > >If you want to speak to me you can ring me here or at home
> > >
> > >
> > >+44 1792 295845
> > >Home: +44 1792 207556

> > >
> > >With very best wishes
> > >
> > >Danny
> > >
> > >
> > >
> >
> >
> > -- _____
> > Eystein Jansen
> > Professor/Director
> > Bjerknes Centre for Climate Research and
> > Dep. of Earth Science, Univ. of Bergen
> > AllÃ©gaten 55
> > N-5007 Bergen
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> > Fax: +47-55-584330
> > ----------------------
> > The Bjerknes Training site offers 3-12 months fellowships to PhD
> > students More info at: www.bjerknes.uib.no/mcts
> >
> --
> ----------
> ----
>
>
>

488. 1106946949.txt
##
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: dirk.verschuren@gfz-potsdam.de
Subject: Re: Dirk
Date: Fri Jan 28 16:15:49 2005
Cc: Stephen.Juggins@newcastle.ac.uk,Valerie Masson-Delmotte
<masson@lsce.saclay.cea.fr>,eystein.jansen@geo.uib.no, Sandy Tudhope
<sandy.tudhope@ed.ac.uk>,dan.charman@plymouth.ac.uk

   Dear Dirk
   good news re your not dropping out . We are happy to have you and if you can do
what you
   can in the time available this would be good. Valerie and I will send a general
message
   Monday am to all WP1 folk to say what is needed now, but we thought it best to
to get
   back to you straight away re specific points raised in Steve's message.
   First, I hope you will be responsible with Dan (and help from Sandy Tudhope) for
   co-ordinating Task 1.4 of WP1 following the concept as we saw it in the
preliminary
   proposal. Of course you would focus on North African (and north and south of this
area)
   work - on the collection, comparison, integration, interpretation of the high and
lower
   resolution records that relate to hydrology. I see Dan as taking the strain
regarding the
   more Northern areas - with obvious attention to wetlands and Sandy helping with
dynamic

links (and ENSO?). Of course there are other records and there will be a need to restrict
"new" collection/laboratory analyses to very specific , justified (and accepted by SC)
situations , but the high resolution core(s) you told me of would be relevant. I suggest
you think in terms of a person to work on this AND data compilation - perhaps a (cheap)
postdoc for 3 years , and money for internal WP1 meetings - say 250KEuro ?
FOR NOW - we need you to liaise with Dan and Sandy to produce what you can for the Task
1.4(see attached old version of proposal to start from) . We will need a "state of the Art"
Scientific objectives and approach details . Your whole Task 1.4 section can only be 1 page
A4 single spaced max.
AFTER LONG DISCUSSION IN LONDON- it was decided that this task would NOW NOT INCLUDE the
paleoflood work - and Eystein will be communicating with Bennitto to (regretfully) to
inform him that we have had to remove his contribution (please do not contact him until
Eystein has done this). We will not put a specific focus on floods (though of course some
work can be done using existing European flood data), because of Rudolf Brazdil , and we
hope , he will accept to be part of WP1 but put some of his requested funds into WP6 .
Hence you 3 can concentrate more on the concept of large scale hydologic variability
,monsoon changes , north south linkages etc. The problem with ENSO persists. I know you
Sandy want to focus entirely on this, but we could compromise perhaps and you do part this
and part Europe? It was decided that we will (somewhere) include data/model comparison
with US droughts , but this does not require effort on out part other than minor data
compilation of existing records [Eystein, we therefore need to ensure Cook is one of the
associated americans]. We will put together an appendix of preliminary records to be used
in each task - just to show impressive new potential integration (but not a priority for
now).
You do not need to sign any forms officially at this stage - just get approval presumably
from your department internally . If we ever get there, forms will be handled at contract
negotiation.
So get in touch with each other (resend ideas , do not assume your previous emails went to
each other), get exchanging ideas and draft what you can .
ON monday , the specific letter to all people will come round , with requested timeline ,
task, deliverables re budget and precise format of Science writing that we need to assemble
the proposal. Then Valerie and I will have to look at the whole thing in the context of our
total 3.7 M budget.
IT WILL ALL SEEM WORTH IT IN 2006
All the very best Keith and Valerie
Keith's home number is 441953 851013

mobile 0776 9732 685
At 12:37 28/01/2005, Stephen Juggins wrote:

> Hi Keith, copy to Eystein, Oyvind
> Just had a long chat with Dirk. It's OK, he's not in Millenium!
> The reason he was pulling out is over committment this year. Anyway, I managed to persuade him to change his mind - the project won't start until Jan 2006 at the very earliest, so any input won't be needed until next year. He was also unsure what to ask for - I suggested he should cost in a post-doc for 3 years and 2 meetings per year, plus some "data workshops". Keith - can you give him some guidance on costing these so they are in line with what others are asking for. I told him that you would look at the overall budges for WP1 and adjust if necessary to meet the target.
> His only short term problem is revising any text for the proposal - he leaves for Kenya next Thursday. I realised that Eystein has only sent the documents to the ssc people so Keith, can you forward these to Dirk and let him know exactly what you need from him for the text and budgets.
> Finally, Dirk was worried that he wouldn't be able to get any paperwork & signatures from his Uni but as I understood from the meeting yesterday this was not needed. Is this right? If there are any forms to fill in we had better get these to him asap.
> Cheers, Steve
> Steve Juggins
> School of Geography, Politics & Sociology
> University of Newcastle Tel: +44 (0)191 222 8799
> Newcastle upon Tyne Fax: +44 (0)191 222 5421
> NE1 7RU, UK Mobile: +44 07740054905
> [1]http://www.campus.ncl.ac.uk/staff/Stephen.Juggins/

--
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[2]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.campus.ncl.ac.uk/staff/Stephen.Juggins/
2. http://www.cru.uea.ac.uk/cru/people/briffa/

489. 1107191864.txt
##########

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: imprint-ssc@bjerknes.uib.no
Subject: RE:
Date: Mon, 31 Jan 2005 12:17:44 +0100
Cc: mschulz@palmod.uni-bremen.de, stocker@climate.unibe.ch

<x-flowed>
Hi, just for clarification as we continue on the
St.2 proposal (you´ll get the mailing tomrrow
with documents, scheduling etc. as planned).
The merger of ICON into Imprint was discussed

several times in the preparatory phase of Imprint
(before name was decided) in meetings we had in
London early last year. However a number of the
present WP leaders did not take part in these
early deliberations, hence this is the reason for
the lack of a collective memory of the
background.
Reasons for including it:
1. Good science,on a topic of high relevance
(abrupt climate change) focussed and with
emphasis on aspects dealing with preedictability
of such changes rather than mapping out their
distribution and impact (as has been done before).
3. Important to avoid competing proposals within
urope to avoid the paleo-community being
marginalised.

Cheers,
Eystein

At 09:18 +0000 31-01-05, Tett, Simon wrote:
>Hi Rainer,
>   Until our recent meeting in London I was
>not aware of the history and do not recall any
>discussion about blending ICON into the project.
>I expect that is a decision Eystein made.
>However, I am very glad that the work is part of
>the IP. I think it will allow much better
>science to be done.
>
>Simon
>
>Dr Simon Tett Managing Scientist, Data development and applications.
>Met Office   Hadley Centre (Reading Unit)
>Meteorology Building, University of Reading Reading RG6 6BB
>Tel: +44 (0)118 378 5614 Fax +44 (0)118 378 5615
>Mobex: +44-(0)1392 886886
>E-mail: simon.tett@metoffice.gov.uk   http://www.metoffice.gov.uk
>Global climate data sets are available from http://www.hadobs.org
>
>
>-----Original Message-----
>From: rainer.zahn@icrea.es [mailto:rainer.zahn@icrea.es]
>Sent: 31 January 2005 08:45
>To: imprint-ssc@bjerknes.uib.no;
>eystein.jansen@geo.uib.no;
>oyvind.paasche@geo.uib.no; Erick.Larson@fa.uib.no
>Cc: mschulz@palmod.uni-bremen.de; stocker@climate.unibe.ch
>Subject:
>
>
>Simon,
>
>I couldn't agree more on the issue of having the science focussed in
>Imprint. I am surprised though that the background behind having WP3 and
>Task 4.6 in Imprint does not appear to be common knowledge within Imprint.
>Thought the merger has been discussed and agreed upon by the consortium.
>
>We will move forward with our WP and see that we get the Holocene part in
>WP3/4.6 strengthened so as to make fit with the timescales of the rest of
>the planned work.
>

>As a note on the side, you may have noted in the comments of the independent
>assessor that Eystein contracted in for advice that he mentions WP3
>specifically for its clarity and relevance. While I tend to agree I am also
>aware that he probably is not the specialist to assess the issue of
>relevance and significance. Yet, in the WP3 description we are asking a set
>of clear-cut questions, which to me doesn't seem the case for other WPs that
>leave an unforturnate impression of confusion. Beyond the needed scientific
>focus mentioned on several occasions in London and your email, clarity is an
>issue that does not seem to be equally distributed throughout the proposal.
>So as much as I do sympathise with the discussion about the sense or
>non-sense of have WP3 in Imprint, I am convinced to my heart that we need to
>improve profoundly on the quality of our WP descriptions if Imprint is to
>stand a chance of being considered for funding.
>
>Cheers,   Rainer
>
>   Rainer Zahn, Professor de Recerca
>   Institució Catalana de Recerca i Estudis Avançats, ICREA
>i Universitat Autònoma de Barcelona
>   Institut de Ciencia i Tecnologia Ambientals
>   Edifici Cn - Campus UAB
>   E-08193 Bellaterra (Cerdanyola), Spain
>
>   Phone: +34 - 93 581 4219
>   Fax: +34 - 93 581 3331
>   email: rainer.zahn@icrea.es, rainer.zahn@uab.es

--
_____
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Fax: +47-55-584330

</x-flowed>

490. 1107454306.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: For your eyes only
Date: Thu Feb 3 13:11:46 2005

    Mike,
       It would be good to produce future series with and without the long
    instrumental series and maybe the documentary ones as well. The long
    measurements can then be used to validate the low-freq aspects at least
    back to 1750, maybe earlier with the documentary. There are some key
    warm decades (1730s, some in the 16th century) which the Moberg
    reconstruction completely misses and gives the impression that all

years are cold between 1500 and 1750.
Away Feb 6-10 and 12-20 and 22-25 (last in Chicago - on the panel to consider the vertical temp work of CCSP).
Cheers
Phil
Cheers
Phil
At 15:26 02/02/2005, you wrote:

Thanks Phil,
Yes, we've learned out lesson about FTP. We're going to be very careful in the future
what gets put there. Scott really screwed up big time when he established that directory
so that Tim could access the data.
Yeah, there is a freedom of information act in the U.S., and the contrarians are going
to try to use it for all its worth. But there are also intellectual property rights
issues, so it isn't clear how these sorts of things will play out ultimately in the U.S.
I saw the paleo draft (actually I saw an early version, and sent Keith some minor
comments). It looks very good at present--will be interesting to see how they deal w/
the contrarian criticisms--there will be many. I'm hoping they'll stand firm (I believe
they will--I think the chapter has the right sort of personalities for that)...
Will keep you updated on stuff...
talk to you later,
mike
At 09:41 AM 2/2/2005, Phil Jones wrote:

Mike,
I presume congratulations are in order - so congrats etc !
Just sent loads of station data to Scott. Make sure he documents everything better
this time ! And don't leave stuff lying around on ftp sites - you never know who is
trawling
them. The two MMs have been after the CRU station data for years. If they ever hear
there
is a Freedom of Information Act now in the UK, I think I'll delete the file rather than
send
to anyone. Does your similar act in the US force you to respond to enquiries within
20 days? - our does ! The UK works on precedents, so the first request will test it.
We also
have a data protection act, which I will hide behind. Tom Wigley has sent me a worried
email when he heard about it - thought people could ask him for his model code. He
has retired officially from UEA so he can hide behind that. IPR should be relevant
here,
but I can see me getting into an argument with someone at UEA who'll say we must adhere
to it !
Are you planning a complete reworking of your paleo series? Like to be involved if

you are.
Had a quick look at Ch 6 on paleo of AR4. The MWP side bar references Briffa, Bradley,
Mann, Jones, Crowley, Hughes, Diaz - oh and Lamb ! Looks OK, but I can't see it
getting past all the stages in its present form. MM and SB get dismissed. All the
right
emphasis is there, but the wording on occasions will be crucial. I expect this to be
the
main contentious issue in AR4. I expect (hope) that the MSU one will fade away. It
seems
the more the CCSP (the thing Tom Karl is organizing) looks into Christy and Spencer's
series, the more problems/issues they are finding. I might be on the NRC review panel,
so will keep you informed.
Rob van Dorland is an LA on the Radiative Forcing chapter, so he's a paleo expert
by GRL statndards.
Cheers
Phil
At 13:41 02/02/2005, you wrote:

Phil--thought I should let you know that its official now that I'll be moving to Penn
State next Fall.
I'll be in the Meteorology Dept. & Earth and Environmental Systems Institute, and plan
to head up a center for "Earth System History" within the institute. Will keep you
updated,
Mike

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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

491. 1107555812.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Re: [Fwd: Re: Zero order draft of Chapter 3, AR4, IPCC]
Date: Fri Feb 4 17:23:32 2005

Kevin,
I was concerned about splitting too, and suggested as a way of getting through the work a little quicker. Pairs will also work as long as we choose the right ones. Agree we need to separate the major from minor, so anything that can be done there in April will be good.
I suspect the comments from the nominated reviewers will all have to answered in a formal way - as a dry run for the FOD and SOD.
On the figures we need to compare notes on these in a few weeks and assign particular people to them. We both worked with Dave on the set of trends. They may not be perfect, but they are better than some of the others. I think we will need to do more of this. Giving responsibility for a handful to some of the LAs is a possibility. We'll need to give clear instructions though and expect loads of iterations. I can deal with 3.2 with David and the HC if we can agree on what and how we want them. Most of the other sections require much more thought. I'll work on this.
I agree 100% with you on the TC section. This will get scrutinized by many more now. I'll report back on the CCSP review. Apart from Lindzen the panel seem pretty good. So, I'll gauge what the key issues appear to be in the panel's minds. Agree that we shouldn't treat it's conclusions as gospel (otherwise why are we bothering), but treat it as a very very major review article.
Must go home now. Have a good trip back to NZ.
Cheers
Phil
At 16:39 04/02/2005, you wrote:

Phil I tried to attach the ppt with all the figues: but it is too big for your server??
Kevin
-------- Original Message --------
Subject: Re: Zero order draft of Chapter 3, AR4, IPCC
Date: Fri, 04 Feb 2005 09:36:00 -0700
From: Kevin Trenberth [1]<trenbert@cgd.ucar.edu>
To: Phil Jones [2]<p.jones@uea.ac.uk>
References: [3]<42024852.7060406@cgd.ucar.edu>
[4]<6.1.2.0.0.20050204144545.03dd6830@pop.uea.ac.uk>
Hi Phil
Not sure how to handle all this. Recall how it was done for GCOS: I don't think that
worked. The official version requires each comment to have name etc on it so it can be
carved up. The CAs won't do that, so I think we have to treat each CA separately, or at
best broken up by section. I can try to get my admin to work on it if we have clear
guidelines.
I am also concerned about splitting: There are a lot of things that can be done by LAs

working in pairs. In previous IPCCs we broke up into sections. Two people worked on each section in parallel. Lots of things can be done that way. But there are some major things that we have to build a consensus on of all of us. I now have a particular interest in making sure the hurricanes are done well. I also am concerend about the UA-MSU etc and clearly you and I should both be engaged there. So sorting out the fairly minor from major points will be a key task.

I am not taken by our set of figures. If I look at them and try to create a story e.g. by ppt, I think they are lacking. I am attaching the ones I have assembled.

I am away next week in Hawaii at the Chapman conference (AGU). Then I am briefly back and then I am gone and out of touch in New Zealand on personal time 20 Feb to 3 March.

Kevin

Phil Jones wrote:

> Kevin,
> At least two of the CAs have already begun reading the ZOD. I hope your clear message is followed by all the CAs. Glad you sent the pdf and not the doc version. Tracked changes would be a nightmare.
> With all these comments, I presume we'll both assemble all the CA comments. WGI will get comments from our nominated (and their) referee's. I presume WGI will somehow collate these, so for example, all comments on section 3.7 or 3.7.1 will be together. Is there a way we can collate all the CA comments similarly? I guess we can decide this later when some more have come in. I reckon we'll have to split the group in Beijing if we are to get through all the comments in the 3.5 days, so separating them would prove useful. Would an email to WGI be useful to see if they can do it for us? Just a thought !
> As you saw, I've reminded our LAs with responsibility for linking with other chapters look at that chapter as well.
> No chance so far to look at the CCSP (vertical temp trends) - 6 sections each of 40-70 pages !!
> Away from today Feb 6-10 in Madrid (EU project meeting) , 12-20 in Pune (extremes workshop - the last one in the current round, for South Asia) and 22-25 at O'Hare Hilton for the CCSP report.
> Only here 11th and 21st. Should have email contact in Madrid and Chicago, but Pune may be hit and miss. Still, not much need for too much contact at this time.
> I'll give the diagrams and other issues some thought whilst away. Albert will be in Pune.

Have a good few weeks and I hope the Landsea issue has subsided.
Cheers
Phil
At 15:50 03/02/2005, you wrote:

Dear CA
The zero order draft of Chapter 3 of the WG1 IPCC AR4 report is now available. Your contribution has helped us put together this draft, and we thank you very much. However, it is NOT yet the first draft; we recognize that it is incomplete in some places (for instance where some CAs did not come through, or through oversight), and we have not even reviewed it fully ourselves, given the tight timetable. So we are seeking constructive comments and your assistance on developing the first draft. What is most helpful is for you to suggest new text and references, and explicit changes. Not "such and such" is bad or needs fixing. We can not promise to use the new text because there are 60 CAs who may well suggest different things. We also have to limit page numbers, so we especially welcome suggestions for shortening. If you care to rewrite a section more succinctly, then we will gladly consider it. The figures are all preliminary and will be thoroughly examined in Beijing in May, so suggestions of improved or more recent figures are welcomed. We also welcome copies of any papers submitted or referred to.
I am sending this out in two parts. This part has the text attached as a pdf. It is order 1 MB. The second part includes the figures, many in color, and it is 3.7 MB.
We need you comments by 1 April 2005 at the latest. If you prefer to focus only on the section in which your contribution appeared, then that is fine, but you are welcome to comment on other parts as well. If you can not comment or prefer not to for some reason or another, a message to that effect would also be welcomed so we can track responses.
Please send your comments, preferably in word, with your name on each page, and clear identification of section, page and line number or figure number. You may like to make a comment, followed by explicit suggestion for addition or change. Please do justify and argue why the change is needed. Please send comments to Kevin Trenberth and Phil Jones, who will assemble them.

Many thanks for your help
Kevin Trenberth
[5]trenbert@ucar.edu
Phil Jones
[6]p.jones@uea.ac.uk

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****
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References

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2. mailto:p.jones@uea.ac.uk
3. mailto:42024852.7060406@cgd.ucar.edu
4. mailto:6.1.2.0.0.20050204144545.03dd6830@pop.uea.ac.uk
5. mailto:trenbert@ucar.edu
6. mailto:p.jones@uea.ac.uk
7. mailto:trenbert@ucar.edu
8. http://www.cgd.ucar.edu/cas/
9. mailto:p.jones@uea.ac.uk
10. mailto:trenbert@ucar.edu
11. http://www.cgd.ucar.edu/cas/

  12. mailto:trenbert@ucar.edu
  13. http://www.cgd.ucar.edu/cas/

492. 1107899057.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: chris.folland@metoffice.gov.uk
Subject: Fwd: Re: FW: "hockey stock" methodology misleading
Date: Tue Feb 8 16:44:17 2005

     X-Sender: mem6u@multiproxy.evsc.virginia.edu
     X-Mailer: QUALCOMM Windows Eudora Version 6.1.1.1
     Date: Fri, 04 Feb 2005 16:04:57 -0500
     To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu,
             tom crowley <tom@ocean.tamu.edu>, tom crowley <tom@ocean.tamu.edu>,
             mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu,
             Keith Briffa <k.briffa@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>
     From: "Michael E. Mann" <mann@virginia.edu>
     Subject: Fwd: Re: FW: "hockey stock" methodology misleading
     X-UEA-MailScanner-Information: Please contact the ISP for more information
     X-UEA-MailScanner: Found to be clean
     X-UEA-MailScanner-SpamScore: s
     sorry, forgot to attach the paper...
     mike

     Date: Fri, 04 Feb 2005 15:54:15 -0500
     To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, Tom Crowley, Tom
Crowley,
     mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, Keith Briffa
<k.briffa@uea.ac.uk>
     From: "Michael E. Mann" <mann@virginia.edu>
     Subject: Fwd: Re: FW: "hockey stock" methodology misleading

     Date: Fri, 04 Feb 2005 15:52:53 -0500
     To: Andy Revkin <anrevk@nytimes.com>
     From: "Michael E. Mann" <mann@virginia.edu>
     Subject: Re: FW: "hockey stock" methodology misleading
     Hi Andy,
     The McIntyre and McKitrick paper is pure scientific fraud. I think you'll find
this
     reinforced by just about any legitimate scientist in our field you discuss this
with.
     Please see the RealClimate response:
     [1]http://www.realclimate.org/index.php?p=111
     and also:
     [2]http://www.realclimate.org/index.php?p=114
     The Moberg et al paper is at least real science. But there are some real
problems with
     it (you'll want to followup w/ people like Phil Jones for a 2nd opinion).
     While the paper actually reinforces the main conclusion of previous studies (it
also
     finds the late 20th century to be the warmest period of the past two
millennia), it
     challenges various reconstructions
     using tree-ring information (which includes us, but several others such as
Jones et al,
     Crowley, etc). I'm pretty sure, by the way, that a very similar version of the
paper was
     rejected previously by Science. A number of us are therefore very surprised
that Nature

is publishing it, given a number of serious problems:
Their method for combining frequencies is problematic and untested:
A. they only use a handful of records, so there is a potentially large sampling bias.
B. worse, they use different records for high-frequencies and low-frequencies, so the
bias isn't even the same--the reconstruction is apples and oranges.
C. The wavelet method is problematic. We have found in our own work that you cannot
simply combine the content in different at like frequencies, because different proxies
have different signal vs. noise characteristics at different frequencies--for some
records, there century-scale variability is likely to be pure noise. They end up
therfore weighting noise as much as signal. For some of the records used, there are real
age model problems. The timescale isn't known to better than +/- a couple hundred years
in several cases. So when they average these records together, the century-scale
variability is likely to be nonsense.
D. They didn't do statistical verification. This is absolutely essential for such
reconstructions (see e.g. the recent Cook et al and Luterbacher et al papers in
Science). They should have validated their reconstruction against long-instrumental
records, as we and many others have. Without having done so, there is no reason to
believe the reconstruction has any reliability. This is a major problem w/ the paper. It
is complicated by the fact that they don't produce a pattern, but just a hemispheric
mean--that makes it difficult to do a long-term verification. But they don't attempt any
sort of verification at all! There are some decades known to be warm from the available
instrumental records (1730s, some in the 16th century) which the Moberg reconstruction
completely misses--the reconstruction gives the impression that all years are cold
between 1500 and 1750. The reconstruction would almost certainly fail cross-validation
against long instrumental records. If so, it is an unreliable estimate of past changes.
We're surprised the Nature Reviewers didn't catch this.
E. They also didn't validate their method against a model (where I believe it would
likely fail). We have done so w/ our own "hybrid frequency-domain" method that combines
information separately at low and high-frequencies, but taking into account the problem
mentioned above. This is described in:
Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K.,
Jones, P.D., [3]Proxy-based Northern Hemisphere Surface Temperature Reconstructions:
Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, Journal
of Climate, in press (2005).
In work that is provisionally accepted in "Journal of Climate" (draft attached), we show
that our method gives the correct history using noisy "pseudoproxy" records

derived from
a climate model simulation with large past changes in radiative forcing. Moberg et al
have not tested their method in such a manner.
F. They argue selectively for favorable comparison w/ other work:
(1) Esper et al: when authors rescaled the reconstruction using the full instrumental
record (Cook et al, 2004), they found it to be far more similar to Mann et al, Crowley
and Lowery, Jones et al, and the roughly dozen or so other empirical and model estimates
consistent w/ it. Several studies, moreover [see e.g.: Shindell, D.T., Schmidt, G.A.,
Mann, M.E., Faluvegi, G., [4]Dynamic winter climate response to large tropical volcanic
eruptions since 1600, Journal of Geophysical Research, 109, D05104, doi:
10.1029/2003JD004151, 2004.] show that extratropical, land-only summer temperatures,
which Esper et al emphasises, are likely to biased towards greater variability--so its
an apples and oranges comparison anyway.
(2) von Storch et al: There are some well known problems here: (a) their forcing is way
too large (Foukal at al in Science a couple months back indicates maybe 5 times too
large), DKMI uses same model, more conventional forcings, and get half the amplitude and
another paper submitted recently by the Belgium modeling group suggests that some severe
spin-up/initialization problems give the large century-scale swings in the model--these
are not reproducible.
(3) Boreholes: They argue that Boreholes are "physical measurements" but many papers in
the published literature have detailed the various biases in using continental ground
surface temperature to estimate past surface air temperature changes--changing snow
cover gives rise to a potentially huge bias (see e.g. : Mann, M.E., Schmidt, G.A.,
[5]Ground vs. Surface Air Temperature Trends: Implications for Borehole Surface
Temperature Reconstructions,Geophysical Research Letters, 30 (12), 1607, doi:
10.1029/2003GL017170, 2003).
Methods that try to correct for this give smaller amplitude changes from borehole
temperatures:
Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., [6]Optimal
Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of
Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003]
[[7]Correction(Rutherford and Mann, 2004)]
Most reconstructions and model estimates still *sandwich" the Mann et al reconstruction.
See e.g. figure 5 in: Jones, P.D., Mann, M.E., [8]Climate Over Past Millennia, Reviews
of Geophysics, 42, RG2002, doi: 10.1029/2003RG000143, 2004.
Ironically, MM say our 15th century is too cold, while Moberg et al say its too warm.
Hmmm....
To recap, I hope you don't mention MM at all. It really doesn't deserve any additional
publicity. Moberg et al is more deserving of discussion, but, as outlined above, there

are some real problems w/ it. I have reason to believe that Nature's own commentary by
Schiermeier will actually be somewhat critical of it.
I'm travelling and largely unavailable until monday. If you need to talk, you can
possibly reach me at 434-227-6969 over the weekend.
I hope this is of some help. Literally got to run now...
mike
At 02:14 PM 2/4/2005, Andy Revkin wrote:

Hi all,
There is a fascinating paper coming in Nature next week (Moberg of Stockholm Univ., et
al) that uses mix of sediment and tree ring data to get a new view of last 2,000 years.
Very warped hockeystick shaft (centuries-scale variability very large) but still
pronounced 'unusual' 1990's blade.
i'd like your reaction/thoughts for story i'll write for next thursday's Times.
also, is there anything about the GRL paper forthcoming from Mc & Mc that warrants a
response?
I can send you the Nature paper as pdf if you agree not to redistribute it (you know the
embargo rules).
that ok?
thanks for getting in touch!
andy

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[9]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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[10]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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[11]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784
[12]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.realclimate.org/index.php?p=111
2. http://www.realclimate.org/index.php?p=114
3. http://www.realclimate.org/RuthetalJClim2004.pdf
4. ftp://holocene.evsc.virginia.edu/pub/mann/Shindelletal-jgr04.pdf
5. ftp://holocene.evsc.virginia.edu/pub/mann/gissgst03.pdf
6. ftp://holocene.evsc.virginia.edu/pub/mann/borehole-jgr03.pdf
7. http://holocene.evsc.virginia.edu/shared/articles/JGRBoreholeCorrection04.pdf
8. ftp://holocene.evsc.virginia.edu/pub/mann/JonesMannROG04.pdf
9. http://www.evsc.virginia.edu/faculty/people/mann.shtml
10. http://www.evsc.virginia.edu/faculty/people/mann.shtml
11. http://www.evsc.virginia.edu/faculty/people/mann.shtml
12. http://www.cru.uea.ac.uk/cru/people/briffa/

493. 1108248246.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>, "Eugene R" <wahle@alfred.edu>, Scott Rutherford <srutherford@rwu.edu>
Subject: Re:
Date: Sat, 12 Feb 2005 17:44:06 -0500
Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

sorry. text revised yet again. no more changes until I receive comments from everyone.
thanks...
mike
At 12:03 PM 2/11/2005, Phil Jones wrote:

Mike,
Keith and Tim are here next week, but very busy with a proposal to the EU. So you may have to hassle them a bit, or hang on for a week or two.
Nature dragged in the IPCC angle which annoyed me. I tried to explain to him how IPCC works. IPCC won't be discussing this in Beijing in May - except as part of Chapter 6. Hans von Storch will likely regret some of the words he's said.
FYI, just as NCAR have put up a web site to give the whole story re Chris Landseas's 'resignation' from a CA in the atmos. obs. chapter (to help Kevin Trenberth out), KNMI are doing the same re Rob van Dorland and that Dutch magazine. The chief scientist at KNMI has got involved as Rob didn't say the things attributed to him. I'll find out more on this in Pune as a guy from KNMI will be there.
Several other CAs on our chapter pulled out, or just didn't do anything. Their stories never got run.
Dick's report was good and my bit in Nature cam across well.
Say hi to all there and wish Steve well.
Cheers
Phil
At 16:19 11/02/2005, Michael E. Mann wrote:

Phil--thanks, that's great. Really happy to hear that everyone is on board with

this.
I'm at a symposium honoring Steve Schneider out at stanford right now. Lots of folks
here--as I talk this over w/ them, and see Dick Kerr's coverage of this, etc. I realize
its not so bad--I was afraid this would be spun as bolstering the contrarians, but it
hasn't. In large part due to quotes from you and others pointing out that the study
actually reinforces the key conclusions, etc., and the fact Dick Kerr showed Keith and
Tim's plot showing the scattering of multiple reconstructions, etc. which takes the
focus off "Mann" a bit...
Nonetheless, I *am* convinced their methodology is suspect, as the analysis I sent
shows. So I will really appreciate input from Keith, Tim, and you to make sure the
language and wording are appropriate and fair...
I will revise as I get input from various people, with an aim to having this
submission-ready in about 10 days (so you can have one final look after you return, and
before you have to head out again).
looking forward to getting people's comments, feedback, etc.
thanks again,
mike
At 08:05 AM 2/11/2005, Phil Jones wrote:

Mike et al,
I've talked to Keith and Tim here and it seems best if we all come in with you on
this response. What you have done is basically fine. We can discuss specific wording
later.
My problem is that I'm off tomorrow to Pune till Feb 20 and email may be
sporadic or non-existent. So can you discuss revised drafts with Keith and Tim,
but keep me on - lower down as I'm away. I'm here on Feb 21 then off to Chicago
to review the vertical temperature report for the NRC/NAS Feb 22-25.
Keep me on the emails in case email works well in Pune.
Cheers
Phil
At 23:35 10/02/2005, Michael E. Mann wrote:

Dear Caspar, Gene, Scott, Phil,
I am attaching a response I've drafted to the Moberg et al paper (attached for those of
you who haven't seen it). The message is pretty clear and simple--their method
overemphasizes the low-frequency variability. To demonstrate this, I've made use of
stuff from Mann and Jones, and from the Mann/Rutherford/Wahl/Ammann J. Climate letter on
Pseudoproxies. So I would welcome any of you to be co-authors on this--just let me now
if you're interested. I've been in touch w/ Keith (he and Tim are potentially working on
their own independent response--waiting to hear further).
This is a very rough draft, so comments much appreciated.
Looking forward to hearing back,
Mike

mail.2005
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[2]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml
Attachment Converted: "c:\eudora\attach\MobergComment2.doc"

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: "Michael E. Mann" <mann@virginia.edu>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: WSJ
Date: Mon, 14 Feb 2005 11:37:07 -0500
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

A good comparison of all of the reconstruction constructive by William Connelly, which
makes it clear that the take-home point is robust, is available here:
[1]http://en.wikipedia.org/wiki/Image:1000_Year_Temperature_Comparison.png
mike
At 10:58 AM 2/14/2005, Tom Wigley wrote:

> Mike,
> I'm sorry we had no time to talk at Stanford.
> Here is the answer to the LIA bounce back idea ...
> For 20th century warming to be a bounce back, the
> heat must come from somewhere. The only source
> consistent with the bounce back idea is the ocean.
> The Levitus data show that heat has been going INTO
> the ocean, not coming out of it.
> This is really obvious, but I have never seem it stated
> anywhere.
> ----------
> Re WSJ. They say ...
> "Statistician Francis Zwiers of Environment Canada, a government agency,
> says he now agrees that Dr. Mann's statistical method "preferentially
> produces hockey sticks when there are none in the data."
> Dr. Mann, while agreeing that his mathematical method tends to find
> hockey-stick shapes, says this doesn't mean its results in this case are
> wrong. Indeed, Dr. Mann says he can create the same shape from the
> climate data using completely different math techniques."
> -----------------
> It is a bit worrying that Francis agrees with M&M -- but it seems that
> you do too.
> My questions are:
> (1) Do other reconstructions (not including Lonnie Thompson's of course)
> suffer from this standardization problem?
> (2) You have stated that simply averaging the data together gives the
> same result. Has this elementary method been published?
> (2a) I note that the PC1 amplitude time series invariably correlates highly
> with the (non-areally-weighted) 'area average'. So this brings up the issue
> of whether you use some area weighting in your PCA -- as we
> invariably do when doing PCA of gridded data?
> (3) From what I can see without reading their full GRL paper,
> M&M think that the RE statistic has an odd sampling distribution.
> It is easy to show this by Monte Carlo simulation -- have you done
> this (i.e., in the abstract, as a statistical exercise, not for the specific
> case of MBH98, etc.)?
> Tom.

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[2]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://en.wikipedia.org/wiki/Image:1000_Year_Temperature_Comparison.png
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml

495. 1108594561.txt
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From: "Michael E. Mann" <mann@virginia.edu>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>, Stephen H Schneider <shs@stanford.edu>, Tom Wigley <wigley@ucar.edu>, Ben Santer <santer1@llnl.gov>, mann@virginia.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, omichael@Princeton.edu, jmahlman@ucar.edu, k.briffa@uea.ac.uk, p.jones@uea.ac.uk
Subject: Fwd: RE: WSJ article
Date: Wed, 16 Feb 2005 17:56:01 -0500

Interesting that Antonio R. doesn't (or at least claims not to) recognize a lack of balance
in the article.
Please treat this email as confidential. I don't believe that sending a letter to the
editor myself would be the best avenue. But perhaps someone else is interested in pursuing
this?
Mike

Subject: RE: WSJ article
Date: Wed, 16 Feb 2005 17:43:10 -0500
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: WSJ article
Thread-Index: AcUUaIg6ON4Ck5ANQ2OfoGmUOQNsvAAAEqMA
From: "Regalado, Antonio" <Antonio.Regalado@wsj.com>
To: "Michael E. Mann" <mann@virginia.edu>
X-OriginalArrivalTime: 16 Feb 2005 22:43:10.0610 (UTC) FILETIME=[E423A720:01C51478]
X-UVA-Virus-Scanned: by amavisd-new at fork11.mail.virginia.edu

Hi Mike,

On the personal stuff, Id go with your first impressions, rather than the perceptions of
others. This isnt a one-sided story. Anyway, I certainly want to find out who is right
here and so I am open to writing more as the papers come out and the facts become
clearer, just as I have written in the past about the Soon and Balliunias business (p.
A3not bad) and about paleo-climate (p. 1 story in 2002 about Gary Comers funding,
feature story on Lonnie Thompsons melting glaciers), etc. Would it surprise you to
hear that anytime I write a story which seems to favor global warming I am also deluged
by accusations of bias and demands for corrections etc.?
Regarding Moberg, I think the issue you are raising is a question of emphasis and not a
matter for a correction. The specific sentences youre thinking of (Indeed, new research
from Stockholm University on historical temperatures suggests past fluctuations were
nearly twice as great as the hockey stick shows. That could mean the 20th-century jump
isn't quite so anomalous.) seem to me be not only factual but precisely to the point of
what the mainstream of science is discussing vis a vis MBH, which was the topic of that
part of my story. For instance, in the Anderson/Woodhouse commentary that accompanied

Moberg in the same issue of Nature, they too stress the increased variability just as I
did and they make no mention of the late 1990s. And as per my email Monday, my article
does also say that other reconstructions also indicate that the 20^th Century was
unusually warm and that the punch line is the same.
Im sure youre fully sick of writing letters, but this may be right opportunity for a
letter to the editor from you or someone who you can second. The person to send a letter
to is [1]Karen.Pensiero@wsj.com. If you want, CC: me and my editor,
[2]Elyse.tanouye@wsj.com. Or even an editorial on the broader topic of where the
science is at. I can give you the name for who to send an editorial to if you want it.
It is probably worth pointing out that no amount of debate can change the facts buried
in those tree rings, etc..
Yes, I will continue to write about climate. The next topic is impacts. What do you
think is the best story there? Id like to write about current impacts rather than only
projected ones as these will be more tangible for the reader. Also, since the Arctic has
been well covered Id be interested in impacts at lower latitudes.
Antonio

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References

1. mailto:Karen.Pensiero@wsj.com
2. mailto:Elyse.tanouye@wsj.com
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

496. 1109014030.txt
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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>
Subject: Re: B8 - REMINDER
Date: Mon Feb 21 14:27:10 2005
Cc: Valerie.Masson@cea.fr

we need to sort out budget - I have received no response from Eystein re rethinking - can
not judge other WPs but suspect too much going into modelling /simple modelling . We would

rather inflate request now and rethink (with wider evidence) later. We need another million
from other WPs .
Keith
At 14:04 21/02/2005, you wrote:

Dear All,
I still miss the B8 section from
WP1 (Keith)
WP4 (Simon)
WP6 (Eduardo)
WP7 (Johann)
WP8 (Viv)
As you know very well time is running short. Please send me the missing B8 no later than
Wedensday (23 February). If you cannot meet this already overdue deadline please let me
know.
For details, see below.
Cheers,
Øyvind
B.8 Detailed implementation plan - first 18 months
MAX 40 PAGES
This section describes in detail the work planned to achieve the objectives of the
proposed project up to its first 18 months in operation. The recommended length,
excluding the forms specified below, is up to 15 pages. An introduction should explain
the structure of this 18-month detailed implementation plan and how the plan will lead
the participants to achieve the objectives aimed for by that time. It should also
identify significant risks and contingency plans for these. The plan must be broken down
into work packages (WPs) which should follow the logical phases of the project during
this period, and include management of the project and assessment of progress and
results to this point. Essential elements of the plan are:

a) Detailed implementation plan introduction - explaining the structure of this
plan and the overall methodology used to achieve the objectives of the first 18 months.
Include a version of the form A3 which is used in Part A of the proposal, but covering
just the first 18 months
b) Work planning, showing the timing of the different WPs and their tasks (Gantt
chart or similar)
WP and Task leaders: Provide input (Max 4 pages per WP) with detail of plans including
milestones and key deliverables
c) Graphical presentation of the components, showing their interdependencies (Pert
diagram or similar)
d) Detailed work description broken down into work packages: Work package list
(use work package list form below);
Deliverables list (use Deliverables list form below);
Description of each work package (use work package description form below, one per work

package):
Note: The number and structure of work packages used must be appropriate to the complexity of the work and the overall value of the proposed project. Each work package should be a major subdivision of the proposed project and should also have a verifiable end-point (normally a deliverable or an important milestone in the overall project).
The planning should be sufficiently detailed to justify the proposed effort and allow progress monitoring by the Commission - the day-to-day management of the project by the consortium may require a more detailed plan.
--

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[1]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

497. 1109018144.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: Re: Canadians and the Millennium
Date: Mon Feb 21 15:35:44 2005

Mike,
FYI only - here is a reply from Francis. He's still onside, just stuck learning French.
Cheers
Phil

X-Mailer: QUALCOMM Windows Eudora Version 6.2.1.2
Date: Mon, 21 Feb 2005 07:14:34 -0800
To: Phil Jones <p.jones@uea.ac.uk>
From: Francis Zwiers <Francis.Zwiers@ec.gc.ca>
Subject: Re: Canadians and the Millennium
Cc: "francis.zwiers@ec.gc.ca" <francis.zwiers@ec.gc.ca>
X-UEA-MailScanner-Information: Please contact the ISP for more information

X-UEA-MailScanner: Found to be clean
Hi Phil,
At 02:29 21/02/2005, you wrote:

Francis,

Been away for the last week and off again tomorrow for the rest of this week.
I was surprised to see comments from you in WSJ saying that McIntyre and McKittrick were likely right and the Mann reconstruction is wrong. I hope it is
a case of misreporting !

Well, this isn't what I said, and its also not what is reported in the WJS article. The
article quotes me as saying that the technique preferentially produces hockey sticks
(actually, I *think* I said that it preferentially produces PC1s with hockey stick
shapes, but that's a distinction that may have escaped the reporter - or I may have
miss-spoken). In any case, this does not mean that the general form of the
reconstruction (illustrating the unusual nature of the 20th century) is wrong - and I
went to pains in the interview to also make that point.

The nearest composite reconstruction to MM in the 15th century is MBH98. All the others have the 15th century cooler than MBH98. There is no way MM are right in the 15th century. Also Moberg et al (2005) has too much long-term variability.
Sorry for the short email, I have loads of others to go through before the end of today. We can discuss in more detail at Duke !

Unfortunately, I won't be at Duke because I'm still stuck in a particular type of
Canadian purgatory called french training.
Cheers, Francis

Cheers
Phil
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Web: [1]http://www.cccma.bc.ec.gc.ca

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References

1. http://www.cccma.bc.ec.gc.ca/

498. 1109021312.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA
Date: Mon Feb 21 16:28:32 2005
Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>

Mike, Ray and Malcolm,
The skeptics seem to be building up a head of steam here ! Maybe we can use this to our advantage to get the series updated !
Odd idea to update the proxies with satellite estimates of the lower troposphere rather than surface data !. Odder still that they don't realise that Moberg et al used the Jones and Moberg updated series !
Francis Zwiers is till onside. He said that PC1s produce hockey sticks. He stressed that the late 20th century is the warmest of the millennium, but Regaldo didn't bother with that. Also ignored Francis' comment about all the other series looking similar to MBH.
The IPCC comes in for a lot of stick.
Leave it to you to delete as appropriate !
Cheers
Phil
PS I'm getting hassled by a couple of people to release the CRU station temperature data.
Don't any of you three tell anybody that the UK has a Freedom of Information Act !

X-Sender: f023@pop.uea.ac.uk
X-Mailer: QUALCOMM Windows Eudora Version 6.1.0.6
Date: Mon, 21 Feb 2005 15:40:05 +0000
To: p.jones@uea.ac.uk
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

Subject: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA
Date: Mon, 21 Feb 2005 15:02:37 -0000
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: pressure grows on climate modellers to relase secret data
Thread-Index: AcUXiV64e/f3Ii8uQSaOX88pndSQgQAl2O1w
From: "Peiser, Benny" <B.J.Peiser@livjm.ac.uk>

To: "cambridge-conference" <cambridge-conference@livjm.ac.uk>
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
CCNet 22/2005 - 21 February 2005
PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

This should have produced a healthy scientific debate. Instead, Mr. Mann tried to shut down debate by refusing to disclose the mathematical algorithm by which he arrived at his conclusions. All the same, Mr. Mann was forced to publish a retraction of some of his initial data, and doubts about his statistical methods have since grown.
--The Wall Street Journal, 18 February 2005
But maybe we are in that much trouble. The WSJ highlights what Regaldo and McIntyre says is Mann's resistance or outright refusal to provide to inquiring minds his data, all details of his statistical analysis, and his code. So this is what I say to Dr. Mann and others expressing deep concern over peer review: give up your data, methods and code freely and with a smile on your face.
--Kevin Vranes, Science Policy, 18 February 2005
Mann's work doesn't meet that definition [of science], and those who use Mann's curve in their arguments are not making a scientific argument. One of Pournelle's Laws states "You can prove anything if you can make up your data." I will now add another Pournelle's Law: "You can prove anything if you can keep your algorithms secret."
--Jerry Pournelle, 18 February 2005
The time has come to question the IPCC's status as the near-monopoly source of information and advice for its member governments. It is probably futile to propose reform of the present IPCC process. Like most bureaucracies, it has too much momentum and its institutional interests are too strong for anyone realistically to suppose that it can assimilate more diverse points of view, even if more scientists and economists were keen to join up. The rectitude and credibility of the IPCC could be best improved not through reform, but through competition.
--Steven F. Hayward, The American Enterprise Institute, 15 February 2005
(1) HOCKEY STICK ON ICE
The Wall Street Journal, 18 February 2005
(2) SCIENCE AND OPEN ALGORITHMS: "YOU CAN PROVE ANYTHING WITH SECRET DATA AND ALGORITHMS"
Jerry Pournell, 18 February 2005
(3) OPEN SEASON ON HOCKEY AND PEER REVIEW
Science Policy, 18 February 2005
(4) CLIMATE CHANGE SCIENCE: TIME FOR TEAM "B"?
The American Enterprise Institute, 15 February 2005
(5) BRING THE PROXIES UP TO DATE!
Climate Audit, 20 February 2005
(6) CARELESS SCIENCE COSTS LIVES
The Guardian, 18 February 2005
(7) RE: MORE TROUBLE FOR CLIMATE MODELS
Helen Krueger <hkrueger@sbcglobal.net>
(8) HOW TO HANDLE ASTEROID 2004 MN4
Jens Kieffer-Olsen <dstdba@post4.tele.dk>
(9) AND FINALLY: EUROPE FURTHER FALLING BEHIND IN TECHNOLOGY AND RESEARCH
EU Observer, 10 February 2005
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(1) HOCKEY STICK ON ICE

The Wall Street Journal, 18 February 2005

[1]http://online.wsj.com/article_email/0,,SB110869271828758608-IdjeoNmlah4n5yta4GHaqyIm4
,00.html
On Wednesday National Hockey League Commissioner Gary Bettman canceled the season, and
we guess that's a loss. But this week also brought news of something else that's been
put on ice. We're talking about the "hockey stick."
Just so we're clear, this hockey stick isn't a sports implement; it's a scientific
graph. Back in the late 1990s, American geoscientist Michael Mann published a chart that
purported to show average surface temperatures in the Northern Hemisphere over the past
1,000 years. The chart showed relatively minor fluctuations in temperature over the
first 900 years, then a sharp and continuous rise over the past century, giving it a
hockey-stick shape.
Mr. Mann's chart was both a scientific and political sensation. It contradicted a body
of scientific work suggesting a warm period early in the second millennium, followed by
a "Little Ice Age" starting in the 14th century. It also provided some visually
arresting scientific support for the contention that fossil-fuel emissions were the
cause of higher temperatures. Little wonder, then, that Mr. Mann's hockey stick appears
five times in the Intergovernmental Panel on Climate Change's landmark 2001 report on
global warming, which paved the way to this week's global ratification -- sans the U.S.,
Australia and China -- of the Kyoto Protocol.
Yet there were doubts about Mr. Mann's methods and analysis from the start. In 1998,
Willie Soon and Sallie Baliunas of the Harvard-Smithsonian Center for Astrophysics
published a paper in the journal Climate Research, arguing that there really had been a
Medieval warm period. The result: Messrs. Soon and Baliunas were treated as heretics and
six editors at Climate Research were made to resign.
Still, questions persisted. In 2003, Stephen McIntyre, a Toronto minerals consultant and
amateur mathematician, and Ross McKitrick, an economist at Canada's University of
Guelph, jointly published a critique of the hockey stick analysis. Their conclusion: Mr.
Mann's work was riddled with "collation errors, unjustifiable truncations of
extrapolation of source data, obsolete data, geographical location errors, incorrect
calculations of principal components, and other quality control defects." Once these
were corrected, the Medieval warm period showed up again in the data.
This should have produced a healthy scientific debate. Instead, as the Journal's Antonio
Regalado reported Monday, Mr. Mann tried to shut down debate by refusing to disclose the
mathematical algorithm by which he arrived at his conclusions. All the same, Mr. Mann
was forced to publish a retraction of some of his initial data, and doubts

about his
statistical methods have since grown. Statistician Francis Zwiers of Environment Canada
(a government agency) notes that Mr. Mann's method "preferentially produces hockey
sticks when there are none in the data." Other reputable scientists such as Berkeley's
Richard Muller and Hans von Storch of Germany's GKSS Center essentially agree.
We realize this may all seem like so much academic nonsense. Yet if there really was a
Medieval warm period (we draw no conclusions), it would cast some doubt on the
contention that our SUVs and air conditioners, rather than natural causes, are to blame
for apparent global warming.
There is also the not-so-small matter of the politicization of science: If climate
scientists feel their careers might be put at risk by questioning some orthodoxy, the
inevitable result will be bad science. It says something that it took two non-climate
scientists to bring Mr. Mann's errors to light.
But the important point is this: The world is being lobbied to place a huge economic bet
-- as much as $150 billion a year -- on the notion that man-made global warming is real.
Businesses are gearing up, at considerable cost, to deal with a new regulatory
environment; complex carbon-trading schemes are in the making. Shouldn't everyone look
very carefully, and honestly, at the science before we jump off this particular cliff?

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(2) SCIENCE AND OPEN ALGORITHMS: "YOU CAN PROVE ANYTHING WITH SECRET DATA AND ALGORITHMS"
Jerry Pournell, 18 February 2005
[2]http://www.jerrypournelle.com/view/view349.html#hockeystick
Science and Open Algorithms: You can prove anything with secret data and algorithms.
There is a long piece on the global "hockey stick" in today's Wall Street Journal that
explains something I didn't understand: Mann, who generated the "hockey stick" curve
purporting to show that the last century was unique in all recorded history with its
sharp climb in temperature, has released neither the algorithm that generated his curve
nor the data on which it was based.
I had refrained from commenting on the "hockey stick" because I couldn't understand how
it was derived. I've done statistical analysis and prediction from uncertainty much of
my life. My first job in aerospace was as part of the Human Factors and Reliability
Group at Boeing, where we were expected to deal with such matters as predicting
component failures, and deriving maintenance schedules (replace it before it fails, but
not so long before it fails that the costs including the cost of the maintenance crew
and the costs of taking the airplane out of service are prohibitive) and other such
matters. I used to live with Incomplete Gamma Functions and other complex integrals; and
I could not for the life of me understand how Mann derived his famous curve.

Now I know:
he hasn't told anyone. He says that telling people how he generated it would be tantamount to giving in to his critics.
More on this after my walk, but the one thing we may conclude for sure is that this is not science. His curve has been distributed as part of the Canadian government's literature on why Canada supports Kyoto, and is said to have been influential in causing the "Kyoto Consensus" so it is certainly effective propaganda; but IT IS NOT SCIENCE.
Science deals with repeatability and openness. When I took Philosophy of Science from Gustav Bergmann at the University of Iowa a very long time ago, our seminar came to a one-sentence "practical definition" of science: Science is what you can put in a letter to a colleague and he'll get the same results you did. Now I don't claim that as original for it wasn't even me who came up with it in the seminar; but I do claim Bergmann liked that formulation, and it certainly appealed to me, and I haven't seen a better one-sentence practical definition of science. Mann's work doesn't meet that definition, and those who use Mann's curve in their arguments are not making a scientific argument.
One of Pournelle's Laws states "You can prove anything if you can make up your data." I will now add another Pournelle's Law: "You can prove anything if you can keep your algorithms secret."
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(3) OPEN SEASON ON HOCKEY AND PEER REVIEW
Science Policy, 18 February 2005

[3]http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000355open_season_on_hocke.html
By Kevin Vranes
The recent 2/14 WSJ article ("Global Warring..." by Antonio Regaldo) addresses the debate that most readers of this site are well familiar with: the Mann et al. hockey stick. The WSJ is still asking - and trying to answer - the basic questions: hockey stick or no hockey stick? But the background premise of the article, stated explicitly and implicitly throughout, is that it was the hockey stick that led to Kyoto and other climate policy. Is it?
I think it's fair to say that to all of us in the field of climatology, the notion that Kyoto is based on the Mann curve is utter nonsense. If a climatologist, or a policy advisor charged with knowing the science well enough to make astute recommendations to his/her boss, relied solely on the Mann curve to prove definitively the existence of anthropogenic warming, then we're in deeper trouble than anybody realizes. (This is essentially what Stephan Ramstorf writes in a 1/27 RealClimate post.) And although it's

easy to believe that national and international policy can hinge on single graphs, I
hope we give policy makers more credit than that.
But maybe we are in that much trouble. The WSJ highlights what Regaldo and McIntyre says
is Mann's resistance or outright refusal to provide to inquiring minds his data, all
details of his statistical analysis, and his code. The WSJ's anecdotal treatment of the
subject goes toward confirming what I've been hearing for years in climatology circles
about not just Mann, but others collecting original climate data.
As concerns Mann himself, this is especially curious in light of the recent RealClimate
posts (link and link) in which Mann and Gavin Schmidt warn us about peer review and the
limits therein. Their point is essentially that peer review is limited and can be much
less than thorough. One assumes that they are talking about their own work as well as
McIntyre's, although they never state this. Mann and Schmidt go to great lengths in
their post to single out Geophysical Research Letters. Their post then seems a bit
ironic, as GRL is the journal in which the original Mann curve was published (1999, vol
26., issue 6, p. 759), an article which is now receiving much attention as being flawed
and under-reviewed. (For that matter, why does Table 1 in Mann et al. (1999) list many
chronologies in the Southern Hemisphere while the rest of the paper promotes a Northern
Hemisphere reconstruction? Legit or not, it's a confusing aspect of the paper that
should never have made it past peer review.)
Of their take on peer review, I couldn't agree more. In my experience, peer review is
often cursory at best. So this is what I say to Dr. Mann and others expressing deep
concern over peer review: give up your data, methods and code freely and with a smile on
your face. That is real peer review. A 12 year-old hacker prodigy in her grandparents'
basement should have as much opportunity to check your work as a "semi-retired Toronto
minerals consultant." Those without three letters after their name can be every bit as
intellectually qualified, and will likely have the time for careful review that typical
academic reviewers find lacking.
Specious analysis of your work will be borne out by your colleagues, and will enter the
debate with every other original work. Your job is not to prevent your critics from
checking your work and potentially distorting it; your job is to continue to publish
insightful, detailed analyses of the data and let the community decide. You can be part
of the debate without seeming to hinder access to it.
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(4) CLIMATE CHANGE SCIENCE: TIME FOR TEAM "B"?
The American Enterprise Institute, 15 February 2005
[4]http://www.aei.org/publications/pubID.21974/pub_detail.asp

By Steven F. Hayward

The Intergovernmental Panel on Climate Change (IPCC) is currently working on its fourth assessment report. Despite the IPCC's noble intent to generate a scientific consensus, a number of factors have compromised the research and drafting process, assuring that its next assessment report will be just as controversial as previous reports in 1995 and 2001. Efforts to reform this large bureaucratic effort are unlikely to succeed. Perhaps the time has come to consider competition as the means of checking the IPCC's monopoly and generating more reliable climate science.

As the Intergovernmental Panel on Climate Change (IPCC) moves toward the release of its fourth assessment report (fourth AR) in 2007, the case of Chris Landsea offers in microcosm an example of why the IPCC's findings are going to have credibility problems.

Last month Landsea, a climate change scientist with the U.S. National Oceanic and Atmospheric Administration (NOAA), resigned as a participant in the producing the report. Landsea had been a chapter author and reviewer for the IPCC's second assessment report in 1995 and the third in 2001, and he is a leading expert on hurricanes and related extreme weather phenomena. He had signed on with the IPCC to update the state of current knowledge on Atlantic hurricanes for the fourth report. In an open letter, Landsea wrote that he could no longer in good conscience participate in a process that is "being motivated by pre-conceived agendas" and is "scientifically unsound."[1]

Landsea's resignation was prompted by an all too familiar occurrence: The lead author of the fourth AR's chapter on climate observations, Kevin Trenberth, participated in a press conference that warned of increasing hurricane activity as a result of global warming.[2] It is common to hear that man-made global warming represents the "consensus" of science, yet the use of hurricanes and cyclones as a marker of global warming represents a clear-cut case of the consensus being roundly ignored. Both the second and third IPCC assessments concluded that there was no global warming signal found in the hurricane record. Moreover, most climate models predict future warming will have only a small effect--if any--on hurricane strength. "It is beyond me," Landsea wrote, "why my colleagues would utilize the media to push an unsupported agenda that recent hurricane activity has been due to global warming."[3] Landsea's critique goes beyond a fit of pique at the abuse of his area of expertise. The IPCC, he believes, has become thoroughly politicized, and is unresponsive to criticism. "When I have raised my concerns to the IPCC leadership," Landsea wrote, "their response was simply to dismiss

my concerns."[4]
Landsea's frustration is not an isolated experience. MIT physicist Richard Lindzen,
another past IPCC author who is not participating in the fourth report, has written: "My
experiences over the past 16 years have led me to the discouraging conclusion that we
are dealing with the almost insoluble interaction of an iron triangle with an iron rice
bowl." (Lindzen's "iron triangle" consists of activists misusing science to get the
attention of the news media and politicians; the "iron rice bowl" is the parallel
phenomenon where scientists exploit the activists' alarm to increase research funding
and attention for the issue.[5]) And Dr. John Zillman, one of Australia's leading
climate scientists, is another ex-IPCC participant who believes the IPCC has become
"cast more in the model of supporting than informing policy development."[6]
And when the IPCC is not ignoring its responsible critics like Landsea and Lindzen, it
is demonizing them. Not long ago the IPCC's chairman, Dr. Rajendra Pachauri, compared
eco-skeptic Bjorn Lomborg to Hitler. "What is the difference between Lomborg's view of
humanity and Hitler's?" Pachauri asked in a Danish newspaper. "If you were to accept
Lomborg's way of thinking, then maybe what Hitler did was the right thing."[7] Lomborg's
sin was merely to follow the consensus practice of economists in applying a discount to
present costs for future benefits, and comparing the range of outcomes with other world
problems alongside climate change. It is hard to judge what is worse: Pachauri's
appalling judgment in resorting to reductio ad Hitlerum, or his abysmal ignorance of
basic economics. In either case, it is hard to have much confidence in the policy advice
the IPCC might have. [...]
Time for "Team B"?
The time has come to question the IPCC's status as the near-monopoly source of
information and advice for its member governments. It is probably futile to propose
reform of the present IPCC process. Like most bureaucracies, it has too much momentum
and its institutional interests are too strong for anyone realistically to suppose that
it can assimilate more diverse points of view, even if more scientists and economists
were keen to join up. The rectitude and credibility of the IPCC could be best improved
not through reform, but through competition....
FULL PAPER at [5]http://www.aei.org/publications/pubID.21974/pub_detail.asp
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(5) BRING THE PROXIES UP TO DATE!
Climate Audit, 20 February 2005
[6]http://www.climateaudit.org/index.php?p=89#more-89
Steve McIntyre
I will make here a very simple suggestion: if IPCC or others want to use "multiproxy"
reconstructions of world temperature for policy purposes, stop using data

ending in 1980
and bring the proxies up-to-date. Let's see how they perform in the warm 1990s - which
should be an ideal period to show the merit of the proxies. I do not believe that any
responsible policy-maker can base policy, even in part, on the continued use of obsolete
data ending in 1980, when the costs of bringing the data up-to-date is inconsequential
compared to Kyoto costs.
I would appreciate comments on this note as I think that I will pursue the matter with
policymakers.
For example, in Mann's famous hockey stick graph, as presented to policymakers and to
the public, the graph used Mann's reconstruction from proxies up to 1980 and
instrumental temperatures (here, as in other similar studies, using Jones' more lurid
CRU surface history rather than the more moderate increases shown by satellite
measurements). Usually (but not always), a different color is used for the instrumental
portion, but, from a promotional point of view, the juxtaposition of the two series
achieves the desired promotional effect. (In mining promotions, where there is
considerable community experience with promotional graphics and statistics, securities
commission prohibit the adding together of proven ore reserves and inferred ore reserves
- a policy which deserves a little reflection in the context of IPCC studies).
Last week, a brand new multiproxy study by European scientists [Moberg et al., 2005] was
published in Nature. On the very day of publication, I received an email from a
prominent scientist telling me that Mann's hockeystick was yesterday's news, that the
"community" had now "moved on" and so should I. That the "community" had had no
opportunity to verify Moberg's results, however meritorious they may finally appear,
seemed to matter not at all.
If you look at the proxy portion of the new Moberg graphic, you see nothing that would
be problematic for opponents of the hockey stick: it shows a striking Medieval Warm
Period (MWP), a cold Little Ice Age and 20th century warming not quite reaching MWP
levels by 1979, when the proxy portion of the study ends. (I'm in the process of
examining the individual proxies and the Moberg reconstruction is not without its own
imperfections.) In the presentation to the public - see the figure in the Nature article
itself, once again, there is the infamous splice between reconstruction by proxy (up to
1980) and the instrumental record thereafter (once again Jones' CRU record, rather than
the satellite record).
One of the first question that occurs to any civilian becoming familiar with these
studies (and it was one of my first questions) is: what happens to the proxies after
1980? Given the presumed warmth of the 1990s, and especially 1998 (the "warmest year in
the millennium"), you'd think that the proxy values would be off the chart. In effect,

the last 25 years have provided an ideal opportunity to validate the usefulness of proxies and, especially the opportunity to test the confidence intervals of these studies, put forward with such assurance by the multiproxy proponents. What happens to the proxies used in MBH99 or Moberg et al [2005] or Crowley and Lowery [2000] in the 1990s and, especially, 1998?

This question about proxies after 1980 was posed by a civilian to Mann in December at realclimate. Mann replied:

Most reconstructions only extend through about 1980 because the vast majority of tree-ring, coral, and ice core records currently available in the public domain do not extend into the most recent decades. While paleoclimatologists are attempting to update many important proxy records to the present, this is a costly, and labor-intensive activity, often requiring expensive field campaigns that involve traveling with heavy equipment to difficult-to-reach locations (such as high-elevation or remote polar sites). For historical reasons, many of the important records were obtained in the 1970s and 1980s and have yet to be updated. [my bold]

Pause and think about this response. Think about the costs of Kyoto and then think again about this answer. Think about the billions spent on climate research and then try to explain to me why we need to rely on "important records" obtained in the 1970s. Far more money has been spent on climate research in the last decade than in the 1970s. Why are we still relying on obsolete proxy data?

As someone with actual experience in the mineral exploration business, which also involves "expensive field campaigns that involve traveling with heavy equipment to difficult-to-reach locations", I can assure readers that Mann's response cannot be justified and is an embarrassment to the paleoclimate community. The more that I think about it, the more outrageous is both the comment itself and the fact that no one seems to have picked up on it.

It is even more outrageous when you look in detail at what is actually involved in collecting the proxy data used in the medieval period in the key multiproxy studies. The number of proxies used in MBH99 is from fewer than 40 sites (28 tree ring sites being U.S. tree ring sites represented in 3 principal component series).

As to the time needed to update some of these tree ring sites, here is an excerpt from Lamarche et al. [1984] on the collection of key tree ring cores from Sheep Mountain and Campito Mountain, which are the most important indicators in the MBH reconstruction:

"D.A.G. [Graybill] and M.R.R. [Rose] collected tree ring samples at 3325 m on Mount Jefferson, Toquima Range, Nevada and 11 August 1981. D.A.G. and M.R.R.

collected samples
from 13 trees at Campito Mountain (3400 m) and from 15 trees at Sheep Mountain (3500 m)
on 31 October 1983."
Now to get to Campito Mountain and Sheep Mountain, they had to get to Bishop, California, which is hardly "remote" even by Paris Hilton standards, and then proceed by road to within a few hundred meters of the site, perhaps proceeding for some portion of the journey on unpaved roads.
The picture below illustrates the taking of a tree ring core. While the equipment may seem "heavy" to someone used only to desk work using computers, people in the mineral exploration business would not regard this drill as being especially "heavy" and I believe that people capable of operating such heavy equipment can be found, even in out-of-the way places like Bishop, California. I apologize for the tone here, but it is impossible for me not to be facetious.
There is only one relatively remote site in the entire MBH99 roster - the Quelccaya glacier in Peru. Here, fortunately, the work is already done (although, needless to say, it is not published.) This information was updated in 2003 by Lonnie Thompson and should be adequate to update these series. With sufficient pressure from the U.S. National Science Foundation, the data should be available expeditiously. (Given that Thompson has not archived data from Dunde drilled in 1987, the need for pressure should not be under-estimated.)
I realize that the rings need to be measured and that the field work is only a portion of the effort involved. But updating 28 tree ring sites in the United States is not a monumental enterprise nor would updating any of the other sites.
I've looked through lists of the proxies used in Jones et al. [1998], MBH99, Crowley and Lowery [2000], Mann and Jones [2003], Moberg et al [2005] and see no obstacles to bringing all these proxies up to date. The only sites that might take a little extra time would be updating the Himalayan ice cores. Even here, it's possible that taking very short cores or even pits would prove adequate for an update and this might prove easier than one might be think. Be that as it may, any delays in updating the most complicated location should not deter updating all the other locations.
As far as I'm concerned, this should be the first order of business for multiproxy studies.
Whose responsibility is this? While the costs are trivial in the scheme of Kyoto, they would still be a significant line item in the budget of a university department. I think that the responsibility here lies with the U.S. National Science Foundation and its equivalents in Canada and Europe. The responsibilities for collecting the proxy updates

could be divided up in a couple of emails and budgets established.
One other important aspect: right now the funding agencies fund academics to do the work
and are completely ineffective in ensuring prompt reporting. At best, academic practice
will tie up reporting of results until the publication of articles in an academic
journals, creating a delay right at the start. Even then, in cases like Thompson or
Jacoby, to whom I've referred elsewhere, the data may never be archived or only after
decades in the hands of the originator.
So here I would propose something more like what happens in a mineral exploration
program. When a company has drill results, it has to publish them through a press
release. It can't wait for academic reports or for its geologists to spin the results.
There's lots of time to spin afterwards. Good or bad - the results have to be made
public. The company has a little discretion so that it can release drill holes in
bunches and not every single drill hole, but the discretion can't build up too much
during an important program. Here I would insist that the proxy results be archived as
soon as they are produced - the academic reports and spin can come later. Since all
these sites have already been published, people are used to the proxies and the updates
will to a considerable extend speak for themselves.
What would I expect from such studies? Drill programs are usually a surprise and maybe
there's one here. My hunch is that the classic proxies will not show anywhere near as
"loud" a signal in the 1990s as is needed to make statements comparing the 1990s to the
Medieval Warm Period with any confidence at all. I've not surveyed proxies in the 1990s
(nor to my knowledge has anyone else), but I've started to look and many do not show the
expected "loud" signal e.g. some of the proxies posted up on this site such as Alaskan
tree rings, TTHH ring widths, and theories are starting to develop. But the discussions
so far do not explicit point out the effect of signal failure on the multiproxy
reconstruction project.
But this is only a hunch and the evidence could be otherwise. The point is this: there's
no need to speculate any further. It's time to bring the classic proxies up to date.
==========
(6) CARELESS SCIENCE COSTS LIVES
The Guardian, 18 February 2005
[7]http://www.guardian.co.uk/comment/story/0,3604,1417224,00.html
Dick Taverne
In science, as in much of life, it is believed that you get what you pay for. According
to opinion polls, people do not trust scientists who work for industry because they only
care about profits, or government scientists because they suspect them of trying to
cover up the truth. Scientists who work for environmental NGOs are more highly

regarded.
Because they are trying to save the planet, people are ready to believe that what they say must be true. A House of Lords report, Science and Society, published in 2000, agreed that motives matter. It argued that science and scientists are not value-free, and therefore that scientists would command more trust "if they openly declare the values that underpin their work".
It all sounds very plausible, but mostly it is wrong. Scientists with the best of motives can produce bad science, just as scientists whose motives may be considered suspect can produce good science. An obvious example of the first was Rachel Carson, who, if not the patron saint, was at least the founding mother of modern environmentalism. Her book The Silent Spring was an inspiring account of the damage caused to our natural environment by the reckless spraying of pesticides, especially DDT.
However, Carson also claimed that DDT caused cancer and liver damage, claims for which there is no evidence but which led to an effective worldwide ban on the use of DDT that is proving disastrous. Her motives were pure; the science was wrong. DDT is the most effective agent ever invented for preventing insect-borne disease, which, according to the US National Academy of Sciences and the WHO, prevented over 50 million human deaths from malaria in about two decades. Although there is no evidence that DDT harms human health, some NGOs still demand a worldwide ban for that reason. Careless science cost lives.
Contrast the benefits that have resulted from the profit motive, a motive that is held to be suspect by the public. Multinationals, chief villains in the demonology of contemporary anti-capitalists, have developed antibiotics, vaccines that have eradicated many diseases like smallpox and polio, genetically modified insulin for diabetics, and plants such as GM insect-resistant cotton that have reduced the need for pesticides and so increased the income and improved the health of millions of small cotton farmers. The fact is that self-interest can benefit the public as effectively as philanthropy.
Motives are not irrelevant, and unselfish motives are rightly admired more than selfish ones. There are numerous examples of misconduct by big companies, and we should examine their claims critically and provide effective regulation to control abuses of power and ensure the safety of their products. Equally, we should not uncritically accept the claims of those who act from idealistic motives. NGOs inspired by the noble cause of protecting our environment often become careless about evidence and exaggerate risks to

attract attention (and funds). Although every leading scientific academy has concluded
that GM crops are at least as safe as conventional foods, this does not stop Greenpeace
reiterating claims about the dangers of "Frankenfoods". Stephen Schneider, a
climatologist, publicly justified distortion of evidence: "Because we are not just
scientists but human beings as well ... we need to ... capture the public imagination
... So we have to offer up scary scenarios, make simplified dramatic statements, and
make little mention of any doubts we have."
But in the end motives are irrelevant to the validity of science. It does not matter if
a scientist wants to help mankind, get a new grant, win a Nobel prize or increase the
profits of her company. It does not matter whether a researcher works for Monsanto or
for Greenpeace. Results are no more to be trusted if the researcher declares his values
and confesses that he beats his wife, believes in God, or is an Arsenal supporter. What
matters is that the work has been peer-reviewed, that the findings are reproducible and
that they last. If they do, they are good science. If not, not. Science itself is
value-free. There are objective truths in science. We can now regard it as a fact that
the Earth goes rounds the sun and that Darwinism explains the evolution of species.
A look at the history of science makes it evident how irrelevant the values of
scientists are. Newton's passion for alchemy did not invalidate his discovery of the
laws of gravitation. To quote Professor Fox of Rutger's University: "How was it relevant
to Mendel's findings about peas that he was a white, European monk? They would have been
just as valid if Mendel had been a Spanish-speaking, lesbian atheist."
· Lord Taverne is chair of Sense About Science and author of The March of Unreason, to
be published next month

========== LETTERS =========
(7) RE: MORE TROUBLE FOR CLIMATE MODELS
Helen Krueger <hkrueger@sbcglobal.net>
Dear Dr. Peiser,
I just want to let you know how much I am enjoying being included in your list so that I
can benefit from your astute handling of alarmist information personally and with my
students.
Thank you so much!
Regards,
Helen A. Krueger
Educational Consultant
Phone: 203-426-8043
FAX: 203-426-3541
==========
(8) HOW TO HANDLE ASTEROID 2004 MN4
Jens Kieffer-Olsen <dstdba@post4.tele.dk>
Dear Benny Peiser,
In CCNet 18/2005 - 11 February 2005 you brought an
interesting article on the possible breakup of

NEA 2004 MN4 in the year 2029:
> But there's another reason for concern. According to Dan
> Durda, another SWRI astronomer, 2004 MN4 is likely to be
> a "rubble-pile" asteroid, consisting of material only
> loosely held together by gravity. Because the asteroid
> will pass us at just 2.5 times Earth's diameter, tidal
> forces could tear it apart. The result would be a trail
> of rocks drifting slowly apart with the passage of time.
> One or more of these might hit Earth in the more distant
> future, creating a spectacular fireball as it burns up
> in the atmosphere.
> --Bill Cooke, Astronomy Magazine, 10 February 2005
First of all, a 300m asteroid could break into 100 pieces each larger than the Tunguska impactor. Secondly, the years for which a TS rating of 1 already exist for the object are NOT in the distant future, but 6, 7, and 8 years later. That reminds us that neither the Torino nor the Palermo scale takes into account the possibility of such a MIRV'ed approach. Furthermore, the Palermo scale is designed to take into account the lead time. Even if 2004 MN4 were not to break up, the lead time to virtual impact in 2029 would be down to one sixth of the time to-day. In other words, if the post-2029 orbit is not being resolved before then, we may as well up the PS rating accordingly. If my math is correct, we should add 0.78 to its Palermo Scale rating, ie. log10(6), for a total of -0.65.
Yours sincerely
Jens Kieffer-Olsen, M.Sc.(Elec.Eng.)
Slagelse, Denmark
==========
(9) AND FINALLY: EUROPE FURTHER FALLING BEHIND IN TECHNOLOGY AND RESEARCH
EU Observer, 10 February 2005
[8]http://www.euobserver.com/?aid=18382&print=1
By Lucia Kubosova
BRUSSELS / EUOBSERVER - Europeans are still failing to show world leadership in technology and research, a new report shows.
The paper, published on Thursday (10 February) has evaluated the EU research and development programmes and their impact on Europe's knowledge-base and potential for innovation.
While it argues that EU funds for the programmes make a "major contribution", it suggests that more resources, industry participation and simplified administration are needed for them to have a greater effect in future.
"We have somehow lost momentum", said Erkki Ormala, chair of the panel issuing the report.
"The EU is falling behind. And we are now under pressue not only compared to our traditional rivals like the US or Japan, but also China, India or Brazil. We are facing a much tougher competition in talent and knowledge than we are used to".
Research Commissioner Janez Potocnik considers the paper's results as a reason for doubling the funds in his portfolio within the next budgetary period of 2007-2013.
"We don't want to achieve our economic growth by lowering the social or environmental standards. So to compete globally, we need to focus on knowledge", Mr Potocnik said to journalists, adding that the EU programmes should "make a bridge between

practical
innovation and research".
The report has listed several possible solutions for tackling outlined setbacks.
It argues that the EU must attract and reward the best talent, mobilise resources for
innovation and boost cooperation between governments, businesses and universities in
research.
It supports the idea of setting up a European Research Council to promote excellence and
encourages more industry involvement, mainly on the part of small and medium-sized
enterprises (SMEs).
However, SME representatives complain that their ideas about EU research and innovation
funding are not taken into consideration.
"It's not about how big the budget is for SMEs and their involvement in such projects.
It is rather about the allocation of the funds. Most of them are granted for huge
long-term projects which cost millions of euro and they can hardly attract smaller
companies", according to Ullrich Schroeder, from UEAPME, the main umbrella organisation.
He argues that while several reports have already pointed out that SMEs must be more
involved if the "Lisbon agenda" goal of 3 percent of GDP to be invested in research and
development in the EU by 2010 is to be achieved, in reality they are not as well
supported as huge transnational companies.
"It is not that the EU member states invest much less in universities than the US, but
the greatest difference is that European SMEs are only investing 8% of the US amount,
and it is simply not enough".
Mr Schroeder also said that while "there is a lot of rhetoric from politicians, that the
SMEs should get involved, innovate and compete, when they come up with good projects,
they are not sufficiently supported".
"The European Commission is more concerned about big companies and hightech areas, while
innovation is needed also in more down-to earth sectors", Mr Schroeder told the EUobserver.

--
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8. http://www.euobserver.com/?aid=18382&print=1
9. http://www.cru.uea.ac.uk/cru/people/briffa/

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From: Valerie Masson-Delmotte <Valerie.Masson@cea.fr>
To: Hugues Goosse <hgs@astr.ucl.ac.be>
Subject: Re: B parts
Date: Tue, 22 Feb 2005 10:53:29 +0100
Reply-to: Valerie.Masson@cea.fr
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, imprint-ssc@bjerknes.uib.no, erick.larson@fa.uib.no, Beatriz Balino <beatriz.balino@bjerknes.uib.no>, loutre@astr.ucl.ac.be, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
Dear Eystein,

Congratulations for a very convincing draft.

Please find attached the suggestions by Hubertus Fischer and myself for the parts B1 to B3.

Valerie.

</x-flowed>

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500. 1109267110.txt
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From: "Michael E. Mann" <mann@virginia.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re:
Date: Thu, 24 Feb 2005 12:45:10 -0500
Cc: Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>, "Wahl, Eugene R" <wahle@alfred.edu>, Scott Rutherford <srutherford@rwu.edu>

Thanks Keith,
I've made these changes and a few very minor changes just to improve the grammar in places,
etc. Also, I'm embarassed to say that Scott's name was accidentally left out of the author
list, so I've included that back in.
There was one bit about the high-pass filtering and low-pass filtering which you changed,
based on I think some minor confusion about what I meant. I've fixed that.
I'm assuming that Tim will be ok w/ the attached, final version, so I'm going to go ahead
and submit to Nature now. We'll have ample opportunity for revision at a later stage.
Lets cross our fingers.
Thanks again everyone,
Mike
At 11:01 AM 2/24/2005, Keith Briffa wrote:

Sorry Mike - still dashing - but attached shows some slight wording changes - only early
and late - missed Track changes so just compare - sorry to mess up - otherwise go with
this for now and lets see reaction
Keith
t 00:40 22/02/2005, you wrote:

Dear Phil et al,
All of the suggested changes have been made, and some others additional changes have
been made for clarification, including descriptions of updated versions of the figures
(Scott: can you get to me pdf versions of figures 1 and 3 that have the correct
"degrees" symbol on the y axis? Also--we need an updated url for the pseudoproxy data at
fox.rwu.edu as noted! thanks in advance for getting back to me ASAP on these)
Changes indicated in yellow highlighting.
Will try to prepare a final draft for submission once I've heard back from Keith, Tim,
and anyone else who has any remaining comments. I've also attached a draft cover letter
to go to Nature along w/ the submission.
Thanks,
Mike
At 09:14 AM 2/21/2005, Phil Jones wrote:

Mike,
Here's a few modifications to the text. Keith and Tim are pretty happy with it
as well, but the'll reply as soon as they have some time.
Off again tomorrow to Chicago. Back in next week.
Happy for you to submit this as soon as you have their and other comments.
Cheers
Phil
At 22:44 12/02/2005, Michael E. Mann wrote:

sorry. text revised yet again. no more changes until I receive comments from

everyone.
thanks...
mike
At 12:03 PM 2/11/2005, Phil Jones wrote:

Mike,
Keith and Tim are here next week, but very busy with a proposal to the EU.
So you may have to hassle them a bit, or hang on for a week or two.
Nature dragged in the IPCC angle which annoyed me. I tried to explain to
him how IPCC works. IPCC won't be discussing this in Beijing in May - except
as part of Chapter 6. Hans von Storch will likely regret some of the words
he's said.
FYI, just as NCAR have put up a web site to give the whole story re Chris
Landseas's
'resignation' from a CA in the atmos. obs. chapter (to help Kevin Trenberth
out), KNMI
are doing the same re Rob van Dorland and that Dutch magazine. The chief
scientist
at KNMI has got involved as Rob didn't say the things attributed to him. I'll
find
out more on this in Pune as a guy from KNMI will be there.
Several other CAs on our chapter pulled out, or just didn't do anything.
Their
stories
never got run.
Dick's report was good and my bit in Nature cam across well.
Say hi to all there and wish Steve well.
Cheers
Phil
At 16:19 11/02/2005, Michael E. Mann wrote:

Phil--thanks, that's great. Really happy to hear that everyone is on board with
this.
I'm at a symposium honoring Steve Schneider out at stanford right now. Lots of
folks
here--as I talk this over w/ them, and see Dick Kerr's coverage of this, etc. I
realize
its not so bad--I was afraid this would be spun as bolstering the contrarians,
but it
hasn't. In large part due to quotes from you and others pointing out that the
study
actually reinforces the key conclusions, etc., and the fact Dick Kerr showed
Keith and
Tim's plot showing the scattering of multiple reconstructions, etc. which takes
the
focus off "Mann" a bit...
Nonetheless, I *am* convinced their methodology is suspect, as the analysis I
sent
shows. So I will really appreciate input from Keith, Tim, and you to make sure
the
language and wording are appropriate and fair...
I will revise as I get input from various people, with an aim to having this
submission-ready in about 10 days (so you can have one final look after you
return, and
before you have to head out again).
looking forward to getting people's comments, feedback, etc.
thanks again,
mike
At 08:05 AM 2/11/2005, Phil Jones wrote:

Mike et al,
I've talked to Keith and Tim here and it seems best if we all come in with
you on

this response. What you have done is basically fine. We can discuss specific wording later.

My problem is that I'm off tomorrow to Pune till Feb 20 and email may be sporadic or non-existent. So can you discuss revised drafts with Keith and Tim, but keep me on - lower down as I'm away. I'm here on Feb 21 then off to Chicago to review the vertical temperature report for the NRC/NAS Feb 22-25.

Keep me on the emails in case email works well in Pune.

Cheers

Phil

At 23:35 10/02/2005, Michael E. Mann wrote:

Dear Caspar, Gene, Scott, Phil,

I am attaching a response I've drafted to the Moberg et al paper (attached for those of you who haven't seen it). The message is pretty clear and simple--their method overemphasizes the low-frequency variability. To demonstrate this, I've made use of stuff from Mann and Jones, and from the Mann/Rutherford/Wahl/Ammann J. Climate letter on Pseudoproxies. So I would welcome any of you to be co-authors on this--just let me now if you're interested. I've been in touch w/ Keith (he and Tim are potentially working on their own independent response--waiting to hear further).

This is a very rough draft, so comments much appreciated.

Looking forward to hearing back,

Mike

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[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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5. http://www.cru.uea.ac.uk/cru/people/briffa/
6. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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From: Phil Jones <p.jones@uea.ac.uk>
To: Gabi Hegerl <hegerl@duke.edu>, Tom Crowley <tcrowley@duke.edu>, Gabi Hegerl <hegerl@duke.edu>, myles <m.allen1@physics.ox.ac.uk>, Tim Barnett

<tbarnett-ul@ucsd.edu>, Nathan Gillett <gillett@ocean.seos.uvic.ca>, "Stott, Peter" <peter.stott@metoffice.com>, David Karoly <dkaroly@rossby.metr.ou.edu>, Reiner Schnur <schnur@dkrz.de>, Karl Taylor <taylor13@llnl.gov>, francis <francis.zwiers@ec.gc.ca>
Subject: Future Directions
Date: Tue Mar 1 08:40:42 2005

Dear All,
I've knocked Chris off this reply. There is a meeting of the CCDD program next week
in Asheville. I guess Chris wants something for this. I'm on the panel, so if you want to
add to
what Gabi and Tom have put together then let me know and I'll feed that in additionally to
what is already there.
From being at the review last week of the vertical temperature trends panel, the
issue of
reducing forcing uncertainties is important. A number of people think that agreement in
the
20th century is all doing to model tuning due to uncertain forcing with sulphates. How to
counter this is one area. One of my own pet areas is trying to reduce uncertainties in the
paleo record for the last millennium, but again this is one of convincing people that we
really
know what has happened. So much is being made of the paleo records, but are they that
important to detection when most of the work is going on with the 20th century records. Is
the
pre-20th century really that important when it comes to D&A?
Cheers
Phil
At 20:45 28/02/2005, Gabi Hegerl wrote:

Hi IDAG people,
Chris Miller needs some input on where detection is going and what should be funded,
appended is a list Tom and I sent him as rapid response, but it sounds like they are
still
in the process of thinking about
this, so please reply (soon) if you have additions/comments (Chris, only thought of
sending
this now, I hope results will be still helpful)
Gabi

1) extending detection to other fields, esp. U.S. possible variables are circulation,
anything hydrological (drought, average rainfall), climate extremes, storms,
all this is getting more feasible as observational data get better, reanalyses get more
reliable (although trend sstill questionable), and models get better and have higher
resolution
2) compiling "showable" scorecard of what has been detected in the system already
3) abrupt changes - Tom thinks the relevance has been overstated of past

changes in the
thermohaline circulation (because of proximity of massive amounts of ice/freshwater).
However, I think it would still be useful to
find a fingerprint of predictors for thermohaline shutdown (from waterhosing experiments), and establish
how early warning signs can be detected.
Another aprupt change that could be dealt with are events such as the mega drought
cycles in the western U.S., which our preliminary work indicates does not correspond
with multidecal peaks in warmth for zonal average temperatures.
4) using paleoclimate data for understanding regional responses to known forcings, such
as pulse of volcanism in early 19th century. tests of a model's predictability on
regional scales. this however would require ensemble runs and a fair amount of legwork,
so probably would be best as a proposal than as an IDAG project.
5) more surface temperature detection as already donw, to keep analyzing 20th century
from models as model
diagnostic and evaluating how to get most model performance information out of this
diagnostic. For this,
updates of forcing estimates, particularly reduced sulfate aerosol uncertainties would
be useful.

-------- Original Message --------
Subject: Re: Directions in D&A
Date: Tue, 22 Feb 2005 10:51:56 -0500
From: Chris Miller <christopher.d.miller@noaa.gov>
Reply-To: christopher.d.miller@noaa.gov
Organization: NOAA
To: Gabi Hegerl <hegerl@duke.edu>
References: <4216317A.7020700@noaa.gov> <421A4F67.1040201@duke.edu>
Gabi, I'm looking for some quick thoughts, which probably means just you and Tom.
Obviously, the rest of IDAG would have ideas but it would take some time to poll them (I
could see it as an agenda item at the IDAG meeting). If you had a couple highlight items
by Thursday morning, that would be helpful as I have an internal meeting where this will
be discussed.
Thanks again, Chris
Gabi Hegerl wrote:

Chris, by when do you need this? From the whole IDAG or just, eg from me
and Tom?
Gabi
Chris Miller wrote:

Tom, Gabi, As you are probably aware, one of the recurring challenges for federal
program managers is to indicate to upper management what the science priorities in the
future should be. NOAA is more future-looking than it has been in the past and we are
now being called upon more frequently to respond to this question. A simplistic answer
would be "more of the same" since we are doing such good work now. This could

be part of
the answer, but not the whole answer. NOAA is interested in new science thrusts, new
observational programs or analyses, new institutional arrangements, etc. (the "new is
better syndrome"). I would appreciate it if you could take a few minutes to think about
this issue and send me a few bullets on where you think the community should be going on
D&A, for both continuing and new investments (from the perspective of the work that IDAG
has been involved in to date).
Thanks for your help and look forward to the next IDAG mtg.
Chris

-- ~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
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Duke University, Durham NC 27708
Ph: 919 684 6167, fax 684 5833
email: hegerl@duke.edu, [1]http://www.env.duke.edu/faculty/bios/hegerl.html

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References

1. http://www.env.duke.edu/faculty/bios/hegerl.html
2. http://www.env.duke.edu/faculty/bios/hegerl.html
3. http://www.env.duke.edu/faculty/bios/hegerl.html

502. 1110150877.txt
##########

From: "olgasolomina" <olgasolomina@yandex.ru>
To: jto@u.arizona.edu, eystein.jansen@geo.uib.no, Valerie.Masson@cea.fr,
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

k.briffa@uea.ac.uk
Subject: Glacier box - comments and suggestions
Date: Sun, 6 Mar 2005 18:14:37 +0300 (MSK)
Reply-to: olgasolomina@yandex.ru

Dear Valerie, Keith, Eystein and Peck,

Here are my comments on the glaciers box and suggestions for some improvements. I apologize that I am commenting the text that I was supposed to write myself, but we all know the reason – it was done in a rush and I had very limited access to the literature in the fall. I spent two weeks in Lamont (just came back) and had opportunity to read more. I want to say that I very much appreciate the help and contribution from all people who saved the situation to get the draft for the ZOD, and I hope that we can sharpen it further now.

In general my main concerns are the following:

1. We are focusing on the continuous records, which is one of the main achievement of the last years, indeed. But the real continuous records come from Scandinavia only – even the Alps are mostly based on moraine datings (wood etc.). The records from FJL and Brooks Range are not continuous, they are just the same as in any other place in the World, presented as continuous curves. So, two potential strategies can be suggested – to forget the rest of the World and keep the picture Scandinavia and Alps only or add more discontinuos records drawn as curves. I would go for the second solution for obvious reason to keep the global prospective. I attach more curves that I got from publications + I asked Tom Lowell and Wibjorn Karlen to make something of this kind for NZealand and Africa. I suggest to focus in detail (with dates etc.)on the Scandinavian records (as we did in our text), but briefly discuss the general picture of Holocene glacier variations referring to the updated picture. I need your opinion before changing the graphics (see comments and suggestions in “Box comments SO” file)

2. During a good half of the Holocene the glaciers were SMALLER than now. I attach here the figure with the same axes as at the Valerie’ picture (warmest/wettest periods), and the detailed comments on it. To be “scientifically correct” we probably can shade these periods for the regions that we are presenting at our figure (see a separate file “smaller than now”). What is unusual about the modern retreat is the RATE, though we do not know much about the rate of the former retreat (again because of the lack of continuous records).

3. I changed the introduction. I believe that it is really important to keep the general prospective and mention the exceptions, namely glacier advances (at a Holocene global scale) reflect mostly temperature, therefore a kind of global synchroniety can be visible, though occasionally precipitation may trigger certain advances – maritime and tropical regions are likely to experience it more often than the rest of the World) . Two papers justifying this point of view appeared recently (Oerlemans, 2005, Mayewski et al., 2004) and deserve attention. I included the refs in the updated text. I am ready to discuss further this part, but I believe that we need changes here!

I am aware that this will require rather big changes in the text and figure, but I hope we are still at the stage when we can change, can we?

I will come soon with comments on the whole text and suggestions for the links to Ch4 (cryosphere), but I am really concern about those glaciers in the box, you know…

Regards,
olga

Thanks! Peck
>

>>Hello,
>>
>>Thanks a lot for the remaind. I (eventually!) got access to the
>>literature (in Columbia University where I am now) and will come
>>soon with comments and improvement of the etxt - at list concerning
>>the glaciers in the Holocene and last two millennia.
>>
>>Regards,
>>olga
>>
>>
>>>Hi all - We have heard from a good number of you, but also have not
>>>heard from some of you. Please note the deadline for the first round
>>>of post-ZOD feedback was yesterday, and more is due next week. If you
>>>have not sent your material, or contacted us yet, please do so as
>>>soon as possible. A small delay is ok, but we need to hear from you
>>>in any case - please respond if you have not already done so.
>>>
>>>Thanks, Peck and Eystein
>>>
>>>>Date: Wed, 9 Feb 2005 11:15:25 -0700
>>>>To: wg1-ar4-ch06@joss.ucar.edu,betteotto-b
>>>>From: Jonathan Overpeck <jto@u.arizona.edu>
>>>>Subject: The next round of work is upon us - IMPORTANT
>>>>Cc:
>>>>Bcc:
>>>>X-Attachments: :Macintosh HD:370627:Glossary WgI TARChap6.doc:
>>>>
>>>>Greetings Chap 6 Lead Authors:
>>>>
>>>>By now, the rush up to the ZOD is hopefully but a fond memory, and
>>>>you're ready to get back into the thick of IPCC chapter work. Both
>>>>Chapter 6 and the other chapters are now on the WG 1 website for all
>>>>of you to enjoy and critique. See your email from the WG1 TSU for
>>>>information on how to get ZOD chapters.
>>>>
>>>>As you read our chapter, you will no doubt be thinking - "it's
>>>>really too bad we did so much at the last minute, and that the ZOD
>>>>is so rough." The science is in there, and you all did a great job,
>>>>but in the future, we won't have the luxury of sending an incomplete
>>>>draft to the TSU. The purpose of this email is to set a deliberate
>>>>pace to ensure that our FOD is as perfect and polished as possible.
>>>>Anything short of this will look bad to our colleagues, and will
>>>>cost us more work in the official post-FOD IPCC review process.
>>>>PLEASE MEET ALL DEADLINES below.
>>>>
>>>>Please read all of this communication and NOTE the deadlines - we
>>>>are asking that you all respond quickly on a couple issues.
>>>>
>>>>****1) Due as soon as you read this email - we would like to
>>>>consider a pre-May LA meeting involving all, or a sub-set of LAs,
>>>>and would like to know when you are available to meet for 2 days
>>>>(plus travel to/from US East Coast). The purpose would be to get
>>>>much further ahead with the FOD and to be able make the most of the
>>>>Beijing LA2 meeting in May. Remember how frustrating the Trieste
>>>>meeting was due to the lack of time. Please let us know if you are
>>>>available to meet April 12,13 (Tues/Wed) and April 19,20 (Tues/Wed).
>>>>We will pick the dates that work best. Funding would be handled in
>>>>the usual IPCC manner.
>>>>
>>>>
>>>>****2) Due February 24, 2005 - each person should read ALL of the
>>>>Chapter 6 ZOD. As you do this, please compiling a list of all the

>>>>issues/tasks you think need to be dealt with and completed before
>>>>the FOD. For example:
>>>>
>>>>o what important issues or disagreements remain unresolved and what
>>>>needs to be done to resolve them?
>>>>o what work is needed to make the text better?
>>>>o what key (relevant) science is missing?
>>>>o what key references are missing or need to be updated?
>>>>o are there key display items that need to be deleted or added?
>>>>o what work is needed to make final draft display items?
>>>>
>>>>Each LA should provide the above information to PECK and EYSTEIN on
>>>>a section-by-section basis by February 24. Please let us know NOW if
>>>>you can't meet this deadline.
>>>>
>>>>
>>>>****3) Due March 3, 2005 - (we have to meet a key IPCC deadline)
>>>>-Now that we have our ZOD, we have been requested to provide input
>>>>for the official IPCC AR4 Glossary. Please see the attached glossary
>>>>document, and follow the instructions included at the top of that
>>>>file. THIS IS JUST AS IMPORTANT AS OUR OTHER WORK. Each LA should
>>>>provide this information TO PECK AND EYSTEIN by March 3. Please let
>> >>us know NOW if you can't meet this deadline.
>>>>
>>>>
>>>>****4) Due March 10, 2005 - in Trieste, we assigned Chapter Liaisons
>>>>for each of the other WG1 chapters. This liaison list is attached
>>>>below. Please note that some of you are liaisons for more than one
>>>>other chapter. For each chapter for which you are liaison (and more
>>>>if you are so inspired), please compile:
>>>>
>>>>o a list of substantive scientific suggestions for the LAs of that
>>>>chapter, particularly as they relate to Chapter 6 - don't get bogged
>>>>down in general editing.
>>>>o a list of issues that our Chapter 6 team must work on to ensure
>>>>compatibility with other chapters; in each case, describe the issue
>>>>and how you think it should best be resolved. Ideally, we can do
>>>>much of this before Beijing.
>>>>
>>>>Each LA should provide the above information to PECK and EYSTEIN by
>>>>March 10. Please let us know NOW if you can't meet this deadline.
>>>>
>>>>**********
>>>>Lastly, we have some good news. As you all know, Bette Otto-Bleisner
>>>>did a great last-minute job in helping with section 6.4.2
>>>>(Equilibrium model evaluations), and has made it possible for us to
>>>>tap into PMIP2 in a much needed manner. We clearly need her
>>>>continued major contribution, and thus asked the IPCC WG1 Bureau to
>>>>appoint her to our LA team. Susan Solomon supported this request and
>>>>we recently received a positive response. So, welcome to the team,
>>>>Bette!
>>>>
>>>>PLEASE work hard to meet deadlines - I think we all know what
>>>>happens when deadlines are not met, and we cannot afford to miss
>>>>deadlines any longer.
>>>>
>>>>Thanks, Peck and Eystein
>>>>
>>>>
>>>>Chapter Six - Paleoclimate
>>>>Cross-Chapter Liaisons
>>>>
>>>>Frequency Asked Questions Stefan

>>>>
>>>>Chapter 1. Historical Dominique (served on SAR)
>>>>
>>>>Chapter 2. Radiation Dominique (trace gas, aerosol)
>>>> David (solar, volcanic, aerosol)
>>>>
>>>>Chapter 3. Atmo Obs Keith
>>>> Ramesh
>>>>
>>>>Chapter 4. Cyro Obs Dick (ice sheets
>>>> Olga (mountain ice)
>>>>
>>>>Chapter 5. Ocean Obs. Jean-Claude
>>>> Eystein
>>>>
>>>>Chapter 7. Biogeochem Fortunat (biogeochem)
>>>> Ricardo (veg dynamics)
>>>>
>>>>Chapter 8. Model Eval Bette
>>>> Dick
>>>> Stefan
>>>> David
>>>>
>>>>Chapter 9. Attribution David
>>>> Valerie
>>>> Keith
>>>>
>>>>Chapter 10. Projections David
>>>> Stefan
>>>>
>>>>Chapter 11. Regional Dan
>>>> Ramesh
>>>> Zhang
>>>> Overpeck
>>>>
>>>>--
>>>>
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>>>>Professor, Department of Geosciences
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Attachment Converted: "c:\eudora\attach\ipcc smaller than now.doc"

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From: Susan Solomon <Susan.Solomon@noaa.gov>

To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>,
Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: Fwd: last millennium
Date: Tue, 15 Mar 2005 12:50:06 -0700

<x-flowed>
Dear Peck,
Thanks for your message. I'll look forward to hearing what you and
your colleagues think.
Susan

At 9:26 AM -0700 3/15/05, Jonathan Overpeck wrote:
>Hi Susan - thanks for sending these along with some interesting
>ideas. I'll cc this email to Keith Briffa, along with Eystein, to
>see if the three of us could chat about the issues. Personally, I
>think the idea of showing the instrumental data near the paleo sites
>is excellent - but we have to see what Keith thinks since it would
>be his (and CA Tim Osborn's) job to do this. But, it makes lots of
>sense. I also like having the composite (average) lines (paleo and
>instrumental) for the simple reason that they connects back to all
>the other reconstructions, and thus make the point that these other
>recons are not so "misleading" after all.
>
>Funny coincidence - Julie and I have been working on the coral trend
>story, and just yesterday decided to do what you are suggesting in
>terms of instrumental data. I'm learning that the coral data are
>trickier than I thought, but this is a good way of figuring out what
>we really can or cannot say with these time series.
>
>More soon, thanks again, Peck
>
>>X-Sieve: CMU Sieve 2.2
>>X-Sender: ssolomon@mailsrvr.al.noaa.gov
>>Date: Mon, 14 Mar 2005 15:40:35 -0700
>>To: Jonathan Overpeck <jto@u.arizona.edu>
>>From: Susan Solomon <Susan.Solomon@noaa.gov>
>>Subject: last millennium
>>Cc: Martin Manning <Martin.Manning@noaa.gov>
>>X-Virus-Scanned: amavisd-new at email.arizona.edu
>>X-Spam-Status: No, hits=0.001 required=7 tests=BAYES_50
>>X-Spam-Level:
>>
>>Hi Jonathan,
>>Here's some cool plots that Tom Crowley whipped up, as per our
>>phone discussion. He indicated that it was OK to send to you.
>>
>>It seems to me that showing these records explicitly will address a
>>lot of the issues in the temperature records for the last
>>millennium. One might or might not choose to try to construct the
>>composites (see slide 2 versus 3 in the attached). To be totally
>>consistent, it would be nice to show individual records for the
>>twentieth century near the sites of the tree ring/cores as well,
>>rather than just the mean over that period. If one did that, the
>>resulting diagram would avoid any averaging (is it really needed to
>>make the point?). A remaining issue would be the calibration of the
>>paleo proxies and how that affects the spread (or lack thereof, in
>>the overlap period).
>>
>>What do you think?
>>Susan
>>
>>

>>--
>>**
>>Please note my new email address for your records:
>>
>>Susan.Solomon@noaa.gov
>>**
>>
>
>
>--
>Jonathan T. Overpeck
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>http://www.ispe.arizona.edu/
>
>Attachment converted: Discovery:crowley.mwp.mar.14.ppt (SLD8/PPT3) (000F0F48)

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****
Please note my new email address for your records:

Susan.Solomon@noaa.gov
****
</x-flowed>

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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, ray <rbradley@geo.umass.edu>
Subject: Re: BBC E-mail: New row on climate 'hockey stick'
Date: Thu Mar 17 13:54:17 2005

    Mike,
      On Horizon, I'm supposed to be called in a few minutes by someone. Not sure
who
    yet. This program is generally good. They did something on global dimming a few
months
    ago and now want to do something on the truth about global warming, IPCC and
    skeptics.
      That's all I know so far. Person's name is Paul Olding. Should be calling
    at 2pm, so 5 minutes time.
    Cheers
    Phil
   At 13:21 17/03/2005, Michael E. Mann wrote:

     HI Phil,
     I agree-like all of these sources (e.g. boreholes, tree-rings, etc.) each one
has its

own potential weaknessses--in this case, I think cold-season precip could be playing a
greater role w/ the mid-latitude glaciers than Oerlemans cares to admit. Not clear that
should give a systematic bias towards underestimating temperature variations though,
which is the argument you'd need to make if you're a boreholer.
The important thing is that it is entirely independent of everything else that has come
before, and looks remarkably like the Bradley and Jones/Mann et al/Jones et al/Crowley &
Lowery/Mann & Jones type reconstructions. Somehow the word hasn't really gotten out on
this.
I've got a call in from a different BBC reporter today, Ben Dempsey, who seems much
better. He's doing something for "Horizon" on climate change.
Do you know anything about this?
Thanks,
mike
At 08:02 AM 3/17/2005, Phil Jones wrote:

Mike,
Reporter was Paul Rincon ("Paul Rincon-NEWSi" <Paul.Rincon@bbc.co.uk>).
No-one seems to have picked up on Oerleman's paper yet. You did send me that
earlier, so I should have told him about that.
Sarah Raper here has some doubts about Oerleman's work, but it does
reproduce the curve very well. Need to be objective though in interpreting it.
Cheers
Phil
At 12:48 17/03/2005, Michael E. Mann wrote:

Hi Phil,
Yes, BBC has been disappointing in the way they've dealt with this--almost seems to be a
contrarian element there.
Do you remember the name of the reporter you spoke to?
Thanks,
Mike
p.s. Interesting that they also don't seem to be aware of the Oerleman's paper, which
reproduces the "Hockey Stick" using completely independent data and method (glacial mass
balance). I've attached in case you haven't seen...
At 03:26 AM 3/17/2005, Phil Jones wrote:

Ray,
I tried to convince the reporter here there wasn't a story, but he went with it
anyway.
At least he put in a quote from me that there are loads of other series that show
similar-ish series to MBH and MJ. Had to mention the Moberg et al series to achieve
this.
The reporter said he'd not seen Moberg et al., and it wasn't flagged up by Nature
to them at the appropriate time. Odd ! Then why are you running with this GRL paper
as there are 10s issued each week. Well, it turns out, not surprisingly, that MM have
issued numerous press releases themselves - using their networks.
Waterhouse is at Anglian Polytechnic Uni (APU) - it's in Cambridge and

Chelmsford.
Keith said what does John Waterhouse know about paleo - my thoughts also !
We've worked with John several years ago on an isotopes in trees project, that didn't
produce much. APU is OK when it comes to counselling studies. Ruth works for them
teaching at Yarmouth !
His quote is typical of many I get to here. Pity the reporter didn't mention this
to me.
My response would have been what is the point of doing any more paleo work, if we
are constrained by the answer we are allowed to get. If we don't have the MWP and LIA
then we are wrong. We have orders of magnitude more data than when these came into
vogue in the 1960s, but we still are expected to find them.
Cheers
Phil
Cheers
Phil
At 17:20 16/03/2005, you wrote:

ray saw this story on BBC News Online and thought you
should see it.
** Message **
Anglia Polytechnic?!!!!
** New row on climate 'hockey stick' **
New controversy has erupted over one of the most provocative symbols of the global
warming debate: the so-called "hockey stick" graph.
< [1]http://news.bbc.co.uk/go/em/fr/-/2/hi/science/nature/4349133.stm >
** BBC Daily E-mail **
Choose the news and sport headlines you want - when you want them, all
in one daily e-mail
< [2]http://www.bbc.co.uk/dailyemail/ >
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webmasters@bbc.co.uk making sure you include the following text: I do
not want to receive "E-mail a friend" mailings.

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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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References

1. http://news.bbc.co.uk/go/em/fr/-/2/hi/science/nature/4349133.stm
2. http://www.bbc.co.uk/dailyemail/
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. http://www.evsc.virginia.edu/faculty/people/mann.shtml

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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: Stuff....
Date: Mon Mar 21 10:08:32 2005

Ben,
I will be at Duke. Get to the airport about 6.30pm on the 29th. Looking forward to seeing you there.
I should have signed off on the CCSP report by Easter. We have to get everything done by March 28. We had a conf. call last Friday.
I can see the argument about an assessment and 'new information'. It is a similar thing in IPCC. Glad to hear you're going to submit it for a paper, because I think it is important. It will unlikely change some peoples views, though.
Just had a long call with Chris Folland. He says that the next CCSP vtt meeting is going to be scheduled for Chicago for the week we should be doing the HC review !
Hope you're still going to come to Exeter. You should have less to do than all the

other chapters !
See you on the 29th late or more likely for breakfast on the 30th.
Cheers
Phil
At 23:16 18/03/2005, you wrote:

Dear Phil,
Sorry about the delay in replying to your email. I picked up a chest infection while I
was at the IPCC meeting in Hawaii, and it proved to be very persistent. I think a
weekend's rest will do me good.
It was great to see you in Chicago, even though the meeting itself was quite difficult
to sit through. As may have been apparent, Roger and I really rub each other the wrong
way. Working with him on this CCSP Report has been a very unpleasant experience.
I am taking your advice, and trying to write up the "amplification factor" stuff that I
showed in Chicago. I presented this in Hawaii, and it sparked a lot of discussion. Just
between you and me, Susan Solomon argued quite forcefully that this new information
should NOT go into the CCSP Report, and that we should not be performing science in
support of an assessment. She was concerned that the CCSP Report might be subject to
unjustified criticism if key conclusions of the Report relied on unpublished work. I
have considerable sympathy with this view. It does seem important to get this work
submitted to a peer-reviewed publication as soon as possible, and then worry later about
whether the material should or should not appear in CCSP.
Are you going to the Duke IDAG meeting? If so, I look forward to seeing you there.
Best regards to you and Ruth,
Ben
--

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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Brohan, Philip" <philip.brohan@metoffice.gov.uk>
Subject: Re: HADCRUT various
Date: Mon Apr 4 09:50:24 2005
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>

Philip,
I'm not unhappy at all. If I am it is more about HadCRUT2 and 3.
I read through the report to DEFRA and will be sending some comments later today. I also commented on what Harry has written as a report for you. I've left those comments with him as he's away this week and I'm off April 6-15.
It is a bit odd with HadCRUT2 that the problem has surfaced now and my old mask hasn't made any difference.
Cheers
Phil
At 15:33 01/04/2005, Brohan, Philip wrote:

Phil.
I've just had a chat with Peter Thorne about HadCRUT2 and 3, and I get the impression that you are concerned, so we thought I should clarify what is going on.
In particular I want to assure you that we are not trying to change the system without your approval.
To make things quite clear, we have two HadCRUT systems here:
1) Peter is running HadCRUT2. This is our operational system which produces the new data every month that we send to you and everyone. This is a fixed system, it does exactly what you agreed with Peter a couple of years ago. We don't plan to change it at all.
We did, unfortunately, make a mistake while running the system; we think a land-mask file was changed. This is what Peter's recent messages have been about.
We're still not quite sure how this happened, but whatever fix we apply will be to restore the system to the original, agreed state.
2) I am coordinating HadCRUT3. This currently encompasses Harry's work on the data,
Simon's work on blending, John Kennedy's work on variance correction, and my work on
errors and gridding. Some combination of this work will become the new dataset. I have a clear picture of what I think should form the new dataset. However, we won't produce HadCRUT3 unless you (and all the other contributors) agree. If I can't persuade you of the value of a change, it won't happen. In particular, I see the land station data as entirely under your control, both now and in the future.
If I (or Peter) misread the vibes and you were not worrying about any of this, please don't start. There are not serious problems with either system.
Have fun,
Philip.

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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: Re: last millennium - responding to Susan
Date: Mon, 4 Apr 2005 23:08:47 -0700
Cc: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Hi Keith and Tim - sorry for the delay in responding. I think the issues you raise are worth discussing, but we can do that in Beijing, and hopefully with Susan. She is keen on the idea, and my gut says it's a good idea to include such "crowley" plots somewhere - at least in the appendix, for example. But, let's talk in person.

In the meantime, we really need your comments on the ZOD - including what you feel has to be done with your section, but also with the others. We have comments from most others, and are expecting the external review comments soon, so please send yours ASAP so they can be included in this important stage.

Thanks! Best, peck

>Jonathan
>
>I am slowly getting teaching duties behind me and certainly turning
>my attention back to IPCC. I have spoken wit Phil re the
>observations chapter and we have discussed the need to show pre 20th
>instrumental data in our chapter in a manner that is relevant to the
>comparison with more recent instrumental (ie N.Hemisphere or global
>mean) records , and the possibility of showing ensembles of regional
>temperature records , and composites in a way that possibly bares on
>the discussions with Susan. We are still considering this question ,
>but certainly there needs to be some "frozen grid" curves as flagged
>in the ZOD.
>I am not sure of the context of the discussion you are having with
>Susan , or the logic for what Tom Crowley is trying to do with the
>ensemble curves of various palaeo-series.
>
>I flagged clearly at the outset that I would like to do some
>regional comparisons of various data/reconstructions . This required
>more time and input than was achievable for the ZOD. I still think
>this is desirable though. Similarly , there is far too little in the
>current version about moisture variability in the last 2000 years
>and too little on the S.Hemisphere in general. It was always clear
>that there would be much more discussion on the scaling issue and
>specific reference to work that will explore the effect of regional,
>seasonal and methodological differences in aggregation and scaling
>(including timescale dependent effects). The problem is that the
>work on much of this is not yet done or published. It should be
>immediately apparent that our greatest enemy , acting against a
>thorough exposition of these issues , is the lack of sufficient
>allotted space.
>
>Now , returning to the Crowley Figures , I do not see how not
>showing an integrated and "appropriately" scaled record helps to
>clarify the picture on the precedence of recent warming in any clear
>way. On the contrary , it merely confuses the issue by omitting to
>tackle the knotty problem of expressing an underlying mean
>large-scale signal , that emerges from the regional noise only
>through aggregation of demonstrably appropriate palaeo-records .
>This aggregation should allow quantification (with appropriate
>uncertainty) of the extent of warming and provide clearly defined

>target for comparison with model simulations.
>
>If it thought appropriate , yes we can show individual records , but
>just normalising them over a common base ignores the different
>sensitivities and regional distribution issues . I am not convinced
>this selective presentation clarifies anything. I would be happy
>for this discussion to opened to the rest of the author team.
>
>best wishes
>
>Keith
>
> At 16:28 15/03/2005, you wrote:
>>Hi Keith - I can't remember when you said you'd be able to get back
>>into the IPCC fray, but I hope it is soon. Please let me and
>>Eystein know what you think regarding the email I just cc'd to you.
>>We should respond to Susan asap. Hope things are going well.
>>Thanks, Peck
>>--
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Parker, David (Met Office)" <david.parker@metoffice.gov.uk>, Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: Chapter 3.4.1
Date: Tue Apr 19 16:12:38 2005
Cc: David Parker <david.parker@metoffice.gov.uk>, Brian Soden <bsoden@rsmas.miami.edu>, Susan Solomon <Susan.Solomon@noaa.gov>, Martin Manning <Martin.Manning@noaa.gov>, "'David R. Easterling'" <david.easterling@noaa.gov>

Kevin,
I plan to look through your 3.4.1 draft tomorrow or later this week. At the same time I also plan to have a go at section 3.2. David has sent me some new figures and there are two new papers to add in. I am having difficulty finding some quality time at the moment, but hope this will come later this week.
I did read all the CCSP report. The review group are having a conf call tomorrow on this, but they have chosen your afternoon, so I can't take part. There were 6 reviewers of the review and one other almost wrote as much as you. Most were positive on the review saying that the report authors have a lot to do, particularly for Chapters 1 and 6. How all this pans out is impossible to tell. The next meeting of the authors is being scheduled for the week after Beijing.
I agree some of their figures are useful, but I too doubt whether we will have much useful for the FOD we have to write. We will likely be doing them in parallel - which is hardly ideal.
I wouldn't send our 3.4.1 to Tom at this time - at least wait till Brian, David and I have been through yours. Also I wouldn't want Tom passing it on to the CCSP VTT authors. I think they will have a lot of hard thinking when they get the NRC review, to worry too much about what we're doing. We do need to have our chapter and their report meshing at some time, but this might have to wait till the SOD (by which time their report might be finished).
Cheers
Phil
At 17:35 18/04/2005, Parker, David (Met Office) wrote:

Kevin
Thanks. You have saved me some work because on my journey back from Geneva I also studied the comments on 3.4.1 (on paper) and was considering making an electronic revised section. I came to the conclusion that 3.4.1 should say that there are 2 schools of thought about Fu et al and other aspects of the temperatures-aloft issue: the jury is still out. That would be a assessment (as opposed to a review) of the current state of the science. Fu may not be correct as he seems to imply upper tropospheric warming rates well outside the error-bars implied by the radiosondes (though I am aware of their problems too). I have not yet read your attachment but will consider it in the next few days.
I looked at the surface temperature comments too and feel it may be best

to wait until in Beijing, as most comments are about what diagrams to choose. I could try to re-order the urban warming section as reviewers suggest, but we may still wish to contact Tsutsumi (who didn't reply to my email a couple of months ago) to write something.
Regards
David
On Mon, 2005-04-18 at 17:13, Kevin Trenberth wrote:
> Hi Phil and David, and Brian
>
> I believe you three are probably closest to the satellite temperature
> record issue and so I am sending this to you. I have thoroughly gone
> over all the comments we received and I have prepared a revised 3.4.1
> which is attached. This is the cleaned up version. The actual
> version has tracking turned on but the changes are so extensive that
> they are very hard to follow. As you know, I have read the entire
> CCSP report and commented extensively on it. I know Phil was on the
> review team and David was there as a lead author. However David and
> Phil may not be as familiar with the whole report.
>
> Obviously this remains a controversial topic. Many of the comments we
> received were diametrically opposed to one another. The rhetoric was
> disappointing (especially from Peter Thorne). In fact Peter's
> comments are mostly not useful and reveal very strong biases against
> Fu and reanalyses. Previously, you'll recall that David provided most
> of the text and I edited it and updated it with the Fu material in a
> somewhat ad hoc fashion that got almost everyone mad. Probably a good
> thing to do in retrospect, as this next version will look so much
> better. Note that I have done nothing with the appendices at this
> point, so that needs to be addressed. I have taken out all the
> tables??
>
> You will see even in the current text that I have 2 sections I would
> like to delete.
>
> While individual comparisons of radiosonde station data with
> collocated satellite data (Christy and Norris, 2004) suggest that the
> median trends of radiosonde temperatures in the troposphere are
> generally very close to UAH trends and a little less than RSS trends,
> trends at individual radiosonde sites vary and root mean square
> differences of UAH satellite data with radiosondes are substantial
> (Hurrell et al., 2000). Moreover, as noted in 3.4.1.1, comparisons
> with radiosonde data are compromised by the multiple problems with the
> latter, and there are diurnal cycle influences on them over land. In
> the stratosphere, radiosonde trends are more negative than both MSU
> retrievals, especially RSS. [DELETE THIS?]
>
> The problem here is the rhetoric of Christy et al. In his
> contribution Christy justifies the UAH record by saying that "median
> trends agree with those of sondes". But he actually sent to us his
> Fig. 2 showing the lack of agreement in general. It is only the
> median that agrees, the agreement with sondes individually is not good
> and this is just for trends. [Hence the median depends on the
> selection of stations]. It is even worse if rms differences are
> examined (as in Hurrell et al 2000). The only reason to include this
> is to rebut Christy's claim. For most other readers it has no
> business being there. Your suggestions appreciated. Maybe this
> should go in the appendix?
>
> You will see that I have stolen 2 figures from the CCSP report. I
> made up the 3rd figure from data provided from the CCSP report plus
> extra material (only the global is in the current draft). It would
> also be nice to include a spatial map of trends at the surface and for
> the troposphere (T2 corrected as from Fu) but no such figure exists

> anywhere, yet. We can get trends from RSS and UAH for T2. It would
> be good to have access to the originals so we can modify them and
> clean up the terminology. {On that score, I don't think the CCSP
> terminology is tenable given the new retrievals of Fu et al (2005) and
> ours, using T2, T3, and T4 is much easier).
>
> At present the CCSP report is not very useful to us. Some figures are
> useful. It may become so, but I actually have my doubts, given the
> vested interests of the authors.
>
> I am tempted to send this to Tom Karl in his role as editor of our
> chapter, and of course he is head of the CCSP effort, but I would NOT
> want him to use it for CCSP (except that it might highlight the
> differences in assessments). What do you think? Via Tom we might get
> better access to the figures and updates? Also I'l l cc David
> Easterling.
> This would be the main basis for FOD.
>
> Ideally also it is desirable to get the figures updated thru 2004, but
> can we?
>
> Please read this version and let me know what you think? (Please be
> kind, I have put in a LOT of work on this)
>
> Best regards
> Kevin
>
> --
> ****************
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Global climate data sets are available from [2]http://hadobs.org

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References

1. http://www.cgd.ucar.edu/cas/
2. http://hadobs.org/

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From: Phil Jones <p.jones@uea.ac.uk>
To: Peter Lemke <plemke@awi-bremerhaven.de>, Kevin Trenberth <trenbert@ucar.edu>

Subject: Re: WG1 LA2 meeting - Overlap cluster A
Date: Wed Apr 20 10:49:38 2005
Cc: Martin Manning <mmanning@al.noaa.gov>, Susan Solomon <ssolomon@al.noaa.gov>, ipcc-wg1@al.noaa.gov, k.briffa@uea.ac.uk

Dear All,
In addition to Kevin's comments and from a quickish look through parts of Chapters 4, 6 and 9, here are a few suggestions.
First for best use of time, I would suggest that Cluster B gets broken into two parts.
Basically separating off the overlap with the paleo and instrumental record including borehole temperatures and glacier length changes from the sea ice/SST, snow/temperature.
OHC/SST, salinity/precip and SLR etc. The latter can be dealt with by Chs 5, 3 and 4.
The former is really for 6, 3 and 4.
Issues for 3 and 6 are the interface of the instrumental and paleo records, particularly how the early 19th century is dealt with. This period of instrumental records is believed by many in the paleo community not to exist, but in Europe and a few other regions it exists back in good order to the late 18th century. The 19th century is, I believe, the key to resolving much of the discussion about the millennium. Much more should be made of this period when comparisons with long forced GCM runs are analyzed. Europe may be a small continent, but the 200-250 year 'perfect proxy' records (which have all seasons!) need to be studied more. As any conclusions relate to Ch 6, the main text should be there, with perhaps a box on the early instrumental period in Ch 3.
Somewhat related to the above, Ch 4 has a section on the recent Oerlemans (2005) work - attached for reference. Mike Mann sent me a figure (see jpg) comparing this with most other reconstructions of parts of the millennium. It seems that this piece of work should be with all the others in Ch 6 and not Ch 4. When producing plots like this getting the right base level is crucial - not just for Oerlemans' series, but also for the boreholes. Also, the degree of smoothing and the y-scale used can easily determine the takeaway message.
Chapter 9 has an interest in both these issues.
Finally, there is one other issue. Do we want to consider having a web site (distributed?) where the data for some selected time series can be downloaded from - not just the smoothed/plotted series, but on the original timescale as well. This possibly comes back also to a consistent way of smoothing time series.
Cheers
Phil

At 08:11 20/04/2005, Peter Lemke wrote:

Dear Martin,
I am also willing to co-chair the cluster B. (As always) Kevin has done a very good job
in listing the most important issues.
Therefore, I have nothing to add at the moment. I will think about this on the weekend.
Best regards,
Peter
Kevin Trenberth schrieb:

Hi Martin
Yes I will do this.
Firstly on cluster A:
I/we have an issue which is: what about changes in radiative forcing from water vapor
(or feedback if you prefer), it is of order 1 W m-2.
So this relates to water vapor changes in chapter 3.
Cluster B: Consistency in observed climate change: atmosphere, ocean, cryosphere. This
may also extend to paleo, chapter 6.
Issues:
Consistency of:
* sea ice with SST
* snow cover with snowfall and temperature
* glacier melting and permafrost changes vs temperatures
* borehole temperatures, glacier changes and paleo record
* overlap between paleo record and instrumental record
* salinity vs precipitation
* ocean heat content with SST and surface fluxes
* sea level rise as an integrator: ocean expansion, melting of land ice, increased water storage on land, and changes in TOA radiation (presumably led by Chapter 5.)

Issues consist of use of consistent temperature and precipitation records (don't use
NCEP surface temperatures as in Ch 4 CQ).
Points of contention:
1) consistency
2) overlap and redundancy
3) where to place integrated assessment?
* sea level: Chapter 5
* snow, ice, temperature chapter 3 section 3.9
* paleo record vs instrumental chapter 6
* overall view including sea level chapter 3, in 3.9
* T increase (land, SST, subsurface ocean), snow retreat, sea ice retreat, thinning, freezing season shorter, glacier melt, sea level rise.
* Precip changes, drought, salinity, ocean currents, P-E, snowfall.

Please see the draft of 3.9.
So in terms of the agenda, the main points are:
1) Ensuring consistency among variables across chapters
2) Agreement on which chapter and what person will handle what, and in particular, that
3.9 will have a look ahead aspect to the chapters that follow.
The above points could all be briefly on the table with the focus on cross-chapter
issues.
Desirable to circulate draft section 3.9 (1 page).
Peter may wish to add or change this?
Regards
Kevin
Martin Manning wrote:

Dear Kevin and Peter
Please find attached our current program for the second Lead Author meeting on May 10 -
12. We will shortly be sending out some more details on the plans for the meeting and
in particular would like to clarify what needs to be done in the Overlap Cluster
meetings shown in the program on Wednesday 11th.
This is to ask if you would be prepared to jointly co-chair the session on Overlap
Cluster B dealing with "Consistency in covering observed climate change" and which will
involve discussion among chapters 3, 4, 5, 9 and 11. The attached program lists, on the
last page, overlap / consistency areas that have been mentioned in the ZOD.
We would really be most grateful for your assistance in this, and if you agree, we would
like to ask that you each to specify what in your view would be the 2 or 3 most
important issues to resolve during the overlap cluster session. We will then use your
input to draw up a specific agenda and circulate agendas for all overlap clusters to all
CLAs prior to the meeting. We hope in this way that we can reach a shared understanding
of the most important overlap and consistency issues and the corresponding key decisions
that will have to be made in Beijing.
I would be grateful if you could let me know whether you are able to help us with this
by Wednesday 20th.
Regards
Martin
--
*Recommended Email address: mmanning@al.noaa.gov
*** Please note that problems may occur with my @noaa.gov address
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NOAA Aeronomy Laboratory Phone: +1 303 497 4479
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****

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References

1. http://www.cgd.ucar.edu/cas/
2. http://www.awi-bremerhaven.de/

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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: [Fwd: Re: WG1 LA2 meeting - Overlap cluster A]
Date: Wed, 20 Apr 2005 15:28:30 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, olgasolomina@yandex.ru

Kevin - ah yes, good fun. Talked w/ Susan about some of this, and we're hoping that Keith
Briffa might be able to participate in "Cluster B" while the rest of our chap 6 team
discusses things that bore Keith. I'll forward this to relevant chap 6 folks. Thx, Peck

Jon
FYI wrt Beijing and overlap issues with chapter 6. You may find some exchanges of
interest as well.
Kevin
-------- Original Message --------
Subject: Re: WG1 LA2 meeting - Overlap cluster A
Date: Wed, 20 Apr 2005 17:12:41 +0100
From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>
References: <5.2.0.9.2.20050418185815.0303d0d0@mailsrvr.al.noaa.gov>
<42654140.2080509@ucar.edu> <42660091.9060600@awi-bremerhaven.de>
<6.1.2.0.0.20050420101527.01d3f508@pop.uea.ac.uk> <42667322.4070101@ucar.edu>
Kevin,
Right on ! Assumes precip doesn't change - i.e. it's constant. Difficult to do
much more for some regions, but could do a lot better for the Alps. Ch 4 has swallowed
this hook, line and sinker and it is really a Ch 6 issue. Ch 6 wasn't even aware of it.
Can't decide who on Ch 4 knew about it as Oerlemans isn't there and the Swiss Glacier
people didn't know about the paper 2 weeks ago when I saw them.
I like the curve as does Mike Mann, but its not for any scientific reason.
Any jury is still out on whether this is right, but I'm glad someone has tried the
approach. It is a quantification of what people have assumed, but there likely isn't
enough detail in the paper to show how it was done.
I've not seen this paper in a proper issue of Science yet. As such I've not been

able to get the supporting material.
This paper is totally independent of all other paleo work. It is much better science
than Mobeg et al. in Nature in February. Susan has been sending a few emails to
Ch 6 about how to display the various millennium series - some of which she's not
thought through.
Just be glad we haven't got paleo in out chapter !
Cheers
Phil
At 16:20 20/04/2005, you wrote:

Hi Phil
I had not read Oerleman's paper, I have now. Some things don't make sense to me: chanes
in precip not included and the time series (esp N America) Also magnitude of implied
early 20Th C warming. What is your take?
Kevin
Phil Jones wrote:

Dear All,
In addition to Kevin's comments and from a quickish look through parts of Chapters
4, 6 and 9, here are a few suggestions.
First for best use of time, I would suggest that Cluster B gets broken into two
parts.
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Kevin Trenberth schrieb:

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GERMANY
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Jon
FYI wrt Beijing and overlap issues with chapter 6. You may find some exchanges of
interest as well.
Kevin
-------- Original Message --------

Subject: Re: WG1 LA2 meeting - Overlap cluster A Date: Wed, 20 Apr 2005 17:12:41 +0100
From: Phil Jones [1]<p.jones@uea.ac.uk> To: Kevin Trenberth [2]<trenbert@ucar.edu>
References: [3]<5.2.0.9.2.20050418185815.0303d0d0@mailsrvr.al.noaa.gov>
[4]<42654140.2080509@ucar.edu> [5]<42660091.9060600@awi-bremerhaven.de>
[6]<6.1.2.0.0.20050420101527.01d3f508@pop.uea.ac.uk>
[7]<42667322.4070101@ucar.edu>

Kevin,
Right on ! Assumes precip doesn't change - i.e. it's constant. Difficult to do
much more for some regions, but could do a lot better for the Alps. Ch 4 has swallowed
this hook, line and sinker and it is really a Ch 6 issue. Ch 6 wasn't even aware of it.
Can't decide who on Ch 4 knew about it as Oerlemans isn't there and the Swiss Glacier

people didn't know about the paper 2 weeks ago when I saw them.
I like the curve as does Mike Mann, but its not for any scientific reason.
Any jury is still out on whether this is right, but I'm glad someone has tried the
approach. It is a quantification of what people have assumed, but there likely isn't
enough detail in the paper to show how it was done.
I've not seen this paper in a proper issue of Science yet. As such I've not been
able to get the supporting material.
This paper is totally independent of all other paleo work. It is much better science
than Mobeg et al. in Nature in February. Susan has been sending a few emails to
Ch 6 about how to display the various millennium series - some of which she's not
thought through.
Just be glad we haven't got paleo in out chapter !
Cheers
Phil
At 16:20 20/04/2005, you wrote:

Hi Phil
I had not read Oerleman's paper, I have now. Some things don't make sense to me: chanes
in precip not included and the time series (esp N America) Also magnitude of implied
early 20Th C warming. What is your take?
Kevin
Phil Jones wrote:

Dear All,
In addition to Kevin's comments and from a quickish look through parts of Chapters
4, 6 and 9, here are a few suggestions.
First for best use of time, I would suggest that Cluster B gets broken

into two
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Basically separating off the overlap with the paleo and instrumental record including
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At 08:11 20/04/2005, Peter Lemke wrote:

Dear Martin,
I am also willing to co-chair the cluster B. (As always) Kevin has done a very good job
in listing the most important issues.

Therefore, I have nothing to add at the moment. I will think about this on the weekend.
Best regards,
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direct tel: +1 520 622-9065
fax: +1 520 792-8795
http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/

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References

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##########

From: trenbert@ucar.edu
To: "Martin Manning" <mmanning@al.noaa.gov>
Subject: Re: WG1 LA2 meeting - Overlap cluster A
Date: Wed, 20 Apr 2005 19:46:31 -0600 (MDT)
Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Peter Lemke" <plemke@awi-bremerhaven.de>,
"Susan Solomon" <ssolomon@al.noaa.gov>, ipcc-wg1@al.noaa.gov, k.briffa@uea.ac.uk

Martin I think you are right: the paleo instrumental issue is likely to
involve mainly Briffa from Chap 6 and Phil from our chapter, so they might
well spin off at some point. Are there others Phil?
Kevin

> Dear Kevin and Phil
>
> As you say Chapter 6 was not implicated in the cluster B overlap issues
> based on the author notes we received with the ZOD. You may want to cover
> the point raised by Phil and in particular where the long instrumental
> records fit, but as this seems to involve only a small number of LAs you
> could consider dealing with that more efficiently in a small group
> separately from the cluster meeting. So the choice is up to you.
>
> If it would be helpful, the TSU could start to compile a list of small
> group meetings requested by CLAs and look for some way of setting up a
> practical timetable for lunch time meetings. But we would need advice on
> the specific individuals who should be involved in each case and all I am
> offering is a "dating service" that would distribute a suggested list of
> times and names that we could possibly update in real time during the
> meeting in Beijing.
>
> Regards
> Martin
>
> At 09:07 AM 4/20/2005, Kevin Trenberth wrote:
>>Hi Martin
>>I agree with what Phil says, but I note that cluster B does not actually
>>have chapter 6 as part of it. So the question is whether chapter 6 will
>>be involved?. If so then we may well want to split into 2 parts. Last
>>night I had a quick look at Chap 9 and I am concerned about redundancy
>> and
>>overlap and conflicts: they are doing some similar things with
>>observations but maybe different obs, and coming to different conclusions
>>e.g. wrt things like dimming.
>>Kevin
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>>> parts of Chapters

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>>>UK
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>>**********
>>Kevin E. Trenberth   e-mail:
>><mailto:trenbert@ucar.edu>trenbert@ucar.edu
>>Climate Analysis Section,
>>NCAR   <http://www.cgd.ucar.edu/cas/>www.cgd.ucar.edu/cas/
>>P. O. Box 3000,   (303) 497 1318
>>Boulder, CO 80307   (303) 497 1333 (fax)
>>
>>Street address: 1850 Table Mesa Drive, Boulder, CO 80303
>>
>
> --
> Recommended Email address: mmanning@al.noaa.gov
> ** Please note that problems may occur with my @noaa.gov address
> Dr Martin R Manning, Director, IPCC WG I Support Unit
> NOAA Aeronomy Laboratory   Phone: +1 303 497 4479
> 325 Broadway, DSRC R/AL8   Fax: +1 303 497 5628
> Boulder, CO 80305, USA

512. 1114088225.txt
##########
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: trenbert@ucar.edu, "Martin Manning" <mmanning@al.noaa.gov>
Subject: Re: WG1 LA2 meeting - Overlap cluster A
Date: Thu Apr 21 08:57:05 2005
Cc: "Peter Lemke" <plemke@awi-bremerhaven.de>, "Susan Solomon"
<ssolomon@al.noaa.gov>, ipcc-wg1@al.noaa.gov, k.briffa@uea.ac.uk

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Cheers
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At 02:46 21/04/2005, trenbert@ucar.edu wrote:

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>>e.g. wrt things like dimming.
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>>

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---

513. 1114113870.txt
##########

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: Input for Chapter 6 in AR4
Date: Thu, 21 Apr 2005 16:04:30 +0200

<x-flowed>
>Hi Keith,

got this paper from Jens Hesselbjerg. Interesting
with respect to the von Storch story.
Eystein

>A few comments in English:
>We have used a different version of the MPI
>coupled modeling system from that described by
>von Storch et al. to simulate the last 500
>years. The model we have used has a different
>ocean component (OPYC in stead of HOPE) and a
>higher resolution in the atmosphere (T42 in
>stead of T31 - by many considered to be a
>substantial improvement in terms of representing
>synoptic behavior). Moreover, we have used
>different reconstructions of the external
>forcing. All these differnces leads to somewhat
>differnt behaviours compared to von Storch, and
>yet the model does seem to depict many of the
>observed major climatic events. Details are
>given in the paper.
>
>venlig hilsen
>Jens Hesselbjerg Christensen
>
>
>

--
_____
Eystein Jansen
Professor/Director
Bjerknes Centre for Climate Research and
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e-mail: eystein.jansen@geo.uib.no
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Attachment Converted: "c:\eudora\attach\stendel_et_al_ClimDyn_final.pdf"

514. 1114130226.txt
##

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: Re: WG1 LA2 meeting - Overlap cluster A
Date: Thu, 21 Apr 2005 20:37:06 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>
Hi Keith and Phil - Thanks. I read this to say that the issue of
pre-1860 instrumental data is figured out ok? Plan outlined below
sounds good if ok with you both.

Best, Peck

>Peck
>FYI
>Phil and have have talked about the need t adress (even if briefly)
>the pre 1860 climate data - and both feel that the overlap with the
>paleo records (see our 1st Figure) in the 2000 year section , is one
>place to address this - though more needs to be done about the
>regional bias in these data
>
>>X-Sender: f028@pop.uea.ac.uk
>>X-Mailer: QUALCOMM Windows Eudora Version 6.1.2.0
>>Date: Thu, 21 Apr 2005 08:57:05 +0100
>>To: trenbert@ucar.edu,"Martin Manning" <mmanning@al.noaa.gov>
>>From: Phil Jones <p.jones@uea.ac.uk>
>>Subject: Re: WG1 LA2 meeting - Overlap cluster A
>>Cc: "Peter Lemke" <plemke@awi-bremerhaven.de>,
>> "Susan Solomon" <ssolomon@al.noaa.gov>,ipcc-wg1@al.noaa.gov,
>> k.briffa@uea.ac.uk
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>>Prof. Phil Jones
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>>School of Environmental Sciences   Fax +44 (0) 1603 507784
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>
>--
>Professor Keith Briffa,
>Climatic Research Unit
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>Phone: +44-1603-593909
>Fax: +44-1603-507784
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>http://www.cru.uea.ac.uk/cru/people/briffa/

--
Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: CCNet: DEBUNKING THE "DANGEROUS CLIMATE CHANGE" SCARE
Date: Wed Apr 27 09:06:53 2005

Mike,
Presumably you've seen all this - the forwarded email from Tim. I got this email from
McIntyre a few days ago. As far as I'm concerned he has the data - sent ages ago. I'll
tell him this, but that's all - no code. If I can find it, it is likely to be hundreds of
lines of
uncommented fortran ! I recall the program did a lot more that just average the series.
I know why he can't replicate the results early on - it is because there was a variance
correction for fewer series.
See you in Bern.
Cheers
Phil
Dear Phil,

In keeping with the spirit of your suggestions to look at some of the other multiproxy
publications, I've been looking at Jones et al [1998]. The methodology here is obviously
more straightforward than MBH98. However, while I have been able to substantially emulate
your calculations, I have been unable to do so exactly. The differences are larger in the
early periods.

Since I have been unable to replicate the results exactly based on available materials, I
would appreciate a copy of the actual data set used in Jones et al [1998] as well as the
code used in these calculations.

There is an interesting article on replication by Anderson et al., some distinguished
economists, here [1]http://research.stlouisfed.org/wp/2005/2005-014.pdf discussing the
issue of replication in applied economics and referring favorably to our attempts in
respect to MBH98.

Regards, Steve McIntyre

X-Mailer: QUALCOMM Windows Eudora Version 6.2.0.14
Date: Tue, 26 Apr 2005 13:28:53 +0100
To: Phil Jones <p.jones@uea.ac.uk>,"Keith Briffa" <k.briffa@uea.ac.uk>
From: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: CCNet: DEBUNKING THE "DANGEROUS CLIMATE CHANGE" SCARE
Keith and Phil,
you both feature in the latest issue of CCNet:

(4) GLOBAL WARMING AND DATA
Steve Verdon, Outside the Beltway, 25 April 2005
[2]http://www.outsidethebeltway.com/archives/10200
A new paper ([3]http://research.stlouisfed.org/wp/2005/2005-014.pdf) from the St. Luis
Federal Reserve Bank has an interesting paer on how important it is to archive not only
the data but the code for empirical papers. While the article looks mainly at economic
research there is also a lesson to be drawn from this paper about the current state of
research for global warming/climate change. One of the hallmarks of scientific research
is that the results can be replicable. Without this, the results shouldn't be considered
valid let alone used for making policy.
Ideally, investigators should be willing to share their data and programs so as to
encourage other investigators to replicate and/or expand on their results.3 Such
behavior allows science to move forward in a Kuhn-style linear fashion, with each
generation seeing further from the shoulders of the previous generation.4 At a minimum,
the results of an endeavor-if it is to be labeled "scientific"-should be replicable,
i.e., another researcher using the same methods should be able to reach the same result.
In the case of applied economics using econometric software, this means that another
researcher using the same data and the same computer software should achieve the same
results.
However, this is precisely the problem that Steven McIntyre and Ross McKitrick have run
into since looking into the methodology used by Mann, Hughes and Bradely (1998) (MBH98),
the paper that came up with the famous "hockey stick" for temperature reconstructions.
For example, this post here shows that McIntyre was prevented from accessing Mann's FTP
site. This is supposedly a public site where interested researchers can download not
only the source code, but also the data. This kind of behavior by Mann et. al. is simply

unscientific and also rather suspicious. Why lock out a researcher who is trying to
verify your results...do you have something to hide professors Mann, Bradley and Huges?
Not only has this been a problem has this been a problem for McIntyre with regards to
MBH98, but other studies as well. This post at Climate Audit shows that this problem is
actually quite serious.
Crowley and Lowery (2000)
After nearly a year and over 25 emails, Crowley said in mid-October that he has
misplaced the original data and could only find transformed and smoothed versions. This
makes proper data checking impossible, but I'm planning to do what I can with what he
sent. Do I need to comment on my attitude to the original data being "misplaced"?
Briffa et al. (2001)
There is no listing of sites in the article or SI (despite JGR policies requiring
citations be limited to publicly archived data). Briffa has refused to respond to any
requests for data. None of these guys have the least interest in some one going through
their data and seem to hoping that the demands wither away. I don't see how any policy
reliance can be made on this paper with no available data.
Esper et al. (2002)
This paper is usually thought to show much more variation than the hockey stick. Esper
has listed the sites used, but most of them are not archived. Esper has not responded to
any requests for data. '
Jones and Mann (2003); Mann and Jones (2004)
Phil Jones sent me data for these studies in July 2004, but did not have the weights
used in the calculations, which Mann had. Jones thought that the weights did not matter,
but I have found differently. I've tried a few times to get the weights, but so far have
been unsuccessful. My surmise is that the weighting in these papers is based on
correlations to local temperature, as opposed to MBH98-MBH99 where the weightings are
based on correlations to the temperature PC1 (but this is just speculation right now.)
The papers do not describe the methods in sufficient detail to permit replication.
Jacoby and d'Arrigo (northern treeline)
I've got something quite interesting in progress here. If you look at the original 1989
paper, you will see that Jacoby "cherry-picked" the 10 "most temperature-sensitive"
sites from 36 studied. I've done simulations to emulate cherry-picking from persistent
red noise and consistently get hockey stick shaped series, with the Jacoby northern
treeline reconstruction being indistinguishable from simulated hockey sticks. The other
26 sites have not been archived. I've written to Climatic Change to get them to
intervene in getting the data. Jacoby has refused to provide the data. He says that his
research is "mission-oriented" and, as an ex-marine, he is only interested in a "few

good" series.
Jacoby has also carried out updated studies on the Gaspé series, so essential to MBH98.
I've seen a chronology using the new data, which looks completely different from the old
data (which is a hockey stick). I've asked for the new data, but Jacoby-d'Arrigo have
refused it saying that the old data is "better" for showing temperature increases. Need
I comment? I've repeatedly asked for the exact location of the Gaspé site for nearly 9
months now (I was going to privately fund a re-sampling program, but Jacoby, Cook and
others have refused to disclose the location.) Need I comment?
Jones et al (1998)
Phil Jones stands alone among paleoclimate authors, as a diligent correspondent. I have
data and methods from Jones et al 1998. I have a couple of concerns here, which I'm
working on. I remain concerned about the basis of series selection - there is an obvious
risk of "cherrypicking" data and I'm very unclear what steps, if any, were taken to
avoid this. The results for the middle ages don't look robust to me. I have particular
concerns with Briffa's Polar Urals series, which takes the 11th century results down
(Briffa arguing that 1032 was the coldest year of the millennium). It looks to me like
the 11th century data for this series does not meet quality control criteria and Briffa
was over-reaching. Without this series, Jones et al. 1998 is high in the 11th century.
Note that none of this actually "disproves" the global warming hypothesis. However, it
does raise very, very serious questions in my opinion. We are talking about enacting
policies to curb global warming that could cost not billions, but trillions of dollars.
Shouldn't we at least be allowed to see the source code, the data and ask for
replication at a minimum? I think the answer is simple: YES!!

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sunclock: [5]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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References

1. http://research.stlouisfed.org/wp/2005/2005-014.pdf
2. http://www.outsidethebeltway.com/archives/10200
3. http://research.stlouisfed.org/wp/2005/2005-014.pdf
4. http://www.cru.uea.ac.uk/~timo/
5. http://www.cru.uea.ac.uk/~timo/sunclock.htm

516. 1114785020.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: HC
Date: Fri Apr 29 10:30:20 2005

Ben,

Tom was here yesterday. He said you were going to the CCSP meeting for a day in Chicago, then flying on to the UK for the HC meeting May 18-19 (and 17th evening).
Do you still want to come on up to Norwich afterwards?
Glad to hear from Tom you've been writing up your CCSP chapter and extending it significantly. He gave me a brief summary. I signed off yesterday on the CCSP report. You should be getting it through Tom Karl later today, or by Monday. As I did Ch 5, if you want to check anything with me feel free to. I wasn't able to stop some comments being put in by Lindzen, but Tom has a paper as does Myles which are enough to ignore his and the Douglass papers.
Cheers
Phil

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517. 1115294935.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: ppt for LA2
Date: Thu May 5 08:08:55 2005

Apologies
Phil
Kevin,
Finally gotten around to putting thoughts down. Mostly on the challenges slides at the start. Maybe you would have said these things.
1. As well as suggesting the model chapters rank models (I don't think they will go with this - even though it is what we should be doing, and there are a whole raft of issues as to how to do it) should we also be dismissing observational papers

that are clearly wrong (or a distortion of the facts and emphasizing the wrong issues).
In some parts of our chapter, we omit the poor papers. Just stressing that we are doing an assessment and not a review. An assessment is our expert view of the science at the present.
For space limitations we must omit many papers, but we must do this objectively. In the NRC review I made the point that most of the papers reviewed were the author's own. It is difficult and we must not fall into that trap. All this again comes back to assessment/review.
With 3.4.1 we mustn't get caught up in having to agree with the CCSP VTT report. We're either doing OUR assessment or we might as well give up.
Gone on for long enough on that one.
2. I think we both believe we should be saying somewhere what we should be measuring (how accurately, where and with what). If we don't say this somewhere, AR5 will be in a worse state. Susan is against this, but I think on this point she's wrong. IPCC has a lot of clout - much more than GCOS and/or WMO. It should be saying something about what we should be doing.
3. Minor point, just land warming more than ocean, not much more.
4. I guess you've expanded on linear trends enough
5. The CCSP diagrams are good, but I'm not keen on running means. I guess though they wouldn't be too different with a better smoother.
6. I guess you'll raise map projections. Could add in the new one Dave has done for precip to show the 30E edge.
The additional slides. Most of these are from a talk I have to give in Bern next month. They relate mostly to issues with Ch 6. Maybe you can add a couple of them.They relate to the issues of:
- making full use of the instrumental records to compare with proxy records
- changes in seasonality
- was the few hundred years before 1850 always colder than the post 1920 period.
The first 2 are the longest European records. The period I'm interested in is the rise up from the late 17th century to the 1730s and then the year 1740. No volcanoes for 20-30 year period may be a factor, solar also, but nothing explains 1740. It is not just in CET. 1730s at CET and De Bilt is the warmest decade until the 1990s. Producing these sorts of things in proxy data is a key.
3rd slide is just some of these longer records filtered. They don't agree that well, so why should proxy series agree. We have more to learn from the early instrumental period.
4th is just a simple example of instrumental/proxy overlap. Highlights seasonality differences.
and 5th just shows how unusual the central European summer was in 2003 - if we wanted a figure for the box.
The interface with Ch 6 and the early instrumental period is crucial. 60% of

the
comments
on Ch 6 were on the 3-4 pages on the last millennium ! Ours weren't that distorted to
one
of our sections.
Issues at UEA and CRU haven't helped me get to 3.2 yet. I hope to by the end of the
day.
Cheers
Phil

At 15:26 03/05/2005, you wrote:

Phil
Did you look at and have comments/suggestion on the ppt for the last day in Beijing?
Kevin
--
****
Kevin E. Trenberth e-mail: trenbert@ucar.edu
Climate Analysis Section, NCAR [1]www.cgd.ucar.edu/cas/
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References

1. http://www.cgd.ucar.edu/cas/

518. 1115297153.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: Aiguo Dai <adai@cgd.ucar.edu>, Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: more on section 3.7 and Marengo
Date: Thu May 5 08:45:53 2005
Cc: Jim Renwick <j.renwick@niwa.co.nz>, Panmao Zhai <pmzhai@cma.gov.cn>, Matilde Rusticucci <mati@at.fcen.uba.ar>, "'David R. Easterling'" <david.easterling@noaa.gov>

Kevin et al,
The diagram looks too good to me. CRU's data are reasonable over Brazil for some of the period, but poor in others, particularly recently. So we would have difficulty in updating this because of station numbers and quality. We could try using the GPCC dataset. They have huge numbers of stations for Brazil, but only for specific regions and periods, so likely problems there also.
We have a couple of papers in submission to J. Hydrology on flows in the subcatchments of the Parana river, which are well reproduced by rainfall, evaporation and a catchment model. Agree with your concerns about the Amazon flows not agreeing with the rainfall. Do the NAR and SAR regions fully encompass the enormous catchment though.

Cheers
Phil

At 17:36 03/05/2005, Aiguo Dai wrote:

One can use the Chen et al. and CRU to produce similar type of plots to validate
Marengo's result.
He did use the CRU rainfall data set, but not for this particular plot.
Aiguo
Kevin Trenberth wrote:

Hi all
As you know we got some manuscripts from Jose Marengo to be considered in our chapter,
and he is a LA on another chapter and will be in Beijing. He has offerred to be CA.
My question concerns how good his data are? I asked Aiguo Dai to comment:
====
One of the interesting results from Marengo's work is that he found the Northern and
Southern Amazonia have opposite phase of decadal rainfall variations (see attached Fig.
from Marengo 2004, Ther. Appl. Climatol.): In the northern Amazonia, rainfall is above
normal during ~1945-1975 and below normal during ~1976-1998; and it is opposite in the
southern Amazonia. He suggested warmer SST in central and eastern Pacific contributed to
the dry conditions in the northern Amazonia during 1976-1998.
As noted in Betts et al. (2005, JHM, in press), Marengo's basin integrated rainfall
index does not correlate well with Amazon river flow during the recent decades (worse
than Chen et al.). This large multidecadal signal seems, however, robust.
=====
Certainly the attached figure is striking. Are we sure it is not due to changes in the
way observations are made? Do other datasets replicate this? The lack of relation with
river flow is a substantial concern. Matilde, can you provide informed commentary? If
the figure is good then maybe we should include it?
Kevin

--
Aiguo Dai, Scientist Email: adai@ucar.edu
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Norwich Email p.jones@uea.ac.uk
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UK

References

1. http://www.cgd.ucar.edu/cas/adai/

519. 1115843111.txt
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From: "Polychronis Tzedakis" <P.C.Tzedakis@leeds.ac.uk>
To: "Rainer Zahn" <rainer.zahn@uab.es>, "Thomas Stocker" <stocker@climate.unibe.ch>, "Atte Korhola" <atte.korhola@helsinki.fi>
Subject: RE: commission performance alpha 5
Date: Wed, 11 May 2005 16:25:11 +0100
Cc: <Eystein.Jansen@geo.uib.no>, <Imprint-partner@bjerknes.uib.no>, <beatriz.balino@bjerknes.uib.no>, <atle.nesje@geo.uib.no>, <oyvind.lie@geo.uib.no>, <john.birks@bio.uib.no>, <Carin.Andersson@bjerknes.uib.no>, <trond.dokken@bjerknes.uib.no>, <ulysses.ninnemann@geo.uib.no>, <Astrid.Bardgard@fa.uib.no>, <richard.telford@bjerknes.uib.no>

Dear all,
First of all a big hand for Eystein and all those who put in so much time into this task. Very disheartening to hear the outcome.

I have muych sympathy with what Rainer Zahn has said, especially on the Brussels front and the client relationships that are cultivated with EU officials.

I think that in addition to a letter to the EU, I would suggest that perhaps an editorial in NAture or something similar, outlining the growing degree of scepticism amongst scientists regarding the transparency of the EU funding process might be in order.

Chronis Tzedakis

-----Original Message-----
From: Rainer Zahn [mailto:rainer.zahn@uab.es]
Sent: Wed 5/11/2005 2:47 PM
To: Thomas Stocker; Atte Korhola
Cc: Eystein.Jansen@geo.uib.no; Imprint-partner@bjerknes.uib.no; beatriz.balino@bjerknes.uib.no; atle.nesje@geo.uib.no; oyvind.lie@geo.uib.no; john.birks@bio.uib.no; Carin.Andersson@bjerknes.uib.no; trond.dokken@bjerknes.uib.no; ulysses.ninnemann@geo.uib.no; Astrid.Bardgard@fa.uib.no; richard.telford@bjerknes.uib.no
Subject: commission performance alpha 5

dear Eystein, dear Imprint consortium,

I am sure I will not make many friends with what follows below. Firstly, it surely is sad and disheartening to see our proposal going down. and there are many issues involved some of which have been named in the recent emails. But then there are those issues left that have not been named but which I consider relevant if we are to make progress on the EU FWP front. Some of these issues may and will touch a personal nerve here and there, but let's face some of the unpleasant realities much rather than sitting back and keeping going with business as usual, a business that soon may go out of existence.

First, I am not convinced that Imprint was the best we could have done. On my side I was surprised to no small extent during our London meeting to see that those from the modeling community and other groups present obviously had no idea why our palaeo-component (a derivative of the planned ICON IP) was part of Imprint, and they were not overly favourable to listen and expand their views. So in a sense, even within our own consortium there was, perhaps still is a lack of insight and understanding as to what a

palaeo-component is about and will have to offer. In the end I am now left with the impression that ICON would have stood a good chance to survive on its own.

Second, as a member of the Imprint consortium I still find it difficult today to sort through this proposal and its various components, tasks, topics, milestones, deliverables etc. Which only tells me how ever so more difficult it must have been for outsiders i.e., reviewers to sift through the bits and pieces and comprehend what this is about. But I also feel that this has to do with the concept of IPs at large as it is not an easy task to compose an IP consortium of the dimension and wide range of expertise envisioned by the commission. The outcome of the whole process in my view confirms the notion that the concept of IPs has fundamentally (and to a large degree predictably) failed. This concept reflects a substantial lack of insight on the side of those who were, presumably still are involved in designing research policies in the commission about what science is about and how it works. Those parties should not be where they are, and they certainly should not be involved in setting up FWP7

This is what I have to say about our proposal.

As for the Commission's performance it is not my impression they are living up to their own standards that they have set up for the quality of proposals requested. In particular the proposal evaluation process is ridiculous and lacks any degree of substance. For instance, the reviews that I did receive in response to my RTN proposal (submitted last year) are mediocre at best, meaningless and useless in detail, beyond anything I would consider expert insight, simply a waste of time and tax payers' money. They are an insult to anybody who did contribute to and put work and effort into that proposal. As for the Impront proposal we now are faced with the prospect that the only IP proposal, Millennium, that is competing with Imprint from the outset was received more favourably than our own proposal. With this I could live were it not for the fact that in Millennium everything is named as a strategy and work plan that we were being advised to not do. This speaks a language of its own and to me reflects a fundamental lack of enthusiasm, professionalism and competence with those who give advice and organize the evaluation process. Obviously, the vision set out by our programme manager(s) never made it to the reviewers who seemed to follow quite different guidelines, if any.

Lastly, from what I can see around me, particularly in the Mediterranean club, it appears more important and beneficial to spend time in Brussels wiping door handles and leaving a professorial - directorial impression rather than composing upbeat cutting edge science proposals. It is ever so disheartening that within the FWP our success seems to depend more on who we know than the quality we present. Last time when programme managerial posts in the commission were reshuffled the primary concern around here was that "we now lose our contacts". This is wrong, a disgrace to our community.

I have had a few conversations with colleagues who were partners in EU proposals, both successful ones and ones that were rejected. From these conversations I sense a growing degree of tiredness about EU science policy and more so, about the chaotic way proposals are being solicited and then turned down on grounds that so very obviously have nothing to do with the science presented. There is also the notion that within the commission climate and paleo-work has fallen from grace, for reasons not known to many. Which brings me back to the point that perhaps we do not have the right programme managers in place to fend our cause.

I am prepared to write a firm letter to the commission, or to contribute to such letter, about the issues impinging on the poor performance of the commision. I rather do that before turning entirely into a full-grown Eurosceptic.

Rainer

Rainer Zahn, Professor de Recerca
Institució Catalana de Recerca i Estudis Avançats, ICREA
i Universitat Autònoma de Barcelona
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Phone: +34 - 93 581 4219
Fax: +34 - 93 581 3331
email: rainer.zahn@uab.es, rainer.zahn@icrea.es

<http://www.icrea.es/pag.asp?id=Rainer.Zahn>http://www.icrea.es/pag.asp?id=Rainer.Zahn

520. 1115887684.txt
##

From: Denis-Didier.Rousseau@uni-bayreuth.de
To: <Eystein.Jansen@geo.uib.no>, <Imprint-partner@bjerknes.uib.no>
Subject: [Fwd: RE: commission performance alpha 5]
Date: Thu, 12 May 2005 04:48:04 +0200 (MEST)

Dear all IMPRINT colleagues,
Being away from Europe, this was a very bad news that I got this morning listening about the rejection of IMPRINT. Eystein did a great job by being able to gather the European paleo community under a common umbrella and he desereves a lot of our consideration.
Concerning now the review process, I have been involved several times in Brussels and I have been able to see the evolution of the evaluating panel session after session.

I am not please with this evaluation and I already addressed my comments to Andre Berger. It is not normal that entering the room where you are supposed to meet the other "panelists" you would not know those who are supposed to be representative of your community, this is my first comment.

Second, the way the referees are selected is somehow strange and involve a political issue which is very sensitive as I'm sure you will understand that a country fair representation is not enough in our field which better involves expertise.

Third and last, having set a consortium of the leading Europe institutions and scientists, how can you expect appropriate expertise? I have been approached to join the evaluating panel but refused as being an IMPRINT member to respect some ethic. If, what I wish, we all didi that way, they one can sincerely expect the worst as I already experienced in a recent past.

Forth, complaining to the commission is a waste of time as these administrative people, even if this is you right, will always provide you with arguments to justify the decision. I complain once to the director of

the programme who just retun me that the referees of my proposal were relevant, what I know was not the case unfortunately. However I totally support the initiative to question the commission on the way the evaluations are performed, but also how the referees are selected.

Fifth, you all are waiting for the reviews. I agree with Rainer that the comments that are provided are useless and in somehow offending the PIs. This is mostly due to the review process and this again must be changed. Furthermore what we receive is the consensus report which passed in the European officers hands to be cleaned of any agressive sentences or words, and must remain politically correct. So effectively these reports are useless. It would be interesting to get also the individual reports on which the consensus one has been established and would better show the real work of every referee, and we would be very surprised sometimes.

Finaly to follow Thomas, Rainer and Eric, I would suggest to continue what has been launched with IMPRINT which is to my sense unique in gathering all the European paleo community under the same umbrella. May be the proposal was too broad, but this was following the commission's aim. The "Millenium" proposal benefited of several consecutive EU supports which apparently helped a lot. Their lobbying seem to have ben very efficient, not only in Brussels but in the journals and meetings. The Utrecht initiative was a good one which must stop today. We have the opportunity to gather regularly at least once during the EGU that we all are attending, why not using such opportunity to reinforce the initiative during such meeting?

All the very best to all of you

cheers

denis

-------- Ursprüngliche Nachricht --------
Betreff: RE: commission performance alpha 5
Von: "Polychronis Tzedakis" <P.C.Tzedakis@leeds.ac.uk>
Datum: Mit, 11.05.2005, 17:25
An: "Rainer Zahn" <rainer.zahn@uab.es>,
"Thomas Stocker" <stocker@climate.unibe.ch>,
"Atte Korhola" <atte.korhola@helsinki.fi>

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To: Thomas Stocker; Atte Korhola

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Phone: +34 - 93 581 4219
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<http://www.icrea.es/pag.asp?id=Rainer.Zahn>http://www.icrea.es/pag.asp?id=Rainer.Zahn

521. 1116017259.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: Katarina Kivel <kivel@stanford.edu>
Subject: Re: Stephen Schneider's request for review of Wahl-Ammann paper on MBH Robustness for Climatic Change
Date: Fri May 13 16:47:39 2005

Katerina,

I will be able to review this, despite just coming back from IPCC.
Cheers
Phil
At 20:04 12/05/2005, you wrote:

Dear Phil,
Attached is a letter from Steve Schneider requesting review of the above referenced
paper, which is also sent as an attachment (ms and four figures).
Please acknowledge receipt and let us know if you need a hard copy.
Regards,
Katarina
Katarina Kivel
Assistant Editor, CLIMATIC CHANGE
Department of Biological Sciences
Stanford University
Stanford, California 94305-5020
TEL 650-725-6508
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Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK
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522. 1116363805.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: [Fwd: Re: URGENT : IMPRINT en RTN ?]
Date: Tue May 17 17:03:25 2005

Eystein
We have now heard from Hans Brelen that Millennium will definitely be funded . This means
that the very worst case scenario has been realised - because it means that the EU are not
likely to call for any palaeoclimate in the next funding round.
I have to say that though there is normally an unfortunate element of randomness in the
refereeing of EU proposals , that to a large extent is unfortunate but inevitable, I
believe strongly that the system has let us down very badly in this case. It is clear that
we, the IMPRINT community were misled ; first by Ib Troen's direction (given publicly in
Utrecht) that we should produce a proposal which was of the scale to unify the whole
Palaeoclimate community , with a specific role to bring data and modelling foci to bear on
the issue of climate predictability; that we should be careful to not to over-emphasise the
collection of new data but rather work mostly to consolidate and jointly interpret existing
data , and that we should formulate a scheme were these fed directly into a

hierarchy of
modelling that would address model viability and issues of probability of future climate
and its causes.
Secondly, We were misled by the accepting , on the basis of the published call, that the EU
required IP proposals of ambitious scope , large enough to move the science of European
palaeoclimate forward as a whole and with relevance to globally important issues, with
aims clearly beyond the scope of "slightly bigger STREPS" . On reading the cursory
referees' responses to our proposal , I am also moved to express my own opinion that they
are an insult to the community of researchers that constitute IMPRINT , and an indictment
of the failure of the referees to address their assessment to the generally publicized aims
of the IP concept. To describe the whole proposal as "too complicated", and to state that
there is " no value" in the first four workpackages , and most of all to rate the quality
of the consortium as 4 out of 5 , all require explicit justification well beyond the few
lines with which we are presented.
While I have no ill will at all regarding the competing proposal Millennium , I feel that
the extended IMPRINT community can justifiably ask very serious questions regarding the
apparent lack of equitable assessment of the two proposals in the light of the published
call requirements - the efforts of the IMPRINT consortium over recent months at least
deserve answers as to how , for the sake of 0.5 of a mark , that proposal will be funded
when it clearly did not address the scope of the original call - in terms of community
integration, emphasis on wider data consolidation, scope of model hierarchy, and specific
addressing of the data/model integration towards the issue of climate sensitivity/predictability.
Expressing these concerns should not be considered "sour grapes " . They are not and I
congratulate the MILLENNIUM team on having succeeded . Rather these comments are justified
because the review process has not taken account of the scope of the IP concept, and the
need to invoke a research plan with the necessary breadth and expertise (and proven
managerial ability - as can be gauged by the assessment of the CARBO OCEAN coordination
plan) , and because the success of the much more limited MILLENNIUM project has already
been cited by European officials as justification for the lack of any need to fund
palaeoclimate research in the next call - effectively cutting off the wider paleoclimate
community from EU research support for the next few years.
I believe we are justified in questioning the operation of the IP concept , beyond the EU
administration, which has , in my opinion has done a serious dis-service to our community
and palaeoclimate in general.

At 08:26 16/05/2005, Valérie Masson-Delmotte wrote:

Dear Eystein and Keith,
I hope that you had a good trip back from Beijing. On our side it was a bit hectic (3
hours delay in Amsterdam, arrival at midnight in Paris and lost of Pascale's luggage
without ability to find where it was lost!).
I have just received this suggestion from a CEA EC representative that there is a RTN
Marie Curie call for september 8th which has a lot of funding - 220 Meuros. You can
apply for up to 6 M euros for a series of PhD thesis and postdocs around a real research
network (up to 3-4 contracts per participant).
I think that it is an excellent idea... if you and your people, Eystein, would be ready
to put some more energy in the proposal.
It would require to re think about the scientific perimeter and the key partners maybe.
Sincerely
Valerie.

Return-Path: <Jean.jouzel@cea.fr>
Received: from muguet.saclay.cea.fr (muguet.saclay.cea.fr [132.166.192.6])
by dsm-mail.saclay.cea.fr (8.12.11/jtpda-5.4) with ESMTP id j4G6I6mU023329
for <masson@dsm-mail.saclay.cea.fr>; Mon, 16 May 2005 08:18:06 +0200
Received: from cincidele.saclay.cea.fr (cincidele.saclay.cea.fr [132.166.192.111])
by muguet.saclay.cea.fr (8.12.10/8.12.10/CEAnet-internes.4.0) with ESMTP id
j4G6I7Tt016417
for <masson@dsm-mail.saclay.cea.fr>; Mon, 16 May 2005 08:18:07 +0200 (MEST)
Received: from agrione.extra.cea.fr (unverified) by cincidele.saclay.cea.fr
(Content Technologies SMTPRS 4.3.17) with ESMTP id
<T70f0fd935584a6c06f85c@cincidele.saclay.cea.fr> for <masson@dsm-mail.saclay.cea.fr>;
Mon, 16 May 2005 08:18:07 +0200
Received: from cirse.extra.cea.fr (cirse.extra.cea.fr [132.166.172.102])
by agrione.extra.cea.fr (8.12.11/8.12.11) with ESMTP id j4G6FXcJ010248;
Mon, 16 May 2005 08:15:33 +0200
(envelope-from jouzel@dsm-mail.saclay.cea.fr)
Received: from shiva.jussieu.fr (shiva.jussieu.fr [134.157.0.129])
by cirse.extra.cea.fr (8.12.10/8.12.10/CEAnet-Internet.4.0) with ESMTP id
j4G6I5AN028850;
Mon, 16 May 2005 08:18:05 +0200 (MEST)
Received: from [134.157.81.169] (169.ext.jussieu.fr [134.157.81.169])
by shiva.jussieu.fr (8.12.11/jtpda-5.4) with ESMTP id j4G6I069096644
; Mon, 16 May 2005 08:18:03 +0200 (CEST)
X-Ids: 165
Mime-Version: 1.0
X-Sender: jzipsl@mailhost.ipsl.jussieu.fr (Unverified)
Message-Id: <v04220801beae642fdb0b@[134.157.81.184]>
In-Reply-To:
<C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>
References: <C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>
Date: Mon, 16 May 2005 07:57:43 -0700
To: CAMINADE Jean Pierre <CAMINADE@dsmdir.cea.fr>
From: Jean Jouzel <Jean.jouzel@cea.fr>
Subject: Re: URGENT : IMPRINT en RTN ?
Cc: masson@dsm-mail.saclay.cea.fr

Content-Type: multipart/alternative;
boundary="============_-1095865763==_ma============"
X-Greylist: Sender IP whitelisted, not delayed by milter-greylist-1.7.2
(shiva.jussieu.fr [134.157.0.165]); Mon, 16 May 2005 08:18:05 +0200 (CEST)
X-Antivirus: scanned by sophie at shiva.jussieu.fr
X-Miltered: at dsm-mail with ID 42883B1E.000 by Joe's j-chkmail
([1]http://j-chkmail.ensmp.fr)!
X-Miltered: at shiva.jussieu.fr with ID 42883B18.001 by Joe's j-chkmail
([2]http://j-chkmail.ensmp.fr)!
X-CEA-Source: externe
X-CEA-DebugSpam: 13%
X-CEA-Spam-Report: The following antispam rules were triggered by this message:
Rule Score Description
DATE_IN_FUTURE_06_12 1.300 Date: is 6 to 12 hours after
Received: date
X-CEA-Spam-Hits: DATE_IN_FUTURE_06_12 1.3, __CT 0, __CTYPE_HAS_BOUNDARY 0,
__CTYPE_MULTIPART 0, __CTYPE_MULTIPART_ALT 0, __HAS_MSGID 0, __MIME_VERSION 0,
__SANE_MSGID 0
X-Spam-Checker-Version: SpamAssassin 2.64 (2004-01-11) on dsm-mail.cea.fr
X-Spam-Level:
X-Spam-Status: No, hits=-2.9 required=4.0 tests=BAYES_00,DATE_IN_FUTURE_06_12
autolearn=no version=2.64
Cher Jean - Pierre,
Excuse-moi de réagir un peu tardivement (je reviens de Chine).
Mais surtout merci pour ce courrier et l'aide proposée ; je pense
vraiment que
cela vaudrait le coup de le relancer sous la forme RTN et que l'obtention de
post-docs
correspond bien à l'idée d'imprint (exploitation des données, modélisation).
Pour faire avancer les choses je mets copie à Valérie Masson - Delmotte
une des
chevilles ouvrières d'IMPRINT au LSCE. Je suggère à valérie de te contacter
directement.
Bien amicalement Jean

Bonjour Jean,
J'ai appris ce matin au GTN environnement qu'IMPRINT n'avait pas été accepté.
Avez-vous pensé à le relancer sous la forme d'un (ou de plusieurs) RTN-Marie
Curie
(Research Training Network) pour l'appel du 8 septembre qui est richement doté
(220
MEuros ! du jamais vu !); le montant demandé peut aller jusqu'à 6 MEuros, pas
très loin
d'IMPRINT.
Il s'agit de proposer une série de post-docs et de thèses articulés autour d'un
véritable projet de recherche; environ 3 à 4 CDD pour chaque participant.
La DSM a une expérience dans ce domaine (Greencycles rien qu'au LSCE); on peut
t'aider à
te monter un projet taillé sur mesures.
Aujourd'hui je ne vois que cette solution car manifestement la ligne
"modélisation
climat" ne repassera pas au 4ème appel et je ne vois rien d'autre d'aussi bien
"doté"
dans le paysage du FP6 (qui est sur sa fin).
Cordialement
JPC

<[3]http://promos.hotbar.com/promos/promodll.dll?RunPromo&El=&SG=&RAND=25607&partner
=hot
bar>

- Université de Versailles Saint-Quentin
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</blockquote></x-html>

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[4]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://j-chkmail.ensmp.fr/
2. http://j-chkmail.ensmp.fr/
3. http://promos.hotbar.com/promos/promodll.dll?RunPromo&El=&SG=&RAND=25607&partner=hotbar
4. http://www.cru.uea.ac.uk/cru/people/briffa/

523. 1116365074.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: IMPRINT
Date: Tue May 17 17:24:34 2005
Cc: Ib Troen

Eystein
We have now heard that Millennium will definitely be funded . This means that the very
worst case scenario has been realised - because it means that the EU are not likely to call
for any palaeoclimate in the next funding round.
I have to say that, though there is normally an element of randomness in the refereeing of
EU proposals , that to a large extent is unfortunate but inevitable, I believe strongly
that the system has let us down very badly in this case.
It is clear that we, the IMPRINT community were misled ; first by Ib Troen's direction
(given publicly in Utrecht) that we should produce a proposal which was of the scale to
unify the whole Palaeoclimate community , with a specific role to bring data and

modelling
foci to bear on the issue of climate predictability; that we should be careful to not to
over-emphasise the collection of new data but rather work mostly to consolidate and jointly
interpret existing data , and that we should formulate a scheme where these are fed
directly into a hierarchy of modelling experiments that would address causes of climate
change, model viability and issues of probability of future climate and its causes.
Secondly, We were misled by the accepting , on the basis of the published call, that the EU
required IP proposals of ambitious scope , large enough to move the science of European
palaeoclimate forward as a whole and with relevance to globally important issues, with
aims clearly beyond the scope of "slightly bigger STREPS" . On reading the cursory
referees' responses to our proposal , I am also moved to express my own opinion that they
are an insult to the community of researchers that constitute IMPRINT , and an indictment
of the failure of the referees to address their assessment to the generally publicised aims
of the IP concept.
To describe the whole proposal as "too complicated", and to state that there is " no value"
in the first four workpackages , and most of all , to rate the quality of the consortium as
4 out of 5 , all require explicit justification well beyond the few lines with which we
are presented.
While I have no ill will at all regarding the competing proposal Millennium , I feel that
the extended IMPRINT community can justifiably ask very serious questions regarding the
apparent lack of equitable assessment of the two proposals in the light of the published
call requirements - the efforts of the IMPRINT consortium over recent months at least
deserve answers as to how , for the sake of 0.5 of a mark , that proposal will be funded
when it clearly did not address the scope of the original call - in terms of community
integration, emphasis on wider data consolidation, scope of model hierarchy, and specific
addressing of the data/model integration towards the issue of climate sensitivity/predictability.
Expressing these concerns should not be considered "sour grapes " . They are not and I
congratulate the MILLENNIUM team on having succeeded . They will do valuable research.
Rather these comments are justified because the review process has not taken account of the
scope of the IP concept, and the need to invoke a research plan with the necessary breadth
and expertise (and proven managerial ability - as can be gauged by the assessment of the
CARBO OCEAN coordination plan) , and because the success of the much more limited
MILLENNIUM project has already been cited by European officials as justification for the
lack of any need to fund palaeoclimate research in the next call - effectively

cutting off
the wider palaeoclimate community from EU research support for the next few years.
I believe we are justified in questioning the operation of the IP concept , and questioning
it in fora beyond the circle of EU administration, which has , in my opinion has done a
serious dis-service to our community and palaeoclimate in general. At the very least , the
"goalposts" regarding IP proposals seem to have been moved and the time of many researchers
has been wasted.
Please feel free to forward this message to the rest of our group .
At 08:26 16/05/2005, Valérie Masson-Delmotte wrote:

Dear Eystein and Keith,
I hope that you had a good trip back from Beijing. On our side it was a bit hectic (3
hours delay in Amsterdam, arrival at midnight in Paris and lost of Pascale's luggage
without ability to find where it was lost!).
I have just received this suggestion from a CEA EC representative that there is a RTN
Marie Curie call for september 8th which has a lot of funding - 220 Meuros. You can
apply for up to 6 M euros for a series of PhD thesis and postdocs around a real research
network (up to 3-4 contracts per participant).
I think that it is an excellent idea... if you and your people, Eystein, would be ready
to put some more energy in the proposal.
It would require to re think about the scientific perimeter and the key partners maybe.
Sincerely
Valerie.
Return-Path: <Jean.jouzel@cea.fr>
Received: from muguet.saclay.cea.fr (muguet.saclay.cea.fr [132.166.192.6])
by dsm-mail.saclay.cea.fr (8.12.11/jtpda-5.4) with ESMTP id j4G6I6mU023329
for <masson@dsm-mail.saclay.cea.fr>; Mon, 16 May 2005 08:18:06 +0200
Received: from cincidele.saclay.cea.fr (cincidele.saclay.cea.fr [132.166.192.111])
by muguet.saclay.cea.fr (8.12.10/8.12.10/CEAnet-internes.4.0) with ESMTP id
j4G6I7Tt016417
for <masson@dsm-mail.saclay.cea.fr>; Mon, 16 May 2005 08:18:07 +0200 (MEST)
Received: from agrione.extra.cea.fr (unverified) by cincidele.saclay.cea.fr
(Content Technologies SMTPRS 4.3.17) with ESMTP id
<T70f0fd935584a6c06f85c@cincidele.saclay.cea.fr> for <masson@dsm-mail.saclay.cea.fr>;
Mon, 16 May 2005 08:18:07 +0200
Received: from cirse.extra.cea.fr (cirse.extra.cea.fr [132.166.172.102])
by agrione.extra.cea.fr (8.12.11/8.12.11) with ESMTP id j4G6FXcJ010248;
Mon, 16 May 2005 08:15:33 +0200
(envelope-from jouzel@dsm-mail.saclay.cea.fr)
Received: from shiva.jussieu.fr (shiva.jussieu.fr [134.157.0.129])
by cirse.extra.cea.fr (8.12.10/8.12.10/CEAnet-Internet.4.0) with ESMTP id
j4G6I5AN028850;
Mon, 16 May 2005 08:18:05 +0200 (MEST)
Received: from [134.157.81.169] (169.ext.jussieu.fr [134.157.81.169])
by shiva.jussieu.fr (8.12.11/jtpda-5.4) with ESMTP id j4G6I069096644

; Mon, 16 May 2005 08:18:03 +0200 (CEST)
X-Ids: 165
Mime-Version: 1.0
X-Sender: jzipsl@mailhost.ipsl.jussieu.fr (Unverified)
Message-Id: <v04220801beae642fdb0b@[134.157.81.184]>
In-Reply-To:
<C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>
References: <C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>
Date: Mon, 16 May 2005 07:57:43 -0700
To: CAMINADE Jean Pierre <CAMINADE@dsmdir.cea.fr>
From: Jean Jouzel <Jean.jouzel@cea.fr>
Subject: Re: URGENT : IMPRINT en RTN ?
Cc: masson@dsm-mail.saclay.cea.fr
Content-Type: multipart/alternative;
boundary="============_-1095865763==_ma============"
X-Greylist: Sender IP whitelisted, not delayed by milter-greylist-1.7.2
(shiva.jussieu.fr [134.157.0.165]); Mon, 16 May 2005 08:18:05 +0200 (CEST)
X-Antivirus: scanned by sophie at shiva.jussieu.fr
X-Miltered: at dsm-mail with ID 42883B1E.000 by Joe's j-chkmail
([1]http://j-chkmail.ensmp.fr)!
X-Miltered: at shiva.jussieu.fr with ID 42883B18.001 by Joe's j-chkmail
([2]http://j-chkmail.ensmp.fr)!
X-CEA-Source: externe
X-CEA-DebugSpam: 13%
X-CEA-Spam-Report: The following antispam rules were triggered by this message:
Rule Score Description
DATE_IN_FUTURE_06_12 1.300 Date: is 6 to 12 hours after
Received: date
X-CEA-Spam-Hits: DATE_IN_FUTURE_06_12 1.3, __CT 0, __CTYPE_HAS_BOUNDARY 0,
__CTYPE_MULTIPART 0, __CTYPE_MULTIPART_ALT 0, __HAS_MSGID 0, __MIME_VERSION 0,
__SANE_MSGID 0
X-Spam-Checker-Version: SpamAssassin 2.64 (2004-01-11) on dsm-mail.cea.fr
X-Spam-Level:
X-Spam-Status: No, hits=-2.9 required=4.0 tests=BAYES_00,DATE_IN_FUTURE_06_12
autolearn=no version=2.64
Cher Jean - Pierre,
Excuse-moi de réagir un peu tardivement (je reviens de Chine).
Mais surtout merci pour ce courrier et l'aide proposée ; je pense
vraiment que
cela vaudrait le coup de le relancer sous la forme RTN et que l'obtention de
post-docs
correspond bien à l'idée d'imprint (exploitation des données, modélisation).
Pour faire avancer les choses je mets copie à Valérie Masson - Delmotte
une des
chevilles ouvrières d'IMPRINT au LSCE. Je suggère à valérie de te contacter
directement.
Bien amicalement Jean

Bonjour Jean,
J'ai appris ce matin au GTN environnement qu'IMPRINT n'avait pas été accepté.
Avez-vous pensé à le relancer sous la forme d'un (ou de plusieurs) RTN-Marie
Curie
(Research Training Network) pour l'appel du 8 septembre qui est richement doté
(220
MEuros ! du jamais vu !); le montant demandé peut aller jusqu'à 6 MEuros, pas
très loin
d'IMPRINT.
Il s'agit de proposer une série de post-docs et de thèses articulés autour d'un
véritable projet de recherche; environ 3 à 4 CDD pour chaque participant.
La DSM a une expérience dans ce domaine (Greencycles rien qu'au LSCE); on peut
t'aider à
te monter un projet taillé sur mesures.
Aujourd'hui je ne vois que cette solution car manifestement la ligne

"modélisation
climat" ne repassera pas au 4ème appel et je ne vois rien d'autre d'aussi bien "doté"
dans le paysage du FP6 (qui est sur sa fin).
Cordialement
JPC

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References

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2. http://j-chkmail.ensmp.fr/
3. http://promos.hotbar.com/promos/promodll.dll?RunPromo&El=&SG=&RAND=25607&partner=hotbar
4. http://www.cru.uea.ac.uk/cru/people/briffa/

524. 1116426671.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein.Jansen@geo.uib.no
Subject: wishing to talk
Date: Wed May 18 10:31:11 2005

so can you give me a number where I can reach you - after your meeting . I am in and out
trying to do various things , but wish to discuss "next steps" . Did you get my email last

evening?
Keith

--
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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

525. 1116440198.txt
##########

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: imprint
Date: Wed, 18 May 2005 14:16:38 +0200

<x-flowed>
Hi Keith,
for your information, I have enclosed the letter
received on the outcome of phase 1, and the
guidance for Stage 2. We will dig up more.
I also talked with Christoph Heinze who said this
definately has the flair of someone in the
review panel having an agenda of revenge, and
that this could be an element of a formal
complaint.

More later,
Eystein

--
_____
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NORWAY
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</x-flowed>

Attachment Converted: "c:\eudora\attach\IMPRINT_QI_letter 1.pdf"

Attachment Converted: "c:\eudora\attach\IMPRINT_ESR 1.pdf"

526. 1116611126.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Empire Strikes Back - return of proper science !
Date: Fri May 20 13:45:26 2005

Mike,

Just reviewed Caspar's paper with Wahl for Climatic Change. Looks pretty good.
Almost reproduced your series and shows where MM have gone wrong. Should keep them quiet for a while. Also they release all the data and the R software. Presume you know all about this. Should make Keith's life in Ch 6 easy !
Also, confidentially for a few weeks, Christy and Spencer have admitted at the Chicago CCSP meeting that their 2LT record is wrong !! They used the wrong sign for the diurnal correction ! Series now warms - not quite as much as the surface but within error bands. Between you and me, we'll be going with RSS in Ch 3 and there will be no discrepancy with the surface and the models. Should make Ch 3 a doddle now ! Keep quiet about this until Bern at least. Can tell you more then.
RSS (Carl Mears and Frank Wentz) found the mistake !
The skeptic pillars are tumbling !
Cheers
Phil

Prof. Phil Jones
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School of Environmental Sciences Fax +44 (0) 1603 507784
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527. 1116646247.txt
##########

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: imprint-ssc@bjerknes.uib.no
Subject: Urgent-next step
Date: Fri, 20 May 2005 23:30:47 +0200
Cc: stocker@climate.unibe.ch, André Berger <berger@astr.ucl.ac.be>

<x-flowed>
Dear friends of the Imprint - SSC,

After seeing the evaluation summary of our proposal, and not least the same for Millennium, it is clear to me that we have been very badly treated, first by the public advice from the Commission in Utrecht who advised the community to create a proposal which we did, but which is orthogonal to what they now have decided to negotiate, later by the random way we were reviewed and the many inconsistencies in the evaluation. Compared to this the Millennium review was full of subjective phrases and a

number of negative aspects were glossed over. The review is an insult, and it appears likely that elements in the panel bear some grudges against our community. In order to get the 0.5 point difference between Imprint and Millennium they had to give a number of very imbalanced statements. They also had to raise the management score of Millennium to 4 by the xtended panel despite critisisms by the reviewers that the management was not well laid out.

I feel that the review was very biased and the result is that they will probably fund a project with only limited relevance to the call, and miss a major opportunity of integrating European paleoclimate research and climate modelling and create a new major step forward.

We have been advised to send a formal letter of complaint to the Commission, asking for a renewed evaluation, not because we think there is a good chance that it will lead to much, but we think it is important that they know that they have upset a community consisting of top level European scientists, This may help us in the longer term.

The advice I have got is to send this to Pierre Valette, co-signed by the key partners, both their PIs and head of administration, with copies to our individual national members of the Global Change Panel of the EU.
So far there is no formal decision on which proposal to fund, this may happen in September after negotiations with the selected proposals. There is a seldom precedence in Europe that such an intervention has been successful, but very rarely.

In phrasing such a letter we have to be very careful and make sure our message is clear and fair, but I think it needs to be done.

I would therefore ask you to respond immediately to this mail as to whether you think we should go this route or not. We will then in a few days send out a draft for comments, if you agree that we shall send in a complaint. We have to move fast here, so I hope you will be quick.

Concerning the other proposals on what to do, there are many good ideas, and I think we should have a meeting in the autumn to discuss the strategy of securing paleo in the 7th Framwork program. The text is out for review now, and we all need to suggest changes through our national representatives. I will distribute a list of who this is for the various countries over the week-end.
I am also working on formulating text to help launch our ideas in teh European Parliament via Atte´s wife.
Best wishes,

Eystein
</x-flowed>

528. 1116902771.txt
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##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: IPCC - your section
Date: Mon, 23 May 2005 22:46:11 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Hi Keith - thanks again for the help in Beijing. We hope you found a
fabulous clay pot or at least some good views of China.

We know it's going to be extra hard on you to get everything done on
time, but we're hoping you can more-or-less stick to the schedule we
just sent around. Your section is going to be the big one, and we
need to make sure we have as much review and polishing as possible.
If we don't we (especially you) will pay heavily at FOD review time.
Lots of work now saves even more work later. Or so the real veterans
tell us.

Lastly, we wanted you to know that we can probably win another page
or two (total, including figs and refs) if you end up needing it.
Susan didn't promise this, but she gave us the feeling that we could
get it if we ask - but probably only for your section, and maybe an
extra page for general refs (although we're not going to mention this
to the others, since we're not sure we can get it). Note that some of
the methodological parts of your sections should go into supplemental
material - this has to be written just as carefully, but it gives you
another space buffer. All this means you can do a good job on
figures, rather than the bare minimum. We're hoping you guys can
generate something compelling enough for the TS and SPM - something
that will replace the hockey-stick with something even more
compelling.

Anyhow, thanks in advance for what is most likely not going to be
your number 1 summer to remember. That said, what we produce should
provide real satisfaction.

Best, Peck and Eystein
--
Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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</x-flowed>

529. 1117120511.txt
##

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: Ch 3
Date: Thu May 26 11:15:11 2005

Kevin,
I'll broach it with the UK people. Need to consider timing in November, once we
get the comments or maybe after the ChCh meeting. Been to Boulder in Jan and Feb
before so know what to expect ! Early Feb would seem best. Not thought about
going to the AMS so won't.
A few problems with Figures today. Hopefully they will get resolved in the not too
distant future. Dave E has at least sent one email.
Seeing our granddaughter on Saturday, but should have some good time for
the Chapter on Sunday and Monday (at home).
Cheers
Phil
At 17:11 25/05/2005, you wrote:

Hi Phil
I am attaching the updated Fig 3.4.? I have also in .ps that can be converted if need
be.
Dennis has also plotted the Fu data and I'll send a version a bit later. But need to
have consistent colors.
I am encouraged that the text is getting a lot better. The FOD is approaching close to
what will be final, we should find. After that point the figs should only be updates
and minor changes, and the text is modified to respond to comments, that we will have to
address more systematically next time. The SOD does become close to final: still
subject to all the reviews and late breaking material.
Key thing is for you and me to make sure we converge, and don't do a wholesale
replacement of a section without careful checking.
I have decided not to attend AMS AGM next year in January so that I can work on the
SOD. I would be glad to invite you to come for a visit for a week and I suspect we can
also come up with some funds to help: at the price of a seminar. e.g. we could split it
by you doing airfare and we do local accommodation or vice versa? This summer Tom
Stocker is here and working with Jerry on chap 10. I think it could be worthwhile, main
question is best timing. Perhaps late Jan or early Feb? That time of year can be cold
here: usually not that much snow or if it does snow it does not last long in Boulder:
great skiing nearby if you are interested in that. Mean T in Jan is about 0C but highs
not uncommon about 10C, and have been over 20C with chinook. Cold at night. So good
idea.

Cheers
Kevin
Phil Jones wrote:

Kevin,
Things seem to be coming in. Will work on 3.5-3.7 tomorrow. 3.2 and the Appendices now back with David. The Appendices read pretty good - lots of useful background material. It will be shame to lose it to a web site. Once David gets these back these should be almost good enough to go out to all on July 15 (or whenever we said).
A thought kept recurring - there must be a better way to do this ! Although the FOD reviews will be different from the ZOD (and many more), I'm prepared to come to Boulder for a week in early 2006 if needed. I think I can get the money from the UK to do this. Question is will be it be worthwhile. Better if we were both locked away somewhere other than one of our institutions, but then we wouldn't have the infrastructure, support (email, printers etc).
Anyway, give it some thought. You'll know more than I do about some much the FOD and SOD change. Q is whether a week or a fortnight is sufficient. If we knew that a few of the key people in the chapter were at their desks, the text should show a marked improvement.
Assuming here the majority of the Figures set by then - just a few need updating.
Cheers
Phil
At 17:03 24/05/2005, you wrote:

Hi Phil
Thanks for update: monday is a holiday here: Memorial Day, seems weird that Brian is working?
My approach to the revisions at this stage is not to take the material sent and wholesale replace it, but cautiously compare and insert if it makes sense. i.e. you and I need to act as editors with a fairly strong hand. I suspect 3.7 may have some useful material but it could degrade the section by further adding material that is not especially relevant. I'll bet it does not shorten it, which is desired still.
I am clearly not on same page as Brian wrt clouds and radiation, and I am interested in his take on it all, given the new material and changes. I am not a fan of Norris' stuff. We have updated Fig 3.4.1 on water vapor thru 2004: the ocean trend drops to 1.2%/decade. So you can help a lot by putting your take on the 3.4 stuff: it may also require some careful wording to accommodate different views if we can't see eye to eye.
For instance, on the dimming, the recent Pinker paper uses ISCCP and I simply

don't
believe the trends from ISCCP at all. Saying Wielicki and ISCCP agree actually damns
them both. Or similarly saying Norris and ISCCP agree causes problems (this relates to
upper cloud, which Norris gets from total minus lower, but those two sets of data are
not homogeneous: there is not a lower cloud ob for every total; using means, esp zonal
means without differencing each ob potentially causes major problems).
Dennis is starting on the 3.6 figs today plus the Sahel one.
Cheers
Kevin

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References

1. mailto:p.jones@uea.ac.uk
2. mailto:trenbert@ucar.edu
3. http://www.cgd.ucar.edu/cas/

530. 1117134760.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "David Easterling" <David.Easterling@noaa.gov>
Subject: Re: Fig. 3.7.1
Date: Thu May 26 15:12:40 2005
Cc: david.parker@metoffice.gov.uk, pmzhai@cma.gov.cn, Kevin Trenberth <trenbert@cgd.ucar.edu>

Dave,
Thanks for the update on the maps. Can you calculate a CRU time series from what you have? Exactly which dataset do you have? Is it CRU TS 2.0? If this is

it
then OK. This is the infilled one, so variance may be a little low in early years.
Hopefully your calculations will agree with Aiguo. I don't have anyone here to do this
at the moment. There seem a lot of deadlines at the moment here, which is making
it hard for me to find quality time for Ch3. Luckily there is a holiday weekend coming
up and I hope to use that to get 3.5-3.7 looked over. 3.2 is now done and agreed
with David. I'll tweak anything when I get your spatial maps. I came in with good
intentions today, but have been answering emails and seeing students.
As for smoothing, we didn't agree. For temperature we are going with the HC
'approximate' 20-year binomial. I'll attach a figure David's produced to let you see that.
I reckon if you did a 13-year binomial you'll get something like it. Remember to send
David all the series for trend estimation when you have them.
I am assuming Bin Wang did 3.7.1. Can you clarify with Dave exactly what 3.7.1
is? Give him the method to calculate it. Also clarify the two Chen's.
I see that David has emailed his reading of the
English. I was about to wright something like this. It is definitely the difference
between
two period averages and not extremes years in the periods. The caption obviously needs
a lot of work - I'll have a go at that when I get to it.
If the 3 of us are having difficulties, what hope have we for the readers. If you can't
get
anything remotely like it I would suggest we drop it - but try David's English translation
first !
Cheers
Phil

At 14:11 26/05/2005, David Easterling wrote:

Phil,
We will have the maps redone next week and I have started reworking the text for 3.3
Do you have a CRU global pcp time series for 1901-2003 you can send or should
we calculate? I have the numbers for the figure Aiguo Dai sent.
Also, we never decided on a standard smoothing routine. My preference is for
a 13 or 9 point binomial with reflected ends, but we need to decide.
Last, it is still not clear who did figure 3.7.1, was it Bin Wang? The two Chen
papers are by different authors, the 2004 EA monsoon paper is by T-C Chen of
Iowa State U., and the 2002 paper and data set creator is Ming Chen at NOAA/CPC.
I have requested the PREC/L data set from CPC. But I am not even sure exactly what
3.7.1 is, the title says change in mean annual range between the two periods, which I
interpret to mean the difference between the highest and lowest years for the post 1976
period
minus the difference between the highest and lowest from the pre-1976 period giving a
measure
of change in year to year consistency of monsoons. Also, there is a reference in the

text that
Chen et al. (2004) compiled PREC/L, but that is not the case, it should be Chen et al.
(2002)
as creator, but with an update to 2003.
Dave
Phil Jones wrote:

Dave,
I still don't understand why Bin Wang is involved in this ! Have you contacted
Chen? Maybe it was Bin Wang. Have you looked into trying to reproduce it?
Panmao has sent me a revised 3.7.3 using HadSLP2. I'm going to contact
Rob Allan about this one as he's been involved in developing HadSLP2.
Will you be in a position to send revised Figures soon? Any date also
when you'll be working on the text of 3.3?
Cheers
Phil
At 19:44 25/05/2005, David Easterling wrote:

Phil,
I am trying to track down the source of Fig. 3.7.1 the epoch difference in
monsoon rainfall map. It has a reference of Chen et al. 2004, which is
the J. Climate paper on the east Asian monsoon, but this figure is not in the paper.
Someone must
of plotted it using their data, but not sure who. Do you know?
Dave
--
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531. 1117757977.txt
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From: Georg Kaser <Georg.Kaser@uibk.ac.at>
To: Olga Solomina <olgasolomina@yandex.ru>
Subject: Re: glacier bullet, glossary, structure
Date: Thu, 2 Jun 2005 20:19:37 +0200 (MEST)
Cc: Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Keith Briffa <k.briffa@uea.ac.uk>, ValÐ'rie Masson-Delmotte <Valerie.Masson@cea.fr>, Oyvind.Paasche@bjerknes.uib.no, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Dear Olga,

I deeple apologize for haveing not read your e-mail earlier. I was so overburden with other obligations when coming back from Beijing that I gave myself the deadline of June 1 to start with IPCC work. As usual, circumstances have forced me to postpone this "dedaline" to next Monday. For this, I had not realised that Chapter 6 has its first deadline tomorrow. I have now gone through the "Glaiers during the LIA" and "Glaciers during the MWP" paragraphs as well as through the "glacier bullet" you send today.

I think the LIA paragraph fits well into the Chapter 4 as a supplement to the "Observations" we concentrate on. The MWP is a bit out of focus (Observations!). As I mentioned earlier, I would be glad if chapter 6 could give glaciers approprate space as being the only climate proxies which are exclus´ively governed by physical processes and are, thus, much safer to interpret than any other proxies. The fact that they give filtered information as a mean over longer time periods enables them to represent climate. Over the last years, glaciologists have started to investigate the impact of climate seasonality on glaciers and have also started to separate thermal and hygric variables driving glaciers. All this deserves much attention also beyond the "Observations" to be coverd in Chapter 4.

A comment on the bullet: this is fine. The only point I would change is the one mentioning Africa. For Lewis Glacier, Mount Kenya, advances have been reconstructed from moraines aoroud 1900 and (measured) thickening took place in the 1970s. Rwenzori glaciers have advanced in the late 1960s and early 1970s. A compilation of this is attached as well as a figure and a table from an ongoing compilation of the post-LIA retreat of tropical glaciers I am working on. Please keep them confidential. Note from this figure also the exception Kilimanjaro glaciers play. They have to be seen separately from anything else we observe in the tropics mainly because of the absolute lack of movement on the Plateau (there are also other reasons which would go beyond a readable e-mail). So, to make the long story short: (i) Afrikan glaciers are no exception to the global picture and (ii) Kilimanjaro glaciers are an exception in Africa, in the Tropics, and on the global picture. Thus, Kili glaciers should not be used as an example neither for Africa nor for the tropics. Although I am highly interested in Kilimanjaro myself running a reserach project there, I strongly suggest to not overestimate its glaciers. Accoding to a request from Suasan Solomon I will address that briefly in Chapter 4.5. By the way, Kili glaciers only cover 2.6 km2 out of 2,500 km2 in the tropics (see table in attachement).

Hope this is of help and if you have any further question feel free to

contact me. Best wishes, Georg

Georg Kaser ---------- Institut fuer Geographie Innrain 52 A-6020 INNSBRUCK Tel: ++43 512 507 5407 Fax: ++43 512 507 2895 http://meteo9.uibk.ac.at/IceClim/CRYO/cryo_a.html

On Thu, 2 Jun 2005, Olga Solomina wrote:

> Dear colleagues,
>
> Please find attached my suggestions for the "Glacier bullet" (chapter 6). It
> accumulates (and replaces) all "glacier cases" mentioned in different places
> in our preliminary draft.
>
> I find that our first subdivision of the chapter to 2ka, 10ka etc. was more
> natural rather than 6ka etc. - now we have a mixture of two systems.
>
> My suggestions for the glossary are:
>
> The Holocene (including Early, Mid, Late with approximate dates)
> Little Ice Age
> Neoglacial
>
> I also attach two paragraphs that I wrote for the Ch4 for the recent glacier
> variations, though it is still unclear where it should be. I think both the
> glacier recession from the LIA maximum positions and glacier advances
> occurred during the MWP should be mentioned somewhere.
>
> Cheers,
> olga
>
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Attachment Converted: "c:\eudora\attach\KASER-1999GPCh.PDF"

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

532. 1118866416.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: Fwd: updated MWP figure
Date: Wed Jun 15 16:13:36 2005

   Eystein
   tried phoning on your mobile - no luck - Don't like this Figure , but still having trouble
   working on ours. Have cut large bits out of my text and suggestions for cutting other bits
   , but will be a little late sending these bits. Can you ring to discuss (and IMPRINT)
   tomorrow ?
   Keith
   At 06:28 15/06/2005, you wrote:

     Hi Keith,
     enclosed for your consideration.
     Eystein

Envelope-to: eystein.jansen@geo.uib.no
Date: Tue, 14 Jun 2005 15:13:28 -0400
From: Tom Crowley <tcrowley@duke.edu>
X-Accept-Language: en-us, en
To: J Overpeck <jto@u.arizona.edu>,
"Jansen, Eystein " <eystein.jansen@geo.uib.no>,
Tim Osborn <t.osborn@uea.ac.uk>
Subject: updated MWP figure
X-checked-clean: by exiscan on alf
X-UiB-SpamFlag: NO UIB: 0 hits, 8.0 required
X-UiB-SpamReport: spamassassin found;
Hello,
I have been fiddling with the best way to illustrate the stable nature of the medieval
warm period - the attached plot has eight sites that go from 946-1960 in decadal std.
dev. units - although small in number there is a good geographic spread -- four are from
the w. hemisphere, four from the east. I also plot the raw composite of the eight sites
and scale it to the 30-90N decadal temp. record.
this record illustrates how the individual sites are related to the composite and also
why the composite has no dramatically warm MWP -- there is no dramatically warm
clustering of the individual sites.
use or lose as you wish, tom

--
_____
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--
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Phone: +44-1603-593909
Fax: +44-1603-507784
[1]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

533. 1118949061.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: An idea
Date: Thu Jun 16 15:11:01 2005

Mike,

I will reply to Yasmine and say no tomorrow. Don't want to do it too soon.
Keith and I and Tim have been having loads of discussions about Ch 6 for IPCC. Keith has to submit his latest draft tomorrow for better for worse.
What I'm thinking is that sometime when the three of us here have some spare time - which may be some ways off, we'd like to do some experiments with different proxy combinations. Would you be happy sending us all the proxies you have (or Scott - the rookie) is putting together? If so can you arrange it. There is no rush. We won't pass any on or put on web sites etc.
If we ever did get some time then we could do something - it will be slowly, not for this IPCC and unlikely to get written up or started until well into 2006.
Cheers
Phil

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534. 1119534778.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Anders Moberg <anders@misu.su.se>
Subject: Re: Reminder
Date: Thu Jun 23 09:52:58 2005
Cc: Isabelle Gouirand <isabelle.gouirand@natgeo.su.se>

Anders,
Sending again. Your server rejected this because of the extensions so changed them. Hoep you get them.
Phil
Anders,
Thanks for the files. I was aware that the EGU was starting a new paleo journal.
I don't think there have been any issues yet.
I thought Keith had put those two series on our web site, but I can't find them either. However, I found them ages and put them with some of the other long tree-ring series. So here they are with others.
The ones you want should be in columns 1 and 2. The file starts in 1628BC, so it takes a while to get to them. They start in AD 500. I vaguely recall chopping off the 402-499 and 441-499 years because of sample size. Keith has more trw series now, so they could be improved. Keith should have a reconstruction from the Grudd et al. (2002) paper in The Holocene, but they must be on his machine.
I hope the papers for the two Fennoscandian series tell you what the base period is. Given the publication dates I would suspect it is 1951-80.

There are newer series for Jasper and Tasmania and I wouldn't bother doing anything
with the two South American series.
Have a good summer break. Ruth and I have sat out every night this week so far.
We watched birds the last two days denuding the cherry tree of cherries.
Cheers
Phil
At 07:52 23/06/2005, you wrote:

Phil,
Here are the data we used in our Nature paper, minus Indigirka and Lauritzen. All series
are interpolated to annual resolution. Brief info in file headers. The details are found
in the online supplementary info on nature.com
Lauritzen's email:
"S. E. Lauritzen" <stein.lauritzen@geo.uib.no>
The Finnish diatom series and all eastern tree ring series have been sent through
personal contacts. The rest comes from the web, apart from GRIP which comes from you.
Could you, in return, send me the data file for the Fennoscandian summer temperature
reconstruction from either Briffa et al (Nature 1990) or Briffa et al (Clim Dyn 1992) -
or both? I could not find any of these series on the CRU website.
I realize that Isabelle Gouirand will have to discuss these two papers. Starting from
there and try to point out something new as regards the work done by Isabelle. By the
way, do you know anything about this journal:
[1]http://www.copernicus.org/EGU/cp/cp.html ? I did not know it existed, before I was
told about it yesterday.
Tomorrow starts my summer holidays, which last over the coming four weeks
Cheers,
Anders
At 10:07 2005-06-17 +0100, you wrote:

Anders,
When I got back the bus was still here and the driver had disappeared.
Hope the train came and you got to Stansted OK.
No rush for the paleo data - just when you have a few minutes.
Hopefully these colour plots are OK. I think I was going to pay something
so forward any bills or tell Michelle to send to me.
Cheers
Phil
At 14:29 16/06/2005, you wrote:

Dear Michelle,
Thanks for your message. I expect your letter to arrive early next week, and I should be
able to answer quickly.
Best regards,
Anders
MTheakst@wiley.co.uk wrote:

Dear Anders
We have just posted you colour proofs of your paper - when you receive
these, please contact me to confirm whether we can proceed to publication.
We will be publishing your paper as part of Volume 25, Issue 9.
Best Wishes

Michelle
##
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##

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References

1. http://www.copernicus.org/EGU/cp/cp.html

535. 1119628345.txt
##
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: First draft of FOD
Date: Fri, 24 Jun 2005 11:52:25 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>
Hi gang - I still have to weigh in on the great
figs/text that Keith and Tim have created, but
here's some feedback in the meantime.

I agree that a mean recon isn't the thing to do.
Let me think more before I weigh in more on the
fig. Working to get other LAs to get their stuff
in.

As for the Southern Hem temperature change fig (and caption and a little text), I agree that you (Ricardo in the lead) should do it as you've proposed. We need a clear S. Hem statement, and although it should stress that the data are too few to create a reliable S Hem recon, we should show the data that are available. Thus, PLEASE proceed Ricardo on this tack. Also, can we include the borehole recon series from S. Africa and Australia (e.g., Pollack and Huang, 98)? I'm sure Henry Pollack would provide fast - cc Huang too, since he might be even faster. Keith and Tim, does that make sense?

Please note that I think we can find room for the above, regardless, if it is compelling enough.

As for ENSO, we will need to address for sure - based mainly on the more direct coral data rather than teleconnected (e.g., tree-ring) relationships. The latter don't seem to be definitive enough at this time - as I think we discussed in China. The same holds true for NAO/AO/PDO etc., and I think that we (Keith and Tim) will need to have this in their section - in a appropriately short manner. I'll provide more feedback on this soon, so don't sweat it for now.

Main thing is to go ahead on the S Hem temp fig/caption/short text., independent of ENSO etc discussions.

Thanks, Peck

>Eystein and Peck
>very quick initial response - as have not seen
>Tim today. The Figure legends with very detailed
>explanations is at the end of the text I sent
>you already. The forcings ARE the ones that went
>into the models , appropriately colour coded for
>direct comparison - it was partly the difficulty
>of getting all of these prescribed or diagnosed
>forcings sorted out for each model that took Tim
>so long.The uncertainty levels are a compromise
>that chose came up with - see description in
>caption , but we are considering other things .
>Will get back to re the colours. Producing a
>mean reconstruction is not in my opinion a
>sensible thing to do so we will have to talk
>about this. The question of space is crucial
>regarding the Figure and reworking needed on
>Regional stuff Ricardo and I need to know how
>the space is panning out , and you opinions on
>the reative importance of a SH regional Figure
>versus an ENSO Figure.- and what about Monsoon
>Peck? By the way, please clarify the space re
>the Medieval Warm Period Box. Does this have to
>come down , thought it was short enough?
>Keith
>

> At 09:03 24/06/2005, Eystein Jansen wrote:
>>Hi Keith and Tim,
>>Lots of thanks for your hard work.
>>I have gone through the FOD draft and the
>>figures. Will send comments on text later today.
>>Here some comments on the figures.
>>I did not see the figure captions so it is not
>>entirely transparent to me what went into the
>>figures, hopefully all is material that is or
>>will be published before the end of 2005. But
>>anyhow, I think these figures are very good and
>>in my view give the different reconstructions,
>>the combined uncertainty as well as
>>reconstructions and simulations brought
>>together. I assume you have the Moberg et al
>>reconstruction included, but not the Oerlemans,
>>which will be treated in Ch. 4 (needs a x-ref).
>>Concerning the way of displaying the
>>uncertainties, it is not transparent to me how
>>the white and grey areas are produced. Would it
>>be viable to make a single curve of the mean of
>>the reconstructions to accompany the
>>simulations? The white area underlying the
>>simulations seem a bit weak, in the sence that
>>a superficial reader might wonder if it
>>displays something without content, perhaps a
>>different shade or colour would be better.
>>Conserning the simulations, it needs to be
>>clarified that the simulations did not
>>necessarily use the forcings displayed above,
>>hence it may be misleading to place the
>>forcings and simulations into the same figure.
>>Concerning the forcings, I am a bit surprised
>>that the amplitude of these are so close to
>>each other. Although I haven´t followed the
>>litterature here in detail, my impression was
>>that there is quite high discrepancies between
>>the various solar reconstructions, but I may be
>>wrong.
>>
>>Ricardo asks about whether Peck and I have
>>Ok-ed his suggested figure. To me it seems a
>>good candidate for an ENSO illustration, with
>>some polishing to make it less technical, but
>>since Peck is more up to speed on this and
>>working on the issue, I would leave it to him
>>to weigh in on this matter.
>>
>>Some first impressions for your consideration.
>>
>>Cheers,
>>Eystein
>>
>>
>>
>>
>>
>>--
>>_____
>>Eystein Jansen
>>Professor/Director
>>Bjerknes Centre for Climate Research and
>>Dep. of Earth Science, Univ. of Bergen

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>
>--
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--
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Professor, Department of Atmospheric Sciences

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http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: First draft of FOD - figures
Date: Mon, 27 Jun 2005 15:42:40 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Hi Keith and Tim - Eystein is going to chat with you tomorrow, and my
goal is to get as much as I can to you guys today and tomorrow.

First, off the figures are great (!) - that was tough job, and I'm
very impressed. Of course, I can already start to sense what the
debates will be, but we can address that in the text. Here are some
comments with respect to the figures - some are relevant to the
text...

1) they really are great

2) is the instrumental series on the first fig (top and bottom) the
same as featured in chapter 3? Need to say that.

3) rather than clogging up the caption with all the notes on each curve, how about a table for each of the two figures. Then you can include some more info on each recon - e.g., number of sites, types of proxies??) I'm thinking mainly that the captions are not pretty, but you may be able to include more summary info on each curve also

4) should we make all the series in their original and modified for the figure form available on a www site so that reviewers can play with the data and make sure they get their two cents in before this thing is all said and published? The WDC-A is ready to help w/ posting of data and figs (see below).

5) I like the expanding time axis, but I'd be prepared to have a second one with a linear axis. In fact, I'd put it up on the www page at the same time with the data. The more we do to help others understand, the better?

6) Also, it would be good to see both the data and the figure w/o the Gaussian-weighted filtering. What do doe these look like, can we make them available as suggested above. At the least, I'd like to see the fig w/o the filtering, even though I know it will be a mess. How about a series of time series plots (same x and y axes as the big fig 1) - in each you show both the filtered and unfiltered series. I know this is a pain for Tim, but we really have to make sure we're not missing anything in the data. And also - that we anticipate what others will do, ask us to do, or squawk about.

7) On the forcing fig (fig 2) - why don't we see all the different experiment curves (e.g., dotted red) in the forcing plots a, b and c? Need to say why in the caption - and if they have the same forcing, so you can't see it on the plot, need to say it. This could be much easier in a table that indicates "same as X").

8) On fig 2 - does the recalulated envelop of reconstructed temps also include instrumental temps? Think so, but you should say it in the caption. Why doesn't the envelop go up to present? Can it? Might look better, and be more consistent w/ fig 1. If the envelop can't go to present, then maybe include the instrumental curve as in Fig 1.

9) reminders for the text (I'll think about these as I read a second time for editing) -

9a) need to explain why the recons don't continue going up w/ instrumental data at the end (post 1990?) - might what to mention something in caption, if you can shift all the other stuff to a table.

9b) there will be lots of discussion (during and post AR4 drafting) about what recon series (Fig 1) should or should not be believed. Thus, I think it is critical for us to same more about each recon - that is to INCLUDE what you wrote in blue, and perhaps to enhance. Need to really convince the reader that while not one recon is alone the truth (and hence Fig 1), they all have important strengths and weaknesses. But, the former outweigh the latter, so we've included them.

9c) I'm sure you saw the recent (to be infamous) Wall Stree Journal editorial - they showed what I think was a IPCC FAR curve - with the good old MWP and LIA etc (Lamb view? - I don't have the FAR w/ me). The way to handle the hocky stick might best be to put it in an historical perspective along with the older IPCC views. First, show your great figs, discuss them and what went into them, and then - after showing the state-of-the-art, discuss how much our understanding and view have changed. In this, simply compare each of

the historical views (FAR, SAR, TAR) to the current view, and while doing so, play down the controversy (s) - especially the hockey stick. The smart folks will realize that that the fluff in the news is just that, but those with a real stake in that debate will hopefully get the point that it doesn't matter...

10) lastly (almost), I'm sorry to ask again, but I still want to know what is wrong with Tom Crowley's latest plot with all the recons shown together back through the Med W Period? I need to send you my edits on the MWP box, but it seem to me that Tom's fig could go in that box - to help make the point that - sorry, guys - the MWP wasn't much compared to the recent GLOBAL warming...

11) lastly (promise) - don't foget that Eystein and I think we can get a page or two extra for your section in the end. This means you can do all the above, and I can help (next) with the modes and extremes sections, and we can get it all in.

Great job!

Thanks, Peck
--
Jonathan T. Overpeck
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Professor, Department of Geosciences
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</x-flowed>

537. 1119924849.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: the Med Warm Period Box - Peck comments/edits
Date: Mon, 27 Jun 2005 22:14:09 -0600

<x-flowed>
Gentlemen - attached is the ZOD Med Warm Period Box with my edits/comments. I don't see anything sent since then, so hope I'm not editing the wrong thing. In any case, the Box was pretty nice as is, so I only made a few changes. Obviously, some updating w/ new studies is needed. The big issues are two:

1) the recent Wall Street Journal editiorial that is creating all the crap in the US actually showed a time series from the IPCC FAR - if you don't have it, or Eystein can't send, I can scan it in (my Republican Dad sends me these things, although he's an increasingly rare breed of moderate Republican). My thought is that it might we worth adding a couple lines documenting how the view of the MWP

changed with each assessment and new knowledge. In doing so, it could be made very clear that there is a reason that scientists don't show those old plots anymore. We need to move the debate beyond the FAR, SAR and TAR on this issue!

2) it would be cool to have another figure that made the point about no single synchronous period warmer than late 20th century. This is where I get soft with respect to Tom's plot. If it is published to the extent we need it, and if the composite or large-area average recon is the same as you are showing in your great new Fig 1, then it seems that it would be reasonable to show Tom's fig as part of the Box - just to show the same thing in a different way, and to hammer in one more nail. That said, I'm not sure if my two conditions above are met (I emailed Tom, no response yet - you might have insight), and I believe you just don't like Tom's fig for some - probably good - reason. But, I wanted us to think extra hard about whether there is SOME fig that might work?

That's it for tonight. Will finish editing your main text next work session tomorrow I hope.

Best, Peck
--
Jonathan T. Overpeck
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</x-flowed>

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

538. 1119957715.txt
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: Re: NEED HELP!
Date: Tue, 28 Jun 2005 07:21:55 -0400

Hi Keith,
Thanks--yes, we seem to back in the days of McCarthyism in the States. Fortunately, we have
some good people who will represent us legally pro bono, and in the best case scenario,
this backfires on these thugs...
The response of the wording is likely to change dramatically after consulation w/ lawyers,
etc. but any feedback on the substance would nonetheless be very helpful...
thanks for both your help and your support,
mike

At 05:48 AM 6/28/2005, you wrote:

Mike
just in and seeing this for time - will digest - but do not like look or implications of
this at all
Keith
At 17:00 25/06/2005, you wrote:

Tim/Keith/Phil,
Please see attached letter from the U.S. House republicans. As Tom has mentioned below,
it would be very helpful if I can get feedback from you all as I proceed w/ drafting a
formal response.
Thanks in advance for any help,
mike

Date: Sat, 25 Jun 2005 09:36:49 -0600
From: Tom Wigley <wigley@cgd.ucar.edu>
Organization: NCAR/CGD
User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.4) Gecko/20030624
Netscape/7.1 (ax)
X-Accept-Language: en-us, en
To: Michael Oppenheimer <omichael@princeton.edu>
Cc: "Michael E. Mann" <mann@virginia.edu>, shs@stanford.edu, dlashof@nrdc.org,
jhansen@giss.nasa.gov, mmaccrac@comcast.net, santer1@llnl.gov,
wigley@ucar.edu, Caspar Ammann <ammann@ucar.edu>
Subject: Re: NEED HELP!
X-UVA-Virus-Scanned: by amavisd-new at fork9.mail.virginia.edu
Mike,
There are broader implications of this, so it is important to respond well. It is
a pity you have to be the guinea pig after what you have gone through already,
but you have many supporters.
I would not advise a legal route. I think you need to consider this as just another
set of referees' comments and respond simply, clearly and directly. On the science
side the key point is that the M&M criticisms are unfounded.
Although this may be difficult, remember that this is not really a criticism of you
personally, but one aspect of a criticism of the foundations of global warming
science by people both inside and outside of Congress who have ulterior motives.
There may, in fact, be an opportunity here. As you know, we suspect that there
has been an abuse of the scientific review process at the journal editor level.
The method is to choose reviewers who are sympathetic to the anti-greenhouse
view. Recent papers in GRL (including the M&M paper) have clearly not been
reviewed by appropriate people. We have a strong suspicion that this is the case,
but, of course, no proof because we do not know *who* the reviewers of these
papers have been. Perhaps now is the time to make this a direct accusation and
request (or demand) that this information be made available. In order to properly
defend the good science it is essential that the reasons for bad science appearing
in the literature be investigated.
The lever here is that the Subcommittee on Oversight and Investigations of the
House Committee on Energy and Commerce is suggesting that your papers are
bad science and asking (their point 8e) for the identity of people who reviewed
your work. In response, it is completely fair and justifiable to point out that

it
is the papers that criticize your and related work that are bad science, and that,
through the Subcommittee you can request the identities of the reviewers of all of these critical papers -- starting with M&M.
When you respond, there are a number of items that require a direct response from you alone. There are also a number of scientific points where you could give a multi-authored response. There are many people who have expertise in this area and familiarity with the scientific issues who I am sure would be willing
to join you (I would be happy to do so).
At this stage, however, I would keep the group small. A few others could be added
to the original email list nevertheless. I took the liberty of copying your plea and
the Subcommittee's letter to Caspar Ammann, primarily because I think he can help with the scientific aspects better than most people. After all, he has been
able to follow your method and reproduce your results, he has shown the flaws in M&M's work, he has investigated the bristlecone pine issue, and he has made all his software available on the web.
The others who could be added at this early stage are Ray Bradley and Malcolm Hughes, your 'co-conspirators' -- and perhaps Phil Jones, Keith Briffa and Tim Osborn. I do not know how 'powerful' these alien opinions may be in the present parochial context, but I note that the instigators of all this are Canadians and that
the science has no national boundaries. Phil, Keith and Tim are useful because they
have demonstrated the flaws in the von Storch work -- which is, I assume, the Science paper that the Subcommittee's letter referes to.
A word of warning. I would be careful about using other, independent paleo reconstruction work as supporting the MBH reconstructions. I am attaching my version of a comparison of the bulk of these other reconstructions. Although these all show the hockey stick shape, the differences between them prior to 1850 make me very nervous. If I were on the greenhouse deniers' side, I would be inclined to focus on the wide range of paleo results and the differences
between them as an argument for dismissing them all.
I attach also a run with MAGICC using central-estimate climate model parameters (DT2x = 2.6 degC, etc. -- see the TAR), and forcings used by Caspar in the runs with paleo-CSM. I have another Figure somewhere that compares MAGICC with paleo-CSM. The agreement is nearly perfect (given that CSM has internally generated noise while MAGICC is pure signal). The support for the hockey stick is not just the paleo reconstructions, but also the model results. If one takes the
best estimates of past forcing off the shelf, then the model results show the hockey
stick shape. No tuning or fudging here; this is a totally independent analysis, and
critics of the paleo data, if they disbelieve these data, have to explain why models
get the same result.
Of course, von Storch's model results do not show such good century timescale agreement, but this is because he uses silly forcing and has failed to account for
the fact that his model was not in equilibrium at the start of the run (the subject
of Tim Osborn et al.'s submitted paper).
This is a pain in the but, but it will all work out well in the end (unintentional pun
--
sorry). Good science will prevail.
Best wishes,

Tom.

Michael Oppenheimer wrote:

Michael:
This is outrageous. I'll contact some people who may be able to help right away.

From: Michael E. Mann [<[1]mailto:mann@virginia.edu>[2]mailto:mann@virginia.edu]
Sent: Friday, June 24, 2005 4:27 PM
To: <[3]mailto:shs@stanford.edu>shs@stanford.edu; <[4]mailto:omichael@Princeton.EDU>omichael@Princeton.EDU; <[5]mailto:dlashof@nrdc.org>dlashof@nrdc.org; <[6]mailto:jhansen@giss.nasa.gov>jhansen@giss.nasa.gov; <[7]mailto:mmaccrac@comcast.net>mmaccrac@comcast.net; <[8]mailto:santer1@llnl.gov>santer1@llnl.gov; <[9]mailto:wigley@ucar.edu>wigley@ucar.edu
Subject: NEED HELP!
Importance: High

dear all,
this was predicted--they're of course trying to make things impossible for me. I need
immediate help regarding recourse for free legal advice, etc.
mike

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e-mail: <[10]mailto:mann@virginia.edu>mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[11]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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[14]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. mailto:mann@virginia.edu
2. mailto:mann@virginia.edu
3. mailto:shs@stanford.edu
4. mailto:omichael@Princeton.EDU%3Eomichael@Princeton.EDU
5. mailto:dlashof@nrdc.org
6. mailto:jhansen@giss.nasa.gov%3Ejhansen@giss.nasa.gov
7. mailto:mmaccrac@comcast.net
8. mailto:santer1@llnl.gov%3Esanter1@llnl.gov
9. mailto:wigley@ucar.edu
10. mailto:mann@virginia.edu
11. http://www.evsc.virginia.edu/faculty/people/mann.shtml
12. http://www.evsc.virginia.edu/faculty/people/mann.shtml
13. http://www.cru.uea.ac.uk/cru/people/briffa/
14. http://www.evsc.virginia.edu/faculty/people/mann.shtml

539. 1119967865.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: Re: updated MWP figure
Date: Tue, 28 Jun 2005 10:11:05 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Hi Keith - might be worth talking on the phone - you, me and Eystein - after you get back. You could be right, but it is a powerful way to look at the issue. The question is whether the normalization could be preventing a warmer than late-20th century signal from appearing?

Should we instead update the Bradley Science graphic? That's not as effective in my opinion.

So, let's talk next week?

Going to a tree day meeting or a three day meeting - it has to be tough looking at tree data all day.

have fun, thx, peck

>Jonathan and Eystein
>I am leaving very early for a tree day meeting in Swansea , and will
>be away til Monday. Presently buried in EC Reporting and other stuff
>- but the reason I dislike the MWP Figure is that the simple
>normalization of series as done , (regardless of regional selection
>of specific proxies) gives a largely random amplitude to the various
>records , depending on their spectral character, and of course,
>equal weight to all regardless of the strength of their link with
>local or NH temperatures). I will think about this - you are the
>ultimate arbiter anyway .
>sorry to be so abruptly communicative
>Keith
>
>At 16:10 28/06/2005, you wrote:
>>Hi Tom -- thanks for the extra effort. I'm pushing others on the
>>author team to think hard about such a figure (space may end up
>>being the hardest part), and I should have something to discuss w/
>>you soon. Thanks for being willing to shift priorities if needed.
>>

>>FYI - I just got reviews back from an EOS piece that took over a
>>1.5 months to get. And of course, they want some edits. Not the
>>speedy venue we once knew a loved, although I bet if you really
>>keep it short and sweet it might go faster.
>>
>>Best, more soon, peck
>>
>>>X-Sieve: CMU Sieve 2.2
>>>Date: Tue, 28 Jun 2005 10:13:49 -0400
>>>From: Tom Crowley <tcrowley@duke.edu>
>>>X-Accept-Language: en-us, en
>>>To: Jonathan Overpeck <jto@u.arizona.edu>
>>>Cc: Eystein Jansen <eystein.jansen@geo.uib.no>
>>>Subject: Re: updated MWP figure
>>>
>>>Hi Jonathan,
>>>
>>>let me answer the last question first - there are actually not
>>>many records that go back that far and I have used, I think, every
>>>one except Quelcaya, which being from the southern tropics makes
>>>for a lonely but potential future inclusion (which makes no
>>>difference on the conclusion).
>>>
>>>several of the sites include multiple time series - e.g., western
>>>U.S. time series, w. Siberia time series, e. Asia, and w.
>>>Greenland. I did not want to overweight any site though because
>>>of the need for a geographic balance -- note that there are four
>>>sites each in the w. hemisphere and e. hemisphere, and that the
>>>distribution of sites in each hemisphere represents a good scatter.
>>>
>>>for almost all of these sites the references are easily imaginable
>>>based on the location of the site, but they can be provided if you
>>>are interested in including the figure.
>>>
>>>can you think of any long sites I have not included? right now I
>>>cannot.....
>>>
>>>in the overlap interval of 1500-1850 our composite has highly
>>>significant correlations with the Mann, Jones, and Briffa
>>>reconstructions that contain much more data -- thereby suggesting
>>>that use of only long time series provides a "reasonable" estimate
>>>of the last 1100 years.
>>>
>>>I have not submitted this for publication but if you are
>>>interested in including this in ipcc I can knock off a tutorial
>>>note to eos on short notice.....
>>>
>>>I am attaching the figure in several different alternate formats -
>>>cannot easily do the two you suggest from my mac, but again I can
>>>get that done with more work if you are interested - let me know
>>>where to go next - note that I originally sent this along fyi,
>>>only to be used if you thought the figure was worthwhile -- if not
>>>I will just reorder the priority of writing it up as a note,
>>>tom
>>>
>>>Jonathan Overpeck wrote:
>>>
>>>>Hi Tom - thanks for sending this plot. I'm a bit late in
>>>>responding since we were moving to (and still into) our
>>>>sabbatical digs in SW CO.
>>>>
>>>>Would you be willing to provide more on this plot in order for me
>>>>to understand it better? I personally like the plot quite a bit,

>>>>but between the space restrictions and other's assessment,
>>>>whether we use it or not will take some real thinking.
>>>>
>>>>For example, it would help to have
>>>>
>>>>1) a higher resolution version - eps or ai?
>>>>2) a caption or text that would spell out which records are
>>>>included, and their origins (references)
>>>>3) a bibliography for those refs.
>>>>4) perhaps, you have a paper with this included? If so, can you
>>>>send a prerprint?
>>>>5) some discussion of why you used the series (sites) you did,
>>>>and not others - more specifically, what's wrong with others?
>>>>
>>>>If you don't mind helping here, I'll promise to get it in the mix
>>>>for serious discussion. Of course, it's already in the mix since
>>>>Eystein forwarded to Keith, and you Tim, but I want to weigh in
>>>>as informed as possible. Trying to keep track of a lot, so your
>>>>help is much appreciated.
>>>>
>>>>Thanks! Peck
>>>>
>>>>>Hello,
>>>>>
>>>>>I have been fiddling with the best way to illustrate the stable
>>>>>nature of the medieval warm period - the attached plot has eight
>>>>>sites that go from 946-1960 in decadal std. dev. units -
>>>>>although small in number there is a good geographic spread --
>>>>>four are from the w. hemisphere, four from the east. I also
>>>>>plot the raw composite of the eight sites and scale it to the
>>>>>30-90N decadal temp. record.
>>>>>
>>>>>this record illustrates how the individual sites are related to
>>>>>the composite and also why the composite has no dramatically
>>>>>warm MWP -- there is no dramatically warm clustering of the
>>>>>individual sites.
>>>>>
>>>>>use or lose as you wish, tom
>>>>>
>>>>
>>>>
>>>
>>>
>>>
>>>
>>
>>
>>--
>>Jonathan T. Overpeck
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>>Professor, Department of Geosciences
>>Professor, Department of Atmospheric Sciences
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>>
>
>--
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>
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--
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</x-flowed>

540. 1120014836.txt
##
##########

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: wg1-ar4-ch06@joss.ucar.edu
Subject: Re: [Wg1-ar4-ch06] abrupt and Important thoughts on References
Date: Tue, 28 Jun 2005 23:13:56 +0200

<x-flowed>
Hi all,

Two things:

1. Concerning the 1470k pacing of DO-events.
There are revisions underway in the
layer-counting of the Greenland Ice Cores. A
meeting in Copenhagen in August co-ordinated by
Sigfus Johnsen will discuss the issue at length,
but there may not be many papers out from the
meeting that are citeable for IPCC. There is
already the Shackleton paper which indicate that
Greenland Ice Cores in MIS3 have an age model
that are off by some millennia, and the
preliminary data on the new age models indicate
substantial revisions as far as I hear from talks
given at various meetings. My thinking is that
we neither can ignore the fact that current data

indicate a 1470 pacing for some time interval of the ice cores if one apply the existing age scales. I think it would be foolish not not refer to it, I think the possibility that the system has the ability to enter into specific cycles is intriguing, and is a result that is well known and IPCC should not pretend we haven´t heard about it. But we should make it less blunt than in the current version of the Abrupt Change subchapter, perhaps stating that the result is highly dependant on age models and we need time to absorb new research in order to verify the result.

2. Having the fortune of not being that close to the darker sides of US politics, I have the feeling that Peck´s comment concerning referencing perhaps is a bit too "paranoic". I think the advice is well taken not to overcite our own research, and make sure not to overlook other important contributions, but we should do our best to cite what we think are key results. In any case we will have the FOD review and have the opportunity to have all our good colleagues keeping us honest on this issue.

Cheers,
Eystein

>Hi all - thanks Fortunat and Stefan for more
>debate on the 1470. Sounds like the final
>decision is up to Eystein, but I can guess the
>way he's thinking.
>
>With regard to refs - remember that our goal is
>to cut the number of references significantly.
>Since this is an assessment and not a review, we
>can delete all but the most recent and
>comprehensive references. I don't like cutting
>out the original refs any more than you, but we
>just don't have room, and its more important to
>have text than exhaustive references. Our
>colleagues will hopefully understand, and if
>they don't then they need to do an ego check.
>It's more important that we make an impact with
>policy makers rather than with citation indices.
>
>Does this make sense?
>
>In any case, please help make sure we trim the
>total references DOWN in number by a significant
>number. This is not happening the to degree it
>should.
>
>Also, please not that in the US, the US Congress
>is questioning whether it is ethical for IPCC
>authors to be using the IPCC to champion their
>own work/opinions. Obviously, this is wrong and
>scary, but if our goal is to get policy makers
>(liberal and conservative alike) to take our

>chapter seriously, it will only hurt our effort
>if we cite too many of our own papers
>(perception is often reality). PLEASE do not
>cite anything that is not absolutely needed, and
>please do not cite your papers unless they are
>absolutely needed. Common sense, but it isn't
>happening. Please be more critical with your
>citations so we save needed space, and also so
>we don't get perceived as self serving or worse.
>
>Again, we can debate this if anyone thinks I've gone off the deep end.
>
>Thanks, peck
>PS - this is not to say anything critical of the
>refs Fortunat is suggesting - we must cite the
>most relevant papers, and we must be as up to
>date as possible.
>
>>Peck and all,
>>
>>Fully agree. This '1470' yr periodicity is highly controversial and I
>>was never convinced.
>>We can use the space for better things that are relevant in the context
>>of the anthropogenic GHG perturbation.
>>
>>I miss the recent and relevant literature. Examples are Pahnke and Zahn,
>>Science, 2005 and Stocker and Johnsen, Paleoceanography 18, 2003, and
>>Knutti et al., Nature, 2004
>>Hemitt et al., Rev Geophysics, 2004 might be a good reference for
>>Heinrich events.
>>
>>
>>
>>Regards,
>>
>>Fortunat
>>
>>
>>Jonathan Overpeck wrote:
>>>
>>> Hi guys - I'm not aware of the age model changes that Eystein is
>>> talking about (however, I'm not in the Euro meeting circles, and
>>> trust he's right), but I know of several studies (e.g., U/Th dated
>>> (well dated) spelothem studies (plus C14 Cariaco) that indicate that
>>> the GISP/GRIP age models are off by quite a bit pre 40kish. The other
>>> studies agree, so it makes sense to me that the ice core gangs are
>>> revising their age models. Regardless of the probabilities (note that
>>> one finds evidence in quasi-periodic variance most all paleo
>>> records), this significant age model change means that the "1470
>>> beat" has to be off/wrong or something else other than we've been led
>>> to believe. For the sake of playing it safe, we should play this beat
>>> way down until there is new evidence that is more convincing that it
>>> is for real. We can mention it, but we make it clear that the
>>> evidence for it is not all that strong - at best.
>>>
>>> I'll cc this to Fortunat and Valerie too - we don't want to rush to
>>> conclusions w/o good discussion.
>>>
>>> Thanks, Peck
>>>
>>> >Hi Eystein,
>>> >
>>> >concerning your comment on the 1470-year beat: I'm aware that in the

>>> >new time scale, it is less regular (at least I heard this, have not
>>> >tested myself yet).
>>> >
>>> >If you have two time scales, one showing a regularity and one not,
>>> >then there are two possibilities.
>>> >(1) The regular one is correct, in the other one the regularity got
>>> >wiped out by random dating errors.
>>> >(2) The one without regularity is correct, in the other one a
>>> >regularity arose by chance due to random dating errors.
>> > >
>>> >The likelyhood of the regularity found with the original GISP2 time
>>> >scale occuring by chance is minute - I've done some more
>>> >calculations, they are not complete yet but the likelyhood is in the
>> > >permil range. I think hypothesis (2) can be exluded at least at 99%
>>> >confidence level.
>>> >
>>> >Stefan
>>> >
>>> >--
>>> >To reach me directly please use: rahmstorf@ozean-klima.de
>>> >(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)
>>> >
>>> >Stefan Rahmstorf
>>> >www.ozean-klima.de
>>> >www.realclimate.org
>>>
>>> --
>>> Jonathan T. Overpeck
>>> Director, Institute for the Study of Planet Earth
>>> Professor, Department of Geosciences
>>> Professor, Department of Atmospheric Sciences
>>>
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>>>
>>> Institute for the Study of Planet Earth
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>>> University of Arizona
>>> Tucson, AZ 85721
>>> direct tel: +1 520 622-9065
>>> fax: +1 520 792-8795
>>> http://www.geo.arizona.edu/
>>> http://www.ispe.arizona.edu/
>>
>>--
>>Climate and Environmental Physics, Physics Institute, University of Bern
>>Sidlerstr. 5, CH-3012 Bern
>>Phone: ++41(0)31 631 44 61   Fax: ++41(0)31 631 87 42
>>e-mail: joos@climate.unibe.ch;   Internet:
>>http://www.climate.unibe.ch/~joos/
>
>
>--
>Jonathan T. Overpeck
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>Professor, Department of Geosciences
>Professor, Department of Atmospheric Sciences
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>direct tel: +1 520 622-9065
>fax: +1 520 792-8795
>http://www.geo.arizona.edu/
>http://www.ispe.arizona.edu/
>_____
>Wg1-ar4-ch06 mailing list
>Wg1-ar4-ch06@joss.ucar.edu
>http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06

--
_____
Eystein Jansen
Professor/Director
Bjerknes Centre for Climate Research and
Dep. of Earth Science, Univ. of Bergen
Allégaten 55
N-5007 Bergen
NORWAY
e-mail: eystein.jansen@geo.uib.no
Phone: +47-55-583491 - Home: +47-55-910661
Fax: +47-55-584330

_____
Wg1-ar4-ch06 mailing list
Wg1-ar4-ch06@joss.ucar.edu
http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
</x-flowed>

541. 1120017435.txt
##########

From: Valérie Masson-Delmotte <Valerie.Masson@cea.fr>
To: Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>,
Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: IPCC ch9 for information and check.
Date: Tue, 28 Jun 2005 23:57:15 +0200
Reply-to: Valerie.Masson@cea.fr

Content-Type: text/plain; charset=ISO-8859-1; format=flowed
X-MIME-Autoconverted: from 8bit to quoted-printable by cirse.extra.cea.fr id
j5SLvFxj010843

<x-flowed>
Dear Keith,

I have read your text - despite of the heat wave here (40°C in my
office in the afternoon...). I am a bit puzzled by the regional aspects.
I think that you should make more clear in the beginning that there is
very little new information / work conducted on the S Hemisphere /
tropics and that most efforts have been focussed on the N Hemisphere,
because you mention almost nothing for the S Hemisphere. Is ENSO
considered as a regional mode of variability? I thought that it had
almost global relevance at least in terms of impacts.

Valérie.

Keith Briffa a écrit :

> Pascale

> I am sending what I sent Peck and Eystein
> The regional stuff at the end is from Ricardo Villalba and will need
> to be shortened /rewritten after advice from CLAs. Please note this is
> only provisional and I have had no feedback from other LA and CLAs and
> the text needs to be vetted/chopped or whatever. Please note also that
> the blue text will likely disappear - no space. The Figure legends are
> at the back of the text file. I will send Figures as a separate message
> cheers
> Keith
> At 15:52 23/06/2005, Pascale Braconnot wrote:
>
>> Dear all,
>>
>> Here is what I send today to gaby and francis as a contribution for
>> the first draft for chapt 9.3
>>
>> We know we have overlap between the two chapters (9 and 6). We need
>> to make sure that the point of view is different (or slightly).
>> in particular, chapter 6 days much more about the data (I nearly
>> supress all ref to data in 9), and may be also on model evaluation
>> (which i do not mention as such).
>>
>> It could be nice you send me your parts in chapter 6 when ready. I
>> will have only a small time to adapt the chapt 9 contribution and
>> make changes in July.
>>
>> How things will work in chapter9 in the coming month.
>>
>> CLA recieved all the contributions, they work together next week
>> (i still need to interact with gbi for the last millenium part and
>> the update of the figure on detection: attribution, but gabi didn't
>> had time to do it at the moment).
>>
>> Then Gabi and Francis will return comments to us (as well as internal
>> comments withing LA of the chapter) and last changes will be provided
>> for the end of July.
>>
>> On my side I am out of contact (mail etc) starting 22 July.
>> I need thus to finish every thing for July 20.
>>
>> I hope the draft 1 writing is going on well on your side
>>
>> Cheers
>>
>> Pascale
>>
>>
>>
>
> --
> Professor Keith Briffa,
> Climatic Research Unit
> University of East Anglia
> Norwich, NR4 7TJ, U.K.
>
> Phone: +44-1603-593909
> Fax: +44-1603-507784
>
> http://www.cru.uea.ac.uk/cru/people/briffa/

</x-flowed>

Attachment Converted: "c:\eudora\attach\masson119.vcf"

542. 1120236419.txt
##

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: What's up with your paper with Eugene?
Date: Fri, 1 Jul 2005 12:46:59 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Stephen Schneider
<shs@stanford.edu>, "Wahl, Eugene R" <wahle@alfred.edu>, Keith Briffa
<k.briffa@uea.ac.uk>

<x-flowed>
Hi Caspar and Gene - Thanks. I look forward to hearing how things go
- if the paper is in press by the first week of August, we'll cite it
in the Chapter 6 of the FOD, but otherwise I guess it'll have to wait
- that's ok too.

But... keep us posted (and send revised preprint when possible). Thanks! Peck

>Hi Peck,
>
>you might have heard.. the thing is flying in everybody's face right
>now... Mike-Ray-Malcolm, IPCC and NSF got these lovely letters from
>the House of Representatives...
>
>Now, I know of - and already have in hand - comments by two reviews
>of the WA paper, both strongly positive. Steve is probably waiting
>on the Canadians to finish theirs. There were two requests for
>additional information over the course of the review so far, I hope
>no other one is required that delays the process. I cc Steve, he
>might give you the best perspective on the progress. Gene is going
>to be at NCAR in early July and we will finish with revisions ASAP.
>
>I hope this helps for now. I'm currently in Rome at a meeting on
>Sun-Climate links,
>Caspar
>
>
>Jonathan Overpeck wrote:
>
>>Hi Caspar - we're working on the IPCC chapter and wonder if you
>>could pls update us w/ the status of Wahl and Ammann? Most
>>important - will it be in press by the end of the month?
>>
>>Thanks! Peck
>
>
>--
>Caspar M. Ammann
>National Center for Atmospheric Research
>Climate and Global Dynamics Division - Paleoclimatology
>1850 Table Mesa Drive
>Boulder, CO 80307-3000
>email: ammann@ucar.edu   tel: 303-497-1705   fax: 303-497-1348

--
Jonathan T. Overpeck

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Professor, Department of Atmospheric Sciences

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http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
</x-flowed>

543. 1120528403.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: "Wahl, Eugene R" <wahle@alfred.edu>
Subject: RE: Wahl-Ammann paper
Date: Mon, 4 Jul 2005 21:53:23 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
Hi Gene - good to hear from you. What you list below seems like it
must be pretty good to me. Of course, we'd like to include all we can
in the FOD, hence the interest in knowing if it's in press or not
before the end of the month.

Just keep us updated, and if you feel comfortable sharing the ms.
that'd be great, but only if you feel ok about sharing it. The key
people are me, Eystein Jansen and Keith Briffa - we won't share it
with others.

Thanks for keeping us up to date. Best, peck

>Hello Jonathan:
>
>Thanks for this info. Could you clue me in--I had heard through the
>grapevine (ultimate source, Jerry Meehl) that the actual in-press
>deadline for IPCC citations in the AR would be Jan 1 of 2006. On
>the IPCC website I see mid-December for the Christchurch meeting.
>
>I assume this the same situation for Chapter 6, and thus the early
>August deadline is for the FOD. Is this getting it correct?
>
>Let me know if viewing the submitted text would be of use to you,
>and I'll ship at once.
>
>
>Hope you are well.
>
>Peace, Gene
>Dr. Eugene R. Wahl
>Asst. Professor of Environmental Studies
>Alfred University
>
>607-871-2604
>1 Saxon Drive

>Alfred, NY 14802
>
>_____
>
>From: Jonathan Overpeck [mailto:jto@u.arizona.edu]
>Sent: Fri 7/1/2005 2:46 PM
>To: Caspar Ammann
>Cc: Eystein Jansen; Stephen Schneider; Wahl, Eugene R; Keith Briffa
>Subject: Re: What's up with your paper with Eugene?
>
>
>
>Hi Caspar and Gene - Thanks. I look forward to hearing how things go
>- if the paper is in press by the first week of August, we'll cite it
>in the Chapter 6 of the FOD, but otherwise I guess it'll have to wait
>- that's ok too.
>
>But... keep us posted (and send revised preprint when possible). Thanks! Peck
>
>>Hi Peck,
>>
>>you might have heard.. the thing is flying in everybody's face right
>>now... Mike-Ray-Malcolm, IPCC and NSF got these lovely letters from
>>the House of Representatives...
>>
>>Now, I know of - and already have in hand - comments by two reviews
>>of the WA paper, both strongly positive. Steve is probably waiting
>>on the Canadians to finish theirs. There were two requests for
>>additional information over the course of the review so far, I hope
>>no other one is required that delays the process. I cc Steve, he
>>might give you the best perspective on the progress. Gene is going
>>to be at NCAR in early July and we will finish with revisions ASAP.
>>
>>I hope this helps for now. I'm currently in Rome at a meeting on
>>Sun-Climate links,
>>Caspar
>>
>>
>>Jonathan Overpeck wrote:
>>
>>>Hi Caspar - we're working on the IPCC chapter and wonder if you
>>>could pls update us w/ the status of Wahl and Ammann? Most
>>>important - will it be in press by the end of the month?
>>>
>>>Thanks! Peck
>>
>>
>>--
>>Caspar M. Ammann
>>National Center for Atmospheric Research
>>Climate and Global Dynamics Division - Paleoclimatology
>>1850 Table Mesa Drive
>>Boulder, CO 80307-3000
>>email: ammann@ucar.edu   tel: 303-497-1705   fax: 303-497-1348
>
>
>--
>Jonathan T. Overpeck
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>http://www.geo.arizona.edu/
>http://www.ispe.arizona.edu/

--
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http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
</x-flowed>

544. 1120593115.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: John Christy <john.christy@nsstc.uah.edu>
Subject: This and that
Date: Tue Jul 5 15:51:55 2005

    John,
       There has been some email traffic in the last few days to a week - quite
   a bit really, only a small part about MSU. The main part has been one of
    your House subcommittees wanting Mike Mann and others and IPCC
    to respond on how they produced their reconstructions and how IPCC
    produced their report.
      In case you want to look at this see later in the email !

      Also this load of rubbish !

       This is from an Australian at BMRC (not Neville Nicholls). It began from the
attached

    article. What an idiot. The scientific community would come down on me in no
    uncertain terms if I said the world had cooled from 1998. OK it has but it is
only
    7 years of data and it isn't statistically significant.

      The Australian also alerted me to this blogging ! I think this is the term !
Luckily

    I don't live in Australia.

     [1]http://mustelid.blogspot.com/2005/06/first-look-at-scs-msu-vn52.html

Unlike the UK, the public in Australia is very very naïve about climate change, mostly because of our governments Kyoto stance, and because there is a proliferation of people with no climate knowledge at all that are prepared to do the gov bidding. Hence the general populace is at best confused, and at worst, antagonistic about climate change - for instance, at a recent rural meeting on drought, attended by politicians and around 2000 farmers, a Qld collegue - Dr Roger Stone - spoke about drought from a climatologist point of view, and suggested that climate change may be playing a role in Australias continuing drought+water problem. He was booed and heckled (and unfortunately some politicians applauded when this happened) - that's what we're dealing with due to columists such as the one I sent to you.

Now to your email. I have seen the latest Mears and Wentz paper (to Science), but am not reviewing it, thank goodness. I am reviewing a couple of papers on extremes, so that I can refer to them in the chapter for AR4. Somewhat circular, but I kept to my usual standards.

The Hadley Centre are working on the day/night issue with sondes, but there are a lot of problems as there are very few sites in the tropics with both and where both can be distinguished. My own view if that the sondes are overdoing the cooling wrt MSU4 in the lower stratosphere, and some of this likely (IPCC definition) affects the upper troposphere as well. Sondes are a mess and the fact you get agreement with some of them is miraculous. Have you looked at individual sondes, rather than averages - particularly tropical ones? LKS is good, but the RATPAC update less so.

As for being on the latest VG analysis, Kostya wanted it to use the surface data. I thought the model comparisons were a useful aside, so agreed. Ben sent me a paper he's submitted with lots of model comparisons that I also thought a useful addition to the subject.

As for resolving all this (as opposed to the dogfight) I'm hoping that CCSP will come up with something - a compromise. I might be naive in this respect. I hope you are still emailing and talking to Carl and Frank. How is CCSP going? Are you still on schedule for end of August for your open review?

What will be interesting is to see how IPCC pans out, as we've been told we can't use any article that hasn't been submitted by May 31. This date isn't binding, but Aug 12 is a little more as this is when we must submit our next draft - the one everybody will be able to get access to and comment upon. The science isn't going to stop from now until AR4 comes out in early 2007, so we are going to have to add in relevant new and important papers. I hope it is up to us to decide what is important and new. So, unless you get something to me soon, it won't be in this version. It shouldn't matter though, as it will be ridiculous to keep later drafts without it. We will be open to criticism though with what we do add

in subsequent drafts. Someone is going to check the final version and the Aug 12 draft. This is partly why I've sent you the rest of this email. IPCC, me and whoever will get accused of being political, whatever we do. As you know, I'm not political. If anything, I would like to see the climate change happen, so the science could be proved right, regardless of the consequences. This isn't being political, it is being selfish.

Cheers

Phil

IPCC stuff ---- just for interest !!!

IPCC ASKED TO COME CLEAN OVER CONTROVERSIAL HOCKEY STICK STUDIES
The Committee on Energy and Commerce, 23 June 2005
[2]http://energycommerce.house.gov/108/Letters/062305_Pachauri.pdf
Joe Barton, Chairman
U.S. House of Representatives
June 23, 2005
To: Dr. Rajendra K. Pachauri
Chairman
Intergovernmental Panel on Climate Change
C/O IPCC Secretariat
World Meteorological Organization
7 bis Avenue de La Paix
C.P. 2300
Ch- 1211 Geneva 2 Switzerland
Dear Chairman Pachauri:
Questions have been raised, according to a February 14, 2005 article in The Wall Street Journal, about the significance of methodological flaws and data errors in studies by Dr. Michael Mann and co-authors of the historical record of temperatures and climate change. We understand that these studies of temperature proxies (tree rings, ice cores, corals, etc.) formed the basis for a new finding in the 2001 United Nation's Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding - that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1,000 years" and that the "1990s was the warmest decade and 1998 the warmest year" - has since been referenced widely and has become a prominent feature of the public debate surrounding climate change policy.
However, in recent peer-reviewed articles in Science, Geophysical Research Letters, Energy & Environment, among others, researchers question the results of this work. As these researchers find, based on the available information, the conclusions concerning temperature histories - and hence whether warming in the 20th century is actually unprecedented - cannot be supported by the Mann et. al. studies. In addition, we understand from the February 14 Journal and these other reports that researchers have failed to replicate the findings of these studies, in part because of problems with the underlying data and the

calculations used to
reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according
to the January 2005
Energy & Environment, the information necessary to replicate the analyses in the studies
has not been made fully available to researchers upon request.
The concerns surrounding these studies reflect upon the quality and transparency of
federally
funded research and of the IPCC review process - two matters of particular interest to the
Committee. For example, one concern relates to whether IPCC review has been sufficiently
robust
and independent. We understand that Dr. Michael Mann, the lead author of the studies in
question, was also a lead author of the IPCC chapter that assessed and reported this very
same work, and that two co-authors of the studies were also contributing authors to the
same chapter. Given the prominence these studies were accorded in the IPCC TAR, we seek to
learn more about the facts and circumstances that led to acceptance and prominent use of
this work in the IPCC TAR and to understand what this controversy indicates about the data
quality of key IPCC studies.
In light of the Committee's jurisdiction over energy policy and certain environmental
issues
in the U.S. House of Representatives, the Committee must have full and accurate information
when considering matters relating to climate change policy. We open this review because the
dispute surrounding these studies bears directly on important questions about the federally
funded work upon which climate studies rely and the quality and transparency of analyses
used
to support the IPCC assessment process. With the IPCC currently working to produce a fourth
assessment report, addressing questions of quality and transparency in the underlying
analyses
supporting that assessment, both scientific and economic, are of utmost importance if
Congress
is eventually going to make policy decisions drawing from this work.
To assist us as we begin this review, and pursuant to Rules X and XI of the U.S. House of
Representatives, please provide the following information requested below on or before July
11,
2005:
1. Explain the IPCC process for preparing and writing its assessment reports, including,
but
not limited to: (a) how referenced studies are reviewed and assessed by the relevant
Working Group; (b) the steps taken by lead authors, reviewers, and others to ensure the

data underlying the studies forming the basis for key findings - particularly proxy and temperature data - are accurate and up to date; and (c) the IPCC requirements governing the quality of data used in reports.

2. What specifically did IPCC do to check the quality of the Mann et. al. studies and underlying data, cited in the TAR? Did IPCC seek to ensure the studies could be replicated?

3. What is your position with regard to: (a) the recent challenges to the quality of the Mann et. al. data, (b) related questions surrounding the sharing of methods and research for others to test the validity of these studies, and (c) what this controversy indicates about the data quality of key IPCC studies?

4. What did IPCC do to ensure the quality of data for other prominent historical temperature or proxy studies cited in the IPCC, including the Folland et. al. and Jones et. al. studies that were sources for the graphic accompanying the Mann et. al. graphic in the Summary for Policy Makers? Are the data and methodologies for such works complete and available for other researchers to test and replicate?

5. Explain (a) the facts and circumstances by which Dr. Michael Mann served as a lead author of the very chapter that prominently featured his work and (b) by which his work became a finding and graphical feature of the TAR Summary for Policymakers.

6. Explain (a) how IPCC ensures objectivity and independence among section contributors and reviewers, (b) how they are chosen, and (c) how the chapters, summaries, and the full report are approved and what any such approval signifies about the quality and acceptance of particular research therein.

7. Identify the people who wrote and reviewed the historical temperature-record portions of the TAR, particularly Section 2.3, "Is the Recent Warming Unusual?" and explain all their roles in the preparation of the TAR, including, but not limited to, the specific roles in the writing and review process.

8. Given the questions about Mann et. al. data, has the Working Group I or the IPCC made any changes to specific procedures or policies, including policies for checking the quality of data, for the forthcoming Fourth Assessment Report? If so, explain in detail any such changes, and why they were made.

9. Does the IPCC or Working Group I have policies or procedures regarding the disclosure and dissemination of scientific data referenced in the reports? If so, explain in detail any such policies and what happens when they are violated.

Thank you for your assistance. If you have any questions, please contact Peter Spencer of the Majority Committee staff at (202) 226-2424.

Sincerely,

Joe Barton Chairman Chairman

Ed Whitfield

Subcommittee on Oversight and Investigations
cc: The Honorable John Dingell, Ranking Member
The Honorable Bart Stupak, Ranking Member,
Subcommittee on Oversight and Investigations
EDITOR'S NOTE: The House of Representatives has also written to National Science Foundation
Director Arden Bement, Dr. Michael Mann, Dr. Malcolm K. Hughes, and Dr. Raymond S. Bradley,
requesting information regarding their global warming studies; see "Letters Requesting
Information Regarding Global Warming Studies" at
[3]http://energycommerce.house.gov/108/Letters/06232005_1570.htm

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
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University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. http://mustelid.blogspot.com/2005/06/first-look-at-scs-msu-vn52.html
2. http://energycommerce.house.gov/108/Letters/062305_Pachauri.pdf
3. http://energycommerce.house.gov/108/Letters/06232005_1570.htm

545. 1120676865.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Neville Nicholls" <N.Nicholls@bom.gov.au>
Subject: RE: Misc
Date: Wed Jul 6 15:07:45 2005

Neville,
Mike's response could do with a little work, but as you say he's got the tone
almost dead on. I hope I don't get a call from congress ! I'm hoping that no-one
there realizes I have a US DoE grant and have had this (with Tom W.) for the last 25
years.
I'll send on one other email received for interest.
Cheers
Phil
At 14:21 06/07/2005, you wrote:

Thanks Phil.
I had seen the estimates of 0.12C for UAH 5.2, but wasnt sure if the version producing
these trends had all the months corrected, and that John was happy with the corrections
(I had heard that his initial estimate was that the change made a major difference to
the trends, but that later calulations didnt support this). I think I have a pretty good
idea now of the trends in the various data sets.
I have seen the Mears/Wentz paper, but will watch out for John's paper (I know I could

have asked John about all of this, but I suspect he feels a bit over-burdened and
harrassed at the moment, and I didnt want to add to the pressure on him, so thanks for
passing this stuff on to me).
I thought Mike Mann's draft response was pretty good - I had expected something more
vigorous, but I think he has got the "tone" pretty right. Do you expect to get a call
from Congress?
Neville Nicholls
Bureau of Meteorology Research Centre
9th Floor, 700 Collins Street
Docklands,Melbourne, AUSTRALIA
PO Box 1289K, Melbourne, AUSTRALIA 3001
Phone: +61 (0)3 9669 4407
Fax: +61 (0)3 9669 4660
-----Original Message-----
From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]
Sent: Wed 7/6/2005 5:57 PM
To: Neville Nicholls
Subject: Fwd: Misc
Neville,
Here's an email from John, with the trend from his latest version in. Also
has trends for RATPAC and HadAT2. If you can stress in your talks that it is
more likely the sondes are wrong - at least as a group. Some may be OK
individually. The tropical ones are the key, but it is these that least is know
about except for a few regions. The sondes clearly show too much cooling in
the stratosphere (when compared to MSU4), and I reckon this must
also affect their upper troposphere trends as well. So, John may be putting
too much faith in them wrt agreement with UAH.
Happy for you to use the figure, if you don't pass on to anyone else. Watch
out for Science though and the Mears/Wentz paper if it ever comes out.
Also, do point out that looking at surface trends from 1998 isn't very clever.
Cheers
Phil
>Date: Tue, 05 Jul 2005 07:59:51 -0500
>From: John Christy <john.christy@nsstc.uah.edu>
>User-Agent: Mozilla/5.0 (Macintosh; U; PPC Mac OS X Mach-O; en-US; rv:1.4)
>Gecko/20030624 Netscape/7.1
>X-Accept-Language: en-us, en
>To: Phil Jones <p.jones@uea.ac.uk>
>Subject: Misc
>X-NSSTC-MailScanner: Found to be clean
>X-NSSTC-MailScanner-SpamCheck: not spam (whitelisted),
>   SpamAssassin (score=-5.8, required 5, BAYES_01 -5.40,
>   RCVD_IN_ORBS 0.11, SIGNATURE_LONG_SPARSE -0.49,
>   USER_AGENT_MOZILLA_UA 0.00)
>X-MailScanner-From: john.christy@nsstc.uah.edu
>X-Spam-Score: 0.0
>X-Spam-Level: /
>X-Spam-Flag: NO
>
>Hi Phil:
>
>I've been getting round-about versions of rumors concerning our newly
>adjusted version 5.2 LT dataset. I believe I had indicated earlier to you
>that the correction was within our published margin of error. In any case
>here are the numbers that describe various aspects of v5.2

>1979-2004
>
>Global Trend +0.115 UAH, +0.125 RATPAC and +0.137 HadAT (note, when
>subsampled for the same latitudes in which sonde observations are
>available, UAH and HadAT are almost exactly the same.)
>
>Update of site by site comparison of UAH LT 5.2 and SH radiosondes from
>Christy and Norris 2004:
>
>All 87 SH stations, no adjustments Raobs + 0.028 UAH +0.040
>74 best sites with adjustments   Raobs +0.030 UAH +0.054
>
>These SH changes from the original publication were very minor because
>most stations were outside the tropics where the diurnal error had
>essentially no impact.
>
>A paper by Sherwood claims that Day minus Night is a legitimate way to go
>about looking at sonde problems. The real problem though is that Day
>minus Night is only an indicator of a sonde change, it does not determine
>the change itself. Most notorious is the Philipps Mark III to Vaisala
>RS-80 where the night warmed by about 0.3 C and the day by a little bit
>less, which means the Day minus Night reveals a negative shift when in
>fact both ob times have a significant positive shift (these sondes form a
>signifciant part of the LKS dataset). Similar results occur for US VIZ
>mini-art 2 to Micro-art software in 1990.
>
>I have many other sone comparisons, and all are more consistent with the
>UAH trends more than RSS and certainly VG. Indeed, I was curious to see
>that your name was on VG's latest paper. I wish I had time to fill you in
>on why the addition of the non-linear terms is a red herring (both UAH and
>RSS have performed the calculations with and without the non-linear terms
>with no impact on the trends) and why the latitudinal difference for
>calculating the coefficients leads one astray. I'm a little nervous now
>that you may have a "dog in this fight" as we say in Alabama while writing
>up the IPCC. I expect my sonde comparisons to be included in the IPCC and
>I will have further results demonstrating the problems with the Day minus
>Night technique within a few months.
>
>I've lots to do now. Thanks for listening.
>
>John C.
>
>--
>**
>John R. Christy
>Director, Earth System Science Center   voice: 256-961-7763
>Professor, Atmospheric Science   fax: 256-961-7751
>Alabama State Climatologist
>University of Alabama in Huntsville
>[2]http://www.nsstc.uah.edu/atmos/christy.html
>
>Mail: ESSC-Cramer Hall/University of Alabama in Huntsville, Huntsville
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>
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References

1. mailto:p.jones@uea.ac.uk
2. http://www.nsstc.uah.edu/atmos/christy.html

546. 1121103374.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: One small thing
Date: Mon Jul 11 13:36:14 2005

Kevin,

In the caption to Fig 3.6.2, can you change 1882-2004 to 1866-2004 and add a reference to Konnen (with umlaut over the o) et al. (1998). Reference is in the list. Dennis must have picked up the MSLP file from our web site, that has the early pre-1882 data in. These are fine as from 1869 they are Darwin, with the few missing months (and 1866-68) infilled by regression with Jakarta. This regression is very good (r>0.8). Much better than the infilling of Tahiti, which is said in the text to be less reliable before 1935, which I agree with.
Cheers
Phil

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---

547. 1121294040.txt
##########

From: Bette Otto-Bliesner <ottobli@ucar.edu>
To: hegerl@duke.edu
Subject: Re: Senstivity, LGM and otherwise
Date: Wed, 13 Jul 2005 18:34:00 -0600 (MDT)
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov, Keith Briffa <k.briffa@uea.ac.uk>, francis.zwiers@ec.gc.ca

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Hi Gabi,

Here is the section from the FOD draft that includes the new PMIP-2 results. The radiative forcings have been modified based on new calculations. Note the PMIP-2 LGM model results included in the FOD do not include vegetation or atmospheric aerosol changes so for these results the radiative forcing estimate is 5.7 +/- 1.3 W/m2.

Bette

_____
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Fax: 303-497-1348
Email: ottobli@ncar.ucar.edu
_____

On Wed, 13 Jul 2005 hegerl@duke.edu wrote:

>
>
> Hi chapter 6,
>
> I am getting a bit nervous about the sensitivity stuff, since
> chapter 10 wants our version from us (blush nowhere near there)
> for their summary of all things sensitivity - so I am in the middle
> of the pipeline....
> ALl I'd need is the text from the ZOD, if you want to update anything
> or make me aware of refs, thats fine, but not as urgent.
> Did the ZOD have the ice age sensitivity?
>
> thank you and sorry...
>
> Gabi
>
> --
> Gabriele Hegerl
> Dept. of Earth and Ocean Sciences, Nicholas School of the Environment
> Duke University, Durham NC 27708
> phone 919-684-6167, fax 919-684-5833
> email: hegerl@duke.edu http://www.eos.duke.edu/Faculty/hegerl.html
> --
>
>
>
</x-flowed>

Attachment Converted: "c:\eudora\attach\What do ice ages tell us_071105.doc"

548. 1121392136.txt
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##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: cddhr@giss.nasa.gov, rahmstorf@ozean-klima.de, Bette Otto-Bleisner
<ottobli@ncar.ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, joos
<joos@climate.unibe.ch>, olgasolomina@yandex.ru, Eystein Jansen
<eystein.jansen@geo.uib.no>, jto@u.arizona.edu

Subject: IMPORTANT - The next steps for chapter 6 enroute to THE FOD
Date: Thu, 14 Jul 2005 21:48:56 -0600

<x-flowed>
Hi all - in the last few emails, we have suggested that you serve as "head" lead authors for the various sections of our chapter. One main purpose of this email is to make sure you are comfortable with the responsibility and have time for it. The other main goal is to explain what is expected of each of you.

First, here's a list of who's heading what sections. We picked you guys since you have proven to be intellectual leaders on the team, but also because you have track records of getting the job done on time. The one person we worry about is Olga, since she is leaving soon for the field, but nonetheless, we'd like all her input on Box 6.3 before she leaves. We will take over after then.

Exec Summary and Section 6.1 - PECK and EYSTEIN
Section 6.2 - DAVID
Section 6.3 - STEFAN
Section 6.4 - BETTE
Section 6.5 - KEITH
Section 6.6 - FORTUNAT
Box 6.1 - DAVID
Box 6.2 - FORTUNAT
Box 6.3 - OLGA
Box 6.4 - KEITH

Second, what is needed? Here is a list that has come to mind. We'd like you all to comment on this list (use the email list used for this email), so that we all agree about what we're doing in the next couple weeks.

1) Your primary job is to make sure your section (text, tables, figs and refs) is as perfect as possible. Each of us has to be careful about how we schedule things so that we have the job DONE by July 24.

2) Each of you should solicit feedback and edits from the ENTIRE LA team, plus relevant CAs. This is obviously to get the best ideas possible, but also to ensure that all on the LA team have had input. Please create a check list and make sure that you have some sort of feedback (at least an "OK") from each LA. We suggest you start asap, and don't expect LAs to just respond to the emails we just sent - many of the LAs just don't respond in a timely fashion (thankfully, you guys are not on that list!).

2.5) Monitor all chapter listserv traffic for your input, as some LAs prefer to communicate only in that way.

3) Please explicitly ask for feedback on the text, tables, figs and refs.

4) With respect to text, try hard to get it down to size (see below), and to ensure that it is FOCUSED on only that science which is policy relevant. ALL TEXT should support an Exec Summary Bullet. If it doesn't the text should be removed, or a bullet created for discussion with our team. Also, although it is ultimately our job to try to make the chapter flow as one document, please do what you can to make your section's text flow with the other sections. Look to make sure all information is compatible across sections, and that the same type of language/style is used (to the extent you can.

4.4) We hope that you will start your process by reading THE ENTIRE CHAPTER carefully, and sending your comments for each section to the

"head" LA for that section. This will get things moving fast, and help with the compatibility issues mentioned in #4 above.

5) With respect to the figures (and table), make sure each one is as compelling as possible. To save space (see below) you might decide a figure has to go. You might decide a new figure has to be included (only if there is space!). Work to get the figure redrafted where needed to be perfect - a sign of ultimate success will be that our figs get into the TS/SPM docs. Peck will be on that team, and will push hard, but figures MUST BE POLICY RELEVANT AND COMPELLING.

6) With respect to refs, please make sure that only the most relevant ones are cited, and that all of the citations are complete and entered into your copy of the master chapter endnote file. Although we expect to cite our own work where it makes sense, please be double sure that we're not going overboard in this regard - it won't look good to the outside world (e.g., skeptics) if we appear self-serving at all.

7) If you run into any debates that can't be easily solved (i.e. with all LAs happy), please consult with us. It is our job to make the ultimate calls, since someone has to do it. Again, it is our goal to make sure that no one is left with a bad feeling about our product. On the other hand, we have to make sure we stick to only the best science.

8) We'll be asking to make sure we have all the CAs listed. Let us know if you need to consult with any new ones. AGain, we must do what it takes to get the science and message as perfect as possible. CA consultation at this point is encouraged where it will help. For example, we need to get out the Pre-Q box to some Pre-Q experts - we are discussing w/ David.

9) At any point you need input, ask. We are happy to talk on the phone, and can call you or a group if you want a conference call. We are doing this already, and it can save lots of time. Or email. Both of us will be mostly around save a day or two.

10) Size and need to cut some sections. Because of recent changes in the TSU, we haven't been able to get the latest word, but we suspect that our comments in the FOD draft just sent are true - some sections have a real space issue (factor in figures), others less so. We'll provide more on this soon, and we expect that if you follow the above guidelines, you'll be getting things into more focus, and hopefully less space - especially section 6.3. When thinking about Figs, Tables and Refs, also be thinking "How can I save space?"

11) Feel free to bring in other LAs to help you coordinate. For example, for section 6.3, Bette and Dominique (to be back soon) can be a big help, Stefan. Keith is working with Tim and Ricardo, but also some others to do the job he has left. Etc.

12) We will start sending more info next week, and will help reach consensus on what we're doing, and by when if needed. Let us know what we've missed, and what might be wrong or unclear.

Ok, that's more than enough.

Thanks again for helping us lead the next big push!

Best, Peck and Eystein
--
Jonathan T. Overpeck

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549. 1121439991.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: paleoT
Date: Fri Jul 15 11:06:31 2005

Tom,
This Briffa series is just a three site average (trees from Tornetrask, Polar Urals
and
Taimyr) - all in northern Eurasia. It is therefore for a limited region and is likely
just the summer, whereas some of the others have regressed on annual T for the NH (or north of 20N).
Of these 3, the first two are in most of the other series (Esper, Crowley, Jones, Mann)
and also for HF in Moberg. Not sure whether Taimyr is in any of the others. Esper uses a different standardization approach, but should have most of the same trees, but only TRW. The others use our reconstructions which have MXD is as well.
Have you tried these correlations after extracting the LF trends (say residuals
from a 30 or 50 yr filter)? Would expect some of them to be much, much lower.
Keith's reconstruction that would be much better is the one that goes back to only about 1400. Do you have this? Go here [1]http://www.ncdc.noaa.gov/paleo/paleo.html
then click on paleo data, then on obtaining and look for Keith's - it says 600 years in
the title. You can get the data.
Cheers
Phil
At 21:57 14/07/2005, you wrote:

Phil,
I eventually refiltered all the paleo data and have compared these with likewise filtered MAGICC output. Very interesting results.
Can you comment, off the record, on Keith's paleo series.
Here are correlations of individual series against the 7 series average.
(Different series lengths, but essentially same results over common lengths.)

SERIES	1000-1610	1610-1995	1000-1995
Briffa	-.272	.262	.207
Esper	.583	.917	.687
Crowley	.879	.946	.902

Jones	.773	.917	.861
Mann	.760	.856	.822
M&J-NH	.929	.965	.936
Moberg	.904	.856	.871

Correlations with the climate model are not the same -- but Briffa is again the clear outlier.
Why?
Tom.

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References

1. http://www.ncdc.noaa.gov/paleo/paleo.html

550. 1121686753.txt
##########

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Your spaghetti figure
Date: Mon, 18 Jul 2005 07:39:13 +0200
Cc: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

<x-flowed>
Hi,
if what Tom writes is correct, then I would think it is not necessary to have a separate paper. But we need to be sure so as not to break any of the regulations since this will be one of the most scrutinized sections of the whole 4AR. I guess it is now up to how Keith and Tim takes the MWP box further and what ends up in the figure.

Cheers,
Eystein

At 21:35 -0600 17-07-05, Jonathan Overpeck wrote:
>Hi Tom - thx for the quick response. It sounds
>like you don't need to do the extra pub. Keith
>and Eystein, do you agree? Tom can help make
>sure everything is ok, and should probably be a
>Contributing Author for the effort. Is that
>appropriate, all? Tom has already given us lots
>of useful review comments, and I suspect (am I
>right, Tom) that would be willing to review some
>more, in addition to helping make sure Keith and
>Tim get the figure we're thinking about right?
>Of course, if we run into a methodological or
>space problem, the fig might still not make it,
>but Keith, Eystein and I talked and have agreed
>that it would be good to hammer home that
>available data do not support the concept of a

>single (or multiple) globally synchronous (e.g.,
>to the degree that the late 20th century is)
>warm events during anyone's definition of
>Medieval times. We also agreed that this fig
>would focus on that issue only, and not Medieval
>warmth vs 20th century. This amplitude issue is
>dealt with in the main "temps of the last 2K"
>figs that Tim and Keith produced. But, given all
>the misunderstanding and misrepresenting that is
>going on wrt to the Medieval Warm Period, we
>concluded that it's worth the extra space to
>address the issue in more than one way - hence
>the decision to try to do something along the
>lines of your figure.
>
>It's in Keith and Tim's hands for the next step - they're working away.
>
>Thanks again to all, best, peck
>
>Thx, peck
>
>>Quoting Jonathan Overpeck <jto@u.arizona.edu>:
>>
>>
>>Jonathan, can do, but I am wondering if we need to - seven of the curves have
>>been processed in the way we describe in the
>>Hegerl et al paper to nature that
>>gabi sent you - s.d.s even listed in
>>supplementary file. the only exception is
>>the Alberta record, which Lockhart (sp?)
>>extended recently to about 900 - that
>>is published too - so each of the records has
>>gone through some peer-processing
>>- so should the figure itself, based on those data, still require an extra
>>reference? if so I will still do it, but I
>>wonder if it is needed. please get
>>back to me soon on this, tom
>>
>>> Hi Tom - Looks like we (Keith) is going to try to come up w/ a new
>>> version of your figure for our MWP Box. We're banking on Susan giving
>>> us the extra space for this and a couple other things, but I
>>> recommend you do that quick EOS paper you mentioned. Still ok?
>>>
>>> Many thanks.
>>>
>>> best, peck
>>> --
>>> Jonathan T. Overpeck
>>> Director, Institute for the Study of Planet Earth
>>> Professor, Department of Geosciences
>>> Professor, Department of Atmospheric Sciences
>>>
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>
>
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_____
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551. 1121721126.txt
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##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: jto@u.arizona.edu,eystein.jansen@geo.uib.no,tcrowley@duke.edu
Subject: thoughts and Figure for MWP box
Date: Mon Jul 18 17:12:06 2005

   Dear Peck, Eystein and Tom
   At this point we thought it was important to review where we think we are with
the MWP
   Figure.
   First, we have no objection to a Figure . Our only concerns have been that we
should
   1/... be clear what we wish this Figure to illustrate (in the specific context
of the MWP
   box) - note that this is very different from trying to produce a Figure in such a
way as to
   bias what it says (I am not suggesting that we are, but we have to guard against
any later
   charge that we did this). We say this because there are intonations in some of
Peck's
   previous messages that he wishes to "nail" the MWP - i.e. this could be
interpreted as
   trying to say there was no such thing, and
    2/ ...agree that we have done this in the best way.
   The truth is that there IS a period of relative warmth around the end of the 1st

and start
of the 2nd millennium C.E. , but that there are much fewer data to base this conclusion on
(and hence the uncertainty around even our multiple calibrated multi-proxy reconstructions
are wide). The geographical spread of data also impart a northern (and land) bias in our
early proxy data. My understanding of Tom's rationale with the Figure is that we should
show how, because the timing of maximum pre-20th century warmth is different in different
records, the magnitude of the warmest period (for the Hemisphere , or globe, as a whole) is
less than the recently observed warmth.
The reconstructions we plot in Chapter 6 already express the mean Hemispheric warmth (after
various selection and scaling of data), and so the additional information that the MWP box
figure should show must relate to the scatter of the proxy data. There seems to be a
consensus that this is best done by showing individual records , and we are happy to agree.
What we worry very much about, however, is that we should not produce a Figure that then
conflicts with the picture of proxy evidence for Hemispheric mean warmth as a whole,shown
in the main Chapter Figure. By showing a composite (as Tom has done) and scaling against
another (30-90degrees N) temperature record - this is just what is done.
As we promised, Tim has produced a similar Figure, using the same series plus a few extras,
but omitting the composite mean and the scaling against instrumental temperatures. The idea
was to include as many of the original input series (to the various reconstructions) as we
could - though avoiding conflicting use of different versions of the same data. The
precise selection of records will have to be agreed and, presumably, based on some clear,
objective criteria that we would need to justify (this will not be straight forward). This,
along with Tom's plot (forwarded by Peck) is in the attachment.
We would like to get your opinion now, and especially Tom's, on the points regarding the
composite and scaling. We would be in favour of just showing the series - but do they make
the point (and emphasise the message of the text in the box)? Or does the scatter of the
various series as plotted, dilute the message about the strength of 20th century mean
warming (note the apparently greater scatter in the 20th century in our figure than in
Tom's)? Can you all chip in here please.
best wishes
Keith and Tim
P.S. We agreed in Beijing that we should definitely ask Tom to be a CA .

--
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Phone: +44-1603-593909
Fax: +44-1603-507784

552. 1121869083.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: Re: thoughts and Figure for MWP box
Date: Wed Jul 20 10:18:03 2005

Date: Tue, 19 Jul 2005 15:38:31 +0100
To: Tom Crowley <tcrowley@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: thoughts and Figure for MWP box
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>
Tom et al
thanks for remarks - in response to Tom's questions
At 18:23 18/07/2005, Tom Crowley wrote:

a few comments -
1) are you trying to choose between my way of presenting things and your way - ie, w
w/out composite?

Yes

2) with your data, do they all go through from beginning to end?

pretty much - and have been standardised over the maximum period for each (not necessarily the best way?)

3) why include chesapeake, which is likely a salinity record?

Because Moberg used it in their latest reconstruction - I agree that I would not use it
because of the dubious temperature signal (salinity effect and no local replication) and
poor dating control (and I do not like the way the Moberg method effectively over
weights the low-frequency predictor series in their analysis).

4) some of your data are from virtually the same site - Mangazeja and yamal are both w.
siberia - I composited data available from multiple sites to produce one time series,
which is equally counted against the other regions, which might (greenland, w.U.S., e.
Asia) or might not have multiple records in them

Just to reiterate - I understood after the group chat with Susan S. in Beijing , that we
were being asked to try to produce a "cloud" diagram including as many of "original"
predictor series ,from all the reconstructions, to see if it provided an "obvious"
picture of the unprecedented warming over the last millennium or so. Tim and I are in no
way trying t produce a different Figure for the sake of producing a different Figure .
In practice this is hard to do (because some records are sensible "local"

composites
already, and how far do you go in showing all input data? The problem of what and how to
composite is tricky - and no obviously "correct" way is apparent.
Having said this , Tom's way is fine with me (provided the composites are robust) and we
get general agreement. Am happy to go with Tom's Figure , or version that incorporates
as many records as possible - but as we have said - without the composite or temperature
scaled add ons.

5) I am not sure whether it is wise to add me to the CA list, just because the reviewer
is supposed to be impartial and a CA loses that appearance of impartiality if he has now
been included as a CA - may want to check with Susan S. on this one to be sure - still
happy to provide advice

My own position on this is that you are an "unofficial" referee, who has (and still is)
making a significant contribution - I see no conflict

6) I am happy to go in either direction - include or not include my figure - all I need
are specific directions as to what to do, as CLAs you people need to decide, and then
just tell me what or what not to do

Agree - CLAs please rule on the individual record/composite question - I am very happy
to go with Tom's Figure. We did ours because we were asked to.

7) I am a little unhappy with the emphasis on hemispheric warmth - lets face it, almost
all of the long records are from 30-90N - the question is: how representative is 30-90N
to the rest of the world? for the 20th c. one can do correlations with the instrumental
record, but co2 has almost certainly increased the correlation scale beyond what it was
preanthropogenic.

Absolutely agree , and hope this comes over in text (and bullets) - if not needs
strengthening (note David R's comments).

you could correlate with quelcaya - not sure how many other records there are that are
annual resolution - in the tropics I have produced a tropical composite (corals +
Quelc.) but it only goes back to ~1780 - corals just don't live v long - in that
interval at least the agreement is satisfactory with the mid latitude reconstruction but
there is only 100 years extra of independent information beyond the instrumental
record..

We have gone round in circles over this , but understand consensus to be that Quelc. not
a clean temperature record. Agree corals would be better longer (the new

coral-based
reconstruction by Rob Wilson et al
goes back to 1700 and shows unprecedented tropical warming . Along with the text from
Julie we can not go much further, but the importance of extending the tropical (and SH
records needs to be very clear)

.THIS MAY NEED TO BE ADDRESSEDAS A GENERAL ISSUE SOMEWHERE (SHORTLY) IN YOUR DOC

Really hope it is already - but advise if you think not

tom

Thanks for this - lets take lead from J and E now (also can you advise on state of play
with the Hegerl et al manuscript?)
thanks
Keith

Jonathan Overpeck wrote:

Hi Keith, Eystein and Tom: See below (BOLD) for my comments. Thanks for moving this
forward and making sure we do it right (i.e., without any bias, or perception of bias).

Dear Peck, Eystein and Tom
At this point we thought it was important to review where we think we are with the MWP
Figure.
First, we have no objection to a Figure . Our only concerns have been that we should
1/... be clear what we wish this Figure to illustrate (in the specific context of the
MWP box) - note that this is very different from trying to produce a Figure in such a
way as to bias what it says (I am not suggesting that we are, but we have to guard
against any later charge that we did this). We say this because there are intonations in
some of Peck's previous messages that he wishes to "nail" the MWP - i.e. this could be
interpreted as trying to say there was no such thing, and

SORRY TO SCARE YOU. I **ABSOLUTELY** AGREE THAT WE MUST AVOID ANY BIAS OR PERCEPTION OF
BIAS. MY COMMENT ON "NAILING" WAS MADE TO MEAN THAT ININFORMED PEOPLE KEEPING COMING
BACK TO THE MWP, AND DESCRIBING IT FOR WHAT I BELIEVE IT WASN'T. OUR JOB IS TO MAKE IT
CLEAR WHAT IT WAS WITHIN THE LIMITS OF THE DATA. IF THE DATA ARE NOT CLEAR, THEN WE HAVE
TO BE NOT CLEAR. THAT SAID, I THINK TOM'S FIGURE CAPTURED WHAT I HAVE SENSED IS THE MWP
FOR A LONG TIME, AND BASED ON OTHER SOURCES OF INFO - INCLUDING KEITH'S PROSE. THE IDEA
OF A FIGURE, IS THAT FIGURES CAN BE MORE COMPELLING AND CONNECT BETTER THAN TEXT. ALSO,
THERE ARE MANY WAYS TO LOOK AT THE MWP, AND AS LONG AS WE DON'T INTRODUCE BIAS OR
ANYTHING ELSE THAT WILL DILUTE THE MESSAGE IN THE END, THE IDEA IS TO SHOW THE

MWP IN
MORE WAYS THAN TWO (THAT IS, THE EXISTING FIGS IN THE TEXT THAT KEITH AND TIM MADE).

2/ ...agree that we have done this in the best way.
The truth is that there IS a period of relative warmth around the end of the 1st and
start of the 2nd millennium C.E. , but that there are much fewer data to base this
conclusion on (and hence the uncertainty around even our multiple calibrated multi-proxy
reconstructions are wide). The geographical spread of data also impart a northern (and
land) bias in our early proxy data.

NEED TO BE CLEAR ABOUT THIS BIAS IN THE CAPTION AND BOX TEXT

My understanding of Tom's rationale with the Figure is that we should show how, because
the timing of maximum pre-20th century warmth is different in different records, the
magnitude of the warmest period (for the Hemisphere , or globe, as a whole) is less than
the recently observed warmth.

YES, BUT IN A WAY THAT SAYS "LOOK, HERE ARE THE ACTUAL REGIONAL CURVES - CHECK IT OUT
FOR YOURSELF" INSTEAD OF JUST SAYING (IN A SCIENTIFICALLY MORE STANDARD MANNER - HERE
ARE THE VARIOUS, MOST ROBUST, LARGE AREA RECONSTRUCTIONS. IN MY MIND, THE LATTER
(KEITH/TIM FIGS IN THE MAIN TEXT) WILL BE THE MOST APPEALING/CONVINCING TO PALEOCLIMATE
SCIENTISTS, BUT TOM'S MIGHT HELP THERE, AND CERTAINLY WITH NON-PALEO SCIENTISTS AND
POLICY FOLKS. MIGHT HELP... IF IT DOESN'T NOTHING LOST, BUT IF IT COULD HURT CONVEYING
UNDERSTANDING, THEN ITS BAD TO USE THE NEW FIGURE.

The reconstructions we plot in Chapter 6 already express the mean Hemispheric warmth
(after various selection and scaling of data), and so the additional information that
the MWP box figure should show must relate to the scatter of the proxy data. There seems
to be a consensus that this is best done by showing individual records , and we are
happy to agree.
What we worry very much about, however, is that we should not produce a Figure that then
conflicts with the picture of proxy evidence for Hemispheric mean warmth as a
whole,shown in the main Chapter Figure. By showing a composite (as Tom has done) and
scaling against another (30-90degrees N) temperature record - this is just what is done.

ABSOLUTELY RIGHT - CAN'T HAVE CONFLICT.

As we promised, Tim has produced a similar Figure, using the same series plus a few
extras, but omitting the composite mean and the scaling against instrumental
temperatures. The idea was to include as many of the original input series (to the

various reconstructions) as we could - though avoiding conflicting use of different
versions of the same data. The precise selection of records will have to be agreed and,
presumably, based on some clear, objective criteria that we would need to justify (this
will not be straight forward). This, along with Tom's plot (forwarded by Peck) is in the
attachment.
We would like to get your opinion now, and especially Tom's, on the points regarding the
composite and scaling. We would be in favour of just showing the series - but do they
make the point (and emphasise the message of the text in the box)? Or does the scatter
of the various series as plotted, dilute the message about the strength of 20th century
mean warming (note the apparently greater scatter in the 20th century in our figure than
in Tom's)? Can you all chip in here please.
best wishes

WHAT ABOUT THE IDEA THAT WE ONLY SHOW THE SERIES FOR THE MWP, SINCE THE COMPARISON TO
THE 20TH CENTURY IS DONE WELL (AND BEST?) IN THE TEXT FIGS (WHICH I'M ATTACHING JUST IN
CASE TOM DOESN'T HAVE, ALONG WITH THE TEXT - IF YOU HAVE TIME, TOM, PLEASE READ COMMENT
ON ANYTHING YOU WISH, BUT CERTAINLY THE LAST 2000 YEARS BIT - ASSUME YOU'LL BE DOING
THIS AT THE REVIEW STAGE ANYHOW...)
ANOTHER THING THAT IS A REAL ISSUE IS SHOWING SOME OF THE TREE-RING DATA FOR THE PERIOD
AFTER 1950. BASED ON THE LITERATURE, WE KNOW THESE ARE BIASED - RIGHT? SO SHOULD WE SAY
THAT'S THE REASON THEY ARE NOT SHOWN? OF COURSE, IF WE ONLY PLOT THE FIG FROM CA 800 TO
1400 AD, IT WOULD DO WHAT WE WANT, FOCUS ON THE MWP ONLY - THE TOPIC OF THE BOX - AND
SHOW THAT THERE WERE NOT ANY PERIODS WHEN ALL THE RECORDS ALL SHOWED WARMTH - I.E., OF
THE KIND WE'RE EXPERIENCING NOW.
TWO CENTS WORTH

Keith and Tim
P.S. We agreed in Beijing that we should definitely ask Tom to be a CA .

TRUE - BUT HAS ANYONE CONFIRMED W/ TOM. TOM, YOU OK W/ THIS?
THANKS - A GREAT DISCUSSION, AND LETS SAY THE JURY IS STILL OUT ON THIS FIGURE UNTIL WE
ALL ARE COMFORTABLE WITH WHAT IT LOOKS LIKE IN THE END.
BEST, PECK

--
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Attachment converted: Macintosh HD:mwpbox_figures.pdf (PDF /«IC») (0008A8AE)

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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/

553. 1121871795.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: Re: the regional section and MWP Figure
Date: Wed Jul 20 11:03:15 2005

From: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
To: "Jonathan Overpeck" <jto@u.arizona.edu>,
"Edward R. Cook" <drdendro@ldeo.columbia.edu>
Cc: "Keith R. Briffa" <k.briffa@uea.ac.uk>,
<eystein.jansen@geo.uib.no>
Subject: Re: the regional section and MWP Figure
Date: Tue, 19 Jul 2005 15:35:39 -0300
X-Mailer: Microsoft Outlook Express 6.00.2800.1437
Dear Keith and Ed,
Please, find attached the new version of the SH figure for the IPCC. I have now included the New Zealand record. All the records have been scaled to 4 ˚C amplitude. Variability in the Tas record is reduced compared to New Zealand and Patagonian records. The reference lines is the mean used for the calibration period in each record, 15 C for New Zealand, 14.95 C for Tasmania and 0 C for the Patagonian records (they show departures). Please, let me know if you want to introduce some changes in the figure. The opposite phase in the Patagonia-New Zealand records is so clear before 1850, which is consistent with our previous TPI. For instance, in the instrumental record the 1971 and 1976 are the coolest summer in northern Patagonian during the past 70 years, but the warmest in New Zealand reconstruction!! This out of phase relationship between regions in the Southern Hemisphere points out to the difficulty of using few records to get a hemispheric average. Cheers,
Ricardo
----- Original Message -----
From: "Jonathan Overpeck" <jto@u.arizona.edu>
To: "edwardcook" <drdendro@ldeo.columbia.edu>
Cc: "Keith Briffa" <>; "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>;
"Eystein Jansen" <>

Sent: Monday, July 18, 2005 1:09 PM
Subject: Re: the regional section and MWP Figure
Thanks Ed - Ricardo, can you get the data from Henry? What do you think, Keith?
Best, Peck
>Given the nature of the SH and what Ricardo put
>together, I would keep the Australian and South
>African borehole records separate. Henry
>Pollack can provide them, I am sure. He gave an
>excellent talk at a meeting in Canberra that I
>recently participated in.
>
>Cheers,
>
>Ed
>
>P.S. Ricardo, here is the Oroko temperature reconstruction.
>
>JANUARY-MARCH TEMPERATURES RECONSTRUCTED FROM
>OROKO SWAMP, NEW ZEALAND SILVER PINE TREE RINGS
>BE ADVISED THAT THE DATA AFTER 1958 ARE INSTRUMENTAL
>TEMPERATURES

YEAR	TEMP °C
900	13.751
901	14.461
902	13.236
903	13.331
904	13.483
905	13.632
906	12.959
907	13.628
908	13.372
909	12.868
910	13.244
911	13.793
912	14.048
913	14.444
914	13.095
915	14.036
916	13.215
917	13.698
918	13.570
919	13.665
920	13.871
921	13.966
922	14.762
923	14.325
924	14.077
925	14.713
926	14.081
927	14.218
928	13.793
929	14.151
930	14.985
931	13.599
932	14.663
933	14.110
934	14.968
935	14.391
936	15.484
937	15.554
938	14.977
939	15.303

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>   940  15.179
>   941  15.591
>   942  14.737
>   943  14.007
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>   945  14.449
>   946  14.350
>   947  15.096
>   948  15.257
>   949  15.789
>   950  15.303
>   951  15.513
>   952  16.111
>   953  15.723
>   954  15.459
>   955  14.015
>   956  13.083
>   957  13.850
>   958  14.069
>   959  13.772
>   960  14.873
>   961  14.692
>   962  14.923
>   963  14.527
>   964  15.034
>   965  14.688
>   966  14.486
>   967  14.444
>   968  14.436
>   969  13.776
>   970  13.809
>   971  14.391
>   972  13.487
>   973  13.995
>   974  14.061
>   975  14.321
>   976  14.882
>   977  14.226
>   978  14.977
>   979  15.447
>   980  14.424
>   981  14.923
>   982  14.180
>   983  15.484
>   984  13.487
>   985  14.168
>   986  14.176
>   987  15.699
>   988  15.187
>   989  16.305
>   990  14.845
>   991  14.647
>   992  15.765
>   993  14.754
>   994  14.271
>   995  13.623
>   996  14.300
>   997  13.937
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>   999  14.011
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>  1011  13.793
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>  1013  13.755
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>  1027  13.194
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>  1029  13.091
>  1030  13.768
>  1031  13.813
>  1032  13.846
>  1033  13.871
>  1034  14.255
>  1035  14.370
>  1036  13.805
>  1037  14.576
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>  1074  13.846
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>  1095  14.688
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>  1107  15.005
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>  1163  15.092
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> 1998 15.417
> 1999 16.354
>
>On Jul 17, 2005, at 10:40 PM, Jonathan Overpeck wrote:
>
>>Thanks Ricardo and Ed! I personally am not a
>>big fan of the Jones and Mann SH recon. It is
>>based on so little. On the other hand, it is in
>>the literature. So, I leave it up to you and
>>Keith to decide - perhaps Eystein can weigh in
>>too.
>>
>>I do, however, think it would be really helpful

>>to included the borehole data (see prev.
>>emails) - either as a single SH curve, or
>>(probably better) two regional curves
>>(Australia and S. Africa). Is there a reason
>>this is not a good idea? Can't complain about
>>snow bias down there...
>>
>>Thanks again - I look forward to seeing the
>>next draft and figure - complete w/ borehole I
>>hope.
>>
>>thx, Peck
>>
>>>Hi Keith,
>>>
>>>Please, find attached my last version of the SH temp. As you know, Ed
Cook
>>>returned my original version of the SH with minor comments. Overall, he
>>>agreed with the text. Still I am waiting from him the Oroco Swamp data to
>>>include in the Figure, which first draft I sent you more than a month
ago.
>>>
>>>In the last version I have included a first paragraph referring to the
Jones
>>>and Mann (2003) temperature reconstruction for the SH. At that time we
have
>>>to decide if we want to have the hemispheric (Jones and Mann) and the
>>>regional views (Tasmania, New Zealand, Patagonia, maybe include
Antarctica
>>>(Ommem et al. 2005)), or just one of them. If we decide to stay with the
>>>hemispheric view, we should include Jones and Mann reconstruction at the
>>>bottom of one of your figures. In cases that we decide to maintain both
>>>hemispheric and regional views, we should include Jones and Mann at the
>>>bottom of my figure. Please, could you check with Peck and Eystein to
see
>>>the best way to proceed? Thanks,
>>>
>>>Ricardo
>>>
>>>
>>>
>>>----- Original Message -----
>>>From: "Keith Briffa" <k.briffa@uea.ac.uk>
>>>To: <jto@u.arizona.edu>; "Eystein Jansen" <Eystein.Jansen@geo.uib.no>
>>>Cc: <ricardo@lab.cricyt.edu.ar>; "Ed Cook" <drdendro@ldgo.columbia.edu>
>>>Sent: Friday, July 15, 2005 11:01 AM
>>>Subject: the regional section and MWP Figure
>>>
>>>> Guys
>>>> still need the SH temp bit from Ricardo/ED to edit and am exploring
the
>>>MWP
>>>> Figure - but the concept still is unclear to me - but we agreed to do
a
>>>> plot like Tom's . The regional section is still a worry - I am happy
to
>>>> very briefly edit the section on NAO (possibly incorporate the ENSO
>>>stuff)
>>>> but my understanding is that this section is best done (to incorporate
>>>also
>>>> the regional moisture work of Ed) by Ricardo /Ed with input my Peck.
This
>>>> is still my opinion. I also would appreciate feedback re the regional

>>>> forcing section that I think we may have to drop - but perhaps not.
>>>> Therefore I ask that when i get the SH temp stuff I will incorporate
it
>>>but
>>>> that you guys (Peck, Ricardo, Ed and Eystein interacting over the
North
>>>> Atlantic bit) first review and redo the regional section .
>>>> It is important to get feedback from Henry re the borehole stuff and
>>>> involve Tom in the debate with all of us , of the value of the Figure
. In
>>>> meantime , will experiment with the Figure and review existing text
and
>>>bullets
>>>> Keith
>>>>
>>>> Keith
>>>>
>>>>
>>>>
>>>> --
>>>> Professor Keith Briffa,
>>>> Climatic Research Unit
>>>> University of East Anglia
>>>> Norwich, NR4 7TJ, U.K.
>>>>
>>>> Phone: +44-1603-593909
>>>> Fax: +44-1603-507784
>>>>
>>>> [1]http://www.cru.uea.ac.uk/cru/people/briffa/
>>>>
>>>>
>>>
>>>Attachment converted: Macintosh HD:Southern
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>>
>>--
>>Jonathan T. Overpeck
>>Director, Institute for the Study of Planet Earth
>>Professor, Department of Geosciences
>>Professor, Department of Atmospheric Sciences
>>
>>Mail and Fedex Address:
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>>direct tel: +1 520 622-9065
>>fax: +1 520 792-8795
>>[2]http://www.geo.arizona.edu/
>>[3]http://www.ispe.arizona.edu/
>>
>==========
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>Doherty Senior Scholar and
>Director, Tree-Ring Laboratory
>Lamont-Doherty Earth Observatory
>Palisades, New York 10964 USA
>Email: drdendro@ldeo.columbia.edu
>Phone: 845-365-8618
>Fax: 845-365-8152
>==========

--
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[4]http://www.geo.arizona.edu/
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--
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Phone: +44-1603-593909
Fax: +44-1603-507784
[6]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.geo.arizona.edu/
3. http://www.ispe.arizona.edu/
4. http://www.geo.arizona.edu/
5. http://www.ispe.arizona.edu/
6. http://www.cru.uea.ac.uk/cru/people/briffa/

554. 1121875880.txt
##
##########

From: David Rind <drind@giss.nasa.gov>
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
Subject: Re: [Wg1-ar4-ch06] Comments on Section 6.3
Date: Wed, 20 Jul 2005 12:11:20 -0400
Cc: wg1-ar4-ch06@joss.ucar.edu

<x-flowed>
Dear Stefan,

The distinction here is that GCMs attempt to calculate from first
principles the zeroth and first order processes that dominate the
problem they are studying, whereas EMICs parameterize many of those
processes. The fact that EMICs can reproduce GCM results suggest that
their parameterizations have been tuned to do so - but this does not
in any way imply that if one alters the forcing or boundary
conditions outside of a small range, or apply them to completely
different problems, that the two types of models will react
similarly. In fact, there is a history of this - the first "EMICs"
had a very large sensitivity to a 2% solar insolation change; then
they had to be re-tuned to prevent that from happening. EMICs are
used for paleo-problems because of their ability to take large
time-steps, but there is no free lunch - in doing so, they sacrifice
calculating the fundamental physical processes the way the real world
does it. GCMs have storms, they have real water vapor transports,

they have winds calculated from solving the conservation of momentum equation, etc. etc. There is a quantum difference between the fundamental approaches - it is not a continuum, in which there are no real differences, everything is simply a matter of opinion, there is no such thing as truth - that's the argument that greenhouse skeptics use to try to make science go away.

Because we can't use GCMs for long-time scale problems, we do the best we can - we use these heavily parameterized models. If we could use GCMs for those problems, EMICs could then be tuned to produce the GCM results on those time-scales as well. But in this case we have no way to validate the EMIC results - and since the first principles are not being used, we cannot know whether they represent a physically consistent solution or not. Therefore all they can do is suggest interactions among processes, a useful though not definitive addition to the field.

David

ps - concerning CLIMBER-2, I asked a number of leading climate scientists to read the model description paper. Peter Stone was the only person I asked who thought the model was at all useful for studying the types of problems we are discussing. And it was not only GCM scientists. If you want to hear further cogent arguments concerning its inapplicability, consider contacting Bill Rossow (the recent winner of a major honor as a leading climate scientist) but make sure your email program or telephone accepts unexpurgated text.

At 4:22 PM +0200 7/20/05, Stefan Rahmstorf wrote:
>Dear David,
>
>I take from your response that you consider all models that
>parameterise an important first-order process "conceptual models". I
>can live with that - but then there are only conceptual climate
>models around. Any coupled climate GCM that I know of parameterises
>oceanic convection (and in a very crude way), hence it is a
>conceptual model in your terms, and there is no fundamental
>distinction of category between your model and our model.
>
>To me the scientific question is not whether an important process is
>parameterised (many are in GCMs) - it is how well this
>parameterisation works, for the task at hand. We have tested the
>feedbacks in great detail (e.g., the cloud, water vapour, lapse rate
>and snow/ice albedo feedbacks for 2xCO2) in our model and they
>perform quantitatively within the range simulated by various GCMs.
>The same is true for many other diagnostics - the model has taken
>part in model intercomparisons with GCMs and always falls within the
>range of different GCMs, in a quantitative way. To repeat that
>point, the quantitative differences between different GCMs are
>larger than the typical difference between our model and a GCM. So I
>see no basis for your claim that this model can only "suggest orders
>of magnitude". That's just plain wrong from all the evidence that I
>have seen (a lot). If you have concrete evidence to the contrary,
>other than just knowing one person who happens to agree with you,
>please come forward with it.
>
>Stefan
>
>--
>To reach me directly please use: rahmstorf@ozean-klima.de
>(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)
>
>Stefan Rahmstorf

>www.ozean-klima.de
>www.realclimate.org

_____
Wg1-ar4-ch06 mailing list
Wg1-ar4-ch06@joss.ucar.edu
http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
</x-flowed>

555. 1121876302.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Tom Crowley <tcrowley@duke.edu>
Subject: Re: CLA feedback on Tom and the MWP
Date: Wed Jul 20 12:18:22 2005
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>,t.osborn@uea.ac.uk

Hi all
think this is resolved now (virtually) -
We use series that total to Tom/Gabi composite , and we can cite this as an example of the
scatter of regional records "in a typical reconstruction". This avoids very difficult issue
of what is the best way to aggregate certain data sets - we are simply illustrating the
point with one published (by then) data set.
The issue of the composite is then not an issue either , because it is not a new
(unpublished) composite that we were concerned about - though I still believe it is a
distraction to put the composite in. It would be best to use data from 800 or 850 at least
, and go to 1500 (?) and presumably normalise over the whole period of data shown. OK? Even
though you guys all wish to go with the reduced period (ie not up the present) , but my own
instinct is that this might later come back to haunt us - but will take your lead.
I agree the look of the Figure should match the others.
So, if Tom will send the data sets (his regional curves) , Tim will plot and send back asap
for scrutiny. Thanks Tom and thanks for your help with this - further comments on latest
version of 6.5 (last 2000 years) still welcome , though will be incorporating a few changes
in response to David and Fortunat input , and SH bit (from Ricardo and Ed) still to go in
and regional section to be revised (after input from Peck et al.)
cheers
Keith
.

At 21:42 19/07/2005, Jonathan Overpeck wrote:

Hi Keith and Tim: Just got off the phone with Eystein, and hopefully he will sleep ok
knowing that we have a plan for the MWP fig and Tom...
Please ask questions if we don't cover all the key points, but here's what we think:
1) the MWP fig should span the MWP only, and should emphasize variation in regional

amplitude (we agree that we must be clear that this fig is not a reconstruction) - that
is, it is best to use time series representing regions, assuming that the regional
series do represent a region ok with one or more input series. We want to avoid a
regional bias if we can - this is what got us into all the MWP misunderstanding in the
first place, perhaps (e.g., nice MWP in Europe/Atlantic region - must be global)
2) If you guys could agree on the series and the interval, that'd be great. We agree it
would be good to start before 1000 and end before the Renaissance (15th century?). If
you want more feedback on these issues, we're happy to provide, but it seems logical
that you pick series and intervals so that each series covers the entire interval
selected.
3) Don't use the Chesapeak record - it is likely biased by salinity
4) We'd like Keith and Tim to draft the final figure so that it matches the look and
style of the other two figs they have made. Hope this is doable. Tom, does Keith have
all the data? Thanks for sending if not.
5) We agree that Tom should NOT be a CA given that he was officially one of the ZOD
reviewers. Of course, this doesn't represent a real conflict, but we need to avoid even
the appearance of conflict. We greatly appreciate all the feedback that Tom is
providing! Is this plan ok w/ you Tom? We think you're cool with it, but just want to
check one more time.
That... it is. Please let us know if there are any more questions. Keith - feel free to
try and get Eystein on his cell doing your work hours if you want quick feedback. Or we
can do this by email - he's not in a very email friendly place right now, but the
fishing appears to be ok.
Again, thanks to you both for all the discussion and thought that has gone into this
figure.
Best, peck
--
Jonathan T. Overpeck
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References

1. http://www.geo.arizona.edu/
2. http://www.ispe.arizona.edu/
3. http://www.cru.uea.ac.uk/cru/people/briffa/

556. 1121877545.txt
##########

From: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
To: David Rind <drind@giss.nasa.gov>
Subject: Re: [Wg1-ar4-ch06] Comments on Section 6.3
Date: Wed, 20 Jul 2005 12:39:05 +0200
Cc: Eystein Jansen <Eystein.Jansen@geo.uib.no>

Dear chapter 6 friends,
I have a request on procedure. In the interest of a good and constructive working atmosphere, I would suggest that all of us focus on sober scientific arguments and refrain from unneccessarily derogatory comments about the work of colleagues. I'm referring in this case to David's comment

- this reference is overused, especially for such a simplistic model

The reference concerned is our theory of DO events which appeared in Nature in 2001 and has since been cited 133 times according to the Web of Science (a sign of overuse?) The model concerned is the CLIMBER-2 model, featured in over 50 peer-reviewed publications since 1998, including 7 in Nature and Science.
This model is different from David's model, because it has been constructed for a differenet purpose, but it is not "simplistic". It would never occur to me to call David's model "simplistic" because it does not include an interactive continental ice sheet model, vegetation model, carbon cycle model, sediment model and isotope model.
I'm absolutely open to any rational scientific criticism and discussion, but I can see no purpose in derogatory statements like the above, which include not even a trace of scientific argument. This kind of thing only poisons the working atmosphere in our group, which I thought was very positive and a great pleasure in Beijing.
Regards, Stefan
--
To reach me directly please use: [1]rahmstorf@ozean-klima.de
(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

Stefan Rahmstorf
[2]www.ozean-klima.de
[3]www.realclimate.org

_____ Wg1-ar4-ch06 mailing list

References

1. mailto:rahmstorf@ozean-klima.de
2. http://www.ozean-klima.de/
3. http://www.realclimate.org/

557. 1121883804.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Tom Crowley <tcrowley@duke.edu>
Subject: Re: CLA feedback on Tom and the MWP
Date: Wed, 20 Jul 2005 14:23:24 -0600
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Tom - thanks. Good points regarding regional labeling. Defn stick to
Tibetan Plateau!

best, peck

>Keith, if you can find more I see no problem - it seems that a lot
>of the data you used was via Cook and colleagues - I was unable to
>locate a full length record from Quebec in that time series, but
>maybe you are relying on something else - if so can I have it!?
>
>other suggestions: provide a more general label to sites - eg,
>mangazeyek (sp)/yamal could be listed as polar urals - taimyr
>central Siberia.
>
>China shoudl be relabeled as east Asia as it does include some
>information from Japan and the Tibetan Plateau (L. Thompson) and we
>don't want to get into some political to-do by calling Tibet
>"Chinese".
>
>that's all I can think of for present, good sailing, tom
>
>Keith Briffa wrote:
>
>>Hi all
>>think this is resolved now (virtually) -
>>
>>We use series that total to Tom/Gabi composite , and we can cite
>>this as an example of the scatter of regional records "in a typical
>>reconstruction". This avoids very difficult issue of what is the
>>best way to aggregate certain data sets - we are simply
>>illustrating the point with one published (by then) data set.
>>The issue of the composite is then not an issue either , because it
>>is not a new (unpublished) composite that we were concerned about -
>>though I still believe it is a distraction to put the composite in.
>>It would be best to use data from 800 or 850 at least , and go to
>>1500 (?) and presumably normalise over the whole period of data
>>shown. OK? Even though you guys all wish to go with the reduced
>>period (ie not up the present) , but my own instinct is that this
>>might later come back to haunt us - but will take your lead.
>>I agree the look of the Figure should match the others.
>>So, if Tom will send the data sets (his regional curves) , Tim will
>>plot and send back asap for scrutiny. Thanks Tom and thanks for
>>your help with this - further comments on latest version of 6.5

>>(last 2000 years) still welcome , though will be incorporating a
>>few changes in response to David and Fortunat input , and SH bit
>>(from Ricardo and Ed) still to go in and regional section to be
>>revised (after input from Peck et al.)
>>cheers
>>Keith
>>.
>>
>>
>> At 21:42 19/07/2005, Jonathan Overpeck wrote:
>>
>>>Hi Keith and Tim: Just got off the phone with Eystein, and
>>>hopefully he will sleep ok knowing that we have a plan for the MWP
>>>fig and Tom...
>>>
>>>Please ask questions if we don't cover all the key points, but
>>>here's what we think:
>>>
>>>1) the MWP fig should span the MWP only, and should emphasize
>>>variation in regional amplitude (we agree that we must be clear
>>>that this fig is not a reconstruction) - that is, it is best to
>>>use time series representing regions, assuming that the regional
>>>series do represent a region ok with one or more input series. We
>>>want to avoid a regional bias if we can - this is what got us into
>>>all the MWP misunderstanding in the first place, perhaps (e.g.,
>>>nice MWP in Europe/Atlantic region - must be global)
>>>
>>>2) If you guys could agree on the series and the interval, that'd
>>>be great. We agree it would be good to start before 1000 and end
>>>before the Renaissance (15th century?). If you want more feedback
>>>on these issues, we're happy to provide, but it seems logical that
>>>you pick series and intervals so that each series covers the
>>>entire interval selected.
>>>
>>>3) Don't use the Chesapeak record - it is likely biased by salinity
>>>
>>>4) We'd like Keith and Tim to draft the final figure so that it
>>>matches the look and style of the other two figs they have made.
>>>Hope this is doable. Tom, does Keith have all the data? Thanks for
>>>sending if not.
>>>
>>>5) We agree that Tom should NOT be a CA given that he was
>>>officially one of the ZOD reviewers. Of course, this doesn't
>>>represent a real conflict, but we need to avoid even the
>>>appearance of conflict. We greatly appreciate all the feedback
>>>that Tom is providing! Is this plan ok w/ you Tom? We think you're
>>>cool with it, but just want to check one more time.
>>>
>>>That... it is. Please let us know if there are any more questions.
>>>Keith - feel free to try and get Eystein on his cell doing your
>>>work hours if you want quick feedback. Or we can do this by email
>>>- he's not in a very email friendly place right now, but the
>>>fishing appears to be ok.
>>>
>>>Again, thanks to you both for all the discussion and thought that
>>>has gone into this figure.
>>>
>>>Best, peck
>>>--
>>>Jonathan T. Overpeck
>>>Director, Institute for the Study of Planet Earth
>>>Professor, Department of Geosciences
>>>Professor, Department of Atmospheric Sciences

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--
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558. 1121893120.txt
##
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Keith Briffa <k.briffa@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: crowley
Date: Wed Jul 20 16:58:40 2005
Cc: p.jones@uea.ac.uk

   Hi Tom,
   as a followup to Keith's email, it might be quite likely that one of the series
you plot is
   replaced by the instrumental record after 1960, because the file from Crowley and
Lowery
   that is available at the WDC-Paleoclimate contains such a record. The header
states:

   Crowley and Lowery 2000 (Ambio 29, 51)

Northern Hemisphere Temperature Reconstruction
Modified as published in Crowley 2000
(Science v289 p.270, 14 July 2000)
Data from Fig. 1, Crowley 2000:
Decadally smoothed time series of Crowley-Lowery reconstruction
spliced into smoothed Jones et al instrumental record after 1860
(labeled CL2.Jns11), and a slight modification (labeled CL2)
of the original Crowley and Lowery reconstruction to 1965.

The URL of this file is:

[1]ftp://ftp.ncdc.noaa.gov/pub/data/paleo/gcmoutput/crowley2000/crowley_lowery2000_nht.txt
and it is listed here:
[2]http://www.ncdc.noaa.gov/paleo/recons.html
Cheers
Tim
At 12:22 18/07/2005, Keith Briffa wrote:

as a first quick response - the Crowley numbers came from his paper with Lowery. I seem to remember that there were 2 versions of the composite that he produced - certainly we used the data that did not include Sargasso and Michigan site data. I presume the other (from the CRU web site) were the data used by Phil and Mike Mann that they got from him (where exactly did you pick then up from?)and could be the other data set (with those sites included). It seems odd that the values are so high in the recent period of this series and could conceivably be instrumental data , but would have to check. The scaling of the data we used to produce the Crowley curve that formed one of the lines in our spaghetti diagram (that we put on the web site under my name and made available to NGDC), was based on taking the unscaled composite he sent and re-calibrating against April - Sept. average for land North of 20 degrees Lat., and repeating his somewhat bazaar calibration procedure (which deliberately omitted the data between 1900-1920 that did not fit with the instrumental data (remember his data are also decadal smoothed values). In fact , as we were using summer data we calibrated over 1881-1900 (avoiding the high early decades that I still believe are biased in summer) and 1920 - 1960 , whereas he used 1856-1880 and 1920-1965. Of the precise details might differ - but the crux of the matter is that I suspect one of the Figures you show may have instrumental data in the recent period - but not ours. If you say exactly where these series came from I can ask Tim (who will have done the calibrations) to check.
As for the second question , the QR data are averaged ring widths from relatively few site chronologies in the high north (mostly N.Eurasia - Scandinavia,Yamal,Taimyr), though with a few other site data added in as stated. The 2001 data are the MXD data from near 400 sites and provide the best interannual to multidecadal indication

of
summer temps for land areas north of 20 degrees than any of the true proxy (ie not
including instrumental) data. No idea what the correlation over the common 600 year
period is - but I have never said that the ring width is anything other than summer
temps for the area it covers .
Keith
At 20:38 15/07/2005, you wrote:

Keith,
Look at the attached. Can you explain to me why these plots
differ -- particularly after 1880?
Could you also explain why the Briffa data in QR 2000 are so poorly
correlated with the Briffa 2001 data?
I think I know the answers, but I want an independent and spontaneous
answer from you.
Thanks,
Tom.

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[3]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. ftp://ftp.ncdc.noaa.gov/pub/data/paleo/gcmoutput/crowley2000/crowley_lowery2000_nht.txt
2. http://www.ncdc.noaa.gov/paleo/recons.html
3. http://www.cru.uea.ac.uk/cru/people/briffa/

559. 1121950297.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: Re: the regional section and MWP Figure
Date: Thu Jul 21 08:51:37 2005
Cc: <drdendro@ldgo.columbia.edu>, <eystein.jansen@geo.uib.no>

Hi Ricardo and all
this all seems fine with me - the question of the temperature observations is a moot one -
but some included seems a good idea - 1 South American and 1 New Zealand is fine - length
not as important as proximity to the records shown (but need to see what they l;look like).
will wait on other numbers - Henry is best qualified to cite most appropriate SH borehole
data and could supply a line of text . Ricardo can you ask him for these?
best wishes
Keith
At 13:52 20/07/2005, Ricardo Villalba wrote:

Hi Keith, Ed, Peck, Eystein
Regarding Peck's suggestions,
1) should we include instrumental data? If not, it could lessen the impact. Rio de Janeiro, starting in 1851, is the longest, homogeneous temperature record from the Southern Hemisphere. In New Zealand and Australia, temperature records start at the same time. We do not have any long record for the 18th century, even the first half of the 19th century. The hemispheric record from the Southern Hemisphere will be discussed in Chapter 2 and we do not have any additional information to provide.
2) we need to include the two borehole (see previous email from me and Ed) Definitely!! I do not have the records here in Mendoza. Keith, do you have access to these data? As soon as I receive the borehole records I will incorporate them in the figures. I would appreciate receiving the key references to properly cite the records.
3) we would like to ask Keith and Tim (pretty
please...) to draft the final figure so that it
matches the other in the section and MWP box. Is
this ok, and do you have the data to do the job.
If not, we trust your kind colleagues can send
upon request?
At the time the figure is ready, I will send all the data to Keith and Tim to draft the final figure, and the final text to incorporate in the FOD.
Cheers,
Ricardo
----- Original Message -----
From: "Jonathan Overpeck" <jto@u.arizona.edu>
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>; <drdendro@ldgo.columbia.edu>; "Eystein Jansen" <eystein.jansen@geo.uib.no>
Sent: Tuesday, July 19, 2005 5:55 PM
Subject: Re: the regional section and MWP Figure
Hi SH gang - Thanks for keeping things moving
Ricardo. Eystein and I just discussed this fig on
the phone and would like to suggest the following:
1) should we include instrumental data? If not, it could lessen the impact.
2) we need to include the two borehole (see previous email from me and Ed)
3) we would like to ask Keith and Tim (pretty
please...) to draft the final figure so that it
matches the other in the section and MWP box. Is
this ok, and do you have the data to do the job.
If not, we trust your kind colleagues can send
upon request?
Many thanks, Peck and Eystein
>Dear Keith and Ed,
>
>Please, find attached the new version of the SH figure for the IPCC. I have
>now included the New Zealand record. All the records have been scaled to 4
>°C amplitude. Variability in the Tas record is reduced compared to New
>Zealand and Patagonian records. The reference lines is the mean used for the
>calibration period in each record, 15 C for New Zealand, 14.95 C for
>Tasmania and 0 C for the Patagonian records (they show departures). Please,
>let me know if you want to introduce some changes in the figure. The
>opposite phase in the Patagonia-New Zealand records is so clear before 1850,
>which is consistent with our previous TPI. For instance, in the instrumental
>record the 1971 and 1976 are the coolest summer in northern Patagonian
>during the past 70 years, but the warmest in New Zealand reconstruction!!
>This out of phase relationship between regions in the Southern Hemisphere
>points out to the difficulty of using few records to get a hemispheric

>average. Cheers,
>
>Ricardo
>
>----- Original Message -----
>From: "Jonathan Overpeck" <jto@u.arizona.edu>
>To: "edwardcook" <drdendro@ldeo.columbia.edu>
>Cc: "Keith Briffa" <>; "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>;
>"Eystein Jansen" <>
>Sent: Monday, July 18, 2005 1:09 PM
>Subject: Re: the regional section and MWP Figure
>
>
>Thanks Ed - Ricardo, can you get the data from Henry? What do you think,
>Keith?
>Best, Peck
>
>>Given the nature of the SH and what Ricardo put
>>together, I would keep the Australian and South
>>Aftrican borehole records separate. Henry
>>Pollack can provide them, I am sure. He gave an
>>excellent talk at a meeting in Canberra that I
>>recently participated in.
>>
>>Cheers,
>>
>>Ed
>>
>>P.S. Ricardo, here is the Oroko temperature reconstruction.
>>
>>JANUARY-MARCH TEMPERATURES RECONSTRUCTED FROM
>>OROKO SWAMP, NEW ZEALAND SILVER PINE TREE RINGS
>>BE ADVISED THAT THE DATA AFTER 1958 ARE INSTRUMENTAL
>>TEMPERATURES
>>   YEAR TEMP ˚C
>>   900 13.751
>>   901 14.461
>>   902 13.236
>>   903 13.331
>>   904 13.483
>>   905 13.632
>>   906 12.959
>>   907 13.628
>>   908 13.372
>>   909 12.868
>>   910 13.244
>>   911 13.793
>>   912 14.048
>>   913 14.444
>>   914 13.095
>>   915 14.036
>>   916 13.215
>>   917 13.698
>>   918 13.570
>>   919 13.665
>>   920 13.871
>>   921 13.966
>>   922 14.762
>>   923 14.325
>>   924 14.077
>>   925 14.713
>>   926 14.081
>>   927 14.218

```
>>    928  13.793
>>    929  14.151
>>    930  14.985
>>    931  13.599
>>    932  14.663
>>    933  14.110
>>    934  14.968
>>    935  14.391
>>    936  15.484
>>    937  15.554
>>    938  14.977
>>    939  15.303
>>    940  15.179
>>    941  15.591
>>    942  14.737
>>    943  14.007
>>    944  14.865
>>    945  14.449
>>    946  14.350
>>    947  15.096
>>    948  15.257
>>    949  15.789
>>    950  15.303
>>    951  15.513
>>    952  16.111
>>    953  15.723
>>    954  15.459
>>    955  14.015
>>    956  13.083
>>    957  13.850
>>    958  14.069
>>    959  13.772
>>    960  14.873
>>    961  14.692
>>    962  14.923
>>    963  14.527
>>    964  15.034
>>    965  14.688
>>    966  14.486
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>>    968  14.436
>>    969  13.776
>>    970  13.809
>>    971  14.391
>>    972  13.487
>>    973  13.995
>>    974  14.061
>>    975  14.321
>>    976  14.882
>>    977  14.226
>>    978  14.977
>>    979  15.447
>>    980  14.424
>>    981  14.923
>>    982  14.180
>>    983  15.484
>>    984  13.487
>>    985  14.168
>>    986  14.176
>  >   987  15.699
>>    988  15.187
>>    989  16.305
>>    990  14.845
```

```
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>>    992  15.765
>>    993  14.754
>>    994  14.271
>>    995  13.623
>>    996  14.300
>>    997  13.937
>>    998  14.040
>>    999  14.011
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>>   1001  13.904
>>   1002  13.500
>>   1003  13.586
>>   1004  14.090
>>   1005  13.809
>>   1006  13.413
>>   1007  13.318
>>   1008  13.892
>>   1009  14.151
>>   1010  14.391
>>   1011  13.793
>>   1012  14.626
>>   1013  13.755
>>   1014  13.838
>>   1015  13.017
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>>   1019  13.087
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>>   1023  13.574
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>>   1026  13.137
>>   1027  13.194
>>   1028  14.036
>>   1029  13.091
>>   1030  13.768
>>   1031  13.813
>>   1032  13.846
>>   1033  13.871
>>   1034  14.255
>>   1035  14.370
>>   1036  13.805
>>   1037  14.576
>>   1038  13.504
>>   1039  13.867
>>   1040  14.927
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>>   1042  15.661
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>>   1044  15.595
>>   1045  14.741
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>>   1048  14.288
>>   1049  13.661
>>   1050  13.665
>>   1051  13.298
>>   1052  14.003
>>   1053  13.826
```

```
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>>   1056  12.976
>>   1057  13.397
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>>   1059  13.549
>>   1060  13.846
>>   1061  14.032
>>   1062  14.820
>>   1063  13.962
>>   1064  14.279
>>   1065  14.151
>>   1066  14.358
>>   1067  14.131
>>   1068  13.652
>>   1069  13.941
>>   1070  14.007
>>   1071  14.403
>>   1072  13.764
>>   1073  13.982
>>   1074  13.846
>>   1075  13.830
>>   1076  13.450
>>   1077  13.632
>>   1078  13.265
>>   1079  13.331
>>   1080  14.267
>>   1081  13.644
>>   1082  13.549
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>>   1084  13.549
>>   1085  14.725
>>   1086  13.479
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>>   1088  12.559
>>   1089  12.926
>>   1090  13.793
>>   1091  14.387
>>   1092  14.531
>>   1093  14.114
>>   1094  14.754
>>   1095  14.688
>>   1096  14.845
>>   1097  14.729
>>   1098  15.059
>>   1099  15.059
>>   1100  15.055
>>   1101  16.057
>>   1102  15.208
>>   1103  15.492
>>   1104  14.519
>>   1105  14.741
>>   1106  14.151
>>   1107  15.005
>>   1108  13.640
>>   1109  13.652
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>>   1111  13.978
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>>   1113  14.180
>>   1114  14.931
>>   1115  14.601
>>   1116  14.403
```

```
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>>   1122  13.863
>>   1123  14.271
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>>   1125  14.882
>>   1126  14.762
>>   1127  14.548
>>   1128  14.403
>>   1129  14.667
>>   1130  14.572
>>   1131  14.057
>>   1132  14.556
>>   1133  15.018
>>   1134  13.892
>>   1135  13.995
>>   1136  13.982
>>   1137  14.853
>>   1138  14.779
>>   1139  15.129
>>   1140  15.117
>>   1141  14.849
>>   1142  15.228
>>   1143  15.216
>>   1144  15.030
>>   1145  14.428
>>   1146  15.063
>>   1147  15.216
>>   1148  15.043
>>   1149  15.034
>>   1150  14.370
>>   1151  15.096
>>   1152  15.410
>>   1153  15.719
>>   1154  16.577
>>   1155  15.769
>>   1156  15.364
>>   1157  15.855
>>   1158  15.422
>>   1159  14.515
>>   1160  15.810
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>>   1162  15.402
>>   1163  15.092
>>   1164  15.298
>>   1165  14.865
>>   1166  14.882
>>   1167  15.274
>>   1168  14.605
>>   1169  14.746
>>   1170  15.472
>>   1171  15.509
>>   1172  15.018
>>   1173  15.369
>>   1174  15.084
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>>   1177  15.571
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```

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>>   1185  14.255
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>>   1187  14.461
>>   1188  15.476
>>   1189  14.882
>>   1190  15.005
>>   1191  14.453
>>   1192  14.729
>>   1193  15.265
>>   1194  14.444
>>   1195  14.696
>>   1196  15.793
>>   1197  14.581
>>   1198  15.014
>>   1199  14.539
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>>   1201  14.733
>>   1202  14.853
>>   1203  15.298
>>   1204  13.772
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>>   1207  14.836
>>   1208  14.440
>>   1209  15.162
>>   1210  14.766
>>   1211  15.010
>>   1212  15.356
>>   1213  14.787
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>>   1222  15.488
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>>   1229  14.502
>>   1230  14.457
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>>   1232  14.985
>>   1233  15.282
>>   1234  14.989
>>   1235  15.237
>>   1236  15.711
>>   1237  15.888
>>   1238  14.259
>>   1239  14.560
>>   1240  15.711
>>   1241  15.195
>>   1242  15.484
```

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>> 1245 16.454
>> 1246 15.480
>> 1247 15.492
>> 1248 16.528
>> 1249 15.150
>> 1250 14.436
>> 1251 14.878
>> 1252 15.723
>> 1253 15.043
>> 1254 15.121
>> 1255 14.845
>> 1256 14.807
>> 1257 14.482
>> 1258 14.585
>> 1259 15.307
>> 1260 15.100
>> 1261 14.354
>> 1262 13.995
>> 1263 14.106
>> 1264 14.403
>> 1265 14.754
>> 1266 14.581
>> 1267 14.799
>> 1268 14.378
>> 1269 14.671
>> 1270 14.193
>> 1271 14.387
>> 1272 14.453
> > 1273 14.510
>> 1274 15.187
>> 1275 15.393
>> 1276 14.498
>> 1277 14.560
>> 1278 15.022
>> 1279 14.498
>> 1280 14.725
>> 1281 13.549
>> 1282 14.977
>> 1283 14.065
>> 1284 14.024
>> 1285 13.603
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>> 1287 15.080
>> 1288 14.898
>> 1289 14.774
>> 1290 15.542
>> 1291 15.212
>> 1292 14.267
>> 1293 14.692
>> 1294 13.644
>> 1295 14.222
>> 1296 15.038
>> 1297 14.721
>> 1298 15.682
>> 1299 13.896
>> 1300 14.766
>> 1301 14.836
>> 1302 14.370
>> 1303 14.812
>> 1304 14.812
>> 1305 13.673

```
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>>   1329  14.779
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>>   1334  14.453
>>   1335  14.535
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>>   1337  14.424
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>>   1347  15.410
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>>   1350  14.576
>>   1351  14.605
>>   1352  14.168
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>>   1356  14.630
>>   1357  15.170
>>   1358  14.919
>>   1359  14.688
>>   1360  14.081
>>   1361  14.799
>>   1362  14.581
>>   1363  15.133
>>   1364  13.838
>>   1365  14.708
>>   1366  13.149
>>   1367  13.281
>>   1368  13.760
```

```
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>>   1370  13.314
>>   1371  14.523
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>>   1373  14.226
>>   1374  14.044
>>   1375  14.271
>>   1376  15.307
>>   1377  14.684
>>   1378  14.168
>>   1379  14.473
>>   1380  13.578
>>   1381  13.586
>>   1382  13.999
>>   1383  13.991
>>   1384  13.710
>>   1385  14.411
>>   1386  13.867
>>   1387  14.255
>>   1388  13.611
>>   1389  13.974
>>   1390  13.916
>>   1391  13.615
>>   1392  14.440
>>   1393  14.787
>>   1394  15.880
>>   1395  16.297
>>   1396  16.289
>>   1397  15.170
>>   1398  16.082
>>   1399  15.463
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>>   1401  14.758
>>   1402  14.902
>>   1403  14.568
>>   1404  15.158
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>>   1407  13.970
>>   1408  13.772
>>   1409  14.523
>>   1410  14.498
>>   1411  14.791
>>   1412  14.007
>>   1413  15.818
>>   1414  13.974
>>   1415  13.776
>>   1416  13.760
>>   1417  14.407
>>   1418  14.498
>>   1419  14.515
>>   1420  14.341
>>   1421  14.374
>>   1422  13.677
>>   1423  14.354
>>   1424  13.223
>>   1425  13.801
>>   1426  14.560
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>>   1429  15.051
>>   1430  14.836
>>   1431  13.999
```

```
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>>   1437  15.789
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>>   1439  15.298
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>>   1441  15.443
>>   1442  14.737
>>   1443  15.385
>>   1444  15.723
>>   1445  14.717
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>>   1447  15.253
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>>   1453  14.151
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>>   1456  14.601
>>   1457  13.813
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>>   1462  15.010
>>   1463  14.139
>>   1464  15.001
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>>   1475  14.395
>>   1476  15.661
>>   1477  15.158
>>   1478  15.414
>>   1479  15.641
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>>   1483  14.981
>>   1484  14.659
>>   1485  15.113
>>   1486  14.754
>>   1487  15.740
>>   1488  15.327
>>   1489  15.125
>>   1490  15.026
>>   1491  15.567
>>   1492  15.265
>>   1493  15.996
>>   1494  16.326
```

>> 1495 14.915
>> 1496 15.831
>> 1497 14.845
>> 1498 15.670
>> 1499 16.156
>> 1500 15.864
> > 1501 15.831
>> 1502 16.581
>> 1503 15.212
>> 1504 15.534
>> 1505 15.270
>> 1506 15.492
>> 1507 15.633
>> 1508 14.420
>> 1509 15.723
>> 1510 14.816
>> 1511 15.282
>> 1512 15.641
>> 1513 14.655
>> 1514 14.510
>> 1515 13.508
>> 1516 14.172
>> 1517 14.251
>> 1518 13.628
>> 1519 13.698
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>> 1521 13.920
>> 1522 13.974
>> 1523 13.978
>> 1524 14.238
>> 1525 14.003
>> 1526 13.298
>> 1527 13.694
>> 1528 15.005
>> 1529 14.218
> > 1530 14.110
>> 1531 14.593
>> 1532 13.916
>> 1533 14.510
>> 1534 14.057
>> 1535 14.048
>> 1536 13.673
>> 1537 14.477
>> 1538 14.090
>> 1539 14.300
>> 1540 14.374
>> 1541 14.387
>> 1542 14.085
>> 1543 14.184
>> 1544 14.597
>> 1545 14.783
>> 1546 15.348
>> 1547 15.859
>> 1548 15.835
>> 1549 14.729
>> 1550 15.451
>> 1551 15.204
>> 1552 15.022
>> 1553 15.352
>> 1554 14.251
>> 1555 14.135
>> 1556 14.609
>> 1557 14.572

```
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>>   1563  14.873
>>   1564  13.652
>>   1565  13.958
>>   1566  15.595
>>   1567  14.898
>>   1568  13.595
>>   1569  14.019
>>   1570  15.030
>>   1571  15.228
>>   1572  15.241
>>   1573  16.355
>>   1574  14.865
>>   1575  14.923
>>   1576  15.542
>>   1577  15.162
>>   1578  14.956
>>   1579  15.657
>>   1580  15.208
>>   1581  15.208
>>   1582  15.166
>>   1583  14.473
>>   1584  14.052
>>   1585  14.213
>>   1586  14.568
>>   1587  14.762
>>   1588  14.288
>>   1589  14.069
>>   1590  13.929
>>   1591  13.479
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>>   1593  14.267
>>   1594  14.288
>>   1595  14.609
>>   1596  14.362
>>   1597  13.846
>>   1598  14.098
>>   1599  14.147
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>>   1602  13.925
>>   1603  13.999
>>   1604  14.688
>>   1605  13.892
>>   1606  15.410
>>   1607  14.325
>>   1608  15.241
>>   1609  15.104
>>   1610  14.531
>>   1611  15.958
>>   1612  14.597
>>   1613  14.337
>>   1614  14.647
>>   1615  13.318
>>   1616  14.424
>>   1617  13.768
>>   1618  14.779
>>   1619  14.886
>>   1620  14.065
```

```
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>>   1626  13.075
>>   1627  13.871
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>>   1630  14.680
>>   1631  14.048
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>>   1633  15.752
>>   1634  14.420
>>   1635  14.085
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>>   1637  15.426
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>>   1639  14.762
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>>   1650  14.296
>>   1651  13.768
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>>   1655  15.146
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>>   1659  14.630
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>>   1661  13.702
>>   1662  14.081
>>   1663  14.312
>>   1664  14.197
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>>   1668  13.768
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>>   1670  14.246
>>   1671  14.812
>>   1672  15.216
>>   1673  15.810
>>   1674  14.869
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>>   1677  14.923
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>>   1681  14.523
>>   1682  15.327
>>   1683  15.666
```

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>>   1694  15.063
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>>   1698  15.694
>>   1699  15.249
>>   1700  14.779
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>>   1702  15.336
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>>   1704  15.154
>>   1705  15.212
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>>   1707  15.472
>>   1708  14.164
>>   1709  13.665
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>>   1711  14.741
>>   1712  15.521
>>   1713  15.410
>>   1714  14.519
>>   1715  15.154
>>   1716  14.597
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>>   1727  14.312
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>>   1730  13.640
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>>   1732  13.953
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>>   1734  14.036
>>   1735  13.937
>>   1736  14.832
>>   1737  14.807
>>   1738  14.325
>>   1739  14.337
>>   1740  14.680
>>   1741  14.779
>>   1742  14.255
>>   1743  14.205
>>   1744  14.024
>>   1745  14.069
>>   1746  15.216
```

```
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>>   1749  15.851
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>>   1752  15.294
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>>   1756  15.319
>>   1757  15.059
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>>   1759  14.725
>>   1760  14.609
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>>   1762  15.212
>>   1763  15.505
>>   1764  14.634
>>   1765  15.175
>>   1766  14.552
>>   1767  15.109
>>   1768  14.312
>>   1769  14.090
>>   1770  14.246
>>   1771  14.127
>>   1772  14.667
>>   1773  14.312
>>   1774  14.659
>>   1775  14.296
>>   1776  14.527
>>   1777  14.069
>>   1778  15.005
>>   1779  14.832
>>   1780  15.146
>>   1781  14.865
>>   1782  14.102
>>   1783  13.735
>>   1784  14.510
>>   1785  14.052
>>   1786  14.795
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>>   1788  15.298
>>   1789  14.325
>>   1790  14.927
>>   1791  14.230
>>   1792  14.230
>>   1793  14.836
>>   1794  15.637
>>   1795  15.022
>>   1796  14.473
>>   1797  14.968
>>   1798  14.028
>>   1799  13.463
>>   1800  14.151
>>   1801  15.187
>>   1802  15.290
>>   1803  15.732
>>   1804  14.985
>>   1805  15.224
>>   1806  16.251
>>   1807  13.289
>>   1808  14.420
>>   1809  14.696
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>>    1810  14.568
>>    1811  15.802
>>    1812  16.082
>>    1813  16.416
>>    1814  16.082
>>    1815  16.309
>>    1816  15.967
>>    1817  16.247
>>    1818  15.208
>>    1819  15.587
>>    1820  15.323
>>    1821  15.505
>>    1822  14.812
>>    1823  15.298
>>    1824  15.022
>>    1825  15.179
>>    1826  15.967
>>    1827  14.040
>>    1828  14.449
>>    1829  14.242
>>    1830  14.548
>>    1831  14.378
>>    1832  15.137
>>    1833  13.496
>>    1834  14.081
>>    1835  15.228
>>    1836  14.700
>>    1837  14.432
>>    1838  14.927
>>    1839  14.482
>>    1840  15.175
>>    1841  14.296
>>    1842  14.762
>>    1843  14.350
>>    1844  14.770
>>    1845  15.026
>>    1846  14.688
>>    1847  14.944
>>    1848  15.088
>>    1849  14.774
>>    1850  14.865
>>    1851  14.787
>>    1852  14.527
>>    1853  14.502
>>    1854  15.183
>>    1855  14.828
>>    1856  15.270
>>    1857  14.436
>>    1858  14.721
>>    1859  14.539
>>    1860  14.407
>>    1861  14.832
>>    1862  14.271
>>    1863  14.490
>>    1864  13.953
>>    1865  15.290
>>    1866  14.473
>>    1867  15.414
>>    1868  14.440
>>    1869  15.129
>>    1870  15.022
>>    1871  15.468
>>    1872  14.993
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>>   1873  14.890
>>   1874  14.638
>>   1875  14.898
>>   1876  14.993
>>   1877  14.366
>>   1878  14.333
>>   1879  13.454
>>   1880  15.369
>>   1881  15.109
>>   1882  15.187
>>   1883  15.278
>>   1884  14.308
>>   1885  15.026
>>   1886  15.385
>>   1887  15.183
>>   1888  14.127
>>   1889  14.985
>>   1890  15.480
>>   1891  14.717
>>   1892  15.773
>>   1893  14.807
>>   1894  15.451
>>   1895  15.179
>>   1896  13.780
>>   1897  14.531
>>   1898  13.912
>>   1899  14.354
>>   1900  15.290
>>   1901  15.752
>>   1902  14.886
>>   1903  15.216
>>   1904  15.938
>>   1905  15.208
>>   1906  14.279
>>   1907  14.923
>>   1908  15.022
>>   1909  15.501
>>   1910  15.633
>>   1911  15.212
>>   1912  13.648
>>   1913  14.329
>>   1914  15.389
>>   1915  14.704
>>   1916  15.983
>>   1917  14.779
>>   1918  14.432
>>   1919  14.024
>>   1920  14.040
>>   1921  14.622
>>   1922  15.315
>>   1923  14.560
>>   1924  15.835
>>   1925  14.927
>>   1926  14.812
>>   1927  15.220
>>   1928  16.433
>>   1929  14.506
>>   1930  14.535
>>   1931  14.073
>>   1932  14.440
>>   1933  15.406
>>   1934  14.708
>>   1935  15.026
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>>   1936  14.106
>>   1937  13.372
>>   1938  14.663
>>   1939  13.842
>>   1940  13.879
>>   1941  14.725
>>   1942  14.510
>>   1943  14.337
>>   1944  15.133
>>   1945  14.189
>>   1946  14.048
>>   1947  14.098
>>   1948  14.923
>>   1949  14.733
>>   1950  14.581
>>   1951  15.121
>>   1952  14.073
>>   1953  14.572
>>   1954  14.106
>>   1955  14.457
>>   1956  14.849
>>   1957  14.626
>>   1958  15.374
>>   1959  15.183
>>   1960  14.970
>>   1961  15.140
>>   1962  15.289
>>   1963  14.991
>>   1964  14.395
>>   1965  14.991
>>   1966  15.587
>>   1967  14.948
>>   1968  14.948
>>   1969  14.629
>>   1970  15.779
>>   1971  16.354
>>   1972  15.247
>>   1973  14.671
>>   1974  15.353
>>   1975  16.141
>>   1976  14.586
>>   1977  14.863
>>   1978  15.332
>>   1979  14.948
>>   1980  14.906
>>   1981  15.481
>>   1982  14.991
>>   1983  14.117
>>   1984  15.353
>>   1985  15.225
>>   1986  15.587
>>   1987  15.140
>>   1988  14.863
>>   1989  16.098
>>   1990  15.417
>>   1991  14.991
>>   1992  14.096
>>   1993  14.160
>>   1994  15.183
>>   1995  15.119
>>   1996  15.630
>>   1997  14.927
>>   1998  15.417
```

>>   1999 16.354
>>
>>On Jul 17, 2005, at 10:40 PM, Jonathan Overpeck wrote:
>>
>>>Thanks Ricardo and Ed! I personally am not a
>>>big fan of the Jones and Mann SH recon. It is
>>>based on so little. On the other hand, it is in
>>>the literature. So, I leave it up to you and
> >>Keith to decide - perhaps Eystein can weigh in
>>>too.
>>>
>>>I do, however, think it would be really helpful
>>>to included the borehole data (see prev.
>>>emails) - either as a single SH curve, or
>>>(probably better) two regional curves
>>>(Australia and S. Africa). Is there a reason
>>>this is not a good idea? Can't complain about
>>>snow bias down there...
>>>
>>>Thanks again - I look forward to seeing the
>>>next draft and figure - complete w/ borehole I
> >>hope.
>>>
>>>thx, Peck
>>>
>>>>Hi Keith,
>>>>
>>>>Please, find attached my last version of the SH temp. As you know, Ed
>Cook
>>>>returned my original version of the SH with minor comments. Overall, he
>>>>agreed with the text. Still I am waiting from him the Oroco Swamp data
to
>>>>include in the Figure, which first draft I sent you more than a month
>ago.
>>>>
>>>>In the last version I have included a first paragraph referring to the
>Jones
>>>>and Mann (2003) temperature reconstruction for the SH. At that time we
>have
>>>>to decide if we want to have the hemispheric (Jones and Mann) and the
>>>>regional views (Tasmania, New Zealand, Patagonia, maybe include
>Antarctica
>>>>(Ommem et al. 2005)), or just one of them. If we decide to stay with the
>>>>hemispheric view, we should include Jones and Mann reconstruction at the
>>>>bottom of one of your figures. In cases that we decide to maintain both
>>>>hemispheric and regional views, we should include Jones and Mann at the
>>>>bottom of my figure. Please, could you check with Peck and Eystein to
>see
>>>>the best way to proceed? Thanks,
>>>>
>>>>Ricardo
>>>>
>>>>
>>>>
>>>>----- Original Message -----
>>>>From: "Keith Briffa" <k.briffa@uea.ac.uk>
>>>>To: <jto@u.arizona.edu>; "Eystein Jansen" <Eystein.Jansen@geo.uib.no>
>>>>Cc: <ricardo@lab.cricyt.edu.ar>; "Ed Cook" <drdendro@ldgo.columbia.edu>
>>>>Sent: Friday, July 15, 2005 11:01 AM
>>>>Subject: the regional section and MWP Figure
>>>>
>>>>>   Guys
>>>>>   still need the SH temp bit from Ricardo/ED to edit and am exploring

>the
>>>>MWP
>>>>>   Figure - but the concept still is unclear to me - but we agreed to
do
>a
>>>>>   plot like Tom's . The regional section is still a worry - I am
happy
>to
>>>>>   very briefly edit the section on NAO (possibly incorporate the ENSO
>>>>stuff)
>>>>>   but my understanding is that this section is best done (to
incorporate
>>>>also
>>>>>   the regional moisture work of Ed) by Ricardo /Ed with input my
Peck.
>This
>>>>>   is still my opinion. I also would appreciate feedback re the
regional
>>>>>   forcing section that I think we may have to drop - but perhaps not.
>>>>>   Therefore I ask that when i get the SH temp stuff I will incorporate
>it
>>>>but
>>>>>   that you guys (Peck, Ricardo, Ed and Eystein interacting over the
>North
>>>>>   Atlantic bit) first review and redo the regional section .
>>>>>   It is important to get feedback from Henry re the borehole stuff and
>>>>>   involve Tom in the debate with all of us , of the value of the
Figure
>. In
>>>>>   meantime , will experiment with the Figure and review existing text
>and
>>>>bullets
>>>>>   Keith
>>>>>
>>>>>   Keith
>>>>>
>>>>>
>>>>>
>>>>>   --
>>>>>   Professor Keith Briffa,
>>>>>   Climatic Research Unit
>>>>>   University of East Anglia
>>>>>   Norwich, NR4 7TJ, U.K.
>>>>>
>>>>>   Phone: +44-1603-593909
>>>>>   Fax: +44-1603-507784
>>>>>
>>>>>   [1]http://www.cru.uea.ac.uk/cru/people/briffa/
>>>>>
>>>>>
>>>>
>>>>Attachment converted: Macintosh HD:Southern
>>>>hemisphere2.doc (WDBN/«IC») (0008A6E0)
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>>>--
>>>Jonathan T. Overpeck
>>>Director, Institute for the Study of Planet Earth
>>>Professor, Department of Geosciences
>>>Professor, Department of Atmospheric Sciences
>>>
>>>Mail and Fedex Address:
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>>>
>>==========
>>Dr. Edward R. Cook
>>Doherty Senior Scholar and
>>Director, Tree-Ring Laboratory
>>Lamont-Doherty Earth Observatory
>>Palisades, New York 10964 USA
>>Email: drdendro@ldeo.columbia.edu
>>Phone: 845-365-8618
>>Fax: 845-365-8152
>>==========
>
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>
>Attachment converted: Macintosh HD:SHregteml 1.JPG (JPEG/«IC») (0008ADC3)
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References

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7. http://www.ispe.arizona.edu/
8. http://www.cru.uea.ac.uk/cru/people/briffa/

560. 1121950401.txt
##
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: Re: CLA feedback on Tom and the MWP
Date: Thu Jul 21 08:53:21 2005

Date: Wed, 20 Jul 2005 09:53:34 -0400
From: Tom Crowley <tcrowley@duke.edu>
User-Agent: Mozilla/5.0 (Macintosh; U; PPC Mac OS X Mach-O; en-US; rv:1.4)
Gecko/20030624 Netscape/7.1
X-Accept-Language: en-us, en
To: Keith Briffa <k.briffa@uea.ac.uk>
CC: Jonathan Overpeck <jto@u.arizona.edu>,
Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk
Subject: Re: CLA feedback on Tom and the MWP
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO
Keith, if you can find more I see no problem - it seems that a lot of the data
you used
was via Cook and colleagues - I was unable to locate a full length record from
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have it!?
other suggestions: provide a more general label to sites - eg, mangazeyek
(sp)/yamal
could be listed as polar urals - taimyr central Siberia.
China shoudl be relabeled as east Asia as it does include some information from
Japan
and the Tibetan Plateau (L. Thompson) and we don't want to get into some
political to-do
by calling Tibet "Chinese".
that's all I can think of for present, good sailing, tom
Keith Briffa wrote:

Hi all
think this is resolved now (virtually) -
We use series that total to Tom/Gabi composite , and we can cite this as an
example of
the scatter of regional records "in a typical reconstruction". This avoids very
difficult issue of what is the best way to aggregate certain data sets - we are
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illustrating the point with one published (by then) data set.
The issue of the composite is then not an issue either , because it is not a
new
(unpublished) composite that we were concerned about - though I still believe
it is a

distraction to put the composite in. It would be best to use data from 800 or 850 at least , and go to 1500 (?) and presumably normalise over the whole period of data shown.
OK? Even though you guys all wish to go with the reduced period (ie not up the present) , but my own instinct is that this might later come back to haunt us - but will take your lead.
I agree the look of the Figure should match the others.
So, if Tom will send the data sets (his regional curves) , Tim will plot and send back asap for scrutiny. Thanks Tom and thanks for your help with this - further comments on latest version of 6.5 (last 2000 years) still welcome , though will be incorporating a few changes in response to David and Fortunat input , and SH bit (from Ricardo and Ed) still to go in and regional section to be revised (after input from Peck et al.)
cheers
Keith
.
At 21:42 19/07/2005, Jonathan Overpeck wrote:

Hi Keith and Tim: Just got off the phone with Eystein, and hopefully he will sleep ok knowing that we have a plan for the MWP fig and Tom...
Please ask questions if we don't cover all the key points, but here's what we think:
1) the MWP fig should span the MWP only, and should emphasize variation in regional amplitude (we agree that we must be clear that this fig is not a reconstruction) - that is, it is best to use time series representing regions, assuming that the regional series do represent a region ok with one or more input series. We want to avoid a regional bias if we can - this is what got us into all the MWP misunderstanding in the first place, perhaps (e.g., nice MWP in Europe/Atlantic region - must be global)
2) If you guys could agree on the series and the interval, that'd be great. We agree it would be good to start before 1000 and end before the Renaissance (15th century?). If you want more feedback on these issues, we're happy to provide, but it seems logical that you pick series and intervals so that each series covers the entire interval selected.
3) Don't use the Chesapeak record - it is likely biased by salinity
4) We'd like Keith and Tim to draft the final figure so that it matches the look and style of the other two figs they have made. Hope this is doable. Tom, does Keith have all the data? Thanks for sending if not.
5) We agree that Tom should NOT be a CA given that he was officially one of the ZOD reviewers. Of course, this doesn't represent a real conflict, but we need to avoid even the appearance of conflict. We greatly appreciate all the feedback that Tom is providing! Is this plan ok w/ you Tom? We think you're cool with it, but just

want to
check one more time.
That... it is. Please let us know if there are any more questions. Keith - feel free to
try and get Eystein on his cell doing your work hours if you want quick feedback. Or we
can do this by email - he's not in a very email friendly place right now, but the
fishing appears to be ok.
Again, thanks to you both for all the discussion and thought that has gone into this
figure.
Best, peck
--
Jonathan T. Overpeck
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References

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561. 1121964854.txt
##########

From: Tom Crowley <tcrowley@duke.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: MWP figure
Date: Thu, 21 Jul 2005 12:54:14 -0400

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>,
Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Tim, we are getting close but there are a few items to discuss:

1) seven of the eight time series are from the Hegerl et al paper, now
out for review in Nature
2) the eighth time series is from Brian Luckmans recent extension of the
Alberta record to the 10th century - we used his original time series in
the H et al paper because the comparisons between model and observations
had been going on for a while, in fact before the new Luckman paper came
out, and we did not want to switch horses in midstream by changing the
composite - as you know the Luckman paper is either accepted or
published in CD, so there is no problem changing that
3) although technically the time series are not the same they are very
close, if you want me to do some comparisons I can, but I could not get
to it until probably tuesday of next week - I don't particularly see any
problem in makng such an addition
4) we cannot extend the time series back to 800 without dropping out
something - the reason we start at 945 is that is the first year when
all the records are available - if we go back to 800 we do so at the
cost of dropping 2 or possibly even 3 records. as our Dark Ages
reconstruction starting at 560 indicates (att.), the biggest warming
between 800-1900 is in the late 10the century (960-995), we did not
think we missing out on anything by starting at 945 rather than 800.

I await your feedback on this increasingly intricate issue, tom

Tim Osborn wrote:

> Hi Tom,
>
> In Keith's email below, when he says "we use series that total to
> Tom/Gabi composite", he doesn't mean that *our* mock up of the figure
> uses these series, but that if the series shown in *your* draft figure
> are the same as those used in the Hegerl/Crowley recon that is
> currently submitted ("...a twice validated climate record...") then we
> will go with *your* figure. It is fine then to include the "composite
> series" and the instrumental data and a temperature scale. Our
> previous concerns about these latter points were that it might be seen
> as another new NH temperature reconstruction. But if in fact the
> composite and its expression as a temperature are not a new NH T
> recon, but are in fact identical to the published (submitted, at
> least) Hegerl/Crowley NH T recon (which is already included in the
> main intercomparison figure) then there's no problem.
>
> Does your figure equate to the new Hegerl/Crowley NH T recon? If so,
> we should go with your MWP figure, though the CLAs want me to draw it
> in the same style as the others and also cut the time period down to a
> few centuries spanning the MWP. Keith suggests beginning in 800 or 850.
>
> Would it be possible therefore to send the data series you used for
> your figure, but beginning in 800/850, so I can plot the figure in the
> required form?
>
> Cheers
>
> Tim
>
> At 14:53 20/07/2005, Tom Crowley wrote:
>
>> Keith, if you can find more I see no problem - it seems that a lot of

>> the data you used was via Cook and colleagues - I was unable to
>> locate a full length record from Quebec in that time series, but
>> maybe you are relying on something else - if so can I have it!?
>>
>> other suggestions: provide a more general label to sites - eg,
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>> central Siberia.
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>> China shoudl be relabeled as east Asia as it does include some
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>>>> Again, thanks to you both for all the discussion and thought that
>>>> has gone into this figure.
>>>>
>>>> Best, peck
>>>> --
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>>>> Professor, Department of Geosciences
>>>> Professor, Department of Atmospheric Sciences
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> sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm
>

</x-flowed>

Attachment Converted: "c:\eudora\attach\CH.DA.jpg"

562. 1121974981.txt
##
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
Subject: Re: the regional section and MWP Figure
Date: Thu Jul 21 15:43:01 2005
Cc: "Jonathan Overpeck" <jto@u.arizona.edu>, <drdendro@ldgo.columbia.edu>,
<eystein.jansen@geo.uib.no>, "Tim Osborn" <t.osborn@uea.ac.uk>

   Ricardo
   Tim is contacting Henry now - so forget Boreholes for time being
   cheers
   Keith
   At 15:23 21/07/2005, Ricardo Villalba wrote:

     Hi Keith and all,
     Following Peck's advise I will include for each reconstruction the observed
     record. Obviously, I have the Patagonian instrumental records, but I need
     from Ed the Tasmania and Hokitika (New Zealand) observed records.
     Sorry for my ignorance on borehole, but I am not familiar with Henry's work.
     If you send me his e-mail, I could ask him for the data and a line of text
     on borehole from the SH. Cheers,
     Ricardo
     ----- Original Message -----
     From: "Keith Briffa" <k.briffa@uea.ac.uk>
     To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>; "Jonathan Overpeck"
     <jto@u.arizona.edu>
     Cc: <drdendro@ldgo.columbia.edu>; <eystein.jansen@geo.uib.no>
     Sent: Thursday, July 21, 2005 4:51 AM
     Subject: Re: the regional section and MWP Figure
     Hi Ricardo and all
     this all seems fine with me - the question of the temperature observations
     is a moot one - but some included seems a good idea - 1 South American and
     1 New Zealand is fine - length not as important as proximity to the
     records shown (but need to see what they l;look like).
     will wait on other numbers - Henry is best qualified to cite most
     appropriate SH borehole data and could supply a line of text . Ricardo can
     you ask him for these?
     best wishes
     Keith
     At 13:52 20/07/2005, Ricardo Villalba wrote:
     >Hi Keith, Ed, Peck, Eystein
     >
     >

>
>Regarding Peck's suggestions,
>
>
>1) should we include instrumental data? If not, it could lessen the impact.
>
>
>
>Rio de Janeiro, starting in 1851, is the longest, homogeneous temperature
>record from the Southern Hemisphere. In New Zealand and Australia,
>temperature records start at the same time. We do not have any long record
>for the 18th century, even the first half of the 19th century. The
>hemispheric record from the Southern Hemisphere will be discussed in
Chapter
>2 and we do not have any additional information to provide.
>
>
>2) we need to include the two borehole (see previous email from me and Ed)
>
>
>
>Definitely!! I do not have the records here in Mendoza. Keith, do you have
>access to these data? As soon as I receive the borehole records I will
>incorporate them in the figures. I would appreciate receiving the key
>references to properly cite the records.
>
>
>3) we would like to ask Keith and Tim (pretty
>please...) to draft the final figure so that it
>matches the other in the section and MWP box. Is
>this ok, and do you have the data to do the job.
>If not, we trust your kind colleagues can send
>upon request?
>
>
>
>At the time the figure is ready, I will send all the data to Keith and Tim
>to draft the final figure, and the final text to incorporate in the FOD.
>Cheers,
>
>Ricardo
>
>
>
>----- Original Message -----
>From: "Jonathan Overpeck" <jto@u.arizona.edu>
>To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
>Cc: "Keith Briffa" <k.briffa@uea.ac.uk>; <drdendro@ldgo.columbia.edu>;
>"Eystein Jansen" <eystein.jansen@geo.uib.no>
>Sent: Tuesday, July 19, 2005 5:55 PM
>Subject: Re: the regional section and MWP Figure
>
>
>Hi SH gang - Thanks for keeping things moving
>Ricardo. Eystein and I just discussed this fig on
>the phone and would like to suggest the following:
>
>1) should we include instrumental data? If not, it could lessen the impact.
>2) we need to include the two borehole (see previous email from me and Ed)
>3) we would like to ask Keith and Tim (pretty
>please...) to draft the final figure so that it
>matches the other in the section and MWP box. Is
>this ok, and do you have the data to do the job.

>If not, we trust your kind colleagues can send
>upon request?
>
>Many thanks, Peck and Eystein
>
> >Dear Keith and Ed,
> >
> >Please, find attached the new version of the SH figure for the IPCC. I
>have
> >now included the New Zealand record. All the records have been scaled to
4
> >˚C amplitude. Variability in the Tas record is reduced compared to New
> >Zealand and Patagonian records. The reference lines is the mean used for
>the
> >calibration period in each record, 15 C for New Zealand, 14.95 C for
> >Tasmania and 0 C for the Patagonian records (they show departures).
Please,
> >let me know if you want to introduce some changes in the figure. The
> >opposite phase in the Patagonia-New Zealand records is so clear before
>1850,
> >which is consistent with our previous TPI. For instance, in the
>instrumental
> >record the 1971 and 1976 are the coolest summer in northern Patagonian
> >during the past 70 years, but the warmest in New Zealand reconstruction!!
> >This out of phase relationship between regions in the Southern Hemisphere
> >points out to the difficulty of using few records to get a hemispheric
> >average. Cheers,
> >
> >Ricardo
> >
> >----- Original Message -----
> >From: "Jonathan Overpeck" <jto@u.arizona.edu>
> >To: "edwardcook" <drdendro@ldeo.columbia.edu>
> >Cc: "Keith Briffa" <>; "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>;
> >"Eystein Jansen" <>
> >Sent: Monday, July 18, 2005 1:09 PM
> >Subject: Re: the regional section and MWP Figure
> >
> >
> >Thanks Ed - Ricardo, can you get the data from Henry? What do you think,
> >Keith?
> >Best, Peck
> >
> >>Given the nature of the SH and what Ricardo put
> >>together, I would keep the Australian and South
> >>Aftrican borehole records separate. Henry
> >>Pollack can provide them, I am sure. He gave an
> >>excellent talk at a meeting in Canberra that I
> >>recently participated in.
> >>
> >>Cheers,
> >>
> >>Ed
> >>
> >>P.S. Ricardo, here is the Oroko temperature reconstruction.
> >>
> >>JANUARY-MARCH TEMPERATURES RECONSTRUCTED FROM
> >>OROKO SWAMP, NEW ZEALAND SILVER PINE TREE RINGS
> >>BE ADVISED THAT THE DATA AFTER 1958 ARE INSTRUMENTAL
> >>TEMPERATURES
> >>   YEAR TEMP ˚C
> >>   900 13.751
> >>   901 14.461

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> >>    902  13.236
> >>    903  13.331
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> >>    910  13.244
> >>    911  13.793
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> >>    915  14.036
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> >>    945  14.449
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> >>    948  15.257
> >>    949  15.789
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> >>    955  14.015
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> >>    959  13.772
> >>    960  14.873
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> >>    962  14.923
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> >>   1079  13.331
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> >>   1433  14.865
> >>   1434  15.063
> >>   1435  15.311
> >>   1436  15.765
> >>   1437  15.789
> >>   1438  15.204
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> >>   1443  15.385
> >>   1444  15.723
> >>   1445  14.717
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> >>   1460  14.919
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> >>   1463  14.139
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> >>   1467  14.399
> >>   1468  14.671
```

> >> 1469 15.092
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> >> 1471 14.948
> >> 1472 15.047
> >> 1473 14.523
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> >> 1502 16.581
> >> 1503 15.212
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> >> 1506 15.492
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> >> 1509 15.723
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> >> 1515 13.508
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> >> 1517 14.251
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> >> 1529 14.218
> > > 1530 14.110
> >> 1531 14.593

```
> >>   1532  13.916
> >>   1533  14.510
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> >>   1540  14.374
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> >>   1542  14.085
> >>   1543  14.184
> >>   1544  14.597
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> >>   1547  15.859
> >>   1548  15.835
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> >>   1551  15.204
> >>   1552  15.022
> >>   1553  15.352
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> >>   1562  14.399
> >>   1563  14.873
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> >>   1565  13.958
> >>   1566  15.595
> >>   1567  14.898
> >>   1568  13.595
> >>   1569  14.019
> >>   1570  15.030
> >>   1571  15.228
> >>   1572  15.241
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> >>   1578  14.956
> >>   1579  15.657
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> >>   1582  15.166
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> >>   1585  14.213
> >>   1586  14.568
> >>   1587  14.762
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> >>   1589  14.069
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> >>   1592  14.044
> >>   1593  14.267
> >>   1594  14.288
```

```
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> >>   1602  13.925
> >>   1603  13.999
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> >>   1607  14.325
> >>   1608  15.241
> >>   1609  15.104
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> >>   1611  15.958
> >>   1612  14.597
> >>   1613  14.337
> >>   1614  14.647
> >>   1615  13.318
> >>   1616  14.424
> >>   1617  13.768
> >>   1618  14.779
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> >>   1620  14.065
> >>   1621  14.085
> >>   1622  14.626
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> >>   1626  13.075
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> >>   1629  13.755
> >>   1630  14.680
> >>   1631  14.048
> >>   1632  14.601
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> >>   1635  14.085
> >>   1636  14.230
> >>   1637  15.426
> >>   1638  16.322
> >>   1639  14.762
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> >>   1642  14.931
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> >>   1644  15.843
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> >>   1652  15.001
> >>   1653  14.944
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> >>   1655  15.146
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> >>   1657  14.803
```

```
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> >>   1670  14.246
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> >>   1672  15.216
> >>   1673  15.810
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> >>   1679  14.956
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> >>   1682  15.327
> >>   1683  15.666
> >>   1684  15.554
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> >>   1686  15.492
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> >>   1688  14.754
> >>   1689  14.741
> >>   1690  14.700
> >>   1691  14.906
> >>   1692  13.904
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> >>   1695  14.399
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> >>   1697  15.360
> >>   1698  15.694
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> >>   1704  15.154
> >>   1705  15.212
> >>   1706  14.750
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> >>   1712  15.521
> >>   1713  15.410
> >>   1714  14.519
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> >>   1716  14.597
> >>   1717  15.212
> >>   1718  14.688
> >>   1719  13.962
> >>   1720  15.109
```

> >> 1721 15.839
> >> 1722 15.765
> >> 1723 15.001
> >> 1724 15.389
> >> 1725 15.088
> >> 1726 14.655
> >> 1727 14.312
> >> 1728 14.824
> >> 1729 14.981
> >> 1730 13.640
> >> 1731 15.043
> >> 1732 13.953
> >> 1733 13.681
> >> 1734 14.036
> >> 1735 13.937
> >> 1736 14.832
> >> 1737 14.807
> >> 1738 14.325
> >> 1739 14.337
> >> 1740 14.680
> >> 1741 14.779
> >> 1742 14.255
> >> 1743 14.205
> >> 1744 14.024
> >> 1745 14.069
> >> 1746 15.216
> >> 1747 15.455
> >> 1748 15.447
> >> 1749 15.851
> >> 1750 15.253
> >> 1751 14.626
> >> 1752 15.294
> >> 1753 15.744
> >> 1754 15.158
> >> 1755 14.750
> >> 1756 15.319
> >> 1757 15.059
> > > > 1758 15.195
> >> 1759 14.725
> >> 1760 14.609
> >> 1761 14.869
> >> 1762 15.212
> >> 1763 15.505
> >> 1764 14.634
> >> 1765 15.175
> >> 1766 14.552
> >> 1767 15.109
> >> 1768 14.312
> >> 1769 14.090
> >> 1770 14.246
> >> 1771 14.127
> >> 1772 14.667
> >> 1773 14.312
> >> 1774 14.659
> >> 1775 14.296
> >> 1776 14.527
> >> 1777 14.069
> >> 1778 15.005
> >> 1779 14.832
> >> 1780 15.146
> >> 1781 14.865
> >> 1782 14.102
> >> 1783 13.735

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> >> 1786 14.795
> > > 1787 15.455
> >> 1788 15.298
> >> 1789 14.325
> >> 1790 14.927
> >> 1791 14.230
> >> 1792 14.230
> >> 1793 14.836
> >> 1794 15.637
> >> 1795 15.022
> >> 1796 14.473
> >> 1797 14.968
> >> 1798 14.028
> >> 1799 13.463
> >> 1800 14.151
> >> 1801 15.187
> >> 1802 15.290
> >> 1803 15.732
> >> 1804 14.985
> >> 1805 15.224
> >> 1806 16.251
> >> 1807 13.289
> >> 1808 14.420
> >> 1809 14.696
> >> 1810 14.568
> >> 1811 15.802
> >> 1812 16.082
> >> 1813 16.416
> >> 1814 16.082
> >> 1815 16.309
> >> 1816 15.967
> >> 1817 16.247
> >> 1818 15.208
> >> 1819 15.587
> >> 1820 15.323
> >> 1821 15.505
> >> 1822 14.812
> >> 1823 15.298
> >> 1824 15.022
> >> 1825 15.179
> >> 1826 15.967
> >> 1827 14.040
> >> 1828 14.449
> >> 1829 14.242
> >> 1830 14.548
> >> 1831 14.378
> >> 1832 15.137
> >> 1833 13.496
> >> 1834 14.081
> >> 1835 15.228
> >> 1836 14.700
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> >> 1838 14.927
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> >> 1840 15.175
> >> 1841 14.296
> >> 1842 14.762
> >> 1843 14.350
> >> 1844 14.770
> >> 1845 15.026
> >> 1846 14.688

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> >>   1848  15.088
> >>   1849  14.774
> >>   1850  14.865
> >>   1851  14.787
> >>   1852  14.527
> >>   1853  14.502
> >>   1854  15.183
> >>   1855  14.828
> >>   1856  15.270
> >>   1857  14.436
> >>   1858  14.721
> >>   1859  14.539
> >>   1860  14.407
> >>   1861  14.832
> >>   1862  14.271
> >>   1863  14.490
> >>   1864  13.953
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> >>   1866  14.473
> >>   1867  15.414
> >>   1868  14.440
> >>   1869  15.129
> >>   1870  15.022
> >>   1871  15.468
> >>   1872  14.993
> >>   1873  14.890
> >>   1874  14.638
> >>   1875  14.898
> >>   1876  14.993
> >>   1877  14.366
> >>   1878  14.333
> >>   1879  13.454
> >>   1880  15.369
> >>   1881  15.109
> >>   1882  15.187
> >>   1883  15.278
> >>   1884  14.308
> >>   1885  15.026
> >>   1886  15.385
> >>   1887  15.183
> >>   1888  14.127
> >>   1889  14.985
> >>   1890  15.480
> >>   1891  14.717
> >>   1892  15.773
> >>   1893  14.807
> >>   1894  15.451
> >>   1895  15.179
> >>   1896  13.780
> >>   1897  14.531
> >>   1898  13.912
> >>   1899  14.354
> >>   1900  15.290
> >>   1901  15.752
> >>   1902  14.886
> >>   1903  15.216
> >>   1904  15.938
> >>   1905  15.208
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> >>   1907  14.923
> >>   1908  15.022
> >>   1909  15.501
```

```
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> >>   1911  15.212
> >>   1912  13.648
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> >>   1915  14.704
> >>   1916  15.983
> >>   1917  14.779
> >>   1918  14.432
> >>   1919  14.024
> >>   1920  14.040
> >>   1921  14.622
> >>   1922  15.315
> >>   1923  14.560
> >>   1924  15.835
> >>   1925  14.927
> >>   1926  14.812
> >>   1927  15.220
> >>   1928  16.433
> >>   1929  14.506
> >>   1930  14.535
> >>   1931  14.073
> >>   1932  14.440
> >>   1933  15.406
> >>   1934  14.708
> >>   1935  15.026
> >>   1936  14.106
> >>   1937  13.372
> >>   1938  14.663
> >>   1939  13.842
> >>   1940  13.879
> >>   1941  14.725
> >>   1942  14.510
> >>   1943  14.337
> >>   1944  15.133
> >>   1945  14.189
> >>   1946  14.048
> >>   1947  14.098
> >>   1948  14.923
> >>   1949  14.733
> >>   1950  14.581
> >>   1951  15.121
> >>   1952  14.073
> >>   1953  14.572
> >>   1954  14.106
> >>   1955  14.457
> >>   1956  14.849
> >>   1957  14.626
> >>   1958  15.374
> >>   1959  15.183
> >>   1960  14.970
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> >>   1962  15.289
> >>   1963  14.991
> >>   1964  14.395
> >>   1965  14.991
> >>   1966  15.587
> >>   1967  14.948
> >>   1968  14.948
> >>   1969  14.629
> >>   1970  15.779
> >>   1971  16.354
> >>   1972  15.247
```

> >>   1973 14.671
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> >>   1975 16.141
> >>   1976 14.586
> >>   1977 14.863
> >>   1978 15.332
> >>   1979 14.948
> >>   1980 14.906
> >>   1981 15.481
> >>   1982 14.991
> >>   1983 14.117
> >>   1984 15.353
> >>   1985 15.225
> >>   1986 15.587
> >>   1987 15.140
> >>   1988 14.863
> >>   1989 16.098
> >>   1990 15.417
> >>   1991 14.991
> >>   1992 14.096
> >>   1993 14.160
> >>   1994 15.183
> >>   1995 15.119
> >>   1996 15.630
> >>   1997 14.927
> >>   1998 15.417
> >>   1999 16.354
> >>
> >>On Jul 17, 2005, at 10:40 PM, Jonathan Overpeck wrote:
> >>
> >>>Thanks Ricardo and Ed! I personally am not a
> >>>big fan of the Jones and Mann SH recon. It is
> >>>based on so little. On the other hand, it is in
> >>>the literature. So, I leave it up to you and
> > >>Keith to decide - perhaps Eystein can weigh in
> >>>too.
> >>>
> >>>I do, however, think it would be really helpful
> >>>to included the borehole data (see prev.
> >>>emails) - either as a single SH curve, or
> >>>(probably better) two regional curves
> >>>(Australia and S. Africa). Is there a reason
> >>>this is not a good idea? Can't complain about
> >>>snow bias down there...
> >>>
> >>>Thanks again - I look forward to seeing the
> >>>next draft and figure - complete w/ borehole I
> > >>hope.
> >>>
> >>>thx, Peck
> >>>
> >>>>Hi Keith,
> >>>>
> >>>>Please, find attached my last version of the SH temp. As you know, Ed
> >Cook
> >>>>returned my original version of the SH with minor comments. Overall,
he
> >>>>agreed with the text. Still I am waiting from him the Oroco Swamp data
>to
> >>>>include in the Figure, which first draft I sent you more than a month
> >ago.
> >>>>
> >>>>In the last version I have included a first paragraph referring to the

> >Jones
> >>>>and Mann (2003) temperature reconstruction for the SH. At that time
we
> >have
> >>>>to decide if we want to have the hemispheric (Jones and Mann) and the
> >>>>regional views (Tasmania, New Zealand, Patagonia, maybe include
> >Antarctica
> >>>>(Ommem et al. 2005)), or just one of them. If we decide to stay with
the
> >>>>hemispheric view, we should include Jones and Mann reconstruction at
the
> >>>>bottom of one of your figures. In cases that we decide to maintain
both
> >>>>hemispheric and regional views, we should include Jones and Mann at
the
> >>>>bottom of my figure. Please, could you check with Peck and Eystein to
> >see
> >>>>the best way to proceed? Thanks,
> >>>>
> >>>>Ricardo
> >>>>
> >>>>
> >>>>
> >>>>----- Original Message -----
> >>>>From: "Keith Briffa" <k.briffa@uea.ac.uk>
> >>>>To: <jto@u.arizona.edu>; "Eystein Jansen" <Eystein.Jansen@geo.uib.no>
> >>>>Cc: <ricardo@lab.cricyt.edu.ar>; "Ed Cook"
<drdendro@ldgo.columbia.edu>
> >>>>Sent: Friday, July 15, 2005 11:01 AM
> >>>>Subject: the regional section and MWP Figure
> >>>>
> >>>>   Guys
> >>>>   still need the SH temp bit from Ricardo/ED to edit and am
exploring
> >the
> >>>>MWP
> >>>>   Figure - but the concept still is unclear to me - but we agreed to
>do
> >a
> >>>>   plot like Tom's . The regional section is still a worry - I am
>happy
> >to
> >>>>   very briefly edit the section on NAO (possibly incorporate the
ENSO
> >>>>stuff)
> >>>>   but my understanding is that this section is best done (to
>incorporate
> >>>>also
> >>>>   the regional moisture work of Ed) by Ricardo /Ed with input my
>Peck.
> >This
> >>>>   is still my opinion. I also would appreciate feedback re the
>regional
> >>>>   forcing section that I think we may have to drop - but perhaps
not.
> >>>>   Therefore I ask that when i get the SH temp stuff I will
incorporate
> >it
> >>>>but
> >>>>   that you guys (Peck, Ricardo, Ed and Eystein interacting over the
> >North
> >>>>   Atlantic bit) first review and redo the regional section .
> >>>>   It is important to get feedback from Henry re the borehole stuff

and
> >>>>>   involve Tom in the debate with all of us , of the value of the
>Figure
> >. In
> >>>>>   meantime , will experiment with the Figure and review existing
text
> >and
> >>>>bullets
> >>>>>   Keith
> >>>>>
> >>>>>   Keith
> >>>>>
> >>>>>
> >>>>>
> >>>>>   --
> >>>>>   Professor Keith Briffa,
> >>>>>   Climatic Research Unit
> >>>>>   University of East Anglia
> >>>>>   Norwich, NR4 7TJ, U.K.
> >>>>>
> >>>>>   Phone: +44-1603-593909
> >>>>>   Fax: +44-1603-507784
> >>>>>
> >>>>>   [1]http://www.cru.uea.ac.uk/cru/people/briffa/
> >>>>>
> >>>>>
> >>>>
> >>>>Attachment converted: Macintosh HD:Southern
> >>>>hemisphere2.doc (WDBN/«IC») (0008A6E0)
> >>>
> >>>
> >>>--
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> >>>Professor, Department of Atmospheric Sciences
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> >>Lamont-Doherty Earth Observatory
> >>Palisades, New York 10964 USA
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> >>Phone: 845-365-8618
> >>Fax: 845-365-8152
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> >
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> >Director, Institute for the Study of Planet Earth

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> >[5]http://www.ispe.arizona.edu/
> >
> >Attachment converted: Macintosh HD:SHregteml 1.JPG (JPEG/«IC») (0008ADC3)
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>--
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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.geo.arizona.edu/
3. http://www.ispe.arizona.edu/
4. http://www.geo.arizona.edu/
5. http://www.ispe.arizona.edu/
6. http://www.geo.arizona.edu/
7. http://www.ispe.arizona.edu/
8. http://www.cru.uea.ac.uk/cru/people/briffa/
9. http://www.cru.uea.ac.uk/cru/people/briffa/

563. 1121976478.txt
##

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Tom Crowley <tcrowley@duke.edu>,Keith Briffa <k.briffa@uea.ac.uk>
Subject: MWP figure
Date: Thu, 21 Jul 2005 16:07:58 +0100
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen
<eystein.jansen@geo.uib.no>

<x-flowed>
Hi Tom,

In Keith's email below, when he says "we use series that total to Tom/Gabi composite", he doesn't mean that *our* mock up of the figure uses these series, but that if the series shown in *your* draft figure are the same as those used in the Hegerl/Crowley recon that is currently submitted ("...a twice validated climate record...") then we will go with *your* figure. It is fine then to include the "composite series" and the instrumental data and a temperature scale. Our previous concerns about these latter points were that it might be seen as another new NH temperature reconstruction. But if in fact the composite and its expression as a temperature are not a new NH T recon, but are in fact identical to the published (submitted, at least) Hegerl/Crowley NH T recon (which is already included in the main intercomparison figure) then there's no problem.

Does your figure equate to the new Hegerl/Crowley NH T recon? If so, we should go with your MWP figure, though the CLAs want me to draw it in the same style as the others and also cut the time period down to a few centuries spanning the MWP. Keith suggests beginning in 800 or 850.

Would it be possible therefore to send the data series you used for your figure, but beginning in 800/850, so I can plot the figure in the required form?

Cheers

Tim

At 14:53 20/07/2005, Tom Crowley wrote:
>Keith, if you can find more I see no problem - it seems that a lot of the
>data you used was via Cook and colleagues - I was unable to locate a full
>length record from Quebec in that time series, but maybe you are relying
>on something else - if so can I have it!?
>
>other suggestions: provide a more general label to sites - eg, mangazeyek
>(sp)/yamal could be listed as polar urals - taimyr central Siberia.
>
>China shoudl be relabeled as east Asia as it does include some information
>from Japan and the Tibetan Plateau (L. Thompson) and we don't want to get
>into some political to-do by calling Tibet "Chinese".
>
>that's all I can think of for present, good sailing, tom
>
>Keith Briffa wrote:
>
>>Hi all
>>think this is resolved now (virtually) -
>>
>>We use series that total to Tom/Gabi composite , and we can cite this as

>>an example of the scatter of regional records "in a typical
>>reconstruction". This avoids very difficult issue of what is the best way
>>to aggregate certain data sets - we are simply illustrating the point
>>with one published (by then) data set.
>>The issue of the composite is then not an issue either , because it is
>>not a new (unpublished) composite that we were concerned about - though I
>>still believe it is a distraction to put the composite in. It would be
>>best to use data from 800 or 850 at least , and go to 1500 (?) and
>>presumably normalise over the whole period of data shown. OK? Even though
>>you guys all wish to go with the reduced period (ie not up the present) ,
>>but my own instinct is that this might later come back to haunt us - but
>>will take your lead.
>>I agree the look of the Figure should match the others.
>>So, if Tom will send the data sets (his regional curves) , Tim will plot
>>and send back asap for scrutiny. Thanks Tom and thanks for your help
>>with this - further comments on latest version of 6.5 (last 2000 years)
>>still welcome , though will be incorporating a few changes in response to
>>David and Fortunat input , and SH bit (from Ricardo and Ed) still to go
>>in and regional section to be revised (after input from Peck et al.)
>>cheers
>>Keith
>>.
>>
>>
>> At 21:42 19/07/2005, Jonathan Overpeck wrote:
>>
>>>Hi Keith and Tim: Just got off the phone with Eystein, and hopefully he
>>>will sleep ok knowing that we have a plan for the MWP fig and Tom...
>>>
>>>Please ask questions if we don't cover all the key points, but here's
>>>what we think:
>>>
>>>1) the MWP fig should span the MWP only, and should emphasize variation
>>>in regional amplitude (we agree that we must be clear that this fig is
>>>not a reconstruction) - that is, it is best to use time series
>>>representing regions, assuming that the regional series do represent a
>>>region ok with one or more input series. We want to avoid a regional
>>>bias if we can - this is what got us into all the MWP misunderstanding
>>>in the first place, perhaps (e.g., nice MWP in Europe/Atlantic region -
>>>must be global)
>>>
>>>2) If you guys could agree on the series and the interval, that'd be
>>>great. We agree it would be good to start before 1000 and end before the
>>>Renaissance (15th century?). If you want more feedback on these issues,
>>>we're happy to provide, but it seems logical that you pick series and
>>>intervals so that each series covers the entire interval selected.
>>>
>>>3) Don't use the Chesapeak record - it is likely biased by salinity
>>>
>>>4) We'd like Keith and Tim to draft the final figure so that it matches
>>>the look and style of the other two figs they have made. Hope this is
>>>doable. Tom, does Keith have all the data? Thanks for sending if not.
>>>
>>>5) We agree that Tom should NOT be a CA given that he was officially one
>>>of the ZOD reviewers. Of course, this doesn't represent a real conflict,
>>>but we need to avoid even the appearance of conflict. We greatly
>>>appreciate all the feedback that Tom is providing! Is this plan ok w/
>>>you Tom? We think you're cool with it, but just want to check one more time.
>>>
>>>That... it is. Please let us know if there are any more questions. Keith
>>>- feel free to try and get Eystein on his cell doing your work hours if
>>>you want quick feedback. Or we can do this by email - he's not in a very
>>>email friendly place right now, but the fishing appears to be ok.

>>>
>>>Again, thanks to you both for all the discussion and thought that has
>>>gone into this figure.
>>>
>>>Best, peck
>>>--
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>>>Professor, Department of Geosciences
>>>Professor, Department of Atmospheric Sciences
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564. 1122052662.txt
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##########

From: David Rind <drind@giss.nasa.gov>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: [wg1-ar4-ch06] Updated 6.1 (inc. Bette's comments)
Date: Fri, 22 Jul 2005 13:17:42 -0400
Cc: wg1-ar4-ch06@joss.ucar.edu

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Hi Eyestein,

Thanks for your comments. With respect to the

suggested changes in paragraphs 1,2 and 4, they seem fine to me. However, I think we need to include in paragraph 5 potential reasons as to why the substantial (and not just significant) high latitude warming that appears in the mid-Pliocene record is not produced in GCMs in response to higher CO2, in general - otherwise we leave the reader with a big question and no possible solution. The tendency of GCM simulations for the future climate to produce an NADW decrease forces those simulations to have minimal high latitude warming in the North Atlantic, exactly opposite the inference from the Pliocene paleo-record (which is quite robust in this respect at least). If the Pliocene record is indicating the opposite of what current models are predicting, it may be offering us a valuable clue...

The suggested reasons also include the comment that the lack of land ice at high northern latitudes might be a strong contributing cause - which would make it a no-analog situation, and hence not fully a GCM problem.

I would favor leaving those two sentences as they were.

David

At 5:19 PM +0200 7/22/05, Eystein Jansen wrote:
>Hi,
>see enclosed some comments to the last version
>of the deep time box. I propose some deletions
>and some toning down of language. What do you
>think?
>
>Eystein
>--
>_____
>Eystein Jansen
>Professor/Director
>Bjerknes Centre for Climate Research and
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>Allégaten 55
>N-5007 Bergen
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>e-mail: eystein.jansen@geo.uib.no
>Phone: +47-55-583491 - Home: +47-55-910661
>Fax: +47-55-584330
>
>Attachment converted: Toltec:IPCC Box
>6.1_latest_EJcomm.doc (WDBN/«IC») (1BE54183)

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http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
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565. 1122126027.txt
##########

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: David Rind <drind@giss.nasa.gov>
Subject: Ad: Re: [wg1-ar4-ch06] Updated 6.1 (inc. Bette's comments)
Date: Sat, 23 Jul 2005 09:40:27 +0200
Reply-to: Eystein Jansen <eystein.jansen@geo.uib.no>
Cc: wg1-ar4-ch06@joss.ucar.edu

Hi David,
thanks for the reply. I think your arguments to add some comments of explanation re Pliocene warmth are convincing and that there is potential relevance for IPCC concerning lat. heat transport in a world with less land and sea ice. My concern is that I don't think the text should be interpreted to imply that the Mid Pliocene was free of Arctic sea ice and Greenland was ice free. There is evidence from the recent IODP Central Arctic Drilling (have to check what ref. to use) of sea ice cover through the Pliocene. I have publishet on IRD evidence for a Greenland ice sheet of some sort. Concerning THC, N Atlantic data indicate strong presence of NADW akin to now, but we cannot constrain overturning rate. Both Nordic Seas an Arctic Ocean was poorly ventilated and deep water formation to feed overflows was shallover, perhaps due to higher temperature?
Instead of deleting the section I proposed, I suggest changing it as follows: After (Rind and Chadler 1991) add , "for which available proxy data are inconclusive", and Instead of writing "absence of land ice", write " reduced extent of land and sea ice". I will find the best refs for this on Monday.
Cheers
Eystein

_____
Wg1-ar4-ch06 mailing list
Wg1-ar4-ch06@joss.ucar.edu
http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06

566. 1122300990.txt
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From: Tom Crowley <tcrowley@duke.edu>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: participation in IPCC
Date: Mon, 25 Jul 2005 10:16:30 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

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Hi all, there is another reason why I should not be formally listed as an LA - it is my understanding that IPCC contributors have to be a little careful about getting involved in political matters that could be used to impugn the integrity of the process - well I am starting to do just that, with the attached commen in Eos, plus some radio interviews where I have been somewhat pointed in my thoughts.

I suppose its still ok to be a reviewer, but even then you might keep these comments in mind, tom

>

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567. 1122394173.txt
##

From: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Last Millennium section 6.5 - comments by SR
Date: Tue, 26 Jul 2005 12:09:33 +0200
Cc: wg1-ar4-ch06@joss.ucar.edu, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <Eystein.Jansen@geo.uib.no>

Hi Keith and all, (please everyone have a look at point (4))
I think section 6.5 is in remarkably good shape (certainly compared to my own..).
There are some comments from me:
(1) About the new proxy reconstructions, the section says: "Most of these are shown..." in
the Figure. This immediately raises the question: why not all? Which one is not shown? This
section will be scrutinised with great suspicion by some people, so we need to be careful.
Can you clarify which one you left out, and why? Or can we just write: "These are shown..."
That would be much nicer.
(2) Several times you say "simply scaled" - would "scaled" do as well? The "simply" in this
context sounds a bit like we criticise that.
(3) Is "predictand" a word that everybody knows? I'd never seen it before.
(4) Now here is my biggest question, that I think we need to discuss in the whole group.
Figure 6.5.2-1 shows simulations of the past millennium, relative to 1500-1899 means. Is
this really the best reference period?
Contra: it differs from how we show the data reconstructions, i.e., relative to 1961-1990.
Everyone knows what that climate actually was, since there are good instrumental data for
1961-1990, so that it makes sense to look at changes relative to that period. Nobody knows
what the real 1500-1899 mean was, so this is a fictitious baseline.
Pro: it gets rid of "end effects", i.e. model initialisation problems at the beginning (as
in Von Storch 04), and different anthropogenic forcings used at the end (e.g. some ignore
aerosols); the simulations look closer together in this way (right?)
I have not formed a clear opinion on what is best.
(5) Also on the figures: I like the grey bands, but here's a suggestion for improvement:
instead of leaving the core region between those two bands white, I think they should also
be shaded - either the same grey, or a darker shade of grey. This makes it more clear that
we are talking about one, wide uncertainty band here, not about two seperate things. It had
me confused at first when I saw it, even though I was there when we discussed this in

Beijing.
Final point: we need to keep an eye on developments concerning the model tests of the proxy
method, there seem to be several important things in the pipeline there.
Cheers, Stefan
--
To reach me directly please use: [1]rahmstorf@ozean-klima.de
(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

Stefan Rahmstorf
[2]www.ozean-klima.de
[3]www.realclimate.org

References

1. mailto:rahmstorf@ozean-klima.de
2. http://www.ozean-klima.de/
3. http://www.realclimate.org/

568. 1122422429.txt
##########

From: Tim Osborn <T.Osborn@uea.ac.uk>
To: eystein.jansen@geo.uib.no, jto@u.arizona.edu, k.briffa@uea.ac.uk
Subject: MWP figure
Date: Tue, 26 Jul 2005 20:00:29 +0100
Cc: t.osborn@uea.ac.uk

Dear Keith, Peck and Eystein,

as you'll have seen from Tom C's replies to my fairly direct requests for the data that went into his MWP
figure, he seems somehow reluctant to send it to me and prefers me to find it myself (including
spending a week re-assembling a Mongolian composite). I have no time to do this, so have instead
reverted to using the very similar data that we already had. I'm sure it's so similar that it tells the same
story.

So, the attached file is my latest attempt at the MWP figure. It shows 8 local/regional proxy series,
normalised over a common period after filtering to the 20-year and longer time scale. It also shows a
composite mean, and no temperature scale. The period covered is 850 to 1350.

What do you think? Hopefully it is what you want.

I've started on the SH figure, having received data from Ricardo and borehole series for SH, S. Africa and
Australia from Jason/Henry. I need to sort out Tasmania / New Zealand instrumental data - Ed has this,
though I could extract appropriate boxes from the Jone et al. gridded data set if necessary.

I'll include these series:

S American trees*2 plus instrumental T overlaid

S African and Australian boreholes (must also overlay instrumental T to explain why values are all

negative - due to early sampling prior to strongest warmng)

Tasmanian and New Zealand trees*2 plus instrumental T)

It may be Friday by the time I get this one done.

Cheers

Tim

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\ipccar4_mwpbox.pdf"

569. 1122557838.txt
##

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: New versions
Date: Thu Jul 28 09:37:18 2005
Cc: Susan Solomon <ssolomon@al.noaa.gov>

Kevin/Susan,
I'll look over 3.9. A quick look at the back references to sections which contain
the detail summarized here, suggests that you've got the right level of section. I guess
we could add a sentence to say that this/these are the principal section(s), but the whole
of the x.x section is likely also relevant.
I've added Susan in to show what we're doing. It might be appropriate for other
chapters. Part of my reason was traceability, but also we are referring to subsequent
sections in Chapters 4 and 5.
The figures seem to be coming along well. Pdfs are also. I'll send another
reminder about these out later today, when I've had one last look for a few of them.
I'll attach section numbers as there are so few now.
Cheers
Phil
The bulletted points and back references are below.
· Global-mean surface temperatures show overall warming of 0.75ºC over the
19012004 period although rates of temperature rise are much greater after 1979. Both land
surface air temperatures and SST show warming although land regions have warmed at a faster
rate than the oceans for both hemispheres in the past few decades, consistent with the much
greater mass and thermal inertia of the oceans. Some areas have not warmed in recent
decades, and a few have cooled although not significantly. [3.2.2]
· The warming of the climate is consistent with a widespread reduction in the
number of frost days in mid-latitude regions. The latter is due to an earlier last day of
frost in spring rather than a later start to the frost season in autumn. The increase in
the number of daily warm extremes and reduction in cold extremes across over 70%

of land
regions studied have been most marked at night over the 1951-2003 period. The greater
increase in nighttime as opposed to daytime temperatures has continued. [3.8.2.1]
· Widespread (but not ubiquitous) decreases in continental DTR since the 1950s
occur with increases in cloud amounts, as expected from the impact of cloud cover on solar
heating of the surface. [3.2.2; 3.4.3]
· The temperature increases are consistent with the observed nearly worldwide
reduction in mountain glacier mass and extent. A few regions of the world where mountain
glacier termini are determined by winter precipitation totals, as opposed to summer
temperatures, do show some advances, but these are consistent with changes in circulation
and associated increases in winter precipitation (e.g., southwestern Norway, parts of
coastal Alaska, southern Chile and Fjordland of the South Island of New Zealand). Tropical
ice caps in South America, Africa and Tibet have all shown remarkable declines in recent
decades. If continued, some may disappear within the next 30 years. Reduction in mass of
such glaciers depends on local heat budgets, which is not necessarily reflected in local
temperature changes. The temperature records all show a slight warming, but nowhere near
the magnitude required to explain the rapid demise of the many of the ice caps. [4.5]
· Snow cover has decreased in many NH regions, particularly in the spring season,
consistent with greater increases in spring as opposed to autumn temperatures in
mid-latitude regions. The decrease is accompanied by increased active layer thickness above
permafrost and decreased seasonally frozen ground depths. [3.3.2.3; 4.2.4, 4.8]
· Sea-ice extents have decreased in the Arctic, particularly in the spring and
summer seasons, and patterns of the changes are consistent with regions showing a
temperature increase, although changes in winds are also a major factor. Decreases are
found in the length of the freeze season of river and lake ice. [3.2.2.3; 4.3, 4.4, 5.3.3]
· Surface temperature variability and trends since 1979 are consistent with those
estimated by most analyses of satellite retrievals of lower-tropospheric temperatures,
provided the latter are adequately adjusted for all issues of satellite drift, orbit decay,
different satellites and stratospheric influence on the T2 records, and also with ERA-40
estimates of lower-tropospheric temperatures. The range from different datasets of global
surface warming since 1979 is 0.15 to 0.18 compared to 0.12 to 0.19 K decade^-1 for MSU
estimates of lower tropospheric temperatures. [3.4.1]
· Stratospheric temperature estimates from radiosondes, satellites (T4) and
reanalyses are in qualitative agreement recording a cooling of between 0.3 and 0.8ºC
decade^-1 since 1979. Increasing evidence suggests increasing warming with altitude from

1979 to 2004 from the surface through much of the troposphere in the tropics, cooling in the stratosphere, and a higher tropopause, consistent with expectations from observed increased greenhouse gases and changes in stratospheric ozone. Over extratropical land, the larger warming at night is associated with larger surface temperature changes. [3.4.1]

· Radiation changes at the top-of the atmosphere from the 1980s to 1990s, possibly ENSO related in part, appear to be associated with reductions in tropical cloud cover, and are linked to changes in the energy budget at the surface and in observed ocean heat content in a consistent way. [3.4.3; 3.4.4]

· Surface specific humidity has also generally increased after 1976 in close association with higher temperatures over both land and ocean. Consistent with a warmer climate, total column water vapour has increased over the global oceans by 1.2 ± 0.3% from 1988 to 2004, consistent in patterns and amount with changes in SST and a fairly constant relative humidity. Upper tropospheric water vapour has also increased in ways such that relative humidity remains about constant, providing a major positive feedback to radiative forcing. [3.4.2]

· Over land a strong negative correlation is observed between precipitation and surface temperature in summer and in low latitudes throughout the year, and areas that have become wetter, such as the eastern United States, have not warmed as much as other land areas. Increased precipitation is associated with increases in cloud and surface wetness, and thus increased evaporation. Although records are sparse, continental-scale estimates of pan evaporation show decreases, due to decreases in surface radiation associated with increases in clouds, changes in cloud properties, and increases in air pollution in different regions from 1970 to 1990. There is tentative evidence to suggest that this has reversed in recent years. The inferred enhanced evaporation and reduced temperature increase is physically consistent with enhanced latent versus sensible heat fluxes from the surface in wetter conditions. [3.3.5; 3.4.4.2]

· Surface observations of cloud cover changes over land exhibit coherent variations on interannual to decadal time scales which are positively correlated with gauge-based precipitation measurements. [3.4.3]

· Consistent with rising amounts of water vapour in the atmosphere, increases in the numbers of heavy precipitation events (e.g., 90/95^th percentile) have been reported from many land regions, even those where there has been a reduction in total precipitation. Increases have also been reported for rarer precipitation events (1 in 50 year return period), but only a few regions have sufficient data to assess such trends

reliably.
[3.4.2; 3.8.2.2]
· Patterns of precipitation change are much more spatially- and seasonally-variable than temperature change, but where significant changes do occur they are consistent with measured changes in streamflow. [3.3.4]
· Droughts have increased in various parts of the world. The regions where they have occurred seem to be determined largely by changes in SSTs, especially in the tropics, through changes in the atmospheric circulation and precipitation. Inferred enhanced evaporation and drying associated with warming and decreased precipitation are important factors in increases in drought. In the western United States, diminishing snow pack and subsequent summer soil moisture reductions have also been a factor. In Australia and Europe, direct links to warming have been inferred through the extreme nature of high temperatures and heat waves accompanying drought. [3.3.4, QACCS 3.3, 3.8.3, 4.x.x]
· Changes in the freshwater balance of the Atlantic Ocean over the past four decades have been pronounced as freshening has occurred in the North Atlantic and also south of 25°S, while salinity has increased in the tropics and subtropics, especially in the upper 500 m. The implication is that there have been increases in moisture transport by the atmosphere from the subtropics to higher latitudes, in association with changes in atmospheric circulation, including the NAO, thereby increasing precipitation over the northern ocean and in adjacent land areas (as observed). [3.3.2, 3.3.3, 5.3.2, 5.5.3]
· Changes in the large-scale atmospheric circulation are apparent. Increasing westerlies have been present in both hemispheres as enhanced annular modes. In the NH, the NAM and NAO change the flow from oceans to continents and are a major part of the wintertime observed change in storm tracks, precipitation and temperature patterns, especially over Europe and North Africa. In the SH, SAM changes, in association with the ozone hole, have been identified with recent contrasting trends of large warming in the Antarctic Peninsula, and cooling over interior Antarctica. [3.5, 3.6, 3.8.3]
· The 19761977 climate shift toward more El Niños has affected Pacific rim countries and monsoons throughout the tropics. Over North America, ENSO and PNA-related changes appear to have led to contrasting changes across the continent, as the west has warmed more than the east, while the latter has become cloudier and wetter. [3.6, 3.7]
· Variations in extratropical storminess are strongly associated mostly with changes in mean atmospheric circulation, such as changes and variations in ENSO, NAO, PDO, and SAM. Wind and significant wave height analysis support the reanalysis-based evidence

for an increase in extratropical storm activity in the NH in recent decades. After the late
1990s, however, some of these variations seemed to change sign. [3.5, 3.6, 3.8.3.2]
· Changes are observed to occur in the number, distribution and tracks of tropical
storms that are clearly related to ENSO phases and to a slightly lesser extent to the AMO
and QBO modulations. Increases in intensity and lifetimes of tropical storms since the
1970s are consistent with increases in SSTs and atmospheric water vapour. [3.8.3.1]
· Sea level likely rose about 18±3 cm during the 20^th century, but increased
3.0±0.4 mm/year after 1992, when confidence increases from global altimetry measurements.
During this period, glacier melt has increased ocean mass by order 1.0 mm/year, increases
in ocean heat content and associated ocean expansion are estimated to contribute 1.6
mm/year, while changes in land water storage are uncertain but may have taken water out of
the ocean. Isostatic rebound contributes about 0.3 mm/year. This near balance gives
increased confidence that the observed sea level rise is a strong indicator of warming, and
an integrator of the cumulative energy imbalance at the top of atmosphere.[4.5, 4.7, 4.9.8,
5.2, 5.5]
At 23:47 27/07/2005, Kevin Trenberth wrote:

Phil
I placed new versions of the figure and text files on my ftp site. I implemented your
suggestion of adding section numbers to the 3.9. I used the ones from the ZOD wrt other
chapters. So they may change. I also added a small piece on freezing seasons on lakes
and rivers that was mentioned in the last para but not in any bullets. You may like to
comment on this as some are x.x, some are y.y.y and some are z.z.z.z.
In the first case the whole section is really applicable and so mentioning each
subsection does not seem worthwhile. Should we go to the z.z.z.z level, as that is not
in the TOC?
In doing this I found that two sections in 3.8 had very similar titles and so I changed
that of 3.8.3 to explicitly say tropical and extratrtopical storms and extreme events,
which are the 3 subsections. The Table of contents (TOC) is all up to date, and now
corrected for one subsection that was mislabeled as level 2 instead of 3.
Several figures have been revised.
I am out tomorrow all day but Lisa tells me she is up to w in the references. So should
have a complete new version on Friday. Hopefully several of the figures will be by
upgraded then too. I have a new Fig 3.3.1 but can't work with it: something wrong with
it, so I've asked Dave E for a different one. Main outstanding stuff is all waiting on
Dave Easterling. I have requests in to Tom Karl on the 2 CCSP figures.
Following my earlier email I have responses on Figs 3.2.3: now good, 3.4.6 I

did, 3.5.2,
     and one from Groisman. So only 7 figures not in final form.
     I believe we have 74 figures in the sense that they are separate files.
     That includes counts of 1 for several multipanel files (like some T ones or the
     hurricane one), but 4 for some 4 panel ones like the ENSO one, where the files
were all
     generated anew and independently. So the good part is that 67 of them are in
great
     shape. We actually have 48 figures counting the 2 TAR ones that will be in
3.9, and 3
     in the 3 QACCS.
     Cheers
     Kevin
     --
     ****
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References

   1. http://www.cgd.ucar.edu/cas/

570. 1122601784.txt
##
##########

From: Fortunat Joos <joos@climate.unibe.ch>
To: Fortunat Joos <joos@climate.unibe.ch>
Subject: Re: [Wg1-ar4-ch06] introduction 6.2.1 - 6.4.1 holocene solar.
Date: Thu, 28 Jul 2005 21:49:44 +0200
Cc: Eystein Jansen <Eystein.Jansen@geo.uib.no>

Content-Type: text/plain; charset=iso-8859-1
Content-Transfer-Encoding: 7bit
X-Mime-Autoconverted: from 8bit to 7bit by courier 0.47

Hi Peck and Eystein,

Here a reduced version of Box 6.2, taking into account suggestions from David
and Bette. The text is now 1.5 pages, i.e. just slightly above target. The
entire Box should now fit on less than 1 IPCC page (Assigned 0.75 page).

I am willing to take the next effort to shorten when the review comments of the
FOD are in.

With best regards,

Fortunat

Quoting Fortunat Joos <joos@climate.unibe.ch>:

> Hi Peck and Eystein,
>
> here my general comment on the introduction and specific comments on section
> 6.2.1 and 6.4.1.
>
> 6.1 and 6.2.1:
>
> Well done!
>
> (1) Perhaps, words such as 'significant' and other value judgment terms could
> be
> used somewhat less. e.g. 'With proper care, current methodologies alloww more
> accurate age models' more accurate than what? We always hope that things are
> done with proper care.
>
> (2) The following sentence must in my opinion be deleted: 'but also note that
> new work reveals that
> cosmogeninc-isotope-derived estimates of solar forcing for the Holocene are
> not
> likely as well-constrained as commonly thought.'
>
> This is a very sweeping statement that is not backed up by the chapter text.
> It
> is also a very policy sensitive statement. We are either able to firmly
> support
> that or to drop it. I suspect that the paleo community would be divided about
> this.
>
> Scott Lehmann has just shown me a plot with a really nice correlatin between
> d18O in N-pachy in the Norht-Atlatnic and sunspots over the past 400 years.
> Yes, there appears to be a link.
>
> I also doubt that some of the existing work, e.g. Fleitman etc can be
> dismissed
> so easily.
>
> section 6.4.1:
>
> (3) I also think that the Holocene text on solar needs some readjustments.
> Linking the studies suggesting solar changes and those with NADW variations
> seems a somewhat improper comparison.
>
> The present text reads:
>
> 'Based on the correlation between changes in atmospheric concentrations of
> cosmogenic isotopes (10Be or 14C) and climate proxy records, some authors
> argue
> that solar activity may be the driver for an organised centennial to
> millennial
> scale variability (e.g., (Bond et al., 2001; Fleitmann et al., 2003) (Karlen,
> 1996) (Wang et al., 2005b), whereas others point to modes of variability
> driven
> by processes within the climate system, for instance related to the deep
> ocean
> circulation (Bianchi and McCave, 1999) (Duplessy et al., 2001) (Oppo et al.,
> 2003) (Marchal et al., 2002).'
>
> I suggest to change it to something along the following line.
>
> "'Based on the correlation between changes in atmospheric concentrations of
> cosmogenic isotopes (10Be or 14C) and climate proxy records, many studies

> suggest that solar activity may be a driver for centennial to millennial
> scale
> variability (e.g., (Bond et al., 2001; Fleitmann et al., 2003) (Karlen, 1996)
> (Wang et al., 2005b). The importance of (forced or unforced) modes of
> variability within the climate system, for instance related to the deep ocean
> circulation has been pointed out (Bianchi and McCave, 1999) (Duplessy et al.,
> 2001) (Oppo et al., 2003) (Marchal et al., 2002)."
>
> With best regards,
>
> Fortunat
>
>
>
> Quoting Fortunat Joos <joos@climate.unibe.ch>:
>
> > Hi Stefan, Peck and all,
> >
> > Here an update on the abrupt event figure and the figure caption. There
> were
> > some lost lines in the one send yesterday - please delete. I have now also
> > numbered some of the D/O events and the A1 to A4 events.
> >
> > The purpose of the figure is to demonstrate the asynchrounous evolution of
> NH
> > and SH temperatuere and the magnitude of the GHG changes during abrupt
> > events.
> >
> > Clearly, it would be great if the figure could be amended by other
> > information,
> > e.g. from the land or sediment records. We may also think about indicating
> > the
> > local Greenland temperatre change for the bigger events.
> >
> > Any ideas, suggestions, comments are welcomed.
> >
> > Peck: please include ERIC MONNIN as a Contributing author.
> >
> > Eric has synchronized the Taylor Dome and Dome C data on the GRIP time
> scale
> > and
> > helped me greatly to put toghether the records for the abrupt event and for
> > the
> > LGM-box figures.
> >
> > With best regards,
> >
> > Fortunat
> >
> > Quoting Fortunat Joos <joos@climate.unibe.ch>:
> >
> > > Hi,
> > >
> > > Here finally the abrupt event figure plus an update of the LGM-box
> figure.
> > > Will provide figure caption, section 6.6. text and shortened LGM-box
> > > tomorrow.
> > >
> > > With best regards,
> > >
> > > Fortunat
> > >
> > > --

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From: Phil Jones <p.jones@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>, "Tett, Simon" <simon.tett@metoffice.gov.uk>
Subject: Re: Bristlecones!
Date: Fri Jul 29 16:30:35 2005
Cc: Keith Briffa <k.briffa@uea.ac.uk>

    Simon,
       If you go to this web page
    [1]http://www.ucar.edu/news/releases/2005/ammann.shtml
    You can click on a re-evaluation of MBH, which leads to a paper submitted
    to Climatic Change. This shows that MBH can be reproduced. The R-code
    to do this can be accessed and eventually the data - once the paper has been
    accepted.
       IPCC will likely conclude that all MM arguments are wrong and have
    been answered in papers that have either come out or will soon. MBH
    is just one curve of many - more now than there were in 2001. MBH is
    still in the spaghetti of curves, and is not an outlier. If there are outliers
    it will be Esper et al. and another one.
       Bristlecones are only crucial to the issue if you are MM. They misused
    them, by their PCA application. This is all well-known to those in the know.
      I have reviewed the CC paper by Wahl and Ammann. It reproduces all
    the mistakes MM have made, so they know how and why their results
    have been achieved. I can send you the paper if you want, subject
    to the usual rules.
       MBH have all responded to the same requests as IPCC got from the
    US Senate. Their responses are all posted at [2]http://www.realclimate.org/
       The skeptics have shot themselves in the foot over this one.
    Cheers
    Phil

At 15:17 29/07/2005, Tim Osborn wrote:

At 14:27 28/07/2005, Tett, Simon wrote:

John Houghton is being quized by bits of the US senate. One question is "Whats the status of the review of the Mann hockey stick temperature curve? I understand that studies by Stephen McIntyre and Ross McKitrick suggest that it relied on the statistically insignificant bristlecone pine. Is the IPCC taking another look at that work, which forms the basis for much of todays climate change debate?"
My current thoughts on an answer is to say that other reconstructions show a similar pattern (though not magnitude). However how many of the other reconstructions use the bristlecone data? [I suspect yours does not]

Hi Simon - I was away yesterday, so couldn't answer then. Hopefully it isn't too late to answer today.

(1) I don't understand what they mean by describing the bristlecone pine as "statistically insignificant".

(2) The Mann, Bradley and Hughes (MBH1999) reconstruction is only one small piece of information in today's climate change debate.

(3) As far as I understand, then yes the MBH1999 reconstruction does give quite a lot of weight to a few western US tree-ring series, which are mostly bristlecone pines for the longest records.

(4) Other reconstructions show similar shape (though not magnitude) and support similar conclusions (regarding the unprecedented nature of recent warmth/warming trend). This is the main argument to make, as you thought. Some of these other reconstructions do not include these bristlecones (e.g. Briffa, 2000; Crowley et al., 2003; Moberg et al., 2005; Briffa et al., 2001). Crowely and Moberg use different Bristlecone records I think. Other reconstructions do use the same Bristlecone pines (e.g., Mann and Jones, 2004). BUT the critical thing is that the studies either do not use these Bristlecone pines, or if they do use them, then they give them much more similar weighting to the other records used. I think MBH1999 is the only one that might give them a dominant weighting.

(5) IPCC is assessing all published work that relates to these issues in preparation for the AR4 in 2007. This includes the McIntyre and McKitrick papers as well as papers that report results contrary to McIntyre/McKitrick, such as the paper in press by Wahl and Amman that shows the Mann et al. results are reproducible.

cc'd for additional comments to Phil and Keith (when he's back).

Cheers

Tim

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References

1. http://www.ucar.edu/news/releases/2005/ammann.shtml
2. http://www.realclimate.org/
3. http://www.cru.uea.ac.uk/~timo/
4. http://www.cru.uea.ac.uk/~timo/sunclock.htm

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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@psu.edu
Subject: Re: Out in latest J. Climate
Date: Thu Aug 4 09:49:54 2005

Mike,
Gabi was supposed to be there but wasn't either. I think Gabi isn't being objective as she might because of Tom C. I recall Keith telling me that her recent paper has been rejected, not sure if outright or not.
Gabi sees the issue from a D&A perspective, not whether any curve is nearer the truth, but just what the envelope of the range might be.
There is an issue coming up in IPCC. Every curve needs error bars, and having them is all that matters. It seems irrelevant whether they are right or how they are used. Changing timescales make this simple use impractical.
We have a new version of HadCRUT just submitted, so soon the'll be HadCRUT3v and CRUTEM3v. The land doesn't change much. This has errors associated with each point, but the paper doesn't yet discuss how to use them.
I'll attach this paper. Only just been submitted to JGR - not in this format though. This format lays it out better.
Thanks for reminding Scott.
Cheers
Phil
At 08:48 04/08/2005, you wrote:

Hi Phil,
Thanks for the heads up. Will be prepared for this then. I thought that Gabi Hegerl was
involved with this guy? Doesn't she know better? It is disturbing that she hasn't set
them straight on this.
By the way, as you may or may not have heard, its been discovered that there is a major
error in Von Storch et al '04 that they now appear to be trying to hide (they have some
obscure article in an Italian journal where they attempt to justify the error).

There
are several comments that have been or are soon to be submitted to Science about this.
As it turns out, they introduces a spurious step in their supposed implementation of the
MBH98 procedure in which they detrended the series first, gives completely wrong
results.. Caspar Ammann and Gene Wahl and David Ritson of Stanford have both
independently discovered this, because they noticed that amplitude of the calibrated
signal in VS04 scales with the signal-to-noise ratio--this was the first clue that there
was a major problem. There may be calls upon Science for them to retract their paper.
The results are completely wrong, aside from the problems w/ the GKSS simulation. You
can expect to hear more about this soon...
I'll remind Scott about the proxies. He and Zhang are in the process of screening the
proxy series for temperature signals, etc. Once they've done that, should be more
useful. I expect we'll be able to get you some stuff by late August.
I did hear about the 3 papers coming out in Science. Apparently Donald Kennedy is doing
an editorial that will discuss this in the context of the whole Barton business. That
should be interesting...There will be articles by both Gavin and Steve Sherwood on
"RealClimate" in coordination with the publication of the papers in Science Express.
This should help turn the debate around.
talk to you later,
mike
Phil Jones wrote:

Mike,
He's been working with Myles Allen. Tim went to the first meeting of this
Dutch funded project near Oxford last week.
Tim said they were doing some odd things, like correlating all the proxy series
they had with CET (yes CET)! Even the few SH proxies they have. The others
who went to the meeting were Zorita and Moberg. Zorita was still showing the
GKSS run with Moberg series, even though its forcing is too large, it doesn't
have aerosols in the 20th century and has spin up problems for the first
200 years.
Meeting wasn't that productive according to Tim. There was a belief amongst
those there that all trees you used have lost low-freq, but this isn't true as you
know.
Also, it was a good job Keith wasn't there (he didn't go as his father died the
weekend before and he's not been in CRU since) as Martin assumed that RCS
was developed by Esper (who also wasn't there). Tim put them right on this
one, but RCS isn't applicable for normal tree sites, nor useful for bristlecones.
Tim said Esper was wrong is his use of RCS, but they wouldn't accept that
as Esper wasn't there to defend himself!
Basically only Tim knew anything about proxy data especially trees. Tim
got the impression that they wanted to find that MBH is wrong. Given the
previous comment, as you weren't there they are using double standards.
So, in conclusion, act carefully. Don't jump in, but some carefully thought

through comments should be productive. Suggest they read the RevG article.
Martin isn't associated with the contrarians, but he's not in possession of the all the facts. He isn't aware of Casper's work, nor your latest study which you sent the other day, nor Rutherford et al.
There still seems to be a belief in these lower responding proxies. This is something we want to work on more here, as the only way it seems to show that these lower-freq proxies aren't that great is to use higher-freq proxies.
When you're back or sometime, can you remind Scott to send your latest set of proxies. I'll have some time in the autumn to work on them as the AR4 should be in by Aug 12.
Science should be publishing 3 papers on the MSU issue by the end of Aug or early Sept. This is Mears/Wentz, Santer et al. and Sherwood et al. Latter shows that sondes are only truly reliable when flown at night. Daytime ones have all manner of problems with heating, just like air temps on board ships - hence the NMAT series.
I'll forward another email for interest.
Cheers
Phil
At 03:40 04/08/2005, you wrote:

Hi Phil,
Thanks, yes I'm in China now. As you might imagine, ,things have been very busy, but
calming down a bit. Looks like Barton may be backing down...
Martin Juckes has an invited talk in my session. I invited him, because he was working
w/ Stott et al, and so I assume he was legit, and not associated with the contrarians.
But if he's associated w/ the Dutch group, he may actually be a problem. Do you have
additional information about him and what he has been up to?
Thanks,
mike
Phil Jones wrote:

Mike,
Good to hear it is out !
Hope the changeover is going OK and life is getting back to normal.
If you're not gone to China yet - you'll meet someone called Martin Dukes (?). He's giving a talk at your session. He knows about maths etc but not much about paleo ! Might need some education, but is probably OK. Not met him, but Tim has. Doing some worked funded by the Dutch govt on the hockey stick.
Cheers
Phil
At 04:05 03/08/2005, you wrote:

Dear Colleagues,
FYI, two papers attached:
First (reprint), Rutherford et al, is now out in latest issue of Journal of Climate.
This paper, aside from addressing other more scientifically-worthwhile issues, also
happens to discredit most of the McIntyre and McKitrick claims.
Second (preprint), Mann et al, is formally in press (i.e., has gone off to the AMS
production staff) in Journal of Climate. This paper strongly challenges the conclusions
of von Storch et al (2004), and raises some methodological issues w/ the approach used
by Moberg et al (2005).
Feel free to pass along to others. Thanks

Mike
--
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References

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2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

573. 1123268256.txt
##

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: MWP figure
Date: Fri, 5 Aug 2005 14:57:36 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>,
<oyvind.paasche@bjerknes.uib.no>

<x-flowed>
Hi Tim and Keith - Hope you're not going to kill
me, but I was talking with Susan Solomon today,
and she impressed me with the need to make
several points if we can.

One issue (other to come in a subsequent email)
is whether we can extend the MWP box figure to
include the 15th century. I don't read the blogs
that regularly, but I guess the skeptics are
making hay of their being a global warm event
around 1450AD. I agree w/ Susan that it is our
obligation to weigh in on issues like this,
so.... can we extend the fig to extend up to
1500AD?

Sorry about this, Tim. Of course we need it yesterday.

Thanks x10**6

best, peck

>Dear Eystein, Peck and Keith,
>
>I spotted a minor error in the MWP figure
>(reference period was 1001-2000 but should have
>been 1001-1980 because some series stop in 1980)
>and a typo in the legend, so here is a revised
>MWP figure with these things corrected and a
>slight adjustment to line thicknesses and font
>sizes.
>
>As before I've included .ps, .pdf and .gif
>versions because I'm not sure what you prefer.
>
>I've also drafted a caption - see attached .doc
>file. Feel free to modify as necessary. I
>think it covers the necessary details including
>normalisation period, but perhaps it is a bit
>"wordy" and unnecessarily repeats things already
>in the MWB box text?
>
>I'm still working on SH figure/caption.
>
>Cheers
>
>Tim
>
>
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>MWP box figure.doc (WDBN/«IC») (0008D1BB)
>Attachment converted: Macintosh HD:ipccar4_mwpbox.gif (GIFf/«IC») (0008D1BC)
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</x-flowed>

574. 1123513957.txt
##
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: MWP figure
Date: Mon, 8 Aug 2005 11:12:37 -0600
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>,
<oyvind.paasche@bjerknes.uib.no>

<x-flowed>
Hi Tim - Decisions, decisions... thanks so much
for taking the initiative. I think - for the
reason you state, we should go for the one that
includes the 20th century. We make clear that
these are not reconstructed temp, but normalized
anomalies - this keeps us out of some trouble.
But, I think the main message is that we're
looking at this issue from every angle. And,
we're letting others see the issue from every
angle. It adds punch.

this means that the MWP box needs to talk about
the period around 1400 - can you make sure that's
on Keith's radar screen. I believe that
historians talk about the Medieval Period going
to at least 1450, so what the heck...

I you can adjust the caption to work, and then
send both it and the final fig to Øyvind, me and
Eystein that would be good - make sure Keith is
ok with it all first, too.

Thanks Tim! Best, Peck

>Hi Peck,
>
>there is a period around 1400 AD when the proxy
>records we've used in this MWP figure do
>indicate a warm period - and all records show
>positive anomalies at the same time. Thus it
>couldn't/shouldn't be dismissed in the same way
>as the MWP, as a period of disparate regional
>behaviour, albeit with more records showing
>warming than cooling. For 1400, all indicate
>warming but with smaller magnitude than the 20th
>century. If the figure were extended to cover
>the 15th century, then it would also seem
>necessary to extend it to the present so that
>the 1400 period could be compared with the 20th
>century.
>
>I've attached 3 versions of the figure.
>
>850-1350 as originally sent.
>
>850-1500 showing warm anomaly in 1400, but
>cannot tell how warm relative to present-day.
>
>850-2000 showing 1400 was not as anomalous as present-day.
>
>Take your pick, Peck!
>
>Cheers
>
>Tim and Keith
>
>At 21:57 05/08/2005, Jonathan Overpeck wrote:
>>Hi Tim and Keith - Hope you're not going to
>>kill me, but I was talking with Susan Solomon
>>today, and she impressed me with the need to
>>make several points if we can.
>>
>>One issue (other to come in a subsequent email)
>>is whether we can extend the MWP box figure to
>>include the 15th century. I don't read the
>>blogs that regularly, but I guess the skeptics
>>are making hay of their being a global warm
>>event around 1450AD. I agree w/ Susan that it
>>is our obligation to weigh in on issues like
>>this, so.... can we extend the fig to extend up
>>to 1500AD?
>>
>>Sorry about this, Tim. Of course we need it yesterday.
>>
>>Thanks x10**6
>>
>>best, peck
>>
>>>Dear Eystein, Peck and Keith,

>>>
>>>I spotted a minor error in the MWP figure
>>>(reference period was 1001-2000 but should
>>>have been 1001-1980 because some series stop
>>>in 1980) and a typo in the legend, so here is
>>>a revised MWP figure with these things
>>>corrected and a slight adjustment to line
>>>thicknesses and font sizes.
>>>
>>>As before I've included .ps, .pdf and .gif
>>>versions because I'm not sure what you prefer.
>>>
>>>I've also drafted a caption - see attached
>>>.doc file. Feel free to modify as necessary.
>>>I think it covers the necessary details
>>>including normalisation period, but perhaps it
>>>is a bit "wordy" and unnecessarily repeats
>>>things already in the MWB box text?
>>>
>>>I'm still working on SH figure/caption.
>>>
>>>Cheers
>>>
>>>Tim
>
>
>Attachment converted: Macintosh HD:mwpbox8502000.pdf (PDF /«IC») (00091133)
>Attachment converted: Macintosh HD:mwpbox8501500.pdf (PDF /«IC») (00091134)
>Attachment converted: Macintosh HD:ipccar4_mwpbox 2.pdf (PDF /«IC») (00091135)
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575. 1123514677.txt
##
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: David Rind <drind@giss.nasa.gov>
Subject: RE: solar MM
Date: Mon, 8 Aug 2005 11:24:37 -0600
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

David - sounds promising. So, the bottom line is that a little disagreement is ok - that's
a reflection of the real uncertainty? But, the discrepancy is not all that big in the end?
No need to take this to a higher level?

Keith Briffa is back on line and finishing off Section 6.5, so you might want to send him
an email w/ suggestions that help keep chap 6 compatible w/ 2 and 9 - for example, with
respect to solar, we acknowledge the forcing could be less than 0.5 W/m**2, and the
uncertaintly wrt to trop aerosols and land albedo is significant - we could easily be
closer to chap 9's estimate. Would you say the key is that our analysis acknowledge the
uncertainty so as to overlap well with the other chapters?

Keith - please make sure you send your new 6.5 to David too - while you were out, he was
working hard w/ chap 2 and 9 to make sure we (the IPCC) avoid saying things that confuse.
The comparison of radiative forcings from 3 different angles is what assessment is all
about, and it's great David has had the patience to help figure it all out.

Thx, Peck

Hi Gabi,

The key to your proposed solution is the updated numbers from Chapter 2. If indeed the
radiative forcing change to 1750 is -1.53, then presumably you have made this consistent
with the earlier part of Chapter 9. The numbers previously looked like this (I haven't
seen the latest version of 6.5, but I've included the previous estimates we had in the
ZOD):

W/m**2

	Chapter 6		Chapter 9
	MM	1750	
Greenhouse gases:	-2.4		-2.6
TROP aerosols:	0.5	0.2	
Solar	-0.5	-0.1	
Volcanic:	?		?
Land albedo:	+0.4	0.03	

Trop O3:		-0.35	-0.4
Strat O3:		+0.15	0.10
1'st indirect aerosol forcing			1.2
STRAT H2O		-0.13	
AVIATION		-0.02	
TOTAL	-2.2	-1.7	

There is essentially no change in greenhouse gas forcing from 1750 to 1700 (see for example Crowley et al., GRL, 2003), so the difference in the estimated numbers is probably due to inclusion of more things or different choices in Chapter 2. A similar statement holds for trop aerosols. One can also use these two to presume that the same also holds true for land albedo. [The value listed for that in Chapter 9 is quite small compared to some other studies; e.g., Govindasamy et al., GRL, 28, 291-294,2001.] So, to the extent these numbers are still discussed in Chapter 6, they should be made consistent with those in chapters 2 and 9.

With respect to your proposed paragraph below: I would drop the comments about trace gas differences but saying land albedo changes may have been greater, along with the additional solar change, could give us the -1.8 W/m**2 forcing.

Concerning the temperature response: the Moberg et al paper itself claims 1°C difference between 1500 and 2000, but the figure seems to show a larger number, perhaps 1.3°C (again, just eye-balling it). However, the coldest time period is not in the MM but before it. I think therefore a better estimate from that paper for the MM would be 1°C.

So, with respect to the sensitivity: if 0.85 W/m**2 is unresolved, then we have a total forcing of ~0.95 W/m**2, and a climate response varying between 0.45°C and 1°C - or a climate sensitivity for 2xCO2 of 1.9°C to 4.2°C, or pretty similar to standard IPCC estimates.

I think this will work!

David

At 1:02 PM -0400 8/6/05, hegerl@duke.edu wrote:

On Sat, 6 Aug 2005 hegerl@duke.edu wrote:
p.s. I modified the text for MM forcing according to below theory (please yell if its off!) which would say (and has questions for you):
During the cool period of the Late Maunder Minimum (approximately 1675-1715), sunspots were generally missing, and solar irradince is believed to have been smaller. This period will be used in Section 9.6 to discuss climate sensitivity; therefore we discuss its radiative forcing

estimates . The estimated difference between present day solar irradiance and the late 17th century Maunder Minimum is presently -1.1 W/m2 (best estimate, range -0.5 to -2 W/m2 , Chapter 2), but with large

uncertainties. This leads to a best estimate radiative forcing of -0.2 W/m2 (-0.1 to -0.35 W/m2 67% confidence interval; note that solar forcing from 1750 to the present is estimated having increased by 0.1 W/m2 , chapter 2). Many radiative forcing changes, particularly those associated with industrialization, are very similar from the present to the Maunder Minimum as they are from the present to preindustrial (total forcing estimated of -1.53 W/m2, see 9.2.1.2). CO2 may have been slightly lower (by???), and land cover changes may also have been glightly greater between the Maunder Minimum and 1750. This yields an approximate net radiative forcing of-1.8 W m-2 (between the late Maunder Minimum and the present, with large uncertainties.
  >
> > Hi David et al, > > I spent some more time pondering the MM forcing.
> I think the best place to start is the updated chapter 2 forcing
> from preindustrial, which is (according to what Joyce pulled out of

> ch 2, so hope its correct):
>
> -1.53 from present to the 1750 period (all included that they deem
> relevant, so no volcanoes because episodic, but all else in there
> including contrails and other weird small stuff, I THINK it also
> includes land cover changes)
>
> We would have to add -0.1 for the more reduced solar (given +0.1 1750 to
> now from ch2, and 0.2 from MM on), and maybe some number for the
> somewhat lower CO2 between 1700 and 1750 (what would that be)? and
> maybe another number for additional changes in land cover?
>
> Overall, the number you had before of -1.8 (after adjusting solar down
> to recent wisdom) seems now pretty good to me.
> Should we keep it, or do you ahve another suggestion?
> I am glad we didn't loose the forcing from MM to present :)))
>
> greetings, let me know what would be good for us to write (and then I'll
> do the arithmetic for the best guess sensitivity once you guys also
> check my numbers for high/low estimates of annual temp changes at that
> period, right now its -0.45 Mann to -1.5 Moberg-readoffplotinahurry by me)
>
> Thanks in advance, I think we are very close to resolve this!
>
> Gabi
>
> On Fri, 5 Aug 2005, David Rind wrote:
>
> > As this continuing exchange has clarified, what's in Chapter 6 is
> > inconsistent with what is in Chapter 2 (and Chapter 9 is caught in
> > the middle!). Worse yet, we've managed to make global warming go
> > away! (Maybe it really is that easy...:)
> >
> > David
> >
> > At 9:49 AM -0600 8/5/05, Bette Otto-Bliesner wrote:
> > >Gabi,
> > >
> > >In Chap 6, we use 2.2 with a range of 1.9 to 2.6 W/m2. The
> > >uncertainty range includes both uncertainties in the ice core
> > >measurements and uncertainties in the radiative transfer
> > >calculations.
> > >

> > >Bette
> > >
> > >_
> >
> >
> > >At 2:27 PM -0400 8/4/05, Gabi Hegerl wrote:
> > >
> > >David, so with the Judith correction only (solar down by 0.4), we
> > >get a total forcing of
> > >0.95 to MM, (after subtracting the 0.85 not realized yet according to Jim)
> > >
> > >Then, if the indirect effect and black carbon is added, wouldn't
> > >this reduce the forcing to nearly nothing?
> > >(or what am I doing wrong, 2.2 changes to 1.8 with new solar, black
> > >carbon and ind aerosol takes away
> > >0.9. yielding 0.9 W/m**2, then Jim says 0.85 of that is unrealized???)
> >
>
> ---
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>
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576. 1123529413.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: [Fwd: wow]
Date: Mon Aug 8 15:30:13 2005

OK. I agree with her on most. I was looking at the file over the weekend. The new 3.8.4 has helped as will the new ones on DTR when we get them In the longer run I would like to get 3.7.1 and 3.7.2 redone - at least plotted better.
Also, in time, we will need to get the Sahel plot updated to have 2004 and 2005 in. Neil Ward was here for a few hours last week. He's now back at IRI, but he was surprised by the UK media and their reporting of the famine in Niger - saying it was all down to lack of rainfall. June in the region was above normal. Problems last year and locusts are the reason. The real reason may not matter on the ground, but the problems will recur as very little is planted this year.
Cheers
Phil
At 15:10 08/08/2005, you wrote:

I had an email exchange with Susan the preceded this.
She is making an early start on reading the chapter and started with ours, using the
version I posted on thursday: so she is referring to the figure file for Ch 3.
Kevin
Phil Jones wrote:

Which ones ? Which version is she looking at?
Susan's been suggesting figures for the paleo chapter. At least we haven't had to cope with that.
Phil
At 15:01 08/08/2005, you wrote:

FYI
-------- Original Message --------
Subject: wow
Date: Fri, 5 Aug 2005 18:08:21 -0600
From: Susan Solomon [1]<ssolomon@al.noaa.gov>
To: [2]trenbert@ucar.edu
References: [3]<p06020416bf194a5ef9bc@[140.172.240.163]>
[4]<4001.128.117.68.3.1123283585.squirrel@webmail.cgd.ucar.edu>
[5]<p0602040bbf19a6388172@[140.172.240.163]>
[6]<4148.24.8.173.64.1123285320.squirrel@webmail.cgd.ucar.edu>
Kevin,
some amazing figures in your chapter, wow
Susan

--
****
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--
****
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References

1. mailto:ssolomon@al.noaa.gov
2. mailto:trenbert@ucar.edu
3. mailto:p06020416bf194a5ef9bc@%5B140.172.240.163%5D
4. mailto:4001.128.117.68.3.1123283585.squirrel@webmail.cgd.ucar.edu
5. mailto:p0602040bbf19a6388172@%5B140.172.240.163%5D
6. mailto:4148.24.8.173.64.1123285320.squirrel@webmail.cgd.ucar.edu
7. mailto:trenbert@ucar.edu
8. http://www.cgd.ucar.edu/cas/
9. mailto:p.jones@uea.ac.uk
10. mailto:trenbert@ucar.edu
11. http://www.cgd.ucar.edu/cas/

577. 1123611283.txt
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jason E Smerdon <jsmerdon@umich.edu>
Subject: Re: SH figure for IPCC AR4
Date: Tue Aug 9 14:14:43 2005
Cc: Henry Pollack <hpollack@umich.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>

Thanks for the comments Jason/Henry. Just wanted to let you know that I've dropped the
uncertainty ranges to be consistent with the other records and also cut the borehole series
at the median sampling dates.
Cheers
Tim
At 16:45 04/08/2005, Jason E Smerdon wrote:

Hi Tim,
Henry and I apologize for not being available the last few days. Henry has been out of
town and I have been in the midst of moving to New York. Nevertheless, we had the chance

to cross paths today and discuss the figure and caption. We hope it is not too late to
add our two cents.
We agree that the uncertainties on the borehole curves should be removed to make the
display more consistent. We have also decided that it would be best to truncate the
borehole curves at their median logging dates. For Australia and Africa those years are
1972 and 1986, respectively. If you wish to discuss the sampling densities, the total
number of boreholes in Australia and Africa are 57 and 92, respectively. The SH has a
total of 165 holes, compared to 695 in the NH.
Let us know if you need anything else. I hope this has not arrived too late and good
luck with everything.
Best Regards,
Jason

578. 1123612499.txt
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>
Subject: New figure for box 6.4 - the Medieval Warm Period
Date: Tue Aug 9 14:34:59 2005

Dear all again,

here is the MWP figure and caption. Note that I don't know what number it should have, because it is for a box not a standard section. So I've just called it "mwpbox" for now. Please can you give it the correct number and put it in the right place in the figures file? Also, when you have numbered it, please let us know so that we can refer to it in the MWP box text.

The figure now goes right up to the present, as requested!

I've attached a word document with caption and .gif figure embedded, but also separate .gif, .pdf and .ps files for the figure. If you don't want all these different formats, then please tell me which one(s) you want and I'll only send those in future!

Cheers

Tim

579. 1123622471.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: jto@u.arizona.edu,eystein.jansen@geo.uib.no
Subject: Section on last 2000-years
Date: Tue Aug 9 17:21:11 2005

Peck and Eystein

in case you tried (!), my phone has been broken for the last few days (yes - honestly).
I am sorry I had to rush off - and stay longer than I had anticipated . The funeral was
delayed
while a post-mortem examination had to be held to establish the precise cause of death.
Ironic
that dad had struggled on having had at least 3 heart attacks, 2 strokes, chronic diabetes
and partial liver and kidney failure for some years (besides being virtually immobile and
completely blind for 18 months). All in all , though it was a release, the actual demise
was sudden and unexpected and I managed to arrive too late to be with him at the end.
Given the time constraint , this "final" revision is not as considered as it might have
been , but we have tried to take into account all comments available , and have given
considerable attention to the IPCC terminology and emphasis on the bullet points . At this
stage , however, there are some clear areas where future work will be required to keep
abreast of recent developments and , perhaps, to re-balance the emphasis and structure. I
apologise for not having responded directly to Fortunat, Stefan, Ricardo.Olga, David and
Tom, but please be aware that I have considered all of their comments and done what I could
to address them .Thanks Fortunat and Ricardo (and Ed - who should be added to the list of
CAs) for the text and Figures and Henry and Jason for the help and data . David's
suggestions about re-ordering the paragraphs was particularly difficult to resolve in my
own mind , because I do see the logic , but equally , did not want to interfere with the
time line approach to describing post- TAR work that underlies the current structure. as
you can see I decided to leave the order as it was. It would be great if David and Fortunat
could check cross Chapter referencing (eg in relation to forcings and detection chapters).
We can revisit this , and the issue of McIntyre and McKitrick (centering of PCs in Mann et
al reconstruction - which is clearly unfounded) until such time as the numerous responses
are published.
The new SH section is in , and the MWP box slightly amended to take account of the new
Figure.
Peck, I have considered your text on the regional section - and you will see that I have
edited out some relating to future (and association between drought and SSTs) . I feel
strongly that you are venturing into "observational" territory and speculation beyond what
we should say. I have also amended the bullet points to reflect this. YOU ARE THE ULTIMATE
ARBITERS and it is up to you if you wish to re-insert , but I will give you a continuing
argument later about our overstepping the "paleo" boundary. Note also that the bullet on

European summer 2004 has bee altered to reflect what was a last minute , one-sentence ,
insertion in the first paragraph regarding Jurg Lutterbacher's Science paper - as there was
no mention of it otherwise. We had to remove the reference to "700 years in France" as I am
not sure what this is , and it is not in the text anyway. The use of "likely" , "very
likely" and my additional fudge word "unusual" are all carefully chosen where used.
Tim has been a rock in the last minute rush here - not only doing the Figures , but also
helping with the text. I am really grateful to him. He has sent the text , with some
comments, and highlighted references, that need attention. If Oyvind can identify
references and handle these problems with Endnote , we are also really grateful.
The final references , if missing , are probably in the current text, the previous Endnote
library , or in sections of text sent by Ricardo, Fortunat, Peck and Eystein. I trust when
you guys have stiched the new text back in and the Figures etc. we will perhaps get a last
chance to correct and check references etc. Thanks
Keith

--
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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

580. 1123685358.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>, Peter Lemke <plemke@awi-bremerhaven.de>
Subject: Re: 3.9
Date: Wed Aug 10 10:49:18 2005

Peter, Kevin
Not having seen Ch 4, I agree that the term 'local heat budget' can be ambiguous. Are
you also discussing the issue of 'dirty' glaciers? For the Alps, the Swiss (well Wilfried
Haeberli) reckon that temperature alone cannot explain all the retreat in some recent
summers (especially 2003). Would local heat budgets include the effects of local
anthropogenic pollutants making the snow less white?
Lonnie Thompson has been on Quelccaya in the last couple of months and reports
that it is in an awful state. Like Kilimanjaro, the recent annual layers aren't
distinguishable. Lonnie reckons a lot of retreat is caused by sublimation. On Quelccaya

Lonnie and Ray Bradley have put up an AWS (on Sajama too). They've not got as much data as they hoped as both have fallen over due to melting and also the guide who helped them put one on Quelccaya later went back and brought it back down to try and sell !

I'm happy with Kevin's draft, if local heat budgets is explained in your chapter.

Cheers

Phil

At 17:29 09/08/2005, Kevin Trenberth wrote:

Peter, Thanks (sorry I can't get rid of the blue).

I am cc'ing Phil on this: Georg has suggested instead the following.

The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent with strongest recession rates in the 1930s and 1940s and after 1990 and little changes around 1970. Tropical glacier changes are synchronous with global ones, Kilimanjaro being an exception with radiatively forced constant retreat of the plateau ice. 20^th Century glacier retreats are consistent with temperature variations. Before 1900, glacier fluctuations are probably not only reflecting temperature variations but mainly precipitation anomalies. In the Tropics, glacier changes are related to atmospheric moisture variations which, in turn, correlate with sea surface temperatures in the respective source regions and varying atmospheric circulation modes. In some regions (Alaska, Patagonia, Karakoram) moderately increased accumulation is observed indicating an amplified hydrological cycle.

I am not altogether happy with this wording. In this bullet it reflects findings from your chapter and ours (wrt precip, temp, circulation etc). I would propose the following as a compromise between the old text and the proposed:

The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent in the 20th century. Tropical glacier changes in South America, Africa and Tibet are synchronous with global ones, and all have shown declines in recent decades. If continued, some may disappear within the next 30 years. Local temperature records all show a slight warming, but not of the magnitude required to explain the rapid reduction in mass of such glaciers (e.g., on Kilimanjaro), which instead depends on local heat budgets. Glaciers and ice caps respond not only to temperatures but also changes in precipitation, and before 1900, glacier fluctuations are probably not only reflecting temperature variations but mainly precipitation anomalies. In some regions moderately increased accumulation observed in recent decades is consistent with changes in atmospheric circulation and associated increases in winter precipitation (e.g., southwestern Norway, parts of coastal Alaska, Patagonia, Karakoram, and Fjordland of the South Island of New Zealand).

Note I have retained a bit more detail on the regions affected, and tried to stay away
from "radiatively forced" (whatever that means) and vague terms like "amplified
hydrological cycle". I also want to retain more specific reference to the precip and
circulation changes going together. Whether "local heat budgets" is adequate is my main
question? I gather this is related to changes in cloud and sunshine, increased heating
that goes into melting and ablation rather than temp increases. Should we spell that
out? Do you deal with that? I also did not add the detail on the dates in first
sentence as those should be in your chapter and they don't relate directly to the other
variables.
Are my terms "20th century" and "recent decades" correct?
Thanks
Kevin
Peter Lemke wrote:

Dear Kevin,
after his return from the Kilimanjaro Georg has supplied a modification to the text in
3.9 concerning the glaciers.
I have made a tiny change further down in the text replacing "order" by "approximately"
meaning 1mm/year and not implying, say, 3mm/year.
Best regards,
Peter

--
****
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References

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2. http://www.cgd.ucar.edu/cas/

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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re:
Date: Wed Aug 10 17:13:37 2005

Fine with me. Let's hope they agree by tomorrow.
Phil
At 17:11 10/08/2005, you wrote:

Ok so here is how it now reads:
The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent in the 20^th century. Tropical glacier changes in South America and Africa, and those in Tibet are synchronous with higher latitude ones, and all have shown declines in recent decades. Local temperature records all show a slight warming, but not of the magnitude required to explain the rapid reduction in mass of such glaciers (e.g., on Kilimanjaro). Glaciers and ice caps respond not only to temperatures but also changes in precipitation, and both global mean winter accumulation and summer melting have increased over the last half century in association with temperature increases. Other factors in recent ablation include changes in cloudiness and water vapour and associated radiation, and surface sensible heat exchange. Precipitation anomalies are also important before 1900 in glacier fluctuations. In some regions moderately increased accumulation observed in recent decades is consistent with changes in atmospheric circulation and associated increases in winter precipitation (e.g., southwestern Norway, parts of coastal Alaska, Patagonia, Karakoram, and Fjordland of the South Island of New Zealand) even as enhanced ablation has led to marked declines in mass balances in Alaska and Patagonia.
Kevin
Phil Jones wrote:

Sort of arguing that way. It is also the before 1900 part. Precip and temp anomalies are important at all times for glaciers. Their influence didn't change around 1900.
So what about Precipitation anomalies are also important before 1900.
I'd not got the implication. Adding also makes it clearer.
Phil
At 16:56 10/08/2005, Kevin Trenberth wrote:

Phil is arguing for changes to 4.5. Maybe the statement is too strong although it is consistent with the last para of 4.5.2.? An alternative might be: Precipitation anomalies are important before 1900. In the context this implies in addition to temperature.
Kevin
Phil Jones wrote:

Georg,
I've now also looked at the figures you sent from Ch 4. Kevin has the sentence, which Peter may have added? I reckon this is too strong. Can we omit it?
Sentence is

Before 1900, glacier fluctuations probably mainly reflect precipitation anomalies.
Reasoning
Is this a general statement. I wonder if we need it. Oerlemans uses estimated glacier termini positions (and related ELA changes) to infer past temperatures and you have his figure. I know he assumes precip to have remained essentially the same but he backs out temperature. Also glaciers in Europe advanced in the 17th and 18th centuries. It was cooler then (more so in winter than summer). I also have a paper resubmitted to JGR where Alpine precip shows no long-term changes since 1800. This uses loads of stations and is from the ALP-IMP project that ZAMG co-ordinate (Reinhard Boehm).
So the advances are caused by more precip, but the retreats by higher summer T and maybe less winter precip.

Cheers
Phil
At 16:23 10/08/2005, Kevin Trenberth wrote:

Hi Georg
Many thanks for the attachments. I had looked at the ZOD but this is much more informative. Based on your comments and the 4.5 section I have come up with the following bullet. Note that here we are writing for a general audience. I have now tried to include more clearly the factors involved. I think these are consistent with your chapter but the language in your chapter might be improved in a couple of places.
For instance an important forcing is radiation (solar and IR) which are greatly impacted by clouds, water vapor, and albedo (the dirty cover on top of snow Phil referred to), and I thought these could be brought out better in your chapter. These are perhaps more basic that temperature lapse rates and precipitation gradients which are consequences.
In 4.5.2 you use the term "radiatively forced" but it is not clear what that means. I suggest using some of these terms. Also it is not clear what "amplified hydrological cycle" means. [FYI, the expectation is for more intense precipitation, not necessarily for more total (owing to pollution effects). The former is determined by increased water vapor]. I took some of your words in the following. We need to emphasize that glaciers are not just high latitudes. I retained Kilimanjaro as that has received a lot of publicity. Some of this is necessarily abrupt, but there will be a reference to 4.5 immediately following this bullet. So the recent reversals in NZ and Norway can not be dealt with here.
Let me know if you have further suggestions. Again, many thanks
Regards
Kevin
o The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent in the 20^th century. Tropical glacier changes in South America and Africa, and those in Tibet are synchronous with higher latitude ones, and all have shown declines in recent decades. Local temperature records all

show a
slight warming, but not of the magnitude required to explain the rapid reduction in mass
of such glaciers (e.g., on Kilimanjaro). Glaciers and ice caps respond not only to
temperatures but also changes in precipitation, and both global mean winter accumulation
and summer melting have increased over the last half century in association with
temperature increases. Other factors in recent ablation include changes in cloudiness
and water vapour and associated radiation, and surface sensible heat exchange. Before
1900, glacier fluctuations probably mainly reflect precipitation anomalies. In some
regions moderately increased accumulation observed in recent decades is consistent with
changes in atmospheric circulation and associated increases in winter precipitation
(e.g., southwestern Norway, parts of coastal Alaska, Patagonia, Karakoram, and Fjordland
of the South Island of New Zealand) even as enhanced ablation has led to marked declines
in mass balances in Alaska and Patagonia.
Georg Kaser wrote:

Kevin,
Have many thanks for compiling and editing 3.9. I agree that the "radiatively forced"
and the "amplified hydrological cycle" should be removed and I also agree with Phil's
comment on the "local heat budget". In glaciology, the sum of each energy flux toward
and from the respective snow/ice surface is considered to make up the "local heat
budget". This also includes the sensible heat flux.
There are some other points in the text which I would like to comment:
1. Tropical glaciers are considered those in the South American Andes between Venezuela
and Norhern Boliva, those in East Africa and those in Irian Jaya (New Guinea). In
Chapter 4, Tibetean glaiers are taken as part of the Asian High Mountains (find the
present state Chapter 4.5. "Glaciers and Ice Caps attached).
2. Alaska, Patagonia, Karakoram, Norway and NZ cannot be merged in the respective
statement. In Alaska and Patagonia, moderately increase accumulation is accompanied by
strongly enhanced ablation making the mass balances markedly negative. From
glaciological site, no studies concerning atmospheric circulation patterns are provided
in the respective studies.
In the Karakoram mountains, enhanced accumulation has led to considerable glacier
advances, increased winter accumulation from the Westerlies is only suggested but not
subject of detailed studies. Heavy debris loads on the tongues probably prevent from
enhanced abaltion.
In Southwest Norway and NZ South Island, glaciers advances have ceded around 2000. I
don't know whether their advances shall still be mentioned in extension; I would not do

so beyond the respective statement in Ch. 4.5.
3. "If continued, some may disappear within the next 30 years." This sentence can stand for every mountain region in the world and should not be used for tropical mountains only. Everywhere, many small glaciers have disappeared since the 19th Century maxima and many will disappear soon in the Alps, the Caucasus, in the Asian High mountains etc. as well as in the Tropics. From the today's perspective Mount Kenya, all Mountains in the Rwenzori Range except Mt. Stanley, Irain Jaya will be without glaciers soon, probably sooner than Kilimanjaro; well known and studied glaciers in the Andes like Chacaltaya, Charquini and Pastoruri will also disappear soon. This is not because of a particular regional climate feature but just because they were already small when retreats started.
As you will see from Figure 4.5.5. Kilimanjaro's plateau ice is particular, slope glaciers are less. The plateau glaciers retreat from their vertical walls where no accumulation is possible and since they do so, there is no way to find an equilibrium besides disappearance. The vertical walls are a result of cold temperatures high sublimation and strong solar radiance. There is no way to replace the retreat by ice dynamics on the flat summit plateau. Slope glaciers are only partially subject of this kind of ablation and their retreat rate seems to have slowed markedly (See insert of Fig 4.5.5). If Kilimanjaro is mentioned in 3.9. it must also be added that it is a particular case with complex relation to climate change.
4. All studies which investigate tropical glacier retreat and climate show the dominance of changes in energy and mass balance terms which are related to the atmospheric moisture content rather than locally measured air temperatures. Both increased and reduced moisture can lead to negative mass balances and it has done so in most cases studied (Cordillera Blanca, Peru, Cordillera Real, Bolivia, Antisana, Ecuador, Rwenzori, Mt. Kenia, Kilimanjaro). Yet, wherever respective analyses were made, correlations were found to anomalies in ENSO or Indian Oceans Indian Ocean Dipole Mode respectively strongly indicating global warming as the principle reason of th eretreat.
I give you this lengthy explanation in order to make sure that the very compressed and condensed bullet in 3.9. gets the right content. I have started to change your paragraph suggestion accordingly but have to admit that, not being a native speaker myself, it either becomes very long or very awkward.
I also appreciate Phil's statement about Quelccaya and Sajama. Doug Hardy and Ray Bradley run AWS' there since a couple of years as well as on Kilimanjaro with all the problems of recording data at such high elevation sites. Doug is preparing a paper on

the climate records there but it has still not reached it's final state.
Information on sublimation on Quelccaya is not published such as the positive mass
balances and advances on several Andean glaciers between 1998 and 2002 are not
published. Kilimanjaro has experienced both ablation as well as accumulation layers on
the horizontal surfaces over the last years. I have just come back from fieldwork there
last week and the last half year was a mass loss year. Being very much involved into
tropical glaciers myself, I have to accept that such detailed information would be
available for several hundreds of glaciers in the world each one providing 10 or more
publications. Going into such details cannot be the aim of the report, I am afraid.
Best wishes,
Georg
Georg Kaser

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[1]http://meteo9.uibk.ac.at/IceClim/CRYO/cryo_a.html

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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: [Fwd: Storch drift]
Date: Fri, 12 Aug 2005 11:21:20 -0400
Reply-to: mann@psu.edu

Cc: Stefan Rahmstorf <rahmstorf@ozean-klima.de>, mann@psu.edu, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

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Hi Caspar,

Thanks for the comments. Frankly, Von storch is being duplicitous here. He may tell certain audiences (like the NCAR group last month) that he is not suggesting that the GKSS simulation is reealistic, because he knows he'll get skewered if he claims othewise. But then he turns around to the press, and talks about how the Moberg et al reconstruction matches their model, etc. I frankly consider this dishonest, at best!

If what Stefan says is true (that the entire long-term trend, including the cold LIA in the model, is all due to the spinup problem), then it completely invalidates the use of that model for testing statistical reconstruction methodologies which require physically-consistent patterns of variance in the calibration period to reconstruct the past. But that's a separate issue.

As we now know, the far more damning fact is that Von Storch et al knowingly applied a procedure which is not the MBH98 procedure, and they think they can get away w/ admitting this now in some obscure Italian journal which isn't even in the ISI database.   Tim/Phil/Keith: you may not know about the latter, but Caspar should be able to fill you in on this shortly...

Meanwhile, lets enjoy the media fiesta on MSU...

Mike

Caspar Ammann wrote:

> Stefan,
>
> this is very important news indeed. The runs will get a huge hit from
> this. The only way a coupled model can get a continued trend (without
> invoking an energy leak somewhere) is when there is a terrible
> deep-ocean spin up available even for their present day
> initialization, not to speak about the subsequent shock to
> pre-industrial conditions. Did you really say 1.5 degrees? Wow, that
> is quite a bit. Seems to me they must have used Levitus ocean data
> with an atmospheric restart file, then hit it with the solar/GHG
> changes. It seems rather large of a drop to come from a fully coupled
> stage. 1.5 degrees is about 30% too large to be exclusively from the
> atmospheric composition and solar irradiance, thus my suspicion
> regarding levitus. Now it would be important to know what happend
> because some people are using the run as a possible real-world
> scenario (although Hans in talks does not claim so).
>
> Caspar
>
> PS Now, bare in mind that the Science paper applies to the
> reconstruction, and for the general discussion the influence of spinup
> should not make that big of a difference (other than inflating the
> difference of the coldest period to the calibration period, which
> creates some issues discussed by Mike previously).
>
>
>
> Michael E. Mann wrote:
>

>>
>>
>> ---
>>
>> Subject:
>> Storch drift
>> From:
>> Stefan Rahmstorf <rahmstorf@ozean-klima.de>
>> Date:
>> Thu, 11 Aug 2005 15:37:27 +0200
>> To:
>> mann@psu.edu
>>
>> To:
>> mann@psu.edu
>> CC:
>> Gavin Schmidt <gschmidt@giss.nasa.gov>, Keith Briffa
>> <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
>>
>>
>> Hi Mike,
>>
>> here is some interesting new info on the drift problem in the VS04
>> runs. Irina Fast and Gerd Bürger submitted a comment about this to
>> Science some months ago; it was rejected and they did not pursue it.
>> I'm trying to encourage them to resubmit this elsewhere. I do not
>> have the ms. but have seen several graphs. There are two key points.
>>
>> 1. The ECHO-G run started at year 900, the VS04 paper of course shows
>> only results starting from year 1000. I've seen the full run now.
>> Between 900 and 1000, the NH temperature drops by about 1.5 ºC!
>> That's how severe their initialisation problem is. From my experience
>> of how the THC responds after such step-function changes in forcing,
>> the strong warming from 1050-1150 in VS04 could well be a rebound
>> effect from the 1.5 ºC cooling that precedes it, since the THC tends
>> to oscillate on such a time scale when forced rapidly.
>>
>> 2. Irina has run ECHO-G initialised with modern climate and then
>> switching to pre-industrial conditions similar to the run shown by
>> VS04, but without any further variability in the forcing. Thus, this
>> shows the pure drift from initialising this run - this is what Tim
>> has been estimating in MAGICC. The actual drift in ECHO-G is even
>> larger and more persistent than what Tim found: there is a cooling
>> between the years 1000 and 2000 of over 0.6 ºC, and this is an almost
>> linear trend over the whole time. I.e., not just drifting during the
>> first few centuries, but over the entire 1000-year period.
>>
>> Cheers, Stefan
>>
>

--
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Director, Earth System Science Center (ESSC)

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From: Phil Jones <p.jones@uea.ac.uk>
To: mann@psu.edu
Subject: Re: [Fwd: Storch drift]
Date: Fri Aug 12 17:18:22 2005

Mike,
Yes it was him !
Phil
At 17:17 12/08/2005, you wrote:

Hi Phil,
Yeah--I've been told that one of the co-authors of the chapter (w/ the initials D.R.)
has behaved poorly. Fortunately, w/ Peck, Stefan R., and Keith all authors on the
chapter, it sounds as if the voices of reason are prevailing...
mike
Phil Jones wrote:

OK. Keith is also away next week. He's
already gone.
He'll need to look more at all this before the
next IPCC meeting in December.
You should have seen some of the crap
comments he got. Not yours, but some
of the other authors on the paleo chapter.
People who you think ought to know
better. Most relating to MM. All mostly
ignored. You'll be able to register to get
the draft by early Sept.
Cheers
Phil
At 16:49 12/08/2005, you wrote:

Thanks Phil,
Can you tell Keith (confidentially) that Ammann and Wahl are submitting a comment to
Science pointing out that von Storch knowingly did not apply the MBH98 procedure, and
that all of the conclusions in that paper are wrong! There may be calls on Science to
retract VS04, because the mistake undermines every single conclusion!!
mike
Phil Jones wrote:

Mike,
We have the Italian paper Well Keith does for his AR4 work.
Submission day for AR4 is today by the way.
I think the Italian journal is the one from a conf I went to
3 weeks after the Berne meeting. I didn't bother sending
anything to the Italian meeting either, just like Berne. The
journal the Italians were planning did look obscure when
I was there, but I didn't write anything down, as I had
no intention of sending anything.

Yes the MSU stuff is out. There will be something in Nature next week on it.
Off next week as a break from IPCC.
Cheers
Phil

At 16:21 12/08/2005, you wrote:

Hi Caspar,
Thanks for the comments. Frankly, Von storch is being duplicitous here. He may tell certain audiences (like the NCAR group last month) that he is not suggesting that the GKSS simulation is reealistic, because he knows he'll get skewered if he claims othewise. But then he turns around to the press, and talks about how the Moberg et al reconstruction matches their model, etc. I frankly consider this dishonest, at best!
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As we now know, the far more damning fact is that Von Storch et al knowingly applied a procedure which is not the MBH98 procedure, and they think they can get away w/ admitting this now in some obscure Italian journal which isn't even in the ISI database.
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From:
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Date:
Thu, 11 Aug 2005 15:37:27 +0200
To:
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To:
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CC:
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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

584. 1124742148.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>, wigley@ucar.edu
Subject: Last week's events

Date: Mon Aug 22 16:22:28 2005

Ben and Tom,

Congratulations on the paper coming out on Aug 12.
I did talk to Nature about the three papers.

Last week seems to have been a good one to have had off.
I did this because of the IPCC submission deadline of Aug 12.
As you said Tom, there were some stupid messages going
around. If only these people would try and write peer-review
papers, provided they get proper reviews. The one from
Sonia should be kept as it proves that E&E is not a
proper journal.
I almost missed the one with Pielke's resignation in. Is this
going to make your CCSP task easier or harder? Presumably
now you'll get all his comments to officially deal with. Maybe
you'll be able to ignore them?
Cheers
Phil

Prof. Phil Jones
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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK
---

585. 1124994521.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@psu.edu,Christoph Kull <christoph.kull@pages.unibe.ch>
Subject: Re: PAGES/CLIVAR workshop
Date: Thu, 25 Aug 2005 14:28:41 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>,"Michael E. Mann" <mann@virginia.edu>, Heinz Wanner <wanner@giub.unibe.ch>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>

<x-flowed>

Christoph,
It also looks OK to me. The bit highlighted in blue, should probably say
something like ...identify the key issues.

I agree with Mike that the last two names on the list should be removed.

I have sent an email about the 4th meeting of IPCC, which I
think is June 26-30, 2006. Just checking it is still that week, so
there won't be a clash.

Cheers
Phil

At 13:40 25/08/2005, Michael E. Mann wrote:
>Dear Christoph,
>
>Looks pretty good to me. Only one issue. In our discussion of possible
>participants in Bern, I think (someone correct me if I'm wrong) we

>concluded that the last two on the list (w/ question marks) would be
>unwise choices because they are likely to cause conflict than to
>contribute to concensus and progress. A preferred alternative who was
>mentioned was Simon Tett (though, it was pointed out, he may not be able
>to participate for other reasons). We also noted that both Keith B. and
>Tim. O are in the same European project as the two individuals in
>question, and could adequately (better, in my opinion) represent any
>contributions to the discussion from that project.
>
>mike
>
>Christoph Kull wrote:
>
>>Dear Phil, Keith, Mike and Heinz,
>>After dealing with the PAGES OSM the past weeks I made an attempt to
>>finalize our "Past Millennia Workshop Concept" in order to contact CLIVAR as
>>soon as possible for requesting support.
>>I incorporated your comments and suggestions in a balanced way and hope that
>>finally all of you may agree to the presented attached draft.
>>
>>Please get back to me with final remarks by Monday next week. I will
>>afterwards contact the CLIVAR office.
>>
>>All the best, thanks a lot for your cooperation and help!
>>Looking forward setting up a hopefully successful project.
>>Christoph
>
>
>--
>Michael E. Mann
>Associate Professor
>Director, Earth System Science Center (ESSC)
>
>Department of Meteorology   Phone: (814) 863-4075
>503 Walker Building   FAX: (814) 865-3663
>The Pennsylvania State University   email: mann@psu.edu
>University Park, PA 16802-5013
>
>http://www.evsc.virginia.edu/faculty/people/mann.shtml
>

Prof. Phil Jones
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School of Environmental Sciences   Fax +44 (0) 1603 507784
University of East Anglia
Norwich   Email   p.jones@uea.ac.uk
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586. 1125067952.txt
##
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: PAGES/CLIVAR workshop
Date: Fri, 26 Aug 2005 10:52:32 -0400
Reply-to: mann@psu.edu

Cc: Heinz Wanner <wanner@giub.unibe.ch>, Christoph Kull <christoph.kull@pages.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>

Dear Phil et al,
I agree on Mike Evans. I'm afraid I don't agree on Zorita. He has engaged in some very
nasty, and in my opinion unprofessional email exchanges with some close colleagues of mine
who have established some fundamental undisclosed errors in work he co-published with von
Storch. Given this, I don't believe he can be involved in constructive dialogue of the
sort we're looking for at this workshop. There are some similarly problematic issues w/
Cubasch, who like von Storch, who has engaged in inflammatory and ad hominem public
commentary. There is no room for that on any side of the debate.
If the Germans need to be represented here, I would suggest instead someone from the
Potsdam group, such as Eva Bauer, who has been doing some very interesting work on
modelling the climate of the past 2K,
mike
Phil Jones wrote:

> Christoph,
> I have checked with IPCC and their 4th meeting is in the June 26-30
> week in Bergen..
> As for Heinz's suggestions
> - Mike Evans would be OK
> - I'm nor sure that Mikami would contribute much
> See Keith's comment on Zorita
> Cheers
> Phil
> At 14:39 26/08/2005, Heinz Wanner wrote:
>
> Dear Christoph,
>
> I have only a few additional comments concerning the planned workshop.
>
> First of all, I support this concept. Related to the topics, I heavily support to
> organize a discussion about how we can reconstruct different paremeters independently.
> It is important to try to reconstruct air pressure as a basic circulation parameter - if
> possible.
>
> Concerning the participants:
> - Write GooSSe;
> - Mikami from Japan (Tokyo Metropolitan University) could be an interesting Asian
> participant;
> - You mentioned Kevin Trenberth or Mark Cane. Both are absolutely okay, but why not
> invite a younger colleague like Mike Evans from Tucson?
> - If Phil and Mike do not support von Storch it does not make sense to invite him (and
> Eduardo Zorita?);
> - For me Ulrich Cubasch is an interesting modeler with good ideas about paleomodeling.
> Maybe Gavin can comment this when he is back from his China trip?

Cheers, Heinz

Dr. Heinz Wanner
Prof., Director NCCR Climate

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Phone +41 (0)31 631 88 85
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Erlachstrasse 9a
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Phone +41 (0)31 631 31 60
Fax +41 (0)31 631 43 38
[2]www.nccr-climate.unibe.ch

[3]wanner@giub.unibe.ch

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email: [5]mann@psu.edu

[6]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.giub.unibe.ch/klimet/
2. http://www.nccr-climate.unibe.ch/
3. mailto:wanner@giub.unibe.ch
4. mailto:p.jones@uea.ac.uk
5. mailto:mann@psu.edu
6. http://www.evsc.virginia.edu/faculty/people/mann.shtml

587. 1125085162.txt
##

From: "Heinz Wanner" <wanner@giub.unibe.ch>
To: "Christoph Kull" <christoph.kull@pages.unibe.ch>
Subject: PAGES/CLIVAR workshop
Date: Fri, 26 Aug 2005 15:39:22 +0200
Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Keith Briffa" <k.briffa@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, "Thorsten Kiefer" <thorsten.kiefer@pages.unibe.ch>

Dear Christoph,

I have only a few additional comments concerning the planned workshop.

First of all, I support this concept. Related to the topics, I heavily support to organize a discussion about how we can reconstruct different paremeters independently. It is important to try to reconstruct air pressure as a basic circulation parameter - if possible.

Concerning the participants:

- Write GooSSe;

- Mikami from Japan (Tokyo Metropolitan University) could be an interesting Asian participant;

- You mentioned Kevin Trenberth or Mark Cane. Both are absolutely okay, but why not invite a younger colleague like Mike Evans from Tucson?

- If Phil and Mike do not support von Storch it does not make sense to invite him (and Eduardo Zorita?);

- For me Ulrich Cubasch is an interesting modeler with good ideas about paleomodeling. Maybe Gavin can comment this when he is back from his China trip?

Cheers, Heinz

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Prof., Director NCCR Climate

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[2]www.nccr-climate.unibe.ch

[3]wanner@giub.unibe.ch

References

1. http://www.giub.unibe.ch/klimet/
2. http://www.nccr-climate.unibe.ch/
3. mailto:wanner@giub.unibe.ch

588. 1127491287.txt

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.m.melvin@uea.ac.uk
Subject: Polar Urals
Date: Fri Sep 23 12:01:27 2005

Tom,
Can you crossdate these two series (trw and mxd) for the Polar Urals?
Particularly check the 1032 value when only 3 samples.
Found this on the blogg site that Tim sent round. Whatever you do,
don't respond on the blogg.
Cheers
Phil and Keith

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784
[1]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

589. 1127614205.txt
##

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>
Subject: Re: Fwd: Re: [Fwd: Re: Chapter 6 - Submitted Papers]
Date: Sat, 24 Sep 2005 22:10:05 -0600
Cc: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>

Hi all - let's see what Keith/Tim say about both papers. Eystein - can you call them on
Monday if we haven't heard from them. If they don't have one or both of the papers, then we
should ask Martin to delete from the chapter - Eystein, feel free to do this as soon as you
get feedback from Keith/Tim. Mysterious...

Thanks, Peck

> Date: Fri, 23 Sep 2005 13:14:19 +0200
> To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
> From: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>
> Subject: Re: [Fwd: Re: Chapter 6 - Submitted Papers]
> Cc:
> Bcc:
> X-Attachments:
>
> eystein-peck,
>
> I think we agreed that the Wilson paper should be deleted, but i don't know why its
> still in there. The Briffa paper is new to me (i think).
>
> Cheers,
>
> Øyvind
>
> Hi Keith,see correspondance below. Just to make sure. is the Briffa et al. paper
> submitted, or should it be deleted from the FOD? The ref to the Wilson et al. paper I
> assume comes from Peck/Julie, who can handle the issue. Right, Peck?
>
> Eystein
>
> Envelope-to: eystein.jansen@geo.uib.no
> Date: Thu, 22 Sep 2005 18:05:33 -0600
> To: eystein.jansen@geo.uib.no, jto@u.arizona.edu
> From: Martin Manning <mmanning@al.noaa.gov>
> Subject: [Fwd: Re: Chapter 6 - Submitted Papers]
> Cc: ssolomon@al.noaa.gov, ipcc-wg1@al.noaa.gov
> X-checked-clean: by exiscan on alf
> X-UiB-SpamFlag: NO UIB: 1.8 hits, 8.0 required
> X-UiB-SpamReport: spamassassin found;
> 0.8 BODY: Contains 'Dear (something)'
> 1.0 BODY: Claims you can be removed from the list
> 0.1 BODY: Message is 30% to 40% HTML
> 0.0 BODY: HTML included in message
>
> Dear Eystein and Peck

Following the release of the first draft of the WG1-AR4 we have had a response from
Steve McIntyre (a name that should ring a bell) regarding unpublished literature in
Chapter 6. He also asks about access to data sets but that is not an IPCC function so is
easily dealt with.
The unpublished papers that he has picked up as not being available are:
Briffa, K.R., T.M. Melvin, V.V. Shishov, and et. al, 2005: Warm season temperatures
across northern Eurasia: a 2000-year tree-ring based study. Quaternary Science
Reviews(In preparation).
and
Wilson and al. 2005 (mentioned on page 6-31)
The first of these was I think meant to be deleted from the text here and we may have
made an error in missing that. The second is cited but does not appear in the reference
list so we did not pick it up as an unpublished paper that needed to be collected.
Could you please let me know:
1) are drafts for either of these papers available yet and if so can you send copies to
the TSU?
2) how do you expect to use these references in the second draft - remembering that we
can only use papers that are in press at that time and that the Briffa et al paper is
used quite a bit - e.g. on page 6-29.
I am attaching the correspondence with McIntyre below for your information but the only
issues you need to consider are those above, and we will handle any further interactions
with McIntyre from here.
Thanks
Martin

Date: Tue, 20 Sep 2005 16:42:00 -0600
From: IPCC-WG1 <ipcc-wg1@al.noaa.gov>
User-Agent: Mozilla/5.0 (Macintosh; U; PPC Mac OS X Mach-O; en-US; rv:1.4) Gecko/20030624 Netscape/7.1
X-Accept-Language: en-us, en
To: martin Manning <mmanning@al.noaa.gov>
Subject: [Fwd: Re: Chapter 6 - Submitted Papers]
X-Rcpt-To: <mmanning@aztec.al.noaa.gov>
X-DPOP: Version number supressed
-------- Original Message --------
Subject: Re: Chapter 6 - Submitted Papers
Date: Tue, 20 Sep 2005 13:30:52 -0400
From: Steve McIntyre [1]<stephen.mcintyre@utoronto.ca>
To: IPCC-WG1 [2]<ipcc-wg1@al.noaa.gov>
References: <026101c5bd56$fbafb280$6402a8c0@herbert> [3]<432F2687.3030101@al.noaa.gov>
<029101c5bd95$4d2ae240$6402a8c0@herbert> [4]<43303CC7.7080401@al.noaa.gov>
It's possible that the references were inadvertently left in, in which case your
suggestion that a comment be pointed on the review form would obviously suffice.
However, it's equally possible that the authors intend to use these references and they
inadvertently failed to post them up on the website. If the latter, then they should ask
the authors to post up the references. Could you verify which applies with the

authors
and, if the latter, take appropriate steps.

Additionally, I have attempted to locate van Ommen, Annals of Glaciology, 39, mentioned
in the same section. Can you confirm that this volume has either been printed or made
available electronically (as I am presently unable to locate wither). If not, then this
should be made available in a pdf form at the website.

I have been unable to locate supplementary information or data archives for several of
the articles posted at the pdf location for Chapter 6 and would appreciate assistance in
this regard.
1) Hegerl et al, submitted. Can you provide me with an ftp location for the proxy data
used in this study (which does not even list the proxies used) or post it at your
website.
2) D'Arrigo et al, submitted. Again, this data has not been archived at WDCP. Can you
provide me with an ftp location for the proxy data used in this study or post it at your
website.

Similarly, the SI to Rutherford et al, 2005 does not contain the Briffa et al. data set.
Again can you provide an ftp location for this dataset or otherwise provide it.

Thank you for your attention, Steve McIntyre

----- Original Message -----
From: [5]IPCC-WG1
To: [6]Steve McIntyre
Sent: Tuesday, September 20, 2005 12:45 PM
Subject: Re: Chapter 6 - Submitted Papers
Dear Dr McIntyre,
It would seem that the authors may have inadvertently left in the Wilson et al. and
Briffa et al. citations, as I do not have copies of the preprints for either. I
apologize for the discrepancy and have made note of this error for the authors for
correction of the next draft, but if you would also like to comment on this in your
review, please do so.
Best regards,
Melinda Tignor
Steve McIntyre wrote:

Thanks for the directions. I found 5 of them there. I was still unable to locate Briffa
et al, 2005 (QSR in prep) or Wilson and al. 2005 (mentioned on page 6-31). Could you
take a look for them. Thanks.

----- Original Message -----

From: [7]IPCC-WG1
To: [8]Steve McIntyre
Sent: Monday, September 19, 2005 4:58 PM
Subject: Re: Chapter 6 - Submitted Papers
Dear Mr McIntyre,
As mentioned on the Reviewer website, copies of unpublished literature may be downloaded
for your review. Papers for each chapter are found at the same website:
URL: [9]http://ipcc-wg1.ucar.edu/restricted/review/FOR/
UserName: WG1-FOR
Password: 2005Nov04
Once inside the site, click on "Download Draft Chapters" on the left side of the page.
From here, scroll down the page where you downloaded the chapter to the last sentence,
"If you wish to see copies of unpublished papers cited in the draft chapters click here"
- click "here" and you will be taken to another page listing all the chapters with
unpublished literature and from clicking on the individual chapter you will be taken to
the list of unpublished literature.
Please let me know if I can be of further assistance.
Best regards,
Melinda Tignor
WGI TSU
Steve McIntyre wrote:

Dear Sirs,
The covering literature indicated that the website would provide access to submitted,
in-press, or otherwise unpublished papers and reports that are cited in the draft WG I
report. In connection with Chapter 6, I was unable to locate the following:
Briffa et al, 2005. (presumably this is the paper denoted as in prep. in the bibliography)
DArrigo et al, submitted
Hegerl et al, submitted
Smerdon et al 2005. JGR (in review)
Tett et al, submitted. Clim. Dyn. submitted.
Wahl and Ammann 2004. (in review)
Wilson and al, 2005 (referred to on page 6-31, but not in bibliography.

Could you please post these on the website or email me pdf's. For these unpublished
articles, could you also provide locations of FTP sites where the underlying data may be
reviewed.
Thank you for your attention,
Stephen McIntyre

--
~~~~~~
IPCC WGI TSU
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Boulder, CO 80305, USA
Phone: +1 303 497 7072
Fax: +1 303 497 5686/5628
~~~~~~

Email: [10]ipcc-wg1@al.noaa.gov

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~~~~~~
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Fax: +1 303 497 5686/5628
Email: [12]ipcc-wg1@al.noaa.gov

--
Recommended Email address: mmanning@al.noaa.gov
** Please note that problems may occur with my @noaa.gov address
Dr Martin R Manning, Director, IPCC WG I Support Unit
NOAA Aeronomy Laboratory Phone: +1 303 497 4479
325 Broadway, DSRC R/AL8 Fax: +1 303 497 5628
Boulder, CO 80305, USA

--

_____
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Fax: +47-55-584330

--

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--

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http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/

References

1. mailto:stephen.mcintyre@utoronto.ca
2. mailto:ipcc-wg1@al.noaa.gov
3. mailto:432F2687.3030101@al.noaa.gov
4. mailto:43303CC7.7080401@al.noaa.gov
5. mailto:ipcc-wg1@al.noaa.gov
6. mailto:stephen.mcintyre@utoronto.ca
7. mailto:ipcc-wg1@al.noaa.gov
8. mailto:stephen.mcintyre@utoronto.ca
9. http://ipcc-wg1.ucar.edu/restricted/review/FOR/
10. mailto:ipcc-wg1@al.noaa.gov
11. mailto:ipcc-wg1@al.noaa.gov
12. mailto:ipcc-wg1@al.noaa.gov

590. 1128000000.txt
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Phil Jones <p.jones@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>,
Jonathan Overpeck <jto@u.arizona.edu>
Subject: McIntyre and D'Arrigo et al (submitted)
Date: Thu, 29 Sep 2005 09:20:00 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
Dear Phil, Eystein and Peck,

I've already talked about this to Phil and Keith, but for Eystein's
and Peck's benefit the emails copied below relate to McIntyre
downloading a PDF of a manuscript cited by the IPCC paleo chapter and
then apparently trying to interfere with the editorial process that
the paper is currently going through at JGR.

I think this is an abuse of McIntyre's position as an IPCC reviewer.

Rosanne replied to my email below, to say that they *do* want this
taken further. So...

Phil has agreed to forward these messages to Susan Solomon and Michael Manning.

Eystein and Peck: do you want to add anything too?

Cheers

Tim

>Date: Wed, 28 Sep 2005 09:08:22 +0100
>To: "Rob Wilson" <rob.wilson@ed.ac.uk>, "Rosanne D'Arrigo"
><druidrd@ldeo.columbia.edu>
>From: Tim Osborn <t.osborn@uea.ac.uk>
>Subject: Re: Fw: D'Arrigo et al, submitted
>Cc: <K.briffa@uea.ac.uk>
>
>Dear Rob and Rosanne,
>
>I strongly agree that this is an abuse of his position as IPCC
>reviewer! The data archiving issues are a separate issue because I
>think there's no need for the data you used to be publicly available
>until the paper is actually published, and I would hope that the
>editor would respond appropriately. But the other comments could
>clearly influence the editorial/review process and this is very
>unfair when your paper has already been reviewed by
>others. McIntyre could of course submit a comment after your paper
>was published if he wished to criticize certain aspects, and that is
>the route he should have followed. He tried to stop publication of
>a paper that I was a co-author on, Rutherford et al. (2005), by
>contacting the editor of J. Climate with various criticisms -
>fortunately the editor told him firmly that the route to take was to
>submit a comment after publication. However, in our case the paper
>was already in press. In your case, with the editor's decision
>still to be made, there is clearly more scope for McIntyre to
>influence the decision in your case - and this certainly should not happen.
>
>The conditions which McIntyre (and all other IPCC reviewers) agreed
>to before downloading your manuscript were:
>
>"This site also provides access to copies of some submitted,
>in-press, or otherwise unpublished papers and reports that are cited
>in the draft WG I report. All such material is made available only
>to support the review of the IPCC drafts. These works are not
>themselves subject to the IPCC review process and are not to be
>distributed, quoted or cited without prior permission from their
>original authors in each instance."
>
>I don't think that contacting the journal editor with criticisms is
>"only to support the review of the IPCC drafts".
>
>I will take this issue up with the chapter lead authors and the WG1
>technical support unit - unless you prefer that I didn't. Please let me know.
>
>Cheers
>
>Tim
>
>At 08:33 28/09/2005, Rob Wilson wrote:
>>Hi Tim and Keith,
>>please see the e-mail (below) from Steve Macintyre to the Editor of JGR.

>>
>>This seems a major abuse of his position as reviewer for IPCC?
>>
>>In some respects, I don't mind having to address his comments (many
>>of which are already adequately explained I think, although a
>>detailed list of all data used could certainly go in an
>>appendix), but this just seems a bit off. After all, we have
>>addressed the reviewers comments and are currently awaiting a
>>decision. This e-mail may effect the decision greatly.
>>
>>Is he going to do this for all papers he does not quite agree with.
>>
>>comments?
>>
>>Rob
>>
>>----------
>>
>>
>>>From: "Steve McIntyre"
>>><<mailto:stephen.mcintyre@utoronto.ca>stephen.mcintyre@utoronto.ca>
>>>To: "Colin O'Dowd" <<mailto:jgr@nuigalway.ie>jgr@nuigalway.ie>
>>>Cc: "Rob Wilson"
>>><<mailto:rjwilson_dendro@blueyonder.co.uk>rjwilson_dendro@blueyonder.co.uk>,
>>>   "Rosanne D'Arrigo"
>>> <<mailto:druidrd@ldeo.columbia.edu>druidrd@ldeo.columbia.edu>
>>>Subject: D'Arrigo et al, submitted
>>>Date: Tue, 27 Sep 2005 10:37:06 -0400
>>>Dear Dr O'Dowd,
>>>I am a reviewer for the IPCC Fourth Assessment Report (IPCC 4AR)
>>>and am writing in respect to a submission to your journal by
>>>D'Arrigo et al., entitled "On the Long-Term Context for Late 20th
>>>Century Warming." This article was referenced in chapter 6 of the
>>>Draft IPCC 4AR and made available to IPCC reviewers. In the course
>>>of my review, I contacted the senior author, Dr. D'Arrigo, for the
>>>FTP location of the data used in this article or for alternative
>>>access to the data. Dr D'Arrigo categorically refused and I was
>>>referred to the journal editor if I desired recourse.
>>>
>>>
>>>Data Citation and Archiving
>>>I point out that AGU policies for data citation and data archiving
>>>(<http://www.agu.org/pubs/data_policy.html>http://www.agu.org/pubs/data_policy.ht
ml
>>>) specifically require that authors provide data citation
>>>according to AGU standards and require that contributors archive
>>>data in permanent archives, such as the World Data Center for
>>>Paleoclimatology. For example, the policy states:
>>>
>>>
>>>1. Data sets cited in AGU publications must meet the same type of
>>>standards for public access and long-term availability as are
>>>applied to citations to the scientific literature. Thus data cited
>>>in AGU publications must be permanently archived in a data center …
>>>2. Data sets that are available only from the author, through
>>>miscellaneous public network services, or academic, government or
>>>commercial institutions not chartered specifically for archiving
>>>data, may not be cited in AGU publications.
>>>
>>>
>>>On page 21 of D'Arrigo et al., there is a listing of "regional
>>>groupings" of data. In some cases, part of the data is archived at
>>>WDCP; in other cases, the data has been collected by the authors,

>>>but has not been archived.
>>>
>>>
>>>In cases, where the data has been archived, it has not been cited
>>>according to AGU policies. For example, the Torntraesk site is
>>>presumably swed019w, but this is not stated. The Polar Urals site
>>>appears to be a combination of russ021w, russ176w and russ022w,
>>>but this is not stated. The Quebec site appears to be a version of
>>>cana036, but a version that differs from the one archived, as it
>>>includes more series. The "Mongolia" site appears to be the
>>>authors' mong003 site, but a different version than the one
>>>archived (which commences at a different date). The "Yukon" series
>>>is a combination of two sites, which are not stated. At least one
>>>of the sites is a different version from the one archived. The
>>>Icefields site is again a different version than the one archived.
>>>Other data sets e.g. Seward, NW North America, Central Alaska,
>>>Wrangells, Coast Alaska, Central NWT, Southern Alaska, have been
>>>collected by the authors and are either not archived at all or
>>>archived in obsolete versions.
>>>
>>>
>>>In order that this submission comply with AGU policies on data
>>>archiving, I request that you require D'Arrigo et al. do (1)
>>>provide accurate data citations complying with AGU policies for
>>>all data sets presently archived at WDCP; (2) archive all "grey"
>>>data used in the article.
>>>
>>>
>>>Methodology
>>>The results of this article depend on methodological details,
>>>especially as to standardization procedures. However, these
>>>procedures are not described in objective or operational terms. I
>>>will illustrate some examples below:
>>> Page 21 - "In select cases, a power transform (PT) was applied
>>> to correct for data biases. This bias was assessed by correlation
>>> and residual analysis against both local and large scale
>>> temperature series." In which cases was PT applied and what were
>>> the objective criteria in the correlation and residual analysis,
>>> which were used to determine whether this should be applied.
>>>
>>>
>>>
>>>Page 21 - "Due to differing populations in the TR data, the
>>>data-sets were often grouped into 'common' populations. No one
>>>strategy is appropriate for all data-sets and careful evaluation
>>>of each composite data-set was made." That's nice, but what were
>>>the operational criteria which were used to allocate each case to
>>>the 5 different alternative procedures.
>>>
>>>
>>>Page 7 - "The standard error of the regression estimate (standard
>>>deviation of the regression residuals) from the full period
>>>calibration was used to generate the 2 sigma error bars and this
>>>was also adjusted (inflated) to account for the change (decrease)
>>>in explained variance in each nest." - The last adjustment is not
>>>described in operational terms. Shouldn't the standard error be
>>>realistically measured by the standard deviation from the
>>>verification period residuals?
>>>
>>>
>>>Page 20. "Successful modeling of paleoclimate data with the high
>>>temperatures of the late 1990s is essential if we are to make
>>>robust, definitive conclusions about past temperature amplitudes

>>>and variability." Abstract - "presently-available paleoclimatic
>>>reconstructions are inadequate for making specific inferences, at
>>>hemispheric scales, about MWP warmth relative to the anthropogenic
>>>period and that such comparisons can only still be made at the
>>>local/regional scale." Page 13. "After this period [mid-1980s],
>>>the divergence between the tree-ring and instrumental data results
>>>in weakening of calibration results and failed verification
>>>statistics". The authors contradict these caveats by proceeding to
>>>make a variety of inferences and claims "at hemispheric scales"
>>>about MWP warmth or lack thereof relative to the modern period. A
>>>comparison of their reconstruction to instrumental temperatures is
>>>prominently made in the Abstract, on page 10 and page 14. If the
>>>reconstructions are inadequate for making these inferences, then
>>>don't make them.
>>>
>>>
>>>Thank you for your consideration,
>>>
>>>
>>>Yours truly,
>>>Stephen McIntyre
>>>

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sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm

</x-flowed>

591. 1132094873.txt
##

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@psu.edu, Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: heads up...
Date: Tue Nov 15 17:47:53 2005
Cc: Phil Jones <p.jones@uea.ac.uk>

   Mike
   thanks for this. When time allows we will do a response to this poster and simply post it
   on our web page. As others have said , the dating of the chronology in the Urals is not
   wrong - but the magnitude of the extreme years in the early Urals reconstruction were not
   adjusted to account for inflated variance related to low chronology replication - so they
   are sort of right that the emphasis on 1032 is probably overdone.
   Anyway thanks again
   Keith
   At 15:29 15/11/2005, Michael E. Mann wrote:

     Thanks Tim, Phil
     yes, I never had any doubt he's wrong. In fact he's been wrong about just about

every
claim he's ever made. He almost had a point w/ the PCA centering, but as we all know,
that doesn't matter at all in the end. The issue isn't whether or not he's right, as we
all well know by now, but whether his false assertions have enough superficial
plausability to get traction. In this case, they might, so probably good to at least be
prepared.
I was told by a journalist Paul Thacker that his poster got prominent placement,
probably not an accident (see forwarded email). I believe that Mike Schlesinger and
David Karoly were there in the same session, so might be worth checking w/ them. I think
Connie Woodhouse and Tom Wigley were also at the meeting, but not sure...
I suspect that this is the first in a line of attacks (I'm sure Tom C is next in line)
that will ultimately get "published" one way or another. The GRL leak may have been
plugged up now w/ new editorial leadership there, but these guys always have "Climate
Research" and "Energy and Environment", and will go there if necessary.
They are telegraphing quite clearly where they are going w/ all of this...
Mike
Tim Osborn wrote:

Thanks for this Mike. We'd spotted an earlier draft of his poster and were a bit
concerned about this receiving prominence at the meeting.
Did it arouse much discussion, do you know? Keith and Tom Melvin looked into the dating
a while back when McIntyre first raised it and were quite satisfied with the published
dating I think. Not sure what should be done - unless he submits something for
peer-review. Cheers, Tim
At 14:53 15/11/2005, Michael E. Mann wrote:

not sure if you guys are aware, McIntyre presented this poster at the CCSP meeting.
Apparently, they gave him a very prominent location, so that everyone entering the
meeting would have seen the poster...
mike
can find at:

<[1]http://www.climatescience.gov/workshop2005/abstracts/p-gc-1.htm>http://www.climatesc
ience.gov/workshop2005/abstracts/p-gc-1.htm
P-GC1.4
More on Hockey Sticks: The Case of Jones et al. [1998]
Stephen McIntyre,
<[2]mailto:stephen.mcintyre@utoronto.ca>stephen.mcintyre@utoronto.ca
Multiproxy studies purporting to show 20th century uniqueness have been applied by
policymakers, but they have received remarkably little independent critical analysis.
Jones et al. [1998] is a prominent multi-proxy study used by IPCC [2001] and others to
affirm the hockey stick shaped temperature reconstruction of Mann et al. [1998].
However, the reconstruction of Jones et al. [1998] is based on only 3-4 proxies in the

controversial Medieval Warm Period, including non-arms-length studies by Briffa et al.
[1992] and Briffa et al [1995]. We show that the Polar Urals data set in Briffa et al
[1992] fails to meet a variety of quality control standards, both in replication and
crossdating. The conclusion of Briffa et al. [1995] that 1032 was the "coldest year" of
the millennium proves to be based on inadequate replication of only 3 tree ring cores,
of which at least 2 are almost certainly incorrectly crossdated. We show that an ad hoc
adjustment to the Tornetrask data set in Briffa et al [1992] cannot be justified. The
individual and combined impact of defects in the Polar Urals data set and Tornetrask
adjustments on the reconstruction of Jones et al [1998] is substantial and can be seen
to have the effect of modifying what would otherwise indicate a pronounced Medieval Warm
Period in the proxy reconstruction. Inhomogeneity problems in the Polar Urals and
Tornetrask data sets, pertaining to altitude, minimum girth bias and pith centering bias
will also be discussed.
--
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<[4]http://www.met.psu.edu/dept/faculty/mann.htm>[5]http://www.met.psu.edu/dept/faculty/
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web: [6]http://www.cru.uea.ac.uk/~timo/
sunclock: [7]http://www.cru.uea.ac.uk/~timo/sunclock.htm

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[8]http://www.met.psu.edu/dept/faculty/mann.htm

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[9]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.climatescience.gov/workshop2005/abstracts/p-gc-1.htm%3Ehttp://www.climatescience.gov/workshop2005/abstracts/p-gc-1.htm
2. mailto:stephen.mcintyre@utoronto.ca
3. mailto:mann@psu.edu%3Emann@psu.edu
4. http://www.met.psu.edu/dept/faculty/mann.htm
5. http://www.met.psu.edu/dept/faculty/mann.htm
6. http://www.cru.uea.ac.uk/~timo/
7. http://www.cru.uea.ac.uk/~timo/sunclock.htm
8. http://www.met.psu.edu/dept/faculty/mann.htm
9. http://www.cru.uea.ac.uk/cru/people/briffa/

592. 1133360497.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: IPCC ref. regarding McIntyre and McKitrick
Date: Wed, 30 Nov 2005 09:21:37 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

<x-flowed>
Thanks Caspar. This is good news. Please keep us posted. Best, Peck

>Hi everybody,
>
>just a quick update that I got word from the Chief Editor of GRL
>(Jay Famiglietti) that our comment in GRL about the MM paper earlier
>this year has finally been accepted. They are now soliciting a
>response from McIntyre and McKitrick, but that should now move
>rather quickly. No official word on the Climatic Change paper just
>yet.
>
>Cheers,
>Caspar
>
>PS Here the full references:
>
>Ammann C.M., and E.R. Wahl, accepted: Comment on "Hockey sticks,
>principle components, and spurious significance" by S. McIntyre and
>R. McKitrick, Geophys. Res. Lett., accepted.
>
>Wahl, E.R and C.M. Ammann, revised: Robustness of the Mann, Bradley,
>Hughes reconstruction of surface temperatures: Examination of
>criticisms based on the nature and processing of proxy climate
>evidence. Climatic Change, revised and in review.
>
>
>--
>Caspar M. Ammann
>National Center for Atmospheric Research
>Climate and Global Dynamics Division - Paleoclimatology

>1850 Table Mesa Drive
>Boulder, CO 80307-3000
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--
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http://www.ispe.arizona.edu/
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593. 1133366680.txt
##
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: [Fwd: u seen?]
Date: Wed, 30 Nov 2005 11:04:40 -0500
Reply-to: mann@psu.edu
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
fair enough, I'll go w/ flimsy. The real problem is the fairly
inflammatory wording of this, and the really flawed interpretations
w.r.t. implicatinos for natural vs. anthropogenic variaiblity.

normally I'd ignore, but the fact that Andy Revkin received this
suggests they are trying to publicize this review paper, which I find a
bit odd...

mike

Tim Osborn wrote:

> Hi Mike,
>
> I've seen this before (and probably Keith has too) because our EU
> "SOAP" project supported Rob Wilson, the second author. I'd say that
> it is "flimsy" rather than "shoddy"! Still, it's only supposed to be
> a "viewpoint" rather than new science.
>
> Tim
>
> At 15:31 30/11/2005, Michael E. Mann wrote:
>
>> thought you guys would be interested. pretty shoddy stuff in my view...
>>
>> mike
>>

>> --
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>> Associate Professor
>> Director, Earth System Science Center (ESSC)
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>> 503 Walker Building FAX: (814) 865-3663
>> The Pennsylvania State University email: mann@psu.edu
>> University Park, PA 16802-5013
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>> http://www.met.psu.edu/dept/faculty/mann.htm
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>>
>> Return-Path: <anrevk@nytimes.com>
>> X-Original-To: mann@meteo.psu.edu
>> Delivered-To: mann@meteo.psu.edu
>> Received: from tr12n04.aset.psu.edu (tr12g04.aset.psu.edu
>> [128.118.146.130])
>> by mail.meteo.psu.edu (Postfix) with ESMTP id 2027520401A
>> for <mann@meteo.psu.edu>; Wed, 30 Nov 2005 10:15:10 -0500 (EST)
>> Received: from nytimes.com (nat-hq-gate-02.nytimes.com
>> [199.181.175.222])
>> by tr12n04.aset.psu.edu (8.13.2/8.13.2) with ESMTP id
>> jAUFF8P22437280
>> for <mann@psu.edu>; Wed, 30 Nov 2005 10:15:08 -0500
>> Message-Id: <6.1.2.0.2.20051130101420.02d14460@smtp-store.nytimes.com>
>> X-Sender: anrevk@smtp-store.nytimes.com
>> X-Mailer: QUALCOMM Windows Eudora Version 6.1.2.0
>> Date: Wed, 30 Nov 2005 10:14:45 -0500
>> To: mann@psu.edu
>> From: Andy Revkin <anrevk@nytimes.com>
>> Subject: u seen?
>> Mime-Version: 1.0
>> Content-Type: multipart/alternative;
>> boundary="====================_79165303==.ALT"
>> X-NYTOriginatingHost: , 10.149.64.222
>> X-Virus-Scanned: amavisd-sophos
>> X-PSU-Spam-Flag: NO
>> X-PSU-Spam-Hits: 0.695
>> X-PSU-Spam-Level: *
>> X-Spam-Checker-Version: SpamAssassin 3.0.2 (2004-11-16) on
>> mail.meteo.psu.edu
>> X-Spam-Level:
>> X-Spam-Status: No, score=-1.6 required=5.0
>> tests=AWL,BAYES_00,HTML_00_10,
>> HTML_MESSAGE,MIME_QP_LONG_LINE autolearn=no version=3.0.2
>>
>> purely fyi.. u seen?
>>
>>
>>> Quaternary Science Reviews, Volume 24, Issues 20-21 , November 2005,
>>> Pages 2164-2166
>>> http://tinyurl.com/b95ee
>>>
>>> Climate: past ranges and future changes
>>>
>>> Jan Esper a), Robert J.S. Wilson b), David C. Frank a), Anders
>>> Moberg c), Heinz Wanner d) and Jürg Luterbacher d)
>>>
>>> a) Swiss Federal Research Institute WSL, 8903 Birmensdorf, Switzerland
>>> b) School of GeoSciences, Grant Institute, Edinburgh University,

>>> Edinburgh, UK
>>> c) Department of Meteorology, Stockholm University, 10691 Stockholm,
>>> Sweden
>>> d) NCCR Climate and Institute of Geography, University of Bern, 3012
>>> Bern, Switzerland
>>>
>>> Abstract
>>>
>>> Comparison of large-scale temperature reconstructions over the past
>>> millennium reveals agreement on major climatic episodes, but
>>> substantial divergence in reconstructed (absolute) temperature
>>> amplitude. We here detail several research priorities to overcome
>>> this 'amplitude desideratum', and discuss the relevance of this
>>> effort for the prediction of future temperature changes and the
>>> meaning of the Kyoto protocol.
>>>
>>> Persisting controversy (Regalado, 2005) surrounding a pioneering
>>> northern hemisphere temperature reconstruction (Mann et al., 1999)
>>> indicates the importance of such records to understand our changing
>>> climate. Such reconstructions, combining data from tree rings,
>>> documentary evidence and other proxy sources are key to evaluate
>>> natural forcing mechanisms, such as the sun's irradiance or volcanic
>>> eruptions, along with those from the widespread release of
>>> anthropogenic greenhouse gases since about 1850 during the
>>> industrial (and instrumental) period. We here demonstrate that our
>>> understanding of the shape of long-term climate fluctuations is
>>> better than commonly perceived, but that the absolute amplitude of
>>> temperature variations is poorly understood. We argue that the
>>> knowledge of this amplitude is critical for predicting future
>>> trends, and detail four research priorities to solve this
>>> incertitude: (i) reduce calibration uncertainty, (ii) preserve
>>> 'colour' in proxy data, (iii) utilize accurate instrumental data,
>>> and (iv) update old and develop new proxy data.
>>>
>>> When matching existing temperature reconstructions (Jones et al.,
>>> 1999; Mann et al., 1999; Briffa, 2000; Esper et al., 2002; Moberg,
>>> et al., 2005) over the past 1000 years, although substantial
>>> divergences exist during certain periods, the timeseries display a
>>> reasonably coherent picture of major climatic episodes: 'Medieval
>>> Warm Period', 'Little Ice Age' and 'Recent Warming' (Fig. 1).
>>> However, when calibrated against instrumental temperature records,
>>> these same reconstructions splay outwards with temperature
>>> amplitudes ranging from 0.4 to 1.0 ˚C for decadal means (Moberg et
>>> al., 2005). Further, a comparison of commonly used regression and
>>> scaling approaches shows that the reconstructed absolute amplitudes
>>> easily vary by over 0.5 ˚C, depending on the method and instrumental
>>> target chosen (Esper et al., 2005). Overall, amplitude discrepancies
>>> are in the order of the total variability estimated over the past
>>> millennium, and undoubtedly confuse future modelled temperature
>>> trends via parameterisation uncertainties related to inadequately
>>> simulated behaviour of past variability.
>>>
>>> Fig. 1. Course of temperature variations. Large-scale temperature
>>> reconstructions scaled to the same mean and variance over the common
>>> period 1000-1979 AD, and their arithmetic mean. The normalisation
>>> highlights the similarity between the records, but broadly ignores
>>> the differing calibration statistics with instrumental data, and
>>> their particular 'shapes' and distribution of variance, e.g. during
>>> the instrumental and pre-instrumental periods. The average
>>> correlation between the original reconstructions is 0.47, and 0.64
>>> after smoothing (as done in the figure using a 40-year low-pass
>>> filter). Lag-1 autocorrelations range from 0.52 (Jones98) to 0.93
>>> (Moberg05; with no variability <4 years represented).

>>>
>>>
>>> Solutions to reduce calibration uncertainty include the use of
>>> pseudo-proxy experiments (Osborn and Briffa, 2004; von Storch et
>>> al., 2004) derived from ensemble simulations of different models
>>> (Knutti et al., 2002; Stainforth et al., 2005) to test statistical
>>> calibration methods, e.g. principal component (Cook et al., 1994)
>>> and timescale-dependent (Osborn and Briffa, 2000) regression. Such
>>> analyses, however, should mimic the character of empirical proxy
>>> data, e.g. the decline of replication (numbers of sites, quality per
>>> site) back in time, and the addition of noise typical to empirical
>>> proxy data (i.e., not just white; Mann and Rutherford, 2002).
>>> Further, reconstructions from areas such as Europe (Luterbacher et
>>> al., 2004; Xoplaki et al., 2005), where long instrumental series and
>>> high densities of proxy records exist, allow extended calibration
>>> periods and increased degrees of freedom enabling the assessment of
>>> robust relationships at all timescales (i.e., low and high
>>> frequency), both critical to reduce calibration uncertainty.
>>> Subsequent comparison of such regional records with hemispheric
>>> reconstructions that can be downscaled should provide greater
>>> understanding of reconstructed amplitudes at larger spatial scales.
>>>
>>> Accurate preservation and assessment of low-to-high frequency
>>> variation ('colour') in proxy data, and a selected use of certain
>>> frequency bands that best fit those of instrumental data (Moberg et
>>> al., 2005), are further desirable when compiling large-scale
>>> reconstructions that seek to yield the true absolute temperature
>>> amplitude. This approach, however, requires a comprehensive
>>> examination of regional proxy data including the seasonality of
>>> temperature signals, and a selection of only those records that
>>> effectively capture low-frequency climate variation. Inclusion of
>>> regional tree ring records in which long-term trends are not
>>> preserved, should be avoided in efforts to reconstruct low frequency
>>> temperature variations (Esper et al., 2004; Melvin, 2004). In these
>>> data, such limitations primarily occur when age-related biases from
>>> tree-ring series are individually estimated and removed ('the
>>> segment length curse' Cook et al., 1995). Similar considerations
>>> apply to documentary evidence, long isotope records and other proxy
>>> sources that should, on a site-by-site basis, be examined for
>>> potential low-frequency limitations.
>>>
>>> The instrumental target data chosen (Esper et al., 2005), and
>>> adjustments made to these data are also vital to the reconstructed
>>> amplitude. A recent analysis of a carefully homogenised instrumental
>>> network from the Alps and surrounding areas (Böhm et al., 2001), for
>>> example, shows the annual temperature trend over the last ca 110
>>> years to be 1.1 °C-twice that observed over the same alpine
>>> gridboxes in the global dataset provided by the Climatic Research
>>> Unit (Jones et al., 1999). Such changes in the character of
>>> observational data, resulting from homogeneity adjustments and
>>> methodology differences (Moberg et al., 2003), directly affect the
>>> temperature amplitude in proxy-based reconstructions, since
>>> instrumental calibration sets the pulse in these paleorecords
>>> (Büntgen et al., 2005). Accurate instrumental data are therefore
>>> crucial to the reconstructed amplitude, and this again argues for
>>> regional studies where mutual verification between proxy and
>>> instrumental records is viable (Frank and Esper, 2005; Wilson et
>>> al., 2005).
>>>
>>> Finally, more proxy data covering the full millennium and
>>> representing the same spatial domain as the instrumental target data
>>> (e.g., hemisphere) are required to solve the amplitude puzzle. The
>>> current pool of 1000-year long annually resolved temperature proxies

>>> is limited to a handful of timeseries, with some of them also
>>> portraying differing seasonal (e.g., summer or annual) responses.
>>> Furthermore, the strength of many of these local records and
>>> literally all tree ring chronologies varies and almost always
>>> declines back in time (Cook et al., 2004). The reasons are manifold
>>> and include dating uncertainty, loss of signal fidelity in the
>>> recent period, assumptions about signal stationarity, reduction of
>>> sample replication, etc., and are generally not considered in the
>>> uncertainty estimates of combined large-scale reconstructions. Also,
>>> data from the most recent decades, absent in many regional proxy
>>> records, limits the calibration period length and hinders tests of
>>> the behaviour of the proxies under the present 'extreme' temperature
>>> conditions. Calibration including the exceptional conditions since
>>> the 1990s would, however, be necessary to estimate the robustness of
>>> a reconstruction during earlier warm episodes, such as the Medieval
>>> Warm Period, and would avoid the need to splice proxy and
>>> instrumental records together to derive conclusions about recent
>>> warmth.
>>>
>>> So, what would it mean, if the reconstructions indicate a larger
>>> (Esper et al., 2002; Pollack and Smerdon, 2004; Moberg et al., 2005)
>>> or smaller (Jones et al., 1998; Mann et al., 1999) temperature
>>> amplitude? We suggest that the former situation, i.e. enhanced
>>> variability during pre-industrial times, would result in a
>>> redistribution of weight towards the role of natural factors in
>>> forcing temperature changes, thereby relatively devaluing the impact
>>> of anthropogenic emissions and affecting future predicted scenarios.
>>> If that turns out to be the case, agreements such as the Kyoto
>>> protocol that intend to reduce emissions of anthropogenic greenhouse
>>> gases, would be less effective than thought. This scenario, however,
>>> does not question the general mechanism established within the
>>> protocol, which we believe is a breakthrough.
>>>
>>> doi:10.1016/j.quascirev.2005.07.001
>>>
>>
>>
>>
>> Andrew C. Revkin, Science Reporter, The New York Times
>> 229 West 43d St. NY, NY 10036
>> Tel: 212-556-7326, 914-441-5556 (mobile); Fax: 509-357-0965
>> Recent Arctic coverage: www.nytimes.com/pages/science/sciencereport
>> Book on the Amazon: The Burning Season (www.islandpress.org/burning)
>> Acoustic-Roots Band: www.sonicbids.com/unclewade
>
>
> Dr Timothy J Osborn
> Climatic Research Unit
> School of Environmental Sciences, University of East Anglia
> Norwich NR4 7TJ, UK
>
> e-mail: t.osborn@uea.ac.uk
> phone: +44 1603 592089
> fax: +44 1603 507784
> web: http://www.cru.uea.ac.uk/~timo/
> sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm
>

--
Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

http://www.met.psu.edu/dept/faculty/mann.htm

</x-flowed>

594. 1133532909.txt
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: "Raymond S. Bradley" <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Caspar Ammann <ammann@ucar.edu>
Subject: Esper et al...
Date: Fri, 02 Dec 2005 09:15:09 -0500
Reply-to: mann@psu.edu

<x-flowed>
thought you all would be interested in this. Esper et al have played right into the hands of the contrarians:

http://www.foxnews.com/story/0,2933,177380,00.html

The wording o their abstract is franklyjust irresponsible...

Mike

--
Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

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The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

http://www.met.psu.edu/dept/faculty/mann.htm

</x-flowed>

595. 1134418588.txt
##########

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: HadCRUT2v
Date: Mon, 12 Dec 2005 15:16:28 -0700
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Ben Santer <santer1@llnl.gov>

<x-flowed>

Phil,

Why is there so much missing data for the South Pole? The period Jan 75 thru
Dec 90 is all missing except Dec 81, July & Dec 85, Apr 87, Apr & Sept 88,
Apr 89. Also, from and including Aug 2003 is missing.

Also -- more seriously but correctable. The S Pole is just represented
by a single
box at 87.5S (N Pole ditto I suspect). This screws up area averaging. It
would be
better to put the S Pole value in ALL boxes at 87.5S.

I have had to do this in my code -- but you really should fix the 'raw'
gridded data.

For area averages, the difference is between having the S Pole represent
the whole
region south of 85S, and having (as now) it represent one 72nd of this
region. It
is pretty obvious to me what is better.

This affects the impression of missing data too of course.

Tom.
</x-flowed>

596. 1134497252.txt
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: P.Jones@uea.ac.uk, "Tom Wigley" <wigley@cgd.ucar.edu>
Subject: Re: HadCRUT2v
Date: Tue Dec 13 13:07:32 2005
Cc: "Ben Santer" <santer1@llnl.gov>

Dear all,
attached is a plot of the monthly anomalies from the only box with non-missing
data in the
bottom row of Phil's grid (centred at 87.5 S). This is from HadCRUT2v that I
picked up
from the CRU data store in June this year.
Clearly the dates Tom listed are missing in my version too. Furthermore, the
values from
1971-1975 are abnormal. They are not all identical, but are all near zero.
Perhaps
multiplied by 0.1?
Similar problems are apparent in HadCRUT and CRUTEM2v too.
But CRUTEM2 has no gaps and no abnormal periods at the South Pole, so perhaps
CRUTEM2 is
fine? Tom - if it's urgent, you could extract the South Pole time series from
CRUTEM2 and
use it to overwrite the other 3 data sets until Phil corrects them.
Regarding the weighting issue...
Given that the grid doesn't have equal-area boxes, there are always going to be
compromises
with weighting. Even if you do something to sort out the problem at the S. Pole,
how about
the isolated boxes around the coast of Antarctica, which will be given much less
weight
than an isolated box in the tropics which might also have only 1 station in.
This is

partly reasonable because of differences in spatial correlation of temperatures between
tropics and high latitudes, but I'm sure that they don't compensate exactly.
Specifically for the poles...
Putting the temperature data into a single box will clearly underweight its contribution in
area averages (is it significant from a practical point of view once you get to hemispheric
or global scales though?).
Replicating it into all boxes in the bottom row will, on the other hand, gives it too much
weight. If the area weighting is calculated simply as cos(latitude) then the South Pole
data will be given this weighting:
72*cos(87.5) = 3.14
whereas one box on the equator (or just off) will be given this weighting:
1*cos(2.5) = 1.00
so, if replicated around all boxes at 87.5 S, the South Pole would have three times the
weight of a single tropical box (compared with 23 times less weight if South Pole data
appears in only one box).
Perhaps put it in every fourth box, giving a weighting of 0.79 (bit less than tropical,
which is reasonable for spatial correlation reasons)?
Cheers
Tim
At 04:11 13/12/2005, P.Jones@uea.ac.uk wrote:

Tom,
In NZ at the IPCC meeting. Will be here until Dec 17.
When I get back I'm off to Switzerland for Christmas on
Dec 21.
The South Pole shouldn't be missing. I have all the
data for Amundsen-Scott from 1957. I put the data in at
one 5 degree grid box, so it doesn't get overweighted.
The South Pole should be at the last grid box (2592)
in the 72 by 36 array. Putting the data in all 87.5-90S
boxes would overweight the S.Pole stations.
There isn't any data at the N. Pole.
Maybe Tim could check on the missing S.Pole data.
I reckon it should be there in all the datasets CRUTEM2
and HadCRUT2 and the v versions.
Cheers
Phil
> Phil,
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> Why is there so much missing data for the South Pole? The period Jan 75
> thru
> Dec 90 is all missing except Dec 81, July & Dec 85, Apr 87, Apr & Sept 88,
> Apr 89. Also, from and including Aug 2003 is missing.
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> by a single
> box at 87.5S (N Pole ditto I suspect). This screws up area averaging. It
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> better to put the S Pole value in ALL boxes at 87.5S.
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> region south of 85S, and having (as now) it represent one 72nd of this
> region. It
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>
> This affects the impression of missing data too of course.
>
> Tom.
>

597. 1134526470.txt
##
##########

From: P.Jones@uea.ac.uk
To: jen.hardwick@metoffice.gov.uk
Subject: [Fwd: Re: HadCRUT2v]
Date: Tue, 13 Dec 2005 21:14:30 -0000 (GMT)
Cc: t.osborn@uea.ac.uk, philip.brohan@metoffice.gov.uk

Dear Jen,
  There seems to be a problem with the South Pole
box (#2592). The data are in CRUTEM2(v) but not in
HadCRUT2(v). See the plot and email from Tim Osborn.

Email Tim if you can find what is up. The boxes in
the two datasets should be the same.

  I'm in NZ at IPCC.

Cheers
Phil

---------- Original Message ----------
Subject: Re: HadCRUT2v
From: "Tim Osborn" <t.osborn@uea.ac.uk>
Date: Tue, December 13, 2005 1:07 pm
To: P.Jones@uea.ac.uk
"Tom Wigley" <wigley@cgd.ucar.edu>
Cc: "Ben Santer" <santer1@llnl.gov>

Dear all,

attached is a plot of the monthly anomalies from the only box with
non-missing data in the bottom row of Phil's grid (centred at 87.5
S). This is from HadCRUT2v that I picked up from the CRU data store
in June this year.

Clearly the dates Tom listed are missing in my version
too. Furthermore, the values from 1971-1975 are abnormal. They are
not all identical, but are all near zero. Perhaps multiplied by 0.1?

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South Pole time series from CRUTEM2 and use it to overwrite the other
3 data sets until Phil corrects them.

Regarding the weighting issue...

Given that the grid doesn't have equal-area boxes, there are always going to be compromises with weighting. Even if you do something to sort out the problem at the S. Pole, how about the isolated boxes around the coast of Antarctica, which will be given much less weight than an isolated box in the tropics which might also have only 1 station in. This is partly reasonable because of differences in spatial correlation of temperatures between tropics and high latitudes, but I'm sure that they don't compensate exactly.

Specifically for the poles...

Putting the temperature data into a single box will clearly underweight its contribution in area averages (is it significant from a practical point of view once you get to hemispheric or global scales though?).

Replicating it into all boxes in the bottom row will, on the other hand, gives it too much weight. If the area weighting is calculated simply as cos(latitude) then the South Pole data will be given this weighting:

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Perhaps put it in every fourth box, giving a weighting of 0.79 (bit less than tropical, which is reasonable for spatial correlation reasons)?

Cheers

Tim

At 04:11 13/12/2005, P.Jones@uea.ac.uk wrote:
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>
> Cheers
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>
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> > region. It
> > is pretty obvious to me what is better.
> >
> > This affects the impression of missing data too of course.
> >
> > Tom.
> >
Dr Timothy J Osborn
Climatic Research Unit
School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk
phone: +44 1603 592089
fax: +44 1603 507784
web: http://www.cru.uea.ac.uk/~timo/
sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm

Attachment Converted: "c:\documents and settings\tim osborn\my
documents\eudora\attach\southpole.gif"

598. 1134572247.txt
##
##########

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>, P.Jones@uea.ac.uk
Subject: Re: HadCRUT2v
Date: Wed Dec 14 09:57:27 2005
Cc: Ben Santer <santer1@llnl.gov>

   At 21:58 13/12/2005, Tom Wigley wrote:

     Phil,
     Before you finalize anything, please let me get back to you with some
     additional thoughts. There are some wrinkles that you and Tim don't
     seem to have thought of.
     Tom.

   Tom
   One further thing (possibly one of the extra wrinkles?) is that while you could
put the S
   Pole data from CRUTEM2 (where it seems correct) into HadCRUT2, it isn't quite
correct to
   put it (as I wrongly suggested) into CRUTEM2v and HadCRUT2v because those should
have their

high frequency deviations scaled to remove sample-size-related biases. Only a minor
difference.
Tim

599. 1134931991.txt
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: more on TS feedback
Date: Sun, 18 Dec 2005 13:53:11 -0700

<x-flowed>
Dear Keith, Bette and Eystein:

This email should be read after the one to the entire team - it provides post LA3/TS feedback on figures. Since Bette is going on a short vacation, she and I emailed about her new LIG fig before I left, so she's ready to go when she gets home.

Keith (and Tim), on the other hand, have lots to consider, and I just wanted to reiterate to you (and Bette) that it's a priority for me and Eystein to help you brainstorm all these figures. Here are a few more comments I got on Keith/Tim Figs:

For 6.8:

1) removing the oldest portion of the records from the plot is only ok IF:
-we can justify on an obvious and objective basis - for example that sample depth hits goes down significantly at ca. 700AD or wherever we want to chop it.
-We don't remove part of the series that will give rise to accusations of bias
Thus, it might be better to leave as was in the FOD, just to be safe, or to try multiple versions.

2) had a long talk with Martin Manning about the idea of multiple plots, vs just the existing one (by the way, the TS team WANTS the instrumental part of the fig as we agreed to modify in Chap 6 sessions). I think the best idea is to keep the bottom panel as is, with modifications
- keep the error bars as is
- try a version with some sort of annually-resolved volc forcing placed at the top of the panel, with eruption (sufate) lines sticking down farther for big eruptions
- try inserting some representation of average (median? or?) sample depth along the bottom (time) side of the panel. This will thus show, lots of sample depth back to ca. 1700, then less and less (in steps?). Martin suggests we go one step farther and color the sample depth part of the plot with different colors, based on our expert judgement of confidence. We could have two or three colors - one color for the interval overwhich we have "very likely" confidence (e.g., in the exec summary) and another for just "very." perhaps we want a third for some term reflecting "don't trust inferences regarding hemispheric temp that much over this interval" - this will obviously take some thinking/creativity, but this fig will go all the way to the TSM, so it's worth the effort.

3) linear axis for sure

4) if would still be good to try a density shaded version of this plot (instead of all the recon lines) for the TS and SPM. When in doubt, make an extra version. We can then share with our team and with Susan.

Thanks for doing this!

Also, FYI, Gabe indicated that her regional plots were not scaled separately. Surprising, but maybe the models are actually better than we thought.

Best, Peck

--
Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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direct tel: +1 520 622-9065
fax: +1 520 792-8795
http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
</x-flowed>

600. 1135033853.txt
##########

From: "David Willans" <david@futerra.co.uk>
To: <training@futerra.co.uk>
Subject: Training Dates
Date: Mon, 19 Dec 2005 18:10:53 -0000

Hello,

Some dates for your new year diary...

Futerra are launching a series of masterclasses on communicating sustainable development in early 2006.

Communicating Climate Change on a Local and Regional Level

12.30 - 5.30pm

Thursday 26 January 2006

Communicating Sustainable Development

12.30 - 5.30pm

Thursday 23 February 2006

Communicating Climate Change

12.30 - 5.30pm

Thursday 30 March 2006

Using international case studies and proven communication tools, each session is designed to build your confidence to plan and implement campaigns.

"Enthusiastic and friendly trainers with a tremendous amount of knowledge" - Past participant

For more information or to book then please see the attached flyer or visit our [1]website. The groups will be kept to only 15 people, so please sign up early to avoid disappointment.

The Futerra team wish you a very merry Christmas!

David

David Willans

Consultant

Futerra Sustainability Communications Ltd

[2]www.futerra.co.uk

We've moved! Please note new contact details

Direct Dial: +44 (0)20 7378 4003

Switchboard: +44 (0)20 7378 4000

84 Long Lane

London SE1 4AU

Attachment Converted: "c:\eudora\attach\Futerra_Masterclass.pdf"

References

1. http://www.futerracom.org/auto.php?inc=case&site_cat=1&site_sub=17&case=0
2. outbind://41-00000000C60442BB81504F4199CB74C59420FE1E049E2A00/www.futerra.co.uk

##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
Subject: Re: [Wg1-ar4-ch06] Follow-up from Christchurch
Date: Mon, 19 Dec 2005 21:32:37 -0700
Cc: wg1-ar4-ch06@joss.ucar.edu

<x-flowed>
Hi Stefan and team - great. David Rind is getting the solar forcing series de jour (latest Lean). I expect Keith back on line soon, and then he can help us figure out what type of simulation(s) we'd like, and what other forcings we ought to use. My take is that it would be good to use the same forcing used in the runs currently in Fig 6.10 (or at least the "best" of those runs - subjective, I'm sure, and all with the old larger amplitude Lean solar), but with the new reduced amplitude forcing.

Fig 6.10 currently has the Bauer et al, 2003 run w/ CLIMBER - is it CLIMBER2? Could/should we just re-run with the new solar in place of the old solar (I don't have the paper here - was the solar used scaled to Lean?).

I'll cc this to the entire team, as there might be other ideas on how to do this - I think we would want two simulations over the last 400 years. One w/ the old Lean solar, one with the new. If we could use one of the existing plotted runs as the "old Lean" run, then we only need one new run. The idea is to show what difference TAR solar (old Lean) vs. AR4 solar (new Lean) means.

So, lets see what Keith and others say, and then line things up to get the run done. If we can do it w/ CLIMBER, great. If we need to involve another EMIC (assuming we're not going to get a AOGCM run done in less than a month), then we need to line that up. Whatever model we use, it should be one already in use by the AR4, so we don't have to worry about the results being published - just the model. Make sense?

Thanks again for the quick reply. Best, Peck

>Dear Jonathan,
>
>concerning item 8: we can deliver a millennium simulation with any
>given forcing provided to us within days. (Actually takes just about
>1 hour to run on the computer with CLIMBER-2.)
>
>Cheers,
>Stefan

--
Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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fax: +1 520 792-8795
http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
_____
Wg1-ar4-ch06 mailing list
Wg1-ar4-ch06@joss.ucar.edu
http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06
</x-flowed>

602. 1135197791.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: [Wg1-ar4-ch06] Follow-up from Christchurch
Date: Wed, 21 Dec 2005 15:43:11 +0000

<x-flowed>

>Date: Sun, 18 Dec 2005 13:53:03 -0700
>To: wg1-ar4-ch06@joss.ucar.edu
>From: Jonathan Overpeck <jto@u.arizona.edu>
>X-Virus-Scanned: amavisd-new at ucar.edu
>Subject: [Wg1-ar4-ch06] Follow-up from Christchurch
>X-BeenThere: wg1-ar4-ch06@joss.ucar.edu
>X-Mailman-Version: 2.1.1
>List-Id: <wg1-ar4-ch06.joss.ucar.edu>
>List-Help: <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=help>
>List-Post: <mailto:wg1-ar4-ch06@joss.ucar.edu>
>List-Subscribe: <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>,
>   <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=subscribe>
>List-Archive: <http://www.joss.ucar.edu/mailman/private/wg1-ar4-ch06>
>List-Unsubscribe: <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>,
>   <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=unsubscribe>
>Sender: wg1-ar4-ch06-bounces@joss.ucar.edu
>X-UEA-Spam-Score: 0.0
>X-UEA-Spam-Level: /
>X-UEA-Spam-Flag: NO
>
>Hi Chapter 6 Friends - Just wanted to thank you all for a great IPCC
>meeting and solid progress toward the SOD of Chapter 6, as well as
>give you a report on the TS meeting that took place on Friday. I'm
>in transit, so haven't been able to see any emails, but I suspect
>Eystein is also sending some updates on what we need to be doing.
>We'll have to work fast and hard to make all the deadlines, but I
>think its safe to say that our chapter will have real impact. I want
>to personally thank you for your dedication to our team effort!
>
>PLEASE READ THE FOLLOWING CAREFULLY
>
>The TS/SPM meeting on Friday was exhausting, as appears to be
>traditional for all things IPCC. But, it was quite impressive in
>terms of how paleo was viewed by the broader WG1 team of authors.
>This is reflected in the decision to consider (without any pushing
>from me, believe it or not) several new figures from our chapter.
>Below I list these along with the others that will need refinement
>for use by the TS. Please note where I insert "ACTION ITEM" - these
>are very time sensitive assignments that should be carried out ASAP
>(i.e., before the new year where possible). Note that everything
>(i.e., figures) in the TS will also have to be in our chapter.
>

>1) the orbital box. Eystein and I have the draft completed by
>Valerie et al in New Zealand. We will read/edit (ACTION ITEM) and
>send around to the group for further editing. The TS version might
>have to be altered to reflect the broader audience, and I'm not yet
>sure what figure would best go with the TS version. I believe
>Valerie (ACTION ITEM) is exploring (with Stefan?) a nice figure that
>illustrates the mechanisms of orbital forcing.
>
>2) there will also be an model evaluation box in the TS that will
>have paleo. Once I get more feedback on this (Chap 8 is leading on
>this box), I'll connect the rest of our team with this effort, with
>Bette in the role of lead chap 6 person.
>
>3) there will a sea level box led by Chap 5. I'm not sure what the
>fig will look like in this box, but if Dick (ACTION ITEM) can
>produce his new Chap 6 sea level figure FAST, we can float it as a
>possible contributor to the TS Box figure. It would be great to get
>paleo sea level perspectives in this box!
>
>4) there will be expanded discussion of abrupt change with focus on
>paleo - Richard Alley is leading this, and I think that will be a
>real plus in making sure the discussion isn't just model based
>
>4) Keith's sites through time figure is also still a TS item. There
>will hopefully also be a fig showing the distribution of
>instrumental sites. Keith has the ACTION ITEM on his figure. Peck
>and Eystein can help get the data released to Keith and Tim if
>needed - just let us know.
>
>5) Keith's 6.8 figure will have to be worked on to find the best
>mode of presentation, and I have a separate email on this one for
>him and Tim. The TS team would like to see inserted on the fig
>(e.g., along the lower edge of the figure, perhaps) some depiction
>of how the site number used changes back in time, and some color
>coding to denote how our expert judgement suggests the implied
>confidence in the recons change back in time. I'm guessing this will
>require some phone conversations to think through with Keith (ACTION
>ITEM for Eystein, Peck and Keith).
>
>6) A NEW FIGURE - depicting inferred solar forcing over the last X
>centuries. The request is that we show Judith Lean et al's latest
>for 1600 to present. This could include the volcanic forcing too,
>but it seems more appropriate that we stick with our plan to add
>this to the expanded 6.8. We'll have to try both figs (this new one,
>and the expanded 6.8) figure w/ and w/o the volcanic series (i.e.,
>detrended multi-core average excess sulfate from each of two polar
>regions) on each fig. I think Keith/Tim gets the ACTION ITEM on all
>this figure stuff - Perhaps David (ACTION ITEM) can send Judith's
>latest solar recon to Keith?
>
>7) Expanded/modified recent forcing figureS by Fortunat (ACTION
>ITEM). One will be for Chap 6, the other will combine Chap 2 and 6
>perspectives into a single figure for the TS. I'll send a separate
>fig to Fortunat with the details, but everyone likes his new rate of
>change depiction, and the TS team also wants a ice core tropospheric
>aerosol record too (e.g., for the last couple centuries - Jean
>Jouzel thought we could do this using Greenland ice core data, and
>we'd add this to the TS fig (and either a chap 2 or 6 figure, since
>everyting in the TS has to also be in a chapter.
>
>8) A NEW FIGURE for the TS (and maybe not chapt 6, since we already
>have 6.8 and 6.10 with most of the info) should be the one of
>Keith's that we showed in our plenary talk on Thursday - the

>multi-model range of simulated change over the last 1000 (red
>shading) superimposed on our chap 6 observed record (represented by
>grey shading as in the fig we showed). Requested modifications for
>Keith/Tim (ACTION ITEM) include: a) using a 20th century ref period
>as in the current Fig 6.8, b) adding (where possible) simulations
>that include natural forcing only (and thus not enough warming in
>20th century) and c) adding one or more EMIC simulations using the
>new Lean solar recon (at least over the last 400 years, with all the
>other forcing). This last one is tricky, since no one at the TS mtg
>thought such a simulation exists, BUT it seems it is ok for us to
>get/use a new long simulation by one of the EMIC models used in Chap
>10. Peck (ACTION ITEM) needs to figure out how to get this, but
>Thomas Stocker indicated he'd help. Stafan - what about you guys
>doing this? Who else could we ask for fast turnaround?
>
>9) Another NEW FIGURE (that I actually fought including since we
>don't want to be seen showing off our own stuff) of Last
>Interglacial (LIG) Change. The TS team (and Susan) really liked this
>paleo message, so we came up with a proposed scheme (which I already
>discussed with Bette - who has the ACTION ITEM) that will involve
>the inclusion of more than one LIG climate simulation, plotted with
>observations superimposed, and perhaps more than one LIG ice sheet
>reconstruction as well. Should Tarasov and Peltier be considered for
>this fig (forced by ice-core inferred LIG climate)? Are there
>others? For this figure to work, it has to be a synthesis of
>multiple studies, not just the recent Otto-Bleisner et al effort.
>
>So, that is the news - all good from the view point of chap 6
>exposure/impact, but of course, not so good in terms of the
>additional fast-turn-around work that is needed. The other tough
>issue is that - after several negotiating sessions with Susan (the
>last one with Jean Jouzel helping) - the best we could do is get our
>page limit increased from 30 to 35 pages. That doesn't sound too
>bad, except that we have to a) get all our existing material into
>less space than now (we're currently at an estimated 36 pages) AND
>b) get the new figures mentioned above in (two I think - solar, plus
>the LIG fig). We can do it, but everyone has to be thinking NOW
>about how to reduce our text.
>
>Again, many thanks for all the travel and hard work over the last
>two weeks. Also (in advance) for all the hard work coming up this
>month and the next two.
>
>Best, Peck
>--
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