CRU CORRESPONDENCE
##
##########

760. 1167752455.txt
##
##########

From: Tom Crowley <tcrowley@duke.edu>
To: mann@psu.edu
Subject: not so fast
Date: Tue, 02 Jan 2007 10:40:55 -0500
Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, rahmstorf@ozean-klima.de, Eric Steig <steig@ess.washington.edu>, gschmidt@giss.nasa.gov, rasmus.benestad@physics.org, garidel@marine.rutgers.edu, Caspar Ammann <ammann@ucar.edu>, William Connelley <wmconnolley@gmail.com>, d-archer@uchicago.edu, rtp1@geosci.uchicago.edu, p.jones@uea.ac.uk

   we still don't have an adequat explanation as to how Jack "cooked up" that figure - I do
   not believe it was purely out of thin air - look at the attached - which I used in the
   Crowley-Lowery composite just because it was "out there" - I made no claim that it was the
   record of record, but just that it had been used beforer. the Lamb ref. is his book dated
   1966. I will have to dig up the page ref later. Dansgaard et al. 1975 Nature paper on
   Norsemen...etc used that figure when comparing what must have been their Camp Century
   record - have to check that too - where the main point of that paper was that the timing of
   Medieval warmth was different in Greenlandn and England!
   25 years later my provocation for writing the CL paper came from a strong statement on the
   MWP by Claus Hammer that the canonical idea of the MWP being warming than the present was
   correct and that the 1999 Mann et al was wrong. he kept going on like that I reminded him
   that he was a co-author on the 1975 paper! that is also what motivated to do my "bonehead"
   sampling of whatever was out there just to see what happened when you added them all
   together - the amazing result was that it looked pretty much like Mann et al. ther rest is
   history -- much ignored and forgotten.
   I might also pointn out that in a 1996 Consequences article I wrote - and that Fred Singer
   loves to cite -- Jack (who was the editor of the journal) basically shoehorned me into
   re-reproducing that figure even though I didn't like it - there was not an alternative. in
   the figure caption it has a similar one to Zielinski except that it states "compiled by
   R.S. Bradley and J.A. Eddy based on J.T. Houghton....so that puts a further twist on this
   because it point to Houghton not Bradley/Eddy as the source. Jack must have written that
   part of the figure caption because I don't think I knew those details.
   but we still don't know where the details of the figure came from - the MWP is clearly more
   schematic than the LIA (actually the detailsl about timing of the samll wiggles in the LIA

are pretty good) - maybe there was a meshing of the Greenland and the England records to do
the MWP part - note that the English part gets cooler. they may also have thrown in the
old LaMarche record - which I also have. maybe I can schlep something together using only
those old three records.
tom
Michael E. Mann wrote:

Ray, happy holidays and thanks for the (quite fascinating) background on this. It would be
good material for a Realclimate article. would be even better if someone could get Chris on
record confirming that this is indeed the history of this graphic...
mike
raymond s. bradley wrote:

I believe this graph originated in a (literally) grey piece of literature that Jack Eddy
used to publish called "Earth Quest". It was designed for, and distributed to, high
school teachers. In one issue, he had a fold-out that showed different timelines,
Cenozoic, Quaternary, last 100ka, Holocene, last millennium, last century etc. The idea
was to give non-specialists a perspective on the earth's climate history. I think this
idea evolved from the old NRC publication edited by L. Gates, then further elaborated on
by Tom Webb in the book I edited for UCAR, Global Changes of the Past. (This was an
outcome of the wonderful Snowmass meeting Jack master-minded around 1990).

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from Jack with a hand-drawn graph, that he asked me to review. Where he got his version
from, I don't know. I think I scribbled out part of the line and amended it in some
way, but have no recollection of exactly what I did to it. And whether he edited it
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perhaps it's not so bad. I note, however, that in the more colourful version of the
much embellished graph that Stefan circulated ([1]

http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html
the end-point has been changed to 2000, which puts quite a different spin on things.
They also seem to have fabricated a scale for the purported temperature changes. In any
case, the graph has no objective basis whatsoever; it is purely a "visual guess" at what
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persistent inquisitor..... (so make sure you don't leave such things on the table...).
What made the last millennium graph famous (notorious!) was that Chris Folland must have
seen it and reproduced it in the 1995 IPCC chapter he was editing. I don't think he
gave a citation and it thus appeared to have the imprimatur of the IPCC. Having

submitted a great deal of text for that chapter, I remember being really pissed off that
Chris essentially ignored all the input, and wrote his own version of the paleoclimate
record in that volume.

There are other examples of how Jack Eddy's grey literature publication was misused. In
a paper in Science by Zielinski et al. (1994) [v.264, p.448-452]--attached--they
reproduced [in Figure 1c] a similarly schematic version of Holocene temperatures giving
the following citation, "Taken from J. A. Eddy and R. S. Bradley, Earth-quest 5 (insert)
(1991), as modified from J. T. Houghton, G. J. Jenkins, J. J. Ephraums, Climate Change,
The IPCC Scientific Assessment (Cambridge Univ. Press, Cambridge, 1990)."
But I had nothing to do with that one!
So, that's how a crude fax from Jack Eddy became the definitive IPCC record on the last
millennium!
Happy New Year to everyone
Ray

Raymond S. Bradley
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Department of Geosciences, University of Massachusetts
Morrill Science Center
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AMHERST, MA 01003-9297
Tel: 413-545-2120
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*Climate System Research Center: 413-545-0659
<[2] http://www.paleoclimate.org>
Paleoclimatology Book Web Site: [3]http://www.geo.umass.edu/climate/paleo/html
Publications (download .pdf files):
[4]http://www.geo.umass.edu/faculty/bradley/bradleypub.html

--
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Director, Earth System Science Center (ESSC)

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503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [5]mann@psu.edu
University Park, PA 16802-5013

[6]http://www.met.psu.edu/dept/faculty/mann.htm

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

References

1. http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html
2. http://www.paleoclimate.org/
3. http://www.geo.umass.edu/climate/paleo/html
4. http://www.geo.umass.edu/faculty/bradley/bradleypub.html
5. mailto:mann@psu.edu
6. http://www.met.psu.edu/dept/faculty/mann.htm

761. 1167754725.txt
##

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Tom Crowley <tcrowley@duke.edu>
Subject: Re: not so fast
Date: Tue, 02 Jan 2007 11:18:45 -0500
Reply-to: mann@psu.edu
Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, rahmstorf@ozean-klima.de, Eric Steig <steig@ess.washington.edu>, gschmidt@giss.nasa.gov, rasmus.benestad@physics.org, garidel@marine.rutgers.edu, Caspar Ammann <ammann@ucar.edu>, William Connelley <wmconnolley@gmail.com>, d-archer@uchicago.edu, rtp1@geosci.uchicago.edu, p.jones@uea.ac.uk

for those who are interested, there is a paper by Goosse et al (I'm a co-author) explaining
why parts of Europe such as central england would have experienced warmer summer conditions
relative to present than other regions, related to early land-use change:
Goosse, H., Arzel, O., Luterbacher, J., Mann, M.E., Renssen, H., Riedwyl, N., Timmermann,
A., Xoplaki, E., Wanner, H., [1]The origin of the European "Medieval Warm Period", Climate
of the Past, 2, 99-113, 2006.
paper available as pdf here:
[2]http://www.meteo.psu.edu/~mann/shared/articles/Goosseetal-CP06.pdf
meanwhile, winter warmth could have been due to a strong AO/NAO pattern associated with
decreased volcanism and high solar, as discussed in the various Shindell et al paper.
this simply underscores the point that we all often make, that one needs to take into
account regional factors when interpreting regional records. This is especially relevant to
the extrapolation of a long record from England to the entire NH (which appears to have
been tacitly done by Jack Eddy?),
mike
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References

1. file://localhost/tmp/Goosseetal-CP06.pdf
2. http://www.meteo.psu.edu/~mann/shared/articles/Goosseetal-CP06.pdf
3. http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html
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8. http://www.met.psu.edu/dept/faculty/mann.htm
9. mailto:mann@psu.edu
10. http://www.met.psu.edu/dept/faculty/mann.htm

762. 1167928837.txt
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: not so fast - an update
Date: Thu, 04 Jan 2007 11:40:37 -0500
Reply-to: mann@psu.edu

<x-flowed>
sounds good Phil, I agree on the forecast. I think its at least
'plausible' ;)

by the way, please remind me what input you need from me at this point
on the Wengen paper. I've attached a review paper I've got in press in
"AREPS". Not sure if I sent this to you before. Its mostly a re-tread of
our '04 Rev Geophys review (which is getting lots of citations if you've
noticed!), but a little bit of newer stuff.

talk to you later,

mike

Phil Jones wrote:

>
> Mike,
>   I'm just beginning to notice this. I talked to AP about 5 hours ago.
> Our google search has noticed 150 in the last 3 hours.
> I checked one - can't recall whether it was Minneapolis of San Diego,
> but it read OK.
>
>   It's a trivial forecast. GW plus ENSO.
>
> Cheers
> Phil
>
>
>   I was hoping to put some of this background to the IPCC figure
> into the Wengen paper, but the more places the merrier.
>
>   By the way - when I'll send out a reminder.
>
> Phil
>
>
> At 16:19 04/01/2007, Michael E. Mann wrote:
>
>> by the way, 2007 to be warmest year headline getting a huge amount of
>> play in the U.S. media today,
>>
>> mike
>>
>> Phil Jones wrote:
>>
>>>
>>> Dear All,
>>>   The net is closing...
>>>
>>> National Research Council, US Committee for the Global Atmospheric
>>> Research Program, Understanding Climatic Change: A Program for Action,
>>> National Academy of Sciences, Washington, DC, (1975), appendix A.
>>>
>>> This book (Fig A2b) has the same figure as Imbrie/Imbrie. It is
>>> rotated.
>>> It also has the same concept of the IPCC 1990 Figure, changes on
>>> various timescales - all rotated. Loads of Lamb diagrams I have
>>> seen countless times before.
>>>
>>> This book also talks about the impending cooling.....
>>>
>>> John Mitchell also thought the figure is in a book by Gribbin
>>> called '1982 CO2 Review". Anyone recall that one. This isn't
>>> in the CRU Library nor UEA's.
>>>
>>> The direct source of the IPCC diagram is the UK Dept of Environment
>>> document from 1989 which is being posted to me. It though has
>>> a source, which isn't in the document. John and Geoff Jenkins
>>> wrote it though. It is possible that just the last millennium panel
>>> was from this source and the others from this 1975 source.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>>

>>>
>>>
>>> Dear All (Tom is off to Texas),
>>>   David Warrilow has found the said report. A photocopy is being
>>> posted
>>> to me, and two others have been asked if they know more about how
>>> it was arrived at.
>>>
>>>   I'll report more when I get news.
>>>
>>> Phil
>>>
>>> Tom,
>>>   Here's a reply from David Warrilow (below). I still think it is
>>> in a UK Dept of the Environment report from 1988/89, as does
>>> Chris Folland, so have asked him to think a little more.
>>>   I've looked at the 1979 edition, and Figure 45 is the one.
>>> It has a curve, but with the 20th century warmer than the
>>> MWP!! It is said to be based on Lamb (1969). This is a
>>> chapter in the World Survey of Climatology Series
>>> edited by Landsberg. I can't see how you can adapt anything
>>> from this. Hubert's chapter has lots of detail, many figures
>>> which have lines with the phrase 'analyst's opinion' - one
>>> of his favourite terms for things he made up. If it is an
>>> adaptation, then it comes from Hubert's ideas about
>>> England and NW Europe, because these are the curves
>>> in the 1969 chapter.
>>>
>>>   Anyone have the 1986 edition, to see if this curve got changed?
>>> The 1986 date is about right for being in the document I recall
>>> seeing. Some of you who've seen my room, will be saying if I had
>>> a better filing system, then I would be able to find it. Despite
>>> keeping
>>> most things I can't find this !
>>>
>>>   By the way, it is GREAT PITY, the First IPCC report didn't use
>>> Fig 45. We'd all be very happy and the skeptics wouldn't be going
>>> on about what came out in 1990.
>>>
>>>   Attached is the Met Office forecast for 2007. It seems that I'm
>>> getting
>>> the credit for this in the media. All I did was talk to the
>>> Independent about
>>> what I thought 2007 had in store weatherwise. With an El Nino going
>>> on,
>>> I thought it might be a record and just trotted off the typical
>>> things that happen
>>> in El Nino years.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>> Phil,
>>>
>>> I can't be sure but I think the original diagram is from Imbrie and
>>> Imbrie :
>>> Imbrie, John and Katherine Palmer Imbrie. Ice ages: Solving the
>>> Mystery. Cambridge, Massachusetts: Harvard University Press, 1979,
>>> 1986 (reprint). ISBN 0-89490-020-X; ISBN 0-89490-015-3; ISBN
>>> 0-674-44075-7. p. 25
>>>
>>> You may have it in your library. I am afraid I don't have it to hand,

>>>
>>> David
>>>
>>>
>>>
>>> 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
>>> NR4 7TJ
>>> UK
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>> Michael E. Mann
>> Associate Professor
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>> Department of Meteorology Phone: (814) 863-4075
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>>
>
> 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
> NR4 7TJ
> UK
> --

>

--
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</x-flowed>

Attachment Converted: "c:\eudora\attach\AREPS-preprint061.pdf"

763. 1167961271.txt
##########

From: William M Connolley <wmc@bas.ac.uk>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: not so fast - an update
Date: Thu, 4 Jan 2007 20:41:11 +0000 (GMT)
Cc: Phil Jones <p.jones@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, "Michael E. Mann" <mann@psu.edu>, "raymond s. bradley" <rbradley@geo.umass.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Eric Steig <steig@ess.washington.edu>, gschmidt@giss.nasa.gov, rasmus.benestad@physics.org, garidel@marine.rutgers.edu, David Archer <d-archer@uchicago.edu>, "Raymond P." <rtp1@geosci.uchicago.edu>

On Thu, 4 Jan 2007, Caspar Ammann wrote:
> check figure A9, there the 17th century is cold, and this is probably
> the curve that was used. In that case, then its Central England from Lamb.

Ah, you mean A9(d) (I thought you meant A9(a) for a bit). Yes, that looks pretty similar to IPCC 1990. Though not identical - the scaling is different, but the timing is similar.

-W.

> Caspar
>
>
> William M Connolley wrote:
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> >>   The net is closing...
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> >>   This book (Fig A2b) has the same figure as Imbrie/Imbrie. It is rotated.
> >>   It also has the same concept of the IPCC 1990 Figure, changes on
> >>   various timescales - all rotated. Loads of Lamb diagrams I have
> >>   seen countless times before.
> >>
> >
> > ? The source for IPCC can't be the 1975 NAS report. That fig is relatively warm
> > about 1600; the IPCC '90 figure is cold then. And as noted the "MWP" is colder
> > than 1950. But NAS 75 is the same as I+I, true (they both source to Lamb 69).
> >
> > Incidentally my I+I says copyright 1979, seventh printing 1998.
> >
> > -W.
> >
> > William M Connolley | wmc@bas.ac.uk | http://www.antarctica.ac.uk/met/wmc/
> > Climate Modeller, British Antarctic Survey | (01223) 221479
> >
> > --
> > This message (and any attachments) is for the recipient only. NERC is subject
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> > the Act. Any material supplied to NERC may be stored in an electronic
> > records management system.
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> --
> 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
>

William M Connolley | wmc@bas.ac.uk | http://www.antarctica.ac.uk/met/wmc/
Climate Modeller, British Antarctic Survey | (01223) 221479

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764. 1168014304.txt
##########

From: Melinda Tignor <tignor@ucar.edu>
To: Melinda Tignor <tignor@ucar.edu>, Kevin Trenberth <trenbert@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Peter Lemke <plemke@awi-bremerhaven.de>, Jurgen Willebrand <jwillebrand@ifm-geomar.de>, Nathan Bindoff <n.bindoff@utas.edu.au>, Matilde Rusticucci <mati@at.fcen.uba.ar>, Brian Hoskins <b.j.hoskins@reading.ac.uk>, zhenlin chen <cdccc@cma.gov.cn>
Subject: Re: Upcoming Observations Teleconference - Scheduling Request
Date: Fri, 05 Jan 2007 11:25:04 -0700
Cc: Susan Solomon <ssolomon@al.noaa.gov>, Martin Manning <mmanning@al.noaa.gov>

Greetings,
I have now heard back from all of you and the only date that will work for all of you will
be Monday, 8 January (that's Tuesday, 9 January for Nathan & Zhenlin). A small adjustment
to the time would be necessary to accommodate all of you. To ensure that we would have
enough time for everyone to participate in the entire call we would need to start 30
minutes earlier. So, that would be 12:30 MST/I9:30 UTC. I am going to hope that is ok and
move forward with establishing the call. Please let me know ASAP if that time adjustment
will NOT work for you. You will receive another email from me shortly with the details.
Please also let me know if the following contact information changes for you.

Susan Solomon	+1 303 497 3483
Martin Manning	+1 303 497 4479
Nathan Bindoff	+61 3 62262986
Kevin Trenberth	+1 303 497 1318
Matilde Rusticucci	+54 11 4797 4672
Phil Jones	+44 1953 605643
Brian Hoskins	+44 118 98411308
Peter Lemke	+49 5193 1458
Jurgen Willebrand	+49 431 688475
Zhenlin Chen	+ 86 10 68406146

Cheers,
Melinda

Melinda Tignor wrote:

Greetings,
I am contacting you to schedule the upcoming teleconference. Due to the extreme variability
in your time zones this will likely be a bit tricky and outside "normal" business hours for
some of you.
Please let me know as soon as possible your availability for the following times for the
week of 8 Jan - 12 Jan:
Nathan - 7:00 (Hobart)
Kevin - 13:00 (MST)
Matilde - 17:00 (Buenos Aires)
Phil, Brian - 20:00 (UK)
Peter, Jurgen - 21:00 (Germany)
Again, I realize that some of you would be most likely taking this call from home due to
the early or late time.
Many thanks in advance for your prompt response.
Cheers,
Melinda
--
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
Melinda M.B. Tignor
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Intergovernmental Panel on Climate Change
Working Group I Technical Support Unit
NOAA Chemical Sciences Division
325 Broadway DSRC CSD08
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Phone: +1 303 497 7072
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~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

References

1. mailto:tignor@ucar.edu
2. mailto:tignor@ucar.edu

765. 1168022320.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: William M Connolley <wmc@bas.ac.uk>,Caspar Ammann <ammann@ucar.edu>
Subject: Figure 7.1c from the 1990 IPCC Report

Date: Fri, 05 Jan 2007 13:38:40 +0000
Cc: Tom Crowley <tcrowley@duke.edu>,"Michael E. Mann" <mann@psu.edu>, "raymond s. bradley" <rbradley@geo.umass.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Eric Steig <steig@ess.washington.edu>,gschmidt@giss.nasa.gov, rasmus.benestad@physics.org,garidel@marine.rutgers.edu, David Archer <d-archer@uchicago.edu>, "Raymond P." <rtp1@geosci.uchicago.edu>,k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, "Mitchell, John FB \(Chief Scientist\)" <john.f.mitchell@metoffice.gov.uk>, "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>, "Warrilow, David \(GA\)" <David.Warrilow@defra.gsi.gov.uk>, Tom Wigley <wigley@cgd.ucar.edu>,mafb5@sussex.ac.uk, "Folland, Chris" <chris.folland@metoffice.gov.uk>

Dear All,

I've added a few extra names in the cc of this email list to see if we can definitively determine where the figure in the subject title comes from. The background is that the skeptics keep referring back to it and I'd like to prove that it is a schematic and it isn't based on real data, but on presumed knowledge at some point around the late 1980s. If you think it is based on something real.

What we'd like to do is show this either on 'Real Climate' or as background in a future paper, or both.

I'm attaching a few diagrams as background (attaching in order of introducing them) and giving some earlier thoughts. I assume you all have a copy of the said diagram in the first IPCC report.

1. This is where the IPCC diagram came from - the top panel is also there, but the middle one from IPCC isn't. This is where Chris Folland knows it came from. He said it was shoehorned in at a very late date. This report comes from a UK Dept of the Environment document - where the first edition predates 1990. David Warrilow says that this was written by Geoff Jenkins and John Mitchell.

John said the following

I think it was based on a diagram A2 in the national Academy of Sciences booklet "Understanding climate change" cirica 1974 if rmeber correctly- I can find out in Reading tomorrow- which I can't find in the library- it was reproduced in one of John Gribbens books and I think a book claled the "1982 CO2 review". I think there 6 diagrams and I remember Tom Wigley commenting that only the first (millions of years) and Last (instrumental record) had any credibility.

and

National Research Council, US Committee for the Global Atmospheric Research Program, Understanding Climatic Change: A Program for Action, National Academy of Sciences, Washington, DC, (1975), appendix A.

2. This 1975 book has the 3rd attachment on p130 . This is very similar to one that David Warrilow said (also attached from Imbrie and Imbrie - second attachment).

from David

I can't be sure but I think the original diagram is from Imbrie and Imbrie : Imbrie, John and Katherine Palmer Imbrie. Ice ages: Solving the Mystery. Cambridge, Massachusetts: Harvard University Press, 1979, 1986 (reprint). ISBN 0-89490-020-X; ISBN 0-89490-015-3; ISBN 0-674-44075-7. p. 25

These look the same if you invert and rotate the one from 1975, and they both say 'winter conditions in Eastern Europe' - well Imbrie/Imbrie do. They also say adapted from Lamb (1969). This is the World Survey of Climatology series from Landsberg, vol2. I've been through this and I can't see much of a plot anything like those I've attached, so some adaptation. Also I've no idea what this Eastern European series is!

The IPCC diagram and the UK report clearly don't originate here.

3. Caspar Amman had John Gribbin's 1982 book and sent the 4th attachment. This has a warmer MWP, but is far too cool recently. So even if this was resmoothed, it wouldn't before the IPCC one.
4. Ray Bradley sent this text:
I believe this graph originated in a (literally) grey piece of literature that Jack Eddy used to publish called "Earth Quest". It was designed for, and distributed to, high school teachers. In one issue, he had a fold-out that showed different timelines, Cenozoic, Quaternary, last 100ka, Holocene, last millennium, last century etc. The idea was to give non-specialists a perspective on the earth's climate history. I think this idea evolved from the old NRC publication edited by L. Gates, then further elaborated on by Tom Webb in the book I edited for UCAR, Global Changes of the Past. (This was an outcome of the wonderful Snowmass meeting Jack master-minded around 1990).
I may have inadvertently had a hand in this millennium graph! I recall getting a fax from Jack with a hand-drawn graph, that he asked me to review. Where he got his version from, I don't know. I think I scribbled out part of the line and amended it in some way, but have no recollection of exactly what I did to it. And whether he edited it further, I don't know. But as it was purely schematic (& appears to go through ~1950) perhaps it's not so bad. I note, however, that in the more colourful version of the much embellished graph that Stefan circulated (

[1]http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html
the end-point has been changed to 2000, which puts quite a different spin on things. They also seem to have fabricated a scale for the purported temperature changes. In any case, the graph has no objective basis whatsoever; it is purely a "visual guess" at what happened, like something we might sketch on a napkin at a party for some overly persistent inquisitor..... (so make sure you don't leave such things on the table...).
What made the last millennium graph famous (notorious!) was that Chris Folland must have seen it and reproduced it in the 1995 IPCC chapter he was editing. I don't think he gave a citation and it thus appeared to have the imprimatur of the IPCC. Having submitted a great deal of text for that chapter, I remember being really pissed off that Chris essentially ignored all the input, and wrote his own version of the paleoclimate record in that volume.

There are other examples of how Jack Eddy's grey literature publication was misused. In a paper in Science by Zielinski et al. (1994) [v.264, p.448-452]--attached-- they reproduced [in Figure 1c] a similarly schematic version of Holocene temperatures giving the following citation, "Taken from J. A. Eddy and R. S. Bradley, Earth-quest 5 (insert) (1991), as modified from J. T. Houghton, G. J. Jenkins, J. J. Ephraums, Climate Change, The

IPCC
Scientific Assessment (Cambridge Univ. Press, Cambridge, 1990)."
But I had nothing to do with that one!
So, that's how a crude fax from Jack Eddy became the definitive IPCC record on the last
millennium!
5. Finally, here's one from Stefan, to show how the IPCC diagram gets
(first another one which appears to be the IPCC 1990 diagram).
The one I want to attach seems to be within Stefan's email so that
is the end of this email. You can also get to this by going to the link
in Ray's piece above.
It shows how you can embellish a diagram and even get Rembrandt in!
I've also seen many other embellishments mentioning Greenland, the Vikings,
Vineyards in York, frost fairs on the Thames etc. Also I've emailed over
the years for the numbers in the 1990 IPCC Figure. I even got a digitized
version once from Richard Tol and told him what he'd done was
ludicrous.
6. So who put to together? Do we blame Ray? Is it a whim of his
excellent imagination? I know we will all likely agree with Ray that
it is based on absolutely nothing. Tom Crowley thinks it might be
based on Lamb and sent the final figure. Now all of those who are
or were in CRU know, you should be very careful with Lamb diagrams!
This one does not stand any scrutiny and there are several more
recent papers by Tom Wigley, Astrid Ogilvie and Graham Farmer
that have shown that this final diagram is irreproducible and it was
much cooler in the 11-13th centuries. It is also England and summer
only. The galling thing is, it does look like the IPCC Figure!!!!!!
When Tom sent the figure, he added this text (see below).
The figure looks like Figure 3O (I've not scanned this one), but will,
from his 1982 (reprinted in 1985 and 1995) called Climate History
and the Modern World. This figure has series for the year, JJA and
DJF.
Someone tell me it isn't based on a Lamb diagram, please....
Phil
Tom Crowley said
we still don't have an adequat explanation as to how Jack "cooked up" that figure - I do
not believe it was purely out of thin air - look at the attached - which I used in the
Crowley-Lowery composite just because it was "out there" - I made no claim that it was the
record of record, but just that it had been used beforer. the Lamb ref. is his book dated
1966. I will have to dig up the page ref later. Dansgaard et al. 1975 Nature paper on
Norsemen...etc used that figure when comparing what must have been their Camp Century
record - have to check that too - where the main point of that paper was that the timing of
Medieval warmth was different in Greenlandn and England!
25 years later my provocation for writing the CL paper came from a strong statement on the
MWP by Claus Hammer that the canonical idea of the MWP being warming than the present was
correct and that the 1999 Mann et al was wrong. he kept going on like that I reminded him
that he was a co-author on the 1975 paper! that is also what motivated to do my "bonehead"
sampling of whatever was out there just to see what happened when you added them all
together - the amazing result was that it looked pretty much like Mann et al. ther rest is
history -- much ignored and forgotten.

I might also pointn out that in a 1996 Consequences article I wrote - and that Fred Singer
loves to cite -- Jack (who was the editor of the journal) basically shoehorned me into
re-reproducing that figure even though I didn't like it - there was not an alternative. in
the figure caption it has a similar one to Zielinski except that it states "compiled by
R.S. Bradley and J.A. Eddy based on J.T. Houghton....so that puts a further twist on this
because it point to Houghton not Bradley/Eddy as the source. Jack must have written that
part of the figure caption because I don't think I knew those details.
but we still don't know where the details of the figure came from - the MWP is clearly more
schematic than the LIA (actually the detailsl about timing of the samll wiggles in the LIA
are pretty good) - maybe there was a meshing of the Greenland and the England records to do
the MWP part - note that the English part gets cooler. they may also have thrown in the
old LaMarche record - which I also have. maybe I can schlep something together using only
those old three records.
tom
Stefan said
the reason why I started to worry about this is the attached graph. Recognise something?
- Used in school teaching in Germany, Austria and Switzerland, is on a website with
officially recommended teacher materials
- Used in university teaching in Germany
- Used in politics in Germany by people within the FDP.
Note the vertical axis label on that, by the way. The text that goes with it claims the
medieval warm period was 2-4 ºC warmer than today.
Climate sceptics material, of course.
Cheers, Stefan
13a7140.jpg
--

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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Embedded Content: 13a7140.jpg: 00000001,00002e31,00000000,00000000 Attachment Converted:
"c:\documents and settings\tim osborn\my documents\eudora\attach\Global Climate Change.pdf"
Attachment Converted: "c:\documents and settings\tim osborn\my
documents\eudora\attach\Diagram - climate of the past 1,000 years.pdf" Attachment
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trends in global climate past million years.pdf" Attachment Converted: "c:\documents and

settings\tim osborn\my documents\eudora\attach\gribbin1982.jpg" Attachment Converted:
"c:\documents and settings\tim osborn\my documents\eudora\attach\IPCC1990.jpg" Attachment
Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\Lamb_ext.pdf"

References

1. http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html

766. 1168124326.txt
##########

From: "Rasmus Benestad" <rasmus.benestad@met.no>
To: <rahmstorf@ozean-klima.de>
Subject: Re: Figure 7.1c from the 1990 IPCC Report
Date: Sat, 6 Jan 2007 17:58:46 -0000 (GMT)
Reply-to: rasmus.benestad@met.no
Cc: <p.jones@uea.ac.uk>, <ammann@ucar.edu>, <wmc@bas.ac.uk>, <tcrowley@duke.edu>, <mann@psu.edu>, <rbradley@geo.umass.edu>, <steig@ess.washington.edu>, <gschmidt@giss.nasa.gov>, <rasmus.benestad@physics.org>, <garidel@marine.rutgers.edu>, <d-archer@uchicago.edu>, <rtp1@geosci.uchicago.edu>, <k.briffa@uea.ac.uk>, <t.osborn@uea.ac.uk>, <john.f.mitchell@metoffice.gov.uk>, <geoff.jenkins@metoffice.gov.uk>, <David.Warrilow@defra.gsi.gov.uk>, <wigley@cgd.ucar.edu>, <mafb5@sussex.ac.uk>, <chris.folland@metoffice.gov.uk>

I think that this story could possible catch on and make headlines, so I agree that we should be careful. But it's important that we bring the *true* picture out, and it is best that this is done by RealClimate rather than a sceptic site. The general scientific side of the IPCC report (i.e. all the peer-reviewed papers ad the scientific theories) is still sound, but to explain how *one* figure was shoe-horned into the report is harder to defend. The sceptics may argue that the IPCC reports are political after all, and this is also what it sounds like if governments 'hoisted the national flag' by having it's own figures inserted last minute. However, by providing an account of the 'evolution of the IPCC report writing', we could possibly give the story a softer landing. E.g. how many times of review the first report underwent as compared to the present report. We should also put this in perspective - the report is large and covers a wide range of topics, and most (all but our case?) is true to the science. There are sometimes a few rotten apples in a good batch, unfortunately. But the important part is that we don't accept rotten apples and that we sort it out! Forthcoming and up-front. Another important side is that this can provide a lesson for the scientific communities.

Rasmus

> Phil, I fully agree. The point is not to blame anyone at all - at least
> my point was to track down the source in order to be able to show the
> skeptics (or in my special case, the school authorities) that this old
> graph is completely superseded and should not be used any more in
> teaching! And I also see your problem: what we are finding out now makes
> the IPCC process look somewhat unsophisticated back in 1990, so it is a
>
> diplomatic conundrum how to be completely truthful in reporting this, as
> we need to be as scientists, without providing the skeptics undue
> fodder for attacking IPCC. But maybe we're too concerned - the skeptics
> can't really attack IPCC easily in this case without shooting

> themselves in the foot.
>
> Cheers, Stefan
>
> --
> Stefan Rahmstorf
> www.ozean-klima.de
> www.realclimate.org

--
Rasmus E. Benestad
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##########

From: Kevin Trenberth <trenbert@ucar.edu>
To: Susan Solomon <Susan.Solomon@noaa.gov>
Subject: Re: Science presentation for Paris
Date: Mon, 08 Jan 2007 15:31:18 -0700
Cc: Phil Jones <p.jones@uea.ac.uk>, mmanning@al.noaa.gov

   One too many 0's. 0.005.
   Kevin
   Susan Solomon wrote:

   Phil,

   Thanks. This comes up both in the presentation and in SPM language.

   A suggested merge of Phil's text below with the SPM language we have implies
replacing the
   sentence on page SPM-5, 6-7 with the following proposal:

   Sites affected by the urban heat island effect are identified and excluded from
these
   averages, so that remaining uncertainties due to this effect are negligible (less
than
   0.0005˚C per decade).

   This would address several comments asking us to explain what is done with UHI.

   OK?

   Susan

   At 3:52 PM +0000 1/8/07, Phil Jones wrote:

     Kevin, Susan,
        On the UHI (slide 9) we should probably change the middle bullet. The first
      and third are not in dispute. May be better to spell out SSTs though, or say
      marine air temperatures. SSTs are used as anomalies though to approximate
      MATs.
      Middle bullet currently says
     o Major influences are identified and excluded from the records used to create
the

continental and global values
Perhaps we should refer directly to David Parker's paper on UHIs, where he couldn't detect any difference in trends (averaged for 200+ cities) in temperatures
on calm nights (when you'd expect the biggest effect) compared to windy nights (when you'd expect the least).
There are two aspects to the major influences.
1. Some sites are removed. This isn't many as a % of the total (about 1%).
2. We include in Brohan et al (2006) an estimate of urbanization in the calculation of the errors. This is 0.0055 deg C/decade since 1900. It is a one-sided 'error'. If you look very closely the error range in this paper and in some of the Ch 3 figures is slightly one-sided.
This figure comes from Jones et al. (2001) , which came from Jones et al. (1990).
Difficulty with all UHI work is that there are countless papers looking at individual sites - which generally use a site in the city centre. This site is rarely one used in the dataset - generally an airport is instead. It is made worse by then looking at individual days and not monthly averages. Only Jones et al. (1990), Parker (2005,2006) and Peterson have looked at large scales.
So

Affected site are identified and excluded from the records used to create the continental and global values (as not all sites are tested, part of the error range
assumes an urban component of 0.0055 deg C/decade)
Cheers
Phil
At 22:47 07/01/2007, Kevin E Trenberth wrote:

Susan
Many thanks for the feedback. My comments and explanations follow. I'll expressly ask Phil to respond to us on the UHI issues and what we should say succinctly. I am keen to get further feedback on what to exclude. I had decided to exclude the full slide on all the regional precip trends becuase it is too detailed and would take too long to go through and so the zonal mean latitude-time series captures a lot of the changes. Personally I would like to have both but the issue will be time and simplicity of message, and hence my decision to drop the series: implicitly those are included of course because they are in the chapter.
> Kevin,
> Many thanks for the preview. I agree that the
> presentation has improved, thanks for that. I
> would like to offer the following suggestions:
>
> 1) Ramaswamy will cover radiative forcings, and
> will do so comprehensively including aerosols,
> ozone, etc. Calling out CO2 and N2O on your
> title slide will likely raise queries about why
> you cite those and not others. I suggest that
> you drop that bullet from your first slide.
Yes slide 1 at present is more comprehensive and perhaps more appropriate for you to use. In general with these slides that context will be desirable but perhaps not for Paris.
>
> 2) The chapter relates changes in DTR to clouds,
> and possibly aerosols and land use. The chapter
> doesn't explicitly say DTR changes are linked to
> dimming. While I personally would agree this is

> scientifically quite reasonable, your slide 8
> would be easier for people to understand and will
> avoid confusion if its language followed the

> chapter so replacing the word dimming on the
> slide with clouds, possibly linked to aerosols
> and land use, would be helpful.
I understand: indeed we did not expressly say "dimming" but in the discussion of dimming it clearly relates to clouds and aerosol. My thinking here is that some may well be aware of dimming but not of changes in clouds, so I thought that terminology might be helpful rather than add confusion. Other views appreciated.
>
> 3) Slide 9 says major influences of UHI are
> identified and excluded. Can this slide please
> be clearer as to what is meant by this and what
> exactly is done? I think it will benefit all if
> we avoid spending a lot of time explaining what
> 'major influences' are and what 'minor
> influences' aren't covered, how big those are,
> etc.
>
Let me ask Phil to suggest a couple of bullets.
> 4) A number of governments have asked for more
> clarity on where heavy precip has increased. You
> show it nicely in slide 16 but language on the
> slide will help us when the discussion of
> language comes up. In the extremes table we say
> that heavy precip has increased 'over most land
> areas' and if the title of this slide were
> 'Proportion of heavy rainfalls have increased
> over most land areas' that would be very helpful
> in laying ground for that.
Heavy precip is confusing, because some analysis are in absolute terms: and others are in terms f the percentage of precip that is heavy. The latter change is much more universal, and the main exceptions are where precip amounts have decreased, implying a drier regional climate. Since our report there is anew report in Science on extremes in India in the monsoon increasing and there they talk about real extremes. In the slide we already say "proportion of heavy rainfalls are increasing" so the suggestion is to add "most land areas"? OK.
>
> 5) What is the reference for slide 20? it's a
> nice image but if it's not in the report then
> we'll need to discuss that. Slide 19 covers
> similar content very well, I think so the second
> one on pdsi could be dropped.
Slide 20 is from Dai et al 2004. It is extensively discussed in the full report in section 3.3.4 and was featured in some email discussions for the TS related to the trend in the previous slide, resulting in some refinement of the FAQ 3.2. Whereas slide 19 is for all of PDSI, slide 20 separates out PDSI above and below a threshold of 3 and -3 and takes it apart to examine the precip and temperature contributions. It is quite complementary in that regard and shows more explicitlt that it is the dry spells that increased first from precip decreases and second from temperature effects.
>
> 6) The Emanuel (2007) slide is nice but that
> paper has not been assessed in our report. If
> you are seen by governments to be making your
> argument for the hurricane statement based on the
> Emanuel (2007) paper, we will almost certainly
> have challenges to the hurricane statement on
> procedural grounds -- which is not what we want
> to invite. Even though it is an update, it is
> substantially different from the published one
> that is assessed.

No that is not true. In our discussion in section 3.8.3 we note that the original Emanuel (2005a)set of curves was revised and discussed in Emanuel (2005b) in response to the comment by Landsea. But that response did not publish the revised curve; instead it appeared on Emanuel's website. It was that curve we discuss in the report (and the main reason we did not show it was because it had changed) and we say "the PDI increasing by about 75% (versus about 100%) since the 1970s (Emanuel 2005b)." The 100% was the original finding. Now there is a further minor refinement in the 2007 paper (in response to further complaints by Landsea, the corrections to the record to make the surface p and wind estimates compatible was not done at the highest wind speeds: very small changes) but an advantage is

that it is updated to include more years: through 2005. It is standard practice for obs time series to be updated and that is mainly what the new curve does. It is not at all at odds with what we discuss already.

>You can make a similar basic
> point using assessed material by putting one of
> the two Webster et al panels next to the SST
> trend in slide 27, highlighting the recent trends
> in both SST and intense storms with your nice
> animated ovals (and replacing the ACE figure,
> which uses non-satellite data). While the
> Webster figure itself wasn't explicitly in the
> chapter, the paper was referenced so I think that
> can be defended.

The SST curve though is for N Atlantic only and the Webster stuff is global. We could replace the ACE curve with the numbers curve from slide 28? With these explanations, I look forward to further suggestions.

>
> To respond to some of your other queries: I
> think slide 5 is better than slide 6 - showing
> all the data is nice. I agree with the idea of
> removing the Sahelian series.

Agree with both.

I suggest putting

> back the large-regions rainfall trends slide for
> several reasons (replacing the zonal mean time
> series figure with the trends figure). It is
> the trends figure that maps to the language in
> the SPM which is what we are trying to explain
> here - the zonal means are not what we explicitly
> talk about in the SPM. If you don't explicitly
> defend our SPM paragraph, then we certainly risk
> losing it or at best wasting a lot of time on it.

See comments above. I'll see if I can do something else.

> I also think the trends image is clearer for the
> non-expert than slide 15 showing the zonal means
> (although as you know I am a big fan of slide 15
> personally on a scientific level).
>
> There probably still are too many slides and it
> will be helpful if we all think hard about which
> of these is most needed. In cases where queries
> are from just one or two governments, or are more
> technical than they are likely to raise in the
> plenary, etc., it will be better to be shorter.
>

I look forward to comments from others as to which, if any, should be excluded. Of course I love them all.

> The comments make clear that we are going to be
> queried on the increases in heat waves statement
> as being too weak and only backed up in the FAQ.
> I personally like the European example but if you

> could also possibly put some text on that slide
> to help back it up more broadly, that will help
> to avoid challenges (please see the comments).
I included slide 22 which shows the shift in distribution of hot days and cold nights, and I thought this might be better than the Alexander et al maps. Again we run into too any slides. The change in hot days of course relates to heat waves, because the change in extremes relates to the whole pdf. The term heat waves is very subjective and the time scale is not always clear. There was a heat wave on east coast (New York 71F yesterday) although part of a month long warm period. The other main discussion of heat waves in our text is for Australia and I took out the slide of Australia temperatures vs precipitation in the first version (that Brian and Matilde have not seen). There is not much we can do here. The preponderance of evidence from all the statistics and studies demonstrates a clear increase in heat waves, even if there is not a definitive study just on heat waves. That is what we have to say.
Regards
Kevin
>
> I'll probably have more comments when we talk but I hope this is helpful.
> bests,
> Susan
>
>
> At 2:17 PM -0700 1/5/07, Kevin Trenberth wrote:
>>Hi all
>>I received some very helpful comments from
>>Jurgen and I have revamped the slides in the
>>light of the comments. I am cc'ing Matilde and
>>Brian as they are part of telecon. Please see
>>the attached. In all cases I have simplified the
>>presentation by placing the take home message at
>>the top. There are 30 slides here. At present
>>3 are hidden as possible alternates. Also some
>>should be dropped: your choice. The slides are

>>designed to address what was seen as the biggest
>>sources of misunderstanding in the comments on
>>the SPM.
>>The telecon will presumably discuss whether my
>>perceptions on that are the same as others.
>>
>>Slide 4 may now be somewhat redundant with the
>>added years on slide 2. Turns out the cleanest
>>separation is for top 8 years graphically, but
>>they do not include 1999 or 2000. Suggestions?
>>I made a new graphic of the land T vs SST
>>differences, and that is slide 6 but it could be
>>replaced by slide 5. Your choice.
>>I simplified slide 14 (on precip) and removed
>>the slide with all the time series.
>>I have cleaned up many others somewhat.
>>I would be inclined not to show the slide on the Sahel drought (21).
>>I added an extra new slide on hurricanes using
>>Kerry Emanuel's updated and corrected series.
>>So at present there are 5 slides on hurricanes
>>and at least 2 of those should be removed. The
>>Emanuel one has the advantage over the Webster
>>one of including SST. Of these only slide 27
>>includes figures from the chapter, yet I would
>>be inclined to drop that one. Your views on this?
>>
>>Slides 2 thru 12 are on aspects of temperature

>>13-16 and maybe 17 are on precipitation
>>17 to 21 are on drought
>>22 and 23 are on extremes and heat waves
>>24 and 25 deal with circulation and relations between T and precip
>>26 to 30 deal with tropical cyclones.
>>
>>To wrap up I repeated the first slide: and I
>>added a little piece to the first slide (I know
>>this will not make Susan happy, and I would not
>>include in Paris, but I thought it was funny).
>>Please view as slide show.
>>
>>That would leave about 24 slides. Some could
>>count as 1, e.g. 9 and 10 go together and would
>>take less than a minute. But I would guess a
>>minute average: order 25 minutes here.
>>Please do not use these slides at least until after the report is
>> approved.
>>
>>Regards
>>Kevin
>>
>>--
>>*****************
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>>
>>
>>Attachment converted: Discovery:C3IPCCParis.ppt (SLD3/«IC») (00377B45)
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References

1. mailto:trenbert@ucar.edu
2. http://www.cgd.ucar.edu/cas/trenbert.html
3. http://www.cgd.ucar.edu/cas/trenbert.html
4. mailto:p.jones@uea.ac.uk
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768. 1168353155.txt
##
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Melinda Marquis <marquis@ucar.edu>, Jonathan Overpeck <jto@u.arizona.edu>,
Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: AR4 Paleoclimate Teleconference
Date: Tue Jan 9 09:32:35 2007
Cc: chen zhenlin <chenzhenlin@hotmail.com>, czl <cdccc@cma.gov.cn>, Susan Solomon
<ssolomon@al.noaa.gov>, Martin Manning <mmanning@al.noaa.gov>

THis time is fine for me and the number you have is correct. Cheers
Keith
At 18:38 08/01/2007, Melinda Marquis wrote:

Dear Peck, Eystein and Keith,
Thank you for agreeing to meet this week (Thurs., Jan. 11) to discuss
paleoclimate
items. Martin will send you a follow-up email with an agenda to focus the
teleconference discussion.
In the meantime, if you would please confirm or correct the phone numbers where
you can
be reached, I would be grateful.
Jonathan Overpeck
Tucson, AZ, U.S.
9:00 a.m., Jan. 11 (Thurs.)
1 520 622 9065
Eystein Jansen
Bergen, Norway (Oslo-time)
5:00 p.m., Jan. 11 (Thurs.)
47 5558 3491
Keith Briffa
Norwich, U.K. (London-time)
4:00 p.m., Jan. 11 (Thurs.)
44 1603 593 909
____
Chen Zhenlin
Beijin, China [Please send phone for a midnight call.]
12 midnight Thurs.-Fri.
Cheers,
Melinda
--
Dr Melinda Marquis, Deputy Director, IPCC WG I Support Unit
NOAA/ESRL   Phone: +1 303 497 4487
325 Broadway, DSRC R/CSD08   Fax: +1 303 497 5628
Boulder, CO 80305, USA

--
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References

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

769. 1168356704.txt
##########

From: Caspar Ammann <ammann@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: That darned diagram
Date: Tue, 09 Jan 2007 10:31:44 -0700

Content-Type: text/plain; charset=ISO-8859-1; format=flowed
X-MIME-Autoconverted: from 8bit to quoted-printable by routt.cgd.ucar.edu id l09HVngh027823

<x-flowed>
Phil,
here the graphs from the Brooks 1949 (2nd edition) that we have at NCAR.
One is temperature the other precip reconstructions.
Caspar

Phil Jones wrote:
>
> Tom, Caspar,
> Keep the attached to yourself. I wrote this yesterday,
> but still need to do a lot more. I added in a section
> about post-Lamb work in CRU, but need to check out
> the references I've added and look at the extra one
> from 1981 that you've sent. This may take me a little
> time as I'm away Weds/Thurs this week. I see my name
> on an abstract, by the way, that I have no recollection of !
> I presume this has something in about instrumental global
> temps. This abstract isn't in my CV!!!!!
>
> So your point (3) needs to document that we knew the
> diagram wasn't any good, as well as how far back it goes.
> Knowing Hubert on some of his other 'breakthroughs!'
> it is clearly possible it goes back to Brooks !
>
> On the post-Lamb work in CRU, I recall talking to Graham
> (maybe mid-1980s) when he was comparing recent CRU work
> with Lamb - correlations etc. Did that ever see the light of day
> in these pubs or elsewhere? I will look. It isn't in the chapter
> Astrid and he wrote in the CRU book from 1997. I recall some
> very low correlations - for periods from 1100 to 1500.
>
> This is all getting quite complex. It clearly isn't something that
> should be discussed online on RC - at least till we know all
> the detail and have got the history right as best we can. A lot
> of this history is likely best left buried, but I hope to summarise
> enough to avoid all the skeptics wanting copies of these

> non-mainstream papers. Finding them in CRU may be difficult!
>
> As for who put the curve in - I think I know who did it. Chris may
> be ignorant of the subject, but I think all he did was use the
> DoE curve. This is likely bad enough.
> I don't think it is going to help getting the real culprit to
> admit putting it together, so I reckon Chris is going to get the blame.
> I have a long email from him - just arrived. Just read that and he
> seems to changing his story from last December, but I still
> think he just used the diagram. Something else happened on
> Friday - that I think put me onto a different track. This is all like
> a mystery whodunit.
>
> In the meantime - any thoughts on the attached welcome. Getting the
> level of detail required is the key.
>
> I need to do a better diagram - better scanning etc.
>
> Cheers
> Phil
>
>
> At 18:02 06/01/2007, Tom Wigley wrote:
>> Phil,
>>
>> I see the problems with this in terms of history, IPCC image,
>> skeptix, etc. I'm sure you can handle it. In doing so, you might
>> consider (or not) some of these points.
>>
>> (1) I think Chris Folland is to blame for this. The issue is not
>> our collective ignorance of paleoclimate in 1989/90, but
>> Chris's ignorance. The text that was in the 1990 report (thanks
>> for reminding us of this, Caspar) ameliorates the problem
>> considerably.
>>
>> (2) Nevertheless, 'we' (IPCC) could have done better even then.
>> The Rothlisberger data were available then -- and could/should
>> have been used.
>>
>> (3) We also already knew that the Lamb UK record was flawed.
>> We published a revision of this -- but never in a mainstream
>> journal because we did not want to offend Hubert. I don't have
>> the paper to hand, but I think it is ...
>>
>> Wigley, T.M.L., Huckstep, N.J., Mortimer, R., Farmer, G., Jones, P.D.,
>> Salinger, M.J. and Ogilvie, A.E.J., 1981: The reconstruction of European
>> climate on decadal and shorter time scales. (In) Extended Abstracts,
>> First Meeting, Reconstruction of Past Climates Contact Group, EEC
>> Directorate-General for Science, Research and Development, Brussels,
>> Belgium, 83-84.
>>
>> It could be ...
>>
>> Wigley, T.M.L., Farmer, G. and Ogilvie, A.E.J., 1986: Climate
>> reconstruction using historical sources. (In) Current Issues in Climate
>> Research (eds. A. Ghazi and R. Fantechi), D. Reidel Publishing
>> Company, Dordrecht, Netherlands, 97-100.
>>
>> The point of this paper (whichever one it is) is that it covers only
>> the decadal variation -- but it shows that Lamb was out to lunch
>> even on these time scales. As you know, this arose from his uncritical
>> use of historical sources -- a problem exposed in a number of CRU
>> papers in the 1980s, staring with Bell and Ogilvie in Climatic Change.

>>
>> So part of the issue is where did Hubert get the century time scale
>> changes in that diagram? The answer is, mainly from his own fertile
>> imagination. For this he tried to synthesize both his flawed historical
>> record for England (and records for Europe, equally flawed) and
>> proxy data from many sources, again accepted uncritically. Still,
>> there almost certainly was a LIA in Europe in the 17th/18th
>> centuries (but not in Iceland -- at least not in the 17th century).
>> Whether or not there was a significant centuries-long MWE is
>> doubtful in my view.
>>
>> On another historical note, Hubert got many of his ideas from
>> C.E.P. Brooks -- possibly Brooks's work is what inspired Hubert
>> to pursue his climate interests. Of course, he went a lot further
>> (too far) because he had a lot more information to work with.
>> However, it is interesting that Fig. 33 in Brooks (1928) looks a
>> lot like the IPCC90/Lamb Figure -- in Brooks the record goes
>> back further, and there is a very warm period from about 500
>> to 950AD.
>>
>> You should be careful about using "recovery from the LIA" to
>> explain warming after the Maunder Minimum. It is easy to show
>> with (e.g.) MAGICC that there is no such thing -- especially if
>> you accept the view on low-frequency solar forcing espoused
>> in the recent Foukal et al. paper in Nature. If you want some
>> support for this (i.e., the spurious recovery idea) I can send you
>> a diagram.
>>
>> Tom.
>>
>> C.E.P. Brooks, 1928: Climate through the ages. A study of the
>> climatic factors and their variations. Yale Univ. Press, New Haven,
>> 439 pp.
>>
>> [There is a cute item in this book that one never sees any more.
>> At the end of the last page it actually say "THE END".]
>
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> --

--
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Attachment Converted: "c:\eudora\attach\Brooks1949_PrecEurope.jpg"

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From: P.Jones@uea.ac.uk
To: "Brian Hoskins" <b.j.hoskins@reading.ac.uk>
Subject: Re: IPCC WG1 Observations Conference Call
Date: Wed, 10 Jan 2007 17:25:07 -0000 (GMT)
Cc: "Susan Solomon" <susan.solomon@noaa.gov>, "Kevin Trenberth" <trenbert@ucar.edu>, "Brian Hoskins" <b.j.hoskins@reading.ac.uk>, martin.manning@noaa.gov, "Matilde Rusticucci" <mati@at.fcen.uba.ar>, "Phil Jones" <p.jones@uea.ac.uk>, "Peter Lemke" <plemke@awi-bremerhaven.de>, "Jurgen Willebrand" <jwillebrand@ifm-geomar.de>, "Nathan Bindoff" <n.bindoff@utas.edu.au>, "zhenlin chen" <cdccc@cma.gov.cn>, "Melinda Marquis" <marquis@ucar.edu>

Dear All,
  Agree with Brian's new bullet. I still think we will get comments about what changes with storms. If this is going to lead somewhere we don't want it and cause problems, then the final part is likely best removed.

  Reading it again, better if we say .. since the 1960s. About is a little vague.

  Back in CRU on Friday. I may be able to get this hotel link to work tomorrow morning.

Cheers
Phil

> Dear All
>
> To me a headline should be kept simple with the detail in the bullets
> below, so I prefer the simple version with "aspects of extreme weather"
> but I guess I am outvoted on that!
>
> For the first part of the bullet on the westerlies I should prefer to
> revert to including the shift and also using the word strengthen rather
> than increase (a number, such as the speed, increases):
>
> Mid-latitude westerly winds have shifted polewards and stengthened since
> about the 1960s.
>
> The next part on the storms is problematic. I agree with Kevin that we
> should steer clear of the causal langauage Susan had used. However
> Kevin's words seemed to link a shift in the storm tracks with an
> increase in the winds. Also, as reviewed in 3.5.3, some papers suggest
> that, in addition to a poleward shift in the storm tracks and an
> increase in their average intensity, there is a decrease in the number
> of storms . This is probably too much for the bullet, so that a less
> specific version may be required.
>
> I think the whole bullet could be:
>
> Mid-latitude westerly winds have shifted polewards and stengthened since
> about the 1960s, with associated changes in storms. (3.5)
>
> Brian
>
>

> Susan Solomon wrote:
>
>> Thanks Brian and Kevin for the help.
>>
>> I agree with Brian about reversing the order in the headline sentence
>> but agree with Kevin that a separate bullet is most helpful. I
>> suggest we keep the headline short and simple and just leave the
>> language we have about wind patterns being one of several things
>> changing there. Otherwise it could be read as putting the circulation
>> change into a very high prominence in the headline which isn't quite
>> the emphasis we were discussing, I think.
>>
>> I tried to combine the suggestions and to keep things clear enough
>> that governments won't complain about lack of specifics. If you look
>> over the comments, you will have seen that above all they will not
>> tolerate vague language. Anybody who was in Shanghai (or any other
>> IPCC meeting) can attest to that so please please everybody help make
>> things as specific as we can.
>>
>> So my suggestion for the wind pattern bullet is:
>>
>> Mid-latitude westerly wind speeds have increased in both hemispheres
>> since about the 1960s. This has caused storm tracks to move towards
>> higher latitudes. {3.6}
>>
>> Regarding the headline that proceeds it, can we consider something
>> like this:
>>
>> At continental or ocean basin scales, numerous changes in climate have
>> been observed. These include sea ice extent, precipitation amounts,
>> ocean salinity, wind patterns, and [aspects of extreme weather] OR
>> [the frequency of heavy precipitation and of heat waves, the intensity
>> and duration of drought, and the intensity of hurricanes and typhoons.]
>>
>> The ice sheets have been taken out of the above because they are
>> moving to a consolidated sea level subsection, to deal with several
>> requests for that.
>>
>> Is the new option after wind patterns too specific? I am a little
>> concerned that we will be challenged on that. We could keep what we
>> have: 'aspects of extreme weather'. Equally, I am worried that they
>> will challenge the vagueness of 'extreme weather' so that is why you
>> see two alternatives here.
>>
>> Thoughts?
>> Susan
>>
>>
>> At 8:54 AM -0700 1/9/07, Kevin Trenberth wrote:
>>
>>> Hi Brian
>>> Do you need the first part? Are you rewriting the headline on SPM p
>>> 5 lines 35-37 or are you adding an extra bullet on circulation?
>>> I thought we agreed on the latter, but your piece seems more like the
>>> former.
>>>
>>> If we left the headline alone and added:
>>
>>> * Changes in large-scale atmospheric circulation are apparent
>>> and, in particular, the mid-latitude westerly winds have
>>> shifted polewards and strengthened, altering storm tracks.
>>>
>>> would be an alternative approach. I think it is helpful to mention

>>> storm tracks but not be specific about how they have changed.
>>> What do you think?
>>> Kevin
>>>
>>> Brian Hoskins wrote:
>>>
>>>> Susan
>>>>
>>>> Headline 2
>>>>
>>>> I suggest the following:
>>>>
>>>> At continental or ocean basin scale, numerous changes in climate
>>>> have been observed. Mid-latitude westerly winds (and the associated
>>>> storms) have shifted polewards and strengthened. Other climate
>>>> changes include precipitation,.....
>>>>
>>>> I have taken the suggestion form SPM_327 to reverse the order of the
>>>> first sentence.
>>>>
>>>> The westerly winds sentence is essentially that in a headline in the
>>>> TS.
>>>>
>>>> I should much prefer not to include the bracketed itallicised phrase
>>>> on storms. The evidence is less strong. There is some evidence for
>>>> reduced numbers of storms also but no room to say that. It was not
>>>> headlined in the chapter or the TS.
>>>>
>>>> Best wishes
>>>>
>>>> Brian
>>>
>>>
>>> --
>>> ****************
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771. 1168883146.txt
##
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: EGU
Date: Mon, 15 Jan 2007 12:45:46 -0500
Reply-to: mann@psu.edu

Cc: raymond s bradley <rbradley@geo.umass.edu>

thanks Phil,
not suggestion you not cite Wegman report, just suggesting you make sure the citation makes
clear what the report is...
mike
p.s. where/when did Tom Crowley use it?
Phil Jones wrote:

Mike,
Thanks.
On 1) Putting the last few years in zooms the CET curve much higher.
Tim took out the last few years. I need to make this clearer in the caption.
Padding is an issue with a 50-year smoother.
2) I agree Wegman isn't a formal publication. This was the highest profile
example I could come up to show abuse of the curve. if you know of any
others then let me know.
Even Tom Crowley shouldn't have used it. There is a belief in the UK, that
a curve of UK/CET past temperatures (by summer and winter) exists. It
doesn't, but the winter curve from Lamb is probably a lot better than the
summer one.
I'll let you know on time-frame when I hear from a few more I've sent the
piece to.
Cheers
Phil
At 14:10 15/01/2007, Michael E. Mann wrote:

Phil,
The attached piece is very good, impressive in the detail you've been able to dig up on
this. Won't pass this along.
A couple minor comments:
1. I understand the point of the 50 year smoothing, but I think it would still be very
useful to show were the most recent decade is on this scale. a lot of the recent warming
is washed out by the padding at the end. People will look at this and say "see medieval
peak was warmer than present". but that doesn't follow because so much of the warmning
has been over past two decades.
2. I would not reference Wegman report as if it is a publication, i.e. a legitimate
piece of scientific literature. Its a piece of something else! It should be cited in
such a way as to indicate it is not a formal publication, wasn't peer-reviewed, i.e.
could be references as a "criticism commissoned by Joe Barton (R, Exxon).
3. I think that Stefan/Gavin were hoping to do something on RC sooner than the timeline
you mention. What do you think about this? Do you want to forward the message to them
and tell them the timeline you have in mind?
talk to you later,
mike
p.s. thanks very much for the 'nomination' :), but you flatter me. I think that someone
farther along in their career such as Keith is more deserving at this time.
Phil Jones wrote:

Ray,
I have been nominating you for several years, as has Andre

and Jean - I think. Not sure how much the last two have been involved recently. I haven't been for a few years.

So, congratulations ! If as in previous years, you get asked about future awards, then consider nominating Keith and/or Mike. In the past it has alternated between ice cores and others.

As for a presentation, something on the lines of where we stand etc. will be great.

Gerard seems to be very flexible with the date for CL28. I've no idea how many abstracts there are yet. Haven't done anything on publicity for the session. Later in the week I'll check how many we have. So suggest the session day you want. Avoid Friday - people leave, also a bit on Thursday. Tuesday and Weds tend to have the most people there. I'll likely put you first in a session - not the early morning, but after coffee or lunch. I'll liaise with Gerard. I have to organize everything by next Monday as I'm at the IPCC in Paris from Jan 23 till Feb 2.

Can you two give me your thoughts on the attached? I think this is best in the Wengen meeting summary. Certainly after IPCC has met and likely after June when the chapters come out. Don't pass on to anyone an don't use in Vienna.

Cheers

Phil

PS Are you two getting loads of press cuttings from Mike Schlesinger?

At 18:25 13/01/2007, Michael E. Mann wrote:

Ray, I hadn't heard the announcement. This is wonderful news. You (like Phil) couldn't be more deserving for this.

I'm sorry that I won't be there (EGU comes at a bad time of the Penn State semester). I owe you a drink when next we meet.

Congratulations again!

mike

raymond s bradley wrote:

I was totally surprised to learn I was selected for the EGU's Oeschger medal this year--so if you had anything to do with that, many, many thanks. I knew Hans quite well and so this is especially meaningful for me. Phil got the first Oeschger Medal so I know I am following in his big shoes. But I can't help feeling it's all a clerical error somehow and a correction letter will appear any day now....

But, assuming this is not so...I was asked to give a talk aimed at a non-specialist audience in one of the sessions. I think your session on the last millennium is the obvious session in which to do this, so I will prepare something along the lines of "climate of the last millennium: status and prospect" so I can briefly summarise where we are at and what seems to be needed.

I'll submit an abstract on-line this weekend.

Ray

Raymond S. Bradley
University Distinguished Professor
Director, Climate System Research Center*
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< [1]http://www.paleoclimate.org>
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[10]http://www.met.psu.edu/dept/faculty/mann.htm

References

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2. http://www.geo.umass.edu/climate/paleo/html
3. mailto:mann@psu.edu
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5. mailto:p.jones@uea.ac.uk
6. mailto:mann@psu.edu
7. http://www.met.psu.edu/dept/faculty/mann.htm
8. mailto:p.jones@uea.ac.uk
9. mailto:mann@psu.edu
10. http://www.met.psu.edu/dept/faculty/mann.htm

772. 1168921050.txt
##########

From: Nathan Bindoff <n.bindoff@utas.edu.au>
To: Susan Solomon <ssolomon@al.noaa.gov>
Subject: Re: IPCC WG1 Observations ppt
Date: Mon, 15 Jan 2007 23:17:30 +1100
Cc: Kevin Trenberth <trenbert@ucar.edu>, Peter Lemke <Peter.Lemke@awi.de>, jwillebrand@ifm-geomar.de, Brian Hoskins <b.j.hoskins@reading.ac.uk>, Martin.Manning@noaa.gov, Matilde Rusticucci <mati@at.fcen.uba.ar>, Phil Jones <p.jones@uea.ac.uk>, zhenlin chen <cdccc@cma.gov.cn>, Melinda Marquis <Marquis@ucar.edu>, Nathan Bindoff <n.bindoff@utas.edu.au>

G'day Folks

Just to pick up on Susan's comment below, that I am interested in, and perhaps also richard alley in using parts of Peter's presentation for the sea-level rise issues....

Hope to have a new version by the close of tomorrow.

Cheers Nathan

On Fri, 2007-01-12 at 11:26 -0700, Susan Solomon wrote:
> Dear All,
> Thanks for looking and thinking about this.
>
> I should clarify that some of what Peter kindly put into his
> presentation may link to the sea level presentation, so may be better
> moved there. We should consider that carefully. I suspect that
> Peter was trying to avoid undue emphasis on Larsen B alone - because
> other places are showing similar things. So we should evaluate that
> too. While none of the figures themselves are explicitly shown in
> Figure 4 (including the Larsen B one), the material referenced is
> assessed there and Peter has carefully given the papers - so if we
> believe this is needed, it could be considered.
>
> I do like Figure 4.13 but think it would be clearer for this audience
> if it showed just the volume changes rather than the two panels. I
> understand why the technical expert likes both but for this audience
> perhaps just something showing the changes in glacier volume (SLR)
> would be clearer.
>
> bests,
> Susan
>
>
> At 9:49 AM -0700 1/12/07, Kevin Trenberth wrote:
> >Hi Peter

> >I am a bit alarmed about all of these slides as being too complex
> >and not using material from the chapters enough.
> >
> >For instance Fig 4.13 I found easy to understand but your first
> >slide is not easy: why is Europe in blue going up in a and level in
> >b when the glaciers are retreating? The reason is because this
> >shows the rate of change not the result of the change isn't it?
> >
> >In your second slide I do like the Larsen B ice shelf picture and
> >that provides a nice back drop for some explanation of the new
> >bullet (which is good). But why include the 3 panels on the left?
> >What do they add?
> >
> >I am not sure the next two are needed especially in their current
> >form. None of these are in the chapter. They add too much new
> >material. In my last ppt version I added some place holders taking
> >some figures from the chapter as they are part of the picture that
> >"global warming is unequivocal". I would urge you to include the
> >first two I had, plus one of yours based on the Larsen B slide but
> >with the message from the bullet added, or something like that.
> >
> >Regards
> >Kevin
> >
> >
> >
> >
> >Peter Lemke wrote:
> >>Dear Colleagues,
> >>please find enclosed a ppt-file addressing issues of Chapter 4.
> >>Slide 1: addresses SPM-312 and 314. I suggest to accept 312. The
> >>figure (4.15 from the chapter) indicates an increased rate of
> >>change after about 1990. But I do not think that we have an
> >>indication of an acceleration (continuously increasing rate of
> >>change).
> >>Slides 2,3 and 4: address the increased flow speed of tributary
> >>glaciers after retreat/thinning/loss of ice shelves or floating
> >>glacier tongues in Antarctica and Greenland (comments SPM-349 to
> >>353)
> >>
> >>I did not find any critical comments concerning snow, sea ice and
> >>frozen ground. Therefore I did not prepare any slides for theses
> >>topics.
> >>Best regards,
> >>Peter
> >>
> >>**************************************
> >>Please note my new e-mail address:
> >>
> >>Peter.Lemke@awi.de
> >>
> >>**************************************
> >>Prof. Dr. Peter Lemke
> >>Alfred-Wegener-Institute
> >>for Polar and Marine Research
> >>Postfach 120161
> >>27515 Bremerhaven
> >>GERMANY
> >>
> >>e-mail: Peter.Lemke@awi.de
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> >>FAX: ++49 (0)471 - 4831 - 1797
> >>http://www.awi.de

> >>**********
> >
> >--
> >**********
> >Kevin E. Trenberth e-mail: trenbert@ucar.edu
> >Climate Analysis Section, www.cgd.ucar.edu/cas/trenbert.html
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> >P. O. Box 3000, (303) 497 1318
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> >
> >Street address: 1850 Table Mesa Drive, Boulder, CO 80305
>

773. 1169050678.txt
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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: See the attached
Date: Wed, 17 Jan 2007 11:17:58 -0500
Reply-to: mann@psu.edu

<x-flowed>
Phil,

I've seen this junk already. Look at the co-authors! DeFrietas, Bob Carter: a couple of frauds. I dont' think anyone will take this seriously...

Do you have any advance knowledge you could pass along that would help us gear up to do something on RealClimate? I assume that there will be no surprises in the paleoclimate chapter, but I haven't seen the final draft. Any hints you can drop would be great...

thanks,

mike

Phil Jones wrote:

>
>> Mike,
>
> You've probably seen this. We are slated about p189/190.
> I hope this doesn't come up at the final IPCC meeting in
> Paris. I've nothing to worry about anyway. I wish they
> wouldn't keep going on about it.
>
> The press release after Paris from WG1, by the way will be Feb 2.
> You might like to gear up Real Climate for the week after. Only the
> SPM will be available then. The chapters come later as you'll know -
> I've heard June mentioned. CUP are doing them again.
>
> Cheers
> Phil
>
>
>
>
> 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
> NR4 7TJ
> UK
> ---

--
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http://www.met.psu.edu/dept/faculty/mann.htm

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774. 1169238969.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Susan Solomon <ssolomon@al.noaa.gov>, Susan Solomon <Susan.Solomon@noaa.gov>, Isaac Held <Isaac.Held@noaa.gov>, Ronald Stouffer <Ronald.Stouffer@noaa.gov>, peter lemke <plemke@awi-bremerhaven.de>
Subject: Re: Fwd: [Wg1-ar4-clas] Shorter presentations at Paris
Date: Fri Jan 19 15:36:09 2007
Cc: Melinda_Tignor <tignor@ucar.edu>, Martin Manning <mmanning@al.noaa.gov>, Melinda.Marquis@noaa.gov

Susan
This is very clear and very useful Thanks
Keith
At 15:21 19/01/2007, Susan Solomon wrote:

Keith, Peter, Isaac, Ron,
Thanks to all of you for helping out.
Keith, the audience for the presentations is the policy makers who will be present in
Paris. As you have already seen from the comments, many of them are not scientists. The
presentations need to be pitched at a non-scientist level. A number of the policy
people will be lawyers, and a number will be legalistically looking to find anything
that can advance their position. Most of them will however just be looking to ask
questions and to better understand, and many will be constructive in how they use the
information provided. So it is quite a mix. They should not be given input that
distracts from the job at hand. Therefore, these presentations should not bring in new

issues not raised in the comments, figures from material outside the report, etc.
I hasten to say that all of us hope there will not be big problems in going through the presentations. The presentations are being carefully prepared by excellent people, so my expectation would be for quite minor changes.
All of the above has been discussed with those preparing the presentations, so a primary role in co-chairing this session is to lend a constructively critical eye, seeking to advance the goal of clarity, conciseness, and sticking to the report rather than straying, if needed. The outcome is not a formal approval statement of the presentation. The outcome is to guide the collective subgroup to a *clear* consensus on what should be changed before the presentation is passed in to the TSU. If there are things that a majority of the group wants to see changed but others do not, you will have a chairman's job to do in finding a solution everyone can live with. It would probably be helpful if you could keep some notes on the agreed changes, since that will help you ensure that you have been clear enough in stating the conclusion. Too often there is a thrash and no closure. A good chair gets agreement with the group.
Thanks again,
Susan
At 1:00 PM +0000 1/19/07, Keith Briffa wrote:

Hi Susan et al
sorry for delayed response - just back from Paris (or so I originally thought as the meeting I was at turned out to be 3 hours away by train). I too am happy to act as you request, though I am still uncertain as to who the specific audience will be and more particularly, what you expect as an outcome of the session (a formal approval statement or recommendation for amendments?).
cheers
Keith
At 00:31 18/01/2007, Susan Solomon wrote:

Dear Peter, Isaac, Ron, and Keith
I am writing to let you know that the agenda for our C/LA meeting to take place in Paris on Saturday and Sunday Jan 27/28 will have your names listed for a proposed role, and I hope you will be able to accept.
At the end of the second day of the meeting, we will go over the set of longer 'science presentations' that will be given informally during the lunchtime sessions. There will be two parallel sessions from 4-6 pm on Sunday, and I am hoping that Peter/Keith can chair one dealing with drivers, obs, and paleo, whle Ron and Isaac can chair one on attribution/sea level/projections.
Earlier on Sat/Sun we will also have gone over the shorter formal presentations that will be used to start each section of the SPM during the meeting.

See below for some more information CLAs requested for preparation of the shorter
presentations.
An important point is that the short and long presentations should be consistent and
should strongly support the SPM approval process (see below).
We are seeking tough chairmen who could a) keep to a strict time schedule and avoid
slippage; b) ensure that a clear statement is made about what the group conclusion is
(e.g., if the group feels that a particular presentation should be changed, that needs
to be made clear to the person who will hand in the final presentation to the TSU); and
c) helps the group to focus on the need for these presentations to communicate with
policy people (not overly technical) and help address the comments received (not to
digress). In short, to be tough, fair, constructive, and well organized.
Thanks in advance for considering helping with this. If you feel you cannot do it, let
me know but I will assume silence is agreement to serve.
best regards,
Susan

Date: Mon, 15 Jan 2007 17:08:01 -0700
From: Susan Solomon <Susan.Solomon@noaa.gov>
To: wg1-ar4-clas@joss.ucar.edu
Cc: zhenlin chen <cdccc@cma.gov.cn>, Martin.Manning@noaa.gov
Subject: [Wg1-ar4-clas] Shorter presentations at Paris
X-BeenThere: wg1-ar4-clas@joss.ucar.edu
List-Id: <wg1-ar4-clas.joss.ucar.edu>
List-Unsubscribe:
<[1]http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>,
        <[2]mailto:wg1-ar4-clas-request@joss.ucar.edu?subject=unsubscribe>
List-Archive: <[3]http://lists.joss.ucar.edu/mailman/private/wg1-ar4-clas>
List-Post: <[4]mailto:wg1-ar4-clas@joss.ucar.edu>
List-Help: <[5]mailto:wg1-ar4-clas-request@joss.ucar.edu?subject=help>
List-Subscribe: <[6]http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>,
        <[7]mailto:wg1-ar4-clas-request@joss.ucar.edu?subject=subscribe>
Sender: wg1-ar4-clas-bounces@joss.ucar.edu
X-Brightmail-Tracker: AAAAAA==
X-Rcpt-To: <ssolomon@aztec.al.noaa.gov>
X-DPOP: Version number supressed
Dear CLAs,
We are writing to address the two types of presentations (shorter and longer) that are
to be given in Paris. A number of you have asked about the shorter presentations in
particular and we want to clarify that here.
We would like to ask the people who served as section coordinators for each section in
our TS/SPM meetings to coordinate pulling together the shorter presentations of not more
than 10 slides (Ramaswamy on drivers; Bindoff on observations; Hegerl on attribution,
Stocker on projections).
Many of you have kindly already sent around draft material for the longer science
presentations, and that has been very helpful. These will occur informally during lunch
breaks, or before the morning sessions at the plenary and will not be subject to

simultaneous translation. The most interested delegates will typically find these very
helpful, and will want to use them to ask you questions.
In addition, during the regular formal sessions and prior to presentation of each of the
major sections of the report (drivers, observations, attribution, and projections), we
will benefit from a very short presentation that introduces the section. The speaker's
words will be subject to simultaneous translation. We suggest that the paleo ice core
material be covered as part of the drivers, that the paleo observations be covered as
part of the observations, etc, to speed things up (we can switch speakers but keep
slides in the same file).
These shorter presentations are extremely important in setting the stage. They must be
very short. We will have an absolute limit of not more than 10 minutes, preferably 5
minutes for the shorter sections of the report namely drivers and attribution). Please
do not include more than a maximum of 10 slides. Questions will be strictly limited by
the session chair (Susan or Dahe) to matters of clarity (e.g., if an axis isn't clear).
We will go over both the shorter and the longer presentations jointly at our preparatory
meeting at the UNESCO center on Sat/Sun Jan 27/28 so please come prepared to do that. An
agenda for the preparatory meeting will be circulated to you shortly.
The shorter presentations can largely be derived from the longer ones. They will be
most helpful if:
- they do seek to provide a general sense of how the section is meant to fit
together and some key highlights.
- they present the figures and tables used in the SPM section to follow, but do
not include figures from the chapters unless absolutely essential. Including figures
from outside the report could create problems and should be avoided.
- they avoid raising new issues or suggesting changes from the distributed SPM.
As some of us have seen in the heated discussions via email about the MOC, sticking to
the agreed consensus obtained in the chapter teams is something our colleagues who will
not be in Paris would appreciate our doing as much as possible. We will need to agree
to all changes to be presented by us to delegates as a team in our preparatory meeting
on Jan 27-28. They will choose to seek more and that is what we will have to jointly
manage.
- they have very little text on them, as simple as possible.
- they do not try to cover each bullet.
You may wish to consider whether it is helpful to alternate speakers between your
science presentation and these short presentations, so that more of you get a chance to
speak.
Some of you asked for sample presentations. You are probably aware that we

completed a
special report on HFCs/ozone in 2005. The short presentation on our section (section 2)
at that session worked extremely well and is appended here as an example in case you
want to glance at it, along with the SPM itself. We had much less material to cover of
course and more time to do it (this is more than 10 slides but don't be tempted as that
was a different situation) but we hope this is still helpful.
We look forward to seeeing you and discussing all of the presentations on Jan 27-28.
Best regards,
Susan, Martin, and Dahe
_____
Wg1-ar4-clas mailing list
Wg1-ar4-clas@joss.ucar.edu
[8]http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

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

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

References

1. http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas
2. mailto:wg1-ar4-clas-request@joss.ucar.edu%3Fsubject=unsubscribe
3. http://lists.joss.ucar.edu/mailman/private/wg1-ar4-clas
4. mailto:wg1-ar4-clas@joss.ucar.edu
5. mailto:wg1-ar4-clas-request@joss.ucar.edu%3Fsubject=help
6. http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas
7. mailto:wg1-ar4-clas-request@joss.ucar.edu%3Fsubject=subscribe
8. http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas
9. http://www.cru.uea.ac.uk/cru/people/briffa/
10. http://www.cru.uea.ac.uk/cru/people/briffa/

775. 1169653761.txt
##########

From: Kevin Trenberth <trenbert@ucar.edu>
To: david.parker@metoffice.gov.uk
Subject: Re: 2006
Date: Wed, 24 Jan 2007 10:49:21 -0700
Cc: "Kennedy, John" <john.kennedy@metoffice.gov.uk>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

Ok that explains several things, I am so glad to know this before going
to Paris tomorrow. I made another minor tweak.
Kevin

david.parker@metoffice.gov.uk wrote:
> Kevin
>
> Thanks. The averages of the values in Fig 3.6 over 1961-1990 turned out
> not to be exactly 0.000 owing to missing data in the reference period (a
> perennial problem Phil is well aware of). But Susan (?) wanted the SPM
> curve to average exactly 0.000 in 1961-1990 so the values were shifted
> by somewhere between 0.02 and 0.03.
>
> Regards
>
> David
>
>
> On Wed, 2007-01-24 at 10:09 -0700, Kevin Trenberth wrote:
>
>> John and David
>> Thanks, I have updated the figure using your new low frequency curves,
>> and so I think 3.6 is now redone.
>> However I do not understand the other figure: the global value for T for
>> 2006 seems to be 0.46 not 0.42: it lies above half way between the
>> ticks. Again I have copied the low frequency curve and replaced the one
>> on our figure, but I don't understand the last point.
>> How do these look?
>> Kevin
>>
>> Kennedy, John wrote:
>>
>>> Kevin,
>>>
>>> I have attached updated versions of the diagrams so that you can see
>>> where the 2006 bars and dots should be moved to.
>>>
>>> John
>>>
>>> On Tue, 2007-01-23 at 14:48 -0700, Kevin Trenberth wrote:
>>>
>>>
>>>> David et al
>>>> For Fig 3.6 we need values for globe, NH and SH. I guessed at NH as
>>>> 0.55 and SH as 0.28. But not sure what the new error bars are. I
>>>> reduced them a bit from old ones but not as much as for last year.
>>>> Anyway, take a look at the attached. I also made a teeny extension of
>>>> the blue in each plot. Should I have done that or did the decadal curve
>>>> already include 2006?
>>>> This is what I can do. If you give me the correct error bars I can
>>>> refine a bit more.
>>>> Let me know
>>>> Kevin
>>>>
>>>> david.parker@metoffice.gov.uk wrote:
>>>>
>>>>
>>>>> Phil, Kevin
>>>>>
>>>>> The 2006 global annual average surface temperature anomaly wrt 1961-1990
>>>>> including December data is 0.42+-0.06C (1 sigma) and 2006 remains 6th.
>>>>> Slight upgrades to November and December land data are expected in due
>>>>> course, but this is the final number so far as IPCC is concerned.

>>>>>
>>>>> Regards
>>>>>
>>>>> David
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>

--
****
Kevin E. Trenberth e-mail: trenbert@ucar.edu
Climate Analysis Section, www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

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776. 1170724434.txt
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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Caspar Ammann <ammann@ucar.edu>, Ben Santer <santer1@llnl.gov>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, James Hansen <jhansen@giss.nasa.gov>
Subject: [Fwd: IPCC and sea level rise, hi-res paleodata, etc.]
Date: Mon, 05 Feb 2007 20:13:54 -0500
Reply-to: mann@psu.edu

Curt, I can't believe the nonsense you are spouting, and I furthermore cannot imagine why
you would be so presumptuous as to entrain me into an exchange with these charlatans. What
ib earth are you thinking? You're not even remotely correct in your reading of the report,
first of all. The AR4 came to stronger conclusions that IPCC(2001) on the paleoclimate
conclusions, finding that the recent warmth is likely anomalous in the last 1300 years, not
just the last 1000 years. The AR4 SPM very much backed up the key findings of the TAR The
Jones et al reconstruction which you refer to actually looks very much like ours, and the
statement about more variability referred to the 3 reconstructions (Jones et al, Mann et
al, Briffa et a) shown in the TAR, not just Mann et al. The statement also does not commit
to whether or not those that show more variability are correct or not. Some of those that
do (for example, Moberg et al and Esper et al) show no similarity to each other. I find it
terribly irresponsible for you to be sending messages like this to Singer and Monckton. You

are speaking from ignorance here, and you must further know how your statements are going to be used. You could have sought some feedback from others who would have told you that you are speaking out of your depth on this. By instead simply blurting all of this nonsense out in an email to these sorts charlatans you've done some irreversible damage. shame on you for such irresponsible behavior! Mike Mann -- 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 Return-Path: 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 160CA2D00B0 for ; Mon, 5 Feb 2007 19:53:22 -0500 (EST) Received: from web60817.mail.yahoo.com (web60817.mail.yahoo.com [209.73.178.225]) by tr12n04.aset.psu.edu (8.13.6/8.13.2) with SMTP id l160rCcf2019402 for ; Mon, 5 Feb 2007 19:53:12 -0500 Received: (qmail 49251 invoked by uid 60001); 6 Feb 2007 00:53:08 -0000 DomainKey-Signature: a=rsa-sha1; q=dns; c=nofws; s=s1024; d=yahoo.com;

h=X-YMail-OSG:Received:Date:From:Subject:To:Cc:In-Reply-To:MIME-Version:Content-Type:Conten t-Transfer-Encoding:Message-ID;

b=folyoWjSumv93mmwcsECLmtGDEGDd6Y3/mv2WavTLrekb/5qH8IhkAvbh8+QfRCfOALVKIAxeGEmhPVbFkhVMGOET

Ykx4oF2q6wyDIVXVl+BSd06vv8o6hjSKJ/M+li1R05sH7KOixpNoxvSdjQNCDt1US3zQI3bmCWA4epZNw8=; X-YMail-OSG: gSuRbqAVM1nhqat8Zt4GNlp5xY8qoAOh_P_TmtEgvuaLnZ0ixbR.Ev2V_eFEhTnCZQ-- Received: from [128.115.27.11] by web60817.mail.yahoo.com via HTTP; Mon, 05 Feb 2007 16:53:07 PST Date: Mon, 5 Feb 2007 16:53:07 -0800 (PST) From: Curt Covey Subject: IPCC and sea level rise, hi-res paleodata, etc. To: Christopher Monckton , Fred Singer Cc: Jim Hansen , mann@psu.edu, Clifford Lee In-Reply-To: <20061229145211.611FC1CE304@ws1-6.us4.outblaze.com> MIME-Version: 1.0 Content-Type: multipart/mixed; boundary="0-1893172854-1170723187=:47787" Content-Transfer-Encoding: 8bit Message-ID: <805971.47787.qm@web60817.mail.yahoo.com> X-Virus-Scanned: amavisd-sophos X-PSU-Spam-Flag: NO X-PSU-Spam-Hits: 0 Christopher and Fred,

Now that the latest IPCC WG1 SPM is published, I can venture more opinions on the above-referenced subjects.

It is indeed striking that IPCC's estimate of maximum plausible 21st century sea-level rise has decreased over time. The latest estimate is 0.5 meters for the A2 emissions scenario (not much higher from the 0.4 meter estimate for the A1B emissions scenario, which the Wall Street Journal editorial page has made much of). On the other hand, the IPCC

seems to have
taken a pass on Hansen's argument. The IPCC says their estimates are "excluding future
rapid dynamical changes in ice flow . . . because a basis in published literature is
lacking."
In this one respect (sea level rise) I agree with today's Journal editorial that the
science is not yet settled. Unfortunately, the editorial runs completely off the tracks
thereafter by (1) comparing 2006 vs. 2001 surface temperatures, among all the 150 or so
years on record, and (2) asserting a "significant cooling the oceans have undergone since
2003" based apparently on one published data-set that contradicts all the others. It is
not appropriate to cherry-pick data points this way. It's like trying to figure out
long-term trends in the stock market by comparing today's value of the Dow with last
Tuesday's value.
Re high-resolution paleodata, I never liked it that the 2001 IPCC report pictured Mann's
without showing alternates. Phil's Jones' data was also available at the time. Focusing
so exclusively on Mann was unfair in particular to Mann himself, who thereby became the
sole target of criticism in the Wall Street Journal etc.
It now seems clear from looking at all the different analyses (e.g. as summarized in last
year's NRC review by North et al.) that Mann is an outlier though not egregiously so. Of
course, like any good scientist Mann argues that his methods get you closer to the truth
than anyone else. But the bottom line for me is simply that all the different studies find
that the rate of warming over the last 50-100 years is unusually high compared with
previous centuries.
Summarizing all this, the latest IPCC does back off a bit from the previous one. It says
on Page 8, "Some recent studies indicate greater variability [than Mann] in
[pre-industrial] Northern Hemisphere temperatures than suggested in the TAR . . ." The
wording is perhaps insufficiently apologetic, but I find it hard to object strenuously to
it in light of the main point noted in the last paragraph.
If you want to discuss any of this further, let me know. I attach my latest presentation
-- and would appreciate seeing both Christopher's report mentioned in the Journal editorial
and Fred's comment on Rahmstorf's article published in Science last week.
Best regards,
Curt
Christopher Monckton <monckton@mail.com> wrote:

Dear Mr. Covey - Many thanks for coming back to me so quickly. You mention Hansen's recent
papers. I have recently been looking at an (attached) earlier projection of his - the
projection of temperature increase which he made to the US Congress in 1988, effectively
starting the "global-warming" scare. Updating his graph shows that annual global

mean land
and sea surface air temperature is not rising anything like as fast as his
attention-grabbing but now manifestly-misconceived Scenario A suggested. Indeed, it is
beginning to look as though temperature is beginning to fall below his estimate based on
CO2 having been stabilized in 1988. Morner, the world's leading authority on sea level, has
been very clear in saying there is very little evidence to justify the IPCC's sea-level
projections. The IPCC itself forecast up to 0.94m sea level rise in a century in its 1996
report; up to 0.88m in its 2001 report; and now 0.43m in its 2007 report. If one loosely
defines whatever t he IPCC says as the "consensus", then not only does the "consensus" not
agree with itself: it is galloping in the direction of the formerly-derided sceptics.

As to future world population, I did some research on this several years ago, because the
UN was making alarmist noises and this alerted me to the likelihood that we were being fed
political propaganda masquerading as science. I learned that the prime determinant of dP in
any population is the general level of prosperity in that population. As prosperity
increases, dP tends to zero. The prosperity factor is many times more potent as an
influence on dP than even enforced, artificial contraception or child-killing. Since I
expect world prosperity to increase in the coming century, I regard it as near-certain that
dP will tend to zero in the next half-century. The reason for the plummet thereafter is the
widespread availability and use of artificial methods of birth-control. The combined
effects of rising general prosperity and the general availability of artificial
birth-control on depressing indigenous population are already discernible in all those
Western European populations not having to cope with mass immigration from poorer
countries. In Russia, the indigenous population is falling so fast that Muslims will soon
form more than half the population.

As to the "hockey-stick" problem, the NAS report does state very clearly that, though the
conclusion of Mann et al. is "plausible", evidence going back more than 400 years before
the present is increasingly unreliable, and that very few reliable conclusions can be drawn
if one goes back more than 900 years. This illustrates one of the problems bedevilling the
climate-change question: too much of the data and processes on the basis of which we are
trying to draw conclusions are unreliable, incomplete or very poorly understood. This
should not deter scientists from trying to make increasingly intelligent guesses: but
anyone with diplomatic knowledge of the fast-emerging, fast-growing fast-polluters such as
China, India, Indonesia and Brazil will tell you that the ruling regimes in these countries

will not try to prevent their people from enjoying the fossil-fuelled economic growth we
have already enjoyed unless and until the science is honest, the uncertainties are admitted
and the case is strengthened by the accumulation of measurements and the improvement of
analytical techniques in the coming years.

Finally, you are right to take me to task for using words such as "rubbish" and "useless".
I apologize. That said, a validation skill not significantly different from zero indicates
that no valid scientific conclusion may be drawn from the "hockey-stick" graph.

----- Original Message -----
From: "Curt Covey"
To: "Christopher Monckton"
Subject: Sea level rise, hi-res paleodata, etc.
Date: Wed, 27 Dec 2006 15:05:51 -0800 (PST)
Dear Dr. Monckton,
Thanks for copying me on your correspondence with Fred and prompting me to look again at
IPCC sea level rise estimates for 2100. I agree you are comparing like-for-like. The
2001 report has an upper limit of 0.7 meters for the A1B scenario. If the 2007 report
lowers this to 0.43 meters (or if the number gets raised again before the report is made
final) it will certainly be appropriate to ask why. After reading Hansen's recent
papers, I don't see how to justify such small upper limits.
It also seems obvious to me (and apparently to you but not to Fred) that the A2 scenario
would entail more sea level rise than A1B. Regarding the relative likelihoods of
scenarios, I don't agree with you that it's "almost certain" that world population will
"plummet" in the second half of this century.
Regarding the issue of recent vs. earlier global warming, when I look at the totality of
data compiled by North et al. this year for their NAS / NRC report (see attached
graphic), it seems clear that most of the warming since about 1850 (or 1900) occurred in
recent decades. Going farther back in time, the data are of course more uncertain and
estimates vary, but it appears that the warming rate for the 20th century was unusually
high compared with the past 2000 years. This conclusion follows whether or not one
includes Mike Mann's data.
For the record, I must add that I do not share your characterization of Mann's work as
"rubbish" or "useless." Nor do I see a situation of "flagrant dishonesty in which the
UN and the scientific journals persist long after the falsity of their absurd and
extreme claims has been properly demonstrated."
Sincerely,
Curt Covey
Christopher Monckton <monckton@mail.com> wrote:

Dear Fred, - Many thanks for sending me this exchange. Some comments:

Temperature: This question, like so many others to do with supposed "climate change", is bedevilled by the recency of reliable, instrument-based observations. Nevertheless, some conclusions can be attempted. The Dalton Minimum is generally considered to have come to an end in 1910. The five-year mean global land and sea surface air temperature anomaly for 1908-1912, calculated from NCDC annual figures, was --0.3579K. By 1940 there had been a rapid increase of 0.4700K to +1121K. By 2004 (again taking the five-year average, including 2006) there had been a further increase of +0.4413K to +0.5534. The mean annual increase in the 30 years 1010-1940 was thus 0.0157K more than two and a quarter times greater than the 0.0069K mean annual increase in the 64 years to 2004. Mean global temperature has hardly risen at all in the five years since the IPCC's last report. And the fact of the 20th-century temperature increase tells us nothing of the cause. It is interesting, for instance, that the polar icecaps on Mars are receding, inferentially in response to increased solar activity. At any rate, it is certain that anthropogenic planetary warming is not responsible. It is possible, therefore, that most of the warming both before and after 1940 was heliogenic.

Sea level: Your correspondent does not disagree with my statement that the IPCC has revised its upper-bound estimate of sea level rise to 17 inches (0.43m). He says, however, that this upper bound is based on the A1 scenario, by which world population will peak in mid-century at ~9bn and fall thereafter. So was the 2001 report's upper bound of 0.88m. I was correctly comparing like for like. The Sunday Telegraph, which reported these figures, has been told that the revisions arise from "better data" now available to the IPCC, supporting skeptics' conclusions that the IPCC's figures are little better than exaggerated guesses. Morner (2004) concludes firmly that there is little evidence for sea level rising any faster now than it has in geologically-recent times. Your correspondent says that the A2 scenario is "business-as-usual": in fact, it is an extreme scenario regarded by very nearly all serious demographers as absurdly unrealistic, in that it posits an increase in world population to 15bn by 2100, when it is now almost certain that rising prosperity and the consequent decrease in birth rates will cause population to peak somewhere between 9bn and 10bn in mid-century, and plummet thereafter.

Reliability of the IPCC's reports: I understand that the IPCC's 2007 draft does not contain an apology for the defective "hockey-stick" graph, which the US National Academy of Sciences has described as having "a validation skill not significantly different from zero". In plain English, this means the graph was rubbish. It is difficult to have confidence in a body which, after its principal conclusion is demonstrated in the peer-reviewed, scientific literature and in numerous independent reports as having been useless, fails to make the appropriate withdrawal and apology. Worse, the UN continues to use the defective graph. This failure of basic academic honesty on the IPCC's part was the main reason why I began my investigation of the supposed climate-change "consensus".

The supposed scientific "consensus": Your correspondent seems unaware of the letter written by 61 Canadian and other scientists in climate and related fields to the Canadian Prime Minister. At the end of the attached commentary on Al Gore's recent attempt to rebut my articles on climate change in the Sunday Telegraph, beneath the references, I have appended the full text of the letter and the names, qualifications and then-current affiliations of all 61 scientists. Al gore and others tend to lean rather more heavily than is wise upon a single, rather bad one-page essay in Science for their contention that there is a scientific consensus to the effect that most of the warming in the past half-century was anthropogenic. The essay was by Oreskes (2004), who said that she had analyzed 928 abstracts mentioning "climate change" published in peer-reviewed journals on the Thomson ISI database between 1993 and 2003, and that none of the 928 had expressed dissent from the "consensus". Dr. Benny Peiser of Liverpool John Moores University subsequently made a more careful enquiry. Science had been compelled to publish an erratum to the effect that the search term used by Oreskes had not been the neutral "climate change" - which returned some 12,000 articles, but the more loaded "global climate change", which returned 1,117 articles. Of these, Dr. Peiser found that only 1% had explicitly endorsed the "consensus" as defined by Oreskes"; that almost three times as many had explicitly expressed doubt or outright disagreement; and that less than one-third had expressed explicit or implicit agreement with the "consensus". He wrote a paper for Science pointing out these serious defects, which pointed to a conclusion diametrically opposite to that of Oreskes. Science at first asked him to shorten his paper, and then said that, because conclusions like his had been widely reported on the internet, his paper would not be published. As far as I can discover, Science has not published any corrigendum to this day, providing

further
confirmation of what I have long suspected: that the leading peer-reviewed journals, having
unwisely taken strongly-political editorial positions on the question of climate change,
are no longer objective.

The need for honest science: It was only after years of increasingly-public pressure that
Nature was induced to oblige Mann et al., the authors of the useless "hockey-stick" graph
that starred in the IPCC's 2001 report, to publish a mealy-mouthed, partial and
unsatisfactory corrigendum. In such an environment of flagrant dishonesty in which the UN
and the scientific journals persist long after the falsity of their absurd and extreme
claims has been properly demonstrated, it is in my view unreasonable to expect China,
India, Indonesia, Brazil and other fast-polluting countries to deny to themselves the
fossil-fuelled economic growth which we in the West have been fortunate enough to enjoy.
Until there is honest science, no one will believe either the UN or the journals to the
extent of adopting the expensive and (on my calculations) probably futile remedial measures
which they and their supporters so stridently advocate. - Christopher

> ----- Original Message -----
> From: "S. Fred Singer"
> To: "Curt Covey"
> Subject: Re: Belated response to "Say You're Sorry"
> Date: Tue, 26 Dec 2006 08:37:25 -0500
> At 07:15 PM 12/18/2006, Curt Covey wrote:
>
> Received your 5 May 2006 e-mail via Andy Revkin last week. Regarding the Wall Street
> Journal and "other forums that substitute quips, showmanship, hyperbole, and conjecture
> for substantial discussion," the following recent quips from their Letters to the Editor
> may interest you:
> Fred Singer's claim (13 December) that "more than 70% of the warming observed since the
> end of the Little Ice Age in 1850 occurred before 1940, and thus before much
> human-emitted CO2." Fred has been saying this for a long time. I think it was true 20
> years ago. Up-to-date records (e.g. this year's NAS report from North et al.) show that
> much more than half the warming since c.1850 has occurred after 1940.
>
> Dear Curt, I am sure you are aware of the fact that such ratios depend entirely on
> the choice of time intervals. I don't want to quibble but surely the relevant fact
> is that most agree (incl IPCC -- but not Tom Wigley) that the pre-1940 warming was
> mostly due to natural causes.
>
> Lord Monckton's claim (13 December) that "The U.N. [presumably IPCC] is about to cut its

high-end estimate of sea-level rise in 2100 from three feet to just 17 inches." We are
not supposed to discuss IPCC reports before they become final, but the last draft I saw
does indeed project 17 inches (0.43 meters) of sea-level rise as the high-end climate
model estimate from Emissions Scenario A1B. The scenario itself, however, is one in
which (to quote IPCC) "global population peaks in mid-century and declines thereafter,
and the rapid introduction of new and more efficient technologies" has atmospheric CO2
leveling off by the end of the century. A business-as-usual scenario (like A2) would
give much higher sea-level rise by 2100.

I don't think so. But you will have to read my forthcoming response to Rahmstorf (in
SciencExpress). Meanwhile, peruse the attached.

Senator Inhofe's comment today (18 December) that "60 scientists" together with "Claude
Allegre, a leading French scientist who is a member of both the U.S. and French National
Academies of Sciences" have concluded that agreements like Kyoto are "unnecessary"
because "the cause of global warming is 'unknown.'" Presumably true, but so what?
Allegre is an award-winning geochemist; the other 60 scientists are unidentified. There
are tens of thousands of members of the American Geophysical Union alone (many of whom
are petroleum geologists). I'm sure you can find a few hundred to support any claim you
want to make about global warming.

I am one of the 60 -- and I am sure you know most of the other 59.
Best for 2007! Fred

S. Fred Singer, President
Science & Environmental Policy Project
1600 S. Eads St, #712-S
Arlington, VA 22202-2907
Tel: 703/920-2744
[1]http ://[2]www.sepp.org
<singer@SEPP.org>
Read about what is really causing warming
Unstoppable Global Warming : Every 1500 Years
(Natural climate cycles as seen in the geological record)
by S. Fred Singer and Dennis T. Avery
Rowman & Littlefield (2007) 260 pp. $25.00 plus $5 S&H
Send tax-deductible donations to SEPP
<< Supreme arguments2.doc >>

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 << nrc_2006_figS1.jpg >>

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Never Miss an Email
Stay connected with Yahoo! Mail on your mobile. [3]Get started! Attachment Converted:
"c:\eudora\attach\covey_glwarm_Feb07.pdf"

References

Visible links
1. http://www.sepp.org/
2. http://www.sepp.org/
3. http://us.rd.yahoo.com/evt=43909/*http://mobile.yahoo.com/services?promote=mail

Hidden links:
4. http://a8-asy.a8ww.net/a8-ads/adftrclick?redirectid=en-mail_a_01
5. http://a8-asy.a8ww.net/a8-ads/adftrclick?redirectid=en-mail_a_01

777. 1171550268.txt

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: EJ on hockey stick
Date: Thu Feb 15 09:37:48 2007

Thanks Eystein
the sceptic troupe are fading away
At 07:58 15/02/2007, you wrote:

> Hi Keith,
> I was asked about AR4 and the Hockey stick by a journalist. This was picked up by
> McIntyre´s blog.
> You can see the issue here: [1]http://www.climateaudit.org/?p=1131
> The last comment gives an Ok translation from Norwegian of what i said.
> Eystein
> _____
> Eystein Jansen, prof., Director
> Bjerknes Centre for Climate Research
> Allégaten 55
> N5007 Bergen
> phone: +47-55583491, fax. +47-55584330
> [2]eystein.jansen@geo.uib.no
> [3]www.bjerknes.uib.no

--
Professor Keith Briffa,
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Phone: +44-1603-593909
Fax: +44-1603-507784
[4]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.climateaudit.org/?p=1131

   2. mailto:eystein.jansen@geo.uib.no
   3. http://www.bjerknes.uib.no/
   4. http://www.cru.uea.ac.uk/cru/people/briffa/

778. 1171901402.txt
##

From: "thomas.c.peterson" <Thomas.C.Peterson@noaa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: [Fwd: Marooned?]
Date: Mon, 19 Feb 2007 11:10:02 -0500

Hi, Phil,

   I thought you might enjoy the forwarded picture and related
commentary below.

    I read some of the USHCN/GISS/CRU brouhaha on web site you sent us.
It is both interesting and sad. It reminds me of a talk that Fred
Singer gave in which he impugned the climate record by saying he didn't
know how different parts were put together. During the question part,
Bob Livzey said, if you don't know how it is done you should read the
papers that describe it in detail. So many of the comments on that web
page could be completely addressed by pointing people to different
papers. Ah well, you can lead a horse to water but you can't make it think.

    Warm regards,
             Tom

http://www.nature.com/nature/journal/v445/n7128/full/445567a.html

Nature 445, 567 (8 February 2007) | doi:10.1038/445567a

Editorial

"The IPCC report has served a useful purpose in removing the last ground from
under the sceptics' feet, leaving them looking marooned and ridiculous."

--
Thomas C. Peterson, Ph.D.
NOAA's National Climatic Data Center
151 Patton Avenue
Asheville, NC 28801
Voice: +1-828-271-4287
Fax: +1-828-271-4328

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779. 1172063883.txt
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From: Kevin Trenberth <trenbert@ucar.edu>
To: Melinda Marquis <marquis@ucar.edu>, Kristen Averyt <averyt@ucar.edu>
Subject: Re: Copy-edited Ch. 3 files
Date: Wed, 21 Feb 2007 08:18:03 -0700

Cc: Phil Jones <p.jones@uea.ac.uk>, Martin Manning <mmanning@al.noaa.gov>, Susan Solomon <ssolomon@al.noaa.gov>

Hi all
I have ftp'd the updated cleaned up files from chapter 3 back onto your ftp site. The
notes accompanying these are attached and are unchanged from yesterday. There are two
references that may not be quite final. These are from Global and Planetary Change and we
have doi's for them as they are published online, but no page numbers as they do not seem
to have appeared yet in print. By the way, there was one notable error in the copy editing
which was confusion over significance and confidence levels. I removed all the references
to confidence levels when it was about significance (of trends etc). I suspect this could
affect other chapters though, so you may want to check that carefully.
The main concerns we have are with the figures, please see the comments on the figure files
and the brief comments in the attached. If you would like me to make any of these changes
(Kristen) or assemble the panels, please let me know.
Regards
Kevin
Phil Jones wrote:

> Melinda et al,
> I'm happy with the chapter once all the mods - mainly to the figures
> - are undertaken. I won't get a chance this weekend, nor the next two days
> as I'm away. I might have some more time next week, but I too have spent about
> 6 hours on Sunday and another 2-3 hours on Monday. So Kevin can send
> back the accepted/tracked version of the chapter, the captions and Appendix 3.B.
> On the figures, will we get a chance to see the Chapter mocked-up with
> figures in their final positions and sizes - as we would do with journal papers?
> There are a number, which we'd like to check to make sure the colours
> are OK.
> I think by the way that you have caught all the spellings correctly. I noted
> 'fall' changing to 'autumn' and the doubling up of letters in words like
> 'modelling'. I hear also from Keith Briffa that Ch 6 now spells the word
> palaeoclimatic, although we normally drop the extra 'a' even in English
> journals.
> Cheers
> Phil
>
> At 23:14 20/02/2007, Kevin Trenberth wrote:
>
> Melinda
> Thanks
> Phil and I have made a preliminary pass through the material. As Kristen is now
> considering the figures, I have attached a preliminary list of the problems. This also
> includes some material for you: acronyms. More detail is given on the full figure
> file. We have left USA as is in the main text, but I note that the Appendix B was not
> copy edited and we have left "United States" there. We have accepted most other changes

even though I would not do them this way! We can send the material back now but I will
wait for a last check by Phil. (I spent over 12 hours on this over the weekend).
Kevin
Melinda Marquis wrote:

Hi, Kevin,
Thank you for reviewing your copy-edited chapter files -- thoroughly and promptly. I'll
try to answer each of your questions.
About the convention for referring to the United States: As this document is published
under the auspices of the United Nations, we are required to use official country names;
the United States of America is to be abbreviated as "USA" for such publications.
Regarding the lower case "antarctica": We have capitalized "Arctic" and "Antarctic"
when they are nouns, and have used lower case "arctic" and "antarctic" when they are
adjectives. We used the AMS Word List
([1]http://www.ametsoc.org/PUBS/Authorsguide/pdf_vs/authguide.pdf) to supplement our
style guide. The AMS list cites "arctic flow" (adj.) and "Arctic Circle" (noun). We
thought it appropriate to treat "antarctic" analogously to "arctic" (the adjectival
form).
About suggested revisions that seem pedantic: If you feel that inserting "the period"
before things like 1961 to 1990 would decrease clarity or change the meaning from what
is intended, then you may of course reject such changes.
Thank you for your careful review. Kristen will be replying to you about the figures.
Please let us know if you have further concerns. We want everything to be correct.
Cheers,
Melinda
Kevin Trenberth wrote:

Melinda
There appear to be changes that I do not agree with. For instance, everywhere we had
"United States" it has been changed to USA. That is not the practice in AMS or AGU
journals. I have also found several instances of Antarctic changed to lower case which
is surely not right!!!! Some changes are very pedantic: inserting "the period" before
things like 1961 to 1990.
Kevin
Melinda Marquis wrote:

Dear CLAs,
Thank you very much for your invaluable assistance during the recent SPM plenary
meeting. As you will realise there are a few remaining steps that need to be completed
before final completion of the WG1-AR4 but these should now be straightforward. This is
to ask for your help in the next of these steps which is to check the

copy-edited
version of your chapter.
A professional copy-editor has reviewed all chapters of the AR4 and made some
revisions. In most cases, her suggestions implement our style guide (see
attached) for
consistency in punctuation, spelling, grammar and language style across all
chapters,
points at which acronyms are spelled out, etc, etc. In a few cases, she has
suggested
revised wording for the sake of clarity, improved grammar or such. All these
changes
that might have some effect on the meaning of a sentence are shown in
track-changes
mode.
We would be grateful if you would now go through these edited chapter files and
either
accept, reject, or modify the copy-editor's tracked revisions and return
"cleaned up"
files to the TSU. During this step you should also:
* make any remaining necessary and minor corrections to text or tables;
* ensure that any corrections or updates provided to the TSU since the
distribution of
the final draft in October 2006, have been included;
* update references that have been published recently by inserting volume and
page
numbers, etc;
* add any adjustments to your chapter that arose from the SPM approval process
in Paris.
Please return a checked file to us with all tracked changes removed.
Please also remember to check your figures and figure captions
carefully including the axis labels, units used, etc. Annotated text
should already have been edited to follow the styles used in the text
where appropriate. In some cases we will be doing further improvements to
the text fonts used in figures but this is your last chance to ensure
that the wording is correct in all places. If you wish to make any small
revisions to figures, please contact Kristen Averyt
([2]averyt@ucar.edu) as soon as
possible.

Please remember that no substantive changes, or new references, can be made to
your
chapter at this stage.
The time line for delivering the camera-ready copy to the publisher is quite
tight. We
ask that you please return your final text and figures files to the TSU by
Friday, March
9.
You may access your chapter files at the following ftp site.
server: [3]ftp.joss.ucar.edu
account: wg1_gnrl
password: EQOKWOWG (Please note that these are zeros - not letters.)
directory: pub/AR4_CopyEditFinal/ChXX
The file names currently contain "_TSU." We ask that you change these
characters to
"_CLA" in the files you return to us. Finally please notify us at
[4]ipcc-wg1@al.noaa.gov when you have uploaded the checked files.
Best regards,
Melinda Marquis
--
Dr Melinda Marquis, Deputy Director, IPCC WG I Support Unit
NOAA/ESRL Phone:
+1 303 497 4487
325 Broadway, DSRC

R/CSD08 Fax:
+1 303 497 5628
Boulder, CO 80305, USA

--
****
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--
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--
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P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

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References

1. http://www.ametsoc.org/PUBS/Authorsguide/pdf_vs/authguide.pdf
2. mailto:averyt@ucar.edu
3. ftp://ftp.joss.ucar.edu/
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7. mailto:trenbert@ucar.edu
8. http://www.cgd.ucar.edu/cas/trenbert.html
9. mailto:p.jones@uea.ac.uk
10. mailto:trenbert@ucar.edu
11. http://www.cgd.ucar.edu/cas/trenbert.html

780. 1172776463.txt
##########

From: "Tim Osborn" <t.osborn@uea.ac.uk>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Re: ppt
Date: Thu, 1 Mar 2007 14:14:23 -0000 (GMT)
Reply-to: t.osborn@uea.ac.uk
Cc: t.osborn@uea.ac.uk

Here is the old version for you to compare with... the only noticeable difference is for the URALS/YAMAL region, which previously had a higher peak near 1000 AD. Although that was quite a big change, once you average it with the other two series, the overall mean series shows very little difference.

Cheers

Tim

On Thu, March 1, 2007 1:57 pm, Keith Briffa wrote:
> Tim
> am back and looking at this now
> thanks
> Keith
> At 12:23 01/03/2007, you wrote:
>>Hi again,
>>
>>please see the attached PDF file. I've not yet put it into powerpoint,
>>because I wanted to check whether it matches what you want, or if you
>> want
>>fewer lines on it etc.
>>
>>Each page is identical layout, for the 3 regions and then the 4th page is
>>for the average across all the data.

>>
>>On each page you have the scatter graphs (and correlation) between the
>>unfiltered and the 10-year smoothed TRW and summer temperature. Plus the
>>3 calibration lines (our normal regression in black, variance matching in
>>orange, and inverting the regression of TRW onto temperature in brown),
>>thin lines between unfiltered data and thick lines between 10-year
>>smoothed data. The solid blue scatter plot points are those used in the
>>1900-1990 calibration period, the blue circles with a cross in are from
>>outside the calibration period.
>>
>>The top panels show the full 2000-yr reconstructions, with the line
>> colour
>>and thickness coordinated to match the calibration lines in the bottom
>>panels. The only exception is that I have omitted the inverse regression
>>between unfiltered data (the line is shown dotted on the bottom left
>>panels), because this resulted in such huge variance that the curves went
>>way off the vertical scale!
>>
>>In this top panel, all series, including the instrumental (blue), are
>>50-year smoothed. In the Scandinavian panel, there's also the longer
>>Tornedalen summer temperatures overlaid in green.
>>
>>So... I can put each of these into a powerpoint slide.
>>
>>Easily, I could also repeat them for a shorter period and less smoothing
>>(e.g. 1500-present with decadal smoothing, or 1800-present with no
>>smoothing).
>>
>>I could also omit some of the curves if you think 5 reconstruction
>>alternatives per panel is too many.
>>
>>With slightly more time, I could make it so that the powerpoint built up
>>with 1 alternative reconstruction at a time, until all 5 were there.
>>
>>I'll call you soon and we can talk about it.
>>
>>Cheers
>>
>>Tim
>>
>>On Thu, March 1, 2007 10:17 am, Keith Briffa wrote:
>> > Hi Tim
>> > thanks
>> > I would be happy with only the usual regression but the plots with
>> > different timescales shown - for each and the average series would be
>> > great
>> > cheers
>> > Keith
>> >
>> >
>> > At 09:51 01/03/2007, you wrote:
>> >>Hi Keith -- I forgot to describe the contents of the PPT file I sent
>> >>yesterday. Basically it starts with a few comparisons of the modern
>> >>period between the MXD-based recons and the instrumental data.
>> >>
>> >>First 3 show data only up to 1960.
>> >>(1) Full MXD reconstruction
>> >>(2) Masked MXD reconstruction (masked by availability of instrumental
>> >> temps)
>> >>(3) Masked temperatures (masked by availability of MXD)
>> >>All with 5-year filter
>> >>
>> >>Then the same as above, except the next 3 show data up to 1995 to

>> >>illustrate the decline.
>> >>
>> >>Then a couple more repeating the above, masked MXD then masked
>> >>temperature, but this time without any time-filtering, so you can see
>> >>individual warm and cold years.
>> >>
>> >>Then finally the full MXD reconstruction back to 1400, but only up to
>> >> 1960.
>> >>
>> >>I'm working from home today. I'll redo the calibrated northern
>> Eurasian
>> >>stuff -- do you want all the options again (i.e. forward and inverse
>> >>regression, variance matching, pre-/post-calibration averaging of the
>> >>regions, low and high pass filtering?).
>> >>
>> >>Then we can make any final slides Friday morning if that's ok with
>> you!
>> >>
>> >>Cheers
>> >>
>> >>Tim
>> >
>> > --
>> > 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/
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> Professor Keith Briffa,
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>
> http://www.cru.uea.ac.uk/cru/people/briffa/
>
>

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

781. 1172871972.txt
##########

From: Valérie Masson-Delmotte <Valerie.Masson@cea.fr>
To: masson@dsm-mail.saclay.cea.fr

Subject: Re: IPCC final text
Date: Fri, 02 Mar 2007 16:46:12 +0100
Reply-to: Valerie.Masson@cea.fr
Cc: tordis.leroen@bjerknes.uib.no, Eystein Jansen <eystein.jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>, David Rind <drind@giss.nasa.gov>, Bette Otto-Bliesner <ottobli@ucar.edu>, joos <joos@climate.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
now for the Figures (this file is crashing my Word software systematically!)

Figure 6.3
OK with suggestion

Someone has to check the many comments on Figure 6.7 and 6.15 (Fortunat?)

Figure 6.9 : I cannot generate S and N latitudes, can someone of you edit the figure to generate positive latitudes?

Same for Figure Box 6.1, Figure 1 : they suggest to label the RH vertical axes but they have the same unit as the LH vertical axis. How should I proceed?

For Figure Box 6.3, 1 : should Olga reprocess it? (they ask for a change in caption)

FAQ should refer to Figure 1, Faq 6.1

APPENDIX

The definition of O-isotopes is partly false.
The isotopic composition of ice depends on temperature not because the fractionation coefficients depend on temperature but due to the progressive distillation of water masses en route for the poles. Even if fractionation coefficients were to be independent of temperature would one see a temperature / isotopic composition relationship.

All the best,

Valérie.

</x-flowed>

782. 1172963659.txt
##########

From: ottobli@cgd.ucar.edu
To: "Eystein Jansen" <eystein.jansen@geo.uib.no>
Subject: Re: AR4 Final Input Please check this mail
Date: Sat, 3 Mar 2007 18:14:19 -0700 (MST)
Cc: drind@giss.nasa.gov, "Bette Otto-Bliesner" <ottobli@ucar.edu>, "Fortunat Joos" <joos@climate.unibe.ch>, Valérie Masson-Delmotte <valerie.masson@cea.fr>, "Keith Briffa" <k.briffa@uea.ac.uk>, "Tim Osborn" <t.osborn@uea.ac.uk>, "Jonathan Overpeck" <jto@u.arizona.edu>, Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>

Dear all,

Below are my comments addressing issues to Section 6.4 and associated figures. It would be good if Fortunat can also check especially Box 6.2, 6.4 intro, 6.4.1.1, and 6.4.1.5 written by Dominique and Fortunat.

Bette

_____

Figures:

* Figure 6.3, Valerie has checked comments.

* Figure 6.4 and 6.7, Fortunat(?) should check the figures, legends, and comments.

* Figure 6.5, line 8: generally feedbacks in glacial-interglacial ...
  line 19: Simon Laplace Climate System Model (IPSL-CM) ...
  line 21: ECBilt-CLIO is not an acronym as far as I can tell.
* Figure 6.6, line 5: minimum ice thickness and extent ...
  line 6: Delete "at approximately 130 to 125 ka".
  line 9: ... and the ECHAM4 HOPE-G (ECHO-G) model ...

* Figure 6.8, Dick should check that the legend is revised correctly and that color code in this figure is consist with text.

_____

Text 6.4:

* Page 6-11, line 9: corresponding to other orbital periods ...

* Page 6-11, line 13: adopt Valerie's wording of last sentence.

* Page 6-11, line 37: the SOD has ~180 ppm and ~265 ppm. Is the change to < intentional in response to a review comment?

* Page 6-11, line 38: adopt Valerie's wording of sentence.

* Page 6-11, line 49: OK to delete redundant sentence.

* Box 6.1: See Valerie comments.

* Box 6.2: Changes look OK. Fortunat should check.

* Page 6-15, lines 49-50: ... to the very different conditions at the LGM.

* Page 6-16, line 2: PMIP-2 simulations ...

* Page 6-16, line 31: Change does not make sense. The PMIP2 models do not simulate changes of greenhouse gases or ice sheets. These are prescribed. This sentence could be revised to read: The PMIP-2 AOGCM simulations using glacial-interglacial ...

* Page 6-17, line 23: I am fine with Last Interglacial. Peck may also want to comment. The SPM uses the last interglacial period.

* Page 6-17, line 41: ... warming over Eurasia and in the Baffin Island/northern Greenland region ... I am OK with taking out "with sea ice retreat" at the end of the sentence if that is awkward.

* Page 6-17, line 43: Kaspar and Cubasch, 2006.

* Page 6-18, line 1: Models and data now show ...

* Page 6-18, line 6: adopt Valerie's wording of sentence.

* Page 6-18, line 38: D-O is one of the abbreviations in the literature so I am fine with this change. Need to be consistent and change Page 6-19, line 49 to D-O.

* Page 6-18, line 43: adopt Valerie's wording of sentence.

* Page 6-21, line 12: Dick can advise if ICE-4G, ICE-5G, and VM2 are acronyms.

* Page 6-21, line 20: Dick should be consulted to make sure sentence meaning is OK with changes.

* Page 6-21, line 43: Dick can advise if J stands for Joseph.

* Page 6-21, line 51: Dick should be consulted on color code in Figure 6.8 and consistency with text.

* Page 6-22, line 2: Replace "longer" with "older". Valerie please comment if this is a more correct wording. Note that neither Landais et al., 2003 or Suwa et al., 2006 are currently in the reference list. Eystein/Peck, please advise if it is possible to add new references. Valerie, could we use Landais et al, 2006, which is already in the reference list, here?

* Page 6-22, line 10: OK to spell out GIS

* Page 6-22, line 11: the growth ...

* Page 6-22, line 16: Peck, can you comment "if sea level rise during the LIG" is an OK edit.

* Page 6-22, lines 25-29: Possible combination of last two sentences. Peck, please check that this conveys your original meaning: Overpeck et al. (2006) argued ... significant retreat of the Greenland Ice Sheet (and perhaps also parts of the Antarctic Ice Sheet) can be expected to occur under this future condition (see also Scherer et al. ...).

_____

References:

Kaspar and Cubasch: published in 2006. editor order should be Sirocko, Claussen, Litt, and Sanchez-Goni. I couldn't find the location or page numbers for this publication.

NRC: OK

Otto-Bliesner: reversal of a and b OK

Peltier and Fairbanks, 2006: 25(23-24), 3322-3337.

Sarnthein: reversal of a and b OK

Taylor: OK

_____

FAQ 6.1 and 6.2 need to be revised to refer to the correct sections of Chapter 6. Most of the references are incorrect and look to be based on an old outline of the chapter.

_____

Appendix:

Need consistency with changes in chapter: Palaeocene, palaeosols(?)

--
Bette L. Otto-Bliesner
National Center for Atmospheric Research
1850 Table Mesa Drive
Boulder, Colorado 80305
Ph: 303-497-1723
Fax: 303-497-1348
Email: ottobli@ucar.edu

783. 1173300731.txt
##

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Hugues Goosse <hgs@astr.ucl.ac.be>
Subject: Re: 7RP / Environment (incl. Climate Change)
Date: Wed, 7 Mar 2007 15:52:11 +0100
Cc: k.briffa@uea.ac.uk

Dear Hugues,

I agree and what Damien said echoes what Keith is concerned about. We need to expand the
timescale of Millennium AND focus much more on sensitivity and predictability.

best wishes

Eystein

Den 7. mar. 2007 kl. 11.22 skrev Hugues Goosse:

Hi Eystein,
Thanks a lot for the information. I agree with you that it is very important that the topic "Earth system dynamics: Palaeoenvironmental analysis" includes explicetly our area of interest. By the way, I have briefly discussed with Damien Cardinal after the meeting yesterday. He tolds me that the EU has already funded recently a very big project over the last Millenium, so they will be reluctant to make a new call covering this subject but we can certainly sell our science in something more general like 'natural variability and climate predictability'.
All the best
Hugues
Le 15:00 06/03/2007, vous avez écrit:

Hi Keith and Hugues,
Here are two documents re. our discussion of FP7 topics.
As you will see the plan is to have the following topic out in 2008 or later: Â·
Earth system dynamics: Palaeoenvironmental analysis
I think it will be important that the topic really comes in 2008 and that it includes the terms natural variability and climate predictability when it is described in the call. If possible our national program committee members should be contacted to propose this. As far as I know there will be a meeting later this spring to discuss the next calls.
Cheers
Eystein
ï¿¼ï¿¼

_____
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Bjerknes Centre for Climate Research
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N5007 Bergen
phone: +47-55583491, fax. +47-55584330
[1]eystein.jansen@geo.uib.no
[2]www.bjerknes.uib.no

Hi Keith and Hugues,
Here are two documents re. our discussion of FP7 topics.
As you will see the plan is to have the following topic out in 2008 or later: ·
Earth system dynamics: Palaeoenvironmental analysis
I think it will be important that the topic really comes in 2008 and that it includes the terms natural variability and climate predictability when it is described in the call. If possible our national program committee members should be contacted to propose this. As far as I know there will be a meeting later this spring to discuss the next calls.
Cheers
Eystein

Content-Type: application/msword;
x-unix-mode=0644;
name=Articulating sub-activity 6 4 2.doc
Content-Disposition: attachment;
filename="Articulating sub-activity 6 4 2.doc"
Content-Type: application/msword;
x-unix-mode=0644;
name=wp topics 2008.doc

Content-Disposition: attachment;
filename="wp topics 2008.doc"

_____
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GOOSSE Hugues
[5]http://www.astr.ucl.ac.be/users/hgs/index.html
Institut d'Astronomie et de Géophysique G. Lemaître
Université catholique de Louvain , Chemin du cyclotron, 2
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e-mail: [6]hgs@astr.ucl.ac.be

_____
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References

1. mailto:eystein.jansen@geo.uib.no
2. http://www.bjerknes.uib.no/
3. mailto:eystein.jansen@geo.uib.no
4. http://www.bjerknes.uib.no/
5. http://www.astr.ucl.ac.be/users/hgs/index.html
6. mailto:hgs@astr.ucl.ac.be
7. mailto:eystein.jansen@geo.uib.no

784. 1173359793.txt
##########

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Richard Somerville <rsomerville@ucsd.edu>
Subject: Re: [Wg1-ar4-clas] Responding to an attack on IPCC and ourselves
Date: Thu, 8 Mar 2007 08:16:33 +0100
Cc: wg1-ar4-clas@joss.ucar.edu

Hi,

just a quick reply. I am in on this, and will respond to a draft letter, in the hope that
you will make the first, Richard? I agree that it can be short. It is strange to see this,
knowing that the delegations I spoke to in/after Paris clearly said that the CLAs got it
their way, and that I believe this is the strong common perception we also had as CLAs
about the outcome.

Best wishes,

Eystein

Den 8. mar. 2007 kl. 03.11 skrev Richard Somerville:

Dear Fellow CLAs,

The British magazine *New Scientist* is apparently about to publish several items critical of the IPCC AR4 WGI SPM and the process by which it was written. There is an editorial, a column by Pearce, and a longer piece by Wasdell which is on the internet and referenced by Pearce.

I think that this attack on us deserves a response from the CLAs. Our competence and integrity has been called into question. Susan Solomon is mentioned by name in unflattering terms. We ought not to get caught up in responding in detail to the many scientific errors in the Wasdell piece, in my opinion, but I would like to see us refute the main allegations against us and against the IPCC.

We need to make the case that this is shoddy and prejudiced journalism. Wasdell is not a climate scientist, was not involved in writing AR4, was not in Paris, and is grossly ignorant of both the science and the IPCC process. His account of what went on is factually incorrect in many important respects.

New Scientist inexplicably violates basic journalistic standards by publicizing and editorially agreeing with a vicious attack by an uncredentialed source without checking facts or hearing from the people attacked. The editorial and Pearce column, which I regard as packed with distortions and innuendo and error, are pasted below, and the Wasdell piece is attached.

My suggestion is that a strongly worded letter to New Scientist, signed by as many CLAs as possible, would be an appropriate response. I think we ought to say that the science was absolutely not compromised or watered down by the review process or by political presure of any kind or by the Paris plenary. I think it would be a mistake to attempt a detailed point-by-point discussion, which would provoke further criticism; that process would never converge.

Please send us all your opinions and suggestions for what we should do, using the email list [1]wg1-ar4-clas@joss.ucar.edu

I am traveling and checking email occasionally, so if enough of us agree that we should respond, I hope one or more of you (not me) will volunteer to coordinate the effort and submit the result to New Scientist.

Best regards to all,

Richard

Richard C. J. Somerville

Distinguished Professor
Scripps Institution of Oceanography
University of California, San Diego
9500 Gilman Drive, Dept. 0224

La Jolla, CA 92093-0224, USA

--

Here's the editorial that will appear in New Scientist on March 10.

Editorial: Carbon omissions

IT IS a case of the dog that didn't bark. The dog in this instance was the Intergovernmental Panel on Climate Change.

For several years, climate scientists have grown increasingly anxious about "positive feedbacks" that could accelerate climate change, such as methane bubbling up as permafrost melts. That concern found focus at an international conference organised by the British government two years ago, and many people expected it to emerge strongly in the latest IPCC report, whose summary for policy-makers was published in Paris last month.

It didn't happen. The IPCC summary was notably guarded. We put that down to scientific caution and the desire to convey as much certainty as possible (New Scientist, 9 February, p 3), but this week we hear that an earlier version of the summary contained a number of explicit references to positive feedbacks and the dangers of accelerating climate change. A critique of the report now argues that the references were removed in a systematic fashion (see "Climate report 'was watered down'").

This is worrying. The version containing the warnings was the last for which scientists alone were responsible. After that it went out to review by governments. The IPCC is a governmental body as well as a scientific one. Both sides have to sign off on the report.

The scientists involved adamantly deny that there was undue pressure, or that the scientific integrity of their report was compromised. We do know there were political agendas, and that the scientists had to fight them. As one of the report's 33 authors put it: "A lot of us devoted a lot of time to ensuring that the changes requested by national delegates did not affect the scientific content." Yet small changes in

language
which individually may not amount to much can, cumulatively, change the tone and message
of a report. Deliberately or not, this is what seems to have happened.

Senior IPCC scientists are not willing to discuss the changes, beyond denying that there
was political interference. They regard the drafting process as private. This is an
understandable reservation, but the case raises serious doubts about the IPCC process. A
little more transparency would go a long way to removing those qualms.

--

Here's the Pearce column:

Climate report 'was watered down'

* 10 March 2007
* From New Scientist Print Edition. [2]Subscribe and get 4 free issues.
* Fred Pearce

BRITISH researchers who have seen drafts of last month's report by the Intergovernmental
Panel on Climate Change claim it was significantly watered down when governments became
involved in writing it.

David Wasdell, an independent analyst of climate change who acted as an accredited
reviewer of the report, says the preliminary version produced by scientists in April
2006 contained many references to the potential for climate to change faster than
expected because of "positive feedbacks" in the climate system. Most of these references
were absent from the final version.

His assertion is based on a line-by-line analysis of the scientists' report and the
final version, which was agreed last month at a week-long meeting of representatives of
more than 100 governments. Wasdell told New Scientist: "I was astounded at the
alterations that were imposed by government agents during the final stage of review. The
evidence of collusional suppression of well-established and world-leading scientific
material is overwhelming."

He has prepared a critique, "Political Corruption of the IPCC Report?", which claims:
"Political and economic interests have influenced the presented scientific material." He
plans to publish the document online this week at [3]www.meridian.org.uk/whats.htm.

Wasdell is not a climatologist, but his analysis was supported this week by two leading
UK climate scientists and policy analysts. Ocean physicist Peter Wadhams of the
University of Cambridge, who made the discovery that Arctic ice has thinned by 40 per
cent over the past 25 years and also acted as a referee on the IPCC report,

told New
Scientist: "The public needs to know that the policy-makers' summary, presented as the
united words of the IPCC, has actually been watered down in subtle but vital ways by
governmental agents before the public was allowed to see it."

"The public needs to know that the summary has been watered down in subtle but vital
ways by governmental agents"

Crispin Tickell, a long-standing UK government adviser on climate and a former
ambassador to the UN, says: "I think David Wasdell's analysis is very useful, and unique
of its kind. Others have made comparable points but not in such analytic detail."

Wasdell's central charge is that "reference to possible acceleration of climate change
[was] consistently removed" from the final report. This happened both in its treatment
of potential positive feedbacks from global warming in the future and in its discussion
of recent observations of collapsing ice sheets and an accelerating rise in sea levels.

For instance, the scientists' draft report warned that natural systems such as
rainforests, soils and the oceans would in future be less able to absorb greenhouse gas
emissions. It said: "This positive feedback could lead to as much as 1.2 °C of added
warming by 2100." The final version does not include this figure. It acknowledges that
the feedback could exist but says: "The magnitude of this feedback is uncertain."

Similarly, the draft warned that warming will increase atmospheric levels of water
vapour, which acts as a greenhouse gas. "Water vapour increases lead to a strong
positive feedback," it said. "New evidence estimates a 40 to 50 per cent amplification
of global mean warming." This was absent from the published version, replaced elsewhere
with the much milder observation "Water vapour changes represent the largest feedback."

The final edit also removed references to growing fears that global warming is
accelerating the discharge of ice from major ice sheets such as the Greenland sheet.
This would dramatically speed up rises in sea levels and may already be doing so. The
2006 draft said: "Recent observations show rapid changes in ice sheet flows," and
referred to an "accelerating trend" in sea-level rise. Neither detail made the final
version, which observed that "ice flow from Greenland and Antarctica... could increase
or decrease in future". Wasdell points out recent findings which show that the rate of
loss from ice sheets is doubling every six years, making the suggestion of a future
decrease "highly unlikely".

Some of the changes were made at the meeting of government invigilators that finalised
the report last month in Paris. But others were made earlier, after the draft report was
first distributed to governments in mid-2006.

Senior IPCC scientists contacted by New Scientist have not been willing to discuss how
any changes took place but they deny any political interference. However, "if it is
true, it's disappointing", says Mike Mann, director of the Earth System Science Center
at Pennsylvania State University in University Park and a past lead author for the IPCC.
"Allowing governmental delegations to ride into town at the last minute and water down
conclusions after they were painstakingly arrived at in an objective scientific
assessment does not serve society well."

From issue 2594 of New Scientist magazine, 10 March 2007, page 10

--

--

<Wasdell_IPCC.pdf>

_____

Wg1-ar4-clas mailing list

[4]Wg1-ar4-clas@joss.ucar.edu

[5]http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

_____
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Wg1-ar4-clas@joss.ucar.edu
http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

References

1. mailto:wg1-ar4-clas@joss.ucar.edu
2. file://localhost/tmp/convertmbox32286.html
3. http://www.meridian.org.uk/whats.htm
4. mailto:Wg1-ar4-clas@joss.ucar.edu
5. http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas
6. mailto:eystein.jansen@geo.uib.no

785. 1173420319.txt
##########

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Ken Denman <ken.denman@ec.gc.ca>
Subject: Re: [Wg1-ar4-clas] draft to sign
Date: Fri, 9 Mar 2007 01:05:19 +0100
Cc: wg1-ar4-clas@joss.ucar.edu

Hi all,

it is in the middle of the night here, and I cannot provide much input to writing. Just
wished to say that I would be willing to sign on the draft as it is, but hope those writing
would consider the input from Susan and Kevin before submitting the final letter.

Eystein

Den 8. mar. 2007 kl. 22.56 skrev Ken Denman:

Hi Piers et al,

I have taken the liberty to suggest a few changes (with change tracker turned on) - while
you Europeans (oops, and Brits) at least are sleeping. And Piers and Richard, thanks a lot
for getting this moving quickly.

Regards, Ken

ps. Piers - my salary is paid by Fisheries and Oceans Canada. They are VERY uneasy when I
speak or write letters to the press, but they get really upset when I don't credit them
appropriately. C'est la vie.

[1]piers@env.leeds.ac.uk wrote:

Hi all

This is the latest draft with Jerry's and Ken's edits. However, in addition I've

deleted the para on the Paris meeting - as it was essentially repeated within

the last paragraph, and slightly reordered the other paragraphs

 Again please make further

edits. Also please could people approve the attachment of their name to such a

letter. Non highlighted names are people who appear to have already given

approval for their name to be used. If you are a yellow highlighted name I think

you are likely (or very likely) to sign!

If we could have a relaxed attitude and sign a letter that is still in the

process of being drafted it would save someone (me) a bunch of work at the end

collecting approvals

Cheers

_____

Wg1-ar4-clas mailing list

[2]Wg1-ar4-clas@joss.ucar.edu

[3]http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

--

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<NewScientist_2_Ken.doc>

_____

Wg1-ar4-clas mailing list

[6]Wg1-ar4-clas@joss.ucar.edu

[7]http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

_____

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N5007 Bergen
phone: +47-55583491, fax. +47-55584330
[8]eystein.jansen@geo.uib.no
www.bjerknes.uib.no

_____ Wg1-ar4-clas mailing list
Wg1-ar4-clas@joss.ucar.edu
http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

References

1. mailto:piers@env.leeds.ac.uk
2. mailto:Wg1-ar4-clas@joss.ucar.edu
3. http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

   4. mailto:ken.denman@ec.gc.ca
   5. http://www.cccma.bc.ec.gc.ca/~kdenman
   6. mailto:Wg1-ar4-clas@joss.ucar.edu
   7. http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas
   8. mailto:eystein.jansen@geo.uib.no

786. 1175952951.txt
##
##########

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
Subject: Re: urgent help re Augusto Mangini
Date: Sat, 7 Apr 2007 09:35:51 -0600
Cc: Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, Eystein Jansen
<eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
Hi Stefan - Valerie was the lead on the Holocene section, so I'll cc
her. I agree that your approach is the smart one - it's easy to show
proxy records (e.g., speleothems) from a few sites that suggest
greater warmth than present at times in the past, but our assessment
was that there wasn't a period of GLOBAL warmth comparable to
present. We used the term likely, however, since there still is a
good deal of work to do on this topic - we need a better global
network of sites.

Keith can comment on the last 1300 years, but again, I think there is
no published evidence to refute what we assessed in the chapter.
Again, one or two records does not hemispheric or global make.

I think Keith or Valerie could comment further if they're not
Eastering. Eystein, likewise might have something, but I think it is
his national responsibility to hit the glaciers over Easter.

Best, Peck

>Dear Peck and IPCC coauthors,
>
>- I know it's Easter, but I'm having to deal with Augusto Mangini, a
>German colleague who has just written an article calling the IPCC
>paleo chapter "wrong", claiming it has been warmer in the Holocene
>than now, and stalagmites show much larger temperature variations
>than tree rings but IPCC ignores them. What should I answer?
>
>One of my points is that IPCC shows all published large-scale proxy
>reconstructions but there simply is none using stalagmites - so
>please tell me if this is true?!! My main point will be the local
>vs hemispheric issue, saying that Mangini only provides local
>examples, while the IPCC statement is about hemispheric or global
>averages.
>
>But how about local variations - do stalagmites show much larger
>ones than tree rings? Any suggestions what other counter-arguments I
>could write? Do we have a stalagmite expert on the author team,
>other than contributing
>author Dominik Fleitmann, whom I've already identified?
>I have to submit my response to the newspaper tomorrow.
>
>Thanks, Stefan
>

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

Mail and Fedex Address:

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715 N. Park Ave. 2nd Floor
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fax: +1 520 792-8795
http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/
</x-flowed>

787. 1176225793.txt
##
##########

From: Susan Solomon <ssolomon@al.noaa.gov>
To: P.Jones@uea.ac.uk
Subject: Re: urban heat island - since 1950? or since 1900
Date: Tue, 10 Apr 2007 13:23:13 -0600
Cc: trenbert@ucar.edu, "Phil Jones" <p.jones@uea.ac.uk>

<x-flowed>
Phil
Thanks for your reply.   I have removed the
'since 1950' from the TS.   That was taken from
your ES but in view of this discussion I think
the reader needs to go to the chapter.

Please note that 'Since 1950' is not (and never
was) in the SPM, so there is no interplay at all
between the issues being discussed in this series
of emails and anything that occurred in Paris or
prior to Paris.

It was, of course, for you to decide what you
wanted in your ES and how to mesh that with the
main text of your chapter. It is entirely a
'within chapter' issue.

best regards,
Susan

At 4:30 PM +0100 4/10/07, P.Jones@uea.ac.uk wrote:
> Susan, Kevin,
>   See attachment, I realise this is an important issue,
>as this wil be one of the areas the skeptics will go over
> with a fine toothcomb. I'm happy either way - either
> with the since 1950 or without. I've explained why it is
> there.
>
>   I'm back in CRU tomorrow am. I'm also
> away on Sunday for the next 2 weeks, so if there is more
> to resolve, we need to do this by Friday.
>
> Cheers
> Phil
>
>
>> Kevin,
>> Thanks for thinking about this.   Based on the chapter referencing
>> Brohan and explicitly saying 1900 regarding the 0.006/decade figure
>> which is what is used as the bottom line, I wonder if this is a typo
>> and since 1950 should perhaps be since 1900 in your ES.
>>
>> The same thing occurs in the TS, and I am checking page proofs for
>> that which is why I got to wondering and checked back in chapter 3,
>> where I found this conundrum.   If it is correct as 1950, fine, but
>> it doesn't look like that to me.
>>
>> I'll wait to hear from Phil, hopefully tomorrow.
>> bests,
>> Susan
>>
>>
>> At 5:28 PM -0600 4/9/07, Kevin Trenberth wrote:
>>>Susan
>>>This is Phil's territory so I'll leave to him to follow up further. Are
>>>you suggesting that something should change? Seems to me that maybe
>>>removing the "(since 1950)" from ES might help?   I am on travel rest of
>>>the week.
>>>Kevin
>>>
>>>>   Kevin
>>>>   Thanks for your reply.
>>>>
>>>>   I am referring to the final distributed draft chapter, which was
>>>> before
>>>>   Paris.
>>>>
>>>>   Your ES pre-Paris (and post-Paris) says 1950 but this seems
>>>>   inconsistent with the text of your pre-Paris chapter, where the
>>>>   hemispheric and global values are given, and post-1900 is stated at
>>>>   that point. The value of 0.006 is clearly associated with post-1900
>>>>   in the text.
>>>>
>>>>   I don't think that this has anything to do with the clarifications to
>>>>   what was meant regarding UHI that were made in the SPM at Paris. The
>>>>   question is a lack of consistency in the pre-Paris chapter's ES and
>>>>   main text.
>>>>
>>>>   Please consult your final draft chapter and let me know.

>>>>
>>>> bests
>>>> Susan
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>
>>>> At 3:18 PM -0600 4/9/07, Kevin Trenberth wrote:
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>>>>>Phil is best to answer this. You may recall this was fiddled with
>>>>>after Paris and the values cited from 1900 were inserted at that
>>>>>stage based on one study. Earlier in the text you will see that
>>>>>most studies are from 1950 on: including those of Parker 2004, 2006,
>>>>>Li et al 2004, etc, and the DTR, Tmax and Tmin are given in Fig 3.2
>>>>>only after 1950; those are indicators also. So in the ES we refer
>>>>>to the several studies since 1950 but the value cited does indeed
>>>>>refer to the period since 1900. Phil would have to say whether
>>>>>this could be changed: certainly, with current wording it explicitly
>>>>>calls out the studies of the post 1950 period and would not be
>>>>>appropriate to change to 1900.
>>>>>
>>>>>My sense is that the awkwardness comes from the late edit.
>>>>>Kevin
>>>>>
>>>>>Susan Solomon wrote:
>>>>>>Kevin and Phil,
>>>>>>
>>>>>>In checking over some text, I noted a statement in your ES that UHI
>>>>>>effects are negligible, where since 1950 is indicated as the
>>>>>>temporal period of application. In the text of the chapter, it
>>>>>>looks more like 1900 to me. Should this be 1950, or 1900? or
> >>>>>something else?
>>>>>>
>>>>>>Thanks,
>>>>>>Susan
>>>>>
>>>>>--
>>>>>*****************
>>>>>Kevin E. Trenberth   e-mail: trenbert@ucar.edu
>>>>>Climate Analysis Section,   www.cgd.ucar.edu/cas/trenbert.html
>>>>>NCAR
>>>>>P. O. Box 3000,   (303) 497 1318
>>>>>Boulder, CO 80307   (303) 497 1333 (fax)
>>>>>
>>>>>Street address: 1850 Table Mesa Drive, Boulder, CO 80305
>>>>
>>>
>>>
>>>_____
>>>Kevin Trenberth
>>>Climate Analysis Section, NCAR
>>>PO Box 3000
>>>Boulder CO 80307
>>>ph 303 497 1318
>>>http://www.cgd.ucar.edu/cas/trenbert.html
>>
>>
>
>Attachment converted: Junior:urbanizationESTS.doc (WDBN/«IC») (00167B2F)

</x-flowed>

788. 1176251075.txt
##

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: p.jones@uea.ac.uk
Subject: Re: urban heat island - since 1950? or since 1900
Date: Tue, 10 Apr 2007 20:24:35 -0600 (MDT)
Reply-to: trenbert@ucar.edu

Phil
seems like we should do the same if we can in our galley proof.
Kevin

> Phil
> Thanks for your reply.   I have removed the
> 'since 1950' from the TS.   That was taken from
> your ES but in view of this discussion I think
> the reader needs to go to the chapter.
>
> Please note that 'Since 1950' is not (and never
> was) in the SPM, so there is no interplay at all
> between the issues being discussed in this series
> of emails and anything that occurred in Paris or
> prior to Paris.
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> It was, of course, for you to decide what you
> wanted in your ES and how to mesh that with the
> main text of your chapter. It is entirely a
> 'within chapter' issue.
>
> best regards,
> Susan
>
>
>
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> At 4:30 PM +0100 4/10/07, P.Jones@uea.ac.uk wrote:
>> Susan, Kevin,
>>   See attachment, I realise this is an important issue,
>>as this wil be one of the areas the skeptics will go over
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>>   I'm back in CRU tomorrow am. I'm also
>> away on Sunday for the next 2 weeks, so if there is more
>> to resolve, we need to do this by Friday.
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>> Cheers
>> Phil
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>>> Brohan and explicitly saying 1900 regarding the 0.006/decade figure
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>>> that which is why I got to wondering and checked back in chapter 3,

>>> where I found this conundrum.   If it is correct as 1950, fine, but
>>> it doesn't look like that to me.
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>>>>> post-1900
>>>>>   in the text.
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>>>>>   I don't think that this has anything to do with the clarifications
>>>>> to
>>>>>   what was meant regarding UHI that were made in the SPM at Paris.
>>>>> The
>>>>>   question is a lack of consistency in the pre-Paris chapter's ES and
>>>>>   main text.
>>>>>
>>>>>   Please consult your final draft chapter and let me know.
>>>>>
>>>>>   bests
>>>>>   Susan
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>
>>>>>   At 3:18 PM -0600 4/9/07, Kevin Trenberth wrote:
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>>>>>>>Susan
>>>>>>>
>>>>>>--
>>>>>>**********
>>>>>>Kevin E. Trenberth   e-mail: trenbert@ucar.edu
>>>>>>Climate Analysis Section,
>>>>>> www.cgd.ucar.edu/cas/trenbert.html
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>>>>>>
>>>>>>Street address: 1850 Table Mesa Drive, Boulder, CO 80305
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>>>>
>>>>
>>>>_____
>>>>Kevin Trenberth
>>>>Climate Analysis Section, NCAR
>>>>PO Box 3000
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>

_____
Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
http://www.cgd.ucar.edu/cas/trenbert.html

789. 1176746137.txt
##
##########

From: P.Jones@uea.ac.uk
To: "C G Kilsby" <c.g.kilsby@newcastle.ac.uk>
Subject: RE: Outputs from WG
Date: Mon, 16 Apr 2007 13:55:37 +0100 (BST)
Cc: david.sexton@metoffice.gov.uk, "Phil Jones" <p.jones@uea.ac.uk>, "Colin Harpham"
<c.harpham@uea.ac.uk>, "H J Fowler" <h.j.fowler@newcastle.ac.uk>

Chris et al,
I'll sedn some more thoughts on Thursday when back from the EGU. It is too hot in Vienna to sit through too many talks !

I suspect we need a subset of indices. The program will calculate all those recommended in various programs. One possibility is to keep them all and let users decide. We do need to make a series of checks though at some stage to make sure they are OK.

I think you'll have some fruitful discussions on some of these on April 24. I hope you can come to closure on a few things.

Cheers
Phil

> All:
>
> Indices
>
> I had a session with UKCIP last week, and we did get on to dicsussing
> what outputs might come out of WG (as well as DDP etc.) and the issue of
> indices derived from daily data (i.e. requiring time series) came up,
> with the distinct possibility of confusion/inconsistency as David
> mentions!
>
> I would be happy to produce indices only from WG, as long as we can
> check they are sensible first of course!
> E.g. heatwave duration (various thresholds), drought duration, various
> accumulations of rainfall ?
> Less clear cut might be gale days (definition?), snow days, proportion
> of days above temp threshold etc.
>
> I think we will need to consider the list in detail, as far as what is
> included (STARDEX list?), how they are calculated/validated and also
> whether they can be calcualted from some other source and found to be
> inconsistent.
> E.g. is it planned to take the (17?) RCM runs and analyse/release these
> indices as well ?
>
>
> Rainfall stats - pdfs
>
> I think (hope?) lag1-ac and skewness will actually be quite well behaved
> (if not realistic) even when you convert/downscale. The more
> validation/analysis we do of these fields the better anyway.
>
>
> Separate topic: measures of reliability
>
> May be a can of worms, but I think we need to address it sooner rather
> than later: UKCIP02 had subjective measures of reliability attached to
> different variables/predicted changes. We must do better, and a case in
> point is the WG where we sidestep the bias issue by using change
> factors. We therefore need to provide some measure (per grid square, per
> varaible?) of reliability.
>
> For example: if control annual rainfall is more than (say) 10% biased,
> reduce reliability measure and inform the user when generating.

> Problem 1: which model runs to use for this check?
> Problem 2: how to assess more complex measures e.g. annual cycle in
> rainfall/temperature?
> Problem 3: need a common, easily understood scale of reliability
> Furthermore - WG procedure introduces more uncertinty, e.g. for wind
>
> Thoughts?
>
> Cheers, Chris
>
>
>
>>-----Original Message-----
>>From: david.sexton@metoffice.gov.uk
>>[mailto:david.sexton@metoffice.gov.uk]
>>Sent: 16 April 2007 08:07
>>To: Phil Jones
>>Cc: david.sexton@metoffice.gov.uk; C G Kilsby; Colin Harpham
>>Subject: RE: Outputs from WG
>>
>>Hi,
>>
>>we will try for lag-1 correlation and skewness but an issue
>>for us is whether something doesn't work when we convert the
>>equilibrium pdfs to time-dependent ones or we downscale to 25km.
>>
>>As Phil has said that you can do all the derived indices
>>except gale days, if we could get a decision from the project
>>management team to cut those variables from MOHC list of
>>outputs without making any extra work for you, then that would
>>free up some time for us to investigate this further.
>>
>>Looking forward to seeing Colin's results on 24th.
>>
>>Cheers, David
>>
>>
>>
>>
>>On Fri, 2007-04-13 at 17:16 +0100, Phil Jones wrote:
>>>   Some more thoughts - keep in on the loop in case i get a chance
>>>   to respond from Vienna or next Thursday.
>>>
>>>   Phil
>>>
>>>
>>> At 16:32 13/04/2007, david.sexton@metoffice.gov.uk wrote:
>>> >Hi,
>>> >
>>> >On Fri, 2007-04-13 at 16:00 +0100, C G Kilsby wrote:
>>> > > Phil, David
>>> > >
>>> > > Briefly, and can respond fully next week when I have
>>>some more time!
>>> > >
>>> > > Some crucial points here,
>>> > > 1. the one re 90%ile of one variable not same as for
>>>other variables.
>>> > > Some simple restrictions need considering before diving off into
>>> > > full joint pdfs etc.
>>> > > Also, another dimension emerges with seasons, e.g. 90%ile winter
>>> > > rainfall, or 90%ile summer rainfall?
>>> >

>>> >Joint pdfs are just an issue for me in that I am giving you several
>>> >inputs to WG and they have to be consistent. For example, we are
>>> >finding we only get wetter summers for lower end of temperature
>>> >increases. Plus we already intend to provide sets of sampled values
>>> >for lots of variables that are consistent for any given point in
>>> >model parameter space.
>>>
>>> The joint pdfs are an issue for the WG as well. Not so much for
>>> Chris, but for us we have to reproduce the statistics for
>>> the other variables. Colin
>>> has solved the double counting issue for the means (for T etc),
>>> but we've yet to look at the variance.
>>>
>>> Colin should be able to show some of the results on the 24th
>>> as to how well the WG works. This fits the WG (with our rainfall
>>> component) to HadRM3 and then applies our modification
>>> technique to an A2 future (for comparison with the true RCM
>>> future for the 2070s). Sunshine is the only real problem.
>>>
>>> I don't think we need to repeat this with the NS rainfall,
>>> but discuss that once you've seen some preliminary results
>>> on the 24tjh.
>>>
>>>
>>> > >
>>> > > 2. Bit concerned to hear David talking of some precip
>>stats being
>>> > > secondary or optional - I would say mean, var and pdry days are
>>> > > all
>>> > > essential: from our experience autocorrelation and skewness are
>>> > > also pretty well behaved and we would rather have them
>>if at all possible!
>>> >
>>> >
>>> >Good. This discussion is throwing up a few discrepancies which need
>>> >clarifying. That some precip stats are of secondary
>>importance, is an
>>> >impression I was getting from Phil's earlier emails last month.
>>>
>>>
>>> I think there is some misunderstanding here. What I said earlier
>>> confirms what Chris has said - if they are available then Chris
>>> would like them. Chris will need to consider is they may be
>>> fully relevant due to the scale issue (25km squares vs points).
>>> Could be an issue for skew and r1.
>>>
>>>   Checking this out a la fitting directly to HadRCM3 control
>>> data might be useful here. See Colin's plots though before
>>> deciding.
>>>
>>>
>>>
>>> >I look forward to the fuller response next week. I will be mainly
>>> >away then which is why I raise these issues now. It would
>>be good to
>>> >have a good chat about them on the 24th.
>>> >
>>> >Cheers, David
>>> >
>>> > >
>>> > >
>>> > > Cheers,
>>> > > Chris

>>> > >
>>> > > >-----Original Message-----
>>> > > >From: Phil Jones [mailto:p.jones@uea.ac.uk]
>>> > > >Sent: 13 April 2007 15:46
>>> > > >To: david.sexton@metoffice.gov.uk
>>> > > >Cc: david.sexton@metoffice.gov.uk; C G Kilsby; Colin Harpham
>>> > > >Subject: Re: Outputs from WG
>>> > > > >
>>> > > > >
>>> > > > > David,
>>> > > > >   More thoughts embedded.
>>> > > > >
>>> > > > > Phil
>>> > > > >
>>> > > >At 15:12 13/04/2007, david.sexton@metoffice.gov.uk wrote:
>>> > > >>Hi,
>>> > > >>
>>> > > >>I think we have clarified or converged on most of my points. I
>>> > > >>have some comments on points 2 and 4.
>>> > > >>
>>> > > >>Cheers, David
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>On Fri, 2007-04-13 at 14:42 +0100, Phil Jones wrote:
>>> > > >> > >2. WG will produce 100 versions of 30-yr sequences for
>>> > > >>all (or just
>>> > > >> > >one?) WG variables for all months for a given combination
>>> > > >>of 30-yr
>>> > > >> > >period, emissions scenario and location.
>>> > > >> > >
>>> > > >> > >I am still not clear how to generate the 100.
>>Percentiles of
>>> > > >> > >PDFs is confusing me. I think Ag needs a clear procedure
>>> > > >>outlined by us
>>> > > >> > >for 24th. I think the easiest way to make WG
>>consistent with
>>> > > >> > >MOHC pdfs is the following (assuming I am correct so far):
>>> > > >> > >
>>> > > >> > >   a. User selects WG, 30-yr period, emissions
>>scenario and
>>> > > >> > location
>>> > > >> > (up
>>> > > >> > >to 1000km^2).
>>> > > >> > >   b. Work out which 25km x 25km box over UK is closest to
>>> > > >> > >this
>>> > > >> > multi-
>>> > > >> > >site location.
>>> > > >> > >   c. For the 30-yr period, emissions scenario and
>>location
>>> > > >> > >in b),
>>> > > >> > DDP
>>> > > >> > >internally produces a table of changes in mean T, %
>>> > > >>changes in mean
>>> > > >> > P,
>>> > > >> > >and changes in variance of P for each month for
>>100 randomly
>>> > > >> > >sampled different model variants. DDP ALREADY needs this
>>> > > >>capability.
>>> > > >> > >   d. So we have an internal matrix with 3*12=36
>>columns and

>>> > > >> > > 100
>>> > > >> > rows. WG
>>> > > >> > >loops through 100 rows, using each set of 36
>>numbers to drive WG.
>>> > > >> > User
>>> > > >> > >gets 100 WG's. Does what they like with it.
>>> > > >> >
>>> > > >> >
>>> > > >> >   Sort of. The 100 versions of the WG I was talking
>>> > > >about will all
>>> > > >> > have
>>> > > >> >   the same statistics. I thought these 100 would be from
>>> > > >one point
>>> > > >> > within
>>> > > >> >   the pdf (or the joint pdf) - say the 10, 50 or 90th
>>> > > >percentile. We
>>> > > >> > could make
>>> > > >> >   this percentile selectable.
>>> > > >> >
>>> > > >> >   The 100 (or 1 or whatever) are representative of some
>>> > > >> > future 30-year period.
>>> > > >> >   Your a) and b) are fine.
>>> > > >> >
>>> > > >> >   Another option is like yours. There is a pdf
>>(or joint pdf).
>>> > > >> > The 100 could be
>>> > > >> >   from each of the 100 percentiles? Does this make sense?
>>> > > >Or the 100
>>> > > >> > could
>>> > > >> >   come from sampling the percentile space assuming a normal
>>> > > >> > distribution?
>>> > > >> >
>>> > > >> >   Your 2) is an important aspect to sort out on the 24th.
>>> > > >>
>>> > > >>
>>> > > >>I agree that we need to discuss this but it would be good to
>>> > > >thrash it
>>> > > >>out a bit more before 24th. UKCIP08 needs the WG pdf to be
>>> > > >>consistent with the MOHC pdf. Your solution tries to
>>do this but
>>> > > >>a problem with selecting a percentile is that a model variant
>>> > > >>that is the 90th percentile for temperature is not
>>90th percentile for other variables.
>>> > > >>There is also a related issue about how you chose a model
>>> > > >variant near
>>> > > >>a given percentile. The solution I propose means these are
>>> > > >not issues.
>>> > > >>So we could sample M model variants and run N WGs for
>>each model
>>> > > >>variant. M has to be a good size to make sample
>>> > > >representative of MOHC
>>> > > >>pdf but N does not have to be large as internal variability
>>> > > >is already
>>> > > >>generated by using a different set of parameters and a
>>> > > >different seed for each WG.
>>> > > >>I think this solution is simpler than the percentile-based
>>> > > >solution. Do
>>> > > >>you agree?
>>> > > > >
>>> > > > >
>>> > > > > Sounds OK. Let's see what Chris thinks.
>>> > > > >
>>> > > > >

>>> > > >
>>> > > >
>>> > > >> > >4. Phil has mentioned in the past that EARWIG produces some
>>> > > >> > diagnostics
>>> > > >> > >e.g. consecutive dry days, frost days etc. from WG. Will
>>> > > >> > >this be done for UKCIP08?
>>> > > >> >
>>> > > >> >
>>> > > >> >   The plan is yes for this. Colin has the software
>>for this.
>>> > > >> > It just needs to be set
>>> > > >> >   up carefully, as the base for all the diagnostics
>>(for the
>>> > > >> > future
>>> > > >> > runs) has to be
>>> > > >> >   based on median run of the WG for the present (61-90).
>>> > > >We shouldn't
>>> > > >> >   allow users to change the 61-90 base period (or the
>>> > > >choice of the
>>> > > >> > median).
>>> > > >> >
>>> > > >> >
>>> > > >> >
>>> > > >>
>>> > > >>Good. I would like your opinion on a problem I am having with
>>> > > >>some of the variables we are providing pdfs for. Some
>>quantities
>>> > > >>are indices derived from daily model data e.g frost days but I
>>> > > >>think
>>> > > >there are two
>>> > > >>problems with this:
>>> > > >>
>>> > > >>1. Model bias e.g. a model that is too warm may have very few
>>> > > >>frost days and therefore the change looks small.
>>Effect will be
>>> > > >>a nonlinear function of bias based on shape of
>>distribution of daily data.
>>> > > >>
>>> > > >>2. WG and pdfs could provide two alternative routes to same
>>> > > >answer and
>>> > > >>they will obviously conflict for reasons we understand e.g.
>>> > > >model bias
>>> > > >>but the users won't understand.
>>> > > >>
>>> > > >>To avoid confusing user and potentially reducing their
>>> > > >>confidence in UKCIP products, I think it makes sense
>>for WGs to
>>> > > >>be the sole route towards a prediction of derived
>>indices. BTW,
>>> > > >>I have a handful of derived indices to do (hot days, wet days,
>>> > > >>gale days, heating and cooling degree days and frost
>>days) and I
>>> > > >>think you cover
>>> > > >some of these
>>> > > >>already. What do you think?
>>> > > >>
>>> > > >>Geoff wants to discuss issues connected to the three strands
>>> > > >of output
>>> > > >>(pdfs, WG, RCM) on the 24th.
>>> > > >
>>> > > >
>>> > > > Model biases will only be a problem with their data
>>used directly.

>>> > > > So this could be a problem with the larger regions
>>where the WG
>>> > > > won't work well. The WG won't have biases as it is based on
>>> > > > 61-90 as the base period. We will be perturbing these
>>with the
>>> > > > RCM-based pdfs.
>>> > > >
>>> > > > Maybe we need to show that the following will/should/must be
>>> > > > the same
>>> > > >
>>> > > >   Model-based scenario for 2070s minus model present
>>(61-90) equals
>>> > > >   WG scenarios for the 2070s minus WG present (61-90).
>>> > > >
>>> > > > Geoff will need to get this across as this is how the three
>>> > > >strands will
>>> > > > produce the same answers.
>>> > > >
>>> > > > The WG and the extremes software will do all the temp/precip
>>> > > > indices but won't do gale days.
>>> > > >
>>> > > >
>>> > > >>Cheers, David
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>--
>>> > > >>_____
>>> > > >>David Sexton PhD Climate Research Scientist Met Office
>>> > > >Hadley Centre
>>> > > >>for Climate Prediction and Research FitzRoy
>>> > > >>Road   Exeter   EX1 3PB   United Kingdom
>>> > > >>Tel: +44 (0)1392 886524 Fax: +44 (0)1392 885681
>>> > > >>E-mail: david.sexton@metoffice.gov.uk
>>http://www.metoffice.com
>>> > > >
>>> > > >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
>>> > > >NR4 7TJ
>>> > > >UK
>>> > > >---
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>>
>

790. 1177158252.txt
##########

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: mann@psu.edu
Subject: Re: FYI
Date: Sat, 21 Apr 2007 08:24:12 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Ben Santer" <santer1@llnl.gov>

Hi Phil
I am sure you know that this is not about the science. It is an attack to undermine the science in some way. In that regard I don't think you can ignore it all, as Mike suggests as one option, but the response should try to somehow label these guys and lazy and incompetent and unable to do the huge amount of work it takes to construct such a database. Indeed technology and data handling capabilities have evolved and not everything was saved. So my feeble suggestion is to indeed cast aspersions on their motives and throw in some counter rhetoric. Labeling them as lazy with nothng better to do seems like a good thing to do.

How about "I tried to get some data from McIntyre from his 1990 paper, but I was unable because he doesn't have such a paper because he has not done any constructive work!"

There is no basis for retracting a paper given in Keenan's message. One may have to offer a correction that a particular sentence was not correct if it claimed something that indeed was not so. But some old instrumental data are like paleo data, and can only be used with caution as the metadata do not exist. It doesn't mean they are worthless and can not be used. Offering to make a correction to a few words in a paper in a trivial manner will undermine his case.

Kevin

> Hi Phil,
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> This is all too predictable. This crowd of charlatans is always looking
> for one thing they can harp on, where people w/ little knowledge of the
> facts might be able to be convinced that there is a controversy. They
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> thing they can say is wrong, and thus generalize that the science is

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> landmass is independently warming, and much as the models predict. So
> they can harp all they want on one Chinese data set, it couldn't
> possibly change the big picture (let alone even the trends for China). The
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> legitimate controversy. The last thing you want to do is help them by
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> longer have their friends in power here in the U.S., and the media has
> become entirely unsympathetic to the rants of the contrarians at least
> in the U.S.--the Wall Street Journal editorial page are about the only
> place they can broadcast their disinformation. So in other words, for
> contrarians the environment appears to have become very unfavorable for
> development. I would advise Wang the same way. Keenan may or may not be
> bluffing, but if he tries this I believe that British law would make it
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> Phil Jones wrote:
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>> http://www.climateaudit.org/?p=1471#comments
>>
>> and
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>> One is about data from a paper 17 years ago (Jones et al. 1990)
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>> one of the co-authors on the 1990 paper. Wei-Chyung is in
>> China, and may not yet have seen this. When he's back in
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>> Jim Renwick recently) , but we don't keep a source code
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>> by country and could reconstruct it, but it would take a while.
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>> all come from the NMSs - well mostly, but some from
>> scientists.
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>> Cheers
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>>
>>
>>
>>
>> From: "D.J. Keenan" <doug.keenan@informath.org>
>> To: "Wei-Chyung Wang" <wang@climate.cestm.albany.edu>
>> Cc: "Phil Jones" <p.jones@uea.ac.uk>
>> Subject: retraction request
>> Date: Fri, 20 Apr 2007 13:31:15 +0100
>> X-Mailer: Microsoft Outlook Express 6.00.2900.3028
>> X-UEA-Spam-Score: 0.0
>> X-UEA-Spam-Level: /
>> X-UEA-Spam-Flag: NO
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>> there are severe problems. In particular, the data was obtained from
>> 84 meteorological stations that can be classified as follows.
>>   49 have no histories   08 have inconsistent histories   18 have
>> substantial relocations   02 have single-year relocations   07 have
>> no relocations Furthermore, some of the relocations are very
>> distant--over 20 km.
>> Others are to greatly different environments, as illustrated here:
>> http://www.climateaudit.org/?p=1323#comment-102970
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>> The above contradicts the published claim to have considered the
>> histories of the stations, especially for the 49 stations that have no
>> histories. Yet the claim is crucial for the research conclusions.
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>> I e-mailed you about this on April 11th. I also phoned you on April
>> 13th: you said that you were in a meeting and would get back to me. I
>> have received no response.
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>> I ask you to retract your GRL paper, in full, and to retract the
>> claims made in Nature about the Chinese data. If you do not do so, I
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>> university at Albany.
>>
>>
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>> http://www.informath.org
>> phone + 44 20 7537 4122
>> The Limehouse Cut, London E14 6N, UK
>>
>>
>>
>>
>> Prof. Phil Jones

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> --
> 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
>
>

_____
Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
http://www.cgd.ucar.edu/cas/trenbert.html

791. 1177163150.txt
##
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: FYI
Date: Sat, 21 Apr 2007 09:45:50 -0400
Reply-to: mann@psu.edu
Cc: trenbert@ucar.edu, Ben Santer <santer1@llnl.gov>

<x-flowed>
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for one thing they can harp on, where people w/ little knowledge of the
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>

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Michael E. Mann
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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>

792. 1177423054.txt
##########

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: FYI
Date: Tue, 24 Apr 2007 09:57:34 -0700
Reply-to: santer1@llnl.gov
Cc: trenbert@ucar.edu, mann@psu.edu

<x-flowed>
Dear Phil,

Sorry about the delay in replying to your email - I've been out of my office for a few days.

This is really nasty stuff, and I'm sorry that it's happened to you. The irony in this is that you are one of the most careful and thorough scientists I know.

Keenan's allegations of research misconduct, although malicious and completely unfounded, clearly require some response. The bottom line is that there are uncertainties inherent in measuring ANY properties of the real-world climate system. You've probably delved deeper than anyone else on the planet into uncertainties in observed surface temperature records. This would be well worth pointing out to Mr. Keenan. The whole tenor of the web-site stuff and Keenan's garbage is that these folks are scrupulously careful data analysts, and you are not. They conveniently ignore all the pioneering work that you've done on identification of inhomogeneities in surface temperature records. The response should mention that you've spent much of your scientific career trying to quantify the effects of such inhomogeneities, changing spatial coverage, etc. on observed estimates of global-scale surface temperature change.

The bottom line here is that observational data are frequently "messy". They are not the neat, tidy beasts Mr. Keenan would like them to be. This holds not only for surface temperature measurements. It also holds - in spades - for measurements of tropospheric temperature from MSU and radiosondes, and for measurements of ocean temperatures from XBTs, profiling floats, etc. We would like observing systems to be more accurate, more stable, and better-suited for monitoring decadal-scale changes in climate. You and Kevin and many other are actively working towards that goal. The key message here is that, despite uncertainties in the surface temperature record - uncertainties which you and others in the field are well aware of, and have worked hard to quantify - it is

now unequivocal that surface temperatures have warmed markedly over the
past 100 years. Uncertainties in the station histories do not negate
this basic message.

Hope some of these random musings might be useful, Phil. Let me know if
there's anything else I can do to help. Will you be at the Hadley Centre
Science Review Group meeting in May?

With best regards,

Ben

P.Jones@uea.ac.uk wrote:
> All,
> Thanks for the thoughts. I'll muse on them whilst
> away. I've decided to ignore the blogs, but will wait
> till I hear from Wei-Chyung when he's back. There is
> no point yet in my responding to Keenan till Wei-Chyung
> hears.
> I'm away much of the next 3 weeks, so I won't be
> responding quickly. I'll be noting down some points
> for a possible response, so anything I'll do will
> be considered rather than my usual quick responses.
> The unequivocal statement in the SPM will be clear
> in any response.
> The whole tone of their argument smacks of a last
> resort challenge. 2007 continues warm for the first
> 3 months.
>
> Cheers
> Phil
>
>> I agree on the blogs: I have refrained from any responses to the attacks
>> on me wrt hurricanes etc.
>> K
>>
>>
>>> I don't disagree w/ Kevin's points here, but I do think it is
>>> dangerous to respond to an accusation made on a blog (a dubious
>>> one at that). It sets a bad precedent. On the other hand, since
>>> the letter to Wang was copied to you, I guess it is legitimate for
>>> you to respond to that. but very carefully as Kevin points out,
>>>
>>> mike
>>>
>>> Kevin Trenberth wrote: Hi Phil I am sure you know that this is not
>>> about the science. It is an attack to undermine the science in some
>>> way.
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>>> editorial page are about the only place they can broadcast their
>>> disinformation. So in other words, for contrarians the environment
>>> appears to have become very unfavorable for development. I would advise
>>> Wang the same way. Keenan may or may not be bluffing, but if he tries
>>> this I believe that British law would make it easy for Wang to win a
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>>> mike Phil Jones wrote: Kevin,   Have a look at this
>>> web site. I see you're away. The websites can wait, but scroll down to
>>> the letter below from Keenan - the last sentence.
>>> http://www.climateaudit.org/?p=1471#comments   and
>>> http://www.climateaudit.org/?p=1479#more-1479   One is about data from a
>>> paper 17 years ago (Jones et al. 1990)   Also there is this email
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>>> sent to Wei-Chyung Wang, who was one of the co-authors on the 1990
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>>> back in Albany, I've suggested he talks to someone there. It is all
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>>> from their experiences.   If it gets worse I will bring Susan in as
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>>> with getting the AR4 WG1 volume out.   On the 1990 paper, I have put the
>>> locations and the data for the rural stations used in the paper on the
>>> CRU website. All the language is about me not being able to send them
>>> the station data used for the grids (as used in 1990!). I don't have
>>> this information, as we have much more data now (much more in Australia
>>> and China than then) and probably more stations in western USSR are as
>>> well.   As for the other request, I don't have the information on the
>>> sources of all the sites used in the CRUTEM3 database. We are adding in
>>> new datasets regularly (all of NZ from Jim Renwick recently) , but we
>>> don't keep a source code for each station. Almost all sites have
>>> multiple sources and only a few sites have single sources. I know
>>> things
>>> roughly by country and could reconstruct it, but it would take a while.
>>> GHCN and NCAR don't have source codes either. It does all come from
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>>> are
>>> in various papers, but they never read these. Also certainly no use
>>> talking to them.   In Geneva all week. David Parker and Tom Peterson

>>> will be there. I can live with the web site abuse, but the Keenan
>>> letter knocked me back a bit.   I seem to be the marked man now !
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>>> Jones" Subject: retraction request Date: Fri, 20 Apr 2007 13:31:15
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>>> Chinese meteorological data analyzed by Wang et al. [GRL, 1990] and
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>>> April
>>> 11th. I also phoned you on April 13th: you said that you were in a
>>> meeting and would get back to me. I have received no response. I ask
>>> you to retract your GRL paper, in full, and to retract the claims made
>>> in
>>> Nature about the Chinese data. If you do not do so, I intend to
>>> publicly
>>> submit an allegation of research misconduct to your university at
>>> Albany.
>>>   Douglas J. Keenan http://www.informath.org phone + 44 20 7537 4122 The
>>> Limehouse Cut, London E14 6N, UK   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 NR4 7TJ UK
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>>>   -- Michael E. Mann Associate Professor Director, Earth
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>>> Phone: (814) 863-4075 503 Walker Building   FAX:
>>> (814)
>>> 865-3663 The Pennsylvania State University   email: mann@psu.edu
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>>
>

--

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FAX: (925) 422-7675
email: santer1@llnl.gov

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793. 1177534709.txt
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From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: FYI
Date: Wed, 25 Apr 2007 16:58:29 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Phil,

I looked at some of the stuff on the Climate Audit web site. I'd really
like to talk to a few of these "Auditors" in a dark alley. They seem to
have no understanding of how science is actually done - no appreciation
of the fact that uncertainty is an integral part of what we do. Once
again, just let me know how I can help....

It will be good to see you in Exeter. I'm looking forward to that. I'll
have two nights in London after the meeting, and am hoping to spend some
time wandering around the British Museum.

I met a very nice lady (Stephanie) while I was giving a series of
climate change lectures in Puerto Rico back in January. She's a
Professor at the University of San Francisco, and (fortuitously),
specializes in the policy implications of climate change, risk
assessment, etc. She also likes hiking and climbing. It's fun to "have a
life" again (as they say over here).

Best wishes to you and Ruth,

Ben
P.Jones@uea.ac.uk wrote:
> Ben,
>   Thanks for the thoughts. I'm in Geneva at the moment,
> so have a bit of time to think. Possibly I'll
> get the raw data from GHCN and do some work to replace
> our adjusted data with these, then make the Raw
> (i.e. as transmitted by the NMSs). This will annoy them
> more, so may inflame the situation.
>
>   Got some ideas/thoughts from Mike, Kevin and Gavin Schmidt.
>
> Some of the stuff on the Climat Audit web site is awful.

>
> Will also be talking to someone at UEA, is they have
> anything useful to say.
>
> Also talking to Wei-Chyung about how he'll respond.
>
> I will be in Exeter. Get back from Tarragona on the
> Weds am, so should be there for dinner on the first day.
>
> Lots of odd things going on at the HC by the way.
>
> See you in Exeter.
>
> Cheers
> Phil
>
>
>> Dear Phil,
>>
>> Sorry about the delay in replying to your email - I've been out of my
>> office for a few days.
>>
>> This is really nasty stuff, and I'm sorry that it's happened to you. The
>> irony in this is that you are one of the most careful and thorough
>> scientists I know.
>>
>> Keenan's allegations of research misconduct, although malicious and
>> completely unfounded, clearly require some response. The bottom line is
>> that there are uncertainties inherent in measuring ANY properties of the
>> real-world climate system. You've probably delved deeper than anyone
>> else on the planet into uncertainties in observed surface temperature
>> records. This would be well worth pointing out to Mr. Keenan. The whole
>> tenor of the web-site stuff and Keenan's garbage is that these folks are
>> scrupulously careful data analysts, and you are not. They conveniently
>> ignore all the pioneering work that you've done on identification of
>> inhomogeneities in surface temperature records. The response should
>> mention that you've spent much of your scientific career trying to
>> quantify the effects of such inhomogeneities, changing spatial coverage,
>> etc. on observed estimates of global-scale surface temperature change.
>>
>> The bottom line here is that observational data are frequently "messy".
>> They are not the neat, tidy beasts Mr. Keenan would like them to be.
>> This holds not only for surface temperature measurements. It also holds
>> - in spades - for measurements of tropospheric temperature from MSU and
>> radiosondes, and for measurements of ocean temperatures from XBTs,
>> profiling floats, etc. We would like observing systems to be more
>> accurate, more stable, and better-suited for monitoring decadal-scale
>> changes in climate. You and Kevin and many other are actively working
>> towards that goal. The key message here is that, despite uncertainties
>> in the surface temperature record - uncertainties which you and others
>> in the field are well aware of, and have worked hard to quantify - it is
>> now unequivocal that surface temperatures have warmed markedly over the
>> past 100 years. Uncertainties in the station histories do not negate
>> this basic message.
>>
>> Hope some of these random musings might be useful, Phil. Let me know if
>> there's anything else I can do to help. Will you be at the Hadley Centre
>> Science Review Group meeting in May?
>>
>> With best regards,
>>
>> Ben
>>

>> P.Jones@uea.ac.uk wrote:
>>> All,
>>> Thanks for the thoughts. I'll muse on them whilst
>>> away. I've decided to ignore the blogs, but will wait
>>> till I hear from Wei-Chyung when he's back. There is
>>> no point yet in my responding to Keenan till Wei-Chyung
>>> hears.
>>> I'm away much of the next 3 weeks, so I won't be
>>> responding quickly. I'll be noting down some points
>>> for a possible response, so anything I'll do will
>>> be considered rather than my usual quick responses.
>>> The unequivocal statement in the SPM will be clear
>>> in any response.
>>> The whole tone of their argument smacks of a last
>>> resort challenge. 2007 continues warm for the first
>>> 3 months.
>>>
>>> Cheers
>>> Phil
>>>
>>>> I agree on the blogs: I have refrained from any responses to the
>>>> attacks
>>>> on me wrt hurricanes etc.
>>>> K
>>>>
>>>>
>>>>> I don't disagree w/ Kevin's points here, but I do think it is
>>>>> dangerous to respond to an accusation made on a blog (a dubious
>>>>> one at that). It sets a bad precedent. On the other hand, since
>>>>> the letter to Wang was copied to you, I guess it is legitimate for
>>>>> you to respond to that. but very carefully as Kevin points out,
>>>>>
>>>>> mike
>>>>>
>>>>> Kevin Trenberth wrote: Hi Phil I am sure you know that this is not
>>>>> about the science. It is an attack to undermine the science in some
>>>>> way.
>>>>> In that regard I don't think you can ignore it all, as Mike suggests
>>>>> as
>>>>> one option, but the response should try to somehow label these guys
>>>>> and
>>>>> lazy and incompetent and unable to do the huge amount of work it takes
>>>>> to
>>>>> construct such a database. Indeed technology and data handling
>>>>> capabilities have evolved and not everything was saved. So my feeble
>>>>> suggestion is to indeed cast aspersions on their motives and throw in
>>>>> some counter rhetoric. Labeling them as lazy with nothng better to do
>>>>> seems like a good thing to do. How about "I tried to get some data
>>>>> from
>>>>> McIntyre from his 1990 paper, but I was unable because he doesn't have
>>>>> such a paper because he has not done any constructive work!" There is
>>>>> no
>>>>> basis for retracting a paper given in Keenan's message. One may have
>>>>> to
>>>>> offer a correction that a particular sentence was not correct if it
>>>>> claimed something that indeed was not so. But some old instrumental
>>>>> data
>>>>> are like paleo data, and can only be used with caution as the metadata
>>>>> do
>>>>> not exist. It doesn't mean they are worthless and can not be used.
>>>>> Offering to make a correction to a few words in a paper in a trivial
>>>>> manner will undermine his case. Kevin Hi Phil, This is
>>>>> all

>>>>> too predictable. This crowd of charlatans is always looking for one
>>>>> thing
>>>>> they can harp on, where people w/ little knowledge of the facts might
>>>>> be
>>>>> able to be convinced that there is a controversy. They can't take on
>>>>> the
>>>>> whole of the science, so they look for one little thing they can say
>>>>> is
>>>>> wrong, and thus generalize that the science is entirely compromised.
>>>>> Of
>>>>> course, as nicely shown in the SPM, every landmass is independently
>>>>> warming, and much as the models predict. So they can harp all they
>>>>> want
>>>>> on one Chinese data set, it couldn't possibly change the big picture
>>>>> (let
>>>>> alone even the trends for China). The So they are simply hoping to
>>>>> blow
>>>>> this up to something that looks like a legitimate controversy. The
>>>>> last
>>>>> thing you want to do is help them by feeding the fire. Best thing is
>>>>> to
>>>>> ignore them completely. They no longer have their friends in power
>>>>> here
>>>>> in the U.S., and the media has become entirely unsympathetic to the
>>>>> rants
>>>>> of the contrarians at least in the U.S.--the Wall Street Journal
>>>>> editorial page are about the only place they can broadcast their
>>>>> disinformation. So in other words, for contrarians the environment
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>>>>> Douglas J. Keenan http://www.informath.org phone + 44 20 7537 4122
>>>>> The
>>>>> Limehouse Cut, London E14 6N, UK 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 NR4 7TJ UK
>>>>> ---
>>>>> -- 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

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>>
>> --
>> --
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>> Tel: (925) 422-2486
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>> email: santer1@llnl.gov
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From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@psu.edu
Subject: Re: quick note on TAR
Date: Sun Apr 29 19:53:16 2007

   Mike

your words are a real boost to me at the moment. I found myself questioning the whole
process and being often frustrated at the formulaic way things had to be done - often
wasting time and going down dead ends. I really thank you for taking the time to say these
kind words . I tried hard to balance the needs of the science and the IPCC , which were not
always the same. I worried that you might think I gave the impression of not supporting you
well enough while trying to report on the issues and uncertainties . Much had to be removed
and I was particularly unhappy that I could not get the statement into the SPM regarding
the AR4 reinforcement of the results and conclusions of the TAR. I tried my best but we
were basically railroaded by Susan. I am happy to pass the mantle on to someone else next
time. I feel I have basically produced nothing original or substantive of my own since this
whole process started. I am at this moment , having to work on the ENV submission to the
forthcoming UK Research Assessment exercise , again instead of actually doing some useful
research ! Anyway thanks again Mike.... really appreciated when it comes from you
very best wishes
Keith
Keith
At 18:14 29/04/2007, you wrote:

Keith, just a quick note to let you know I've had a chance to read over the key bits on
last millennium in the final version of the chapter, and I think you did a great job.
obviously, this was one of the most (if not the most) contentious areas in the entire
report, and you found a way to (in my view) convey the the science accurately, but in a
way that I believe will be immune to criticisms of bias or neglect--you dealt w/ all of
the controversies, but in a very even-handed and fair way. bravo!
I hope you have an opportunity to relax a bit now. looking forward to buying you a beer
next time we have an opportunity :)
mike
--
Michael E. Mann
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503 Walker Building FAX: (814) 865-3663
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Fax: +44-1603-507784

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

References

1. http://www.met.psu.edu/dept/faculty/mann.htm
2. http://www.cru.uea.ac.uk/cru/people/briffa/

795. 1178107838.txt
##########

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Multi-model SST detection results
Date: Wed, 02 May 2007 08:10:38 -0700
Reply-to: santer1@llnl.gov
Cc: Nathan Gillett <n.gillett@uea.ac.uk>, peter gleckler <gleckler1@llnl.gov>, i.harris@uea.ac.uk

<x-flowed>
Dear Phil,

Thanks very much for the quick reply. It would be nice to get hold of CRU TS 3.0, even at the 0.5 x 0.5 degree resolution.

For the SST detection and attribution analysis that I described yesterday, I reduced the spatial dimensionality (to get better estimates of covariance matrices, EOFs, etc.) by regridding all model and observational SST data to a common 10 x 10 lat/long grid. I think it would make sense to do the detection and attribution analysis involving the land 2m temperature changes at the same 10 x 10 resolution. So it isn't essential for me to get the CRU TS 3.0 data at 5 x 5 resolution - we might as well have just one regridding step (from 0.5 x 0.5 to 10 x 10) rather than two. As in the SST case, the primary focus would be on land 2m temperature changes over 1950 to 2006. I'm hopeful that the changing coverage/variance issues won't be that severe over this period.

Let me back up a little and outline why I want to look at CRU TS 3.0.

I've always thought that it would be fun to contrast the S/N behavior of SST and land 2m temperature. Based purely on the amplitude of unforced variability, one might expect S/N ratios to be more more favorable for SST changes than for land 2m temperature changes. But it's not that simple! Due to land/ocean differences in specific and total heat capacity, we expect the GHG-induced surface temperature signal to be larger over land than over oceans. And then there's the issue of the spatial heterogeneity of the forcings. Arguably, anthropogenic forcings over land are more spatially heterogeneous than over oceans (e.g., no changes in land surface properties over oceans!). Such land/ocean forcing differences must also influence the S/N behavior of temperature changes over land and oceans.

So I suspect, based on S/N arguments, that it's better to search for an anthropogenic surface temperature signal over the oceans rather than the land. Actually showing this might be useful.

Cheers,

Ben
Phil Jones wrote:
>
> Ben,

>   CRU doesn't have an infilled land database at the 5 by 5 degree
> resolution.
> We do at the 0.5 by 0.5 degree resolution though. It would take a
> bit of work to average these together to the coarser resolution, but it
> ought to be possible.
>   We have a new version of this (CRU TS 3.0) that Ian Harris (Harry)
> is finishing off. It runs from 1900 to 2006. It doesn't take care of
> variance issues, so will have problems when in regions with poor data
> earlier in the 20th century. Should be OK though from 1950, if you
> want to start then.
>   Harry is i.harris@uea.ac.uk. I think the temperature is finished, but
> Nathan could check. I'm away now till the HC meeting in Sweden
> and Spain.
>   Another option is to use the infilled 5 by 5 dataset that Tom Smith
> has put together at NCDC. All infilling has the problem that when there
> is little data it tends to revert to the 1961-90 average of zero. All
> infilling techniques do this - alluded to countless times by Kevin
> Trenberth and this is in Ch 3 of AR4. This infilling is in the current
> monitoring version of NCDC's product. The infilling is partly the reason
> they got 2005 so warm, by extrapolating across the Arctic from the
> coastal stations. I think NCDC and the HC regard the permanent
> sea ice as 'land', as it effectively is.
>   As a side issue , the disappearance of sea ice in the Arctic is going
> to cause loads of problems monitoring temps there as when SST data
> have come in from the areas that have been mostly sea ice, it is always
> warm as the 61-90 means are close to -1.8C. Been talking to Nick
> Rayner about this. It isn't serious yet, but it's getting to be a problem.
> In the AR4 chapter, we had to exclude the SST from the Arctic plot
> as the Arctic (north of 65N) from 1950 was above the 61-90 average
> for most of the years that had enough data to estimate a value.
>
>   See you in Exeter in a week's time.
>
> Cheers
> Phil
>
>
>
> At 01:40 02/05/2007, Ben Santer wrote:
>> Dear Nathan,
>>
>> I'm now in the process of transferring SST data from the AR4
>> pre-industrial control runs. I'm hoping that the data transfer will be
>> finished by tomorrow. As described in the Supporting Text of our PNAS
>> water vapor paper, I've changed the time model of all control runs.
>> The time model is the same as in the 20c3m runs - i.e., "months since
>> 1800". This slightly complicates life if you want to subtract a
>> model's instantaneous control run drift from its 20c3m run. You then
>> have to figure out the time (in the new "months since 1800" time
>> model) at which the 20c3m run was spawned from the pre-industrial
>> control. I find, however, that the advantages of using a uniform time
>> model far outweigh the disadvantages.
>>
>> With some help from Peter, I managed to obtain some preliminary
>> results for the detection of an anthropogenic fingerprint in observed
>> SST data. To my knowledge, most formal pattern-based D&A work that has
>> dealt with temperature changes close to Earth's surface has used
>> combined SSTs and land 2m temperatures. I'm not aware of any
>> pattern-based work (other than your work with SST changes in the
>> Atlantic and Pacific tropical cyclogenesis regions) that has focused
>> on SST changes alone. I'm assuming that the dearth of "SST only"
>> fingerprint work arises in part from pesky masking and regridding
>> problems (the same problems we had to address in the PNAS water vapor

>> paper).
>>
>> As I mentioned several days ago, I essentially replicated all of the
>> data "pre-processing" we had done for the water vapor paper: i.e., the
>> same procedures were used for masking and regridding SST data to a
>> uniform 10 x 10 lat/long grid, calculation of the V and No-V SST
>> fingerprints, and concatenation of SST data from the V and No-V
>> control runs. I also employed the same spatial domain that we used for
>> the PW analysis (all oceans, 50N-50S).
>>
>> One of the choices I have to make in estimating detection time is the
>> selection of a "start date" for calculation of trends in the signal
>> time series Z(t) and Z*(t) (the projections of the observed data onto
>> the raw and optimized fingerprints, respectively). For the water vapor
>> paper, the start date was dictated by the start date of the SSM/I PW
>> data (1988). Here, however, we are using NOAA ERSST data, which are
>> available from 1880 onwards. I chose a start date in 1950. I think
>> this is a defensible choice, partly because the spatial coverage of
>> SST data is more stable over time in the second half of the 20th
>> century than in the first. Furthermore, a 1950 start date is a
>> somewhat conservative choice in view of the "flattening" of the
>> observed global-scale SST increase in the 1960s and 1970s. A start
>> date in the mid-1970s would probably yield shorter detection times.
>>
>> The detection time results are encouraging. In the "spatial mean
>> included" case, we invariably obtain robust detection of the V and
>> No-V model fingerprints in the NOAA ERSST data. As you pointed out
>> previously, Nathan, the fingerprint estimated from the No-V 20c3m runs
>> is basically an "ANTHRO-ONLY" fingerprint. For a 1950 start date, the
>> detection times are all with +/- 5 years of 1980, irrespective of
>> whether the V or No-V models are used to estimate fingerprints,
>> optimize fingerprints, or assess statistical significance. This means
>> that, if we had begun monitoring observed SST changes in 1950, we
>> would have been able to identify an anthropogenic fingerprint roughly
>> 30 years later. I should point out that (as in the vapor paper), we've
>> tried to be conservative in our significance testing procedure, and
>> have intentionally retained residual control run drift.
>>
>> Results are more ambiguous in the "spatial mean removed" case. In that
>> setting, whether we can or cannot detect an anthropogenic fingerprint
>> is much more sensitive to V/No-V dataset choices. Why might that be? A
>> preliminary hypothesis is that in the "mean removed" case, greater
>> attention is focused on differential SST changes in the western and
>> eastern Pacific. The recent GRL paper by Soden and Vecchia provides
>> some model-based evidence that such differential SST changes may be
>> forced, and are accompanied by changes in the Walker circulation. I
>> suspect that these differential west/east SST changes may evolve in a
>> complex way over time, and that in the "mean removed" case, we might
>> have more luck detecting an "ANTHRO" fingerprint if go to full
>> space-time optimal detection. But that's only a guess on my part, and
>> my intuition has often been wrong!
>>
>> In the next few days, I'll fool around with several different "start
>> dates", and will also start looking at the spatial patterns of the raw
>> and optimized fingerprints, the dominant noise modes, etc. As I
>> mentioned previously, it would be nice to contrast the "SST-only" D&A
>> results with "land-only" D&A results. Does CRU have "land-only"
>> temperature data in which missing land 2m temperatures have been
>> statistically infilled? In other words, is there a land 2m temperature
>> counterpart to the HadISST product? (I've copied this email to Phil,
>> who I'm sure will be able to answer my last question.)
>>
>> Anyway, looks like this work is worth pursuing. It will be very

>> interesting to compare your space-time results with the results we've
>> obtained thus far.
>>
>> With best regards,
>>
>> Ben
>> --
>>
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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: More Rubbish
Date: Thu, 17 May 2007 11:46:30 -0400
Reply-to: mann@psu.edu

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yep, I'm watching the changing of the guard live on TV here!

New Scientist was good. Gavin and I both had some input into that. They
are nicely dismissive of the contrarians on just about every point,
including the HS!

Heard anything back from IUGG yet? I thought Mike's email was helpful,

if that doesn't do the trick I don't know what will,

mike

Phil Jones wrote:
>
> Mike,
>
>   Apparently there is a lot in New Scientist this week. As usual
> our copy has gone walkabout!
>
>   Blair is out on June 27 - Gordon Brown then !
>
> Phil
>
>
> At 16:33 17/05/2007, you wrote:
>> as I was looking at this, I had CNN on in the background. Live
>> conference, with Bush and Blair both agreeing about the importance of
>> significantly cutting greenhouse gas emissions.
>>
>> jokes like Carter have become completely irrelevant. they are a sad
>> anachronism...
>>
>> mike
>>
>> Phil Jones wrote:
>>>
>>>> Just in case you've not seen it. Another piece of bad science.
>>>
>>>   It is the same old stuff, so not worth doing anything at Real
>>> Climate,
>>> but might be worth doing something on Figure 5.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>>
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<mokssit@mpret.gov.ma>, <Linda.Mortsch@ec.gc.ca>, <gcisc@comsats.net.pk>,
<sanigmi@albatros.uz>, <joel.noilhan@meteo.fr>, <nyongao@hisen.org>,
<leocadieo@yahoo.com>, <wogana@uonbi.ac.ke>, <taikan@iis.u-tokyo.ac.jp>,
<onuma@econ.keio.ac.jp>, <ouldbba@mtpnet.gov.ma>, <jpizarro@lauca.usach.cl>,
<mati@eco.edu.ee>, <mcco@magicnet.mn>, <j.quinn@niwa.co.nz>, <meteo@tjinter.com>,
<aramire@strix.ciens.ucv.ve>, <paolo.reggiani@wldelft.nl>, <nsr@ceh.ac.uk>,
<Kim.ritman@brs.gov.au>, <rolp7543@cueyatl.uam.mx>, <rolp7543@cueyatl.uam.mx>,
<nj.rosenberg@pnl.gov>, <jroy@cal2.vsnl.net.in>, <roy.rene@ouranos.ca>,
<layesarr@ucad.sn>, <MSavard@nrcan.gc.ca>, <antje.schwalb@tu-bs.de>,
<michael.scott@pnl.gov>, <qc2@nwqc.gov.cn>, <gcisc@comsats.net.pk>,
<ishiklom@zb3627.spb.edu>, <anond@start.or.th>, <southgate.1@osu.edu>,
<csu@ceh.ac.uk>, <joesylla2002@yahoo.fr>, <jan.szolgay@stuba.sk>,
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<Eelco.vanBeek@Wldelft.nl>, <exlaure@yahoo.fr>, <wangsr@cma.gov.cn>,

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<Robert.S.Webb@noaa.gov>, <r.woods@niwa.co.nz>, <Sabine.wurzler@lua.nrw.de>,
<zxie@lasg.iap.ac.cn>, <yafid2@yahoo.fr>, <yongyuan.yin@sdri.ubc.ca>,
<slzhang@mwr.gov.cn>, <jyzhang@mwr.gov.cn>, <zhaoyong@tsinghua.edu.cn>,
<zxc@h2o-china.com>, <unnayan@sdnbd.org>, <jamador@tooji.cigefi.ucr.ac.cr>,
<andressen@intercable.net.ve>, <pmd@pakmet.com>, <mboko50@hotmail.com>,
<bbkganta@yahoo.com>, <rexcruz@yahoo.com>, <cristobal@citma.cu>,
<john@firth.surfonwater.com>, <jn_fobil@hotmail.com>, <jcgsal@fibertel.com.ar>,
<mukirigithendu@yahoo.com>, <hosflorent@hotmail.com>, <zmd@zamnet.zm>,
<jinhkim1@hanmail.net>, <michelkoo@fagro.edu.uy>, <fplansigan@yahoo.com>,
<jllanesreg@yahoo.com>, <Ja_marengo@yahoo.com>, <ceest@intafrica.com>,
<nlmiller@hwr.arizona.edu>, <monirul.mirza@yahoo.ca>, <mmudasser@yahoo.com>,
<uzhymet@meteo.uz>, <tonynyong@yahoo.com>, <abdelaziz_ouldbba@yahoo.fr>,
<p_batima@yahoo.com>, <ilhom_rajabov@hotmail.com>, <paolo_reggiani@hotmail.com>,
<patyromerolankao@yahoo.com.mx>, <patyromerolankao@yahoo.com.mx>,
<srosenb435@aol.com>, <roy.rene@hydro.qc.ca>, <aasarr@netscape.net>,
<munir2k1@hotmail.com>, <joesylla2002@hotmail.com>, <exlaure@hotmail.com>,
<zxie@mail.iap.ac.cn>, <fidele.yabi@laposte.net>, <hwyang66@hotmail.com>,
<zheng-xingcan@cemi.com.cn>
Subject: RE: Invitation to review IPCC Technical Paper on Climate Change and Water
Date: Mon, 21 May 2007 12:45:15 +0100

Dear colleague,

Please find attached the spreadsheet needed for submitting your review comments on the IPCC
Technical Paper on Climate Change and Water. This was accidentally omitted from the email
below.

The Technical Paper and supporting review documents are also available online at:

[1]www.ipcc-wg2.org/review/index.html

username: GEreview

password: water08

Regards,

Paul

Paul van der Linden, Deputy Head (IT)
IPCC WGII TSU, Met Office, Fitzroy Road
Exeter EX1 3PB, United Kingdom
Tel: + 44 (0)1392 88 4665
Mobile: + 44 (0) 7881 603 501
paul.vdl@metoffice.gov.uk   www.ipcc-wg2.org/

At 12:00 2007.05.21, you wrote:

Dear colleague,

The First-Order Draft of the IPCC Technical Paper on Water

The IPCC requested the preparation of a Technical Paper on Water, to be based primarily on the results of the Fourth Assessment (AR4), and to involve all three Working Groups.

Organization of the process is in the charge of Working Group II.

The Expert Review for the First-Order Draft of the Technical Paper on Water will begin on May 21^st, and will run for four weeks until June 17^th. It is essential for the success of the process that we involve the widest community of internationally-recognized researchers in the review. We have identified you as someone whose reputation and contribution to the science is such that your participation is important. Therefore, we are sending you a First-Order Draft, with a request to review the Technical Paper. We would be most grateful if you can find time from your busy schedule to review the Technical Paper. If you can only find time to review those sections that are most close to your professional interests, we would still be pleased to receive your comments, although of course we also need reviews which take a broader view of the coherence and completeness of the document as a whole.

We attach the following:

1. The draft Technical Paper on Water. This is in PDF format, because it is important to preserve the page and line numbers.
2. Background information on the Technical Paper, in the form of a Scoping Note.
3. A spreadsheet for you to use to make your comments. Instructions on how to use this spreadsheet are provided at the beginning.

The deadline for the submission of review comments is June 17th. Comments should be submitted, using the spreadsheet, to [2]ipcc-wg2@metoffice.gov.uk .

Please note that, if you have been nominated by your government for any role in the Fourth Assessment other than Review Editor, you may receive a separate invitation from us inviting you to be an Expert Reviewer in that capacity.

We do hope that you will be able to find time to comment on this draft. In advance, we would like to express our deep gratitude for any contribution you can make. In recognition of the importance of the reviewing process, reviewers' names will be listed in the final published Technical Paper.

Yours sincerely,

Osvaldo Canziani
Co-Chair, Working Group II

Martin Parry
Co-Chair, Working Group II

Jean Palutikof
Head, WGII TSU

<<Useful Information for Review.pdf>> <<IPCC_TP_Water.pdf>> <<Invitation letter for expert reviewers.pdf>>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\rev.xls"

References

1. http://www.ipcc-wg2.org/review/index.html
2. mailto:ipccwg2@metoffice.gov.uk

798. 1180342271.txt
##

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Wengen section
Date: Mon, 28 May 2007 04:51:11 -0400 (EDT)
Reply-to: gschmidt@giss.nasa.gov
Cc: mann@psu.edu, Caspar Ammann <ammann@ucar.edu>

<x-flowed>

Hi Phil, sorry for the long delay. But here is a first draft of the forcings and models section I was supposed to take the lead on. Hopefully, we can merge that with whatever Caspar has.

Thanks

Gavin

==========

4 Forcing (GS/CA/EZ) 4-5pp

Histories (CA)
How models see the forcings, especially wrt aerosols/ozone and increasing model complexities (GS)

An important reason for improving climate reconstructions of the past few millenia is that these reconstructions can help us both evaluate climate model responses and sharpen our understanding of important mechanisms and feedbacks. Therefore, a parallel task to improving climate reconstructions is to assess and independently constrain forcings on the climate system over that period.

Forcings can generically be described as external effects on a specific system. Responses within that system that also themselves have an impact on its internal state are described as feeebacks. For the atmosphere, sea surface temperature changes could therefore be considered a forcing, but in a coupled ocean-atmosphere model they could be a feedback to another external factor or be intrinsic to the coupled system. Thus the distinction between forcings and feedbacks is not defined a priori, but is a function of the scope of the modelled system. This becomes especially important when dealing with the bio-geo-chemical processes in climate that effect the trace gas concentrations (CO2 and CH4) or aerosols. For example, if a model
contains a carbon cycle, than the CO2 variations as a function of climate will be a feedback, but for a simpler physical model, CO2 is often imposed directly as a forcing from observations, regardless of whether in the real world it was a feedback to another change, or a result of human industrial activity.

It is useful to consider the pre-industrial period (pre-1850 or so) seperately from the more recent past, since the human influence on

many aspects of atmospheric composition has increased dramatically in the 20th Century. In particular, aerosol and land use changes are poorly constrained prior to the late 20th Century and have large uncertainties. Note however, there may conceivably be a role for human activities even prior to the 19th Century due to early argiculatural activity (Ruddiman, 2003; Goosse et al, 2005).

In pre-industrial periods, forcings can be usefully separated into purely external changes (variations of solar activity, volcanic eruptions, orbital variation), and those which are intrinsic to the Earth system (greenhouse gases, aerosols, vegetation etc.). Those changes in Earth system elements will occur predominantly as feedbacks to other changes (whether externally forced or simply as a function of internal climate 'noise'). In the more recent past, the human role in affecting atmospheric composition (trace gases and aerosols) and land use have dominated over natural processes and so these changes can, to large extent, be considered external forcings as well.

Traditionally, the 'system' that is most usually implied when talking about forcings and feedbacks are the 'fast' components atmosphere-land surface-upper ocean system that, not coincidentally, corresponds to the physics contained within atmospheric general circulation models (AGCMs)
coupled to a slab ocean. What is not included (and therefore considered as a
forcing according to our previous definition) are 'slow' changes in vegetation, ice sheets or the carbon cycle. In the real world these features will change as a function of other climate changes, and in fact may do so on relatively 'fast' (i..e multi-decadal) timescales. Our choice then of the appropriate 'climate system' is thus slightly arbitrary and does not give a complete picture of the long term sensitivity of the real climate.

These distinctions become important because the records available for atmospheric composition do not record the distinction between feedback or forcing, they simply give, for instance, the history of CO2 and CH4. Depending on the modelled system, those records will either be a modelling input, or a modelling target.

While there are good records for some factors (particularly the well mixed greenhouse gases such as CO2 and CH4), records for others are either hopelessly incomplete (dust, vegetation) due to poor spatial or temporal resolution or non-existant (e.g. ozone). Thus estimates of the magnitude of these forcings can only be made using a model-based approach. This can be done using GCMs that include more Earth system components (interactive aerosols, chemistry, dynamic vegetation, carbon cycles etc.), but these models are still very much a work in progress and have not been used extensively for paleo-climatic purposes. Some initial attempts have been made for select feedbacks and forcings (Gerber et al, 2003; Goosse et al 2006) but a comprehensive assessment over the millennia prior to the pre-industrial does not yet exist.

Even for those forcings for which good records exist, there is a question of they are represented within the models. This is not so much of an issue for the well-mixed greenhouse gases (CO2, N2O, CH4) since there is a sophisticated literature and history of including them within models (IPCC, 2001) though some aspects, such as minor short-wave absorption effects for CH4 and N2O are still not universally included
(Collins et al, 2006). However, solar effects have been treated in quite varied ways.

The most straightforward way of including solar irradiance effects on climate is to change the solar 'constant' (preferably described as total solar irradiance - TSI). However, observations show that solar variability is highly dependent on wavelength with UV bands having about 10 times as much amplitude of change than TSI over a solar cycle (Lean, 2000). Thus including this spectral variation for all solar changes allows for a slightly different behaviour (larger solar-induced changes in the stratosphere where the UV is mostly absorbed for instance). Additionally, the changes in UV affect ozone production in both the stratosphere and troposphere, and this mechanism has been shown to affect both the total radiative forcing and dynamical responses (Haigh 1996, Shindell et al 2001; 2006). Within a chemistry climate model this effect would potentially modify the radiative impact of the original solar forcing, but could also be included as an additional (parameterised) forcing in standard GCMs.

There is also a potential effect from the indirect effect of solar magnetic variability on the sheilding of cosmic rays, which have been theorised to affect the production of cloud condensation nuclei (Dickinson, 1975). However, there have been no quantitative calculations of the magnitude of this effect (which would require a full study of the relevant aerosol and cloud microphysics), and so its impact on climate is not (yet) been included.

Large volcanic eruptions produce significant amounts of sulpher dioxide (SO2). If this is injected into the tropical stratosphere during a particularly explosive eruption, the resulting sulphate can persist in the atmosphere for a number of years (e.g. Pinatubo in 1991). Less explosive, but more persistent eruptions (e.g. Laki in 1789??) can still affect climate though in a more regional way and for a shorter term (Oman et al, 2005). These aerosols have both a shortwave (reflective) and longwave (absorbing) impact on the radiation and their local impact on stratospheric heating can have important dynamical effects. It is therefore better to include the aerosol absorber directly in the radiative transfer code. However, in less sophisticated models, the impact of the aerosols has been parameterised as the equivalent decrease in TSI. For extreme eruptions it has been hypothesised that sulphate production might saturate the oxidative capacity of the stratosphere leaving significant amounts of residual SO2. This gas is a greenhouse gas and would have an opposite effect to the cooling aerosols. This effect however has not yet been quantified.

Land cover changes have occured both due to deliberate modification by humans (deforestation, imposed fire regimes, arguculture) as well as a feedback to climate change (the desertification of the Sahara ca. 5500 yrs ago). Changing vegetation in a standard model affects the seasonal cycle of albedo, the surface roughness, the impact of snow, evapotranspiration (through different rooting depths) etc. However, modelling of the yearly cycle of crops, or incorporating the effects of large scale irrigation are still very much a work in progress.

Aerosol changes over the last few milllenia are very poorly constrained (if at all). These might have arisen from climatically or human driven changes in dust emissions, ocean biology feedbacks on circulation change, or climate impacts on the emission volatile organics from plants (which also have an impact on ozone chemistry). Some work on modelling a subset of those effects has been done for the last glacial maximum or the 8.2 kyr event (LeGrande et al, 2006), but there have been no quantitative estimates for the late Holocene (prior to the industrial period).

Due to the relative expense of doing millennial simulations with

state-of-the-art GCMs, exisiting simulations have generally done the
minimum required to include relevant solar, GHG and volcanic forcings.
Progress can be expected relatively soon on more sophisticated treatments
of those forcings and the first quantitative estimates of additional
effects.

==========

--
| Gavin Schmidt   NASA/Goddard Institute for Space Studies |
| 2880 Broadway |
| Tel: (212) 678 5627   New York, NY 10025 |
| |
| gschmidt@giss.nasa.gov   http://www.giss.nasa.gov/~gavin |
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799. 1180536574.txt
##
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Past Millennia Climate Variability - Review Paper - reminder
Date: Wed, 30 May 2007 10:49:34 -0400
Reply-to: mann@psu.edu

   Hi Phil,
   Off travelling again, will check in when I return next week on status of Perugia
(arggh!).
   Papers is looking good. I've attached draft of Mann et al (2007) which should
have the
   references you're looking for. Please don't distribute, we'd like to wait until
galleys are
   available to begin distributing the paper.
   One small thing, this statement at end of 1st paragraph on page 18 in the draft
didn't seem
   appropriate:
   The question of whether the proxies used by MBH98 were themselves subject to
amplitude
   limitations is not the focus of this section, and is examined in Section 2 above.
   These issues are implicit in section 2, but have nothing to do w/ MBH98
specificially. As
   written this is misleading/confusing, and I don't think it adds anything.
   Phil Jones wrote:

      Dear All,
          There has been some progress. I have contributions from Gene and Gavin.
      Keith (2.3) and Tim (3) here in CRU tell me they are working on their parts.
Francis
      (5) also tells me he has also started. Tas told me about 6 weeks ago he would
      finish the ice core part (section 2.3) shortly.
         So we are getting there. I still need input from Caspar (section 4), Nick
     (section 2.6), Peck (section 2.5). I have added in the section names of the
      missing sections to help you all along.
        Also need people to begin reading through the whole paper, but
      this is premature yet.
      I saw Thorsten at the EGU and he emailed recently saying that Larry (EPRI)
      is keen to see this submitted soon. Remember it was through PAGES and EPRI
      support that we had such a great few days in Wengen almost a year ago!

If we all put some effort in over June we could be there.
Can Gene and Gavin send me their references when they have a few minutes. I suspect most will be in Mann et al. (2007), so if I can get that I can add them
in. I won't pass this on to any others.
Cheers
Phil

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 [1]p.jones@uea.ac.uk
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--
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: [2]mann@psu.edu
University Park, PA 16802-5013

[3]http://www.met.psu.edu/dept/faculty/mann.htm

Attachment Converted: "c:\eudora\attach\MRWAJGR06-revisedfinal.doc"

References

1. mailto:p.jones@uea.ac.uk
2. mailto:mann@psu.edu
3. http://www.met.psu.edu/dept/faculty/mann.htm

800. 1180539376.txt

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Past Millennia Climate Variability - Review Paper - reminder
Date: Wed, 30 May 2007 11:36:16 -0400
Reply-to: mann@psu.edu

thanks Phil,
yeah, I figured we mights as well wait until all contributions have been received before
going over the full text and making necessary revisions...
off to Oregon now. talk to you later,
mike
Phil Jones wrote:

Mike,
Thanks for the paper. Gene wrote that bit. I'll flag it for modifying at my next draft - when I get a chance to add the refs in. Likely the weekend. May have got some other responses by then.
Cheers
Phil

At 15:49 30/05/2007, you wrote:

Hi Phil,
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Cheers
Phil

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[6]http://www.met.psu.edu/dept/faculty/mann.htm

References

1. mailto:p.jones@uea.ac.uk
2. mailto:mann@psu.edu
3. http://www.met.psu.edu/dept/faculty/mann.htm
4. mailto:p.jones@uea.ac.uk
5. mailto:mann@psu.edu
6. http://www.met.psu.edu/dept/faculty/mann.htm

801. 1182179459.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Humphrey, Kathryn (CESA)" <kathryn.humphrey@DEFRA.GSI.GOV.UK>
Subject: Fwd: RE: Outstanding comms plan issues
Date: Mon, 18 Jun 2007 11:10:59 +0100
Cc: "Roger Street" <roger.street@ukcip.org.uk>, "Clare Goodess"
<C.Goodess@uea.ac.uk>,<david.sexton@metoffice.gov.uk>, "Winter, Guy (SEERAD)"
<Guy.Winter@scotland.gsi.gov.uk>, "Vicky Pope" <vicky.pope@metoffice.gov.uk>,
"Steven Wilson" <stwi@nerc.ac.uk>, "Sear, Chris (CESA)"
<chris.sear@DEFRA.GSI.GOV.UK>, "Rob Wilby" <rob.wilby@environment-agency.gov.uk>,
"Rachel Warren" <r.warren@uea.ac.uk>, "Prosser, Havard (WAG-EPC)"
<Havard.Prosser@Wales.GSI.Gov.UK>, "Phil Newton" <ppn@nerc.ac.uk>,"Phil Jones"
<p.jones@uea.ac.uk>, "Phil James" <philip.james@ncl.ac.uk>, "Marguerite Gascoine"
<m.b.gascoine@reading.ac.uk>, "Linda Livingston"
<linda.livingston@metoffice.gov.uk>, "Geoff Jenkins"
<geoff.jenkins@metoffice.gov.uk>, "geoff jenkins at home" <geoff.jenkins@ic24.net>,
"David Sexton" <david.sexton@metoffice.gov.uk>, "Chris Kilsby"
<C.G.Kilsby@newcastle.ac.uk>, "Butt, Adrian (CESA)" <adrian.butt@DEFRA.GSI.GOV.UK>,
"Bryan Lawrence" <b.n.lawrence@rl.ac.uk>, "Brian Hoskins"

<b.j.hoskins@reading.ac.uk>, "Barry McAuley" <barry.mcauley@doeni.gsi.gov.uk>, "Ag Stephens" <A.Stephens@rl.ac.uk>

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Kathryn,
      Made some slight mods to the WG definition. Maybe Chris should check this and then we'll be there on this definition.

  Cheers
  Phil

>X-VirusChecked: Checked
>X-Env-Sender: kathryn.humphrey@DEFRA.GSI.GOV.UK
>X-Msg-Ref: server-13.tower-67.messagelabs.com!1182153653!16925857!1
>X-StarScan-Version: 5.5.12.11; banners=-,-,-
>X-Originating-IP: [195.92.40.48]
>X-IronPort-AV: E=Sophos;i="4.16,434,1175468400";
>   d="doc'32?scan'32,208,32";a="3997439"
>Subject: RE: Outstanding comms plan issues
>Date: Mon, 18 Jun 2007 09:00:44 +0100
>X-MS-Has-Attach: yes
>X-MS-TNEF-Correlator:
>Thread-Topic: Outstanding comms plan issues
>Thread-Index: AcewxUEWmbycgv6dRPW5zHVRv1IojQAuHs8g
>From: "Humphrey, Kathryn (CESA)" <kathryn.humphrey@DEFRA.GSI.GOV.UK>
>To:

>X-OriginalArrivalTime: 18 Jun 2007 08:02:06.0823 (UTC)
>FILETIME=[F6D0E770:01C7B17E]
>X-UEA-Spam-Score: 0.0
>X-UEA-Spam-Level: /
>X-UEA-Spam-Flag: NO
>
>I'm very happy to send this to the users' panel for recommendation to
>the SG, if those suggested below (Geoff, David S, Roger, Chris K, Phil
>Jones) are happy to work up definitions based on the latest version we
>have, attached.
>
>Kathryn
>
>PS congratulations on your Gong, Brian!
>
>-----Original Message-----
>From: Roger Street [mailto:roger.street@ukcip.org.uk]
>Sent: 17 June 2007 10:51
>To: Clare Goodess; Humphrey, Kathryn (CESA);
>david.sexton@metoffice.gov.uk
>Cc: Winter, Guy (SEERAD); Vicky Pope; Steven Wilson; Sear, Chris (CESA);
>Rob Wilby; Rachel Warren; Prosser, Havard (WAG-EPC); Phil Newton; Phil
>Jones; Phil James; Marguerite Gascoine; Linda Livingston; Geoff Jenkins;
>geoff jenkins at home; David Sexton; Chris Kilsby; Butt, Adrian (CESA);
>Bryan Lawrence; Brian Hoskins; Barry McAuley; Ag Stephens
>Subject: Re: Outstanding comms plan issues
>
>With respect to the changes suggested by Clare (green inserts within the
>
>text) I am comfortable with the suggested changes. I am, however,
>somewhat
>concerned with the definition for weather generator but this relates to
>a

>personal perception and my concerns as to how this would be interpreted
>by
>users. I would prefer not suggesting that the weather generator
>generates
>weather data but that it generates weather variables at the daily and
>sub-daily level consistent with the projected climate. As such, I would
>
>prefer something along the lines of the following definition:
>
>Weather generators are statistically-based computer programs that use
>existing weather records and random number sampling to produce long
>timeseries of synthetic daily and sub-daily variables. The statistical
>properties of the generated weather-like variables are expect to be
>similar
>to those of the existing weather record. The UKCIP08 weather generator
>bases its daily and sub-daily variables for future time periods on the
>statistical nature of the PDF data chosen to drive it. The variables
>generated are those required by many applications: precipitation,
>maximum
>and minimum temperature, rainfall, solar radiation and wind speed, as
>well
>as measures of atmospheric water vapour and evapotranspiration.
>
>In terms of the definitions for scenarios and projections, those
>ascribed to
>me are actually those developed through the deliberations within Chapter
>2
>of the IPCC WGII for which Tim Carter was one of the Lead Authors. My
>understanding after talking with Tim was that these definitions, which
>are
>the result of considerable discussion within the IPCC impacts,
>vulnerability
>and adaptation community, will be included with the WGII publication. I
>suggest that the definitions to be included and used within UKCIP08 do
>need
>further consideration to ensure that they are clearly identifying what
>UKCIP08 will be delivering - probabilistic projections and scenarios.
>The
>definitions within UKCIP08 should be informed not constrained by the
>IPCC
>deliberations and should be directed at informing the user community
>(client
>focused).
>
>I also agree with Clare that we should be providing a definition of what
>is
>meant by probabilistic within the context of UKCIP08.
>
>In terms of a way forward, would it be reasonable to ask the following
>to
>develop for the specified terms definitions for approval by the SG
>(after
>seeking views of the Users' Panel):
>MOHC - baseline period, climate, climate change, climate model,
>deterministic, and probability/probabilistic density function;
>Newcastle - weather generator; and
>UKCIP - scenarios and projections.
>
>These could be done over the next couple of weeks with a single request
>for
>views going out to the Users' Panel in July.
>
>Roger

>
>
>----- Original Message -----
>From: "Clare Goodess" <C.Goodess@uea.ac.uk>
>To: <david.sexton@metoffice.gov.uk>; "Humphrey, Kathryn (GA)"
><kathryn.humphrey@DEFRA.GSI.GOV.UK>
>Cc: "Roger Street" <roger.street@ukcip.org.uk>; "Ag Stephens"
><A.Stephens@rl.ac.uk>; "Barry McAuley" <barry.mcauley@doeni.gsi.gov.uk>;
>
>"Brian Hoskins" <b.j.hoskins@reading.ac.uk>; "Bryan Lawrence"
><b.n.lawrence@rl.ac.uk>; "Butt, Adrian (CESA)"
><adrian.butt@DEFRA.GSI.GOV.UK>; "Chris Kilsby"
><C.G.Kilsby@newcastle.ac.uk>;
>"David Sexton" <david.sexton@metoffice.gov.uk>; "geoff jenkins at home"
><geoff.jenkins@ic24.net>; "Geoff Jenkins"
><geoff.jenkins@metoffice.gov.uk>;
>"Linda Livingston" <linda.livingston@metoffice.gov.uk>; "Marguerite
>Gascoine" <m.b.gascoine@reading.ac.uk>; "Phil James"
><philip.james@ncl.ac.uk>; "Phil Jones" <p.jones@uea.ac.uk>; "Phil
>Newton"
><ppn@nerc.ac.uk>; "Prosser, Havard (WAG-EPC)"
><Havard.Prosser@Wales.GSI.Gov.UK>; "Rachel Warren" <r.warren@uea.ac.uk>;
>
>"Rob Wilby" <rob.wilby@environment-agency.gov.uk>; "Sear, Chris (CESA)"
><chris.sear@DEFRA.GSI.GOV.UK>; "Steven Wilson" <stwi@nerc.ac.uk>; "Vicky
>
>Pope" <vicky.pope@metoffice.gov.uk>; "Winter, Guy (SEERAD)"
><Guy.Winter@scotland.gsi.gov.uk>
>Sent: Friday, June 15, 2007 6:59 PM
>Subject: RE: Outstanding comms plan issues
>
>
> > Dear all
> >
> > I was looking at this glossary on the train yesterday and have a few
> > relatively minor comments on some of the entries - added in green to
> > Kathryn's latest draft.
> >
> > But I found the definitions of projections and scenarios very
> > confusing, with problems in both the IPCC and Roger's wording which I
> > couldn't think how to resolve - so it was interesting to see this
> > email discussion. There do seem to be some fundamental differences
> > and still confusion, so I'm afraid that some more discussion is
> > needed (sorry Kathryn!).
> >
> > We agreed at the last meeting to add deterministic - and following
> > this logic through, I think that we should also have added
>probabilistic.
> >
> > According to the key messages, UKCIP08 will be providing
> > 'probabilistic projections'. It therefore seems rather confusing to
> > read that 'projections are generally less comprehensive than
> > scenarios'. This implies to the user that the UKCIP08 probabilistic
> > projections are less comprehensive than the UKCIP02 scenarios. Which
> > is not the intended message - though it depends what you mean by
> > 'less comprehensive'.
> >
> > Over the last few months, I have been persuaded (by discussions with
> > people like Tim Carter) that we should avoid talking about
> > 'probabilistic scenarios'.
> >
> > I agree with David that it makes no sense to say that scenarios
> > include projections - when our definition of the latter includes

> > uncertainties/probabilities. Perhaps the solution is to make a clear
> > distinction between 'projections' - which can be deterministic or
> > probabilistic - and 'probabilistic projections'.
> >
> > At least we all seem agreed on not using 'prediction'!
> >
> > I hope that this has not further muddied the waters, best wishes,
>Clare
> >
> >
> >
> > At 15:23 14/06/2007, david.sexton@metoffice.gov.uk wrote:
> >>Hi,
> >>
> >>I am off for a week and half now and have a few things to sort out
>here
> >>so I won't be able to give you any text for PDFs. I think that might
>be
> >>best left until the report is written because it depends a lot on what
> >>the report writers think. Other comments in the text...
> >>
> >>On Thu, 2007-06-14 at 11:03 +0100, Humphrey, Kathryn (CESA) wrote:
> >> > All,
> >> >
> >> > You seem to have all more or less agreed on the key messages which
>is
> >> > great. However, the glossary is continuing to bring up a range of
> >> > divergent views!
> >> >
> >> > I've had more comments and have got amended definitions in the
> >> > attached. David and Chris, who couldn't make last week's meeting,
> >> > have questioned the use of the AR4 definitions (Chris- too
>technical
> >> > for the layperson, see comments in the attached) and the
> >> > projections/scenarios definition (David- not in agreement with MOHC
> >> > definitions). David, I am keen not to open up the debate again on
>the
> >> > differences between scenarios, projections and predictions (the
>latter
> >> > of which we're not using at all) as we've already had an
>astonishingly
> >> > long conversation on this one and I thought had come to agreement.
> >>
> >>For the time being I think we should remove any reference to "climate
> >>predictions" in the AR4 definition of projections because we haven't
>got
> >>a glossary term for "climate prediction". So "...climate models.
>Climate
> >>projections depend upon the emission/conce..." would be better.
> >>
> >>
> >>
> >> > However if you can find support from the rest of the SG then I'll
> >> > open this one up again; otherwise, I'd like to stick with the
> >> > definitions we have which are consistent with the AR4 WG2 ones,
> >> > defining projections as the bit that includes uncertainty and
> >> > scenarios not.
> >>
> >>I must be missing something here but where does AR4 say "projections
>as
> >>the bit that includes uncertainty and scenarios not". Anyway, AR4 also
> >>says "climate projections serve as the raw material for scenarios" so
> >>how can scenarios not include uncertainty when projections do?

> >>
> >>I still think there is confusion and that this issue will arise again
> >>when it comes to report writing.
> >>
> >>
> >>
> >> >
> >> > Can I also have actual text if you want to change the definitions,
>as
> >> > otherwise I am just guessing on what you are asking for (David, I
>like
> >> > your point on providing an explicit def of probability and PDF, but
> >> > can you offer me some text, plus some for stochastic and error if
>you
> >> > want these in)?
> >>
> >>I don't think we need stochastic and error, I just wondered why we had
> >>"deterministic" there in the first place.
> >>
> >>
> >>Cheers, David
> >>
> >>
> >>
> >> >
> >> > Kind Regards,
> >> >
> >> > Kathryn
> >> >
> >> >
> >> >
> >> >
> >> >
>_____
> >> > From: Roger Street [mailto:roger.street@ukcip.org.uk]
> >> > Sent: 14 June 2007 07:21
> >> > To: Humphrey, Kathryn (CESA); 'Ag Stephens'; 'Barry McAuley';
>'Brian
> >> > Hoskins'; 'Bryan Lawrence'; Butt, Adrian (CESA); 'C Goodess';
>'Chris
> >> > Kilsby'; 'David Sexton'; 'Geoff Jenkins'; 'Geoff Jenkins'; 'Linda
> >> > Livingston'; 'Marguerite Gascoine'; 'Phil James'; 'Phil Jones';
>'Phil
> >> > Newton'; Prosser, Havard (WAG-EPC); 'Rachel Warren'; 'Rob Wilby';
> >> > Sear, Chris (CESA); 'Steven Wilson'; 'Vicky Pope'; Winter, Guy
> >> > (SEERAD)
> >> > Subject: RE: Outstanding comms plan issues
> >> >
> >> >
> >> >
> >> > As this information is being used by the impacts, vulnerability and
> >> > adaptation community and Chapter 2 within the IPCC WGII
>specifically
> >> > discussed these concepts and definitions as part of their remit
>from
> >> > that perspective, I would prefer to use the definitions they have
> >> > developed.
> >> >
> >> >
> >> >
> >> > I will look for these other definitions later today.
> >> >
> >> >

> >> >
> >> > Roger
> >> >
> >> >
> >> >
> >> >
> >> >
> >> >
>_____
> >> >
> >> > From: Humphrey, Kathryn (CESA)
> >> > [mailto:kathryn.humphrey@DEFRA.GSI.GOV.UK]
> >> > Sent: 13 June 2007 16:32
> >> > To: Ag Stephens; Barry McAuley; Brian Hoskins; Bryan Lawrence;
>Butt,
> >> > Adrian (CESA); C Goodess; Chris Kilsby; David Sexton; Geoff
>Jenkins;
> >> > Geoff Jenkins; Humphrey, Kathryn (CESA); Linda Livingston;
>Marguerite
> >> > Gascoine; Phil James; Phil Jones; Phil Newton; Prosser, Havard
>(WAG-
> >> > EPC); Rachel Warren; Rob Wilby; Roger Street; Sear, Chris (CESA);
> >> > Steven Wilson; Vicky Pope; Winter, Guy (SEERAD)
> >> > Subject: Outstanding comms plan issues
> >> >
> >> >
> >> >
> >> >
> >> > All,
> >> >
> >> > Attached is an updated set of key messages and glossary for the
> >> > UKCIP08 comms plan.
> >> >
> >> > For the glossary, the AR4 definitions for projections and scenarios
> >> > differ to those Roger has from the co-author of the WGII report.
> >> > Which do you want to use? Also if anyone has a better definition
>of
> >> > deterministic pls let me have it as the AR4 doesn't give one.
>You'll
> >> > also want to check the other definitions as I've either cut them
>down
> >> > from those presented in the AR4, or added sections to make them
> >> > UKCIP08 specific. Also the only definition I can find of a weather
> >> > generator is very old!
> >> >
> >> > Comments back to me by close Friday would be v helpful.
> >> >
> >> > Kathryn
> >> >
> >> > <<2007-06-13 comms plan Key Messages and glossary.doc>>
> >> >
> >> >
> >> >
> >> > Department for Environment, Food and Rural Affairs (Defra)
> >> >
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> >> > recorded to secure the effective operation of the system and for
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> >> > lawful purposes.
> >> > email message attachment
> >> > On Thu, 2007-06-14 at 11:03 +0100, Humphrey, Kathryn (CESA) wrote:
> >> > > Cc: Ag Stephens <A.Stephens@rl.ac.uk>, Barry McAuley
> >> > > <barry.mcauley@doeni.gsi.gov.uk>, Brian Hoskins
> >> > > <b.j.hoskins@reading.ac.uk>, Bryan Lawrence
> >> > > <b.n.lawrence@rl.ac.uk>, "Butt, Adrian (CESA)"
> >> > > <adrian.butt@DEFRA.GSI.GOV.UK>, Clare Goodess
><C.Goodess@uea.ac.uk>,
> >> > > Chris Kilsby <C.G.Kilsby@newcastle.ac.uk>, David Sexton
> >> > > <david.sexton@metoffice.gov.uk>, geoff jenkins at home
> >> > > <geoff.jenkins@ic24.net>, Geoff Jenkins
> >> > > <geoff.jenkins@metoffice.gov.uk>, Linda Livingston
> >> > > <linda.livingston@metoffice.gov.uk>, Marguerite Gascoine
> >> > > <m.b.gascoine@reading.ac.uk>, Phil James
><philip.james@ncl.ac.uk>,
> >> > > Phil Jones <p.jones@uea.ac.uk>, Phil Newton <ppn@nerc.ac.uk>,
> >> > > "Prosser, Havard (WAG-EPC)" <Havard.Prosser@Wales.GSI.Gov.UK>,
> >> > > Rachel Warren <r.warren@uea.ac.uk>, Rob Wilby
> >> > > <rob.wilby@environment-agency.gov.uk>, Roger Street
> >> > > <roger.street@ukcip.org.uk>, "Sear, Chris (CESA)"
> >> > > <chris.sear@DEFRA.GSI.GOV.UK>, Steven Wilson <stwi@nerc.ac.uk>,
> >> > > Vicky Pope <vicky.pope@metoffice.gov.uk>, "Winter, Guy (SEERAD)"
> >> > > <Guy.Winter@scotland.gsi.gov.uk>, "Murphy, James"
> >> > > <james.murphy@metoffice.gov.uk>
> >> > > In-Reply-To:
> >> > >
><65D9B941E291E141821FEC1AB608D203210AC9@SAMC2V1T.DEMETER.ZEUS.GSI.GOV.UK
> >
> >> > > References:
> >> > >
> >> > >
><65D9B941E291E141821FEC1AB608D203210AC9@SAMC2V1T.DEMETER.ZEUS.GSI.GOV.UK
> >
> >> > > Content-Type: text/plain
> >> > > Date: Thu, 14 Jun 2007 10:05:52 +0100
> >> > > Message-Id:
> >> > > <1181811953.5610.55.camel@eld432.desktop.frd.metoffice.com>
> >> > > Mime-Version: 1.0
> >> > > X-Mailer: Evolution 2.0.2 (2.0.2-27.rhel4.6)
> >> > > Content-Transfer-Encoding: 7bit
> >> > > X-OriginalArrivalTime: 14 Jun 2007 09:05:53.0499 (UTC) FILETIME=
> >> > > [360A52B0:01C7AE63]
> >> > > Return-Path: david.sexton@metoffice.gov.uk
> >> > >
> >> > > Hi,
> >> > >
> >> > > here are some quick comments. I probably made some similar ones a
> >> > > while
> >> > > back.
> >> > >
> >> > > General comment on glossary:
> >> > >
> >> > >
> >> > > A general comment is that I can see the point of having a
>glossary
> >> > > early
> >> > > on so that terms are consistent across different communications.
>But

> >> > > I
> >> > > really feel that a lot of these are scientific and that they need
>to
> >> > > be
> >> > > correct for the report and consistent with the ideas of the
>report
> >> > > writers (Geoff and James and to a lesser extent me, Phil and
>Chris
> >> > > and
> >> > > Stephen Dye). These ideas will develop as the report is written
>so I
> >> > > don't think it helps the report writers to set in stone these
>terms.
> >> > >
> >> > > Also, I think the glossary has several inconsistencies in it
>which
> >> > > will
> >> > > cause confusion. So here are my comments:
> >> > >
> >> > > Finally, we have to be really careful with the terms "prediction"
> >> > > and
> >> > > "uncertainty" because both have connotations to the lay person
>which
> >> > > are
> >> > > different to the scientist - scientific predictions should always
> >> > > have
> >> > > an estimate of uncertainty associated with them, where a
>prediction
> >> > > to a
> >> > > lay person might mean a one-off value. "Error" is another good
> >> > > example.
> >> > > I would try to avoid these terms in the glossary and the report.
> >> > >
> >> > >
> >> > > Specific comments:
> >> > >
> >> > > PROJECTIONS, SCENARIOS and "predictions":
> >> > > At MOHC we see a climate projection as some plausible climate
>that
> >> > > is an
> >> > > outcome of some inputs e.g. emission scenario. It has no
>likelihood
> >> > > assigned to it. Here, we see "climate predictions" as a set of
> >> > > projections which have been calibrated by the observations and
> >> > > therefore
> >> > > have an assigned likelihood. It seems this is more like the AR4
> >> > > definition of SCENARIO as AR4 use observed data (see AR4 defn)
>and
> >> > > therefore scenarios DO ascribe likelihoods. This seems to
>contradict
> >> > > Roger's last line on "projections" which says scenarios do not
> >> > > ascribe
> >> > > likelihoods. Also, the product has always been referred to as the
> >> > > "UKCIP08 scenarios" and they definitely assign likelihoods. I
>also
> >> > > disagree with Roger's last sentence on "PROJECTIONS" - I'd say
> >> > > projections are not probabilistic.
> >> > >
> >> > > So a temporary suggestion would be to use the AR4 definition of
> >> > > "PROJECTION" but delete the confusing bit relating it to
> >> > > "predictions"
> >> > > which haven't been defined in the glossary i.e. delete
> >> > > "distinguished...projections".

> >> > >
> >> > >
> >> > >
> >> > > PDF: I would use "Probability Distribution Function" cos it has
>an
> >> > > element of subjective uncertainty in it. Probability Density
> >> > > functions
> >> > > are to me more analytical e.g. Gaussian, exponential. Also, the
> >> > > definition does describe what a PDF is, but it doesn't convey how
> >> > > the
> >> > > PDF should be viewed because it doesn't convey what "probability"
>is
> >> > > measuring. For UKCIP08, probability is measuring the degree to
>which
> >> > > future climates are consistent with the information used to
> >> > > construct
> >> > > the scenarios (climate model data, and observations) and the
> >> > > assumptions
> >> > > and methods used in constructing them i.e. they are a convenient
> >> > > summary
> >> > > statement of all that data given some assumptions, which are more
> >> > > usable
> >> > > than the data itself in helping planners make decisions. This is
> >> > > different to the definition learnt at school where probability of
> >> > > say
> >> > > rolling a dice can be measured by a repeated experiment. Climate
>is
> >> > > a
> >> > > one-off so there is no repeated experiment and so the schoolboy
> >> > > definition doesn't apply and this needs to be explained. A
> >> > > consequence
> >> > > of this is the PDF will change in UKCIPnext because better
>models,
> >> > > methods and more observations will change it.
> >> > >
> >> > > Deterministic: means the output (i.e. from a single run of a
>typical
> >> > > climate model) is based solely on the inputs (here the model, its
> >> > > input
> >> > > parameter values, and the initial conditions). What word are you
> >> > > contrasting this against. It should be contrasted against
>"random"
> >> > > or
> >> > > "stochastic" where there is a random element involved that can
> >> > > change
> >> > > the sytem. Hopefully, this is not be contrasted against
> >> > > "probabilistic".
> >> > >
> >> > >
> >> > > Cheers, David
> >> > >
> >> > >
> >> > >
> >> > >
> >> > >
> >> > > On Wed, 2007-06-13 at 16:32 +0100, Humphrey, Kathryn (CESA)
>wrote:
> >> > > > All,
> >> > > >
> >> > > > Attached is an updated set of key messages and glossary for the
> >> > > > UKCIP08 comms plan.
> >> > > >
> >> > > > For the glossary, the AR4 definitions for projections and

> >> > > scenarios
> >> > > > differ to those Roger has from the co-author of the WGII
>report.
> >> > > > Which do you want to use? Also if anyone has a better
>definition
> >> > > of
> >> > > > deterministic pls let me have it as the AR4 doesn't give one.
> >> > > You'll
> >> > > > also want to check the other definitions as I've either cut
>them
> >> > > down
> >> > > > from those presented in the AR4, or added sections to make them
> >> > > > UKCIP08 specific. Also the only definition I can find of a
> >> > > weather
> >> > > > generator is very old!
> >> > > >
> >> > > > Comments back to me by close Friday would be v helpful.
> >> > > >
> >> > > > Kathryn
> >> > > >
> >> > > > <<2007-06-13 comms plan Key Messages and glossary.doc>>
> >> > > >
> >> > > >
> >> > > > Department for Environment, Food and Rural Affairs (Defra)
> >> > > >
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>and/or
> >> > > > recorded to secure the effective operation of the system and
>for
> >> > > other
> >> > > > lawful purposes.
> >> > > --
> >> > > ____
> >> > > David Sexton PhD Climate Research Scientist
> >> > > Met Office Hadley Centre FitzRoy Road   Exeter   EX1 3PB UK
> >> > > Tel: +44 (0)1392 886524 Fax: +44 (0)1392 885681
> >> > > E-mail: david.sexton@metoffice.gov.uk
>http://www.metoffice.gov.uk
> >> > >
> >> > >
> >> > email message attachment
> >> > On Thu, 2007-06-14 at 11:03 +0100, Humphrey, Kathryn (CESA) wrote:
> >> > > <<2007-06-13 comms plan Key Messages and glossary.doc>> Some
>initial
> >> > > suggestions and comments
> >> > > I think UKCIP needs its own defs. AR4 too complex and
>'scientific'
> >> > > for lay users.
> >> > > Chris
> >> > >
> >>--

> >>_____
> >>David Sexton PhD Climate Research Scientist
> >>Met Office Hadley Centre FitzRoy Road   Exeter   EX1 3PB UK
> >>Tel: +44 (0)1392 886524 Fax: +44 (0)1392 885681
> >>E-mail: david.sexton@metoffice.gov.uk   http://www.metoffice.gov.uk
> >
>
>
>--
>--------
>
>
> > Dr Clare Goodess
> > Climatic Research Unit
> > School of Environmental Sciences
> > University of East Anglia
> > Norwich
> > NR4 7TJ
> > UK
> >
> > Tel: +44 -1603 592875
> > Fax: +44 -1603 507784
> > Web: http://www.cru.uea.ac.uk/
> >   http://www.cru.uea.ac.uk/~clareg/clare.htm
> >
> >
> >
>
>

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
NR4 7TJ
UK

</x-flowed>

Attachment Converted: "c:\eudora\attach\2007-06-14 comms plan Key Messages and glossary_goodess11.doc"

802. 1182255717.txt
##
##########

From: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: FW: retraction request
Date: Tue, 19 Jun 2007 08:21:57 -0400
Cc: Wei-Chyung Wang <wang@climate.cestm.albany.edu>

   Thanks Phil,
   We R now responding to a former TV weather forecaster who has got press, He has a web site
   of 40 of the USHCN stations
   showing less than ideal exposure. He claims he can show urban biases and exposure biases.
   We are writing a response for our Public Affairs. Not sure how it will play out.
   Regards, TOm

Phil Jones said the following on 6/19/2007 4:22 AM:

Wei-Chyung and Tom,
The Climate Audit web site has a new thread on the Jones et al. (1990) paper, with lots of quotes from Keenan. So they may not be going to submit something to Albany. Well may be?!?
Just agreed to review a paper by Ren et al. for JGR. This refers to a paper on urbanization effects in China, which may be in press in J. Climate. I say 'may be' as Ren isn't that clear about this in the text, references and responses to earlier reviews. Have requested JGR get a copy a copy of this in order to do the review.
In the meantime attaching this paper by Ren et al. on urbanization at two sites in China.
Nothing much else to say except:
1. Think I've managed to persuade UEA to ignore all further FOIA requests if the people have anything to do with Climate Audit.
2. Had an email from David Jones of BMRC, Melbourne. He said they are ignoring anybody who has dealings with CA, as there are threads on it about Australian sites.
3. CA is in dispute with IPCC (Susan Solomon and Martin Manning) about the availability of the responses to reviewer's at the various stages of the AR4 drafts. They are most interested here re Ch 6 on paleo.
Cheers
Phil

At 16:48 12/06/2007, Wei-Chyung Wang wrote:

FYI. WCW
PS I am flying out to Norway this afternoon. Keep in touch.
-----Original Message-----
From: Wei-Chyung Wang [[1]mailto:wang@climate.cestm.albany.edu]
Sent: Tuesday, June 12, 2007 11:46 AM
To: [2]doug.keenan@informath.org
Cc: 'WCW'; '[3]Kld@Asrc.Cestm.Albany.Edu'
Subject: RE: retraction request
Date: June 12, 2007
To: D. J. Keenan
Cc: K. Demerjian, Director, ASRC/SUNY-Albany

Dr. Keenan,
The only valid scientific issue described in your June 11, 2007 e-mailed pdf file (attached here as reference) concerning our 1990 GRL paper is the "station histories", while others are strictly your own opinions and therefore irrelevant to your inquiry. So let me elaborate further on this issue.
Digitization of the hard copies of "station histories" was prepared in 1989-90 by Ms. Zhao-Mei Zeng (IAP/CAS) only for the 60-station network, while the "station histories" of other stations, including those we used in 1990 urban warming study, were available in paper form, as I have already indicated in my 4/30/07 e-mail to you. Therefore, the use of the word "fabrication" in your document is totally absurd.
Concerning the current status of these hard copies of "station histories", Ms. Zeng told me when I was in Beijing in April 2007, that she no longer has the access to these information because it has been a long time (since 1990) and also IAP has moved office. But if you are interested, you can make an inquiry to the China Meteorological Administration using the web site: [4]http://211.147.16.25/ywwz/about/cma.php.
I believe that I have made it very clear what we had done with regard to the "station histories" in 1990 urban warming study. What and how you are going to proceed from now on is entirely your decision.
WCW
****

Dr. Wei-Chyung Wang

Professor of Applied Sciences
Atmospheric Sciences Research Center
State University of New York
251 Fuller Road
Albany, New York 12203
Tel: 518-437-8708
Fax: 518-437-8713
E-mail: [5]wang@climate.cestm.albany.edu
****
-----Original Message-----
From: D.J. Keenan [[6]mailto:doug.keenan@informath.org]
Sent: Monday, June 11, 2007 8:43 AM
To: Wei-Chyung Wang
Subject: Re: retraction request
Dear Dr. Wang,
I had something urgent arise, and so had to leave this matter for a while. Please find attached a rough draft report. If you believe the report to be inaccurate or misrepresentative, kindly let me know.
I hope that you will reconsider. If you decide to publish retractions, I will cease to bring this forward.
Sincerely,
Douglas Keenan
----- Original Message -----
From: [7]<wang@climate.cestm.albany.edu>
To: "'D.J. Keenan'" [8]<doug.keenan@informath.org>
Cc: "'Phil Jones'" [9]<p.jones@uea.ac.uk>; [10]<Thomas.R.Karl@noaa.gov>; "'Wei-Chyung Wang'" [11]<wang@climate.cestm.albany.edu>; "'Zeng Zhaomei'" [12]<zzm@tea.ac.cn>
Sent: Monday, 30 April, 2007 6:14
Subject: Re: retraction request
> Dr. Keenan,
>
> The discussion with Ms. Zeng last week in Beijing have re-affirmed
> that she used the hard copies of station histories to make sure that
> the selected stations for the study of urban warming in China have
> relatively few, if any, changes in instrumentation, location, or
> observation times over the study period (1954-1983).
>
> Regards,
>
> WCW
>
> ----------4/22/2007 4:46 PM e-mail Wang to Keenan---------
> Dear Dr. Keenan,
>
> I was really surprised to see your e-mail (below) after I logged into
> SUNYA webmail in Nanjing/China, after several days of disconnection
> (from internet) while travelling in central China.
>
> I flew to China early morning on 4/14, the day after your call to my
> office when I was in a meeting. My understanding was that you are
> going to call me again, but you never did.
>
> In any case, becuase of 4/14 trip to China, I origionally plan to
> respond to your 4/11 e-mailed questions when I return to Albany the
> end of this month. To answer your questions more accurately, I need
> to look into the file (if I can find it since it has been a long
> time), and also contact the co-author, Ms. Zeng, who brought the data
> and visited SUNYA as a visiting scientist from the Institute of
> Atmospheric Physics, Chinese Academy of Sciences, during that time.
>
> Regards,
>

> WCW
>
> ----- Original Message -----
> From: "D.J. Keenan" [13]<doug.keenan@informath.org>
> Date: Friday, April 20, 2007 8:31 am
> Subject: retraction request
>
>> Dear Dr. Wang,
>>
>> Regarding the Chinese meteorological data analyzed by Wang et al.
>> [GRL, 1990] and Jones et al. [Nature, 1990], it now seems clear that
>> there are severe problems. In particular, the data was obtained
>> from 84 meteorological stations that can be classified as follows.
>> 49 have no histories
>> 08 have inconsistent histories
>> 18 have substantial relocations
>> 02 have single-year relocations
>> 07 have no relocations
>> Furthermore, some of the relocations are very distant--over 20 km.
>>
>> Others are to greatly different environments, as illustrated here:
>> [14]http://www.climateaudit.org/?p=1323#comment-102970
>>
>> The above contradicts the published claim to have considered the
>> histories of the stations, especially for the 49 stations that have
>> no histories. Yet the claim is crucial for the research conclusions.
>>
>> I e-mailed you about this on April 11th. I also phoned you on April
>> 13th: you said that you were in a meeting and would get back to me.
>> I have received no response.
>>
>> I ask you to retract your GRL paper, in full, and to retract the
>> claims made in Nature about the Chinese data. If you do not do so, I
>> intend to publicly submit an allegation of research misconduct to
>> your university at Albany.
>>
>>
>> Douglas J. Keenan
>> [15]http://www.informath.org
>> phone + 44 20 7537 4122
>> The Limehouse Cut, London E14 6N, UK
>>

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 [16]p.jones@uea.ac.uk
NR4 7TJ
UK

--

Dr. Thomas R. Karl, L.H.D.

Director

NOAA's National Climatic Data Center

Veach-Baley Federal Building

151 Patton Avenue

Asheville, NC 28801-5001

Tel: (828) 271-4476

Fax: (828) 271-4246

[17]Thomas.R.Karl@noaa.gov

References

1. mailto:wang@climate.cestm.albany.edu
2. mailto:doug.keenan@informath.org
3. mailto:Kld@Asrc.Cestm.Albany.Edu
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7. mailto:wang@climate.cestm.albany.edu
8. mailto:doug.keenan@informath.org
9. mailto:p.jones@uea.ac.uk
10. mailto:Thomas.R.Karl@noaa.gov
11. mailto:wang@climate.cestm.albany.edu
12. mailto:zzm@tea.ac.cn
13. mailto:doug.keenan@informath.org
14. http://www.climateaudit.org/?p=1323#comment-102970
15. http://www.informath.org/
16. mailto:p.jones@uea.ac.uk
17. mailto:Thomas.R.Karl@noaa.gov

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##########

From: Thomas C Peterson <Thomas.C.Peterson@noaa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: Jones et al 1990
Date: Wed, 20 Jun 2007 08:27:50 -0400

Fascinating. Thanks for keeping me in the loop, Phil. I won't pass it on but I will keep
it in the back of my mind when/if Russ asks about appropriate responses to CA requests.
Russ' view is that you can never satisfy them so why bother to try?
It seems to me that what they are saying is the equivalent of accusing a doctor of
malpractice for not seeing a broken bone in a Chinese x-ray taken in 1985 when the break is
clearly visible in a state of the art 2005 Canadian MRI scan examined while wearing their
special problem finding glasses.
They also don't seem to understand the collaborative nature of the work, equivalent to
accusing you of faulty reading of metadata at the USHCN station in Reno because you quoted
a general USHCN statement that wasn't fully applicable to Reno.
Good luck.
Tom
Phil Jones said the following on 6/20/2007 3:59 AM:

Tom P.
Just for interest. Don't pass on.

Might be a precedent for your paper to J. Climate when
it comes out.
There are a few interesting comments on the CA web site.
One says it is up to me to prove the paper from 1990 was correct,
not for Keenan to prove we're wrong. Interesting logic.
Cheers
Phil
Wei-Chyung, Tom,
I won't be replying to either of the emails below, nor to any
of the accusations on the Climate Audit website.
I've sent them on to someone here at UEA to see if we
should be discussing anything with our legal staff.
The second letter seems an attempt to be nice to me,
and somehow split up the original author team.
I do now wish I'd never sent them the data after their FOIA
request!
Cheers
Phil

X-YMail-OSG:
wrT8WAEVM1myBGklj9hAiLvnYW9GqqFcbArMYvXDn17EHo1e0Vf5eSQ4WIGJljnsEw--
From: "Steve McIntyre" [1]<stephen.mcintyre@utoronto.ca>
To: "Phil Jones" [2]<p.jones@uea.ac.uk>
Subject: Jones et al 1990
Date: Tue, 19 Jun 2007 13:44:58 -0400
X-Mailer: Microsoft Outlook, Build 10.0.2627
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO
Dear Phil,

Jones et al 1990 cited a 260-station temperature set jointly collected by the
US
Deparment of Energy and the PRC Academy of Sciences, stating in respect to the
Chinese
stations:

The stations were selected on the basis of station history: we chose those with
few, if
any, changes in instrumentation, location or observation times.

This data set was later published as NDP-039
[3]http://cdiac.ornl.gov/epubs/ndp/ndp039/ndp039.html , coauthored by Zeng
Zhaomei,
providing station histories only for their 65-station network, stating that
station
histories for their 205-station network (which includes many of the sites in
Jones et al
1990) were not available:

(s. 5) Unfortunately, station histories are not currently available for any of
the
stations in the 205-station network; therefore, details regarding
instrumentation,
collection methods, changes in station location or observing times, and
official data
sources are not known.

(s. 7) Few station records included in the PRC data sets can be considered
truly
homogeneous. Even the best stations were subject to minor relocations or
changes in
observing times, and many have undoubtedly experienced large increases in

urbanization.
Fortunately, for 59 of the stations in the 65-station network, station histories (see
Table 1) are available to assist in proper interpretation of trends or jumps in the
data; however, station histories for the 205-station network are not available. In
addition, examination of the data from the 65-station data set has uncovered evidence of
several undocumented station moves (Sects. 6 and 10). Users should therefore exercise
caution when using the data.

Accordingly, it appears that the quality control claim made in Jones et al 1990 was
incorrect. I presume that you did not verify whether this claim was correct at the time
and have been unaware of the incorrectness of this representation. Since the study
continues to be relied on, most recently in AR4, I would encourage you to promptly issue
an appropriate correction.

Regards, Steve McIntyre

From: "D.J. Keenan" [4]<doug.keenan@informath.org>
To: "Steve McIntyre" [5]<stephen.mcintyre@utoronto.ca>
Cc: "Phil Jones" [6]<p.jones@uea.ac.uk>
Subject: Wang fabrications
Date: Tue, 19 Jun 2007 20:45:15 +0100
X-Mailer: Microsoft Outlook Express 6.00.2900.3138
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO
Steve,
I thought that I should summarize what has happened with the Wang case.
First, I concluded that the claims made about Chinese stations by Jones et al. [Nature,
1990] and Wang et al. [GRL, 1990] were very probably fabricated. (You very likely came
to the same conclusion.)
Second, some investigation showed that Phil Jones was wholly blameless and that
responsibility almost certainly lay with Wang.
Third, I contacted Wang, told him that I had caught him, and asked him to retract his
fabricated claims. My e-mails were addressed to him only, and I told no one about
them. In Wang's reply, though, Jones, Karl, Zeng, etc. were Cc'd.
Fourth, I explained to Wang that I would publicly accuse him of fraud if he did not
retract. Wang seemed to not take me seriously. So I drafted what would be the text of
a formal accusation and sent it to him. Wang replied that if I wanted to make the
accusation, that was up to me.
Fifth, I put a draft on my web site--
[7] http://www.informath.org/apprise/a5620.htm
--and e-mailed a few people, asking if they had any recommendations for improvement.
I intend to send the final version to Wang's university, and to demand a formal
investigation into fraud. I will also notify the media. Separately, I have

had a
preliminary discussion with the FBI--because Wang likely used government funds to commit
his fraud; it seems that it might be possible to prosecute Wang under the same statute
as was used in the Eric Poehlman case. The simplicity of the case makes this easier--no
scientific knowledge is required to understand things.
I saw that you have now e-mailed Phil (Cc'd above), asking Phil to publish a retraction
of Wang's claims: [8]http://www.climateaudit.org/?p=1741#comment-115879
There could be a couple problems with that. One problem is that it would be difficult
for Phil to publish anything without the agreement of Wang and the other co-authors
(Nature would simply say "no").
Another problem is that your e-mail says that you presume Phil was "unaware of the
incorrectness" of Wang's work. I do not see how that could be true. Although the
evidence that Phil was innocent in 1990 seems entirely conclusive, there is also the
paper of Yan et al. [Advances in Atmospheric Sciences, 18: 309 (2001)], which is cited
on my web page. Phil is a co-author of that paper.
Phil, this proves that you knew there were serious problems with Wang's claims back in
2001; yet some of your work since then has continued to rely on those claims, most
notably in the latest report from the IPCC. It would be nice to hear the explanation
for this. Phil?
Kind wishes, Doug
* * * * * * * * * * * *
Douglas J. Keenan
[9]http://www.informath.org
phone + 44 20 7537 4122
The Limehouse Cut, London E14 6N, UK

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
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Fax: +1-828-271-4328

References

1. mailto:stephen.mcintyre@utoronto.ca
2. mailto:p.jones@uea.ac.uk
3. http://cdiac.ornl.gov/epubs/ndp/ndp039/ndp039.html
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   9. http://www.informath.org/
  10. mailto:p.jones@uea.ac.uk

804. 1182346299.txt
##
##########

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Re: Fwd: Jones et al 1990
Date: Wed, 20 Jun 2007 09:31:39 -0600 (MDT)
Reply-to: trenbert@ucar.edu

Phil
Hang in there. I went thru this on the hurricane stuff and it was hard to
take. But responding to these guys unless they write papers is not the
thing to do.
Kevin
>
>   Kevin,
>   My problem is that I don't know the best course of action.
>   Just sitting tight at the moment taking soundings.
>   I'd be far happier if they would write some papers and act
>   in the normal way. I'd know how to respond to that. In
>   a way this all seems a different form of attack from that on Ben and
>   Mike in previous IPCCs.
>   I know I'm on the right side and honest, but I seem to be
>   telling myself this more often recently! I also know that 99.9%
>   of my fellow climatologists know the attacks are groundless.
>
>   Cheers
>   Phil
>
>
> At 14:54 20/06/2007, you wrote:
>>Phil
>>It is nasty. It is also very inappropriate. Even were some problems to
>>emerge over time, those should be addressed in a new paper by these guys.
>>Unfortunately all they do is criticise.
>>Kevin
>>
>>
>> >
>> >   Kevin,
>> >   Have also forwarded these emails to Susan and Martin, just
>> >   so they are aware of what is going on. The second email
>> >   is particularly nasty.
>> >
>> >   I'm not worried and stand by the original paper and also
>> >   Wei-Chyung. I do plan to do some more work on urban-related
>> >   issues. I also think there is some urban influence in more recent
>> >   Chinese series from the 1980s onwards. I've seen some Chinese
>> >   papers on this. They are not that well written though.
>> >
>> >   The CA web site has also had a go at David Parker's paper in
>> >   J. Climate (2006). David sent them the site locations and where
>> >   the data came from at NCDC. There are also threads on CA about
>> >   US HCN (Tom Karl and Peterson aware of these) and also about
>> >   IPCC and our responses to the various drafts.

>> >
>> >   Apologies for sharing these with you. It is useful to send to a
>> >   very small group, as it enables me to get on with some real work.
>> >
>> >   Cheers
>> >   Phil
>> >
>> >   Wei-Chyung, Tom,
>> >   I won't be replying to either of the emails below, nor to any
>> >   of the accusations on the Climate Audit website.
>> >
>> >   I've sent them on to someone here at UEA to see if we
>> >   should be discussing anything with our legal staff.
>> >
>> >   The second letter seems an attempt to be nice to me,
>> >   and somehow split up the original author team.
>> >
>> >   I do now wish I'd never sent them the data after their FOIA
>> >   request!
>> >
>> >   Cheers
>> >   Phil
>> >
>> >>X-YMail-OSG:
>> >>wrT8WAEVM1myBGklj9hAiLvnYW9GqqFcbArMYvXDn17EHo1e0Vf5eSQ4WIGJljnsEw--
>> >>From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
>> >>To: "Phil Jones" <p.jones@uea.ac.uk>
>> >>Subject: Jones et al 1990
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>> >>Jones et al 1990 cited a 260-station temperature set jointly
>> >>collected by the US Deparment of Energy and the PRC Academy of
>> >>Sciences, stating in respect to the Chinese stations:
>> >>
>> >>The stations were selected on the basis of station history: we chose
>> >>those with few, if any, changes in instrumentation, location or
>> >>observation times.
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>> >>This data set was later published as NDP-039
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>> >>, coauthored by Zeng Zhaomei, providing station histories only for
>> >>their 65-station network, stating that station histories for their
>> >>205-station network (which includes many of the sites in Jones et al
>> >>1990) were not available:
>> >>
>> >>(s. 5) Unfortunately, station histories are not currently available
>> >>for any of the stations in the 205-station network; therefore,
>> >>details regarding instrumentation, collection methods, changes in
>> >>station location or observing times, and official data sources are not
>> >> known.
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>> >>(s. 7) Few station records included in the PRC data sets can be
>> >>considered truly homogeneous. Even the best stations were subject to
>> >>minor relocations or changes in observing times, and many have
>> >>undoubtedly experienced large increases in urbanization.
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>> > From: "D.J. Keenan" <doug.keenan@informath.org>
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>> > Steve,
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>> > Fifth, I put a draft on my web site--
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>> > my web page. Phil is a co-author of that paper.
>> >
>> > Phil, this proves that you knew there were serious problems with
>> > Wang's claims back in 2001; yet some of your work since then has
>> > continued to rely on those claims, most notably in the latest report
>> > from the IPCC. It would be nice to hear the explanation for this.
>> Phil?
>> >
>> > Kind wishes, Doug
>> >
>> > *   *   *   *   *   *   *   *   *   *   *   *
>> > Douglas J. Keenan
>> > http://www.informath.org
>> > phone + 44 20 7537 4122
>> > The Limehouse Cut, London E14 6N, UK
>> >
>> >
>> > 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
>> > NR4 7TJ
>> > UK
>> >
>> ----------
>>
>>
>>_____
>>Kevin Trenberth
>>Climate Analysis Section, NCAR
>>PO Box 3000
>>Boulder CO 80307
>>ph 303 497 1318
>>http://www.cgd.ucar.edu/cas/trenbert.html
>
> 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
> NR4 7TJ
> UK
> ----------
>

_____
Kevin Trenberth

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Boulder CO 80307
ph 303 497 1318
http://www.cgd.ucar.edu/cas/trenbert.html

805. 1182361058.txt
##########

From: "Wahl, Eugene R" <wahle@alfred.edu>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: personal
Date: Wed, 20 Jun 2007 13:37:38 -0400

Hi Phil:

Glad I can help, even if quite indirectly. I know what you mean about the need for community when under duress. The individual quality of being a scientist works against us in this way. Attached are the original letter and the official UCAR response. I don't know what the lawyers might have written, other than their input to the official response letter. I do know they sought information from Caspar (and myself, but less so). I don't recall if we made available to them our correspondance with Steve Schneider about our responses to the review of WA that McIntyre did, which had a lot of information in it that debunked his claims about withholding contrary results, etc, etc.. In fact, we have never mentioned this to Steve, to make sure that he was in the situation to make editorial decisions as focused soley on the science as possible.

I was wondering if there is any way we as the scientific community can seek some kind of "cease and desist" action with these people. They are making all kinds of claims, all over the community, and we act in relatively disempowered ways. Note that UCAR did send the response letter to the presidents of the two academic institutions with which MM are associated, although this seems to have had no impact. Seeking the help of the attorneys you speak about would be useful, I should think. I know that Mike has said he looked into slander action with the attorneys with whom he spoke, but they said it is hard to do since Mike is, in effect, a "public" person -- and to do so would take a LOT of his time (assuming that the legal time could somewhow be supported financially). If I might ask, if you do get legal advice, could you inquire into the possibility of acting proactively in response via the British system? Maybe the "public" person situation does not hold there, or less so. I only ask you to consider this question on my part; obviously, please do what you deem best for your situation.

Finally, I have shared the MM letter and UCAR response before only with one other scientist, a now retired emminent person here in the US whom I asked to look over all the materials and give me his frank opinion if he felt we had done anything inappropriate. He came back with a solid "NO", and said that what MM were attempting was "unspeakable". Caspar has mentioned that UCAR said to him they did not want to disseminate these materials publically, and I have kept to that, other than the case mentioned. It seems clear to me that providing them to you is appropriate; I have not contacted Caspar to think about it at this point, and don't feel I need to. Anyway, this is just to give you the context on that side of things. I would imagine that sharing the doc's with legal persons you trust would be OK.

Note that I am now out of contact through July 9. I wish you all the best!!

Peace, Gene

From: Phil Jones [mailto:p.jones@uea.ac.uk]

Sent: Wed 6/20/2007 4:06 AM
To: Wahl, Eugene R
Subject: Fwd: Jones et al 1990

Gene,
Thanks for the email of support! I've taken up the idea of asking someone at UEA about legal advice.
I would like to see the original letter if possible. I won't pass this on. Did the NCAR/UCAR legal staff put anything in writing, as this might help me decide if the advice I might get here is reasonable? I'm sure it will be and I know I've nothing to worry about, as I've done nothing wrong and neither has Wei-Chyung.
It is good to share these sorts of things with a few people. I know Ben and Mike have been through this, but wasn't aware you and Caspar had. Thanks for your strength !

Cheers
Phil

Wei-Chyung, Tom,
I won't be replying to either of the emails below, nor to any of the accusations on the Climate Audit website.

I've sent them on to someone here at UEA to see if we should be discussing anything with our legal staff.

The second letter seems an attempt to be nice to me, and somehow split up the original author team.

I do now wish I'd never sent them the data after their FOIA request!

Cheers
Phil

X-YMail-OSG: wrT8WAEVM1myBGklj9hAiLvnYW9GqqFcbArMYvXDn17EHo1e0Vf5eSQ4WIGJljnsEw--
From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Jones et al 1990
Date: Tue, 19 Jun 2007 13:44:58 -0400
X-Mailer: Microsoft Outlook, Build 10.0.2627
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO

Dear Phil,

Jones et al 1990 cited a 260-station temperature set jointly collected by the US Deparment of Energy and the PRC Academy of Sciences, stating in respect to the Chinese stations:

The stations were selected on the basis of station history: we chose those with few, if any, changes in instrumentation, location or observation times.

This data set was later published as NDP-039 http://cdiac.ornl.gov/epubs/ndp/ndp039/ndp039.html , coauthored by Zeng Zhaomei, providing station histories only for their 65-station network, stating that station histories for their 205-station network (which includes many of the sites in Jones

et al 1990) were not available:

(s. 5) Unfortunately, station histories are not currently available for any of the stations in the 205-station network; therefore, details regarding instrumentation, collection methods, changes in station location or observing times, and official data sources are not known.

(s. 7) Few station records included in the PRC data sets can be considered truly homogeneous. Even the best stations were subject to minor relocations or changes in observing times, and many have undoubtedly experienced large increases in urbanization. Fortunately, for 59 of the stations in the 65-station network, station histories (see Table 1) are available to assist in proper interpretation of trends or jumps in the data; however, station histories for the 205-station network are not available. In addition, examination of the data from the 65-station data set has uncovered evidence of several undocumented station moves (Sects. 6 and 10). Users should therefore exercise caution when using the data.

Accordingly, it appears that the quality control claim made in Jones et al 1990 was incorrect. I presume that you did not verify whether this claim was correct at the time and have been unaware of the incorrectness of this representation. Since the study continues to be relied on, most recently in AR4, I would encourage you to promptly issue an appropriate correction.

Regards, Steve McIntyre

From: "D.J. Keenan" <doug.keenan@informath.org>
To: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
Cc: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Wang fabrications
Date: Tue, 19 Jun 2007 20:45:15 +0100
X-Mailer: Microsoft Outlook Express 6.00.2900.3138
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO

Steve,

I thought that I should summarize what has happened with the Wang case.

First, I concluded that the claims made about Chinese stations by Jones et al. [Nature, 1990] and Wang et al. [GRL, 1990] were very probably fabricated. (You very likely came to the same conclusion.)

Second, some investigation showed that Phil Jones was wholly blameless and that responsibility almost certainly lay with Wang.

Third, I contacted Wang, told him that I had caught him, and asked him to retract his fabricated claims. My e-mails were addressed to him only, and I told no one about them. In Wang's reply, though, Jones, Karl, Zeng, etc. were Cc'd.

Fourth, I explained to Wang that I would publicly accuse him of fraud if he did not retract. Wang seemed to not take me seriously. So I drafted what would be the text of a formal accusation and sent it to him. Wang replied that if I wanted to make the accusation, that was up to me.

Fifth, I put a draft on my web site--
http://www.informath.org/apprise/a5620.htm
<http://www.informath.org/apprise/a5620.htm> --and e-mailed a few people, asking if they had any recommendations for improvement.

I intend to send the final version to Wang's university, and to demand a formal

investigation into fraud. I will also notify the media. Separately, I have had a preliminary discussion with the FBI--because Wang likely used government funds to commit his fraud; it seems that it might be possible to prosecute Wang under the same statute as was used in the Eric Poehlman case. The simplicity of the case makes this easier--no scientific knowledge is required to understand things.

I saw that you have now e-mailed Phil (Cc'd above), asking Phil to publish a retraction of Wang's claims: http://www.climateaudit.org/?p=1741#comment-115879 There could be a couple problems with that. One problem is that it would be difficult for Phil to publish anything without the agreement of Wang and the other co-authors (Nature would simply say "no").

Another problem is that your e-mail says that you presume Phil was "unaware of the incorrectness" of Wang's work. I do not see how that could be true. Although the evidence that Phil was innocent in 1990 seems entirely conclusive, there is also the paper of Yan et al. [Advances in Atmospheric Sciences, 18: 309 (2001)], which is cited on my web page. Phil is a co-author of that paper.

Phil, this proves that you knew there were serious problems with Wang's claims back in 2001; yet some of your work since then has continued to rely on those claims, most notably in the latest report from the IPCC. It would be nice to hear the explanation for this. Phil?

Kind wishes, Doug

* * * * * * * * * * * *

Douglas J. Keenan
http://www.informath.org
<http://www.informath.org/> phone + 44 20 7537 4122
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Prof. Phil Jones
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NR4 7TJ
UK

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

Attachment Converted: "c:\eudora\attach\UCAR_response_to_MM V6.doc"

806. 1182795642.txt
##########

From: rob.allan@metoffice.gov.uk
To: Malcolm.Haylock@partnerre.com
Subject: Re: hello
Date: Mon, 25 Jun 2007 14:20:42 +0100
Cc: Gil Compo <compo@colorado.edu>, Gil Compo <Gilbert.P.Compo@noaa.gov>, Henry Beverley <Beverley.Henry@nrw.qld.gov.au>, Roger Stone <stone@usq.edu.au>, Adrian Simmons <Adrian.Simmons@ecmwf.int>, Brönnimann Stefan <stefan.bronnimann@env.ethz.ch>, Frank Le Blancq <leblancq.f@jerseymet.gov.je>, Phil Jones <p.jones@uea.ac.uk>, Pamela_Heck@swissre.com,

Paul.Della-Marta@meteoswiss.ch, Scott D Woodruff <Scott.D.Woodruff@noaa.gov>,
Meinke@metoffice.gov.uk, Holger <holger.meinke@wur.nl>, Juerg Luterbacher
<juerg@giub.unibe.ch>, tlorencak@bluewin.ch

On Mon, 2007-06-25 at 14:50 +0200, Malcolm.Haylock@partnerre.com wrote:
>
> Hi Rob,
>
> Great to hear about the new project and the support of the Queensland
> Government. It sounds like a very worthwhile project from both a
> scientific and user's perspective.
>
> I wrote a summary of your email and your good work with historical SLP
> and sent it to my boss, Hervé Castella, who is the head of research at
> PartnerRe. He is well aware of the value of reanalyses as we use ERA40
> extensively for developing our European storm climatology.
>
> We would be very interested to attend such a meeting bringing the data
> developers and users together. We would also be happy to partly
> sponsor such a meeting. However the main concern, as with the case of
> ERA40 data, is that the final data can be very expensive for
> commercial users so sponsorship would probably require an agreement
> about access.
>
> Regarding venues, if you'd like input from the reinsurance industry
> then there is no better location than Zurich. It also has excellent
> access to Nth America becuase of the financial connections.
>
> Malcolm
>
> rob.allan@metoffice.gov.uk wrote on 19/06/2007 11:15:06:
>
> > On Tue, 2007-06-19 at 10:45 +0200, Malcolm.Haylock@partnerre.com
> wrote:
> > >
> > > Hi Rob,
> > >
> > > How's it going? Paul and I saw Tara yesterday. It's great to have
> her
> > > in Zurich. She said things are looking brighter for you at the
> > > MetOffice. Still, whay not come and join the growing Aussie empire
> in
> > > Switzerland?
> > >
> > > Malcolm
> > > DISCLAIMER: This e-mail contains information solely intended for
> > named recipients and is confidential and proprietary to PartnerRe.
> > If you are not one of the intended recipients of this message, you
> > must not read, use or disseminate the information in it and should
> > notify the sender by replying to this message and deleting it
> > afterwards from your mail system. Please be aware that unauthorized
> > reproduction or distribution of this communication is prohibited.
> > >
> >
> >
> > Malcolm,
> >   Good to hear from you.
> >
> >   Glad that you guys caught up with Tara, it's great that she
> > has fellow Aussies in the
> > vicinity to catch up with.
> >
> >   I just spoke to Paul Della-Marta on the phone about matters

> > to do with my new role
> > here in the Hadley Centre, and I'd like any thoughts you might have
> > on a potential meeting
> > linked to that new role.
> >
> > NEW ROLE
> >
> > Basically, as of next month, I'll be officially the Project
> > Manager of an initiative
> > called ACRE (Atmospheric Circulation Reconstructions over the
> > Earth). Though based in the
> > Hadley Centre, this post is being primarily funded by the Queensland
> > Climate Change Centre of
> > Excellence (QCCCE) in Australia!! It is an 'end-to-end' project
> > covering data and reanalyses
> > at one end and looking to make the reanalyses products flow
> > 'seamlessly' into various climate
> > applications models at the other. I came up with the concept, got
> > the infrastructure together
> > to make it work and sold QCCCE on it without any Met Office or
> > Hadley Centre input initially.
> >
> > Anyway, a major component of my new role is to support and
> > facilitate the global daily
> > to sub-daily surface pressure data requirements for historical
> surface
> > observations only reanalyses (the 20th Century Reanalysis Project)
> that
> > a colleague, Dr Gil Compo at NOAA ESRL/CIRES/CDC in the US, is
> leading -
> > see this link for an overview of the 20th Century Reanalysis Project
> > (http://www.noaanews.noaa.gov/stories2007/s2771.htm).
> >
> > We aim to build on the expertise developed by the 20th
> Century
> > Reanalysis Project to provide the basis for surface observations-
> based
> > reanalyses which have sufficient data coverage to be valid globally
> back
> > to the mid-19th century and specifically over the North Atlantic-
> > European region from the mid-18th century to the present.
> >
> > MEETING AS PART OF MY NEW ROLE
> >
> > The background to this is as follows:
> >
> > Gil Compo and I plus those in the GCOS AOPC/OOPC Surface
> > Pressure Working Group (SPWG) have had the hope for a while now
> > of being able to fund a meeting of the SPWG in its own right, rather
> > than 'piggy backing' on other meetings all the time. The US members
> of
> > the SPWG had been hoping for a meeting in, or closer to, the US.
> With
> > all that in mind I suggested Bermuda as a venue, given that the
> > Biological Institute of Ocean Sciences there have strong links to
> the
> > reinsurance industry and a particular focus on European storminess.
> >
> > The Bermuda idea has waxed and waned a bit, and though there
> is
> > now the possibility of some potential funding via Howard Diamond
> (the US
> > GCOS Rep) to support such a meeting, doing the figures shows that it

> is
> > going to be too expensive to hold it in Bermuda. However, with my
> new
> > role as the Project Manager of the ACRE initiative developing in
> > parallel with the above, I'm now thinking of a somewhat more
> effective
> > and reshaped meeting probably held in Europe.
> >
> >   My current thoughts revolve around the idea of holding a
> smallish
> > but manageable meeting. The focus being on bringing together the
> GCOS
> > AOPC/OOPC Working Groups on pressure (SPWG), SST and sea-ice,
> > atmospheric reference observations plus the new one on observational
> > datasets for reanalysis, with climate applications and reinsurance
> > people, to focus on the various reanalysis data needs and on
> potential
> > climate applications and impacts usage of such reanalysis products.
> > This type of meeting fits the very core of what my ACRE Project
> > Manager's role is about. I also think strategically it might provide
> a
> > very useful focus all round which will promote the need for more
> data,
> > clarify the current and potential situation with the various
> reanalysis
> > efforts and their needs, and give the climate applications community
> a
> > better idea of what the data and reanalysis products can be best
> used
> > for.
> >
> >   One recent example highlights the sort of problem that exists
> > over this way with reanalyses and the climate applications side. The
> > European Environment Agency (EEA) have been talking to ECMWF about
> using
> > their reanalysis products (for wind and energy planning plus
> storminess
> > trends), but from what I've heard and discussed with Adrian Simmons
> (the
> > AOPC Chair and ECMWF ERA reanalysis person), the EEA really don't
> > understand the strengths and weaknesses of the ERA reanalysis
> product
> > and how best to use it for their needs. As a result, this potential
> > linkage has tended to flounder somewhat.
> >
> >   I also understand that a Spanish colleague is looking to set
> up a
> > COST (Co-operation on Science and Technology) Action under the EC
> COST
> > program that would focus on reanalyses and I think applications.
> I'm
> > going to suggest to him that the sort of meeting I'm looking to
> initiate
> > could also be linked to his efforts and be an initial meeting for
> such a
> > COST Action.
> >
> >   I've talked to Roger Stone and Holger Meinke on the climate
> > applications side, plus others on the climate and reanalysis side of
> > things (Gil Compo, Adrian Simmons, Stefan Bronnimann) about such a
> > meeting and have had considerable interest. Roger mentioned his
> links
> > with the reinsurance industry in Europe in looking to link them

> (maybe
> > even part fund) into such a meeting, and I'm going to follow up on a
> > similar tack. I'm thinking that it could be a milestone for the
> first
> > year of my contract, and something that could also be duplicated in
> > Australia or elsewhere.
> >
> > Thus, I'd be very keen to hear your thoughts on any of the
> above,
> > and how we might be able to make it happen for the benefit of all.
> Some
> > ideas for venues I've had are Jersey or Guernsey in the Channel
> Islands
> > and Dublin (this might be easiest for US attendees to get to).
> >
> > Cheers, Rob.
> >
> >
> > Dr Rob Allan Climate Scientist
> > Met Office FitzRoy Road Exeter EX1 3PB United Kingdom
> > Tel: +44 (0)1392 886904 Fax: +44 (0)1392 885681
> > E-mail (W): rob.allan@metoffice.gov.uk http://www.metoffice.gov.uk
> > E-mail (H): rallan@onetel.com

Malcolm,
  Thanks for that, much appreciated.

  I'll forward it on to Gil Compo and others linked to ACRE and the AOPC WGs. I think that Roger Stone from Queensland knows some of your people, so there should be some good links all round.

  I've also gone back to Howard Diamond, the US GCOS Rep, from whom I'm hoping to get some financial support for such a meeting to gauge his reaction to holding it in Europe.

  Cheers, Rob.

--
Dr Rob Allan ACRE Project Manager
Met Office FitzRoy Road Exeter EX1 3PB United Kingdom
Tel: +44 (0)1392 886904 Fax: +44 (0)1392 885681
E-mail (W): rob.allan@metoffice.gov.uk http://www.metoffice.gov.uk
E-mail (H): rallan@onetel.com

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##########

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>, Tom Melvin <t.m.melvin@uea.ac.uk>
Subject: Re: AD 536
Date: Tue, 3 Jul 2007 17:52:39 +0600
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith and Tom,

thank you to include me in co-authors list of the paper.
I'm not sure that it is right, nevertheless I can't refuse. However,
if you consider to reduce number of co-authors I would not be offended
if you exclude me.

My corrections and suggestions:

1) Table S1: for Yamal - elevation 10-60 m, east - 70°, north - 67°30'

2) may be add reference to presence of frost rings in AD 536 in
Siberian pine in Mongolia (D'Arrigo et al., Climatic change, 49,
239-246, 2001) and frost and light rings in larch from Yamal (our
data)?

3) if possible, add to acknowledgments my thanks for funding to
Russian Foundation for Basic Research (project # 07-05-00989)

4) just to satisfy my curiosity - if dating of ice layers is not too
precise, why not suppose that first peak of sulphate deposits (about
AD 529 in fig. 3b) correspond to AD 536? May be two eruptions are
reason of relative long growth suppression? By the way, in larch from
Yamal frost rings formed in 536, 543 (two times as much as 536), and
545 (previous frost rings year was AD 404, next AD 627).

I'm sorry, I didn't reply to your previous letter concerning
manuscript to Philosophical Transactions of Royal Society. If it is
not too late, please correct my name in co-authors list (Rashit).

Best regards

Rashit Hantemirov

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8 Marta St., 202
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Russia
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Fax: +7(343) 260-65-00, 260-82-56
E-mail: rashit@ipae.uran.ru

2 èþëÿ 2007 ã., 19:29:57 you wrote:

> Dear Matti, Kurt, Hakan, Bjorn, Rashit and Mukhtar,

> Attached is a letter of explanation from Keith (Briffa) and a draft
> of a paper to be submitted with a request for you all to be co-authors.

> The list of authors details, the tree-ring data Figure 1, and
> supporting table all need to be checked.

> (e.g. Kurt - is there a better name for your sites?)

> Thanks

> Tom

> Dr. Tom Melvin

> Climatic Research Unit
> University of East Anglia
> Norwich, NR4 7TJ, U.K.

> Phone: +44-1603-593161
> Fax: +44-1603-507784

> _____ NOD32 2369 (20070702) Information _____

> This message was checked by NOD32 antivirus system.
> http://www.eset.com

808. 1183753398.txt
##
##########

From: Martin Juckes <m.n.juckes@rl.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: Mitrie
Date: Fri, 6 Jul 2007 16:23:18 +0100
Cc: Jan Esper <esper@wsl.ch>, anders.moberg@natgeo.su.se, Eduardo.Zorita@gkss.de,
hegerl@duke.edu, k.briffa@uea.ac.uk, m.allen1@physics.ox.ac.uk, weber@knmi.nl

Content-Type: text/plain;
  charset="utf-8"
Content-Disposition: inline
X-MIME-Autoconverted: from 8bit to quoted-printable by oin.rl.ac.uk id
l66FNNrC019808

Thanks to Tim and Keith for that correction.

I've inserted that, and also reworded the paragraph in the conclusions which
talked about "serious flaws" along the lines suggested by Tim. It now reads:
"The IPCC2001 conclusion that temperatures of the past millennium
are unlikely to have been as warm, at any time prior to the 20th
century, as the last decades of the 20th century is supported
by subsequent research and by the results obtained here.
We have also reviewed and, in some cases, tested with new
analysis, papers which claim to refute that IPCC2001 conclusion and
found that their claims are not well supported."

This version attached with the revised supplementary material.
I need to go over the `changes' document again, and the response, but I hope
to send it in on Monday.

cheers,
Martin

On Wednesday 04 July 2007 16:54, Tim Osborn wrote:
> Hi Martin & Jan (and others)
>
> Keith and I have put together the attached text as an alternative,
> hopefully more accurate, version to the current paragraph about
> differences between tree series. We did this before/while Jan's
> email arrived, so some overlap but hopefully what we say is
> compatible with Jan's comment. Note we haven't discussed the ice
> core data from Fisher, just the tree-ring series.

> How does the attached sound?
>
> Cheers
>
> Tim
>
> At 22:15 03/07/2007, Jan Esper wrote:
> >Martin
> >This is quite a task, as I do not really remember which version of a
> >dataset was used in which paper.
> >
> >For ECS2002, I detrended all data via two RCS runs applied to the
> >"linear" and "non-linear" sub-groups as identified in that paper.
> >All data except for Boreal and Upper Wrigth (both from Lisa
> >Graumlich) and Mongolia (from Gordon Jacoby) were measured at WSL.
> >
> >I wouldn't necessarily claim that the regional chronologies from the
> >ECS approach are highly useful records, i.e. for a regional analysis
> >I would use data that are detrended region-by-region.
> >
> >(ÂŠthat used by ECS2002 is based on the same tree-ring data as that
> >used by MSH2005, but with a different standardisation method.)
> >Not fully sure what MSH2005 did, but this is very likely correct,
> >i.e. they likely used a "regional" version from Briffa and/or Grudd.
> >
> >(The Fennoscandia data used by JBB1998, MBH1999 also come from the
> >Tornetraesk area, but from a different group of trees.)
> >Hm..., I don't believe that these studies used different trees. Up
> >to the recent update by Hakan Grudd, that is currently in review
> >with Climate Dynamics, there was effectively only one dataset from
> >Tornetrask. Keith or Tim might know this better.
> >
> >(The Polar Urals series used by ECS2005 is also a reanalysis of the
> >data used to create the Northern Urals series used by JBB1998, MBH1999.)
> >I wouldn't necessarily call this a reanalysis. Perhaps better say
> >'differently detrended'. Anyway, I doubt that there is a long
> >dataset from the Northern Ural as there is little wood preserved in
> >that area. This is likely the same data, i.e. both are Polar Ural.
> >
> >(The Taymir data used by HCA2007 is a smoothed version of that used
> >in ECS2002, MSH2005.)
> >This I really don't knowÂŠ but it would be better to use a regionally
> >detrended version of the data...
> >
> >(The Greenland stack data used by MBH1999 is a composite of data
> >analysed by \citet{fisher_etal1996}, but the precise nature of the
> >composite is not described by \citet{fisher_etal1996}.")
> >Agreed. Just read the paper again, and it is indeed difficult to say
> >which data was combined.
> >
> >(I've kept the phrase about "serious flaws" in the conclusion,
> >despite Tim's suggestion, supported by Nanne, of a weaker wording,
> >because I think it is important to draw attention to the serious
> >flaws which are there.)
> >I also think that a less aggressive wording would be more effective.
> >
> >-- Jan
> >
> >
> >
> >
> >At 16:41 Uhr +0100 3.7.2007, Martin Juckes wrote:

> >>Hello,
> >>
> >>another version of our paper is attached.
> >>
> >>I've added the following paragraph to the discussion of Table 1, and I'd
be
> >>grateful if Jan and Keith could check that it is accurate:
> >>"Evaluation of past work is further compicated by confusion between
closely
> >>related proxy series. In Tab.~1 there are two series referred to as
> >>Tornetraesk: that used by ECS2002 is based on the same tree-ring data as
that
> >>used by MSH2005, but with a different standardisation method. The
> >>Fennoscandia data used by JBB1998, MBH1999 also come from the Tornetraesk
> >>area, but from a different group of trees. The Polar Urals series used by
> >>ECS2005 is also a reanalysis of the data used to create the Northern Urals
> >>series used by JBB1998, MBH1999. The Taymir data used by HCA2007 is a
> >>smoothed version of that used in ECS2002, MSH2005.
> >>The Greenland stack data used by MBH1999 is a composite of data analysed
by
> >>\citet{fisher_etal1996}, but the precise nature of the composite is not
> >>described by \citet{fisher_etal1996}."
> >>
> >>I've also moved a few things around and tried to follow most of the
> >>suggestions from Anders and Nanne. I've kept the phrase about "serious
flaws"
> >>in the conclusion, despite Tim's suggestion, supported by Nanne, of a
weaker
> >>wording, because I think it is important to draw attention to the serious
> >>flaws which are there. One reviewer has implied that we should not discuss
> >>flawed work at length because in oding so we give it credibility it does
not
> >>deserve. I believe that since this stuff is published and influential in
some
> >>quarters we should discuss it and draw attention to the fact that it is
> >>seriously flawed.
> >>
> >>cheers,
> >>Martin
> >>
> >>Attachment converted: Hennes:cp-2006-0049-rv 3.pdf (PDF /Â«ICÂ») (001588D6)
> >
> >
> >
> >--
> >
> >Jan Esper
> >Head Dendro Sciences Unit
> >Swiss Federal Research Institute WSL
> >Zuercherstrasse 111, 8903 Birmensdorf, Switzerland
> >Voice: +41-44-739 2510 or +41-44-739 2579
> >Fax: +41-44-739 2515http://www.wsl.ch/staff/jan.esper
>

Attachment Converted: "c:\eudora\attach\cp-2006-0049-rv4.pdf"

Attachment Converted: "c:\eudora\attach\cp-2006-0049-sp1.pdf"

809. 1184779319.txt
##
##########

From: P.Jones@uea.ac.uk
To: david.parker@metoffice.gov.uk
Subject: RE: UHI corrections
Date: Wed, 18 Jul 2007 13:21:59 +0100 (BST)
Cc: "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>, "Jones, Phil" <p.jones@uea.ac.uk>

Geoff,
David is essentially right. In 1986 we rejected 38 (if my memory from 1986) is correct! I don't recall the number we looked at so I can't give a percentage, as I'm not that much of a trainspotter.
The % would be small though, as we looked the homogeneity of about 2500 then. Also some which might have been affected by urbanization might have been rejected for other reason. I'm half asleep here in my hotel room in Beijing (same hotel as the IPCC meeting David!) as it is just gone 8pm! I have the pdf of the 1986 paper and 38 rejected for urban warming trends (31 in N. America and 7 in Europe - none elsewhere) out of 2666. 239 were rejected for other reasons.

Brohan et al is the best reference. We included urbanization as one of the biases (one sided as urban should lead to warming, so if you look very, very closely at the error range in the paper you'll see it is slightly one-sided.

I've been giving some talks here and have more tomorrow. At CMA I've found they have a homogenized dataset of 745 stations for the country which they are preapred to give me at some point for inclusion. They have adjusted for all site moves but not for urbanization. It seems that it is almost impossible for sites here to be rural (maybe only 1% of the total). Sites move out of the city at regular intervals as the cities expand. So Beijing has 6-7 site moves since 1951! Also China seems to be the only country that doesn't use airport sites. None are located at airports. I'm going to give them my Chinese sites in return so they can do some comparisons. I'll talk with their person (Mr Li) more tomorrow.

Another interesting bit of work here is that they also have an homogenized set of monthly wind speed data from 1951. Not sure how they homogenize this for site moves, but almost all the sites (about 200) show declines in mean wind speeds since 1951. NCEP and ERA-40 also show this for wind speeds at 1000, 925 and 850hPa as well. Odd thing is that they think the decline in wind speeds is due to urbanization! - Li's English isn't great though, so I could be wrong. Another person I've been talking to has been looking at precip trends from 1951 - again they think declines in N. China are due to urbanization! Odd then that there are increases in S. China, which is also urbanized at similar rates.

Air quality here is awful - I saw the sun for the first time since arrival on Sunday, after a long downpour cleared the air this morning! The haze will be back tomorrow. Apparently they will closing the worst factories and getting half the cars off the road next August for the Olympics! Traffic might

flow better for the latter, but can't see the former
doing that much good. What they need to do is to get
a heavy downpour every early morning!

Cheers
Phil

> Geoff
>
> It is correct that Phil Jones removes stations that appear to have urban
> warming, unlike Hansen et al. who correct them. I don't know the
> percentage of stations that Phil removes; details were probably
> originally given in the Jones et al 1985 and 1986 USDoE reports (see
> references given in Jones and Moberg, 2003 (attached); the reports are
> probably only available on paper and are not now in my collection of
> box-files!) and could take some time to collate. But to do this might
> not be useful as Phil could have rejected further stations from the
> additional datasets he accrued since then. Nevertheless I expect the
> rejection rate is small.
>
> Brohan et al is the best reference for a discussion of the urbanization
> uncertainty in land surface air temperatures.
>
> I hope this helps somewhat.
>
> Regards
>
> David
>
>
> On Wed, 2007-07-18 at 11:46 +0100, Jenkins, Geoff wrote:
>> David
>>
>> If I understand Phil right, there are no stations which are CORRECTED
>> for UHI effects, but there are several (roughly what percentage?) which
>> are REMOVED. I would be grateful if you could give me the best ref to
>> this (is it Brohan et al 2006), to pass to an outside sceptical enquirer
>> (one Nigel Lawson, remember him?). He already knows about yr recent
>> windy/calm comparison paper via the "Briefing" booklet I did.
>>
>> Thanks
>>
>> Geoff
>>
>> -----Original Message-----
>> From: P.Jones@uea.ac.uk [mailto:P.Jones@uea.ac.uk]
>> Sent: 16 July 2007 21:59
>> To: Jenkins, Geoff
>> Subject: Re: UHI corrections
>>
>>
>>
>> Geoff,
>>   In China this week and away next week. Best Ref is
>> really Ch3 of AR4 (IPCC). We don't make adjustments
>> just remove the stations affected.
>>
>>   Best if you contact David Parker. There is also
>> some stuff in Brohan et al. (2006) in JGR. Also
>> David P has a couple of papers on the subject.
>>
>>   We incorporate possible residual urban effects into

>> the error estimates of global T.
>>
>> Cheers
>> Phil
>>
>>
>> > Phil
>> >
>> > Sorry to keep bombarding you. What is the best ref to your corrections
>>
>> > of land surface temps (in CRUTEM, presumably) for heat island effects,
>>
>> > please?
>> >
>> > Geoff
>> >
>> > Dr Geoff Jenkins
>> > Manager, Climate Change Scenarios
>> > Hadley Centre
>> > Met Office
>> > FitzRoy Road, EXETER, EX1 3PB, UK
>> > tel: +44 (0) 787 966 1136
>> > geoff.jenkins@metoffice.gov.uk
>> > www.metoffice.gov.uk
>> >
>> >
>>
> --
> David Parker Met Office Hadley Centre FitzRoy Road EXETER EX1 3PB UK
> E-mail: david.parker@metoffice.gov.uk
> Tel: +44-1392-886649   Fax: +44-1392-885681
> http://www.metoffice.gov.uk
>
>
>

810. 1188412866.txt
##
##########

From: Kevin Trenberth <trenbert@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Something not to pass on
Date: Wed, 29 Aug 2007 14:41:06 -0600
Cc: "Michael E. Mann" <mann@meteo.psu.edu>

   Phil
   Confidential: Dennis Shea just had angiogram: 75% blockage: having open heart
surgery
   tomorrow morning. He does not want this known till the operation results are
known.
   ============
   This is awful stuff and I can't imagine that this could be published. I know of
this
   fellow Peiser though and he is extremely biased (against you likely). So
treading with
   caution is warranted. The email seems to invite a comment but not a review. You
should
   probably only respond with something that you would not mind being published.
You can also

point out errors of fact. Whether you point out errors of logic or opinion is another
matter altogether. If you write just to the editor you can try to evaluate the comment and
point out that it lacks substance.
I think my approach would be to try to stick to science.e.g.
I don't know what was done for the 1990 paper but obviously sound practice is
1) we attempt to use homogeneous data
2) Site moves are one indication of lack of homogeneity but there are standard means of
adjusting for such moves especially when there is an overlap in the record.
3) All data are scrutinized for possible problems and discontinuities, especially if there
is a question about a possible move and the date is known.
4) Site movements do not necessarily prejudice the record toward warming or cooling: a move
from the inner city to an outlying airport can result in cooling, for instance.
5) Revisions are made when new information becomes available.
6) It is helpful if researchers can improve the records and provide updated analyses.
Or something to this effect. You could try a patronizing approach of over explaining the
difficulties.
At the very least you should be critical of the statement in 4. that he "politely requested
an explanation". He quotes you as saying:
"Why should I make the data available to you, when your aim is to try and find something
wrong with it?".[1][1]
_____

[2][1] McIntyre S. (19 July 2006), Submission to the Subcommittee on Oversight and
Investigations (Committee on Energy and Commerce, U.S. House of Representatives). This is a
sworn statement by McIntyre. [It is available at

[3]http://energycommerce.house.gov/reparchives/108/Hearings/07192006hearing1987/McIntyre.pd
f.]

but you have no reason to be defensive: if there was a problem with the data and all due
care was taken, then if there is something wrong with it, it was the responsibility of
those who took the data, not those who used it responsibly. You should also point out that
the data are just as available to anyone as to you.

In the IPCC report we are careful to say that there are urban effects and they are
important and we have a lot about them. But they are small on the global scale. His
conclusions are wrong. Also the IPCC evaluates published works and does not do research or
deal with raw data.
In the appendix, presumably the quotes are based on the best information at the time. That
was then.
The conclusions of the author that fabrication occurred is not valid. Maybe things could
have been done better, but that universally applies.
Let me know if you want more concrete suggestions

Kevin
Phil Jones wrote:

Kevin, Mike,
Sending just for your thoughts. The Appendix of this attachment has gone to SUNY Albany and is being dealt with by them. Not sure when, but Wei-Chyung has nothing to worry about.
I've sent to Wei-Chyung and also to Tom Karl. Q is should I respond? If I don't they will misconstrue this to suit their ends. I could come up with a few sentences pointing out the need to look at the Chinese data rather than just the locations of the sites. Looking further at Keenan's web site, he's not looked at the temperature data, nor realised that the sites he's identified are the urban stations from the 1990 paper. He has no idea if the sites for the rural Chinese stations moved, as he doesn't seem to have this detail. Whatever I say though will be used for whatever, so it
seems as though I'm damned if I do and damned if I don't.
Does the email suggest to you this is a request for a formal review? E&E have an awful track record as a peer-review journal.
Footnote 8 is interesting. Grape harvest dates are one of the best documentary proxies.
Cheers
Phil

Subject: review of E&E paper on alleged Wang fraud
Date: Wed, 29 Aug 2007 15:18:04 +0100
X-MS-Has-Attach: yes
X-MS-TNEF-Correlator:
Thread-Topic: review of E&E paper on alleged Wang fraud
thread-index: AcfqPgYII3NKEW8US8uwftlkhnxNhgAB/4xQAAA5K8A=
From: "Peiser, Benny" [4]<B.J.Peiser@ljmu.ac.uk>
To: [5]<p.jones@uea.ac.uk>
X-OriginalArrivalTime: 29 Aug 2007 14:18:06.0729 (UTC) FILETIME=[6B4F5F90:01C7EA47]
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO
Dear Dr Jones
I have attached a copy of Doug Keenan's paper on the alleged Wang fraud that was submitted for the forthcoming issue of Energy & Environment [6]http://www.ingentaconnect.com/content/mscp/ene.
I was wondering whether you would be happy to comment on its content and factual accuracy. Your comments and suggestions would be much appreciated. We would need your feedback by Sept 17.
I look forward to hearing from you.
Yours sincerely
Benny Peiser
Guest editor, E&E
Liverpool John Moores University, UK

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 [7]p.jones@uea.ac.uk
NR4 7TJ
UK

--
****
Kevin E. Trenberth e-mail: [8]trenbert@ucar.edu

Climate Analysis Section, [9]www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

References

Visible links
1. file://localhost/tmp/convertmbox5320.html#_ftn1
2. file://localhost/tmp/convertmbox5320.html#_ftnref1
3. http://energycommerce.house.gov/reparchives/108/Hearings/07192006hearing1987/McIntyre.pdf
4. mailto:B.J.Peiser@ljmu.ac.uk
5. mailto:p.jones@uea.ac.uk
6. http://www.ingentaconnect.com/content/mscp/ene
7. mailto:p.jones@uea.ac.uk
8. mailto:trenbert@ucar.edu
9. http://www.cgd.ucar.edu/cas/trenbert.html

Hidden links:
10. file://localhost/tmp/convertmbox5320.html#_ftn1

811. 1188478901.txt
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: RE: review of E&E paper on alleged Wang fraud
Date: Thu, 30 Aug 2007 09:01:41 -0400
Cc: Kevin Trenberth <trenbert@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>
thanks Phil,

I did take the liberty of discussing w/ Gavin, who can of course be trusted to maintain the confidentiality of this. We're in agreement that Keenan has wandered his way into dangerous territory here, and that in its current form this is clearly libellous; there is not even a pretense that he is only investigating the evidence. Furthermore, while many of us fall under the category of 'limited public figures' and therefore the threshold for proving libel is quite high, this is *not* the case for Wei-Chyung. He is not a public figure. I believe they have made a major miscalculation here in treating him as if he is. In the UK, where E&E is published, the threshold is even lower than it is in the states for proving libel. We both think he should seek legal advice on this, as soon as possible.

With respect to Peiser's guest editing of E&E and your review, following up on Kevin's suggestions, we think there are two key points. First, if there are factual errors (other than the fraud allegation) it is very important that you point them out now. If not, Keenan could later allege that he made the claims in good faith, as he provided you an opportunity to respond and you did now. Secondly, we think you need to also focus on the legal implications. In particular, you should mention that the publisher of a libel is also liable for damages - that might make Sonja B-C be a little wary. Of course, if it does get published, maybe the resulting settlement would shut down E&E and Benny and Sonja all together! We can only hope, anyway. So maybe in an odd way its actually

win-win for us, not them. Lets see how this plays out...

RealClimate is of course always available to you as an outlet, if it seems an appropriate venue. But we should be careful not to jump the gun here.

Kevin: very sorry to hear about Dennis. Please pass along my best wishes for a speedy recovery if and when it seems appropriate to do so...

Mike

Phil Jones wrote:
> Mike, Kevin,
>   Thanks for your sets of thoughts. I've been in touch with Wei-Chyung,
> who's in China at the moment. He forwarded the 'paper!' to the people
> dealing
> with Keenan's allegations at SUNY. He got a reply to say that Keenan
> has now violated the confidentiality agreement related to
> the allegation. So, it isn't right to respond whilst this is
> ongoing. I will
> draft something short though, whilst it's all fresh in my mind. Then
> I can
> get onto something else.
>   I did send the email below to Peiser clarifying whether he wanted
> a review or just thoughts. I got the amazing reply - sent to three
> reviewers!
>   So, letting the SUNY process run its course. Once finished, Real
> Climate
> may be one avenue to lay out all the facts/details.
>
>   Away tomorrow. I think you have Monday off, so have a good long
> weekend!
>
> Cheers
> Phil
>
>> Subject: RE: review of E&E paper on alleged Wang fraud
>> Date: Wed, 29 Aug 2007 17:48:43 +0100
>> X-MS-Has-Attach:
>> X-MS-TNEF-Correlator:
>> Thread-Topic: review of E&E paper on alleged Wang fraud
>> thread-index: AcfqVG3NykjMc9doTBWIfTqkHPH+xwACAfp3
>> From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
>> To: "Phil Jones" <p.jones@uea.ac.uk>
>> X-OriginalArrivalTime: 29 Aug 2007 16:53:26.0748 (UTC)
>> FILETIME=[1E7969C0:01C7EA5D]
>> X-UEA-Spam-Score: 0.0
>> X-UEA-Spam-Level: /
>> X-UEA-Spam-Flag: NO
>>
>> Dear Phil
>>
>> The paper has been sent to three reviewers. Of course I will take
>> your comments and assessment into consideration. Indeed, if the
>> claims are unsubtantiated, I would certainly reject the paper.
>>
>> I hope this clarifies your query.
>>
>> With best regards
>> Benny
>>
>>
>>

>>
>> _____
>>
>> From: Phil Jones [mailto:p.jones@uea.ac.uk]
>> Sent: Wed 8/29/2007 16:51
>> To: Peiser, Benny
>> Subject: Re: review of E&E paper on alleged Wang fraud
>>
>>
>>
>>
>>   Benny,
>>   Energy and Environment is presumably a peer-review journal. Your
>>   email wasn't clear as to whether you want me to review the paper?
>> If you
>>   want me to, will you take any notice of what I might say - such as
>>   reject the paper? Or has the contribution already been reviewed?
>>
>>   Phil
>>
>>
>> At 15:18 29/08/2007, you wrote:
>> >Dear Dr Jones
>> >
>> >I have attached a copy of Doug Keenan's paper on the alleged Wang fraud
>> >that was submitted for the forthcoming issue of Energy & Environment
>> >http://www.ingentaconnect.com/content/mscp/ene.
>> >
>> >
>> >I was wondering whether you would be happy to comment on its content
>> and
>> >factual accuracy. Your comments and suggestions would be much
>> >appreciated. We would need your feedback by Sept 17.
>> >
>> >I look forward to hearing from you.
>> >
>> >Yours sincerely
>> >
>> >Benny Peiser
>> >Guest editor, E&E
>> >Liverpool John Moores University, UK
>> >
>> >
>>
>> 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
>> NR4 7TJ
>> UK
>> --
>>
>
> 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
> --

>

--
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FAX: (814) 865-3663
email: mann@psu.edu

http://www.met.psu.edu/dept/faculty/mann.htm

</x-flowed>

812. 1188508827.txt
##########

From: wang@climate.cestm.albany.edu
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: review of E&E paper on alleged Wang fraud
Date: Thu, 30 Aug 2007 17:20:27 -0400
Cc: Thomas.R.Karl@noaa.gov, 'Wei-Chyung Wang' <wang@climate.cestm.albany.edu>

Phil,

I think you need to respond by providing E&E with a simple answer of "false" to Keenan's write-up, based on the communication with me (but no mention of SUNYA confidentiality issue, it has to come directly from SUNYA). That will force E*E to contact either me directly or SUNYA. If the former, I can refer to SUNYA also, and let the university to handle it.

My reading is that, since the IPCC policy report is coming out soon (in October?), Keenan is in panic and wants to tint the Nature paper as much and as soon as possible, so he can not wait for SUNYA to conduct "inquery" (not investigation) which he knows he is not getting what he wants. Going to news medium will not do his trick because he can not really explain it. So in a way Keenan traps himself now, betting on that the "station history" was not available and that the stations have moved a lot (he does not know that at all). We are facing a tricky person and group, and the only way to do it is to follow the procedure to drive them crazy. E&E is not going to publish it without giving me the chance to respond, and that is when SUNYA comes in and that is what Keenan does not want to see as well, he wants to create a smocky screen before the truth comes out. We are not going to let Keenan doing things his way. So be easy, and respond directly what you learn from me (and any other scienctific issues you can identify) and perhaps even ask E&E to contact me/or SUNYA for verification.

I know you are under tremendous pressure, but Keenan is in panic and what he has done is going back to burn him, badly. We should be thinking, after the whole odeal is over, to take legal (or other) actions against Keenan. This is time I regre not been a rich person, otherwise I can throw a million dollar lawsuit against him.

Let me know what you want to do. I have also asked SUNYA's opinion

about what you should do within the SUNYA framework. But be careful
that you do not know much about SUNYA action.

WCW

----- Original Message -----
From: Phil Jones <p.jones@uea.ac.uk>
Date: Thursday, August 30, 2007 10:16 am
Subject: Re: Fwd: review of E&E paper on alleged Wang fraud

>
> Wei-Chyung,
>   Been thinking. A couple of thoughts:
>
> 1. Libel is quite easy to prove in the UK as you're not a public
> figure. Perhaps when you're back you ought to consider taking
> some legal
> advice from SUNY. Assuming the paper is published that is.
>
> 2. More important. I think I should send a short email to the editor
> Peiser and inform him that Keenan has broken his agreement with
> SUNY over this issue. If I don't, they could say I had the chance
> and didn't. Can you check with SUNY whether the folks there think
> I should? I just don't want to do anything that later could be
> construed as the wrong thing now. I could also point out some
> factual errors.
>
> Cheers
> Phil
>
>
> At 10:06 30/08/2007, wang@climate.cestm.albany.edu wrote:
> >the confidentiality means that keenan needs to keep the "inquery"
> >confidential during the process of sunya "inquery".
> >
> >WCW
> >----- Original Message -----
> >From: Phil Jones <p.jones@uea.ac.uk>
> >Date: Thursday, August 30, 2007 4:03 am
> >Subject: Re: Fwd: review of E&E paper on alleged Wang fraud
> >
> > >
> > > Wei-Chyung and Tom,
> > >   Thanks for the quick response. I won't do anything then
> until
> > > the SUNY process has run its course. Can you clarify what you
> > mean> > by violated confidentiality? I presume you mean that
> > Keenan agreed
> > > to do nothing on the issue until the SUNY process has run its
> > > course. I presume this will conclude sometime this autumn. Keep
> > > me informed of when the final decision might be, as after this
> > > we
> > > ought to do
> > > something about the paper in Energy and Environment. I checked
> > > with their guest editor and got this amazing reply! See below.
> > > So, if we didn't already think this was the worst journal in the
> > > world, now we know for certain it is, and have clear information
> > > from them
> > > to prove it.
> > >
> > >   When I mean doing something, I don't mean sending anything
> > to E&E,
> > > as that will be useless. The Real Climate blog site is a

> > > possibility, but
> > > there are other avenues.
> > >   I will make a few notes and send them to you to forward to
> SUNY.> > Only after doing this can I get onto something else!
> > >
> > >   I'm away tomorrow - back in on Monday.
> > >
> > > Cheers
> > > Phil
> > >
> > >
> > > From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
> > > To: "Phil Jones" <p.jones@uea.ac.uk>
> > > X-OriginalArrivalTime: 29 Aug 2007 16:53:26.0748 (UTC)
> > > FILETIME=[1E7969C0:01C7EA5D]
> > > X-UEA-Spam-Score: 0.0
> > > X-UEA-Spam-Level: /
> > > X-UEA-Spam-Flag: NO
> > >
> > > Dear Phil
> > >
> > > The paper has been sent to three reviewers. Of course I will take
> > > your comments and assessment into consideration. Indeed, if the
> > > claims are unsubtantiated, I would certainly reject the paper.
> > >
> > > I hope this clarifies your query.
> > >
> > > With best regards
> > > Benny
> > >
> > >
> > >
> > >
> > > _____
> > >
> > > From: Phil Jones [mailto:p.jones@uea.ac.uk]
> > > Sent: Wed 8/29/2007 16:51
> > > To: Peiser, Benny
> > > Subject: Re: review of E&E paper on alleged Wang fraud
> > >
> > >
> > >
> > >
> > >
> > >   Benny,
> > >   Energy and Environment is presumably a peer-review
> journal. Your
> > >   email wasn't clear as to whether you want me to review the
> > > paper? If you
> > >   want me to, will you take any notice of what I might say -
> such as
> > >   reject the paper? Or has the contribution already been
reviewed?
> > >
> > >   Phil
> > >
> > >
> > >
> > >
> > >
> > > At 23:17 29/08/2007, wang@climate.cestm.albany.edu wrote:
> > >
> > > >hi from beijing. thanks for the information, and i have
> > > forwarded the
> > > >file to the vp research and she wrote back to me that keenan has

> > > >violetted the confidentiality, as i have told her in the very
> > > >beginning. in any case, i am letting the university to
> handle this.
> > > >send me whatever you have and i will forward to sunya.
> keenan does
> > > >not follow on any rules at all, reasoning with him is
> useless, but
> > > >this will come back to badly hurt him.
> > > >
> > > >before i left for beijing, i wrote my offical responses (see
> > > >attached). please keep it to yourself. there is no doubt
> that zeng
> > > >had access and examined the station history to pick up the 42-
> pair> > >stations. also remember that, the statements made in
> both papers
> > > >address changes in all the relevant parameters "location,
> > > >instrumentation, observation time, etc." without specifically
> > > focus on
> > > >relocation.
> > > >
> > > >sunya is going through a very careful procedure, as i request
> > > them to
> > > >do because keenan will jump on any slip in procedure.
the "fraud"
> > > >charge, which will not stand any chance, is just his strategy of
> > > >getting attention on the station relocation effect. so
> better to
> > > >start thinking along that line.
> > > >
> > > >i am here attending the meeting of The 3rd Alexander von
Humboldt
> > > >International Conference on "the East Asian monsoon, past,
> > > present and
> > > >future" in Beijing. I am going to take some time off
> travelling in
> > > >southern China after the meeting, when my wife join me this
> weekend.> > >There is a good chance that I might not have e-mail
> access. Have a
> > > >good day.
> > > >
> > > >WCW
> > > >
> > > >
> > > >
> > > >-----Original Message -----
> > > >From: Phil Jones <p.jones@uea.ac.uk>
> > > >Date: Wednesday, August 29, 2007 10:46 am
> > > >Subject: Fwd: review of E&E paper on alleged Wang fraud
> > > >
> > > > > Wei-Chyung and Tom,
> > > > >
> > > > >   Just received this. I won't be responding.
> > > > >
> > > > > Knowing this journal there is no point, not even if I said
> > > > > I ought to review the paper. Peiser is a well-known skeptic
> > > > > in the UK. Not sure what to do. I guess you (WCW) should
> > > > > forward this to whoever needs to see it at Albany.
> > > > >
> > > > >   If you think I should respond then I can. I will
> forward this
> > > > > to someone here, but mainly for their file.
> > > > >
> > > > >   I did say the quote on p3 about 2-3 years ago. I am still

> > > > > not releasing the CRU station data collected over all the
> last> > > > 25 years.
> > > > >
> > > > > Cheers
> > > > > Phil
> > > > >
> > > > > >Subject: review of E&E paper on alleged Wang fraud
> > > > > >Date: Wed, 29 Aug 2007 15:18:04 +0100
> > > > > >X-MS-Has-Attach: yes
> > > > > >X-MS-TNEF-Correlator:
> > > > > >Thread-Topic: review of E&E paper on alleged Wang fraud
> > > > > >thread-index: AcfqPgYII3NKEW8US8uwftlkhnxNhgAB/4xQAAA5K8A=
> > > > > >From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
> > > > > >To: <p.jones@uea.ac.uk>
> > > > > >X-OriginalArrivalTime: 29 Aug 2007 14:18:06.0729 (UTC)
> > > > > >FILETIME=[6B4F5F90:01C7EA47]
> > > > > >X-UEA-Spam-Score: 0.0
> > > > > >X-UEA-Spam-Level: /
> > > > > >X-UEA-Spam-Flag: NO
> > > > > > >
> > > > > >Dear Dr Jones
> > > > > >
> > > > > >I have attached a copy of Doug Keenan's paper on the alleged
> >Wang
> > > > > fraud>that was submitted for the forthcoming issue of
> Energy &
> > > > > Environment>http://www.ingentaconnect.com/content/mscp/ene.
> > > > > >
> > > > > >
> > > > > >I was wondering whether you would be happy to comment on its
> > > > > content and
> > > > > >factual accuracy. Your comments and suggestions would be
much
> > > > > >appreciated. We would need your feedback by Sept 17.
> > > > > >
> > > > > >I look forward to hearing from you.
> > > > > >
> > > > > >Yours sincerely
> > > > > >
> > > > > >Benny Peiser
> > > > > >Guest editor, E&E
> > > > > >Liverpool John Moores University, UK
> > > > > >
> > > > > >
> > > > > >
> > > > > 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
> > > > > NR4 7TJ
> > > > > UK
> > > > > --
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> > > ----
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> > > > >
> > > >
> > > 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
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> 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
> NR4 7TJ
> UK
> ---
> ---
>

813. 1188557698.txt
##########

From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: review of E&E paper on alleged Wang fraud
Date: Fri, 31 Aug 2007 06:54:58 -0600

<x-flowed>
Phil,

Seems to me that Keenan has a valid point. The statements in the papers
that he quotes seem to be incorrect statements, and that someone (WCW
at the very least) must have known at the time that they were incorrect.

Whether or not this makes a difference is not the issue here.

Tom.

++++++++++++++++++++++++++

Phil Jones wrote:

> Tom,
> Just for interest! Keep quiet about both issues.
>
> In touch with Wei-Chyung Wang. Just agreed with him
> that I will send a brief response to Peiser. The allegation by Keenan
> has
> gone to SUNY. Keenan's about to be told by SUNY that submitting this has
> violated a confidentiality agreement he entered into with SUNY when he
> sent the complaint. WCW has nothing to worry about, but it still
> unsettling!
> All related to a paper in Nature from 1990! Keenan ought to look at the
> temperature data (which he has) rather than going on and on about
> site moves.
>
> See the end of this email and the response about E&E and the 3
> reviewers.
> Amazing! We all knew the journal was awful.

>
>   On something completely different - just agreed to review another
> crappy
> paper by Chappell/Agnew on Sahel Rainfall. Chappell is out of a job -
> and still
> he tries to write papers saying the Sahel drought might not have
> happened!
>
>   Both are just time wasters - but necessary to do unfortunately.
>
>   Weekend away with the family now - back Monday!
>
> Cheers
> Phil
>
>> Subject: review of E&E paper on alleged Wang fraud
>> Date: Wed, 29 Aug 2007 15:18:04 +0100
>> X-MS-Has-Attach: yes
>> X-MS-TNEF-Correlator:
>> Thread-Topic: review of E&E paper on alleged Wang fraud
>> thread-index: AcfqPgYII3NKEW8US8uwftlkhnxNhgAB/4xQAAA5K8A=
>> From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
>> To: <p.jones@uea.ac.uk>
>> X-OriginalArrivalTime: 29 Aug 2007 14:18:06.0729 (UTC)
>> FILETIME=[6B4F5F90:01C7EA47]
>> X-UEA-Spam-Score: 0.0
>> X-UEA-Spam-Level: /
>> X-UEA-Spam-Flag: NO
>>
>> Dear Dr Jones
>>
>> I have attached a copy of Doug Keenan's paper on the alleged Wang fraud
>> that was submitted for the forthcoming issue of Energy & Environment
>> http://www.ingentaconnect.com/content/mscp/ene.
>>
>>
>> I was wondering whether you would be happy to comment on its content and
>> factual accuracy. Your comments and suggestions would be much
>> appreciated. We would need your feedback by Sept 17.
>>
>> I look forward to hearing from you.
>>
>> Yours sincerely
>>
>> Benny Peiser
>> Guest editor, E&E
>> Liverpool John Moores University, UK
>
> Dear Phil
>
> The paper has been sent to three reviewers. Of course I will take your
> comments and assessment into consideration. Indeed, if the claims are
> unsubtantiated, I would certainly reject the paper.
>
> I hope this clarifies your query.
>
> With best regards
> Benny
>
>
>
>
> _____

> From: Phil Jones [mailto:p.jones@uea.ac.uk]
> Sent: Wed 8/29/2007 16:51
> To: Peiser, Benny
> Subject: Re: review of E&E paper on alleged Wang fraud
>
>
>
>
>   Benny,
>   Energy and Environment is presumably a peer-review journal. Your
>   email wasn't clear as to whether you want me to review the paper? If
> you
>   want me to, will you take any notice of what I might say - such as
>   reject the paper? Or has the contribution already been reviewed?
>
>   Phil
>
>
>
>
>
> 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
> NR4 7TJ
> UK
> --

</x-flowed>

814. 1189515774.txt
##
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: paper on alleged Wang fraud
Date: Tue, 11 Sep 2007 09:02:54 -0400
Reply-to: mann@psu.edu
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>
Phil,

sorry, first version of my message was a bit garbled. Here is the full
message:

thanks for forwarding. It may be difficult for me to sue them over a
footnote, and in fact he is very careful only to intimate accusations
against me in a response to your comments. Note that he does not do so
in the paper. I'm sure they know that I would sue them for that, and
that I have a top lawyer already representing me.

Wei Chyung needs to sue them, or at the least threaten a lawsuit. If he
doesn't, this will set a dangerous new precedent. I could put him in

touch w/ anleading attorney who would do this pro bono. Of course, this has to be done quickly. The threat of a lawsuit alone my prevent them from publishing this paper, so time is of the essence. Please feel free to mention this directly to Wei Chyung, in particular that I think he needs to pursue a legal course her independent of whatever his university is doing. He cannot wait for Stony Brook to complete its internal investigations! If he does so, it will be too late to stop this.

Gavin is in Shanghai, but perhaps may be able to provide some brief thoughts himself on this,

mike

Michael E. Mann wrote:
> Phil,
>
> thanks for forwarding. It may be difficult for me to sue them over a
> footnote, and in fact he is very careful only to intimate accusations
> against me in a response to your comments. Note that he does not do so
> in the paper. I'm sure they know that I would sue them for that, and
> that I have a top lawyer already representing me.
>
> Wei Chyung needs to sue them, or at the least threaten a lawsuit. If
> he doesn't, this will set a dangerous new precedent. I could put him
> in touch w/ anleading attorney who would do this pro bono. Of course,
> this has to be done quickly. The threat of a lawsuit alone my prevent
> them from publishing this paper, so time is of the essence. Please
> feel free to mention this directly to Wei Chyung, in particular that I
> think he needs to pursue a legal course here here independent of
> whatever his university is doing. He wait for Stony Brook to complete
> its internal investigations!
>
> Gavin is in Shanghai, but hopefully
>
> Phil Jones wrote:
>> Mike, Gavin,
>>   Don't pass on, just for interest. It seems as though E&E will likely
>> publish this paper. I've responded briefly, pointing out that Tao et al
>> (1991) doesn't claim that it explicitly states...
>>   The response to my point 7 sums up Keenan. It also seems
>> as though he will run with the footnote 3, but it's only a footnote!
>> The fraud allegation against you Mike is only in passing!
>>
>>   Wei-Chyung is in Vienna. Have forwarded this to him to pass onto
>> SUNY.
>> I wish they would conclude their assessment of malpractice.
>>
>>   Cheers
>> Phil
>>
>> PS to Gavin - been following (sporadically) the CA stuff about the
>> GISS data and
>> release of the code etc by Jim. May take some of the pressure of you
>> soon, by releasing a list of the stations we use - just a list, no code
>> and no data. Have agreed to under the FOIA here in the UK.
>>
>> Oh Happy days!
>>
>>> Subject: paper on alleged Wang fraud
>>> Date: Mon, 10 Sep 2007 18:39:02 +0100
>>> X-MS-Has-Attach: yes
>>> X-MS-TNEF-Correlator:
>>> Thread-Topic: paper on alleged Wang fraud

>>> thread-index: AcfzsbCIlqEe9LxLSeGz6CASlEIWmgAHs4oa
>>> From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
>>> To: <p.jones@uea.ac.uk>
>>> X-OriginalArrivalTime: 10 Sep 2007 17:39:03.0905 (UTC)
>>> FILETIME=[7AE76D10:01C7F3D1]
>>> X-UEA-Spam-Score: 0.0
>>> X-UEA-Spam-Level: /
>>> X-UEA-Spam-Flag: NO
>>>
>>> Phil
>>>
>>> I have attached Doug's response to your comments. As far as I can
>>> see, his basic accusation seems unaffected by your criticism. Unless
>>> there is any compelling evidence that Keenan's main claim is
>>> unjustified or unsubstantiated, I intend to publish his paper in the
>>> forthcoming issue of E&E.
>>>
>>> Please let me know by the end of the week if you have any additional
>>> arguments that may sway me in my decision.
>>>
>>> With best regards
>>> Benny
>>>
>>>
>>>
>>>
>>
>> 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
>> NR4 7TJ
>> UK
>> --

>
>
>
>

--
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>

815. 1189536059.txt
##
##########

From: "Burgess Jacquelin Prof \(ENV\)" <Jacquie.Burgess@uea.ac.uk>

To: "Jones Philip Prof \(ENV\)" <P.Jones@uea.ac.uk>
Subject: RE: Possible problem looming
Date: Tue, 11 Sep 2007 14:40:59 +0100

Thanks Phil,
I will keep your email and hope we don't have to mobilise. This is very close to harassment, isn't it.
Jacquie

-----Original Message-----
From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: 11 September 2007 14:06
To: Burgess Jacquelin Prof (ENV)
Cc: Mcgarvie Michael Mr (ACAD)
Subject: Possible problem looming

Jacquie,
I've been in discussion with Michael over the past several months about a number of Freedom of Information (FOI) requests for CRU data. I've responded to one and will be responding to another in the next few days. Michael suggested I bring you up to speed on the issue. To cut a very long story short, I'm attaching 3 things that relate to what's happened since responding to the first request.

1. A paper from 1990 by me and others in Nature. The request was for the station data from the rural station networks in the three regions studied.

This led to a person in London (Douglas Keenan) putting some material on his website claiming fraud against one of the co-authors on the paper (Wei-Chyung Wang of the State University of Albany, SUNY, in NY, USA). He then put an allegation of fraud into SUNY against Wang. SUNY are dealing with this - not quickly, but I have seen Wang's response.

2. Keenan then submitted a paper (attached) to the world's worst journal, Energy and Environment. According to Wang this is in breach of an agreement with SUNY not to do anything whilst the allegation is being dealt with.
According to Wang, SUNY have told Keenan this.

I was sent the paper to comment on the factual allegations in the paper. After discussing this with Wang (who informed SUNY) I sent 9 comments.

3. My comments - with Keenan's responses embedded within (this is the new bit for you Michael).
I have subsequently told the E&E guest editor that Keenan's response to my point # 5 is wrong. I sent him Tao et al. (1991) so he can see this. Keenan's response to my point 7 illustrates his arrogance.

I have loads more background to all this, and it has taken some time over the last few weeks and months in responding.

You are now partly up to speed on the issue. I'm away next week.
I don't know when E&E might publish, nor when the SUNY review process (which is being dealt with by their Director of Research) will conclude. Wang and I both know that the allegations are groundless, but it is likely it will not look good when it first comes out. This is just another of the attempts by climate skeptics to get the public and the media thinking that there is disagreement amongst scientists and that we shouldn't be doing anything about global warming. I will be discussing this with some IPCC people when I meet them in early October.

Cheers
Phil

Phil,

Thanks for forwarding this. I am shocked about this - if a formal review is underway at the University of Albany it is surely improper to publish a paper in a journal about the matter!

I suggest that you alert Jacquie Burgess to this, as the new Head of School.

I would like to suggest that we ask Dave Palmer to comment on the events on the FOIA request - I don't think I fully agree with the story presented here. Do you agree?

I also think we should alert the Press Office in due course.

Regards

Michael

Michael McGarvie
Senior Faculty Manager
Faculty of Science
Room 0.22C
University of East Anglia
Norwich NR4 7TJ
tel: 01603 593229
fax: 01603 593045
m.mcgarvie@uea.ac.uk

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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
---

816. 1189722851.txt
##########

From: "Wahl, Eugene R" <wahle@alfred.edu>
To: "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: RE: Wahl & Ammann AND Ammann & Wahl papers
Date: Thu, 13 Sep 2007 18:34:11 -0400
Cc: "Eystein Jansen" <eystein.jansen@geo.uib.no>, <t.osborn@uea.ac.uk>, "Keith Briffa" <k.briffa@uea.ac.uk>

Hello Peck, Eystein, Tim, Keith:

Please find attached the e-versions of the WA and AW papers re: the "hockey-stick". These are now available as "to-come-in-print" articles from Climatic Change. I believe the WA one was just loaded yesterday. As I understand it, official "print" publication will be this November. These versions HAVE gone through the author proof process, and thus I anticipate no possibility of them being further changed before print publication.

Note brief correspondence yesterday with Phil Jones re: proof-level changes that were made to WA (copied below).

Peace, Gene

Dr. Eugene R. Wahl
Assistant Professor of Environmental Studies
Division of Environmental Studies and Geology
Alfred University

One Saxon Drive
Alfred, NY 14802
607.871.2604

****
****
From: Wahl, Eugene R
Sent: Wednesday, September 12, 2007 6:44 PM
To: 'Phil Jones'; Caspar Ammann
Subject: RE: Wahl/Ammann

Hi Phil:

There were inevitably a few things that needed to be changed in the final version of the WA paper, such as the reference to the GRL paper that was not published (replaced by the AW paper here), two or three additional pointers to the AW paper, changed references of a Mann/Rutherford/Wahl/Ammann paper from 2005 to 2007, and a some other very minor grammatical/structural things. I tried to keep all of this to the barest minimum possible, while still providing a good reference structure. I imagine that MM will make the biggest issue about the very existence of the AW paper, and then the referencing of it in WA; but that was simply something we could not do without, and indeed AW does a good job of contextualizing the whole matter.

Steve Schneider seemed well satisfied with the entire matter, including its intellectual defensibility (sp?) and I think his confidence is warranted. That said, any other thoughts/musings you have are quite welcome.

Peace, Gene

-----Original Message-----
From: Phil Jones [mailto:p.jones@uea.ac.uk]

Sent: Wednesday, September 12, 2007 11:30 AM
To: Wahl, Eugene R; Caspar Ammann
Subject: Wahl/Ammann

Gene/Caspar,
Good to see these two out. Wahl/Ammann doesn't appear to be in CC's online first, but comes up if you search.
You likely know that McIntyre will check this one to make sure it hasn't changed since the IPCC close-off date July 2006!
Hard copies of the WG1 report from CUP have arrived here today.

Ammann/Wahl - try and change the Received date! Don't give those skeptics something to amuse themselves with.

Cheers
Phil

Attachment Converted: "c:\eudora\attach\Ammann_ClimChange2007.pdf"

Attachment Converted: "c:\eudora\attach\Wahl_ClimChange2007.pdf"

817. 1189797973.txt
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: recent WSJ article
Date: Fri, 14 Sep 2007 15:26:13 -0400
Reply-to: mann@psu.edu
Cc: Phil Jones <p.jones@uea.ac.uk>, Richard Somerville <richard.somerville@wanadoo.fr>

Kevin,
can you send me the link once its up?
thanks,
Mike
Kevin Trenberth wrote:

Mike
You should have seen the first version. I drafted that yesterday and then today toned it down. I did add a couple of points, including the link you suggested. Will try to send off later today but just to nature.com
Thanks
Kevin
Michael E. Mann wrote:

guys, I've got a few minutes before I have to head out again.
Kevin--thanks for helping return the Nature blog to respectability after a dubious start...I'd like to direct RealClimate readers to your piece as soon as it is up, so please let me know when that happens...
Looks like Phil has hit several of the key points, but here are a few more:
1. The 'discrediting' that Akasofu cites has been discredited. IPCC Chapter 6

rejected the
McIntyre and McKitrick's claims in no uncertain terms, referencing the Wahl and Ammann work
(reprints attached) who show that (a) the reconstruction is readily reproducible and (b)
McIntyre and McKitrick only failed to reproduce the reconstruction because of multiple
errors on their part. This is true in addition to the more general point that Kevin has
made (that multiple independent studies confirm and in fact now extend the previous
conclusions, rather than contradict them).
2. To the extent that the "LIA" and "MWP" can be meaningfully defined, there has been much
work (published in Nature, Science, etc.) showing that the main variations (both in terms
of hemispheric mean changes and spatial patterns) can indeed be explained in terms of the
response of the climate system to natural radiative forcing changes (solar and volcanism).
Only someone completely unfamiliar with the advances of the past ten years in climate
science would claim that there are no explanations for these.
3. Continuing in this theme, to claim that the modern warming is some sort of 'rebound'
reflects a thorough apparent lack of understanding of how the climate system works. The
climate doesn't rebound. It responds (with some lag) to changes in radiative forcing. The
main patterns of variation of past centuries have been explained in terms of such responses
to natural radiative forcing changes. As shown in countless studies, the late 20th century
warming can only be explained in terms of the response to anthropogenic changes in
radiative forcing. Kevin has more or less already made this point, in different words, in
the current draft.
4. The bogus talking point that co2 lagging the warming in the ice cores has been debunked
countless times before, and its an embarassment that it continues to be raised by one who
ostensibly considers himself a scientist. This is total nonsense, and a nice refutation has
been provided by Eric Steig on RealClimate here:

[1]http://www.realclimate.org/index.php/archives/2007/04/the-lag-between-temp-and-co2/
Perhaps worth just linking to that explanation?
Kevin, perhaps you're too gentle in attributing this simply to some 'confusion' about the
facts. Either Mr. Akasofu has literally no familiarity whatsoever with the advances in
climate science of the past two decades, or he has intentionally sought to deceive. In
either case, his piece is embarassment.
Finally, let me withdraw my initial suggestion. For strategic reasons, it might make sense
to submit this as letter to editor to WSJ (easy and quick to do online), and then publish
it on the Nature blog in short order. I sea that as win-win because you can either call
the WSJ for refusing to run your letter (which is very likely what will happen),

or use
the Nature blog piece to draw attention to your letter, should WSJ actually choose to
publish your letter...
please don't hesitate to let me know if I can be of any further help here. Will be back
online a bit later today,
mike
Phil Jones wrote:

> Kevin,
>
> A few quick thoughts. Article is awful as we all know.
>
> It is important to learn about past climate change, especially over the past 1000
> years, but it is even important to use new and improved evidence from proxy
> sources (i.e. not to cling to outdated concepts of the past such as the MWP
> and LIA). How can we ever hope to progress if we have conform to incorrect
> concepts?
> On the early mid-20th century warming - look at the figures in Ch 9.
> The decrease from 1940-75 didn't happen if you look at global records.
> MBH was published in 1998 and wasn't just a tree-ring study.
> The Thames doesn't and never did freeze solid. It did so 25 times
> between 1400 and 1820. Only about 5-6 of these were frost fairs. Most
> of these have CET data, so what is the use of the freeze dates!
> He plucks various figures out of the air!
> I think the reductions in Arctic sea ice this summer/September are
> alarming. They are 20% below the 2005 record. He comes from
> Alaska. Has he not seen the effects on the coast there?
> Cheers
> Phil
>
> 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 [2]p.jones@uea.ac.uk
> NR4 7TJ
> UK
> ---

--
Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

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[4]http://www.met.psu.edu/dept/faculty/mann.htm

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****
Kevin E. Trenberth e-mail: [5]trenbert@ucar.edu
Climate Analysis Section, NCAR [6]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 80305

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[8]http://www.met.psu.edu/dept/faculty/mann.htm

References

1. http://www.realclimate.org/index.php/archives/2007/04/the-lag-between-temp-and-co2/
2. mailto:p.jones@uea.ac.uk
3. mailto:mann@psu.edu
4. http://www.met.psu.edu/dept/faculty/mann.htm
5. mailto:trenbert@ucar.edu
6. http://www.cgd.ucar.edu/cas/
7. mailto:mann@psu.edu
8. http://www.met.psu.edu/dept/faculty/mann.htm

818. 1191550129.txt
##########

From: Peter Thorne <peterwthorne@btinternet.com>
To: "Smith, Fiona" <fiona.smith@metoffice.gov.uk>
Subject: Re: URGENT: Press office ...
Date: Thu, 4 Oct 2007 22:08:49 +0000 (GMT)
Cc: p.jones@uea.ac.uk

Thanks Fiona, I am cc'ing in Phil who will let relevant people at UEA know. Please can you
get press office to advise if I will have to be in during next week or whether solely being
on my mobile will suffice. I am flexible on the TOIL next week Tuesday onwards (land Monday
at 06.00) but would like to know by the time I leave if poss. Just to remind that my mobile
is 07834034418.

Cheers

Peter
----- Original Message ----
From: "Smith, Fiona" <fiona.smith@metoffice.gov.uk>
To: Peter Thorne <peterwthorne@btinternet.com>
Cc: "Gromett, Barry" <barry.gromett@metoffice.gov.uk>
Sent: Friday, 5 October, 2007 1:40:04 AM
Subject: RE: URGENT: Press office ...

Peter,

Sorry for the delay. The head of the press office was off sick for a few days and they have
been incredibly busy.

Yes, the Press Office will go ahead with a press release and we will contact UEA

to make
sure we have a consistent message.

Will let you see any relevant communication.

Fiona

Fiona Smith
Met Office Hadley Centre for Climate Change
FitzRoy Road Exeter EX1 3PB United Kingdom
Tel: +44 (0) 1392 884240
E-mail: fiona.smith@metoffice.gov.uk [1]http://www.metoffice.gov.uk

---

From: Peter Thorne [mailto:peterwthorne@btinternet.com]
Sent: Thursday, October 04, 2007 9:26 AM
To: Smith, Fiona
Subject: URGENT: Press office ...
intentional silence? I need a decision ASAP to plan next week and let Phil Jones and UEA
know. Please request resolution on whether we will run something or not so wheels can be
set rolling if necessary.

Thanks

Peter

References

1. http://www.metoffice.gov.uk/

819. 1196795844.txt
##########

From: carl mears <mears@remss.com>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Tue, 04 Dec 2007 14:17:24 -0800
Cc: Phil Jones <p.jones@uea.ac.uk>,santer1@llnl.gov, Tom Wigley <wigley@cgd.ucar.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>,Karl Taylor <taylor13@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>,Frank Wentz <frank.wentz@remss.com>

<x-flowed>
But you are assuming that there is no noise (instrumental or "weather") in
the observations.

-Carl
At 01:57 PM 12/4/2007, Tom Wigley wrote:
>All,
>
>Depends on whether the runs are independent. Are models independent?
>

>A billion runs would indeed reduce the statistical uncertainty to near
>zero. What is left (if one compared with absolutely correct observed data)
>is the mean model bias.
>
>Tom.
>
>++++++++++++++++++
>
>carl mears wrote:
>
>>Hi Ben, Phil and others
>>
>>To me, the fundamental error is 2.3.1. Expecting the observed values to
>>lie within
>>+/- 2*sigma(SE) (i.e. sigma/(sqrt(N-1)) of the distribution of N model
>>trends) is just
>>wrong.
>>If this were correct, we could just run the models a lot of times, say a
>>billion or so, and have a
>>very, very, very small sigma(SE) (assuming the sigma didn't grow
>>much) and we'd never
>>have "agreement" with anything. Absurd.
>>
>>Does IJC publish comments?
>>
>>-Carl
>>
>>At 02:09 AM 12/4/2007, Phil Jones wrote:
>>
>>> Ben,
>>>   It sure does! Have read briefly - the surface arguments are wrong.
>>> I know editors have difficulty finding reviewers, but letting this one
>>> pass is awful - and IJC was improving.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>>At 17:53 30/11/2007, Ben Santer wrote:
>>>
>>>>Dear folks,
>>>>
>>>>I'm forwarding this to you in confidence. We all knew that some
>>>>journal, somewhere, would eventually publish this stuff. Turns out that
>>>>it was the International Journal of Climatology. Strengthens the need
>>>>for some form of update of the Santer et al. (2005) Science paper.
>>>>
>>>>With best regards,
>>>>
>>>>Ben
>>>>--
>>>>Benjamin D. Santer
>>>>Program for Climate Model Diagnosis and Intercomparison
>>>>Lawrence Livermore National Laboratory
>>>>P.O. Box 808, Mail Stop L-103
>>>>Livermore, CA 94550, U.S.A.
>>>>Tel: (925) 422-2486
>>>>FAX: (925) 422-7675
>>>>email: santer1@llnl.gov
>>>>--
>>>>
>>>>
>>>>

>>>>X-Account-Key: account1
>>>>Return-Path: <anrevk@nytimes.com>
>>>>Received: from mail-2.llnl.gov ([unix socket])
>>>>   by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
>>>>   Fri, 30 Nov 2007 08:39:49 -0800
>>>>Received: from smtp.llnl.gov (nspiron-3.llnl.gov [128.115.41.83])
>>>>   by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 $) with
>>>> ESMTP id lAUGdl5E004790
>>>>   for <santer1@mail.llnl.gov>; Fri, 30 Nov 2007 08:39:48 -0800
>>>>X-Attachments: DCPS-proofs_IJC07.pdf
>>>>X-IronPort-AV: E=McAfee;i="5100,188,5173"; a="21323766"
>>>>X-IronPort-AV: E=Sophos;i="4.23,235,1194249600";
>>>>   d="pdf'?scan'208,217";a="21323766"
>>>>Received: from nsziron-1.llnl.gov ([128.115.249.81])
>>>>   by smtp.llnl.gov with ESMTP; 30 Nov 2007 08:39:47 -0800
>>>>X-Attachments: DCPS-proofs_IJC07.pdf
>>>>X-IronPort-AV: E=McAfee;i="5100,188,5173"; a="6674079"
>>>>X-IronPort-AV: E=Sophos;i="4.23,235,1194249600";
>>>>   d="pdf'?scan'208,217";a="6674079"
>>>>Received: from smtp-nv-vip1.nytimes.com (HELO nytimes.com)
>>>>([199.181.175.116])
>>>>   by nsziron-1.llnl.gov with ESMTP; 30 Nov 2007 08:39:43 -0800
>>>>Message-Id: <6.2.5.6.2.20071130111858.03540590@nytimes.com>
>>>>X-Mailer: QUALCOMM Windows Eudora Version 6.2.5.6
>>>>Date: Fri, 30 Nov 2007 11:38:52 -0500
>>>>To: santer1@llnl.gov, broccoli@envsci.rutgers.edu, mears@remss.com
>>>>From: Andrew Revkin <anrevk@nytimes.com>
>>>>Subject: sorry to take your time up, but really do need a scrub of this
>>>>   singer/christy/etc effort
>>>>Mime-Version: 1.0
>>>>Content-Type: multipart/mixed;
>>>>   boundary="====================_67524015==_"
>>>>X-NYTOriginatingHost: [10.149.144.50]
>>>>
>>>>hi,
>>>>for moment please do not distribute or discuss.
>>>>trying to get a sense of whether singer / christy can get any traction
>>>>with this at all.
>>>>
>>>>
>>>>
>>>>*_ ANDREW C. REVKIN
>>>><http://www.nytimes.com/revkin>_*The New York Times / Environment / Dot
>>>>Earth <http://dotearth.blogs.nytimes.com/>Blog
>>>><http://dotearth.blogs.nytimes.com/>620 Eighth Ave., NY, NY 10018-1405
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>>>
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820. 1196872660.txt
##########

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: Re: Even more on Loehle's 2000 year climate analysis]
Date: Wed, 05 Dec 2007 11:37:40 -0500
Reply-to: mann@psu.edu

well put Phil,
I think you've put your finger right on it. JGR-Atmospheres has been publishing some truly
awful papers lately; we responded (Gavin, me, James Annan) to the awful Schwartz
sensitivity estimate paper, but there are so many other bad papers that are appearing there
(Chylak, etc.) that its just impossible to respond to them all.
I hadn't seen this latest one though. McKitrick and Michaels team up again, wow! maybe
McKitrick has figured ou the difference between radians and degrees this time!
talk to you later,
mike
Phil Jones wrote:

> Mike,
> Also I see him writing things - then people saying you should
> write this up for a paper, as though it can be knocked up in an
> afternoon. He realises he can't do this - as it takes much longer.
> Then we wastes more and more time opening up new threads.
> He doesn't seem clever enough to realise this.
> Gavin and Rasmus have seen the attached piece of garbage!
> UAH is correct, therefore the land surface must be wrong.
> Let's adjust it for a dodgy reason - ah, it now agrees with UAH.
> Let's forget that the land now disagrees with the ocean surface.
> If only I'd thought of that first, I could have not bothered with
> the awful analysis. If only I'd just believed RSS in the first place.
> Cheers
> Phil
> At 15:16 05/12/2007, you wrote:
>
> HI Phil,
> thanks--thats good.
> Re, Loehle, McIntyre. Funny--w/ each awful paper E&E publishes, McIntyre realizes that
> it compromises the integrity of his own "work" even further. He can't distance himself

from E&E much as he'd like to. He also seems to be losing lots of credibility now w/
all but his most loyal followers, which is good to see...
mike
Phil Jones wrote:

Mike,
Yes the 1990 graphic is in an Appendix. The last few are being regularly hassled
by Thorsten. The guy from EPRI (Larry) really wants something submitted soon.
So working here to get something in by end of Jan. Keith is going to get
it fast-tracked through the Holocene - well that's the plan.
The Loehle paper is awful as you know. So is another article on the IPCC process
in E&E. I did look at Climate Audit a week or two back - I got the impression
that McIntyre is trying to distance himself from some of these E&E articles by
saying we have to be equally skeptical about them as well.
Cheers
Phil

At 14:00 04/12/2007, you wrote:

Hey Phil,
thanks--nice coincidence in timing. So the 1990 graphic will be discussed in this review
paper, right? Perfect, I'll let Gavin know.
Will look into the AGU fellowship situation ASAP.
I don't read E&E, gives me indigestion--I don't even consider it peer-reviewed science,
and in my view we should treat it that way. i.e., don't cite, and if journalists ask us
about a paper, simply explain its not peer-reviewed science, and Sonja B-C, the editor,
has even admitted to an anti-Kyoto agenda!
I do hope that Wei-Chyung pursues legal action here.
So didn't see this recent paper, nor have I heard about the IJC paper, Christy and
Spencer continue to lose more and more scientific credibility with each awful paper they
publish.
Gavin is planning to do something on the Loehle paper on RealClimate, I'm staying away
from it. I have a revised set of hemispheric reconstructions which I'll send you soon,
its basically what I showed at AGU last year. Submitted to PNAS--more soon on that,
mike
Phil Jones wrote:

Mike,
Some text came last night from Caspar. Keith/Tim writing their parts still.
I have text from Francis, so almost all here now. Still need to find some time
- maybe the Christmas/New Year break here - to put it all together. There
is so much else going on here at the moment with other papers, it will
be hard to find some time. I wish they had all responded much sooner!
As for AGU - just getting one of their Fellowships would be fine.
I take it you've seen the attached in E&E. I've not heard any more from
Wei-Chyung in the past couple of months. I'm working on a paper
on urbanization. I can show China is hardly affected. Will send for you
to look over when I have it in a form that is sendable. Would appreciate
your thoughts on how I will have said things.
Have another awful pdf of a paper accepted in IJC !! It ws rejected
by all three reviewers for GRL! It is by Douglass, Christy , Singer et al

- thus you'll know what it is on.
Have booked flights for Tahiti in April, just need to do the hotel now.
Cheers
Phil
Cheers
Phil
At 02:07 04/12/2007, you wrote:

Hi Phil,
I hope things are going well these days, and that the recent round of attacks have died
down. seems like some time since I've heard from you.
Please see below: Gavin was wondering if there is any update in status on this?
By the way, still looking into nominating you for an AGU award, I've been told that the
Ewing medal wouldn't be the right one. Let me know if you have any particular options
you'd like me to investigate...
thanks,
mike
-------- Original Message --------
Subject: Re: Even more on Loehle's 2000 year climate analysis
Date: 03 Dec 2007 20:59:58 -0500
From: Gavin Schmidt [1]<gschmidt@giss.nasa.gov>
To: Michael E. Mann [2]<mann@psu.edu>
References: [3]<3.0.3.32.20071203130209.0123fd18@mail.skybest.com>
[4]<3.0.3.32.20071202224717.012384a8@mail.skybest.com>
[5]<3.0.3.32.20071201123550.01237954@mail.skybest.com>
[6]<3.0.3.32.20071201123550.01237954@mail.skybest.com>
[7]<3.0.3.32.20071202224717.012384a8@mail.skybest.com>
[8]<3.0.3.32.20071203130209.0123fd18@mail.skybest.com>
[9]<3.0.3.32.20071203141259.0126c33c@mail.skybest.com>
[10]<475457F3.9070102@meteo.psu.edu>
this reminds me. What's the status of Phil Jones and Caspar's investigation of the IPCC90 curve? Phil wanted us to hold off for some reason, but is that done with?

That's a great story that needs to be told.

Gavin

On Mon, 2007-12-03 at 14:24, Michael E. Mann wrote:
> thanks Eric,
>
> That's great. I've again copied in Gavin so that he has this info too.
>
> Will keep you in the loop!
>
> mike
>
> Eric Swanson wrote:
> > Hi Mike,
> >
> > I do hope you all are able to put this all together.
> > There were several comments on CA about RealClimate, suggesting that
> > RC wouldn't say anything, as E&E publication has such a bad rap.
> >
> > Perhaps my biggest complaint was also one mentioned by another

> > poster
> > on CA. I don't like using a simple linear interpolation between
> > data points for these series where there are many years
between
> > samples.
> > Here's the other fellow's comments:
> >
> >

[11]
http://www.climateaudit.org/?p=2380#comment-162478
> >

[12]
http://www.climateaudit.org/?p=2380#comment-162654
> >

[13]
http://www.climateaudit.org/?p=2380#comment-162665
> >
> > I would go further than that. These data sets represent
samples
of
> > time records. The sampling does not produce a value for a
single
> > year.
> > Rather, each sample represents some number of years of the
variable
> > as averaged in the process of collecting the material to be
> > analyzed.
> >
> > Consider an ocean sediment core, such as Keigwin's data. The
> > subcores
> > are sampled every 1.0 cm. Assume the material is taken with a
device
> > that
> > collects mud from a 0.4 cm area along the core. Thus, the
sample
> > would
> > contain 4/10 of the material deposited at that 1 cm per sample
rate
> > of
> > change in time. If the age/depth model at that point yields a
100
> > year
> > per cm rate, then the sample would represent an average over
40
> > years.
> > Simple linear interpolation assumes a continuously varying
change
> > between
> > the points, while the sampling process would give a brief 40
year
> > value
> > with the other 60 years being unknown. What if the entire cm
of
the
> > core

> > were analyzed? One would not know unless one had contacted
each
> > research
> > group that did the analysis and requested more information
than
that
> > which
> > might be found in the published reports.
> >
> > NOTE: I looked at Keigwin's data when I wrote a comment on
Loehle's
> > 2004 paper
> >
> > Comments on "Climate change: detection and attribution of
trends
> > from long-term
> > geologic data" by C. Loehle [Ecological Modelling 171 (4)
(2004)
> > 433-450],
> > Ecological Modelling 192 (2006) 314-316
> >
> > You may add my name to the list for what it's worth.
> >
> > Best Regards,
> >
> > Eric Swanson
> > ----------
> > At 01:18 PM 12/3/07 -0500, you wrote:
> > >>>>
> >   Eric--this is
great, thanks for all of the info. I've taken
> >   the liberty of
forwarding to Gavin, as we're thinking of
> >   doing an RC
post on this, and this would be very useful. We
> >   should
certainly list you as a "co-author" on this, if thats
> >   ok w/ you?
> >
> >   Looking
forward
to hearing what else you find here!
> >
> >   mike
> >
> >
>
>
>
> --
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> Director, Earth System Science Center (ESSC)
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[26]http://www.met.psu.edu/dept/faculty/mann.htm

References

Visible links
1. mailto:gschmidt@giss.nasa.gov
2. mailto:mann@psu.edu
3. mailto:3.0.3.32.20071203130209.0123fd18@mail.skybest.com
4. mailto:3.0.3.32.20071202224717.012384a8@mail.skybest.com
5. mailto:3.0.3.32.20071201123550.01237954@mail.skybest.com

6. mailto:3.0.3.32.20071201123550.01237954@mail.skybest.com
7. mailto:3.0.3.32.20071202224717.012384a8@mail.skybest.com
8. mailto:3.0.3.32.20071203130209.0123fd18@mail.skybest.com
9. mailto:3.0.3.32.20071203141259.0126c33c@mail.skybest.com
10. mailto:475457F3.9070102@meteo.psu.edu
11. http://www.climateaudit.org/?p=2380#comment-162478
12. http://www.climateaudit.org/?p=2380#comment-162654
13. http://www.climateaudit.org/?p=2380#comment-162665
14. mailto:mann@psu.edu
15. http://www.met.psu.edu/dept/faculty/mann.htm
16. mailto:mann@psu.edu
17. http://www.met.psu.edu/dept/faculty/mann.htm
18. mailto:p.jones@uea.ac.uk
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21. mailto:p.jones@uea.ac.uk
22. mailto:mann@psu.edu
23. http://www.met.psu.edu/dept/faculty/mann.htm
24. mailto:p.jones@uea.ac.uk
25. mailto:mann@psu.edu
26. http://www.met.psu.edu/dept/faculty/mann.htm

Hidden links:
27. http://www.met.psu.edu/dept/faculty/mann.htm

821. 1196877845.txt
##########

From: Ben Santer <santer1@llnl.gov>
To: Peter Thorne <peter.thorne@metoffice.gov.uk>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Wed, 05 Dec 2007 13:04:05 -0800
Reply-to: santer1@llnl.gov
Cc: Carl Mears <mears@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>, Steve Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, Dian Seidel <dian.seidel@noaa.gov>, Melissa Free <melissa.free@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve Klein <klein21@mail.llnl.gov>

<x-flowed>
Dear folks,

Thank you very much for all of your emails, and my apologies for the delay in replying - I've been on travel for much of the past week.

Peter, I think you've done a nice job in capturing some of my concerns about the Douglass et al. paper. Our CCSP Report helped to illustrate that there were large structural uncertainties in both the radiosonde- and MSU-based estimates of tropospheric temperature change. The scientific evidence available at the time we were finalizing the CCSP Report - from Sherwood et al. (2005) and the (then-unpublished) Randel and Wu paper - strongly suggested that a residual cooling bias existed in the sonde-based estimates of tropospheric temperature change. As you may recall, we showed results from both the RATPAC and HadAT2 radiosonde datasets in the CCSP Report and the Santer et al. (2005) Science paper. From the latter (see, e.g., our Figure 3B and Figures 4C,D), it was clear that there were physically-significant differences between the simulated temperature trends in the tropical lower

troposphere (over 1979 to 1999) and the trends estimated from RATPAC, HadAT2, and UAH data. In both the Science paper and the CCSP Report, we judged that residual biases in the observations provided the most likely explanation for these model-versus-data trend discrepancies.

Douglass et al. come to a fundamentally different conclusion, and ascribe model-versus-data differences to model error. They are not really basing this conclusion on new model data or on new observational data. The only "new" observational dataset that they use is an early version of Leo Haimberger's radiosonde dataset (RAOBCORE v1.2). Leo's dataset was under development at the time all of us were working on the CCSP Report and the Santer et al. Science paper. It was not available for our assessment in 2005. As Leo has already shared with you, newer versions of RAOBCORE (v1.3 and v1.4) show amplification of surface warming in the tropical troposphere, in reasonable agreement with the model results that we presented in Fig. 3B of our Science paper. Douglass et al. did not use these newer versions of RAOBCORE v1.2. Nor did Douglass et al. use any "inconvenient" observational datasets (such as the NESDIS-based MSU T2 dataset of Zou et al., or the MSU T2 product of Vinnikov and Grody) showing pronounced tropospheric warming over the satellite era. Nor did Douglass et al. discuss the "two timescale issue" that formed an important part of our Science paper (i.e., how could models and multiple observational datasets show amplification behavior that was consistent in terms of monthly variability but inconsistent in terms of decadal trends?) Nor did Douglass et al. fairly portray results from Peter's 2007 GRL paper. In my personal opinion, Douglass et al. have ignored all scientific evidence that is in disagreement with their view of how the real world should be behaving.

I don't think it's a good strategy to submit a response to the Douglass et al. paper to the International Journal of Climatology (IJC). As Phil pointed out, IJC has a large backlog, so it might take some time to get a response published. Furthermore, Douglass et al. probably would be given the final word.

My suggestion is to submit (to Science) a short "update" of our 2005 paper. This update would only be submitted AFTER publication of the four new radiosonde-based temperature datasets mentioned by Peter. The update would involve:

1) Use of all four new radiosonde datasets.

2) Use of the latest versions of the UAH and RSS TLT data, and the latest versions of the T2 data from UAH, RSS, UMD (Vinnikov and Grody), and NESDIS (Zou et al.).

3) Use of the T2 data in 2) above AND the UAH and RSS T4 data to calculate tropical "TFu" temperatures, with all possible combinations of T4 and T2 datasets (e.g., RSS T4 and UMD T2, UAH T4 and UMD T2, etc.)

4) Calculating synthetic MSU temperatures from all model 20c3m runs currently available in the IPCC AR4 database. Calculation of synthetic MSU temperatures would rely on a method suggested by Carl (using weighting functions that depend on both the surface type [land, ocean] and the surface pressure at each grid-point) rather than on the static global-mean weighting function that we used previously. This is probably several months of work - but at least it will keep me off the streets and out of trouble.

5) Formal determination of statistical significance of model-versus-observed trend differences.

6) Brief examination of timescale-dependence of amplification factors.

7) As and both Peter and Melissa suggested, brief examination of sensitivity of estimated trends to the selected analysis period (e.g., use of 1979 to 1999; use of 1979 to 2001 or 2003 [for the small number of model 20c3m runs ending after 1999]; use of data for the post-NOAA9 period).

This will be a fair bit of effort, but I think it's worth it. Douglass et al. will try to make maximum political hay out of their IJC paper - which has already been sent to Andy Revkin at the New York Times. You can bet they've sent it elsewhere, too. I'm pretty sure that our colleague JC will portray Douglass et al. as definitive "proof" that all climate models are fundamentally flawed, UAH data are in amazing agreement with sonde-based estimates of tropospheric temperature change, global warming is not a serious problem, etc.

One of the most disturbing aspects of Douglass et al. is its abrupt dismissal of the finding (by Sherwood et al. and Randel and Wu) of a residual tropospheric cooling bias in the sonde data. Douglass et al. base this dismissal on the Christy et al. (2007) JGR paper, and on Christy's finding of biases in the night-time sonde data that magically offset the biases in the day-time data. Does that sound familiar? When did we last hear about new biases magically offsetting the effect of recently-discovered biases? As Yogi Berra would say, this is deja vu all over again....

I hope that one of the papers on the new sonde-based datasets directly addresses the subject of 'error compensation' in the day-time and night-time sonde data. This would be important to do.

It's unfortunate that Douglass et al. will probably be published well before the appearance of the papers on the new radiosonde datasets, and before an updated comparison of modeled-and observed tropospheric temperature trends.

I'd be grateful if you could let me know whether you are in agreement with the response strategy I've outlined above, and would like to be involved with an update of our 2005 Science paper.

With best regards,

Ben

Peter Thorne wrote:
> All,
>
> There are several additional reasons why we may not expect perfect
> agreement between models and obs that are outlined in the attached
> paper.
>
> It speaks in part to the trend uncertainty that Carl alluded to - taking
> differences between linear trend estimates is hard when the underlying
> series is noisy and perhaps non-linear. Work that John and Dian have
> done also shows this. Taking the ratio between two such estimates is
> always going to produce noisy results over relatively short trend
> periods when the signal is small relative to the natural variability.
>
> Also, 1979 as a start date may bias those estimates towards a "bias", I
> believe (this is unproven) because of endpoint effects due to natural
> variability that tend to damp the ratio of Trop/Surf trends (ENSO
> phasing and El Chichon) for any trend period with this start date. Given
> the N-9 uncertainty a reasonable case could be made for an evaluation of
> the obs that started only after N-9 and this may yield a very different
> picture.

> It also shows that the model result really is constrained to perturbed
> physics, at least for HadCM3. Unsurprising as convective adjustment is
> at the heart of most models. Certainly ours anyway. This result was
> cherry-picked and the rest of the paper discarded by Douglass et al.
>
> In addition to this, the state of play on the radiosondes has moved on
> substantially with RAOBCORE 1.4 (accepted I believe, Leo Haimberger
> should be in this - I'm adding him) which shows warming intermediate
> between UAH and RSS and I know of three additional efforts on
> radiosondes all of which strongly imply that the raobs datasets used in
> this paper are substantially under-estimating the warming rate (Steve
> Sherwood x2 and our automated system). So, there's going to be a whole
> suite of papers hopefully coming out within the next year or so that
> imply we at least cannot rule out from the radiosonde data warming
> consistent even with the absurd "mean of the model runs" criteria that
> is used in this paper.
>
> For info, our latest results imply a true raobs trend for 2LT in the
> tropics somewhere >0.08K/decade (we cannot place a defensible upper
> limit) ruling out most of the datasets used in the Douglass paper and
> ruling in possibility of consistency with models.
>
> Douglass et al also omit the newer MSU studies from the NESDIS group
> which in the absence of a reasonable criteria (a criteria I think we are
> some way away from still) to weed out bad obs datasets should be
> considered. Placing all obs datasets and the likely new raobs datasets
> would pretty much destroy this paper's main point. There's been a fair
> bit of cherry picking on the obs side which needs correcting here.
>
> Peter
>
> On Tue, 2007-12-04 at 15:40 -0800, carl mears wrote:
>> Karl -- thanks for clarifying what I was trying to say
>>
>> Some further comments.....
>>
>> At 02:53 PM 12/4/2007, Karl Taylor wrote:
>>> Dear all,
>>> 2) unforced variability hasn't dominated the observations.
>> But on this short time scale, we strongly suspect that it has
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>> observed trend cannot be proven to
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>> It we interesting to see if we can say anything more, when we start culling
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>> as Ben has suggested.
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>> -Carl
>>
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>>

--

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##########

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this
singer/christy/etc effort]
Date: Wed, 05 Dec 2007 14:19:17 -0800
Reply-to: santer1@llnl.gov
Cc: carl mears <mears@remss.com>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley
<wigley@ucar.edu>, Tom Wigley <wigley@cgd.ucar.edu>, "Thorne, Peter"
<peter.thorne@metoffice.gov.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, John
Lanzante <John.Lanzante@noaa.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>,
Melissa Free <Melissa.Free@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve
Klein <klein21@mail.llnl.gov>, Leopold Haimberger
<leopold.haimberger@univie.ac.at>, peter gleckler <gleckler1@llnl.gov>

<x-flowed>
Dear Phil,

Just a quick response to the issue of "model weighting" which you and
Carl raised in your emails.

We recently published a paper dealing with the identification of an
anthropogenic fingerprint in SSM/I-based estimates of total column water
vapor changes. This was a true multi-model detection and attribution
("D&A") study, which made use of results from 22 different A/OGCMs for
fingerprint and noise estimation. Together with Peter Gleckler and Karl
Taylor, I'm now in the process of repeating our water vapor D&A study
using a subset of the original 22 models. This subset will comprise
10-12 models which are demonstrably more successful in capturing
features of the observed mean state and variability of water vapor and
SST - particularly features crucial to the D&A problem (such as the
low-frequency variability). We've had fun computing a whole range of
metrics that might be used to define such a subset of "better" models.
The ultimate goal is to determine the sensitivity of our water vapor D&A
results to model quality. I think that this kind of analysis will be

unavoidable in the multi-model world in which we now live. Given substantial inter-model differences in simulation quality, "one model, one vote" is probably not the best policy for D&A work!

Once we've used Carl's method to calculate synthetic MSU temperatures from the IPCC AR4 20c3m data (as described in my previous email), it should be relatively easy to do a similar "model culling" exercise with MSU T2, T4, and TLT. In fact, this is what we had already planned to do in collaboration with Carl and Frank.

One key point in any model weighting or selection strategy is to avoid circularity. In the D&A context, it would be impermissible to include information on trend behavior as a criterion used for selecting "better" models. Likewise, if our interest is in assessing the statistical significance of model-versus-observed trend differences, we can't use model performance in simulating "observed" tropospheric or stratospheric trends (whatever those might be!) as a means of identifying more credible models.

A further issue, of course, is that we are relying on results from fully coupled A/OGCMs, and are making trend comparisons over relatively short periods (several decades). On these short timescales, estimates of the "true" trend in response to the applied 20c3m forcings are quite sensitive to natural variability noise (as Peter Thorne's 2007 GRL paper clearly illustrates). Because of such chaotic variability, even a hypothetical model with perfect physics and forcings would yield a distribution of tropospheric temperature trends over 1979 to 1999, some of which would show larger or smaller cooling than observed. This is why it's illogical to stratify model results according to correspondence between modeled and observed surface warming - something which John Christy is very fond of doing.

What we've done (in the new water vapor work described above) is to evaluate the fidelity with which the AR4 models simulate the observed mean state and variability of precipitable water and SST - not the trends in these quantities. We've looked at a model performance in a variety of different regions, and on multiple timescales. The results are fascinating, and show (at least for water vapor and SST) that every model has its own individual strengths and weaknesses. It is difficult to identify a subset of models that CONSISTENTLY does well in many different regions and over a range of different timescales.

My guess is that we would obtain somewhat different results for MSU temperatures - particularly for comparisons involving variability. Clearly, the absence of volcanic forcing in roughly half of the 20c3m experiments will have a large impact on the estimated variability of synthetic T4 temperatures (and perhaps even on T2), and hence on model-versus-data variability comparisons. It's also quite possible that the inclusion or absence of volcanic forcing has an impact not only on the amplitude of the variability of global-mean T4 anomalies, but also on the pattern of T4 variability. So model ranking exercises based on performance in simulating the mean state and variability of T4 and T2 may show some connection to the presence or absence of volcanic/ozone forcing.

The sad thing is we are being distracted from doing this fun stuff by the need to respond to Douglass et al. That's a real shame.

With best regards,

Ben

Phil Jones wrote:

> All,
>   IJC do have comments but only very rarely. I see little point in
> doing this
> as there is likely to be a word limit, and if the system works properly
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> print - it should be in the IJC early online view fairly soon based on
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>   A paper pointing out the issues of trying to weight models in some way
> would be very beneficial to the community. AR5 will have to go down this
> route at some point. How models simulate the
> recent trends at the surface and in the troposphere/stratosphere and
> how they might be ranked is a possibility. This could bring in the
> new work Peter alludes to with the sondes.
>   There are also some aspects of recent surface T changes that could be
> discussed as well. These relate to the growing dominance of buoy SSTs
> (now 70% of the total) vs conventional ships. There is a paper in J.
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> could conceivably be cooler than ship-based SST by about 0.1C - meaning
> that the last 5-10 years are being gradually underestimated over the
> oceans.
> Overlap is still too short to be confident about this, but it highlights a
> major systematic change occurring in surface ocean measurements. As the
> buoys are presumably better for absolute SSTs, this means models
> driven with fixed SSTs should be using fields that are marginally cooler.
>
>   And then there is the continual reference to Kalnay and Cai, when
> Simmons et al (2004) have shown the problems with NCEP. It is possible
> to add in the ERA-Interim analyses and operational analyses to
> being results from ERA-40 up to date.
>
> Cheers
> Phil
>
>
> At 23:40 04/12/2007, carl mears wrote:
>> Karl -- thanks for clarifying what I was trying to say
>>
>> Some further comments.....
>>
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>>
>> -Carl
>>
>>
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>>
>
> 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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> UK
> --

>

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Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

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##########

From: Ben Santer <santer1@llnl.gov>
To: Melissa Free <Melissa.Free@noaa.gov>
Subject: Re: [Fwd: sorry to take your time up, but really do need a   scrub of this
singer/christy/etc effort]
Date: Thu, 06 Dec 2007 10:52:42 -0800
Reply-to: santer1@llnl.gov
Cc: Phil Jones <p.jones@uea.ac.uk>, carl mears <mears@remss.com>, Karl Taylor
<taylor13@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Tom Wigley <wigley@cgd.ucar.edu>,
 "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Steven Sherwood
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Seidel'" <dian.seidel@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve Klein
<klein21@mail.llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>,

peter gleckler <gleckler1@llnl.gov>

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Dear Melissa,

No, this would not be dire. What is dire is Douglass et al.'s willful neglect of any observational datasets that do not support their arguments. Recall that our 2005 Science paper presented information from all observational datasets available to us at that time, even from datasets that showed large differences relative to the model data. We did not present results from RSS alone.

With best regards,

Ben
Melissa Free wrote:
> One further question about the Douglass paper: What about the
> implications of a real model-observation difference for upper-air
> trends? Is this really so dire?
> -Melissa
>

--

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FAX: (925) 422-7675
email: santer1@llnl.gov

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##########

From: Dian Seidel <dian.seidel@noaa.gov>
To: santer1@llnl.gov
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Thu, 06 Dec 2007 13:04:20 -0500
Cc: Phil Jones <p.jones@uea.ac.uk>, carl mears <mears@remss.com>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Tom Wigley <wigley@cgd.ucar.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve Klein <klein21@mail.llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, peter gleckler <gleckler1@llnl.gov>

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Hello Ben and Colleagues,

I've been following these exchanges with interest. One particular point in your message below is a little puzzling to me. That's the issue of trying to avoid circularity in the culling of models for any given D&A study.

Two potential problems occur to me. One is that choosing models on the

basis of their fidelity to observed regional and short term variability may not be completely orthogonal to choosing based on long-term trend. That's because those smaller scale changes may contribute to the trends and their patterns. Second, choosing a different set of models for one variable (temperature) than for another (humidity) seems highly problematic. If we are interested in projections of other variables, e.g. storm tracks or cloud cover, for which D&A has not been done, which group of models would we then deem to be most credible? I don't have a good alternative to propose, but, in light of these considerations, maybe one-model-one-vote doesn't appear so unreasonable after all.

With regards,
Dian

Ben Santer wrote:
> Dear Phil,
>
> Just a quick response to the issue of "model weighting" which you and
> Carl raised in your emails.
>
> We recently published a paper dealing with the identification of an
> anthropogenic fingerprint in SSM/I-based estimates of total column
> water vapor changes. This was a true multi-model detection and
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> Ben
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>>
>>   And then there is the continual reference to Kalnay and Cai, when
>> Simmons et al (2004) have shown the problems with NCEP. It is possible
>> to add in the ERA-Interim analyses and operational analyses to
>> being results from ERA-40 up to date.
>>
>> Cheers
>> Phil
>>
>>
>> At 23:40 04/12/2007, carl mears wrote:
>>> Karl -- thanks for clarifying what I was trying to say
>>>
>>> Some further comments.....
>>>
>>> At 02:53 PM 12/4/2007, Karl Taylor wrote:
>>>> Dear all,
>>>> 2) unforced variability hasn't dominated the observations.
>>>
>>> But on this short time scale, we strongly suspect that it has
>>> dominated. For example, the
>>> 2 sigma error bars from table 3.4, CCSP for satellite TLT are 0.18
>>> (UAH) or 0.19 (RSS), larger
>>> than either group's trends (0.05, 0.15) for 1979-2004. These were
>>> calculated using a "goodness
>>> of linear fit" criterion, corrected for autocorrelation. This is a
>>> probably a reasonable
>>> estimate of the contribution of unforced variability to trend
>>> uncertainty.
>>>
>>>
>>>
>>>> Douglass et al. have *not* shown that every individual model is in
>>>> fact inconsistent with the observations. If the spread of
>>>> individual model results is large enough and at least 1 model
>>>> overlaps the observations, then one cannot claim that all models
>>>> are wrong, just that the mean is biased.
>>>
>>>
>>> Given the magnitude of the unforced variability, I would say "the
>>> mean *may* be biased." You can't prove this
>>> with only one universe, as Tom alluded. All we can say is that the
>>> observed trend cannot be proven to
>>> be inconsistent with the model results, since it is inside their range.
>>>
>>> It we interesting to see if we can say anything more, when we start
>>> culling out the less realistic models,
>>> as Ben has suggested.
>>>
>>> -Carl
>>>
>>>
>>>
>>>
>>
>> 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
>> --

>>
>
>

--
~~~~~~
Dian J. Seidel
NOAA Air Resources Laboratory (R/ARL)
1315 East West Highway
Silver Spring, MD 20910

Dian.Seidel@noaa.gov
Phone: +1-301-713-0295 ext. 126
Fax: +1-301-713-0119
http://www.arl.noaa.gov/ss/climate
~~~~~~

</x-flowed>

825. 1197325034.txt
##
##########

From: Tom Wigley <wigley@ucar.edu>
To: santer1@llnl.gov
Subject: Re: [Fwd: [Fwd: FW: Press Release from The Science & Environmental Policy
Project]]
Date: Mon, 10 Dec 2007 17:17:14 -0700
Cc: carl mears <mears@remss.com>, Frank Wentz <frank.wentz@remss.com>, Tom Wigley
<wigley@cgd.ucar.edu>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante
<John.Lanzante@noaa.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free
<Melissa.Free@noaa.gov>, Karl Taylor <taylor13@llnl.gov>, Steve Klein
<klein21@mail.llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>,
"Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Philip D. Jones'"
<p.jones@uea.ac.uk>

<x-flowed>
Dear all,

I think the scientific fraud committed by Douglass needs to
be exposed. His co-authors may be innocent bystanders, but
I doubt it.

In normal circumstances, what Douglass has done would cause
him to lose his job -- a parallel is the South Korean cloning
fraud case.

I have suggested that someone like Chris Mooney should be
told about this.

Tom.

++++++++++++++++++++

Ben Santer wrote:

> Dear folks,
>
> I knew this would happen. In my opinion, we should respond to this
> continued misrepresentation of the science sooner rather than later.
>
> With best regards,
>
> Ben
> --
>
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-2486
> FAX: (925) 422-7675
> email: santer1@llnl.gov
> --
>
>
> --
>
> Subject:
> [Fwd: FW: Press Release from The Science & Environmental Policy Project]
> From:
> "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
> Date:
> Mon, 10 Dec 2007 17:23:12 -0500
> To:
> _NESDIS NCDC CCSP Temp Trends Lead Authors
> <CCSPTempTrendAuthors.NCDC@noaa.gov>
>
> To:
> _NESDIS NCDC CCSP Temp Trends Lead Authors
> <CCSPTempTrendAuthors.NCDC@noaa.gov>
>
>
> FYI --- related to trop-sfc temps
>
> --
> *From:* George Marshall Institute [mailto:info@marshall.org]
> *Sent:* Monday, December 10, 2007 4:24 PM
> *To:* info@marshall.org
> *Subject:* Press Release from The Science & Environmental Policy Project
>
> */Press Release from The Science & Environmental Policy Project/**/ /*
>
> **Where & When**
>
> *The National Press Club*
>
> *529 14th Street, NW, 13th Floor*
>
> *Lisagor Room*
>
> *Washington, DC 20045*
>
> **
>
> **December 14, 2007 **

> **8am-11am **
>
> **
>
> *Breakfast refreshments will be served.*
>
> **
>
> **/To RSVP, please email info@sepp.org <mailto:info@sepp.org>. /**
>
> //
>
>
>
> You are invited to a timely breakfast briefing
>
> on December 14, 2007 at 8:30 a.m. at the National Press Club,
> organized by
>
> The Science & Environmental Policy Project (SEPP).
>
> As Al Gore collects his Nobel Prize and 15,000(more or less) in Bali
> struggle to find a successor regime for the ineffective and unlamented
> Kyoto Protocol, an 'inconvenient truth' has emerged:
>
> NATURE RULES THE CLIMATE: HUMAN-PRODUCED GREENHOUSE GASES ARE NOT
> RESPONSIBLE FOR GLOBAL WARMING. Therefore, schemes to control CO2
> emissions are ineffective and pointless, though very costly.
>
> Come and listen to the authors of a peer-reviewed scientific study,
> just published in the International Journal of Climatology (of the
> Royal Meteorological Society), present their startling findings.
>
> Presenters:
>
> *Prof. David Douglass*, University of Rochester: GH Models clash with
> best observations
>
> *Prof. John Christy*, University of Alabama: How GH models
> overestimate GH warming
>
> *Prof. S. Fred Singer*, University of Virginia: Changes in solar
> activity control the climate.
>
> I am sure you will appreciate the importance of their new result. Once
> one accepts the documented evidence that CO2 is insignificant in
> warming the climate, all kinds of consequences follow logically:
>
> * ·* Unburdened by climate fears, the US can pursue a more
>
> rational energy policy, leading to less dependence on oil/gas
>
> imports.
>
> *·* The current legislative efforts to cap CO2, or to control its
>
> emission in other ways, are utterly useless.
>
> *·* Ambitious programs claiming to reduce CO2 emissions (like
>
> ethanol, wind power, carbon sequestration, etc.) are a
>

> complete waste.
>
> *·* The EPA can now deny California's request for a waiver on
>
> CAFE.
>
> *·* The EPA can now respond properly to the Supreme Court
>
> ruling on CO2.
>
> *·* International negotiations can assume a different dimension.
>
> SEPP has reserved the Lisagor Room at the National Press Club for
> Friday December 14 from 8-11 am. Breakfast will be served.
>
> **_Please e-mail your acceptance to info@sepp.org._**
>
>
>
> *Forward email
>
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<http://visitor.constantcontact.com/d.jsp?v=001J6Npdb-1OsnhGJdJTw8_maCaaf4df6fcRfvPZumONJIf2OvuCb_hnaPXbf8Hkm0L&p=un>
>
> This email was sent to info@marshall.org, by info@marshall.org
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> Update Profile/Email Address
>
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>
> The George C. Marshall Institute | 1625 K St. NW Suite, 1050 |
> Washington | DC | 20006
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>
> --
>
> *Dr. Thomas R. Karl, L.H.D.*
>
> */Director/*//
>
> NOAA’s National Climatic Data Center
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> Veach-Baley Federal Building
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> 151 Patton Avenue

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> Tel: (828) 271-4476
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> Fax: (828) 271-4246
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> Thomas.R.Karl@noaa.gov <mailto:Thomas.R.Karl@noaa.gov>
>

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826. 1197507092.txt
##

From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: Douglass paper
Date: Wed, 12 Dec 2007 19:51:32 -0800
Reply-to: santer1@llnl.gov
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>

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Dear Tim,

Thanks for the "heads up". As Phil mentioned, I was already aware of this. The Douglass et al. paper was rejected twice before it was finally accepted by IJC. I think this paper is a real embarrassment for the IJC. It has serious scientific flaws. I'm already working on a response.

Phil can tell you about some of the other sordid details of Douglass et al. These guys ignored information from radiosonde datasets that did not support their "models are wrong" argument (even though they had these datasets in their possession). Pretty deplorable behaviour...

Douglass is the guy who famously concluded (after examining the temperature response to Pinatubo) that the climate system has negative sensitivity. Amazingly, he managed to publish that crap in GRL. Christy sure does manage to pick some brilliant scientific collaborators...

With best regards,

Ben

Tim Osborn wrote:
> Hi Ben,
>
> I guess it's likely that you're aware of the Douglass paper that's just
> come out in IJC, but in case you aren't then a reprint is attached.
> They are somewhat critical of your 2005 paper, though I recall that some
> (most?) of Douglass' previous papers -- and papers that he's tried to
> get through the review process -- appear to have serious problems.
>
> cc Phil & Keith for your interest too!
>
> Cheers
>
> Tim

> Dr Timothy J Osborn, Academic Fellow
> 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
>

--

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Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

827. 1197590292.txt
##########

From: Ben Santer <santer1@llnl.gov>
To: carl mears <mears@remss.com>
Subject: Re: [Fwd: sorry to take your time up, but really do need a   scrub of this singer/christy/etc effort]
Date: Thu, 13 Dec 2007 18:58:12 -0800
Reply-to: santer1@llnl.gov
Cc: SHERWOOD Steven <steven.sherwood@yale.edu>, Tom Wigley <wigley@cgd.ucar.edu>, Frank Wentz <frank.wentz@remss.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, John Lanzante <John.Lanzante@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, "David C. Bader" <bader2@llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>

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Dear folks,

I've been doing some calculations to address one of the statistical issues raised by the Douglass et al. paper in the International Journal of Climatology. Here are some of my results.

Recall that Douglass et al. calculated synthetic T2LT and T2 temperatures from the CMIP-3 archive of 20th century simulations ("20c3m" runs). They used a total of 67 20c3m realizations, performed with 22 different models. In calculating the statistical uncertainty of the model trends, they introduced sigma{SE}, an "estimate of the uncertainty of the mean of the predictions of the trends". They defined sigma{SE} as follows:

sigma{SE} = sigma / sqrt(N - 1), where

"N = 22 is the number of independent models".

As we've discussed in our previous correspondence, this definition has serious problems (see comments from Carl and Steve below), and allows Douglass et al. to reach the erroneous conclusion that modeled T2LT and T2 trends are significantly different from the observed T2LT and T2 trends in both the RSS and UAH datasets. This comparison of simulated and observed T2LT and T2 trends is given in Table III of Douglass et al. [As an amusing aside, I note that the RSS datasets are referred to as "RSS" in this table, while UAH results are designated as "MSU". I guess there's only one true "MSU" dataset...]

I decided to take a quick look at the issue of the statistical significance of differences between simulated and observed tropospheric temperature trends. My first cut at this "quick look" involves only UAH and RSS observational data - I have not yet done any tests with radiosonde datas, UMD T2 data, or satellite results from Zou et al.

I operated on the same 49 realizations of the 20c3m experiment that we used in Chapter 5 of CCSP 1.1. As in our previous work, all model results are synthetic T2LT and T2 temperatures that I calculated using a static weighting function approach. I have not yet implemented Carl's more sophisticated method of estimating synthetic MSU temperatures from model data (which accounts for effects of topography and land/ocean differences). However, for the current application, the simple static weighting function approach is more than adequate, since we are focusing on T2LT and T2 changes over tropical oceans only - so topographic and land-ocean differences are unimportant. Note that I still need to calculate synthetic MSU temperatures from about 18-20 20c3m realizations which were not in the CMIP-3 database at the time we were working on the CCSP report. For the full response to Douglass et al., we should use the same 67 20c3m realizations that they employed.

For each of the 49 realizations that I processed, I first masked out all tropical land areas, and then calculated the spatial averages of monthly-mean, gridded T2LT and T2 data over tropical oceans (20N-20S). All model and observational results are for the common 252-month period from January 1979 to December 1999 - the longest period of overlap between the RSS and UAH MSU data and the bulk of the 20c3m runs. The simulated trends given by Douglass et al. are calculated over the same 1979 to 1999 period; however, they use a longer period (1979 to 2004) for calculating observational trends - so there is an inconsistency between their model and observational analysis periods, which they do not explain. This difference in analysis periods is a little puzzling given that we are dealing with relatively short observational record lengths, resulting in some sensitivity to end-point effects.

I then calculated anomalies of the spatially-averaged T2LT and T2 data (w.r.t. climatological monthly-means over 1979-1999), and fit least-squares linear trends to model and observational time series. The standard errors of the trends were adjusted for temporal autocorrelation of the regression residuals, as described in Santer et al. (2000) ["Statistical significance of trends and trend differences in layer-average atmospheric temperature time series"; JGR 105, 7337-7356.]

Consider first panel A of the attached plot. This shows the simulated and observed T2LT trends over 1979 to 1999 (again, over 20N-20S, oceans only) with their adjusted 1-sigma confidence intervals). For the UAH and RSS data, it was possible to check against the adjusted confidence intervals independently calculated by Dian during the course of work on the CCSP report. Our adjusted confidence intervals are in good

agreement. The grey shaded envelope in panel A denotes the 1-sigma standard error for the RSS T2LT trend.

There are 49 pairs of UAH-minus-model trend differences and 49 pairs of RSS-minus-model trend differences. We can therefore test - for each model and each 20c3m realization - whether there is a statistically significant difference between the observed and simulated trends.

Let bx and by represent any single pair of modeled and observed trends, with adjusted standard errors s{bx} and s{by}. As in our previous work (and as in related work by John Lanzante), we define the normalized trend difference d as:

d = (bx - by) / sqrt[(s{bx})**2 + (s{by})**2]

Under the assumption that d is normally distributed, values of d > +1.96 or < -1.96 indicate observed-minus-model trend differences that are significant at the 5% level. We are performing a two-tailed test here, since we have no information a priori about the "direction" of the model trend (i.e., whether we expect the simulated trend to be significantly larger or smaller than observed).

Panel c shows values of the normalized trend difference for T2LT trends. the grey shaded area spans the range +1.96 to -1.96, and identifies the region where we fail to reject the null hypothesis (H0) of no significant difference between observed and simulated trends.

Consider the solid symbols first, which give results for tests involving RSS data. We would reject H0 in only one out of 49 cases (for the CCCma-CGCM3.1(T47) model). The open symbols indicate results for tests involving UAH data. Somewhat surprisingly, we get the same qualitative outcome that we obtained for tests involving RSS data: only one of the UAH-model trend pairs yields a difference that is statistically significant at the 5% level.

Panels b and d provide results for T2 trends. Results are very similar to those achieved with T2LT trends. Irrespective of whether RSS or UAH T2 data are used, significant trend differences occur in only one of 49 cases.

Bottom line: Douglass et al. claim that "In all cases UAH and RSS satellite trends are inconsistent with model trends." (page 6, lines 61-62). This claim is categorically wrong. In fact, based on our results, one could justifiably claim that THERE IS ONLY ONE CASE in which model T2LT and T2 trends are inconsistent with UAH and RSS results! These guys screwed up big time.

SENSITIVITY TESTS

QUESTION 1: Some of the model-data trend comparisons made by Douglass et al. used temperatures averaged over 30N-30S rather than 20N-20S. What happens if we repeat our simple trend significance analysis using T2LT and T2 data averaged over ocean areas between 30N-30S?

ANSWER 1: Very little. The results described above for oceans areas between 20N-20S are virtually unchanged.

QUESTION 2: Even though it's clearly inappropriate to estimate the standard errors of the linear trends WITHOUT accounting for temporal autocorrelation effects (the 252 time sample are clearly not independent; effective sample sizes typically range from 6 to 56), someone is bound to ask what the outcome is when one repeats the paired trend tests with non-adjusted standard errors. So here are the results:

T2LT tests, RSS observational data: 19 out of 49 trend differences are significant at the 5% level.
T2LT tests, UAH observational data: 34 out of 49 trend differences are significant at the 5% level.

T2 tests, RSS observational data: 16 out of 49 trend differences are significant at the 5% level.
T2 tests, UAH observational data: 35 out of 49 trend differences are significant at the 5% level.

So even under the naive (and incorrect) assumption that each model and observational time series contains 252 independent time samples, we STILL find no support for Douglass et al.'s assertion that: "In all cases UAH and RSS satellite trends are inconsistent with model trends." Q.E.D.

If Leo is agreeable, I'm hopeful that we'll be able to perform a similar trend comparison using synthetic MSU T2LT and T2 temperatures calculated from the RAOBCORE radiosonde data - all versions, not just v1.2!

As you can see from the email list, I've expanded our "focus group" a little bit, since a number of you have written to me about this issue.

I am leaving for Miami on Monday, Dec. 17th. My Mom is having cataract surgery, and I'd like to be around to provide her with moral and practical support. I'm not exactly sure when I'll be returning to PCMDI - although I hope I won't be gone longer than a week. As soon as I get back, I'll try to make some more progress with this stuff. Any suggestions or comments on what I've done so far would be greatly appreciated. And for the time being, I think we should not alert Douglass et al. to our results.

With best regards, and happy holidays! May all your "Singers" be carol singers, and not of the S. Fred variety...

Ben

(P.S.: I noticed one unfortunate typo in Table II of Douglass et al. The MIROC3.2 (medres) model is referred to as "MIROC3.2_Merdes"....)

carl mears wrote:
> Hi Steve
>
> I'd say it's the equivalent of rolling a 6-sided die a hundred times, and
> finding a mean value of ~3.5 and a standard deviation of ~1.7, and
> calculating the standard error of the mean to be ~0.17 (so far so
> good). An then rolling the die one more time, getting a 2, and
> claiming that the die is no longer 6 sided because the new measurement
> is more than 2 standard errors from the mean.
>
> In my view, this problem trumps the other problems in the paper.
> I can't believe Douglas is a fellow of the American Physical Society.
>
> -Carl
>
>
> At 02:07 AM 12/6/2007, you wrote:
>> If I understand correctly, what Douglass et al. did makes the stronger
>> assumption that unforced variability is *insignificant*. Their
>> statistical test is logically equivalent to falsifying a climate model
>> because it did not consistently predict a particular storm on a
>> particular day two years from now.

>
>
> Dr. Carl Mears
> Remote Sensing Systems
> 438 First Street, Suite 200, Santa Rosa, CA 95401
> mears@remss.com
> 707-545-2904 x21
> 707-545-2906 (fax))

--

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Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

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Attachment Converted: "c:\eudora\attach\douglass_reply1.pdf"

828. 1197590293.txt
##########

From: Ben Santer <santer1@llnl.gov>
To: carl mears <mears@remss.com>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Thu, 13 Dec 2007 18:58:12 -0800
Reply-to: santer1@llnl.gov
Cc: SHERWOOD Steven <steven.sherwood@yale.edu>, Tom Wigley <wigley@cgd.ucar.edu>, Frank Wentz <frank.wentz@remss.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, John Lanzante <John.Lanzante@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, "David C. Bader" <bader2@llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>

<x-flowed>
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With best regards, and happy holidays! May all your "Singers" be carol singers, and not of the S. Fred variety...

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Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

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documents\eudora\attach\douglass_reply1.pdf"

829. 1197660675.txt
##########

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To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this
singer/christy/etc effort]
Date: Fri, 14 Dec 2007 14:31:15 -0800
Reply-to: santer1@llnl.gov
Cc: carl mears <mears@remss.com>, SHERWOOD Steven <steven.sherwood@yale.edu>, Tom
Wigley <wigley@cgd.ucar.edu>, Frank Wentz <frank.wentz@remss.com>, "'Philip D.
Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein
<klein21@mail.llnl.gov>, John Lanzante <John.Lanzante@noaa.gov>, "Thorne, Peter"
<peter.thorne@metoffice.gov.uk>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa
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>
> --
>
> *Dr. Thomas R. Karl, L.H.D.*
>
> */Director/*//
>
> NOAA's National Climatic Data Center
>
> Veach-Baley Federal Building
>
> 151 Patton Avenue
>
> Asheville, NC 28801-5001
>
> Tel: (828) 271-4476
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>

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830. 1197739308.txt
##########

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Date: Sat, 15 Dec 2007 12:21:48 -0500
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I suspect you will find there is indeed stat sig. similar trends incl. amplification.
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areas, and then calculated the spatial averages of monthly-mean, gridded T2LT and T2
data over tropical oceans (20N-20S). All model and observational results are for the
common 252-month period from January 1979 to December 1999 - the longest period of
overlap between the RSS and UAH MSU data and the bulk of the 20c3m runs. The simulated
trends given by Douglass et al. are calculated over the same 1979 to 1999

period;
however, they use a longer period (1979 to 2004) for calculating observational trends -
so there is an inconsistency between their model and observational analysis periods,
which they do not explain. This difference in analysis periods is a little puzzling
given that we are dealing with relatively short observational record lengths, resulting
in some sensitivity to end-point effects.
I then calculated anomalies of the spatially-averaged T2LT and T2 data (w.r.t.
climatological monthly-means over 1979-1999), and fit least-squares linear trends to
model and observational time series. The standard errors of the trends were adjusted for
temporal autocorrelation of the regression residuals, as described in Santer et al.
(2000) ["Statistical significance of trends and trend differences in layer-average
atmospheric temperature time series"; JGR 105, 7337-7356.]
Consider first panel A of the attached plot. This shows the simulated and observed T2LT
trends over 1979 to 1999 (again, over 20N-20S, oceans only) with their adjusted 1-sigma
confidence intervals). For the UAH and RSS data, it was possible to check against the
adjusted confidence intervals independently calculated by Dian during the course of work
on the CCSP report. Our adjusted confidence intervals are in good agreement. The grey
shaded envelope in panel A denotes the 1-sigma standard error for the RSS T2LT trend.
There are 49 pairs of UAH-minus-model trend differences and 49 pairs of RSS-minus-model
trend differences. We can therefore test - for each model and each 20c3m realization -
whether there is a statistically significant difference between the observed and
simulated trends.
Let bx and by represent any single pair of modeled and observed trends, with adjusted
standard errors s{bx} and s{by}. As in our previous work (and as in related work by John
Lanzante), we define the normalized trend difference d as:
d = (bx - by) / sqrt[(s{bx})**2 + (s{by})**2]
Under the assumption that d is normally distributed, values of d > +1.96 or < -1.96
indicate observed-minus-model trend differences that are significant at the 5% level. We
are performing a two-tailed test here, since we have no information a priori about the
"direction" of the model trend (i.e., whether we expect the simulated trend to be
significantly larger or smaller than observed).
Panel c shows values of the normalized trend difference for T2LT trends.
the grey shaded area spans the range +1.96 to -1.96, and identifies the region where we
fail to reject the null hypothesis (H0) of no significant difference between observed
and simulated trends.
Consider the solid symbols first, which give results for tests involving RSS data. We
would reject H0 in only one out of 49 cases (for the CCCma-CGCM3.1(T47) model).

The open
symbols indicate results for tests involving UAH data. Somewhat surprisingly, we get the
same qualitative outcome that we obtained for tests involving RSS data: only one of the
UAH-model trend pairs yields a difference that is statistically significant at the 5%
level.
Panels b and d provide results for T2 trends. Results are very similar to those achieved
with T2LT trends. Irrespective of whether RSS or UAH T2 data are used, significant trend
differences occur in only one of 49 cases.
Bottom line: Douglass et al. claim that "In all cases UAH and RSS satellite trends are
inconsistent with model trends." (page 6, lines 61-62). This claim is categorically
wrong. In fact, based on our results, one could justifiably claim that THERE IS ONLY ONE
CASE in which model T2LT and T2 trends are inconsistent with UAH and RSS results! These
guys screwed up big time.
SENSITIVITY TESTS
QUESTION 1: Some of the model-data trend comparisons made by Douglass et al. used
temperatures averaged over 30N-30S rather than 20N-20S. What happens if we repeat our
simple trend significance analysis using T2LT and T2 data averaged over ocean areas
between 30N-30S?
ANSWER 1: Very little. The results described above for oceans areas between 20N-20S are
virtually unchanged.
QUESTION 2: Even though it's clearly inappropriate to estimate the standard errors of
the linear trends WITHOUT accounting for temporal autocorrelation effects (the 252 time
sample are clearly not independent; effective sample sizes typically range from 6 to
56), someone is bound to ask what the outcome is when one repeats the paired trend tests
with non-adjusted standard errors. So here are the results:
T2LT tests, RSS observational data: 19 out of 49 trend differences are significant at
the 5% level.
T2LT tests, UAH observational data: 34 out of 49 trend differences are significant at
the 5% level.
T2 tests, RSS observational data: 16 out of 49 trend differences are significant at the
5% level.
T2 tests, UAH observational data: 35 out of 49 trend differences are significant at the
5% level.
So even under the naive (and incorrect) assumption that each model and observational
time series contains 252 independent time samples, we STILL find no support for Douglass
et al.'s assertion that: "In all cases UAH and RSS satellite trends are inconsistent
with model trends."
Q.E.D.
If Leo is agreeable, I'm hopeful that we'll be able to perform a similar trend

comparison using synthetic MSU T2LT and T2 temperatures calculated from the RAOBCORE
radiosonde data - all versions, not just v1.2!
As you can see from the email list, I've expanded our "focus group" a little bit, since
a number of you have written to me about this issue.
I am leaving for Miami on Monday, Dec. 17th. My Mom is having cataract surgery, and I'd
like to be around to provide her with moral and practical support. I'm not exactly sure
when I'll be returning to PCMDI - although I hope I won't be gone longer than a week. As
soon as I get back, I'll try to make some more progress with this stuff. Any suggestions
or comments on what I've done so far would be greatly appreciated. And for the time
being, I think we should not alert Douglass et al. to our results.
With best regards, and happy holidays! May all your "Singers" be carol singers, and not
of the S. Fred variety...
Ben
(P.S.: I noticed one unfortunate typo in Table II of Douglass et al. The MIROC3.2
(medres) model is referred to as "MIROC3.2_Merdes"....)
carl mears wrote:

Hi Steve
I'd say it's the equivalent of rolling a 6-sided die a hundred times, and
finding a mean value of ~3.5 and a standard deviation of ~1.7, and
calculating the standard error of the mean to be ~0.17 (so far so
good). An then rolling the die one more time, getting a 2, and
claiming that the die is no longer 6 sided because the new measurement
is more than 2 standard errors from the mean.
In my view, this problem trumps the other problems in the paper.
I can't believe Douglas is a fellow of the American Physical Society.
-Carl
At 02:07 AM 12/6/2007, you wrote:

If I understand correctly, what Douglass et al. did makes the stronger assumption that
unforced variability is *insignificant*. Their statistical test is logically equivalent
to falsifying a climate model because it did not consistently predict a particular storm
on a particular day two years from now.

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--
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--

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References

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3. mailto:Thomas.R.Karl@noaa.gov
4. mailto:Thomas.R.Karl@noaa.gov

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From: Leopold Haimberger <leopold.haimberger@univie.ac.at>
To: John.Lanzante@noaa.gov
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Sun, 23 Dec 2007 15:50:17 +0100
Cc: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, santer1@llnl.gov, Sherwood Steven <steven.sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, 'Susan Solomon' <susan.solomon@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>

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Dear all,

I have attached a plot which summarizes the recent developments
concerning tropical radiosonde temperature datasets and which could be
a candidate to be included in a reply to Douglass et al.
It contains trend profiles from unadjusted radiosondes, HadAT2-adjusted
radiosondes, RAOBCORE (versions 1.2-1.4) adjusted radiosondes
and from radiosondes adjusted with a neighbor composite method (RICH)
that uses the break dates detected with RAOBCORE (v1.4) as metadata.
RAOBCORE v1.2,v1.3 are documented in Haimberger (2007), RAOBCORE v1.4
and RICH are discussed in the manuscript I mentioned in my previous email.
Latitude range is 20S-20N, only time series with less than 24 months of
missing data are included. Spatial sampling of all curves is the same
except HadAT which contains less stations that meet the 24month

criterion. Sampling uncertainty of the trend curves is ca.
+/-0.1K/decade (95% percentiles estimated with bootstrap method).

RAOBCORE v1.3,1.4 and RICH are results from ongoing research and warming trends from radiosondes may still be underestimated.
The upper tropospheric warming maxima from RICH are even larger (up to 0.35K/decade, not shown), if only radiosondes within the tropics (20N-20S) are allowed as reference for adjustment of tropical radiosonde temperatures. The pink/blue curves in the attached plot should therefore not be regarded as upper bound of what may be achieved with plausible choices of reference series for homogenization.

Please let me know your comments.

I wish you a merry Christmas.

With best regards

Leo

John Lanzante wrote:
> Ben,
>
> Perhaps a resampling test would be appropriate. The tests you have performed
> consist of pairing an observed time series (UAH or RSS MSU) with each one
> of 49 GCM times series from your "ensemble of opportunity". Significance
> of the difference between each pair of obs/GCM trends yields a certain
> number of "hits".
>
> To determine a baseline for judging how likely it would be to obtain the
> given number of hits one could perform a set of resampling trials by
> treating one of the ensemble members as a surrogate observation. For each
> trial, select at random one of the 49 GCM members to be the "observation".
> From the remaining 48 members draw a bootstrap sample of 49, and perform
> 49 tests, yielding a certain number of "hits". Repeat this many times to
> generate a distribution of "hits".
>
> The actual number of hits, based on the real observations could then be
> referenced to the Monte Carlo distribution to yield a probability that this
> could have occurred by chance. The basic idea is to see if the observed
> trend is inconsistent with the GCM ensemble of trends.
>
> There are a couple of additional tweaks that could be applied to your method.
> You are currently computing trends for each of the two time series in the
> pair and assessing the significance of their differences. Why not first
> create a difference time series and assess the significance of it's trend?
> The advantage of this is that you would reduce somewhat the autocorrelation
> in the time series and hence the effect of the "degrees of freedom"
> adjustment. Since the GCM runs are based on coupled model runs this
> differencing would help remove the common externally forced variability,
> but not internally forced variability, so the adjustment would still be
> needed.
>
> Another tweak would be to alter the significance level used to assess
> differences in trends. Currently you are using the 5% level, which yields
> only a small number of hits. If you made this less stringent you would get
> potentially more weaker hits. But it would all come out in the wash so to
> speak since the number of hits in the Monte Carlo simulations would increase
> as well. I suspect that increasing the number of expected hits would make the
> whole procedure more powerful/efficient in a statistical sense since you
> would no longer be dealing with a "rare event". In the current scheme, using
> a 5% level with 49 pairings you have an expected hit rate of 0.05 X 49 = 2.45.
> For example, if instead you used a 20% significance level you would have an

> expected hit rate of 0.20 X 49 = 9.8.
>
> I hope this helps.
>
> On an unrelated matter, I'm wondering a bit about the different versions of
> Leo's new radiosonde dataset (RAOBCORE). I was surprised to see that the
> latest version has considerably more tropospheric warming than I recalled
> from an earlier version that was written up in JCLI in 2007. I have a
> couple of questions that I'd like to ask Leo. One concern is that if we use
> the latest version of RAOBCORE is there a paper that we can reference --
> if this is not in a peer-reviewed journal is there a paper in submission?
> The other question is: could you briefly comment on the differences in
> methodology used to generate the latest version of RAOBCORE as compared to
> the version used in JCLI 2007, and what/when/where did changes occur to
> yield a stronger warming trend?
>
> Best regards,
>
> _____John
>
>
>
> On Saturday 15 December 2007 12:21 pm, Thomas.R.Karl wrote:
>
>> Thanks Ben,
>>
>> You have the makings of a nice article.
>>
>> I note that we would expect to 10 cases that are significantly different
>> by chance (based on the 196 tests at the .05 sig level). You found 3.
>> With appropriately corrected Leopold I suspect you will find there is
>> indeed stat sig. similar trends incl. amplification. Setting up the
>> statistical testing should be interesting with this many combinations.
>>
>> Regards, Tom
>>
>
>

--
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From: Ben Santer <santer1@llnl.gov>
To: John Lanzante <John.Lanzante@noaa.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>,
carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J.
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Subject: More significance testing
Date: Thu, 27 Dec 2007 16:26:19 -0800
Reply-to: santer1@llnl.gov

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Dear folks,

This email briefly summarizes the trend significance test results. As I mentioned in yesterday's email, I've added a new case (referred to as "TYPE3" below). I've also added results for tests with a stipulated 10% significance level. Here is the explanation of the four different types of trend test:

1. "OBS-vs-MODEL": Observed MSU trends in RSS and UAH are tested against trends in synthetic MSU data in 49 realizations of the 20c3m experiment. Results from RSS and UAH are pooled, yielding a total of 98 tests for T2 trends and 98 tests for T2LT trends.

2. "MODEL-vs-MODEL (TYPE1)": Involves model data only. Trend in synthetic MSU data in each of 49 20c3m realizations is tested against each trend in the remaining 48 realizations (i.e., no trend tests involving identical data). Yields a total of 49 x 48 = 2352 tests. The significance of trend differences is a function of BOTH inter-model differences (in climate sensitivity, applied 20c3m forcings, and the amplitude of variability) AND "within-model" effects (i.e., is related to the different manifestations of natural internal variability superimposed on the underlying forced response).

3. "MODEL-vs-MODEL (TYPE2)": Involves model data only. Limited to the M models with multiple realizations of the 20c3m experiment. For each of these M models, the number of unique combinations C of N 20c3m realizations into R trend pairs is determined. For example, in the case of N = 5, C = N! / [R!(N-R)!] = 10. The significance of trend differences is solely a function of "within-model" effects (i.e., is related to the different manifestations of natural internal variability superimposed on the underlying forced response). There are a total of 62 tests (not 124, as I erroneously reported yesterday!)

4. "MODEL-vs-MODEL (TYPE3)": Involves model data only. For each of the 19 models, only the first 20c3m realization is used. The trend in each model's first 20c3m realization is tested against each trend in the first 20c3m realization of the remaining 18 models. Yields a total of 19 x 18 = 342 tests. The significance of trend differences is solely a function of inter-model differences (in climate sensitivity, applied 20c3m forcings, and the amplitude of variability).

REJECTION RATES FOR STIPULATED 5% SIGNIFICANCE LEVEL

Test type	No. of tests	T2 "Hits"	T2LT "Hits"
1. OBS-vs-MODEL	49 x 2 (98)	2 (2.04%)	1 (1.02%)
2. MODEL-vs-MODEL (TYPE1)	49 x 48 (2352)	58 (2.47%)	32 (1.36%)
3. MODEL-vs-MODEL (TYPE2)	--- (62)	0 (0.00%)	0 (0.00%)
4. MODEL-vs-MODEL (TYPE3)	19 x 18 (342)	22 (6.43%)	14 (4.09%)

REJECTION RATES FOR STIPULATED 10% SIGNIFICANCE LEVEL

Test type	No. of tests	T2 "Hits"	T2LT "Hits"
1. OBS-vs-MODEL	49 x 2 (98)	4 (4.08%)	2 (2.04%)
2. MODEL-vs-MODEL (TYPE1)	49 x 48 (2352)	80 (3.40%)	46 (1.96%)

3. MODEL-vs-MODEL (TYPE2)	---	(62)	1	(1.61%)	0	(0.00%)
4. MODEL-vs-MODEL (TYPE3)	19 x 18	(342)	28	(8.19%)	20	(5.85%)

REJECTION RATES FOR STIPULATED 20% SIGNIFICANCE LEVEL

Test type	No. of tests		T2 "Hits"		T2LT "Hits"	
1. OBS-vs-MODEL	49 x 2	(98)	7	(7.14%)	5	(5.10%)
2. MODEL-vs-MODEL (TYPE1)	49 x 48	(2352)	176	(7.48%)	100	(4.25%)
3. MODEL-vs-MODEL (TYPE2)	---	(62)	4	(6.45%)	3	(4.84%)
4. MODEL-vs-MODEL (TYPE3)	19 x 18	(342)	42	(12.28%)	28	(8.19%)

Features of interest:

A) As you might expect, for each of the three significance levels, TYPE3 tests yield the highest rejection rates of the null hypothesis of "No significant difference in trend". TYPE2 tests yield the lowest rejection rates. This is simply telling us that the inter-model differences in trends tend to be larger than the "between-realization" differences in trends in any individual model.

B) Rejection rates for the model-versus-observed trend tests are consistently LOWER than for the model-versus-model (TYPE3) tests. On average, therefore, the tropospheric trend differences between the observational datasets used here (RSS and UAH) and the synthetic MSU temperatures calculated from 19 CMIP-3 models are actually LESS SIGNIFICANT than the inter-model trend differences arising from differences in sensitivity, 20c3m forcings, and levels of variability.

I also thought that it would be fun to use the model data to explore the implications of Douglass et al.'s flawed statistical procedure. Recall that Douglass et al. compare (in their Table III) the observed T2 and T2LT trends in RSS and UAH with the overall means of the multi-model distributions of T2 and T2LT trends. Their standard error, sigma{SE}, is meant to represent an "estimate of the uncertainty of the mean" (i.e., the mean trend). sigma{SE} is given as:

sigma{SE} = sigma / sqrt{N - 1}

where sigma is the standard deviation of the model trends, and N is "the number of independent models" (22 in their case). Douglass et al. apparently estimate sigma using ensemble-mean trends for each model (if 20c3m ensembles are available).

So what happens if we apply this procedure using model data only? This is rather easy to do. As above (in the TYPE1, TYPE2, and TYPE3 tests), I simply used the synthetic MSU trends from the 19 CMIP-3 models employed in our CCSP Report and in Santer et al. 2005 (so N = 19). For each model, I calculated the ensemble-mean 20c3m trend over 1979 to 1999 (where multiple 20c3m realizations were available). Let's call these mean trends b{j}, where j (the index over models) = 1, 2, .. 19. Further, let's regard b{1} as the surrogate observations, and then use Douglass et al.'s approach to test whether b{1} is significantly different from the overall mean of the remaining 18 members of b{j}. Then repeat with b{2} as surrogate observations, etc. For each layer-averaged temperature series, this yields 19 tests of the significance of differences in mean trends.

To give you a feel for this stuff, I've reproduced below the results for tests involving T2LT trends. The "OBS" column is the ensemble-mean T2LT trend in the surrogate observations. "MODAVE" is the overall mean trend in the 18 remaining members of the distribution, and "SIGMA" is the 1-sigma standard deviation of these trends. "SIGMA{SE}" is 1 x SIGMA{SE} (note that Douglass et al. give 2 x SIGMA{SE} in their Table III; multiplying our SIGMA{SE} results by two gives values similar to

theirs). "NORMD" is simply the normalized difference (OBS-MODAVE) / SIGMA{SE}, and "P-VALUE" is the p-value for the normalized difference, assuming that this difference is approximately normally distributed.

MODEL	"OBS"	MODAVE	SIGMA	SIGMA{SE}	NORMD	P-VALUE
CCSM3.0	0.1580	0.2179	0.0910	0.0215	2.7918	0.0052
GFDL2.0	0.2576	0.2124	0.0915	0.0216	2.0977	0.0359
GFDL2.1	0.3567	0.2069	0.0854	0.0201	7.4404	0.0000
GISS_EH	0.1477	0.2185	0.0906	0.0214	3.3153	0.0009
GISS_ER	0.1938	0.2159	0.0919	0.0217	1.0205	0.3075
MIROC3.2_T42	0.1285	0.2196	0.0897	0.0211	4.3094	0.0000
MIROC3.2_T106	0.2298	0.2139	0.0920	0.0217	0.7305	0.4651
MRI2.3.2a	0.2800	0.2111	0.0907	0.0214	3.2196	0.0013
PCM	0.1496	0.2184	0.0907	0.0214	3.2170	0.0013
HADCM3	0.1936	0.2159	0.0919	0.0217	1.0327	0.3018
HADGEM1	0.3099	0.2095	0.0891	0.0210	4.7784	0.0000
CCCMA3.1	0.4236	0.2032	0.0769	0.0181	12.1591	0.0000
CNRM3.0	0.2409	0.2133	0.0918	0.0216	1.2762	0.2019
CSIRO3.0	0.2780	0.2113	0.0908	0.0214	3.1195	0.0018
ECHAM5	0.1252	0.2197	0.0895	0.0211	4.4815	0.0000
IAP_FGOALS1.0	0.1834	0.2165	0.0917	0.0216	1.5314	0.1257
GISS_AOM	0.1788	0.2168	0.0916	0.0216	1.7579	0.0788
INMCM3.0	0.0197	0.2256	0.0790	0.0186	11.0541	0.0000
IPSL_CM4	0.2258	0.2142	0.0920	0.0217	0.5359	0.5920

T2LT: No. of p-values .le. 0.05: 12. Rejection rate: 63.16%
T2LT: No. of p-values .le. 0.10: 13. Rejection rate: 68.42%
T2LT: No. of p-values .le. 0.20: 14. Rejection rate: 73.68%

The corresponding rejection rates for the tests involving T2 data are:

T2:   No. of p-values .le. 0.05: 12. Rejection rate: 63.16%
T2:   No. of p-values .le. 0.10: 13. Rejection rate: 68.42%
T2:   No. of p-values .le. 0.20: 15. Rejection rate: 78.95%

Bottom line: If we applied Douglass et al.'s ridiculous test of difference in mean trends to model data only - in fact, to virtually the same model data they used in their paper - one would conclude that nearly two-thirds of the individual models had trends that were significantly different from the multi-model mean trend! To follow Douglass et al.'s flawed logic, this would mean that two-thirds of the models really aren't models after all...

Happy New Year to all of you!

With best regards,

Ben

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From: Leopold Haimberger <leopold.haimberger@univie.ac.at>
To: santer1@llnl.gov
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Sat, 29 Dec 2007 22:10:30 +0100
Cc: John.Lanzante@noaa.gov, "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Sherwood Steven <steven.sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, 'Susan Solomon' <susan.solomon@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>

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Ben,

I have attached the tropical mean trend profiles, now for the period 1979-1999.

RAOBCORE versions show much more upper tropospheric heating for this period, RICH shows slightly more heating.
Note also stronger cooling of unadjusted radiosondes in stratospheric layers compared to 1999-2004.

Just for information I have included also zonal mean trend plots for the unadjusted radiosondes (tm), RAOBCORE v1.4 (tmcorr) and RICH (rgmra)
I do not suggest that these plots should be included but some of you maybe want to know about the spatial coherence
of the zonal mean trends. It is interesting to see the lower tropospheric warming minimum in the tropics in all three plots,
which I cannot explain. I believe it is spurious but it is remarkably robust against my adjustment efforts.

Meridional resolution is 10 degrees.
As you can imagine, the tropical upper tropospheric heating maximum at 5S and the cooling in the unadjusted radiosondes at 5N are based on very few long records in these belts. 2-3 in 5S, about 5 in 5N.

Best regards and I wish you all a happy new year.

Leo

Ben Santer wrote:
> Dear Leo,
>
> The Figure that you sent is extremely informative, and would be great
> to include in a response to Douglass et al. The Figure clearly

> illustrates that the "structural uncertainties" inherent in
> radiosonde-based estimates of tropospheric temperature change are much
> larger than Douglass et al. have claimed. This is an important point
> to make.
>
> Would it be possible to produce a version of this Figure showing
> results for the period 1979 to 1999 (the period that I've used for
> testing the significance of model-versus-observed trend differences)
> instead of 1979 to 2004?
>
> With best regards, and frohes Neues Jahr!
>
> Ben
> Leopold Haimberger wrote:
>> Dear all,
>>
>> I have attached a plot which summarizes the recent developments
>> concerning tropical radiosonde temperature datasets and which could
>> be a candidate to be included in a reply to Douglass et al.
>> It contains trend profiles from unadjusted radiosondes,
>> HadAT2-adjusted radiosondes, RAOBCORE (versions 1.2-1.4) adjusted
>> radiosondes
>> and from radiosondes adjusted with a neighbor composite method (RICH)
>> that uses the break dates detected with RAOBCORE (v1.4) as metadata.
>> RAOBCORE v1.2,v1.3 are documented in Haimberger (2007), RAOBCORE v1.4
>> and RICH are discussed in the manuscript I mentioned in my previous
>> email.
>> Latitude range is 20S-20N, only time series with less than 24 months
>> of missing data are included. Spatial sampling of all curves is the
>> same except HadAT which contains less stations that meet the 24month
>> criterion. Sampling uncertainty of the trend curves is ca.
>> +/-0.1K/decade (95% percentiles estimated with bootstrap method).
>>
>> RAOBCORE v1.3,1.4 and RICH are results from ongoing research and
>> warming trends from radiosondes may still be underestimated.
>> The upper tropospheric warming maxima from RICH are even larger (up
>> to 0.35K/decade, not shown), if only radiosondes within the tropics
>> (20N-20S) are allowed as reference for adjustment of tropical
>> radiosonde temperatures. The pink/blue curves in the attached plot
>> should therefore not be regarded as upper bound of what may be
>> achieved with plausible choices of reference series for homogenization.
>> Please let me know your comments.
>>
>> I wish you a merry Christmas.
>>
>> With best regards
>>
>> Leo
>>
>> John Lanzante wrote:
>>> Ben,
>>>
>>> Perhaps a resampling test would be appropriate. The tests you have
>>> performed
>>> consist of pairing an observed time series (UAH or RSS MSU) with
>>> each one
>>> of 49 GCM times series from your "ensemble of opportunity".
>>> Significance
>>> of the difference between each pair of obs/GCM trends yields a certain
>>> number of "hits".
>>>
>>> To determine a baseline for judging how likely it would be to obtain
>>> the

>>> given number of hits one could perform a set of resampling trials by
>>> treating one of the ensemble members as a surrogate observation. For
>>> each
>>> trial, select at random one of the 49 GCM members to be the
>>> "observation".
>>> From the remaining 48 members draw a bootstrap sample of 49, and
>>> perform
>>> 49 tests, yielding a certain number of "hits". Repeat this many
>>> times to
>>> generate a distribution of "hits".
>>>
>>> The actual number of hits, based on the real observations could then be
>>> referenced to the Monte Carlo distribution to yield a probability
>>> that this
>>> could have occurred by chance. The basic idea is to see if the observed
>>> trend is inconsistent with the GCM ensemble of trends.
>>>
>>> There are a couple of additional tweaks that could be applied to
>>> your method.
>>> You are currently computing trends for each of the two time series
>>> in the
>>> pair and assessing the significance of their differences. Why not first
>>> create a difference time series and assess the significance of it's
>>> trend?
>>> The advantage of this is that you would reduce somewhat the
>>> autocorrelation
>>> in the time series and hence the effect of the "degrees of freedom"
>>> adjustment. Since the GCM runs are based on coupled model runs this
>>> differencing would help remove the common externally forced
>>> variability,
>>> but not internally forced variability, so the adjustment would still be
>>> needed.
>>>
>>> Another tweak would be to alter the significance level used to assess
>>> differences in trends. Currently you are using the 5% level, which
>>> yields
>>> only a small number of hits. If you made this less stringent you
>>> would get
>>> potentially more weaker hits. But it would all come out in the wash
>>> so to
>>> speak since the number of hits in the Monte Carlo simulations would
>>> increase
>>> as well. I suspect that increasing the number of expected hits would
>>> make the
>>> whole procedure more powerful/efficient in a statistical sense since
>>> you
>>> would no longer be dealing with a "rare event". In the current
>>> scheme, using
>>> a 5% level with 49 pairings you have an expected hit rate of 0.05 X
>>> 49 = 2.45.
>>> For example, if instead you used a 20% significance level you would
>>> have an
>>> expected hit rate of 0.20 X 49 = 9.8.
>>>
>>> I hope this helps.
>>>
>>> On an unrelated matter, I'm wondering a bit about the different
>>> versions of
>>> Leo's new radiosonde dataset (RAOBCORE). I was surprised to see that
>>> the
>>> latest version has considerably more tropospheric warming than I
>>> recalled
>>> from an earlier version that was written up in JCLI in 2007. I have a

>>> couple of questions that I'd like to ask Leo. One concern is that if
>>> we use
>>> the latest version of RAOBCORE is there a paper that we can
>>> reference --
>>> if this is not in a peer-reviewed journal is there a paper in
>>> submission?
>>> The other question is: could you briefly comment on the differences
>>> in methodology used to generate the latest version of RAOBCORE as
>>> compared to the version used in JCLI 2007, and what/when/where did
>>> changes occur to
>>> yield a stronger warming trend?
>>>
>>> Best regards,
>>>
>>> _____John
>>>
>>>
>>>
>>> On Saturday 15 December 2007 12:21 pm, Thomas.R.Karl wrote:
>>>
>>>> Thanks Ben,
>>>>
>>>> You have the makings of a nice article.
>>>>
>>>> I note that we would expect to 10 cases that are significantly
>>>> different by chance (based on the 196 tests at the .05 sig level).
>>>> You found 3. With appropriately corrected Leopold I suspect you
>>>> will find there is indeed stat sig. similar trends incl.
>>>> amplification. Setting up the statistical testing should be
>>>> interesting with this many combinations.
>>>>
>>>> Regards, Tom
>>>>
>>>
>>>
>>
>
>

--
Ao. Univ. Prof. Dr. Leopold Haimberger
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From: Susan Solomon <Susan.Solomon@noaa.gov>
To: Tom Wigley <wigley@ucar.edu>, "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: Douglass et al. paper
Date: Sun, 30 Dec 2007 10:18:04 -0700
Cc: John.Lanzante@noaa.gov, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, santer1@llnl.gov, Sherwood Steven <steven.sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>, myles <m.allen1@physics.ox.ac.uk>, Bill Fulkerson <wfulk@utk.edu>

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Dear All,

Thanks very much for the helpful discussion on these issues.

I write to make a point that may not be well recognized regarding the character of the temperature trends in the lowermost stratosphere/upper troposphere. I have already discussed this with Ben but want to share with others since I believe it is relevant to this controversy at least at some altitudes. The question I want to raise is not related to the very important dialogue on how to handle the errors and the statistics, but rather how to think about the models.

The attached paper by Forster et al. appeared recently in GRL. It taught me something I didn't realize, namely that ozone losses and accompanying temperature trends at higher altitudes can strongly affect lower altitudes, through the influence of downwelling longwave. There is now much evidence that ozone has decreased significantly in the tropics near 70 mbar. What we show in the attached paper by Forster et al is that ozone depletion near 70 mbar affects temperatures not only at that level, but also down to lower altitudes. I think this is bound to be important to the tropical temperature trends at least in the 100-50 mbar height range, possibly lower down as well, depending upon the degree to which there is a 'substratosphere' that is more radiatively influenced than the rest of the troposphere. Whether it can have an influence as low as 200 mbar - I don't know. But note that having an influence could mean reducing the warming there, not necessarily flipping it over to a net cooling. This 'long-distance' physics, whereby ozone depletion and associated cooling up high can affect the thermal structure lower down, is not a point I had understood despite many years of studying the problem so I thought it

worthwhile to point it out to you here. It has often been said (I probably said it myself five years ago) that ozone losses and associated cooling can't happen or aren't important in this region - but that is wrong.

Further, the fundamental point made in the paper of Thompson and Solomon a few years back remains worth noting, and is, I believe, now resolved in the more recent Forster et al paper: that the broad structure of the temperature trends, with quite large cooing in the lowermost stratosphere in the tropics, comparable to that seen at higher latitudes, is a feature NOT explained by e.g. CO2 cooling, but now can be explained by the observed ozone losses. Exactly how big the tropical cooling is, and exactly how low down it goes, remains open to quantitative question and improvement of radiosonde datasets. But I believe the fundamental point we made in 2005 remains true: the temperature trends in the lower stratosphere in the tropics are, even with corrections, quite comparable to that seen at other latitudes. We can now say it is surely linked to the now-well-observed trends in ozone there. The new paper further shows that you don't have to have ozone trends at 100 mbar to have a cooling there, due to down-welling longwave, possibly lower down still. Whether enhanced upwelling is a factor is a central question.

No global general circulation model can possibly be expected to simulate this correctly unless it has interactive ozone, or prescribes an observed tropical ozone trend. The AR4 models did not include this, and any 'discrepancies' are not relevant at all to the issue of the fidelity of those models for global warming. So in closing let me just say that just how low down this effect goes needs more study, but that it does happen and is relevant to the key problem of tropical temperature trends is one that I hope this email has clarified.

Happy new year,
Susan

At 6:13 PM -0700 12/29/07, Tom Wigley wrote:
>Tom,
>
>Yes -- I had this in an earlier version, but I did not want to
>overwhelm people with the myriad errors in the D et al. paper.
>
>I liked the attached item -- also in an earlier version.
>
>Tom.
>
>++++++++++++
>
>Thomas.R.Karl wrote:
>

>>Tom,
>>
>>This is a very nice set of slides clearly
>>showing the problem with the Douglass et al
>>paper. One other aspect of this issue that
>>John L has mentioned and we discussed when we
>>were doing SAP 1.1 relates to difference
>>series. I am not sure whether Ben was
>>calculating the significance of the difference
>>series between sets of observations and model
>>simulations (annually). This would help offset
>>the effects of El-Nino and Volcanoes on the
>>trends.
>>
>>Tom K.
>>
>>Tom Wigley said the following on 12/29/2007 1:05 PM:
>>
>>>Dear all,
>>>
>>>I was recently at a meeting in Rome where Fred Singer was a participant.
>>>He was not on the speaker list, but, in
>>>advance of the meeting, I had thought
>>>he might raise the issue of the Douglass et
>>>al. paper. I therefore prepared the
>>>attached power point -- modified slightly since returning from Rome. As it
>>>happened, Singer did not raise the Douglass et al. issue, so I did not use
>>>the ppt. Still, it may be useful for members
>>>of this group so I am sending it
>>>to you all.
>>>
>>>Please keep this in confidence. I do not want
>>>it to get back to Singer or any
>>>of the Douglass et al. co-authors -- at least
>>>not at this stage while Ben is still
>>>working on a paper to rebut the Douglass et al. claims.
>>>
>>>On slide 6 I have attributed the die tossing
>>>argument to Carl Mears -- but, in
>>>looking back at my emails I can't find the
>>>original. If I've got this attribution
>>>wrong, please let me know.
>>>
>>>Other comments are welcome. Mike MacCracken and Ben helped in putting
>>>this together -- thanks to both.
>>>
>>>Tom.
>>>
>>>++
>>
>>
>>--
>>
>>*Dr. Thomas R. Karl, L.H.D.*
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