

CRU CORRESPONDENCE 2009

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From: Tim Johns <tim.johns@metoffice.gov.uk>
To: "Folland, Chris" <chris.folland@metoffice.gov.uk>
Subject: Re: FW: Temperatures in 2009
Date: Mon, 05 Jan 2009 09:34:49 +0000
Cc: "Smith, Doug" <doug.smith@metoffice.gov.uk>, p.jones@uea.ac.uk, Tim Johns <tim.johns@metoffice.gov.uk>

Dear Chris, cc: Doug

Mike McCracken makes a fair point. I am no expert on the observational uncertainties in tropospheric SO2 emissions over the recent past, but it is certainly the case that the SRES A1B scenario (for instance) as seen by different integrated assessment models shows a range of possibilities. In fact this has been an issue for us in the ENSEMBLES project, since we have been running models with a new mitigation/stabilization scenario "E1" (that has large emissions reductions relative to an A1B baseline, generated using the IMAGE IAM) and comparing it with A1B (the AR4 marker version, generated by a different IAM). The latter has a possibly unrealistic secondary SO2 emissions peak in the early 21st C - not present in the IMAGE E1 scenario, which has a steady decline in SO2 emissions from 2000. The A1B scenario as generated with IMAGE also show a decline rather than the secondary emissions peak, but I can't say for sure which is most likely to be "realistic".

The impact of the two alternative SO2 emissions trajectories is quite marked though in terms of global temperature response in the first few decades of the 21st C (at least in our HadGEM2-AO simulations, reflecting actual aerosol forcings in that model plus some divergence in GHG forcing). Ironically, the E1-IMAGE scenario runs, although much cooler in the long term of course, are considerably warmer than A1B-AR4 for several decades! Also - relevant to your statement - A1B-AR4 runs show potential for a distinct lack of warming in the early 21st C, which I'm sure skeptics would love to see replicated in the real world... (See the attached plot for illustration but please don't circulate this any further as these are results in progress, not yet shared with other ENSEMBLES partners let alone published). We think the different short term warming responses are largely attributable to the different SO2 emissions trajectories.

So far we've run two realisations of both the E1-IMAGE and A1B-AR4 scenarios with HadGEM2-AO, and other partners in ENSEMBLES are doing similar runs using other GCMs. Results will start to be analysed in a multi-model way in the next few months. CMIP5 (AR5) prescribes similar kinds of experiments, but the implementation details might well be different from ENSEMBLES experiments wrt scenarios and their SO2 emissions trajectories (I haven't studied the CMIP5 experiment fine print to that extent).

Cheers,
Tim

On Sat, 2009-01-03 at 21:31 +0000, Folland, Chris wrote:
> Tim and Doug
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> Please see McCrackens email.

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> Any guidance welcome

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> Chris

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> Prof. Chris Folland

> Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

>

> Met Office Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United Kingdom

> Email: chris.folland@metoffice.gov.uk

> Tel: +44 (0)1647 432978

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> Fellow of the Met Office

> Hon. Professor of School of Environmental Sciences, University of East Anglia

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>

> -----Original Message-----

> From: Mike MacCracken [<mailto:mmacrac@comcast.net>]

> Sent: 03 January 2009 16:44

> To: Phil Jones; Folland, Chris

> Cc: John Holdren; Rosina Bierbaum

> Subject: Temperatures in 2009

>

> Dear Phil and Chris--

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> Your prediction for 2009 is very interesting (see note below for notice that went around to email list for a lot of US Congressional staff)--and I would expect the analysis you have done is correct. But, I have one nagging question, and that is how much SO₂/sulfate is being generated by the rising emissions from China and India (I know that at least some plants are using desulfurization--but that antidotes are not an inventory). I worry that what the western nations did in the mid 20th century is going to be what the eastern nations do in the next few decades--go to tall stacks so that, for the near-term, "dilution is the solution to pollution". While I understand there are efforts to get much better inventories of CO₂ emissions from these nations, when I asked a US EPA representative if their efforts were going to also inventory SO₂ emissions (amount and height of emission), I was told they were not. So, it seems, the scientific uncertainty generated by not having good data from the mid-20th century is going to be repeated in the early 21st century (satellites may help on optical depth, but it would really help to know what is being emitted).

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> That there is a large potential for a cooling influence is sort of evident in the IPCC figure about the present sulfate distribution--most is right over China, for example, suggesting that the emissions are near the surface--something also that is,

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so to speak, 'clear' from the very poor visibility and air quality in China and India. So, the quick, fast, cheap fix is to put the SO₂ out through tall stacks. The cooling potential also seems quite large as the plume would go out over the ocean with its low albedo--and right where a lot of water vapor is evaporated, so maybe one pulls down the water vapor feedback a little and this amplifies the sulfate cooling influence.

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> We all, and you all in particular, need to be prepared.

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> For additional information see:
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> <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
> <http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230.html>

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Tim Johns Manager Global Coupled Modelling
Met Office Hadley Centre

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Please note I work part time, normally Monday-Tuesday Thursday-Friday

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I seem to be getting an email a week from skeptics saying where's the warming gone. I know the warming is on the decadal scale, but it would be nice to wear their smug grins away.

Chris - I presume the Met Office continually monitor the weather forecasts. Maybe because I'm in my 50s, but the language used in the forecasts seems a bit over the top re the cold. where I've been for the last 20 days (in Norfolk)
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LWC. I've just submitted a paper on the UHI for London - it is 1.6 deg C for the countryside 5-6 deg C cooler than city centres on recent nights. The paper shows the UHI hasn't got any worse since 1901 (based on St James Park and Rothamsted).

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[4]<http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-set-to-be-colder-than-in-Iceland.html>

> [5]<http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
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References

1. <http://www.metoffice.gov.uk/>
2. <mailto:mmaccrac@comcast.net>
3. <http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>
4. <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-set-to-be-colder-than-in-Iceland.html>
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To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: FW: Temperatures in 2009
Date: Tue, 6 Jan 2009 10:04:57 -0000

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Phil

Maybe in your conclusions you should comment on the fact that some more general studies show relationships between the population or size of cities and the urban effect. This seems not to be true here. Is there any evidence from other studies of a "saturation effect" on urban warming in some cases? And why this might be so?

Chris

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Sent: 05 January 2009 17:02

To: Folland, Chris

Subject: RE: FW: Temperatures in 2009

Chris,

will look at later. Here is the UHI paper I submitted today to weather. Didn't take long to do. I started doing it as people kept on saying the UHI in London (and this is only Central London) was getting worse. I couldn't see it and Rothamsted and Wisley confirmed what I'd thought.

Any comments appreciated. Remember it is just weather, and I tried to make it quite simple ! David did see it last month.

Cheers
Phil

At 16:46 05/01/2009, you wrote:

>Phil

>

>Strictly very much in confidence, this was submitted to Nature

>Geosciences just before Xmas after discussion with them.

>

>Night-time temperatures seem to have been rather underestimated here as
>well since the cold spell started. Daytime forecasts have been better,
>allowing for 1000 feet of elevation. Real cold would shock all under 30!

>

>Chris

>

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> > are in remarkable harmony about the temperature of 2009.
> > >
> > > Any guidance welcome
> > >
> > > Chris
> > >
> > >
> > > Prof. Chris Folland
> > > Research Fellow, Seasonal to Decadal Forecasting (from 2 June
> > > 2008)

mail.2009

> > >
> > > Met Office Hadley Centre, Fitzroy Rd, Exeter,
> > > Devon EX1 3PB United Kingdom
> > > Email: chris.folland@metoffice.gov.uk
> > > Tel: +44 (0)1647 432978
> > > Fax: (in UK) 0870 900 5050
> > > (International) +44 (0)113 336 1072)
> > > <<http://www.metoffice.gov.uk>> Fellow of the Met Office Hon.
> > > Professor of School of Environmental
> > > Sciences, University of East Anglia
> > >
> > >
> > >
> > >
> > > -----Original Message-----
> > > From: Mike MacCracken [mailto:mmaccrac@comcast.net]
> > > Sent: 03 January 2009 16:44
> > > To: Phil Jones; Folland, Chris
> > > Cc: John Holdren; Rosina Bierbaum
> > > Subject: Temperatures in 2009
> > >
> > > Dear Phil and Chris--
> > >
> > > Your prediction for 2009 is very interesting
> > > (see note below for notice that went around to email list for a lot
> > > of US Congressional staff)--and I would expect the analysis you have
> > > done is correct. But, I have one nagging question, and that is how
> > > much SO2/sulfate is being generated by the rising emissions from
> > > China and India (I know that at least some plants are using
> > > desulfurization--but that antidotes are not an inventory). I worry
> > > that what the western nations did in the mid 20th century is going
> > > to be what the eastern nations do in the next few decades--go to
> > > tall stacks so that, for the near-term, "dilution is the solution to
> > > pollution". While I understand there are efforts to get much better
> > > inventories of CO2 emissions from these nations, when I asked a US
> > > EPA representative if their efforts were going to also inventory
> > > SO2 emissions (amount and height of emission), I was told they were
> > > not. So, it seems, the scientific uncertainty generated by not
> > > having good data from the mid-20th century is going to be repeated
> > > in the early 21st century (satellites may help on optical depth, but
> > > it would really help to know what is being emitted).
> > >
> > > That there is a large potential for a cooling
> > > influence is sort of evident in the IPCC figure about the present
> > > sulfate distribution--most is right over China, for example,
> > > suggesting that the emissions are near the surface--something also
> > > that is, so to speak, 'clear' from the very poor visibility and air
> > > quality in China and India. So, the quick, fast, cheap fix is to put
> > > the SO2 out through tall stacks. The cooling potential also seems
> > > quite large as the plume would go out over the ocean with its low
> > > albedo--and right where a lot of water vapor is evaporated, so maybe
> > > one pulls down the water vapor feedback a little and this amplifies
> > > the sulfate cooling influence.
> > >
> > >
> > > Now, I am not at all sure that having more
> > > tropospheric sulfate would be a bad idea as it would limit
> > > warming--I even have started suggesting that the least expensive and
> > > quickest geoengineering approach to limit global warming would be to
> > > enhance the sulfate loading--or at the very least we need to
> > > maintain the current sulfate cooling offset while we reduce CO2
> > > emissions (and presumably therefore, SO2 emissions, unless we manage
> > > things) or we will get an extra bump of warming. Sure, a bit more
> > > acid deposition, but it is not harmful over the ocean (so we

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> > only/mainly emit for trajectories heading out over the ocean) and
> > the impacts of deposition may well be less that for global warming
> > (will be a tough comparison, but likely worth looking at). Indeed,
> > rather than go to stratospheric sulfate injections, I am leaning
> > toward tropospheric, but only during periods when trajectories are
> > heading over ocean and material won't get rained out for 10 days or so.
> > > would be an interesting issue to do research on--see what could be done.

> > >
> > > In any case, if the sulfate hypothesis is
> > right, then your prediction of warming might end up being wrong. I
> > think we have been too readily explaining the slow changes over past
> > decade as a result of variability--that explanation is wearing thin.
> > I would just suggest, as a backup to your prediction, that you also
> > do some checking on the sulfate issue, just so you might have a
> > quantified explanation in case the prediction is wrong. Otherwise,
> > the skeptics will be all over us--the world is really cooling, the
> > models are no good, etc.
> > And all this just as the US is about ready to get serious on the issue.

> > >
> > > We all, and you all in particular, need to be prepared.

> > >
> > > Best, Mike MacCracken

> > >
> > > Researchers Say 2009 to Be One of Warmest Years on Record

> > >
> > > On December 30, climate scientists from the
> > UK Met Office and the University of East Anglia projected 2009 will
> > be one of the top five warmest years on record. Average global
> > temperatures for 2009 are predicted to be 0.4°C above the 1961-1990
> > average of 14 ° C. A multiyear forecast using a Met Office climate
> > model indicates a ³rapid return of global temperature to the
> > long-term warming trend,² with an increasing probability of record
> > temperatures after 2009. ³The fact that 2009, like 2008, will not
> > break records does not mean that global warming has gone away . . .
> > . What matters is the underlying rate of warming,² said Dr. Phil
> > Jones, the director of climate research at the University of East
> > Anglia. The presence of La Nina during the last year partially masked this
> > underlying rate.

> > ³Phenomena such as El Nino and La Nina have a significant influence
> > on global surface temperature,² said Dr. Chris Folland of the Met
> > Office Hadley Center.

> > > ³Further warming to record levels is likely
> > once a moderate El Nino develops.² The transition from a La Nina
> > effect to an El Nino one is expected late next year.

> > >
> > > For additional information see:
> > > <http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>

> > >
> > > <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-set>

> > > -t
> > > o-be-c
> > > older-than-in-Iceland.html

> > > <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>

> > > <http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230>.

> > > ht
> > > ml

> > >
> > >

> > >
> > >

> > >
> > >

> > >
> > > Tim Johns Manager Global Coupled Modelling
> > Met Office Hadley Centre
> > FitzRoy Rd Exeter Devon EX1 3PB United Kingdom

mail.2009
> > Tel: +44 (0)1392 886901 Fax: +44 (0)1392 885681
> > E-mail: tim.johns@metoffice.gov.uk http://www.metoffice.gov.uk
> >
> > Please note I work part time, normally Monday-Tuesday
> > Thursday-Friday
> >
> > Met Office climate change predictions can now be viewed on Google
> > Earth http://www.metoffice.gov.uk/research/hadleycentre/google/
> >
> >
> >
> 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
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>
>
>

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From: Stephen H Schneider <shs@stanford.edu>
To: santer1@llnl.gov
Subject: Re: [Fwd: data request]
Date: Tue, 6 Jan 2009 10:50:56 -0800 (PST)
Cc: "David C. Bader" <bader2@llnl.gov>, Bill Goldstein <goldstein3@llnl.gov>, Pat Berge <berge1@llnl.gov>, Cherry Murray <murray38@llnl.gov>, George Miller <milller21@llnl.gov>, Anjuli Bamzai <Anjuli.Bamzai@science.doe.gov>, Tomas Diaz De La Rubia <delarubia@llnl.gov>, Doug Rotman <rotman1@llnl.gov>, Peter Thorne <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "Philip D. Jones" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

"Thanks" Ben for this, hi all and happy new year. I had a similar experience--but not FOIA since we at Climatic Change are a private institution--with Stephen McIntyre demanding that I have the Mann et al cohort publish all their computer codes for papers published in Climatic Change. I put the question to the editorial board who debated it for weeks. The vast majority opinion was that scientists should give enough information on their data sources and methods so others who are

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scientifically capable can do their own brand of replication work, but that this does not extend to personal computer codes with all their undocumented sub routines etc. It would be odious requirement to have scientists document every line of code so outsiders could then just apply them instantly. Not only is this an intellectual property issue, but it would dramatically reduce our productivity since we are not in the business of producing software products for general consumption and have no resources to do so. The NSF, which funded the studies I published, concurred--so that ended that issue with Climatic Change at the time a few years ago.

This continuing pattern of harassment, as Ben rightly puts it in my opinion, in the name of due diligence is in my view an attempt to create a fishing expedition to find minor glitches or unexplained bits of code--which exist in nearly all our kinds of complex work--and then assert that the entire result is thus suspect. Our best way to deal with this issue of replication is to have multiple independent author teams, with their own codes and data sets, publishing independent work on the same topics--like has been done on the "hockey stick". That is how credible scientific replication should proceed.

Let the lawyers figure this out, but be sure that, like Ben is doing now, you disclose the maximum reasonable amount of information so competent scientists can do replication work, but short of publishing undocumented personalized codes etc. The end of the email Ben attached shows their intent--to discredit papers so they have no "evidentiary value in public policy"--what you resort to when you can't win the intellectual battle scientifically at IPCC or NAS.

Good luck with this, and expect more of it as we get closer to international climate policy actions, we are witnessing the "contrarian battle of the bulge" now, and expect that all weapons will be used.

Cheers, Steve

PS Please do not copy or forward this email.

Stephen H. Schneider
Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies,
Professor, Department of Biology and
Senior Fellow, Woods Institute for the Environment
Mailing address:
Yang & Yamazaki Environment & Energy Building - MC 4205
473 Via Ortega
Ph: 650 725 9978
F: 650 725 4387
websites: climatechange.net
patientfromhell.org

----- Original Message -----

From: "Ben Santer" <santer1@llnl.gov>
To: "Peter Thorne" <peter.thorne@metoffice.gov.uk>, "Leopold Haimberger" <leopold.haimberger@univie.ac.at>, "Karl Taylor" <taylor13@llnl.gov>, "Tom Wigley" <wigley@cgd.ucar.edu>, "John Lanzante" <John.Lanzante@noaa.gov>, "Susan Solomon" <ssolomon@frii.com>, "Melissa Free" <Melissa.Free@noaa.gov>, "peter gleckler" <gleckler1@llnl.gov>, "Philip D. Jones" <p.jones@uea.ac.uk>, "Thomas R Karl" <Thomas.R.Karl@noaa.gov>, "Steve Klein" <klein21@mail.llnl.gov>, "carl mears" <mears@remss.com>, "Doug Nychka" <nychka@ucar.edu>, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, "Steven Sherwood" <Steven.Sherwood@yale.edu>, "Frank Wentz" <frank.wentz@remss.com>
Cc: "David C. Bader" <bader2@llnl.gov>, "Bill Goldstein" <goldstein3@llnl.gov>, "Pat Berge" <berge1@llnl.gov>, "Cherry Murray" <murray38@llnl.gov>, "George Miller" <millier21@llnl.gov>, "Anjuli Bamzai" <Anjuli.Bamzai@science.doe.gov>, "Tomas Diaz De La Rubia" <delarubia@llnl.gov>, "Doug Rotman" <rotman1@llnl.gov>
Sent: Tuesday, January 6, 2009 9:23:41 AM GMT -08:00 US/Canada Pacific
Subject: [Fwd: data request]

Dear coauthors of the Santer et al. International Journal of Climatology paper (and other interested parties),

mail.2009

I am forwarding an email I received this morning from a Mr. Geoff Smith.

The email concerns the climate model data used in our recently-published International Journal of Climatology (IJoC) paper. Mr. Smith has requested that I provide him with these climate model datasets. This request has been made to Dr. Anna Palmisano at DOE Headquarters and to Dr. George Miller, the Director of Lawrence Livermore National Laboratory.

I have spent the last two months of my scientific career dealing with multiple requests for these model datasets under the U.S. Freedom of Information Act (FOIA). I have been able to do little or no productive research during this time. This is of deep concern to me.

From the beginning, my position on this matter has been clear and consistent. The primary climate model data used in our IJoC paper are part of the so-called "CMIP-3" (Coupled Model Intercomparison Project) archive at LLNL, and are freely available to any scientific researcher. The primary observational (satellite and radiosonde) datasets used in our IJoC paper are also freely available. The algorithms used for calculating "synthetic" Microwave Sounding Unit (MSU) temperatures from climate model data (to facilitate comparison with actual satellite temperatures) have been documented in several peer-reviewed publications. The bottom line is that any interested scientist has all the scientific information necessary to replicate the calculations performed in our IJoC paper, and to check whether the conclusions reached in that paper were sound.

Neither Mr. Smith nor Mr. Stephen McIntyre (Mr. McIntyre is the initiator of the FOIA requests to the U.S. DOE and NOAA, and the operator of the "ClimateAudit.com" blog) is interested in full replication of our calculations, starting from the primary climate model and observational data. Instead, they are demanding the value-added quantities we have derived from the primary datasets (i.e., the synthetic MSU temperatures).

I would like a clear ruling from DOE lawyers - ideally from both the NNSA and DOE Office of Science branches - on the legality of such data requests. They are troubling, for a number of reasons.

1. In my considered opinion, a very dangerous precedent is set if any derived quantity that we have calculated from primary data is subject to FOIA requests. At LLNL's Program for Climate Model Diagnosis and Intercomparison (PCMDI), we have devoted years of effort to the calculation of derived quantities from climate model output. These derived quantities include synthetic MSU temperatures, ocean heat content changes, and so-called "cloud simulator" products suitable for comparison with actual satellite-based estimates of cloud type, altitude, and frequency. The intellectual investment in such calculations is substantial.

2. Mr. Smith asserts that "there is no valid intellectual property justification for withholding this data". I believe this argument is incorrect. The synthetic MSU temperatures used in our IJoC paper - and the other examples of derived datasets mentioned above - are integral components of both PCMDI's ongoing research, and of proposals we have submitted to funding agencies (DOE, NOAA, and NASA). Can any competitor simply request such datasets via the U.S. FOIA, before we have completed full scientific analysis of these datasets?

3. There is a real danger that such FOIA requests could (and are already) being used as a tool for harassing scientists rather than for valid scientific discovery. Mr. McIntyre's FOIA requests to DOE and NOAA

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are but the latest in a series of such requests. In the past, Mr. McIntyre has targeted scientists at Penn State University, the U.K. Climatic Research Unit, and the National Climatic Data Center in Asheville. Now he is focusing his attention on me. The common denominator is that Mr. McIntyre's attention is directed towards studies claiming to show evidence of large-scale surface warming, and/or a prominent human "fingerprint" in that warming. These serial FOIA requests interfere with our ability to do our job.

Mr. Smith's email mentions the Royal Meteorological Society's data archiving policies (the Royal Meteorological Society are the publishers of the International Journal of Climatology). Recently, Prof. Glenn McGregor (the Chief Editor of the IJoC) provided Mr. McIntyre with the following clarification:

"In response to your question about data policy my position as Chief Editor is that the above paper has been subject to strict peer review, supporting information has been provided by the authors in good faith which is accessible online (attached FYI) and the original data from which temperature trends were calculated are freely available. It is not the policy of the International Journal of Climatology to require that data sets used in analyses be made available as a condition of publication."

As many of you may know, I have decided to publicly release the synthetic MSU temperatures that were the subject of Mr. McIntyre's FOIA request (together with additional synthetic MSU temperatures which were not requested by Mr. McIntyre). These datasets have been through internal review and release procedures, and will be published shortly on PCMDI's website, together with a technical document which describes how synthetic MSU temperatures were calculated. I agreed to this publication process primarily because I want to spend the next few years of my career doing research. I have no desire to be "taken out" as scientist, and to be involved in years of litigation.

The public release of the MSU data used in our IJoC paper may or may not resolve these problems. If Mr. McIntyre's past performance is a guide to the future, further FOIA requests will follow. I would like to know that I have the full support of LLNL management and the U.S. Dept. of Energy in dealing with these unwarranted and intrusive requests.

I do not intend to reply to Mr. Smith's email.

Sincerely,

Ben Santer

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
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Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

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From: "Folland, Chris" <chris.folland@metoffice.gov.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: FW: Temperatures in 2009
Date: Tue, 6 Jan 2009 17:01:37 -0000

Phil

Thanks. Bad news today. Nature Geosciences wont publish this because the Real Climate Blog mentions (more vaguely) the basic content of what we have written. That is indeed the reason Nature Geosciences have given. It seems blogs can now prevent publication! I have suggested to Jeff we try GRL but only after raising this issue with them.

Chris

Prof. Chris Folland
Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

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Fellow of the Met Office
Hon. Professor of School of Environmental Sciences, University of East Anglia

-----Original Message-----

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: 06 January 2009 14:56
To: Folland, Chris
Subject: RE: FW: Temperatures in 2009

Chris,

City population size and urban effects are not related that well. I think a lot depends on where the city is in relation to the sea, large rivers and water bodies as well.

I did try and get population figures for London from various times during the 20th century.

I found these, but the area of London they referred to kept changing. Getting the areas proved more difficult, as I though population density would be better. Those

I could find showed that the area was increasing, so I sort of gave up on it.

Whether London is saturated is not clear. The fact that LWC has a bigger UHI than SJP implies that if you did more development around SJP it could be raised. I doubt though that there will be any development in the Mall and on Horseguards Parade!

The Nature Geosciences paper looks good - so hope it gets reviewed favourably. It will be a useful thing to refer to, but I can't see it cutting any ice with the skeptics.

They think the models are wrong, and can't get to grips with natural variability!

Thanks for the CV. I see I'm on an abstract for the Hawaii meeting! Only noticed as it was the last one on your list.

Cheers

Phil

At 10:04 06/01/2009, you wrote:

>Phil

>

>Maybe in your conclusions you should comment on the fact that some more
>general studies show relationships between the population or size of
>cities and the urban effect. This seems not to be true here. Is there
>any evidence from other studies of a "saturation effect" on urban
>warming in some cases? And why this might be so?

>

>Chris

>

>

>Prof. Chris Folland

>Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

>

>Met Office Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United
>Kingdom

>Email: chris.folland@metoffice.gov.uk

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><<http://www.metoffice.gov.uk>> Fellow of the Met Office Hon. Professor
>of School of Environmental Sciences, University of East Anglia

>

>

>

>

>-----Original Message-----

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

>Sent: 05 January 2009 17:02

>To: Folland, Chris

>Subject: RE: FW: Temperatures in 2009

>

>

> Chris,
> will look at later. Here is the UHI paper I submitted today to weather.
> Didn't take long to do. I started doing it as people kept on saying the UHI
> in London (and this is only Central London) was getting worse. I couldn't
> see it and Rothamsted and Wisley confirmed what I'd thought.

>

> Any comments appreciated. Remember it is just weather,
> and I tried to make it quite simple ! David did see it last month.

>

> Cheers

> Phil

>

>

>At 16:46 05/01/2009, you wrote:

> >Phil

> >

> >Strictly very much in confidence, this was submitted to Nature

> >Geosciences just before Xmas after discussion with them.

> >

> >Night-time temperatures seem to have been rather underestimated here

> >as well since the cold spell started. Daytime forecasts have been

> >better, allowing for 1000 feet of elevation. Real cold would shock all under 30!

> >

> >Chris

> >

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> >
> >Prof. Chris Folland
> >Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)
> >
> >Met Office Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United
> >Kingdom
> >Email: chris.folland@metoffice.gov.uk
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> ><<http://www.metoffice.gov.uk>> Fellow of the Met Office Hon. Professor
> >of School of Environmental Sciences, University of East Anglia
> >
> >
> >
> >
> >-----Original Message-----
> >From: Phil Jones [mailto:p.jones@uea.ac.uk]
> >Sent: 05 January 2009 16:18
> >To: Johns, Tim; Folland, Chris
> >Cc: Smith, Doug; Johns, Tim
> >Subject: Re: FW: Temperatures in 2009
> >
> >
> > Tim, Chris,
> > I hope you're not right about the lack of warming lasting
> > till about 2020. I'd rather hoped to see the earlier Met Office
> > press release with Doug's paper that said something like -
> > half the years to 2014 would exceed the warmest year currently on
> > record, 1998!
> > Still a way to go before 2014.
> >
> > I seem to be getting an email a week from skeptics saying
> > where's the warming gone. I know the warming is on the decadal
> > scale, but it would be nice to wear their smug grins away.
> >
> > Chris - I presume the Met Office continually monitor the weather
> > forecasts.
> > Maybe because I'm in my 50s, but the
> > language used in the forecasts seems
> > a bit over the top re the cold. Where I've been for the last 20
> > days (in Norfolk)
> > it doesn't seem to have been as cold as the forecasts.
> >
> > I've just submitted a paper on the UHI for London - it is 1.6
> > deg C for the LWC.
> > It comes out to 2.6 deg C for night-time minimums. The BBC forecasts has
> > the countryside 5-6 deg C cooler than city centres on recent nights.
> > The paper
> > shows the UHI hasn't got any worse since 1901 (based on St James Park
> > and Rothamsted).
> >
> > Cheers
> > Phil
> >
> >
> >
> >At 09:34 05/01/2009, Tim Johns wrote:
> > >Dear Chris, cc: Doug
> > >
> > >Mike McCracken makes a fair point. I am no expert on the
> > >observational uncertainties in tropospheric SO2 emissions over the
> > >recent past, but it is certainly the case that the SRES A1B

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> > >scenario (for instance) as seen by different integrated assessment
> > >models shows a range of possibilities. In fact this has been an
> > >issue for us in the ENSEMBLES project, since we have been running
> > >models with a new mitigation/stabilization scenario "E1" (that has
> > >large emissions reductions relative to an A1B baseline, generated
> > >using the IMAGE
> > >IAM) and comparing it with A1B (the AR4 marker version, generated
> > >by a different IAM). The latter has a possibly unrealistic
> > >secondary SO2 emissions peak in the early 21st C - not present in
> > >the IMAGE E1 scenario, which has a steady decline in SO2 emissions
> > >from 2000. The A1B scenario as generated with IMAGE also show a
> > >decline rather than the secondary emissions peak, but I can't say
> > >for sure which is most likely to be "realistic".

> > >
> > >The impact of the two alternative SO2 emissions trajectories is
> > >quite marked though in terms of global temperature response in the
> > >first few decades of the 21st C (at least in our HadGEM2-AO
> > >simulations, reflecting actual aerosol forcings in that model plus
> > >some divergence in GHG forcing). Ironically, the E1-IMAGE scenario
> > >runs, although much cooler in the long term of course, are
> > >considerably warmer than
> > >A1B-AR4 for several decades! Also - relevant to your statement -
> > >A1B-AR4 runs show potential for a distinct lack of warming in the
> > >early 21st C, which I'm sure skeptics would love to see replicated
> > >in the real world... (See the attached plot for illustration but
> > >please don't circulate this any further as these are results in
> > >progress, not yet shared with other ENSEMBLES partners let alone
> > >published). We think the different short term warming responses are
> > >largely attributable to the different SO2 emissions trajectories.

> > >
> > >So far we've run two realisations of both the E1-IMAGE and A1B-AR4
> > >scenarios with HadGEM2-AO, and other partners in ENSEMBLES are
> > >doing similar runs using other GCMs. Results will start to be
> > >analysed in a multi-model way in the next few months. CMIP5 (AR5)
> > >prescribes similar kinds of experiments, but the implementation
> > >details might well be different from ENSEMBLES experiments wrt
> > >scenarios and their
> > >SO2 emissions trajectories (I haven't studied the CMIP5 experiment
> > >fine print to that extent).

> > >
> > >Cheers,
> > >Tim

> > >
> > >On Sat, 2009-01-03 at 21:31 +0000, Folland, Chris wrote:
> > > > Tim and Doug

> > > >
> > > > Please see McCrackens email.

> > > >
> > > > We are now using the average of 4 AR4
> > > > scenarios you gave us for GHG + aerosol. What is the situation
> > > > likely to be for AR5 forcing, particularly anthropogenic aerosols.
> > > > Are there any new estimates yet? Particularly, will there be a
> > > > revision in time for the 2010 forecast? We do in the meantime have
> > > > an explanation for the interannual variability of the last decade.
> > > > However this fits well only when an underlying net GHG+aerosol
> > > > warming of 0.15C per decade is fitted in the statistical models.
> > > > In a sense the methods we use would automatically fit to a reduced
> > > > net warming rate so Mike McCracken can be told that. In other
> > > > words the method creates its own transient climate sensitivity for
> > > > recent warming. But the forcing rate underlying the method
> > > > nevertheless perhaps sits a bit uncomfortably with the
> > > > absolute forcing figures we are using from AR4.
> > > > However having said this, interestingly, the statistics and

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> > > DePreSys are in remarkable harmony about the temperature of 2009.

> > > >

> > > Any guidance welcome

> > > >

> > > Chris

> > > >

> > > >

> > > > Prof. Chris Folland

> > > > Research Fellow, Seasonal to Decadal Forecasting (from 2 June

> > > > 2008)

> > > >

> > > > Met Office Hadley Centre, Fitzroy Rd, Exeter,

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> > > > <<http://www.metoffice.gov.uk>> Fellow of the Met Office Hon.

> > > > Professor of School of Environmental

> > > > Sciences, University of East Anglia

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> > > little and this amplifies the sulfate cooling influence.
> > > >
> > > > Now, I am not at all sure that having more
> > > tropospheric sulfate would be a bad idea as it would limit
> > > warming--I even have started suggesting that the least expensive
> > > and quickest geoengineering approach to limit global warming would
> > > be to enhance the sulfate loading--or at the very least we need to
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> > > > would be an interesting issue to do
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> > > past decade as a result of variability--that explanation is wearing thin.
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> > > also do some checking on the sulfate issue, just so you might have
> > > a quantified explanation in case the prediction is wrong.
> > > Otherwise, the Skeptics will be all over us--the world is really
> > > cooling, the models are no good, etc.
> > > And all this just as the US is about ready to get serious on the issue.
> > > >
> > > > we all, and you all in particular, need to be prepared.
> > > >
> > > > Best, Mike MacCracken
> > > >
> > > >
> > > > Researchers Say 2009 to Be One of Warmest Years on Record
> > > >
> > > > On December 30, climate scientists from the
> > > UK Met Office and the University of East Anglia projected 2009
> > > will be one of the top five warmest years on record. Average
> > > global temperatures for 2009 are predicted to be 0.4°C above the
> > > 1961-1990 average of 14 ° C. A multiyear forecast using a Met
> > > Office climate model indicates a ³rapid return of global
> > > temperature to the long-term warming trend,² with an increasing
> > > probability of record temperatures after 2009. ³The fact that
> > > 2009, like 2008, will not break records does not mean that global warming has
> > > gone away . . .
> > > . What matters is the underlying rate of warming,² said Dr. Phil
> > > Jones, the director of climate research at the University of East
> > > Anglia. The presence of La Nina during the
> > > last year partially masked this underlying rate.
> > > ³Phenomena such as El Nino and La Nina have a significant
> > > influence on global surface temperature,² said Dr. Chris Folland
> > > of the Met Office Hadley Center.
> > > ³Further warming to record levels is likely
> > > once a moderate El Nino develops.² The transition from a La Nina
> > > effect to an El Nino one is expected late next year.
> > > >
> > > > For additional information see:
> > > > <http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>
> > > >

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> > > <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-s-et>
> > > et
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> > > > older-than-in-Iceland.html
> > > > <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
> > > > <http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230>.
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> > > Tim Johns Manager Global Coupled Modelling
> > > Met Office Hadley Centre
> > > FitzRoy Rd Exeter Devon EX1 3PB United Kingdom
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> > > Please note I work part time, normally Monday-Tuesday
> > > Thursday-Friday
> > >
> > > Met Office climate change predictions can now be viewed on Google
> > > Earth <http://www.metoffice.gov.uk/research/hadleycentre/google/>

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Folland, Chris" <chris.folland@metoffice.gov.uk>
Subject: RE: FW: Temperatures in 2009
Date: Wed Jan 7 12:51:51 2009

Chris,

Apart from contacting Gavin and Mike Mann (just informing them) you should appeal.

In essence it means that Real Climate is a publication.

If you do go to GRL I wouldn't raise the issue with them. Happy to be a suggested reviewer if you do go to GRL.

Cheers

Phil

Chris,

worth pursuing - even if only GRL.

Possibly worth sending a note to Gavin Schmidt at Real Climate to say what Nature have used as a refusal!

Cheers

Phil

At 17:01 06/01/2009, you wrote:

Phil

Climate
indeed
publication!

Thanks. Bad news today. Nature Geosciences wont publish this because the Real Blog mentions (more vaguely) the basic content of what we have written. That is the reason Nature Geosciences have given. It seems blogs can now prevent

have suggested to Jeff we try GRL but only after raising this issue with them.

Chris

Prof. Chris Folland

Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

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<[1]http://www.metoffice.gov.uk>

Fellow of the Met Office

Hon. Professor of School of Environmental Sciences, University of East Anglia

-----Original Message-----

From: Phil Jones [[2]mailto:p.jones@uea.ac.uk]

Sent: 06 January 2009 14:56

To: Folland, Chris

Subject: RE: FW: Temperatures in 2009

Chris,

water
bodies as well.

City population size and urban effects are not related that well. I think a lot depends on where the city is in relation to the sea, large rivers and

20th
century.

I did try and get population figures for London from various times during the

the
areas proved more difficult, as I though population density would be better.

I found these, but the area of London they referred to kept changing. Getting

Those I
could find showed that the area was increasing, so I sort of gave up on it.

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whether London is saturated is not clear. The fact that LWC has a bigger UHI than SJP implies that if you did more development around SJP it could be raised. I doubt though that there will be any development in the Mall and on Horseguards Parade!

The Nature Geosciences paper looks good - so hope it gets reviewed favourably.

It will be a useful thing to refer to, but I can't see it cutting any ice with the skeptics.

They think the models are wrong, and can't get to grips with natural variability!

Thanks for the CV. I see I'm on an abstract for the Hawaii meeting! Only noticed as

it was the last one on your list.

Cheers

Phil

At 10:04 06/01/2009, you wrote:

>Phil

>

>Maybe in your conclusions you should comment on the fact that some more
>general studies show relationships between the population or size of
>cities and the urban effect. This seems not to be true here. Is there
>any evidence from other studies of a "saturation effect" on urban
>warming in some cases? And why this might be so?

>

>Chris

>

>

>Prof. Chris Folland

>Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

>

>Met Office Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United

>Kingdom

>Email: chris.folland@metoffice.gov.uk

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>of School of Environmental Sciences, University of East Anglia

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>-----Original Message-----

>From: Phil Jones [[4]<mailto:p.jones@uea.ac.uk>]

>Sent: 05 January 2009 17:02

>To: Folland, Chris

>Subject: RE: FW: Temperatures in 2009

>

>

> Chris,

> will look at later. Here is the UHI paper I submitted today to weather.
> Didn't take long to do. I started doing it as people kept on saying the UHI
> in London (and this is only Central London) was getting worse. I couldn't
> see it and Rothamsted and Wisley confirmed what I'd thought.

>

> Any comments appreciated. Remember it is just weather,
> and I tried to make it quite simple ! David did see it last month.

>

> Cheers

> Phil

>

>

mail.2009

>At 16:46 05/01/2009, you wrote:

>>Phil

>>

>>Strictly very much in confidence, this was submitted to Nature

>>Geosciences just before Xmas after discussion with them.

>>

>>Night-time temperatures seem to have been rather underestimated here

>>as well since the cold spell started. Daytime forecasts have been

>>better, allowing for 1000 feet of elevation. Real cold would shock all under

30!

>>

>>Chris

>>

>>

>>Prof. Chris Folland

>>Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

>>

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>>Kingdom

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>>of School of Environmental Sciences, University of East Anglia

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>>-----Original Message-----

>>From: Phil Jones [[6]<mailto:p.jones@uea.ac.uk>]

>>Sent: 05 January 2009 16:18

>>To: Johns, Tim; Folland, Chris

>>Cc: Smith, Doug; Johns, Tim

>>Subject: Re: FW: Temperatures in 2009

>>

>>

>> Tim, Chris,

>> I hope you're not right about the lack of warming lasting

>> till about 2020. I'd rather hoped to see the earlier Met Office

>> press release with Doug's paper that said something like -

>> half the years to 2014 would exceed the warmest year currently on

>> record, 1998!

>> Still a way to go before 2014.

>>

>>

>> I seem to be getting an email a week from skeptics saying

>> where's the warming gone. I know the warming is on the decadal

>> scale, but it would be nice to wear their smug grins away.

>>

>> Chris - I presume the Met Office continually monitor the weather

>> forecasts.

>> Maybe because I'm in my 50s, but the

>> language used in the forecasts seems

>> a bit over the top re the cold. Where I've been for the last 20

>> days (in Norfolk)

>> it doesn't seem to have been as cold as the forecasts.

>>

>> I've just submitted a paper on the UHI for London - it is 1.6

>> deg C for the LWC.

>> It comes out to 2.6 deg C for night-time minimums. The BBC forecasts has

>> the countryside 5-6 deg C cooler than city centres on recent nights.

>> The paper

>> shows the UHI hasn't got any worse since 1901 (based on St James Park

mail.2009

> > and Rothamsted).

> >

> > Cheers

> > Phil

> >

> >

> >

> >At 09:34 05/01/2009, Tim Johns wrote:

> >>Dear Chris, cc: Doug

> >>

> >>Mike McCracken makes a fair point. I am no expert on the

> >>observational uncertainties in tropospheric SO2 emissions over the

> >>recent past, but it is certainly the case that the SRES A1B

> >>scenario (for instance) as seen by different integrated assessment

> >>models shows a range of possibilities. In fact this has been an

> >>issue for us in the ENSEMBLES project, since we have been running

> >>models with a new mitigation/stabilization scenario "E1" (that has

> >>large emissions reductions relative to an A1B baseline, generated

> >>using the IMAGE

> >>IAM) and comparing it with A1B (the AR4 marker version, generated

> >>by a different IAM). The latter has a possibly unrealistic

> >>secondary SO2 emissions peak in the early 21st C - not present in

> >>the IMAGE E1 scenario, which has a steady decline in SO2 emissions

> >>from 2000. The A1B scenario as generated with IMAGE also show a

> >>decline rather than the secondary emissions peak, but I can't say

> >>for sure which is most likely to be "realistic".

> >>

> >>The impact of the two alternative SO2 emissions trajectories is

> >>quite marked though in terms of global temperature response in the

> >>first few decades of the 21st C (at least in our HadGEM2-AO

> >>simulations, reflecting actual aerosol forcings in that model plus

> >>some divergence in GHG forcing). Ironically, the E1-IMAGE scenario

> >>runs, although much cooler in the long term of course, are

> >>considerably warmer than

> >>A1B-AR4 for several decades! Also - relevant to your statement -

> >>A1B-AR4 runs show potential for a distinct lack of warming in the

> >>early 21st C, which I'm sure skeptics would love to see replicated

> >>in the real world... (See the attached plot for illustration but

> >>please don't circulate this any further as these are results in

> >>progress, not yet shared with other ENSEMBLES partners let alone

> >>published). We think the different short term warming responses are

> >>largely attributable to the different SO2 emissions trajectories.

> >>

> >>So far we've run two realisations of both the E1-IMAGE and A1B-AR4

> >>scenarios with HadGEM2-AO, and other partners in ENSEMBLES are

> >>doing similar runs using other GCMs. Results will start to be

> >>analysed in a multi-model way in the next few months. CMIP5 (AR5)

> >>prescribes similar kinds of experiments, but the implementation

> >>details might well be different from ENSEMBLES experiments wrt

> >>scenarios and their

> >>SO2 emissions trajectories (I haven't studied the CMIP5 experiment

> >>fine print to that extent).

> >>

> >>Cheers,

> >>Tim

> >>

> >>On Sat, 2009-01-03 at 21:31 +0000, Folland, Chris wrote:

> >>> Tim and Doug

> >>>

> >>> Please see McCrackens email.

> >>>

> >>> We are now using the average of 4 AR4

> >>> scenarios you gave us for GHG + aerosol. What is the situation

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> > > likely to be for AR5 forcing, particularly anthropogenic aerosols.
> > > Are there any new estimates yet? Particularly, will there be a
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> > > recent warming. But the forcing rate underlying the method
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> absolute forcing figures we are using from AR4.

> > > However having said this, interestingly, the statistics and
> > > DePreSys are in remarkable harmony about the temperature of 2009.

> > > >
> > > > Any guidance welcome

> > > > Chris

> > > > Prof. Chris Folland
> > > > Research Fellow, Seasonal to Decadal Forecasting (from 2 June
> > > > 2008)

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> > > > Professor of School of Environmental
> > > > Sciences, University of East Anglia

> > > > -----Original Message-----

> > > > From: Mike MacCracken [[8]<mailto:mmaccrac@comcast.net>]
> > > > Sent: 03 January 2009 16:44
> > > > To: Phil Jones; Folland, Chris
> > > > Cc: John Holdren; Rosina Bierbaum
> > > > Subject: Temperatures in 2009

> > > > Dear Phil and Chris--

> > > > Your prediction for 2009 is very interesting
> > > (see note below for notice that went around to email list for a
> > > lot of US Congressional staff)--and I would expect the analysis
> > > you have done is correct. But, I have one nagging question, and
> > > that is how much SO2/sulfate is being generated by the rising
> > > emissions from China and India (I know that at least some plants
> > > are using desulfurization--but that antidotes are not an
> > > inventory). I worry that what the western nations did in the mid
> > > 20th century is going to be what the eastern nations do in the
> > > next few decades--go to tall stacks so that, for the near-term,
> > > "dilution is the solution to pollution". While I understand there
> > > are efforts to get much better inventories of CO2 emissions from
> > > these nations, when I asked a US EPA representative if their
> > > efforts were going to also inventory
> > > SO2 emissions (amount and height of emission), I was told they
> > > were not. So, it seems, the scientific uncertainty generated by
> > > not having good data from the mid-20th century is going to be

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> > > repeated in the early 21st century (satellites may help on optical
> > > depth, but it would really help to know what is being emitted).
> > >
> > > > That there is a large potential for a cooling
> > > influence is sort of evident in the IPCC figure about the present
> > > sulfate distribution--most is right over China, for example,
> > > suggesting that the emissions are near the surface--something also
> > > that is, so to speak, 'clear' from the very poor visibility and
> > > air quality in China and India. So, the quick, fast, cheap fix is
> > > to put the SO2 out through tall stacks. The cooling potential also
> > > seems quite large as the plume would go out over the ocean with
> > > its low albedo--and right where a lot of water vapor is
> > > evaporated, so maybe one pulls down the water vapor feedback a
> > > little and this amplifies the sulfate cooling influence.
> > >
> > > > Now, I am not at all sure that having more
> > > tropospheric sulfate would be a bad idea as it would limit
> > > warming--I even have started suggesting that the least expensive
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> > >
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> > >
> > >
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> > > temperature to the long-term warming trend,² with an increasing
> > > probability of record temperatures after 2009. ³The fact that
> > > 2009, like 2008, will not break records does not mean that global warming

has gone
away . . .

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>

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References

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2. <mailto:p.jones@uea.ac.uk>
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13. <http://www.metoffice.gov.uk/>
14. <http://www.metoffice.gov.uk/research/hadleycentre/google/>

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From: Ben Santer <santer1@llnl.gov>
To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>
Subject: Data published
Date: Thu, 15 Jan 2009 19:12:35 -0800
Reply-to: santer1@llnl.gov
Cc: "David C. Bader" <bader2@llnl.gov>, Bill Goldstein <goldstein3@llnl.gov>, Pat Berge <berge1@llnl.gov>, Janet Tulk <tulk1@llnl.gov>, Kathryn Craft Rogers <CraftRogers1@llnl.gov>, George Miller <miller21@llnl.gov>, Tomas Diaz De La Rubia <delarubia@llnl.gov>, Cherry Murray <murray38@llnl.gov>, Doug Rotman <rotman1@llnl.gov>, "Bamzai, Anjali" <Anjali.Bamzai@science.doe.gov>, mann <mann@psu.edu>, Anthony Socci <socci@ametsoc.org>, Bud Ward <wardbud@gmail.com>, "Peter U. Clark" <clarkp@onid.orst.edu>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>,
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Stephen H Schneider <shs@stanford.edu>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Tim Barnett <tbarnett-ul@ucsd.edu>, "Verardo, David J." <dverardo@nsf.gov>, Branko Kosovic <kosovic1@llnl.gov>, Bill Fulkerson <wfulk@utk.edu>, Michael Wehner <mfwehner@lbl.gov>, Hal Graboske <graboske1@llnl.gov>, Tom Guilderson <tguilderson@llnl.gov>, Luca Delle Monache <ldm@llnl.gov>, "Celine J. W. Bonfils" <bonfils2@llnl.gov>, "Dean N. Williams" <williams13@llnl.gov>, Charles Doutriaux <doutriaux1@llnl.gov>, Anne Stark <stark8@llnl.gov>

<x-flowed>

Dear coauthors of the Santer et al. International Journal of Climatology paper (and other interested parties),

I have now publicly released the synthetic MSU tropical lower tropospheric temperatures that were the subject of Mr. Stephen McIntyre's request to the U.S. Dept. of Energy/National Nuclear Security Agency under the U.S. Freedom of Information Act (FOIA). I have also released additional synthetic MSU temperatures which were not requested by Mr. McIntyre. These synthetic MSU datasets are available on PCMDI's publicly-accessible website. The link to the datasets is:

<http://www-pcmdi.llnl.gov/projects/msu/index.php>

Technical information about the synthetic MSU datasets is provided in a document entitled:

"Information regarding synthetic Microwave Sounding Unit (MSU) temperatures calculated from CMIP-3 archive"

The link to the technical document is:

http://www-pcmdi.llnl.gov/projects/msu/MSU_doc.pdf

I hope that these datasets will prove useful for bona fide scientific research, and will be employed for such purposes only.

I am also hopeful that after publication of these datasets, I will be able to return to full-time research, unencumbered by further FOIA requests from Mr. McIntyre. In my opinion, Mr. McIntyre's FOIA requests are for the purpose of harassing Government scientists, and not for the purpose of improving our understanding of the nature and causes of climate change.

I'd like to thank Dave Bader, Bill Goldstein, and Pat Berge for helping me complete the process of reviewing, releasing, and publishing the synthetic MSU datasets and the technical document. And thanks to all of you for your support and encouragement over the past two months. It is deeply appreciated.

With best regards,

Ben

Benjamin D. Santer
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Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

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954. 1233245601.txt

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From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: Good news! Plus less good news
Date: Thu, 29 Jan 2009 11:13:21 -0800
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Phil,

Yeah, I had already seen the stuff from McIntyre. Tom Peterson sent it to me. McIntyre has absolutely no understanding of climate science. He doesn't realize that, as the length of record increases and trend confidence intervals decrease, even trivially small differences between an individual observed trend and the multi-model average trend are judged to be highly significant. These model-versus-observed trend differences are, however, of no practical significance whatsoever - they are well within the structural uncertainties of the observed MSU trends.

It would be great if Francis and Myles got McIntyre's paper for review. Also, I see that McIntyre has put email correspondence with me in the Supporting Information of his paper. What a jerk!

I will write to Keith again. The Symposium wouldn't be the same without him. I think Tom would be quite disappointed.

Have fun in Switzerland!

with best regards,

Ben

P.Jones@uea.ac.uk wrote:

> Ben,
> I'm at an extremes meeting in Riederalp - near Brig. I'm too
> old to go skiing. I'll go up the cable car to see the Aletsch Glacier
> at some point - when the weather is good. Visibility is less than
> 200m at the moment.

>
> It is good news that Rob can come. I'm still working on
> Keith. It might be worth you sending him another email,
> telling him what he'll be missing if he doesn't go. I think
> Sarah will come, but I've not yet been in CRU when she has.

>
> With free wifi in my room, I've just seen that M+M have
> submitted a paper to IJC on your H2 statistic - using more
> years, up to 2007. They have also found your PCMDI data -
> laughing at the directory name - FOIA? Also they make up
> statements saying you've done this following Obama's
> statement about openness in government! Anyway you'll likely
> get this for review, or poor Francis will. Best if both
> Francis and Myles did this. If I get an email from Glenn I'll
> suggest this.

>
> Also I see Pielke Snr has submitted a comment on Sherwood's
> work. He is a prat. He's just had a response to a comment

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> piece that David Parker, Tom Peterson and I wrote on a paper
> they had in 2007. Pielke wouldn't understand independence if it
> hit him in the face. Both papers in JGR online. Not worth you
> reading them unless interested.
>
> Cheers
> Phil
>
>
>

--

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email: santer1@llnl.gov

</x-flowed>

955. 1233249393.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: Good news! Plus less good news
Date: Thu, 29 Jan 2009 12:16:33 -0800
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Phil,

Congratulations on the AGU Fellowship! That's great news. I'm really
delighted. I hope that Mr. Mc "I'm not entirely there in the head" isn't
there to spoil the occasion...

With best regards,

Ben

P.Jones@uea.ac.uk wrote:

> Ben,
> Meant to add - hope you're better! You were missed at
> IDAG. Meeting went well though.
>
> I heard during IDAG that I've been made an AGU Fellow.
> Will likely have to go to Toronto to Spring AGU to collect it.
> I hope I don't see a certain person there!
> Have to get out of a keynote talk I'm due to give in
> Finland the same day!
>
> Cheers
> Phil
>
>

mail.2009

> Ben,
 > I'm at an extremes meeting in Riederalp - near Brig. I'm too
 > old to go skiing. I'll go up the cable car to see the Aletsch Glacier at
 > some point - when the weather is good. Visibility is less than 200m at
 > the moment.
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 > Keith. It might be worth you sending him another email,
 > telling him what he'll be missing if he doesn't go. I think
 > Sarah will come, but I've not yet been in CRU when she has.
 >
 > with free wifi in my room, I've just seen that M+M have
 > submitted a paper to IJC on your H2 statistic - using more
 > years, up to 2007. They have also found your PCMDI data -
 > laughing at the directory name - FOIA? Also they make up
 > statements saying you've done this following Obama's
 > statement about openness in government! Anyway you'll likely
 > get this for review, or poor Francis will. Best if both
 > Francis and Myles did this. If I get an email from Glenn I'll
 > suggest this.
 >
 > Also I see Pielke Snr has submitted a comment on Sherwood's
 > work. He is a prat. He's just had a response to a comment
 > piece that David Parker, Tom Peterson and I wrote on a paper
 > they had in 2007. Pielke wouldn't understand independence if it
 > hit him in the face. Both papers in JGR online. Not worth you
 > reading them unless interested.
 >
 > Cheers
 > Phil
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 >
 >

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 email: santer1@llnl.gov

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 #####

From: Ben Santer <santer1@llnl.gov>
 To: Smithg <smithg49@starhub.net.sg>
 Subject: Re: data request
 Date: Fri, 30 Jan 2009 09:33:53 -0800
 Reply-to: santer1@llnl.gov

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Dear Mr. Smith,

Please do not lecture me on "good science and replicability". Mr. McIntyre had access to all of the primary model and observational data necessary to replicate our results. Full replication of our results would have required Mr. McIntyre to invest time and effort. He was unwilling to do that.

Our results were published in a peer-reviewed publication (the International Journal of Climatology). These results were fully available for "independent testing and replication by others". Indeed, I note that David Douglass et al. performed such independent testing and replication in their 2007 International Journal of Climatology paper.

Douglass et al. used the same primary climate model data that we employed. They did what Mr. McIntyre was unwilling to do - they independently calculated estimates of "synthetic" Microwave Sounding Unit (MSU) temperatures from climate model data. The Douglass et al. "synthetic" MSU temperatures are very similar to our own. The scientific differences between the Douglass et al. and Santer et al. results are primarily related to the different statistical tests that the two groups employed in their comparisons of models and observations. Demonstrably, the Douglass et al. statistical test contains several serious flaws, which led them to reach incorrect inferences regarding the level of agreement between modeled and observed temperature trends.

Mr. McIntyre could easily have examined the appropriateness of the Douglass et al. statistical test and our statistical test with randomly-generated data (as we did in our paper). Mr. McIntyre chose not to do that. He preferred to portray himself as a victim of evil Government-funded scientists. A good conspiracy theory always sells well.

Mr. Smith, you chose to take the extreme step of writing to LLNL and DOE management to complain about my "unresponsiveness" and my failure to provide data to Mr. McIntyre. You made your complaint on the basis of the information available on Mr. McIntyre's blog. You did not understand - and still do not understand - that the primary model data used in our paper have always been freely available to any scientific researcher, and are currently being used by many hundreds of scientists around the world. Any competent climate scientist could perform full replication of our calculation of "synthetic" MSU temperatures - as Douglass et al. have already done.

Your email to George Miller and Anna Palmisano was highly critical of my behavior in this matter. Your criticism was entirely unjustified, and damaging to my professional reputation. I therefore see no point in establishing a dialogue with you. Please do not communicate with me in the future. I do not give you permission to distribute this email or post it on Mr. McIntyre's blog.

Sincerely,

Dr. Ben Santer

Smithg wrote:

> Dear Dr. Santer,

>

> I'm pleased to see that the requested data is now available on line.

> Thank you for your efforts to make these materials available.

>

> My "dog in this fight" is good science and replicability. I note the

> following references:

>

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> The American Physical Society on line statement reads (in part):
>
> "The success and credibility of science are anchored in the willingness
> of scientists to:
>
> 1. Expose their ideas and results to independent testing and
> replication by others. This requires the open exchange of data,
> procedures and materials.
> 2. Abandon or modify previously accepted conclusions when confronted
> with more complete or reliable experimental or observational
> evidence."
>
> Also I note the NAS booklet "On Being a Scientist: Responsible Conduct
> in Research" (2nd edition) states "After publication, scientists expect
> that data and other research materials will be shared with qualified
> colleagues upon request. Indeed, a number of federal agencies, journals,
> and professional societies have established policies requiring the
> sharing of research materials. Sometimes these materials are too
> voluminous, unwieldy, or costly to share freely and quickly. But in
> those fields in which sharing is possible, a scientist who is unwilling
> to share research materials with qualified colleagues runs the risk of
> not being trusted or respected. In a profession where so much depends on
> interpersonal interactions, the professional isolation that can follow a
> loss of trust can damage a scientist's work". I know that the 3rd
> edition is expected soon, but I cannot imagine this position will be
> weakened. Indeed, with electronic storage of data increasing
> dramatically, I expect that most of the exceptions are likely to be
> dropped.
>
> I understand that science is considered by some to be a "blood sport"
> and that there are serious rivalries and disputes. Nevertheless, the
> principles above are vital to the continuation of good science, wherever
> the results may lead.
>
> Again, I thank you for making the data available, and I wish you success
> in your future research.

> Kind regards,

> Geoff Smith

> ----- Original Message -----
> *From:* Smithg <mailto:smithg49@starhub.net.sg>
> *To:* santer1@llnl.gov <mailto:santer1@llnl.gov>
> *Sent:* Tuesday, January 06, 2009 11:23 PM
> *Subject:* data request

> Dear Dr. Santer

> ref: Santer, et. al.
> Consistency of modelled and observed temperature trends in the
> tropical troposphere
> International Journal of Climatology
> Volume 28, Issue 13, Date: 15 November 2008, Pages: 1703-1722

> As a courtesy, I would like to advise you that I have requested data
> to support the above paper (monthly model data (49 series) used for
> statistical analysis in Santer et al 2008 or a link to a URL with a
> file of the data as used in the paper) be made available to me via a
> request to Dr. Anna Palmisano of the DOE, Office of Science, the
> funding agency. This request is for "recorded factual material
> commonly accepted in the scientific community as necessary to
> validate research findings".

mail.2009

>
 > This data is already the subject of an FOIA request, but I have
 > asked Dr. Palisano to obtain and send me the data independently of
 > the outcome of any FOIA review. My reasons are:
 >
 > 1) further analysis of the data is important for public policy
 > 2) there is no valid intellectual property justification for
 > withholding this data
 > 3) the data is readily available as obviously you (Dr. Santer) used
 > the information in preparing the recently published paper
 >
 > My request has been copied to Dr. George Miller.
 >
 > Since I have not asked you directly for the data, I now request this
 > data directly from you (monthly model data (49 series) used for
 > statistical analysis in Santer et al 2008 or a link to a URL with a
 > file of the data as used in the paper).
 >
 > Your reported replies to requests of other individuals that the
 > datasets are publicly available is non-responsive to the request.
 >
 > You may be aware that the head of the Royal Meteorological Society
 > (who are the publishers of the above referenced journal) has
 > announced that their data archiving policies will be reviewed at
 > their next general editors meeting. That may change things for the
 > future, but a future change does not have retrospective force.
 > Nevertheless, there is a high probability that requests for this
 > data will continue until provided.
 >
 > In the absence of the requested data, it is very likely this
 > publication will be judged "non-replicable" and therefore of no
 > evidentiary value in public policy.
 >
 > Kind regards,
 >
 > Geoff Smith
 >

--

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 email: santer1@llnl.gov

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 #####

From: Ben Santer <santer1@llnl.gov>
 To: P.Jones@uea.ac.uk
 Subject: Re: [Fwd: data availability]
 Date: Mon, 02 Feb 2009 10:02:55 -0800
 Reply-to: santer1@llnl.gov

mail.2009

<x-flowed>
Dear Phil,

Yes, this is the same Geoff Smith who wrote to me. Do you know who he is? From his comments about the RMS, he seems to be a Brit.

In his email to you, Mr. Smith notes that: "there is a strong case to be made that intermediate results, e.g., collation of such data and the relevant code should be made available in studies such as this one, since there is an important possibility of errors in trying to replicate such a collation".

This is a key point. Douglass et al. already audited our "collation" of the primary temperature data (i.e., our calculation of synthetic MSU temperatures). As I've already told Mr. Smith, Douglass et al. obtained synthetic MSU temperatures very similar to the ones published in our IJoC paper. Mr. Smith does not understand this. Nor does he understand that the algorithms used to calculate synthetic MSU temperatures from raw model temperature data have already been published and documented in the peer-reviewed literature.

I think it would be useful to raise these issues with Paul Hardaker.

Cheers,

Ben

P.Jones@uea.ac.uk wrote:

> Ben,
> Is this the Smith who has emailed? why does he think
> you've not informed your co-authors that you've made the
> data available? Most odd - though he does accept that the
> raw data was already there. Pity that loads of people on
> CA including McIntyre didn't seem to accept or realise this.
> I'm not on an RMS committee at the moment, but I could
> try and contact Paul Hardaker if you think it might be useful.
> Possibly need to explain what is raw and what is intermediate.
>
> I wasn't going to give this guy Smith the satisfaction of a reply!
>
> Cheers
> Phil

> ----- Original Message -----
> Subject: data availability
> From: "Smithg" <smithg49@starhub.net.sg>
> Date: Sun, February 1, 2009 2:09 pm
> To: p.jones@uea.ac.uk

> -----
> Dear Prof. Jones,
>
> ref: Santer, et. al.
> Consistency of modelled and observed temperature trends in the tropical
> troposphere
> International Journal of Climatology
> Volume 28, Issue 13, Date: 15 November 2008, Pages: 1703-1722
>
> As you are a co-author of the referenced paper, you may be interested to
> know of developments (in case you have not heard already).
>
> You will be aware that intermediate data ("monthly model data (49 series)

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> used for statistical analysis in Santer et al 2008 or a link to a URL with
> a file of the data as used in the paper") had been requested from the
> first author, Dr. Santer. A refusal has been posted on line, but in the
> meantime the data is now available at
> [http:// www-pcmdi.llnl.gov/projects/msu/index.php](http://www-pcmdi.llnl.gov/projects/msu/index.php) .

>
> Perhaps you had this data already, but other co-authors have reportedly
> claimed (earlier) they did not have the data. A typical reported response
> to a FOIA request was "I have examined my files and have no monthly time
> series from climate models used in the paper referred to, and no
> correspondence regarding said time series".

>
> No one disputes Dr. Santer's claim that the "primary model data" is
> publicly available, but there is a strong case to be made that
> intermediate results, e.g., collation of such data and the relevant code
> should be made available in studies such as this one, since there is an
> important possibility of errors in trying to replicate such a collation.
> The archiving of such intermediate results is required for econometrics
> journals, among others.

>
> It is further reported on line that the posting of the data was not
> pursuant to an FOIA order, but posted voluntarily (although likely at the
> request of the funding agency, the Department of Energy, Office of
> Science). I hope other scientists will take this type of voluntary action.
> You may have heard that Professor Hardaker, the CEO of the Royal
> Meteorological Society which publishes the International Journal of
> Climatology, has confirmed the issue of data archiving will be on the
> agenda for the next meeting of the Society's Scientific Publishing
> Committee. There is a need for journals as well as funding agencies, and
> publishing scientists themselves, to establish and enforce good data and
> code archiving policies. A more precise definition of "recorded factual
> material commonly accepted in the scientific community as necessary to
> validate research findings" is probably overdue.

>
> I hope the Hadley Centre will take a lead in this issue. From time to time
> I'll look at the progress on archiving, but in the meantime, no reply is
> necessary.

> Kind regards,

> Geoff Smith

>
> -----
> Dear Prof. Jones,

> ref: Santer, et. al.
> Consistency of modelled and observed temperature trends in the tropical
> troposphere
> International Journal of Climatology
> Volume 28, Issue 13, Date: 15 November 2008, Pages: 1703-1722

>
> As you are a co-author of the referenced paper, you may be interested to
> know of developments (in case you have not heard already).

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> You will be aware that intermediate data ("monthly model data (49
> series) used for statistical analysis in Santer et al 2008 or a link to
> a URL with a file of the data as used in the paper") had been requested
> from the first author, Dr. Santer. A refusal has been posted on line,
> but in the meantime the data is now available at
> <http://www-pcmdi.llnl.gov/projects/msu/index.php> .

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> Perhaps you had this data already, but other co-authors have reportedly
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 > response to a FOIA request was "I have examined my files and have no
 > monthly time series from climate models used in the paper referred to,
 > and no correspondence regarding said time series".
 >
 > No one disputes Dr. Santer's claim that the "primary model data" is
 > publicly available, but there is a strong case to be made that
 > intermediate results, e.g., collation of such data and the relevant code
 > should be made available in studies such as this one, since there is an
 > important possibility of errors in trying to replicate such a collation.
 > The archiving of such intermediate results is required for econometrics
 > journals, among others.
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 > Science). I hope other scientists will take this type of voluntary
 > action. You may have heard that Professor Hardaker, the CEO of the Royal
 > Meteorological Society which publishes the International Journal of
 > Climatology, has confirmed the issue of data archiving will be on the
 > agenda for the next meeting of the Society's Scientific Publishing
 > Committee. There is a need for journals as well as funding agencies, and
 > publishing scientists themselves, to establish and enforce good data and
 > code archiving policies. A more precise definition of "recorded factual
 > material commonly accepted in the scientific community as necessary to
 > validate research findings" is probably overdue.
 >
 > I hope the Hadley Centre will take a lead in this issue. From time to
 > time I'll look at the progress on archiving, but in the meantime, no
 > reply is necessary.
 >
 > Kind regards,
 >
 > Geoff Smith

--

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 email: santer1@llnl.gov

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 #####

From: "peter.thorne" <peter.thorne@metoffice.gov.uk>
 To: Phil Jones <p.jones@uea.ac.uk>
 Subject: Re: Visit to Met Office
 Date: Tue, 10 Feb 2009 09:54:16 +0000
 Cc: David Parker <david.parker@metoffice.gov.uk>

Phil, David,

mail.2009

as David says I'll be away in Oklahoma first week in March. Antarctic data first piqued my interest with the Science paper on raobs trends which was clearly non-physical but hard to nail down how wrong it was. I did some minor digging into READER and found that in the UA domain it was qc'ed but not homogenised. I've made a rather rash assumption that this would also be the case for the surface data but am happy to be corrected.

Its clear to me that Antarctica is a uniquely difficult environment to collect long-term homogeneous data in. So I have substantial doubts that all the manned station pegs in Steig et al. are adequate. Does this really matter? I'm not sure.

What Steig et al., satellites, and potentially reanalyses does do is allow us, in principle, at least to get around the no-neighbours issue in assessing homogeneity away from the peninsula.

For example we could use a bootstrapping of the Steig et al approach by creating say 50 realisations of each station series using randomly seeded combinations of manned station pegs as the S et al. RegEM constraint (excluding the candidate station) to make a neighbour composite ensemble. We could then add in the available reanalysis field estimates and satellite estimates and make a reasonable punt about the existence and magnitude of any breaks based upon multiple lines of evidence (of course, we lose some of these before 1979 ...). We could use this information to assess in a more rigorous way than has been done to date the homogeneity of these sparse stations. Then cleaned up data could be fed back through Steig et al. afterwards to see how it impacts that analysis making for a nice clean self-contained study.

My understanding from the blog discussion of Steig et al. is that the analysis step is fairly trivial so such an ensemble realisation approach should be plausible with a humble PC so long as it has the coding platform available.

Of course, this doesn't resolve any fundamental methodological concerns about the S et al. approach that may exist but it does give us a reasonable chance of creating a much more homogeneous READER manned station dataset for next IPCC AR and our future products.

My suspicion is that actually changing the manned station data in this way may make S et al. more different to the straight average of the READER data as used (effectively) in AR5 and point to the importance of the long-term homogeneity of the data pegs in RegEM ... this may, of course, be felt to be a can of worms too far ...

Peter

On Mon, 2009-02-09 at 16:53 +0000, Phil Jones wrote:

> David,
> I think I misinterpreted your email when in Switzerland. I think I thought
> you wanted a talk and a possible project. Now I read it and it is just a
> possible project.
> I've done a lot with the Antarctic temperature data - I also have an
> archive of MSLP data for most sites (for some it is station level pressure).
> With regards homogeneity it is difficult to do much beyond the Peninsula
> (and be confident about anything) as the stations are too far apart. There is
> an issue I could ask Adrian - whether ERA-INTERIM is good enough since
> 1988? This could also assess the AVHRR, but this may be circular.
> I've read Steig et al now, and I can see all the comments on the CA and
> RC sites about some of the data. It seems that BAS have made some mistakes

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> with some of the AWS sites. The only AWS site used in CRUTEM3 is the one
> at Byrd, as this is at one of the manned sites. The issue with the AWS's is
> getting reasonable data in real time. whilst I was away the checked monthly
> data arrived for 2002! I will add Byrd's data in. The problem is
> that some sites
> get buried, but still seem to transmit.
> what Steig et al have done is a paleo-type reconstruction of the
> full field
> from the AVHRR for a recent period and extended it back to 1957. If the
> data are OK, all you're assuming is that covariance structure
> remains the same.

>
> I did this paper (attached) ages ago, but it doesn't seem all
> that relevant.

>
> Anyway - I do need to come down to see Ian. Possibilities would be coming
> mid week, say Feb 25/26 or March 4/5. How do these dates suit? I'd need to
> spend the night - maybe that Travel-lodge near you, it is only one night!

>
> Cheers
> Phil

>
> At 16:04 30/01/2009, David Parker wrote:

> >Phil

> >
> >Thanks. I hope the GCOS meeting goes well: Roger Saunders will be there.
> >We look forward to your thoughts on the Antarctic data, and to your
> >visit whenever that may be convenient for you,

> >
> >David

> >
> >On Fri, 2009-01-30 at 15:56 +0000, P.Jones@uea.ac.uk wrote:

> > > David,

> > > The Swiss extremes workshop has afternoons off for skiing.
> > > As I don't, I've been on 60 or 90 mins walks along snow covered
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> > > > I understand you're busy as I am expecting to attend the Malaria meeting
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> > > > Hope you've had good meetings in Geneva

> > > > David

> > > > --
> > > > David Parker Met Office Hadley Centre FitzRoy Road EXETER EX1 3PB UK
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> > > > >--
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Peter Thorne Climate Research Scientist
Met Office Hadley Centre, FitzRoy Road, Exeter, EX1 3PB
tel. +44 1392 886552 fax +44 1392 885681
www.metoffice.gov.uk/hadobs

959. 1234302123.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: David Parker <david.parker@metoffice.gov.uk>
Subject: Re: Visit to Met Office
Date: Tue Feb 10 16:42:03 2009
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>, "Simpson, Ian.R"
<ian.r.simpson@metoffice.gov.uk>

mail.2009

David, Peter, Ian,

Let's go for the week with Feb 25/26 in it. I could come down for late on the 25th then spend most of the 26th discussing Ian's work and also the Antarctic ideas. Presumably John Prior and others will be available at some point on the 26th.

The Antarctic surface T data that are in CRUTEM3 have come from my searches over the years and also from READER. Much of the early stuff in READER has come from the archives here, except where BAS have got the original digitized data from the Antarctic Institutes in all the countries.

I also have some files of when some of the manned stations on the ice have moved. These are forced moves, as the station moves, but they have never been accounted for. Halley and Casey are affected.

There are issues to discuss about the AWSs and also, as David knows from AOPC, work that Wisconsin are doing in putting together all the historic US series. I've talked to them about this - mainly to try and stop them calculating mean T a different way. If they do this it will screw their series up. It all relates to them saying that the mean of min and max is not a great way in the Antarctic to calculate mean T. They say they can now do the mean of every 3 hours, but it needs the historic series and the routine updating to change at the same time - which is unlikely to happen.

Cheers

Phil

At 18:13 09/02/2009, David Parker wrote:

Phil

Thanks. I think Feb 25-26 is better as Peter, who suggested the Reader-data project, will be away in the first week of March. Ian will be here except, I think, on Feb 27th when he is going to a chess tournament. The hotel next to the Met Office should be OK but I haven't checked availability - that can be done when the date is chosen.

David

On Mon, 2009-02-09 at 16:53 +0000, Phil Jones wrote:

> David,

thought > I think I misinterpreted your email when in Switzerland. I think I

> you wanted a talk and a possible project. Now I read it and it is just a possible project.

pressure) > I've done a lot with the Antarctic temperature data - I also have an > archive of MSLP data for most sites (for some it is station level

is > with regards homogeneity it is difficult to do much beyond the Peninsula > (and be confident about anything) as the stations are too far apart. There

> an issue I could ask Adrian - whether ERA-INTERIM is good enough since > 1988? This could also assess the AVHRR, but this may be circular.

> I've read Steig et al now, and I can see all the comments on the CA and > RC sites about some of the data. It seems that BAS have made some mistakes > with some of the AWS sites. The only AWS site used in CRUTEM3 is the one > at Byrd, as this is at one of the manned sites. The issue with the AWS's is > getting reasonable data in real time. Whilst I was away the checked monthly > data arrived for 2002! I will add Byrd's data in. The problem is

> that some sites

> get buried, but still seem to transmit.

> what Steig et al have done is a paleo-type reconstruction of the > full field

> from the AVHRR for a recent period and extended it back to 1957. If the > data are OK, all you're assuming is that covariance structure > remains the same.

>

> I did this paper (attached) ages ago, but it doesn't seem all > that relevant.

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>
> Anyway - I do need to come down to see Ian. Possibilities would be coming
> mid week, say Feb 25/26 or March 4/5. How do these dates suit? I'd need to
> spend the night - maybe that Travel-lodge near you, it is only one night!
>
> Cheers
> Phil
>
>
> At 16:04 30/01/2009, David Parker wrote:
> >Phil
> >
> >Thanks. I hope the GCOS meeting goes well: Roger Saunders will be there.
> >We look forward to your thoughts on the Antarctic data, and to your
> >visit whenever that may be convenient for you,
> >
> >David
> >
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> > > > at Imperial on 12-13 Feb when you aren't available.

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http:[1]www.metoffice.gov.uk

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References

- 1. <http://www.metoffice.gov.uk/>

960. 1234821995.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>, Jean Jouzel <jean.jouzel@lsce.ipsl.fr>
Subject: Re: EGU2009 - Presentation Selection
Date: Mon Feb 16 17:06:35 2009

mail.2009

Mike,

It would be good to get some fresh blood. Caspar and Pascal would be good choices. Discuss with Jean in Hawaii.

The meeting in Il Ciocco was a very good one - but so was the one in Wengen. It is just a matter of getting the right people and the right venue. The EGU and AGU meetings don't really work.

Cheers

Phil

At 15:41 15/02/2009, Michael Mann wrote:

thanks Jean,
yes, I've heard much about the legendary Il Ciocco meeting, sadly it was before I got into this field. I understand how you might want to discontinue being a co-convenor of this session, since its somewhat disconnected from the recent directions of your research. In fact, perhaps we should consider recruiting entirely new, more junior scientist convenors to take this over. Perhaps e.g. Caspar and Pascal. Phil--interested in your thoughts on this. Jean--looking forward to seeing you in Hawaii!
mike

On Feb 15, 2009, at 6:07 AM, Jean Jouzel wrote:

Dear mike and Phil,
This looks quite good (including poster presentations). I confirm that I will be unable to attend this year (IPCC plenary in Turkey this same week). I hope that it will be better next year. As you can see, I'am less and less involved in studies dealing with the last millenium. Obviously, I have still a lot of interest since the NATO meeting we organized at Il Ciocco with Ray Bradley and Phil about the climate of the 2000 years (and a great pleasure to interact with both of you). But, as far as our session, it may be wise to think of someone more directly invoved for the coming years. You certainly have names in mind and this would be very welcome (one of my suggestion could be Pascal Yiou). I'am sorry not to be with you in Vienna but I will be in Hawaii (Mike I feel that you will be there too).

Cheers Jean

At 9:07 +0000 13/02/09, Phil Jones wrote:

Mike, Jean,
I won't be in Hawaii. I did register, but I've just been travelling too much and have more meetings coming up in late March and April. I've decided not to go to the AGU in Toronto, partly as I couldn't find a replacement for a keynote talk I've been down to give at a meeting in Finland on the same day. Apparently about 5 of the 30 AGU Fellows listed can't make it either. As for the EGU, the session looks good. Pity you have got Friday - numbers will be quite low for the poster session in the late afternoon. The one thing to add in would be Chairpersons for the two oral sessions. I managed to get them in last year, but can't recall how. If I recall correctly Jean said he had an

mail.2009

IPCC

meeting,
so maybe put Gene down as chairing the first morning slot. Nick would be
another

option. Assume you'll do the second morning slot.

Cheers

Phil

At 03:09 13/02/2009, Michael Mann wrote:

Hi Phil, Jean,

I've attached the final version of our session program. They allowed
us a half day or oral sessions (12 15 minute talks, 4 were solicited),
and the rest are in poster.

Please let me know if you see any problems. I think its still possible
to make changes if absolutely necessary.

thanks,

mike

p.s. will I see either of you at the IPCC meeting in Hawaii in March?

On Feb 9, 2009, at 8:12 AM, Phil Jones wrote:

Jean,

I think he is as well.

Cheers

Phil

At 13:07 09/02/2009, Jean Jouzel wrote:

Dear Michael

I think that you rae taking care Cheers Jean

MailScanner-NULL-Check: 1234782259.34667@KQFMks6eL6kkqBwrCA/5pQ

X-Ids: 166

To: [1]jean.jouzel@cea.fr

Subject: EGU2009 - Presentation Selection

Reply-to: [2]egu2009@copernicus.org

From: [3]egu2009@copernicus.org

X-Co-Tag: aa43ed727bfee453a8c3def9b6ff53b8

Date: Mon, 9 Feb 2009 12:04:08 +0100 (CET)

X-Greylist: IP, sender and recipient auto-whitelisted, not delayed

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09 Feb 2009 12:04:16 +0100 (CET)

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j-chkmail dot ensm dot fr)!

X-j-chkmail-Envelope: 49900DAF.00D/132.166.172.107/sainfoin-

out.extra.cea.fr/sainfoin-out.extra.cea.fr/<[4]egu2009@copernicus.org>

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score : . :

R=. U=. O=# B=0.086 -> S=0.108

X-j-chkmail-Status: Ham

X-IPSL-MailScanner: Found to be clean

X-IPSL-SpamCheck: not spam, SpamAssassin (not cached, score=-0.149,
required 5, BAYES_05 -1.11, NO_REAL_NAME 0.96)

X-IPSL-From: [5]egu2009@copernicus.org

Dear Mr Jouzel,

The Programme Group Chairs of the EGU2009 scheduled your following

Session:

CL10

Climate of the last millennium: reconstructions, analyses and

explanation of regional and seasonal changes

Now you are kindly asked to finalize the actual programme of your

Session from 10 Feb 2009 to 14 Feb 2009. Please enter the tool

SOIII - Presentation Selection at

[6]http://meetingorganizer.copernicus.org/EGU2009/sessionmodification/218 by

mail.2009

using your

Copernicus Office User ID 100391.

The following tasks should be taken into account:

- 1) subdivide your Abstracts into Oral and Poster presentations;
- 2) define the sequence and the length of the different Oral presentations;
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Your entries generate the draft programme which will be finally approved by the Programme Group Chairs and published online afterwards. The authors will then receive the Letter of Schedule, informing them about the details of their presentation.

We thank you very much in advance for your cooperation, and please do not hesitate to contact us in case that any questions may arise!

with kind regards,

Katja Gänger

Copernicus Meetings

[7]egu2009@copernicus.org

on behalf of the Programme Committee Chair

--

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Associate Professor

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The Pennsylvania State University

email: [10]mann@psu.edu

University Park, PA 16802-5013

website: [11]<http://www.meteo.psu.edu/~mann/Mann/index.htm>

"Dire Predictions" book site:

[12]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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Page 51

mail.2009

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X-Ids: 166

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Subject: EGU2009 - Presentation Selection

Reply-to: [14]egu2009@copernicus.org

From: [15]egu2009@copernicus.org

X-Co-Tag: aa43ed727bfee453a8c3def9b6ff53b8

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j-chkmil dot ensmp dot fr)!

X-j-chkmil-Envelope:

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egu2009@copernicus.org>

X-j-chkmil-Score: MSGID : 49900DAF.00D on jchkmil.jussieu.fr : j-chkmil

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Copernicus Meetings

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18. http://meetingorganizer.copernicus.org/EGU2009/sessionmodification/218
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21. mailto:p.jones@uea.ac.uk
22. mailto:mann@psu.edu
23. http://www.meteo.psu.edu/~mann/Mann/index.html
24. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
25. mailto:p.jones@uea.ac.uk
26. mailto:jean.jouzel@lsce.ipsl.fr
27. mailto:mann@psu.edu
28. http://www.meteo.psu.edu/~mann/Mann/index.html
29. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

961. 1236358770.txt

#####

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
To: "K.Briffa@uea.ac.uk" <K.Briffa@uea.ac.uk>
Subject: Re: 2k Arctic synthesis
Date: Fri, 6 Mar 2009 11:59:30 -0700

<x-flowed>

mail.2009

Great. I'll play with both the composite series and the three individuals. I was hoping to get some spatially distributed information, so might include all three. I will also subdivide by proxy time and use PCA to examine spatial patterns. I'll take a stab at revising the text to include a few sentences about how we chose the tree-ring series. Then maybe you can take a look on Monday. Have a good weekend. Darrell

On Mar 6, 2009, at 11:54 AM, K.Briffa@uea.ac.uk wrote:

> Darell
> the short answer is yes - you need to give the appropriate weight
> to the
> Eurasian aggregate series though ie this one series should count as
> 3 in
> an average of all high -latitude (e.g. compared to Rosanne D'Arrigo
> west
> N. American series) unless you use the 3 separate
> series(Fennoscandia,Yamal, Taimyr) individually. I would use my single
> average series as is though. while you are doing this work , I
> suggest you
> also produce separate proxy type series (ice, lakes, trees) - for
> explicit
> comparison and perhaps separate half-hemisphere (US side and Eurasian
> side) though not sure if Greenland ice should go in either. Cheers
> Keith

>
>
>
>
> directly> Keith:
>> Thanks for the update. I'd like to revise the composite proxy record
>> over the weekend (my only spare time). Can I assume that I need to
>> omit the three tree-ring series that I took from Mann et al. (2008)
>> because they were not processed to retain the low frequency signal,
>> and that I should replace the Euraisan series with the three from
>> your recent Phil Trans paper (using the data on your website)?
>>
>> If you agree, I can work on revising all of the calculations and
>> figures and we can modify the text early next week.
>>
>> Would that work?
>> Darrell

>>
>> On Mar 6, 2009, at 9:52 AM, Keith Briffa wrote:
>>
>>> Darrell
>>> REALLY sorry - have not done this yet - had back
>>> to back meetings for 2 days and am due to leave
>>> now for the weekend - couple of days away from
>>> computer - my comments are nothing earth
>>> shattering or voluminous but I would still like
>>> to make them for your consideration. I will try
>>> to do this on Monday now - if too late - just ignore me . Sorry
>>> again
>>> Keith
>>>
>>> thanks for your consideration
>>> cheers
>>> Keith
>>>

mail.2009

>>> At 15:01 03/03/2009, you wrote:

>>>> Keith:

>>>> I appreciate your willingness to squeeze this in on such short
>>>> notice. If you could get your comments to me by the end of the
>>>> week,

>>>> that would be more than I had hoped for. Thank you. Darrell

>>>>

>>>>

>>>> On Mar 3, 2009, at 7:56 AM, Keith Briffa wrote:

>>>>

>>>>> Darrell

>>>>> I would like to make some comments but the
>>>>> earliest I can get to this is Thursday (we have
>>>>> visitors here all day tomorrow. In short I would
>>>>> like to be involved - but I would rather wait and
>>>>> see the basis of your reaction to my initial
>>>>> thoughts when I get a Tracked changes version
>>>>> back to you. You are correct that there are
>>>>> clear limitations in the preservation of trend
>>>>> over two millennia in SOME of the data Mann et al
>>>>> used - and in the current series you cite for
>>>>> Yamal (Hantemirov et al) . I do believe that the
>>>>> composite series in our Phil Trans paper is a
>>>>> convenient representation of the circum-western
>>>>> Eurasian Arctic tree-line data - though the Grudd
>>>>> and Nauzbaev papers are virtually similar to our
>>>>> data for their areas. However I have a few
>>>>> reservations/comments on other aspects of the
>>>>> manuscript that I believe any likely referee
>>>>> might pick up on . Is it ok to wait til Thursday
>>>>> or will this not be acceptable for getting
>>>>> comments back? I know how these time lines are crucial. Best
>>>>> wishes

>>>>> Keith

>>>>>

>>>>> At 14:15 02/03/2009, you wrote:

>>>>>> Hello Keith:

>>>>>> Following the recommendations of Malcolm and Phil (via Ray), it's
>>>>>> clear that I should have come to you sooner. I am now well along
>>>>>> on a
>>>>>> manuscript that summarizes 2000-year-long proxy temperature
>>>>>> records
>>>>>> from the Arctic (attached). The impetus for the paper is the new
>>>>>> compilation of high-resolution lake records that my group
>>>>>> recently
>>>>>> published in J Paleolimnology.

>>>>>> On the tree-ring side, it's clear to me now that I should not
>>>>>> have

>>>>>> used the series from the Mann et al. compilation, and I hadn't
>>>>>> see

>>>>>> your 2008 Phil Trans paper until just last week. As far as I can
>>>>>> tell, the only records that meet the criteria for this study are
>>>>>> your

>>>>>> three new RCS series from Eurasia and D'Arrigo's Gulf of Alaska
>>>>>> record. Apparently, none of the Malcolm's series in Mann et al.
>>>>>> were

>>>>>> processed in a way that would preserve the millennial trend, and
>>>>>> these should be omitted from the synthesis.

>>>>>>

>>>>>> I now need to substantially revamp the manuscript. Before I do, I
>>>>>> want to be sure that I get it right this time and hope that you
>>>>>> will

mail.2009

>>>>> be interested in joining as co-author to help guide the tree-ring
>>>>> component of the synthesis. I see that you have posted the Phil
>>>>> Trans
>>>>> data on your website, but would much prefer to have your
>>>>> involvement
>>>>> before using the data.

>>>>> Unfortunately, the timing for submission is an issue. I am
>>>>> leading a
>>>>> 12-PI proposal that is currently pending and would benefit
>>>>> greatly if
>>>>> this paper were accepted for publication.

>>>>> Please have a look at the manuscript, which I realize needs
>>>>> substantial revisions, and let me know if you have time and
>>>>> interest
>>>>> in getting involved.

>>>>> Thanks,
>>>>> Darrell
>>>>> ï¿¼

>>>>> Darrell S. Kaufman
>>>>> Professor of Geology and Environmental Sciences
>>>>> Northern Arizona University
>>>>> 928-523-7192
>>>>> <http://jan.ucc.nau.edu/~dsk5/>

>>>>> Hello Keith:
>>>>> Following the recommendations of Malcolm and
>>>>> Phil (via Ray), it's clear that I should have
>>>>> come to you sooner. I am now well along on a
>>>>> manuscript that summarizes 2000-year-long proxy
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>>>>> The impetus for the paper is the new compilation
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>>>>> recently published in J Paleolimnology.

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>>>>> I can tell, the only records that meet the
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>>>>> Alaska record. Apparently, none of the Malcolm's
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mail.2009

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>>>>>
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>>>>> Thanks,
>>>>> Darrell

>>>>>
>>>>> Darrell S. Kaufman
>>>>> Professor of Geology and Environmental Sciences
>>>>> Northern Arizona University
>>>>> 928-523-7192
>>>>> <<http://jan.ucc.nau.edu/~dsk5/>><http://jan.ucc.nau.edu/~dsk5/>

>>>>> --
>>>>> 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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>>>>>
>>>>> <http://www.cru.uea.ac.uk/cru/people/briffa/>

>>>>>
>>>>>
>>>>>
>>>>>

</x-flowed>

962. 1236958090.txt

#####

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tom Melvin <t.m.melvin@uea.ac.uk>
Subject: Fwd: NERC Consortium Proposal
Date: Fri Mar 13 11:28:10 2009

X-Authentication-warning: ueamailgate01.uea.ac.uk: defang set sender to
<turneychris@gmail.com> using -f
DKIM-Signature: v=1; a=rsa-sha256; c=relaxed/relaxed;
d=gmail.com; s=gamma;

mail.2009

h=domainkey-signature:received:received:message-id:from:to
:content-type:mime-version:subject:date:cc:x-mailer;
bh=vzM4qpeBuZ3NQSbfkIPACp4rqI5xIH9tfl60UhwjxcE=;
b=EAAG1b17JLNg2YRgwSZWUqtdNH6FABtHYku6HP2vIb37BakYy+nAI9oPe2vJmn1vkJ
NNnqybDof85G8yHA50MDK14+vLRSz1w49oSH4z1YMaJmpw74/NwvRwysDSoyithvoaeo
du0IYmPQvWXg+hHATrIfMR3WSPuzT+bsHby1M=

DomainKey-Signature: a=rsa-sha1; c=noFWS;

d=gmail.com; s=gamma;

h=message-id:from:to:content-type:mime-version:subject:date:cc
:x-mailer;

b=vshpN16BnkBlTzIbqZGkikhZRRLDty4h9YDrCcr1ar1UpXQoFm7wGfURAY9lINDGiv
rTtJrNYHwK42PcQotJXHe7XlhwBVuII6hxTU5X811ycdc4IcIXNIYRWDYYJGZMFSHdyj
IJjD59a4V+w1eHp2Kkv9yixdawsBeshQE2gvQ=

From: Chris Turney <turneychris@gmail.com>

To: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, t.osborn@uea.ac.uk

Subject: NERC Consortium Proposal

Date: Mon, 9 Mar 2009 12:42:53 +0100

Cc: Philip Brohan <philip.brohan@metoffice.gov.uk>,
Rob Allan <rob.allan@metoffice.gov.uk>,
Peter Cox <P.M.Cox@exeter.ac.uk>

X-Mailer: Apple Mail (2.930.3)

X-Canit-CHI2: 0.00

X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f023)

X-Spam-Score: 0.00 () [Tag at 5.00] HTML_MESSAGE,SPF(pass,0)

X-CanitPRO-Stream: UEA:f023 (inherits from

UEA:10_Tag_Only,UEA:default,base:default)

X-Canit-Stats-ID: 18712069 - 127314cabecf (trained as not-spam)

X-Antispam-Training-Forget:

[1]<https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=f>

X-Antispam-Training-Nospam:

[2]<https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=n>

X-Antispam-Training-Spam:

[3]<https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=s>

X-Scanned-By: CanIt (www.roaringpenguin.com) on 139.222.131.184

Hi Keith, Phil and Tim,

Please find attached an outline bid for the NERC Consortium bid we discussed at the end of last year. I must apologise for the delay in getting back to you. Exeter has suddenly gone mad with appointments of staff and postgrads. It's all good fun but it's taken up a lot of my time over the past couple of months.

For a NERC Consortium we need to put in a 2 page document as an expression of interest. If approved we can then go forward for submission. The next deadline is 1 July.

Can you have a look at the attached and let me know what you think?

Could you let me know what sort of support you'd need if we go forward. We have up to £3.5 million to spend over 5 years. Included in the document we have to include a summary of the funding we'd like to get from NERC, any other funds we have in support and other benefits e.g. training. For the latter we envisaged approximately 1 postdoc and 2 PhDs per UK institution.

I'm currently in Copenhagen for a meeting so my email contact will be a little erratic but am back Friday.

Hope things are going well.

All the best,

Chris

Professor Chris Turney FRSA FRGS

Author of Ice, Mud and Blood: Lessons from Climates Past

Popular science website:

[4]www.christurney.com

Journal of Quaternary Science Asian and Australasian Regional Editor

School of Geography, Archaeology and Earth Resources

The University of Exeter
Exeter
Devon
EX4 4RJ
UK

Times Higher University of the Year 2007-08
Director of Carbonscape, Fixing carbon the way nature intended
Home page:

[5]www.sogaer.ex.ac.uk/geography/people/staff/c_turney/main.shtml
E-mail: c.turney@exeter.ac.uk
Office Tel.: +44 (0)1392 263331
Fax.: +44 (0)1392 263342

Slartibartfast: Science has achieved some wonderful things of course, but I'd far rather be happy than right any day.

Arthur Dent: And are you?

Slartibartfast: No. Thats where it all falls down of course.

Arthur Dent: Pity. It sounded like quite a good lifestyle otherwise. The Hitchhikers Guide to the Galaxy, Douglas Adams

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Popular science website:

[7]www.christurney.com

[8]Journal of Quaternary Science Asian and Australasian Regional Editor
School of Geography, Archaeology and Earth Resources

The University of Exeter

Exeter
Devon
EX4 4RJ
UK

Times Higher University of the Year 2007-08
Director of [9]Carbonscape, Fixing carbon the way nature intended
Home page:

mail.2009

[10]www.sogaer.ex.ac.uk/geography/people/staff/c_turney/main.shtml
E-mail: [11]c.turney@exeter.ac.uk
Office Tel.: +44 (0)1392 263331
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--

Professor Keith Briffa,
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Phone: +44-1603-593909

Fax: +44-1603-507784

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

References

1. <https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=f>
2. <https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=n>
3. <https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=s>
4. <http://www.christurney.com/>
5. http://www.sogaer.ex.ac.uk/geography/people/staff/c_turney/main.shtml
6. <http://us.macmillan.com/icemudandblood>
7. <http://www.christurney.com/>
8. <http://www.interscience.wiley.com/journal/jqs>
9. <http://www.carbonscape.com/>
10. http://www.sogaer.ex.ac.uk/geography/people/staff/c_turney/main.shtml
11. <mailto:c.turney@exeter.ac.uk>
12. <http://www.cru.uea.ac.uk/cru/people/briffa/>

963. 1236962118.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Tom's Symposium
Date: Fri, 13 Mar 2009 12:35:18 -0700
Reply-to: santer1@llnl.gov
Cc: Phil Jones <p.jones@uea.ac.uk>, Sarah Raper <S.Raper@mmu.ac.uk>

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Dear Keith,

I'm very sorry to hear that both you and Sarah have not been well. I hope that both of you are feeling better soon. While I understand your decision, it's very sad that you won't be there on June 19th. I was really looking forward to a reunion of the "CRU gang". Despite its relatively small size, CRU has had (and continues to have!) a rather remarkable "fingerprint" in the world of climate science. The times we spent together while Tom was Director of CRU were exciting and extraordinary. It would have been fun to get together and celebrate those times, and to celebrate CRU's achievements under Tom's leadership.

mail.2009

Once again, best wishes to you and Sarah. Get well soon, and please let me know if you reconsider.

With best regards,

Ben

Keith Briffa wrote:

> Ben and Phil
> Sorry but I am going to decline the invitation. You will know the respect I have for Tom and the high personal regard I have for him. I will send him a personal message explaining my decision. Sorry for the time it has taken to come to this decision but I had to think hard about it. At this moment I do not know whether Sarah will make it. She like me has not been well over the Christmas/New Year period but she has not yet managed a single day back at work yet. I will have to leave it to her to let you know her thoughts on this.
> Best wishes
> Keith

>
> At 17:58 30/01/2009, you wrote:

>> Dear Keith,

>>

>> Thanks for the update.

>>

>> Phil and I would like to send out a general announcement in the next few weeks, so that folks can put the Symposium on their calendars. It would be nice if we could send out a list of confirmed speakers together with the general announcement. So I'd be very grateful if you could get back to me in the next week or two.

>>

>> Once again, just let me say that it would be great to see you and Sarah in Boulder...

>>

>> With best regards,

>>

>> Ben

>>

>> Keith Briffa wrote:

>>> Ben

>>> I can not confirm. Sorry. Everything you say is true. It didn't need saying, but things may not be straight forward. Will get back to you.

>>> I am not saying no for the present. I know you need to know one way or the other. Best wishes

>>> Keith

>>> At 22:30 29/01/2009, you wrote:

>>>> Dear Keith,

>>>>

>>>> I just wanted to check with you regarding your availability for Tom's Symposium on June 19th. I'm really hoping that you'll be able to attend. It would be great to see you in Boulder, and I know that Tom would be delighted if both you and Sarah could make it.

>>>>

>>>> The way I see it, Tom had a big impact on the scientific careers of many people, but particularly on the scientific lives of you, me, Phil, and Sarah.

>>>>

>>>> Tom and I may not have seen eye-to-eye on everything - but Tom taught me how to be a scientist, and the lessons I learned at CRU have helped me through subsequent difficult times. I view the Symposium as a means of saying "thanks". It would be nice to say thanks in the company of Tom's friends and colleagues.

mail.2009

>>>>
>>>> It would be great to share a few beers in Boulder, and reminisce
>>>> about our infrequent "play 'til you drop" squash games at UEA...

>>>>
>>>> Hope you and Sarah and Amy and Kerstie are all well.

>>>>
>>>> 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) 422-3840
>>>> FAX: (925) 422-7675
>>>> email: santer1@llnl.gov

>>>> -----

>>>> -- Professor Keith Briffa,
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FAX: (925) 422-7675
email: santer1@llnl.gov

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964. 1237289045.txt

#####

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Support letter request
Date: Tue, 17 Mar 2009 07:24:05 -1000
Cc: Edward Cook <drdendro@ldeo.columbia.edu>

Hi Phil, Thanks for this. Here is a support letter from Matt Collins that you can use as a

guide on what to say. It was forwarded to me by Lowell. Cheers, Ed

===== Dr. Edward R. Cook Doherty Senior Scholar and

Director,

Tree-Ring Laboratory Lamont-Doherty Earth Observatory Palisades, New York 10964

USA Email:

drdendro@ldeo.columbia.edu Phone: 845-365-8618 Fax: 845-365-8152

===== On Mar 17, 2009, at 3:13 AM, Phil Jones wrote:

> > Ed, >

I can do this. Do you have any details of what you'd like me to > say? > Does Lowell have

any in yet? > Away all next week. > > Cheers > Phil > > > At 03:09 17/03/2009, you wrote:

>> Hi Phil, >> >> I wonder if you would be willing to write a letter of support for a >>

fairly massive NSF Science and Technology Center (STC) proposal that >> will be submitted

in mid-April. The STC would be the Center for >> Regional Decadal Climate Projections. This

is a 5-year, \$25 million >> dollar, effort spearheaded by Lowell Stott (Department of Earth

>> Science, University of Southern California). It is multi- >> institutional >> with both

climate modelers and palaeoclimatologists (including me) >> involved in an effort to

develop skillful climate prediction >> capability on decadal time scales. See the attached

project summary >> from the pre-proposal that was was accepted by NSF for a full >>

proposal >> to be submitted. If you are willing to write a letter of support, it >> is

probably best that it be written to Lowell: >> >> Dr. Lowell Stott >> Department of Earth

Science >> University of Southern California >> Los Angeles, CA 90089 >> >>

However, you should send the letter to me for forwarding on to >> Lowell. >> The letter emailed to me as

a pdf with electronic signature works >> fine. Thanks for any help you can give me. I am

happy to answer any >> questions you might have as well. >> >> Cheers, >> >> Ed >> >>

===== >> Dr. Edward R. Cook >> Doherty Senior

mail.2009

Scholar and >>

Director, Tree-Ring Laboratory >> Lamont-Doherty Earth Observatory >> Palisades, New York 10964 USA >> Email: drdendro@ldeo.columbia.edu >> Phone: 845-365-8618 >> Fax: 845-365-8152

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===== >>
Dr. Edward R. Cook >> Doherty Senior Scholar and >> Director, Tree-Ring Laboratory >>
Lamont-Doherty Earth Observatory >> Palisades, New York 10964 USA >> Email: drdendro@ldeo.columbia.edu >> Phone: 845-365-8618 >> Fax: 845-365-8152 >>
===== >> >> 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 >

----- > Hi Phil,

Thanks for this. Here is a support letter from Matt Collins that you can use as a guide on what to say. It was forwarded to me by Lowell.

Cheers,

Ed

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

=====
Dr. Edward R. Cook
Doherty Senior Scholar and
Director, Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: [1]drdendro@ldeo.columbia.edu

mail.2009

Phone: 845-365-8618
Fax: 845-365-8152

=====
On Mar 17, 2009, at 3:13 AM, Phil Jones wrote:

Ed,
I can do this. Do you have any details of what you'd like me to say?
Does Lowell have any in yet?
Away all next week.
Cheers
Phil

At 03:09 17/03/2009, you wrote:

Hi Phil,
I wonder if you would be willing to write a letter of support for a fairly massive NSF Science and Technology Center (STC) proposal that will be submitted in mid-April. The STC would be the Center for Regional Decadal Climate Projections. This is a 5-year, \$25 million dollar, effort spearheaded by Lowell Stott (Department of Earth Science, University of Southern California). It is multi-institutional with both climate modelers and palaeoclimatologists (including me) involved in an effort to develop skillful climate prediction capability on decadal time scales. See the attached project summary from the pre-proposal that was accepted by NSF for a full proposal to be submitted. If you are willing to write a letter of support, it is probably best that it be written to Lowell:

Dr. Lowell Stott
Department of Earth Science
University of Southern California
Los Angeles, CA 90089

However, you should send the letter to me for forwarding on to Lowell. The letter emailed to me as a pdf with electronic signature works fine. Thanks for any help you can give me. I am happy to answer any questions you might have as well.
Cheers,
Ed

=====
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Director, Tree-Ring Laboratory
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Email: [2]drdendro@ldeo.columbia.edu
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mail.2009

If you are willing to write a letter of support, it is probably best that it be written

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However, you should send the letter to me for forwarding on to Lowell. The letter

help you can emailed to me as a pdf with electronic signature works fine. Thanks for any

give me. I am happy to answer any questions you might have as well.

Cheers,
Ed

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=====

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References

- 1. <mailto:drdendro@ldeo.columbia.edu>
- 2. <mailto:drdendro@ldeo.columbia.edu>
- 3. <mailto:drdendro@ldeo.columbia.edu>
- 4. <mailto:p.jones@uea.ac.uk>

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#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>, "Michael E. Mann" <mann@psu.edu>
Subject: FYI
Date: Thu Mar 19 10:52:54 2009

Gavin, Mike,

See the link below! Don't alert anyone up to this for a while. See if they figure it out for themselves.

I've sent this to the Chief Exec of the RMS, who said he was considering changing data policy with the RMS journals. He's away till next week. I just wanted him to see what a load of plonkers he's dealing with! I'm hoping someone will pick this up and put it somewhere more prominently.

The responses are even worse than you get on CA.

I've written up the London paper for the RMS journal weather, but having trouble with their new editor. He's coming up with the same

mail.2009

naive comments that these responders are. He can't understand that London has a UHI of X, but that X has got no bigger since 1900. I'm away all next week.

Cheers
Phil

[1][http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-](http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/)

[by-a-major-climate-scientist/](http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/)
"Phil Jones, the director of the Hadley Climate Center in the UK."

--
Thomas C. Peterson, Ph.D.
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References

1.
<http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/>

966. 1237480766.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: FYI
Date: Thu Mar 19 12:39:26 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Mike,
I want to get the more extensive London paper in first.
I hope my missive to the Chief Exec of the RMS does something next week.
By the way the HC doesn't have a Director.
John Mitchell is Head of Climate Science
Chris Gordon is Deputy Director of the HC.
It has never had a Director with that particular title.
It is impossible for anyone to find this on their web site. Only if you were on the HC Scientific Review Group would you be aware.

Cheers
Phil

At 12:24 19/03/2009, Michael Mann wrote:

HI Phil,
thanks, we've already seen numerous comments about this at RealClimate. Its a paper that is easily misunderstood and/or intentionally misrepresented by contrarians (or both).

mail.2009

One possibility is that you might consider writing a guest article for RC
placing this in proper perspective. What do you think?

mike

On Mar 19, 2009, at 6:52 AM, Phil Jones wrote:

Gavin, Mike,

See the link below! Don't alert anyone up to this for a while. See if they
figure it out for themselves.

I've sent this to the Chief Exec of the RMS, who said he was considering
changing data policy with the RMS journals. He's away till next week. I just
wanted him to see what a load of plonkers he's dealing with! I'm hoping
someone will pick this up and put it somewhere more prominently.

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that London has a UHI of X, but that X has got no bigger since 1900.

I'm away all next week.

Cheers

Phil

[1]<http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/>

"Phil Jones, the director of the Hadley Climate Center in the UK."

--

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--

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The Pennsylvania State University

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University Park, PA 16802-5013

website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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References

1. <http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/>
2. <mailto:mann@psu.edu>
3. <http://www.meteo.psu.edu/~mann/Mann/index.html>
4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

967. 1237496573.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
 To: santer1@llnl.gov
 Subject: Re: See the link below
 Date: Thu Mar 19 17:02:53 2009

Ben,

I don't know whether they even had a meeting yet - but I did say I would send something to their Chief Exec.

In my 2 slides worth at Bethesda I will be showing London's UHI and the effect that it hasn't got any bigger since 1900. It's easy to do with 3 long time series. It is only one urban site (St James Park), but that is where the measurements are from. Heathrow has a bit of a UHI and it has go bigger.

I'm having a dispute with the new editor of weather. I've complained about him to the RMS Chief Exec. If I don't get him to back down, I won't be sending any more papers to any RMS journals and I'll be resigning from the

RMS.

The paper is about London and its UHI!

Cheers

Phil

At 16:48 19/03/2009, you wrote:

Thanks, Phil. The stuff on the website is awful. I'm really sorry you have to deal with that kind of crap.

If the RMS is going to require authors to make ALL data available - raw data

PLUS

results from all intermediate calculations - I will not submit any further papers to RMS journals.

Cheers,

Ben

Phil Jones wrote:

Paul,

I sent you this last night, but in another email. I should have sent you two emails - apologies. The issues were not linked. This email is to bring your attention to the link at the end.

The next few sentences repeat what I said last night.

I had been meaning to email you about the RMS and IJC issue of data availability

from for numbers and data used in papers that appear in RMS journals. This results

has made the issue that arose with the paper by Ben Santer et al in IJC last year. Ben

mail.2009

the data available that this complainant wanted. The issue is that this is intermediate data. The raw data that Ben had used to derive the intermediate data was all fully available. If you're going to consider asking authors to make some or all of the data available, then they had done already. The complainant didn't want to have to go to the trouble of doing all the work that Ben had done. I hope this is clear. Another issue that should be considered as well is this. With many papers, we're using Met Office observations. We've abstracted these from BADC to use them in the papers. We're not allowed to make these available to others. We'd need to get the Met Office's permission in all cases.

This email came overnight - from Tom Peterson, who works at NCDC in Asheville.

[1]<http://>

wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-ma

jor-climate-scientist/

"Phil Jones, the director of the Hadley Climate Center in the UK."

We all know that this is not my job. The paper being referred to appeared in JGR

last year. The paper is

Jones, P.D., Lister, D.H. and Li, Q., 2008: Urbanization effects in large-scale

temperature records, with an emphasis on China. /J. Geophys. Res/. *113*, D16122,

doi:10.1029/2008/JD009916.

The paper clearly states where I work - CRU at UEA. There is no mention of the Hadley Centre!

There is also no about face as stated on the web page.

Sending this as it gives a good example of the sort of people you are dealing with when you might be considering changes to data policies at the RMS.

Several years ago I decided there was no point in responding to issues raised on blog sites. Ben has made the same decision as well.

There are probably wider issues due to climate change becoming more main

stream

in the more popular media that the RMS might like to consider. I just think you should

be aware of some of the background. CRU has had numerous FOI requests since the

beginning of 2007. The Met Office, Reading, NCDC and GISS have had as well - many

related to IPCC involvement. I know the world changes and the way we do things changes,

but these requests and the sorts of simple mistakes, should not have an influence

on the way things have been adequately dealt with for over a century.

Cheers

Phil

--

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References

1. <http://>

968. 1237805013.txt

#####

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
To: David Schneider <dschneid@ucar.edu>, Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, Bradley Ray <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Miller Giff <gmiller@Colorado.EDU>, Otto-Bleisner Bette <ottobli@ucar.edu>, Overpeck Jonathan <jto@u.arizona.edu>
Subject: Submitted!
Date: Mon, 23 Mar 2009 06:43:33 -0700

with thanks to all. I'll let you know when I hear anything. Darrell i¼ Darrell S. Kaufman
Professor of Geology and Environmental Sciences Northern Arizona University
928-523-7192
<http://jan.ucc.nau.edu/~dsk5/>

with thanks to all.

I'll let you know when I hear anything.

Darrell

Attachment Converted: "c:\eudora\attach\2k synthesis submitted.pdf"

mail.2009

Darrell S. Kaufman
Professor of Geology and Environmental Sciences
Northern Arizona University
928-523-7192
[1]http://jan.ucc.nau.edu/~dsk5/

References

1. <http://jan.ucc.nau.edu/~dsk5/>

969. 1239572061.txt

#####

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Tim Osborn <t.osborn@uea.ac.uk>, Fortunat Joos <joos@climate.unibe.ch>, Jonathan Overpeck <jto@u.arizona.edu>, David Rind <drind@giss.nasa.gov>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, cddhr@giss.nasa.gov, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Jouzel@dsms-mail.extra.cea.fr, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, Dominique Raynaud <raynaud@lgge.obs.ujf-grenoble.fr>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, jean-claude.duplessy@lsce.cnrs-gif.fr, dolago@uonbi.ac.ke, peltier@atmosph.physics.utoronto.ca, rramesh@prl.res.in, olgasolomina@yandex.ru, derzhang@msn.com, Heinz Wanner <wanner@giub.unibe.ch>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>, Eric W Wolff <ewwo@bas.ac.uk>, fatima.abrantes@ineti.pt, j.dearing@soton.ac.uk, jerome@lgge.obs.ujf-grenoble.fr, jose_carriquiry@uabc.mx, moha_umero@yahoo.com, Michael Schulz <mschulz@uni-bremen.de>, nakatsuka.takeshi@f.mbox.nagoya-u.ac.jp, Bette Otto-Bliesner <ottobli@ucar.edu>, peter.kershaw@arts.monash.edu.au, pfrancus@ete.inrs.ca, scolman@d.umn.edu, whitlock@montana.edu, zlding@mail.iggcas.ac.cn
Subject: Key new IPCC relevant paleo-science
Date: Sun, 12 Apr 2009 17:34:21 +0200
Cc: Laurent Labeyrie <Laurent.Labeyrie@lsce.ipsl.fr>, Gavin Schmidt <gschmidt@giss.nasa.gov>

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Dear friends,

The scoping of IPCC AR5 will happen in July this year. In the community there have been opinions raised regarding paleo-science in the next report, e.g. whether to have paleo-science dispersed into various topical chapters, e.g. forcing, model-evaluation, sea level etc., or whether it might be best to do as in AR4 to have a separate Paleo-chapter.

There are good arguments for both options, and it is not the intent of this email to voice a specific opinion. Rather it is important to let the scoping process be aware of all the relevant new paleo-science which would be assessed in AR5, thereby leading to the need for a strong presence of paleoclimate scientists in the LA-team of AR5, particularly in WG1, but also in WG2.

In order to make the case that paleo-science continues to be highly relevant for IPCC, Peck and I have agreed to be the editors of a Slide-series (ppt style) which can be used to make the case in the scoping, and which of course could be a useful product for various outreach activities of PAGES and the paleoclimate community at large. The PAGES office will assist in producing the slides

We therefore send this email to you who worked as LAs in AR4 or who

mail.2009

are on SSC or other relevant PAGES panels and ask for your input. What we hope you can help with is the following:

1. Provide your best examples of key new IPCC (Policy) relevant new results post AR4, i.e. accepted after July 2006, that provide compelling arguments for paleoclimate science as a key contributor to IPCC. Please limit this to the results which are clearly IPCC-relevant
2. Ongoing projects or programmes that are likely to deliver such results in the next 2-3 years can also be included. The information must, however, be specific and compelling to a non-paleo audience.
3. Send PDF of the paper or other material (like ppt slide) to Peck (jto@u.arizona.edu), Myself and Thorsten Kiefer (thorsten.kiefer@pages.unibe.ch) at PAGES, preferably by May 2.

We think this might become a very useful service to our community and to the climate change communities at large, and will be very rewarding. Hoping to hear back from many of you.

Best wishes

Peck and Eystein

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Professor/Director Bjerknes Centre for Climate Research
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e-mail: eystein.jansen@bjerknes.uib.no
tel: 55-589803/55-583491
fax: 55-584330

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From: David Rind <drind@giss.nasa.gov>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Key new IPCC relevant paleo-science
Date: Mon, 20 Apr 2009 15:03:17 -0400
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Fortunat Joos <joos@climate.unibe.ch>, Jonathan Overpeck <jto@u.arizona.edu>, David Rind <drind@giss.nasa.gov>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, cddhr@giss.nasa.gov, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Jouzel@dsm-mail.extra.cea.fr, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, Dominique Raynaud <raynaud@lgge.obs.ujf-grenoble.fr>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, jean-claude.duplessy@lsce.cnrs-gif.fr, dolago@uonbi.ac.ke, peltier@atmos.physics.utoronto.ca, rramesh@prl.res.in, olgasolomina@yandex.ru, derzhang@msn.com, Heinz Wanner <wanner@giub.unibe.ch>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>, Eric W Wolff <ewwo@bas.ac.uk>, j.dearing@soton.ac.uk, jerome@lgge.obs.ujf-grenoble.fr, jose_carriquiry@uabc.mx, moha_umero@yahoo.com, Michael Schulz <mschulz@uni-bremen.de>, nakatsuka.takeshi@f.mbox.nagoya-u.ac.jp, Bette Otto-Bleisner <ottobli@ucar.edu>, peter.kershaw@arts.monash.edu.au, pfrancus@ete.inrs.ca, scolman@d.umn.edu, whitlock@montana.edu, zlding@mail.iggcas.ac.cn, Laurent Labeyrie

mail.2009

<Laurent.Labeyrie@lsce.ipsl.fr>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Hi Eystein and Jonathan,

With respect to the question of a separate paleo-climate chapter: if paleoclimate is an adjunct to all of the other chapters, what would happen - would there be a paleo-climate person on each of those chapters, just for that component? If so, the person would not carry much influence - and if chapters had to be trimmed (which we know always happens), there's a chance that a lot of the paleoclimate aspect would be the first to go. I'm afraid that little in-depth discussion would survive.

On the other hand: now that there's been a paleoclimate chapter, a lot of the 'introductory' material would not really be needed - just the 'updates', which make for much fewer pages. Perhaps, then, paleoclimate observations could be part of the climate observation chapter; and paleoclimate modeling, part of the modeling chapter. That way, at least several people with paleoclimate heritage could be part of each of these chapters, and allow for a proper representation of the state of our understanding in these areas. It would also allow for better integration of paleoclimates with the current climate. As in the case of present climate, care would have to be taken to ensure that the observations and modeling chapters have strong linkages.

Concerning what new topic should be addressed: there should be a discussion about the use of paleoclimates as analogs for the future. Some scientists (including at least one at GISS) are certain of their utility in this regard. I think the topic should be addressed from all sides.

And as for 'new' paleoclimate work: we have an article about to come out in GRL on stratospheric ozone during the LGM; here's the link:

[1]<http://www.agu.org/journals/gl/papersinpress.shtml#id2009GL037617>

David

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References

1. <http://www.agu.org/journals/gl/papersinpress.shtml#id2009GL037617>

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mail.2009

From: Pierre Francus <pfrancus@ete.inrs.ca>
To: Jonathan Overpeck <jto@email.arizona.edu>
Subject: Re: Key new IPCC relevant paleo-science
Date: wed, 22 Apr 2009 07:03:50 -0400
Cc: Steve Colman <scolman@d.umn.edu>, Eystein Jansen <Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Fortunat Joos <joos@climate.unibe.ch>, David Rind <drind@giss.nasa.gov>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, "cddhr@giss.nasa.gov" <cddhr@giss.nasa.gov>, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, "Jouzel@dsm-mail.extra.cea.fr" <Jouzel@dsm-mail.extra.cea.fr>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, Dominique Raynaud <raynaud@lgge.obs.ujf-grenoble.fr>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "jean-claude.duplessy@lsce.cnrs-gif.fr" <jean-claude.duplessy@lsce.cnrs-gif.fr>, "dolago@uonbi.ac.ke" <dolago@uonbi.ac.ke>, "peltier@atmosph.physics.utoronto.ca" <peltier@atmosph.physics.utoronto.ca>, "ramesh@prl.res.in" <ramesh@prl.res.in>, "olgasolomina@yandex.ru" <olgasolomina@yandex.ru>, "derzhang@msn.com" <derzhang@msn.com>, Heinz Wanner <wanner@giub.unibe.ch>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>, Eric W Wolff <ewwo@bas.ac.uk>, "fatima.abrantes@ineti.pt" <fatima.abrantes@ineti.pt>, "j.dearing@soton.ac.uk" <j.dearing@soton.ac.uk>, "jose_carriquiry@uabc.mx" <jose_carriquiry@uabc.mx>, "moha_umero@yahoo.com" <moha_umero@yahoo.com>, Michael Schulz <mschulz@uni-bremen.de>, "nakatsuka.takeshi@f.mbox.nagoya-u.ac.jp" <nakatsuka.takeshi@f.mbox.nagoya-u.ac.jp>, Bette Otto-Bliesner <ottobli@ucar.edu>, "peter.kershaw@arts.monash.edu.au" <peter.kershaw@arts.monash.edu.au>, Francus Pierre <Pierre.Francus@ete.inrs.ca>, Whitlock Cathy <whitlock@montana.edu>, "zlding@mail.igccas.ac.cn" <zlding@mail.igccas.ac.cn>, Laurent Labeyrie <Laurent.Labeyrie@lsce.ipsl.fr>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Dear all,

I guess one point that can be outlined for the next IPCC report is about the regional differences in climate change and variability.

We can see that in the paleo record, and it is very clear from the work of the PAGES "last 2k regional groups".

There is for instance a new Arctic 2k summary in Journal of Paleolimnology (Kauffman et al 2009), and another paper in prep (I guess you are co-author Peck).

All the best

Pierre

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PASADO-CANADA: [4]<http://www2.ete.inrs.ca/pasado/PASADO-Canada/welcome.html>
GEOTOP: [5]<http://www.geotop.uqam.ca/>

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CEN: [6]<http://www.cen.ulaval.ca/>

PAGES: [7]<http://www.pages.unibe.ch/>

On 21-avr.-09, at 16:42, Jonathan Overpeck wrote:

Thanks Steve and friends I still need to read all the feedback, and appreciate it. I think you hit on a biggie that paleo provides critical evaluations of model realism. with regard to the others, the key for inclusion in an IPCC assessment, is to synthesize the published literature in a way that informs policy makers (the top audience) on what is happening in the climate system, and more important even what will happen in the he climate system.

Taking the terrific speleothem work for example, what are the key lessons that are NEW and important to highlight to policy makers? This is the kind of relevant science we need to compile/highlight. It's harder than at first glance, but that's ok quality of relevant issues is more important than quantity.

Again, thanks all for taking this exercise seriously. Feel free to seek input from colleagues, although please help us by only sending what you think has a chance of fitting with the criteria above it is unclear if there will be time for anyone to read all the strong literature that has come out since mid-2006, so please summarize each key point with a couple sentence bullet, the complete reference (maybe even send the pdf), and if you think the key point isn't easy to understand to a non-paleo person a little (e.g., para) supporting text, w/ a key figure if you think it really helps.

Best, peck

On 4/16/09 1:48 PM, "Steve Colman" <[\[8\]scolman@d.umn.edu](mailto:[8]scolman@d.umn.edu)> wrote:

Dear Peck and Eystein,

I tend to agree with Stefan that it would be conceptually nice to weave paleo-science into all the chapters of the next report, but that, as a practical matter, a tighter focus on paleo results would have more impact. Most people seem to accept that past history is the only way to assess what the climate system can actually do (e.g., how fast it can change). However, I think that the fact that reconstructed history provides the only calibration or test of models (beyond verification of modern simulations) is under-appreciated.

In terms of recent or near-future new results, I think that two areas of continental paleoclimate research are exciting: (1) the new speleothem records, which are producing extremely high-resolution, well-dated histories, especially in monsoon areas; and (2) the network of long-term continental climate histories coming from drill cores in lakes (Titicaca, Malawi, Bosumtwi, Peten Itza, Qinghai, El' gygytgyn) is reaching the point where stimulating syntheses may be possible.

Best,

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References

1. mailto:pfrancus@ete.inrs.ca
2. http://www.inrs-ete.quebec.ca/professeur.php?page=PierreFrancus
3. http://www.ete.inrs.ca/profs/pf/itrax/home.htm
4. http://www2.ete.inrs.ca/pasado/PASADO-Canada/welcome.html
5. http://www.geotop.uqam.ca/
6. http://www.cen.ulaval.ca/
7. http://www.pages.unibe.ch/
8. file://localhost/tmp/scolman@d.umn.edu
9. http://www.d.umn.edu/llo
10. http://www.d.umn.edu/llo
11. file://localhost/tmp/jto@u.arizona.edu
12. file://localhost/tmp/regalado@email.arizona.edu

972. 1241415427.txt

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From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: [Fwd: CCNet Xtra: Climate Science Fraud at Albany University?]-FROM TOM W
Date: Mon, 04 May 2009 01:37:07 -0600
Cc: Ben Santer <santer1@llnl.gov>

Content-Type: text/plain; charset=UTF-8; format=flowed
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n457EfQ5005459

<x-flowed>
Phil,

Do you know where this stands? The key things from the Peiser items are ...

"Wang had been claiming the existence of such exonerating documents for nearly a year, but he has not been able to produce them. Additionally, there was a report published in 1991 (with a second version in 1997) explicitly stating that no such documents exist. Moreover, the report was published as part of the Department of Energy Carbon Dioxide Research Program, and Wang was the Chief Scientist of that program."

and

"Wang had a co-worker in Britain. In Britain, the Freedom of Information Act requires that data from publicly-funded research be made available. I was able to get the data by requiring Wang's co-worker to release it, under British law. It was only then that I was able to confirm that Wang had committed fraud."

You are the co-worker, so you must have done something like provide Keenan with the DOE report that shows that there are no station records for 49 of the 84 stations. I presume Keenan therefore thinks that it was not possible to select stations on the basis of ...

"... station histories: selected stations have relatively few, if any, changes in instrumentation, location, or observation times"
[THIS IS ITEM "X"]

Of course, if the only stations used were ones from the 35 stations that *did* have station histories, then all could be OK. However, if some of the stations used were from the remaining 49, then the above selection method could not have been applied (but see below) -- unless there are other "hard copy" station history data not in the DOE report (but in China) that were used. From what Wang has said, if what he says is true, the second possibility appears to be the case.

What is the answer here?

The next puzzle is why Wei-Chyung didn't make the hard copy information available. Either it does not exist, or he thought it was too much trouble to access and copy. My guess is that it does not exist -- if it did then why was it not in the DOE report? In support of this, it seems that there are other papers from 1991 and 1997 that show that the data do not exist. What are these papers? Do they really show this?

Now my views. (1) I have always thought W-C W was a rather sloppy scientist. I therefore would not be surprised if he screwed up here. But ITEM X is in both the W-C W and Jones et al. papers -- so where does it come from first? Were you taking W-C W on trust?

(2) It also seems to me that the University at Albany has screwed up. To accept a complaint from Keenan and not refer directly to the complaint and the complainant in its report really is asking for trouble.

(3) At the very start it seems this could have been easily dispatched. ITEM X really should have been ...

"Where possible, stations were chosen on the basis of station histories and/or local knowledge: selected stations have relatively few, if any, changes in instrumentation, location, or observation times"

Of course the real get out is the final "or". A station could be selected if either it had relatively few "changes in instrumentation" OR "changes in location" OR "changes in observation times". Not all three, simply any one of the three. One could argue about the science here -- it would be better to have all three -- but this is not what the statement says.

Why, why, why did you and W-C W not simply say this right at the start? Perhaps it's not too late?

I realise that Keenan is just a trouble maker and out to waste time, so

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I apologize for continuing to waste your time on this, Phil. However, I *am* concerned because all this happened under my watch as Director of CRU and, although this is unlikely, the buck eventually should stop with me.

Best wishes,
Tom

P.S. I am copying this to Ben. Seeing other peoples' troubles might make him happier about his own parallel experiences.

</x-flowed>
Return-Path: <b.j.peiser@ljmu.ac.uk>
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Received: from nscan3.ucar.edu (nscan3.ucar.edu [128.117.64.193])
by post2.cgd.ucar.edu (Postfix) with ESMTMP id CB38C3803F;
Sun, 3 May 2009 08:57:40 -0600 (MDT)
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Sun, 3 May 2009 08:57:40 -0600 (MDT)
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by localhost (nscan3.ucar.edu [127.0.0.1]) (amavisd-new, port 10024)
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charset="utf-8"
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Date: Sun, 3 May 2009 15:57:08 +0100
Message-ID: <08927B60D87D374DB001D814D5D2250F01663F4F@exch4.jmu.ac.uk>
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X-MS-TNEF-Correlator:
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Thread-Index: AcnIu00vOgPY3fShTXip0PBdcf9mwwAAWuOQAGIoisAAbhWS4A==
From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
To: "cambridge-conference" <cambridge-conference@livjm.ac.uk>
X-Virus-Scanned: amavisd-new at ucar.edu

CCNet Xtra - 3 May 2009 -- Audiatur et altera pars

CLIMATE SCIENCE FRAUD AT ALBANY UNIVERSITY?

The University at Albany is in a difficult position. If the University received such records as part of the supposed misconduct investigation, then they could easily resolve the problem by making them available to the scientific community and to readers. If the University does not have such records then they have been complicit in misconduct and in coverup of misconduct. If the University at Albany does have such records, but such records are not in accordance with the stated methodology of the publications, then the University has more serious difficulties.

"Investigations" of scientific misconduct should themselves align with the usual principles of scientific discourse (open discussion, honesty, transparency of method, public disclosure of evidence, open public analysis and public discussion

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and reasoning underlying any conclusion). This was not the case at the University at Albany. When you see universities reluctant to investigate things properly, it provides reasonable evidence that they really don't want to investigate things properly.

-- Aubrey Blumsohn, Scientific Misconduct Blog, 2 May 2009

- (1) ALLEGATIONS OF FRAUD AT ALBANY - THE WANG CASE
Aubrey Blumsohn, Scientific Misconduct Blog, 2 May 2009
- (2) THE FRAUD ALLEGATION AGAINST SOME CLIMATIC RESEARCH OF WEI-CHYUNG WANG
Douglas J. Keenan, Informath, April 2009
- (3) KAFKA AT ALBANY
Peter Risdon, Freeborn John, 15 March 2009

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- (1) ALLEGATIONS OF FRAUD AT ALBANY - THE WANG CASE

Scientific Misconduct Blog, 2 May 2009

<http://scientific-misconduct.blogspot.com/2009/05/allegations-of-fraud-at-albany-wan-g.html>

Aubrey Blumsohn

Professor Wei-Chyung Wang is a star scientist in the Atmospheric Sciences Research Center at the University at Albany, New York. He is a key player in the climate change debate (see his self-description here). Wang has been accused of scientific fraud.

I have no inclination to "weigh in" on the topic of climate change. However the case involves issues of integrity that are at the very core of proper science. These issues are the same whether they are raised in a pharmaceutical clinical trial, in a basic science laboratory, by a climate change "denialist" or a "warmist". The case involves the hiding of data, access to data, and the proper description of "method" in science.

The case is also of interest because it provides yet another example of how *not* to create trust in a scientific misconduct investigation. It adds to the litany of cases suggesting that Universities cannot be allowed to investigate misconduct of their own star academics. The University response has so far been incoherent on its face.

Doug Keenan, the mathematician who raised the case of Wang is on the "denialist" side of the climate change debate. He maintains that "almost by itself, the withholding of their raw data by [climate] scientists tells us that they are not scientists".

Below is my own summary of the straightforward substance of this case. I wrote to Wei-Chyung Wang, to Lynn Videka (VP at Albany, responsible for the investigation), and to John H. Reilly (a lawyer at Albany) asking for any correction or comments on the details presented below. My request was acknowledged prior to publication, but no factual correction was suggested.

Case Summary

The allegations concern two publications. These are:

Jones P.D., Groisman P.Y., Coughlan M., Plummer N., Wang W.-C., Karl T.R. (1990), "Assessment of urbanization effects in time series of surface air temperature over land", *Nature*, 347: 169-172. (PDF here)

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Wang W.-C., Zeng Z., Karl T.R. (1990), "Urban heat islands in China", Geophysical Research Letters, 17: 2377-2380. (PDF here)

The publications concern temperature at a variety of measuring stations over three decades (1954-1983). Stations are denoted by name or number. A potential confounder in such research is that measuring stations may be moved to different locations at different points in time. It is clearly important that readers of publications understand the methodology, and important confounders.

The publications make the following statements:

(Statement A) "The stations were selected on the basis of station history: we chose those with few, if any, changes in instrumentation, location or observation times." [Jones et al.]

(Statement B) "They were chosen based on station histories: selected stations have relatively few, if any, changes in instrumentation, location, or observation times..." [Wang et al.]

The publications refer to a report produced jointly by the U.S. Department of Energy (DOE) and the Chinese Academy of Sciences (CAS) which details station moves, and the publications further suggest that stations with few if any moves or changes were selected on the basis of that report. However:

Of 84 stations that were selected, Keenan found that information about only 35 are available in the DOE/CAS report

Of those 35 stations at least half did have substantial moves (e.g 25 km). One station had five different locations during 1954-1983 as far as 41 km apart.

It therefore appears that Statements A and B must be false. If false, readers would have been misled both in terms of the status of the stations and the manner in which they had been selected (or not selected).

Keenan then communicated with the author of one of the publications (Jones) to ask about the source of location information pertaining to the other 49 stations that had not been selected using the described methodology. Jones informed Keenan that his co-author Wang had selected those stations in urban and rural China based on his "extensive knowledge of those networks".

On 11 April 2007 Keenan E-mailed Wang, asking "How did you ensure the quality of the data?". Wang did not answer for several weeks, but on 30 April 2007 he replied as follows:

"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)"

Keenan points out that the "hard copies" to which Wang refers were not found by the authors of the DOE/CAS report, who had endeavored to be "comprehensive" (and that the DOE/CAS report was authored in part by Zeng, one of the co-authors on Wang). Keenan further notes that any form of comprehensive data covering these stations during the Cultural Revolution would be implausible.

In August 2007 Keenan submitted a report to the University at Albany, alleging fraud. Wang could at that stage have made the "hard copy" details of the stations selected available to the scientific community. However, he failed to do so.

In May 2008, the University at Albany wrote to Keenan that they had conducted an investigation and asked him to comment on it (see the rather odd letter). However they refused to show him the report of the investigation or any of the evidence to

allow any comment (further odd letter).

In August 2008 the University sent Keenan an astonishing letter of "determination" stating that they did not find that Wang had fabricated data, but that they refused to provide any investigation report or any other information at all because "the Office of Research Integrity regulations preclude discussion of any information pertaining to this case with others who were not directly involved in the investigation".

Wang has still not made the station records available to the scientific community. If he provided such records to the University as part of a misconduct investigation, then the University has apparently concealed them.

Comments

In the absence of any explanation to the contrary, it seems that the methodology for station selection as described in these two publications was false and misleading.

Wang maintains that hard copy records do exist detailing the location of stations selected by himself outwith the published methodology. However the refusal to clarify "method" is inappropriate and a form of misconduct in and of itself. It does not lend credence to Wang's assertion that fraud did not take place. It would also be necessary to see records of stations that were not selected, in order to confirm that selection was indeed random, and only "on the basis of station history".

The University at Albany is in a difficult position.

If the University received such records as part of the supposed misconduct investigation, then they could easily resolve the problem by making them available to the scientific community and to readers.

If the University does not have such records then they have been complicit in misconduct and in coverup of misconduct.

If the University at Albany does have such records, but such records are not in accordance with the stated methodology of the publications, then the University has more serious difficulties.

"Investigations" of scientific misconduct should themselves align with the usual principles of scientific discourse (open discussion, honesty, transparency of method, public disclosure of evidence, open public analysis and public discussion and reasoning underlying any conclusion). This was not the case at the University at Albany. When you see universities reluctant to investigate things properly, it provides reasonable evidence that they really don't want to investigate things properly.

For further information on this case see here and here.

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(2) THE FRAUD ALLEGATION AGAINST SOME CLIMATIC RESEARCH OF WEI-CHYUNG WANG

Informath, April 2009
<http://www.informath.org/apprise/a5620.htm>

Douglas J. Keenan

Following are some remarks about my exposé, "The fraud allegation against some climatic research of Wei-Chyung Wang".

Wei-Chyung Wang is a professor at the University at Albany, State University of New York. He has been doing research on climate for over 30 years, and he has authored or co-authored more than 100 peer-reviewed scientific articles. He has also received an Appreciation Plaque from the Office of Science in the U.S.A., commending him,

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â€œFor your insightful counsel and excellent science. ...â€. The plaque resulted in particular from his research on global warming.

I have formally alleged that wang committed fraud in important parts of his global-warming research. Below is a relevant timeline.

03 August 2007 My report, â€œwei-Chyung wang fabricated some scientific claimsâ€, is sent to the Vice President for Research at Wang's university.

31 August 2007 The university notifies me that it is initiating an inquiry into suspected research misconduct by wang. (The notification includes a copy of the university's Policy and Procedures on Misconduct in Research and Scholarship.)

12 November 2007 My exposÃ© on wang's alleged fraud is published (reference below).

07 December 2007 Myself and the university's Inquiry Committee have a conference call.

20 February 2008 The university sends me the Report of the Inquiry Committee. The Committee unanimously concluded that â€œthere was no dataâ€ (thus implicitly concluding wang must have fabricated data) and that a full investigation should be undertaken.

23 May 2008 The university sends me a notice: the Investigation Committee has completed its work and found no evidence of fraud. The investigation was conducted without interviewing me, which is a violation of the university's policy. The university asks me to comment on the Committee's report; I am, however, not allowed to see the report.

04 June 2008 The university informs me that I am not allowed to see the report because they did not interview me when preparing it.

06 June 2008 I submit comments to the university, listing ways in which I believe the university has acted in breach of U.S. regulations and its own policy.

11 July 2008 I submit a complaint to the Public Integrity Bureau at the Office of the Attorney General of New York State, alleging criminal fraud.

12 August 2008 The university sends me the determination for its investigation, saying that there is â€œno evidence whatsoever [of] ... any research misconductâ€.

07 October 2008 I telephone the Public Integrity Bureau and am told that it might be some months before the Bureau begins to review the complaint.

17 March 2009 I telephone the Public Integrity Bureau and am told that the complaint is under review by an attorney.

18 March 2009 I file three requests under the Freedom of Information Law of New York State: for a copy of the full report by the Inquiry Committee; for a copy of the full report by the Investigation Committee; and, given that the relevant federal funding agencies are required to be notified when a misconduct investigation is initiated, for copies of all such notifications that were sent by the university and pertain to the investigation of wang.

24 March 2009 Given that wang received funding for the fraudulent research from the U.S. Department of Energy (DOE) and that the DOE has since supplied more funding to wang, I report the fraud and the university's apparent cover up to the Office of Inspector General at the DOE.

This web page will be updated with news about the case, as the investigations progress.

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(3) KAFKA AT ALBANY

Freeborn John, 15 March 2009
<http://freebornjohn.blogspot.com/2009/03/kafka-at-albany.html>

Peter Risdon

Last June I reported on the allegations of academic fraud levelled by a British mathematician, Doug Keenan, against Professor Wei-Chyung Wang of New York State University at Albany.

Dr Keenan alleged that in work that has come to be widely cited in climate studies, work that included the collation of data from temperature measuring stations in China, Professor Wang made statements that "cannot be true and could not be in error by accident. The statements are fabricated."

In August 2007, Dr Keenan submitted a report (pdf) of his allegations to the Vice President for Research at Wang's university and an inquiry was initiated. In February 2008 this was escalated into a full investigation by the Inquiry Committee.

All this was summarised in my earlier post, together with quotations from Dr Keenan's allegation.

So far, things had run as might be expected. A fraud had been alleged, the University at Albany looked into it and decided to hold a formal investigation. Dr Keenan waited to be contacted by the investigation and asked to put his case, in line with the university's Policy and Procedures on Misconduct in Research and Scholarship (.doc). The relevant section of this document runs as follows (emphasis added):

III. A. Rights and Responsibilities of the Complainant

Rights: The Vice President for Research will make every effort to ensure the privacy and confidentiality of complainants. The University will protect, to the maximum extent possible, the position and the reputation of those who in good faith report alleged misconduct in research.

The Vice President for Research will work to ensure that complainants will not be retaliated against in the terms and conditions of their employment or other status at the University and will review instances of alleged retaliation for appropriate action. Any alleged or apparent retaliation should be reported immediately to the Vice President for Research.

The complainant will be provided a copy of the formal allegations when and if an inquiry is opened. The complainant will have the opportunity to review portions of the inquiry and investigation reports pertinent to the complainant's report or testimony, and will be informed in writing of the results of the inquiry and investigation, and of the final determination. After the final determination and upon request to the Vice President for Research, the complainant shall be given access to the full documentation.

Responsibilities: The complainant is responsible for making allegations in good faith, maintaining confidentiality, and cooperating fully with an inquiry and/or investigation.

Dr Keenan lived up to the responsibility as stated in the final paragraph above so far as he could. He had made the allegation in good faith and given Professor Wang an opportunity to explain how he had reached his results, an opportunity the Professor had not taken. Keenan maintained confidentiality. In order to cooperate with the investigation, though he would first have to be contacted by it. Dr Keenan waited.

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Late in May 2008 a communication arrived from Albany. It said:

After careful review of the evidence and thoughtful deliberation, the Investigation Committee finds no evidence of the alleged fabrication of results and nothing that rises to the level of research misconduct having been committed by DR. Wang.

As the institutional official responsible for this case, I have accepted the Committee's findings and the Report. You have fourteen (14) calendar days from the date of this letter to provide any comments to add to the report for the record.

Contrary to its own rules, the Committee had not given Keenan the opportunity to "review portions of the inquiry and investigation reports".

That's astonishing, but here's where it becomes Kafkaesque. Keenan was being asked, in this most recent communication, to comment on the report of the Committee. But he was not sent a copy of the report. When he challenged this, he received an email from Adrienne Bonilla explaining that:

[Keenan] did not receive a copy of the Investigation report because the report did not include portions addressing your role and opinions in the investigation phase.

Per the UAlbany Misconduct policy:

VI. E. Investigation Report and Recommendations of the Vice President for Research

"...The Vice President for Research will provide the respondent with a copy of the draft investigation report for comment and rebuttal and will provide the complainant with those portions of the draft report that address the complainant's role and opinions in the investigation. The respondent and complainant will be given 14 calendar days from the transmission of the report to provide their written comments. Any written responses to the report by either party will be made part of the report and record.

Keenan then wrote to the Vice President for Research at Albany, Lynn Videka, pointing out the various ways in which the University had breached its own policy, stating that its behaviour was consistent with a cover up, and pointing out that Professor Wang has received more than \$7 million in grants from a couple of US federal agencies.

In August 2008, Lynn Videka wrote to Keenan enclosing a final copy of a "determination" of the investigation. In her covering note, she stated:

I am notifying you of the case outcome because you were the complainant in this case. The University's misconduct policies and the Office of Research Integrity regulations preclude discussion of any information pertaining to this case with others who were not directly involved in the investigation.

To summarise, the university initiated an investigation, then broke its own rules by not involving Dr Keenan. It then produced a report that carefully avoided mentioning Dr Keenan, so it could claim he was not entitled to see a copy of this report. It then asked Keenan to comment on the report. It has completely disregarded its own policy that "After the final determination and upon request to the Vice President for Research, the complainant shall be given access to the full documentation."

But Doug Keenan is a tenacious man. In July 2008, after being refused sight of the report, he submitted a formal complaint (pdf) to the Public Integrity Bureau at the Office of the Attorney General of New York State, alleging criminal fraud. In this complaint, he said:

Wei-Chyung wang is a professor at the University at Albany, State University of New York. He has been doing research for over 30 years. For this research, wang has received at least \$7 million. The funds have come primarily from the Department of Energy, with additional funding from other federal agencies (DOD, FAA, NSF). I have

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formally alleged that wang committed fraud in important parts of his research. My allegation was submitted to the University at Albany; a copy is enclosed.

The university conducted a preliminary inquiry; a copy of the report from the inquiry is enclosed (redacted, by the university). Briefly, wang claimed that there were some documents that could exonerate him. The inquiry concluded that there should be a full investigation, which should be "recharged with obtaining and reviewing any such additional evidence ... so that a final resolution may be made regarding the allegation against Dr. wang".

wang had been claiming the existence of such exonerating documents for nearly a year, but he has not been able to produce them. Additionally, there was a report published in 1991 (with a second version in 1997) explicitly stating that no such documents exist. Moreover, the report was published as part of the Department of Energy Carbon Dioxide Research Program, and wang was the Chief Scientist of that program.

The university conducted an investigation. The investigation concluded that wang is innocent. I believe that the case against wang is strong and clear, and that the university is trying to cover up the fraud so as to protect its reputation. wang is one of the university's star professors. The conduct of the investigation violated several of the university's own stated policies: details are given in an attached e-mail (dated 06 June 2008).

The e-mail was sent to Lynn Videka, Vice President for Research at the university: Videka was in charge of overseeing the investigation. Note, in particular, that the documents that wang was relying on were never produced.

I have only examined a little of wang's research; so I do not know the full extent of the fraud. It is difficult to examine more in part because wang has not willingly made his data available: when asked for the data from the research that I later reported as fraudulent, wang refused. For that research, though, wang had a co-worker in Britain. In Britain, the Freedom of Information Act requires that data from publicly-funded research be made available. I was able to get the data by requiring wang's co-worker to release it, under British law. It was only then that I was able to confirm that wang had committed fraud. Details are given in my report to the university (page 4, last paragraph). I would be willing to help examine other research that wang has done, if more data were made available.

There was another case of research fraud with a professor at the University of Vermont, in 2005. There, Prof. Eric Poehlman was convicted of making false statements on federal grant applications; he was sentenced to a year and a day in prison. wang has done the same as Poehlman. The fraudulent work described in my report dates from 1990; wang has been relying on that work in some of his grant applications since then. As I understand things, each of those applications is a violation of statute. (Additionally, wang has been using the grants to go on frequent trips to China.)

In October 2008 Dr Keenan was told there could be a wait of several months while his complaint is investigated.

I'll let you know when there are any further developments.

UPDATE: I didn't mention this in the main piece above, but I did mail the relevant person at Albany myself, some time ago, asking for news of the investigation against Professor wang. I received no reply.

However, within a couple of hours of this being posted, someone at Albany came to look at it, from the host aspmi-cc326.cc.albany.edu (169.226.172.35), having apparently been sent an email about it.

So even if they are not communicative about this case, it seems someone at Albany is keeping their eyes open for reports of it.

mail.2009

UPDATE: On reflection, the hit from Albany is also consistent with someone using Google Alerts to monitor coverage of this issue.

UPDATE: Doug Keenan has been told on the telephone that this case is now under review by an attorney at the OAG Public Integrity Bureau.

UPDATE: Also see new findings on the effect of urban warming.

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#####

From: "peter.thorne" <peter.thorne@metoffice.gov.uk>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: CRUTEM4
Date: Tue, 12 May 2009 08:54:44 +0100

Phil,

there may be some money this FY, substantial sums. Management here are casting around for ideas. As its to be spent this FY its largely going to be consultant work as we never have a cats chance in hell of recruiting on that timescale. what resource do you think we could contract from CRU (you, Harry, others?) for doing a CRUTEM4 which I would maintain had two aims ...

1. Rescue and incorporation of recent data (I'm pinging NCDC too to see what they could do vis-a-vis collating and sending the non-wmo US stations and other data you may not have ... their bi-lats may have sig. extra stations for Iran, Aus, Canada etc.)

2. A more robust error model that led to production of a set of equi-probable potential gridded products (HadSST3 will do simnilarly so we could combine to form HadCRUT4 equi-probable). This error model determination would ideally be modular so that we could assess how wrong our assumptions about the error would have to be to "matter" and what error sources are important for our ability to characterise the long-term trend (trivially these will be the red noise I know but then most people seem blind to the trivial sadly ...). The HadCRUT3 paper clearly started well down that path but a recent paper I had the displeasure of reviewing on my way back from WMO shows its poorly understood (deliberately so in this particular case ...).

We have a meeting Thursday. If it passes muster there we'll put it to DECC and see what happens. No promises.

mail.2009

This would mean we'd have HadCRUT4 which would be HadSST3 + CRUTEM4 each with more data and better error models well before AR5 which seems sensible ...

Mr. Fraudit never goes away does he? How often has he been told that we don't have permission? Ho hum. Oh, I heard that fraudit's Santer et al comment got rejected. That'll brighten your day at least a teensy bit?

Peter

--

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974. 1242136391.txt

#####

From: "peter.thorne" <peter.thorne@metoffice.gov.uk>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: CRUTEM4
Date: Tue, 12 May 2009 09:53:11 +0100

Phil,

I can't believe that people think it remotely reasonable behaviour to send that sort of crud. They'd never say that to your face. I guess their home is just that much more cosy and impersonal.

Cash would need spending in FY09/10 as I understand it, but someone for six months (assuming they could start this Sept.) could be a route forwards. It would be a good paper for them career-wise.

HadsST3 is in first draft form. I'm not sure what papers you assume will arise. I think we were thinking of developing HadsST3 and CRUTEM4 seperately (but in a joined up way) and publishing as separate papers and then doing a paper that covers combination to HadCRUT4 and perhaps, for example, a d&a sensitivity to error model assumptions.

Peter

On Tue, 2009-05-12 at 09:43 +0100, Phil Jones wrote:

> Peter,
> Below is one of three emails I got last night following a new thread on CA.
> I'll ignore them and wait for the FOI requests, which we have dealt
> with before.
> I did send an email to Thomas Stocker alerting him up to comment #17.
> These are all about who changed what in various chapters of AR4. I
> expect these
> to get worse with AR5.
>
> Anyway back to the matter in hand.
>
> I'm planning to come down to see Ian Simpson (probably on June
> 1). I'll get back
> to David on this later today.
> We've done some of what you aim for. We've sorted out the new Canadian
> WMO numbers and have extra data for Australia and NZ in. Australia comes in
> by email once a month. I'll have to find a new contact in NZ now
> Jim Salinger has

mail.2009

> been sacked - but it's only a small country. Iran is pretty good.
> The US is the large bit of work. The US already has better
> station density than
> almost anywhere else, so the effort won't make much difference. But
> it is probably
> worth doing, as it would reduce errors - even if no-one understands
> them. Glad
> you got the poor paper to review!
> Soon we will be adding data for the Greater Alpine Region (32 sites) which
> go back to 1760. These data all have adjustments for screen issues prior to
> about 1880. This makes summers cooler by about 0.4 deg C and winters about
> the same. Similarly, we will also add a load of stations for Spain
> (again with Screen
> biases in). There is probably more we could add for European countries,
> but again it is likely to make little difference, except to lower errors.
> The real issue is South America and Africa. We have the whole
> Argentine network,
> but this is only digitized back to 1959 and the data we had wasn't
> that bad anyway.
> Problem in South America is Brazil. Africa is OK in a few
> countries, but poor in many.
> We could add loads in China.
> Issue with all this is that most of the additions wouldn't be
> available from whenever
> we stop. We can probably do the US in real time like Australia.
> we've also been trying to add in the precip for many of these
> extra stations (not
> the Alpine countries and Spain).
> There is a timing issue. As I understand HadSST3 won't be
> available to be merged
> with until it is successfully reviewed. So need to consider this as well.
>
> A final issue is people here. We're OK for most of 2010 for all.
> We have a good
> student finishing a PhD by Sept who wants to stay, so couldn't
> really do anything
> till then.
>
> Cheers
> Phil

>
>
> Dear Mr Jones

>
> As a UK tax payer from the productive economy, could you please
> explain why you restrict access to data sets that are gathered using
> tax payer funds e.g. CRUTEM3. Can you believe how embarrassing this is
> to a UK TAX PAYER, putting up with your amateurish non disclosure of
> environmental information.
>
> For reference <http://www.climateaudit.org/?p=5962> refers to your
> abysmal attitude to public data, although this is just the latest in
> an embarrassing set of reasonable requests from CRU, who the hell do
> you think you are? There will of course be an FOI on the back of this
>
> Regards
> Ian
>
> At 08:54 12/05/2009, peter.thorne wrote:
> >Phil,
> >
> >there may be some money this FY, substantial sums. Management here are

mail.2009

> >casting around for ideas. As its to be spent this FY its largely going
> >to be consultant work as we never have a cats chance in hell of
> >recruiting on that timescale. what resource do you think we could
> >contract from CRU (you, Harry, others?) for doing a CRUTEM4 which I
> >would maintain had two aims ...

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> >1. Rescue and incorporation of recent data (I'm pinging NCDC too to see
> >what they could do vis-a-vis collating and sending the non-wmo US
> >stations and other data you may not have ... their bi-lats may have sig.
> >extra stations for Iran, Aus, Canada etc.)

> >
> >2. A more robust error model that led to production of a set of equi-
> >probable potential gridded products (HadsSST3 will do simnilarly so we
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> >people seem blind to the trivial sadly ...). The HadCRUT3 paper clearly
> >started well down that path but a recent paper I had the displeasure of
> >reviewing on my way back from WMO shows its poorly understood
> >(deliberately so in this particular case ...).

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> >We have a meeting Thursday. If it passes muster there we'll put it to
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> >with more data and better error models well before AR5 which seems
> >sensible ...

> >
> >Mr. Fraudit never goes away does he? How often has he been told that we
> >don't have permission? Ho hum. Oh, I heard that fraudit's Santer et al
> >comment got rejected. That'll brighten your day at least a teensy bit?

> >
> >Peter

> >--
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#####

From: Michael Mann <mann@meteo.psu.edu>

mail.2009

To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: nomination: materials needed!
Date: Tue, 19 May 2009 12:12:55 -0400

thanks much Phil,

that sounds good. So why don't we wait until next round (June '10) on this then.
That will give everyone an opportunity to get their ducks in a row. Plus I'll have one more Nature and one more Science paper on my resume by then (more about that soon!). I'll be sure to send you a reminder sometime next may or so!

Thanks for sending that paper. It takes some work to get a paper rejected by IJC.
Want to take a bet that some version of this appears in "Energy and Environment"? Of course, any paper that appears there is not taken seriously anyway, its almost a joke.

The contrarians attacks certainly have not abated. The only hope is that they'll increasingly be ignored.

talk to you later,

mike

On May 19, 2009, at 9:03 AM, Phil Jones wrote:

Mike,

Have gotten replies - the're both happy to write supporting letters, but both are too busy to take it on this year. One suggested waiting till next year. Malcolm is supporting one other person this year. I'd be happy to do it next year, so I can pace it over a longer period. Malcom also said that Singer had an AGU Fellowship!!

Apart from my meetings I have skeptics on my back - still, can't seem to get rid of them. Also the new UK climate scenarios are giving govt ministers the jitters as they don't want to appear stupid when they introduce them (late June?).

Talking of skeptics - the attached was rejected by IJC. He put it up on something xarchiv. Easy to see why it was rejected. Parts appear quite well written, but they always go too far. Obviously have no idea how to write a paper.

Cheers

Phil

At 14:35 18/05/2009, you wrote:

thanks much Phil,
hopefully will see you before Vienna, but if not, I look forward to seeing you there
next year,
talk to you later,
mike

On May 18, 2009, at 9:28 AM, Phil Jones wrote:

Mike,

I'll email Ray and Malcolm. I'd be happy to contribute. Away all next week and another couple of weeks in June.

EGU will be in Vienna again. It is set for May 2-7, 2010.

It will also be Vienna in 2011.

Cheers

Phil

At 22:31 16/05/2009, you wrote:

mail.2009

Hey Phil,
I hope all is well w/ you these days. Been a while since I've actually seen you. Perhaps can convince you to make it to EGU next year? Looks like it will be in Vienna again. I rather enjoyed this one, and I think I may go back next year. On a completely unrelated note, I was wondering if you, perhaps in tandem w/ some of the other usual suspects, might be interested in returning the favor this year ;) I've looked over the current list of AGU fellows, and it seems to me that there are quite a few who have gotten in (e.g. Kurt Cuffey, Amy Clement, and many others) who aren't as far along as me in their careers, so I think I ought to be a strong candidate. anyway, I don't want to pressure you in any way, but if you think you'd be willing to help organize, I would naturally be much obliged. Perhaps you could convince Ray or Malcolm to take the lead? The deadline looks as if it is again July 1 this year. looking forward to catching up w/ you sometime soon, probably at some exotic location of Henry's choosing ;) mike

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503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [2]mann@psu.edu
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website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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<0905.0445.pdf>

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website: [7]http://www.meteo.psu.edu/~mann/Mann/index.html
"Dire Predictions" book site:
[8]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. mailto:p.jones@uea.ac.uk
2. mailto:mann@psu.edu
3. http://www.meteo.psu.edu/~mann/Mann/index.html
4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
5. mailto:p.jones@uea.ac.uk
6. mailto:mann@psu.edu
7. http://www.meteo.psu.edu/~mann/Mann/index.html
8. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

976. 1243369385.txt

#####

From: Gifford Miller <gmiller@colorado.edu>
To: Darrell Kaufman <Darrell.Kaufman@nau.edu>
Subject: Re: Fwd: Your Science manuscript 1173983 at revision
Date: Tue, 26 May 2009 16:23:05 -0400
Cc: David Schneider <dschneid@ucar.edu>, Nick McKay <nmckay@email.arizona.edu>,
Caspar Ammann <ammann@ucar.edu>, Bradley Ray <rbradley@geo.umass.edu>, Keith
Briffa <k.briffa@uea.ac.uk>, Miller Giff <gmiller@colorado.edu>,
Otto-Bleisner Bette <ottobli@ucar.edu>, Overpeck Jonathan <jto@u.arizona.edu>

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Darrell (from AGU Toronto):

Great news from Science!

A quick comment on Amplification and signal to noise issues (comment 1 below). It think you meant that the referee felt that Arctic amplification did not translate to a more robust signal because the noise would be equally amplified. I don't know that we can challenge the "climate noise" but we can make the case that the "proxy noise", that is, the uncertainty in proxy calibration, is, as far as I know, the same in the Arctic as in lower latitudes. Consequently, the larger temperature signal expected in the Arctic can be more reliably detected by our proxies because it is more likely to exceed the sensitivity limits of our proxies. If we assume the "climate noise" is more or less gaussian, then we should be better able to detect the relatively subtle temp changes of the Holocene in the Arctic than elsewhere.

Giff

mail.2009

>Co-authors:

>I just received the reviewers' comments and editor's decision on our
>SCIENCE manuscript (attached). The decision isn't final, but it
>looks like good news, with very reasonable revisions. Reviewer #1
>had nothing substantial to suggest. Reviewer #2 was rather thorough.
>I think I can address his/her suggestions but could use some help
>with three:

>
>(1) The reviewer challenged our assertion that, because climate
>change is amplified in the Arctic, the signal:noise ratio should be
>higher too. We don't have more than 1 sentence to expand on the
>assertion in the text. We could plead the case to editor and hope
>that it doesn't trip up the final acceptance, or we could omit it
>from the text. Suggestions?

>
>(2) The reviewer suggested that, if we are concerned about outliers
>influencing the mean values of the composite record, we should
>attempt a so-called "robust" regression procedure, such as median
>absolute deviation regression. Does anyone have experience with this?

>
>(3) The reviewer was concerned that we overestimated the strength of
>the relation between temperature and insolation in the long CCSM
>simulation. Namely s/he criticized the leveraging effect of the one
>outlier in the model-generated insolation vs temperature plot (Fig.
>4b), and suggested that we use 10-year means instead of 50 year.
>Dave: you up for this, please?

>
>Please forward any input to me and I'll compile them, and let you
>all have a look before I submit the final revisions. I'm hoping we
>can turn this around this week.

>
>Thanks.
>Darrell

>

--

Gifford H. Miller, Professor
INSTAAR and Geological Sciences
University of Colorado at Boulder
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#####

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: AR5
Date: Wed, 27 May 2009 09:57:14 +0200
Cc: Jonathan Overpeck <jto@email.arizona.edu>

<x-flowed>
Hi Keith,

Nice to hear from you, and sorry to hear about your mother.

Contrary to what I heard a few days ago, I received yesterday the invitation to the Scoping meeting in July and look forward to be joining Peck in providing the paleo-input to the scoping of the report. On the issue of a separate chapter I agree that this option is most practical, yet I don't think there is solid support for that avenue,

mail.2009

and fear that it will not be pursued. This means that there is a danger that single paleo-persons distributed into the chapters might become marginalised, and would need some x-chapter support in time to deal with the issues, and also to provide enough breadth of knowledge about paleo-literature to be assessed. The Plan B option would require that there is a mechanism that pulls together the combined paleo-competence amongst the LAS during the writing of AR5. I think there will be a strong emphasis in AR5 on regional changes, and on climate predictions. Predictions need a strong basis in knowledge about natural modes of variability and the interplay between natural and man made changes on the regional scale, areas where the paleo perspectives are clearly needed and contribute. I think many new results are emerging, and we need to be on top of this to make the case in Venice. It would be great if you could, within the next week preferably, send us a list of what new results you think will be good to use for the scoping.

Cheers
Eystein

Den 21. mai. 2009 kl. 18.36 skrev Jonathan Overpeck:

> Hi Keith - thanks. Sorry to hear about your Mother.
>
> I think the invites have gone out for Venice, and so far the only
> one from
> AR4 Chap 6 going is me - or rather, I haven't heard from anyone else.
> Eystein isn't going since Norway has a bunch from the other WGs. Seems
> "representation" isn't working in our favor. I would really like
> more there,
> and I fear that if it's just me, it's another sign that paleo won't
> be a
> chapter since I can't be CLA again of such a chapter (fortunately
> for my
> family!). Based on limited discussions w/ Thomas, I also get the
> sense of a
> paleo chapter might be an uphill battle, but on the other hand, a
> conservative approach would be to stick close to the AR4 outline.
> That said,
> it appears that the gov's are pushing even harder for more regional,
> so...
>
> Your list is a big help, and I wonder if you could arm us with some
> good
> graphics where you can on these issues, especially the latest on
>
> Paleo model evaluation - showing what the models can and can't do. Of
> course, the non-paleo folks like to argue that if there is mismatch,
> it's
> the paleo data, but with the right results and presentation, that
> can be
> overcome. Need some compelling graphics that are post AR4 - if there
> are
> papers or manuscripts that's even better, but even if not at that
> stage.
>
> I'm going to guess that Gabi will be there (do you know?) and will
> do the
> sensitivity part. But, if you know of new stuff, pls send also.
>

mail.2009

> Your regional idea is a good one - want to share some compelling
> examples of
> where paleo (more than one proxy always good) is informing the full
> range of
> variability in specific regions, and illustrating ca last 50 years
> vs the
> longer record. I can think of some good examples, but you might have
> some
> recent ones I haven't seen.
>
> wegan followup - should I ask Caspar? I haven't heard anything, but
> it would
> be good...
>
> Hydrologic fits well with regional, so I think I'd emphasize it,
> although
> some temp would be good too. More on extremes? Anything out there
> that's new
> and compelling?
>
> This is just a scoping mtg, so only a small subset of those who will
> be
> involved. You need to get your gov to push you once the chapter
> outline is
> decided (i.e., you get nominated for specific roles in specific
> chapters -
> or at least that is how it worked before - suspect you know the
> drill).
>
> I'm guessing that if there is no paleo chapter, then the backup will
> be to
> have strong paleo (at least a person) in relevant chapters, with a
> cross-cutting paleo caucus or something so that the paleo Las across
> the AR5
> can work together to ensure there is consensus on things and that
> the parts
> make up a coherent and compelling whole. But, I'll be pushing for a
> chapter
> since that is clearly the best outcome. Need those compelling
> examples to
> make it work - need to show it's too much great stuff to be sprinkled
> throughout other chapters.
>
> Thanks again, Peck
>
>
> On 5/21/09 7:43 AM, "Keith Briffa" <k.briffa@uea.ac.uk> wrote:
>
>> Hi Peck and Eystein
>> sorry have not responded to recent emails re Palaeo stuff in next
>> IPCC assessment - have been away from the Unit and email because of
>> the death of my mother and ensuing issues. I simply would add that in
>> terms of pure pragmatism, efficiently stitching in Paleodata into
>> separate chapters is likely to be impractical - a self-standing
>> chapter - even of restricted length would be more feasibly achieved.
>> In terms of specific issues, top of my list would be model
>> validation progress, and a description of where we are in attempts
>> to constrain estimates of climate sensitivity with the use of
>> palaeodata - covered I know in Gab's chapter last time. Updating the
>> high-resolution work would have to be in there for continuity but
>> perhaps with an attempt to assess specific regional changes, and
>> between-proxy comparisons. If completed, "the big challenge" work
>> that arose from the wengen meeting would be good. Then "new" data -

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>> e.g. new proxies or areas not covered before - with much more on
>> hydrologic change. I agree about the inclusion of less-resolved
>> proxies. Finally, the "important issues we highlighted at the end of
>> the AR4 chapter should be reviewed and the issues updated.
>> Do you know whether the list for the scoping meeting in Venice has
>> been selected - if I have not been invited does this mean I will
>> not be?

>>
>> cheers
>> Keith

>>
>>
>>

>> --
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>>
>>

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From: Jonathan Overpeck <jto@email.arizona.edu>
To: Darrell Kaufman <Darrell.Kaufman@nau.edu>, David Schneider <dschneid@ucar.edu>, Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, Bradley Ray <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Miller Giff <gmiller@Colorado.EDU>, Otto-Bleisner Bette <ottobli@ucar.edu>, Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Your Science manuscript 1173983 at revision
Date: Thu, 28 May 2009 12:22:57 -0700

Hi Darrell et al - got a chance to read the paper and comments enroute to Atlanta. Here's some feedback..

General - comments are modest and should be easy to accommodate. That said, I think we have to take the comments of Rev 2 seriously. I'm guessing that its Francis Zwiers and in any case, he knows what he's talking about regarding stats.

Also - IMPORTANT - I'd make sure we check and recheck every single calculation and dataset.

This paper is going to get the attention of the skeptics and they are going to get all the data and work hard to show were we messed up. We don't want this - especially you, since it could take way more of your time than you'd like, and it'll look bad. VERY much worth the effort in advance.

Ok Rev 1 - wow - never had it so good.

Rev 2

General comment - we should take this one seriously. Get Caspar and Bette's help.

The new synthesis could be telling us (especially when the outlier in Fig 4B is discounted - see below) that the Arctic is, in reality, more sensitive to changes in radiative forcing than reflected in the model. Are there other experiments or reasons to think this is true? If so, let's make this point and back it up with these other pieces of evidence. For example, does the CCSM get Arctic warming from the earl/mid Holocene to present correctly? Does the model underestimate the Arctic change obs over the last 100 years. Since the reviewer raised this, you could add some refs and prose if needed to respond. Not a lot, but some.

And, we need to respond one way or the other.

Specific comments

1. agree, in the abstract, I suggest changing the sentence to read "This trend likely reflects a steady orbitally-driven reduction in summer insolation, as confirmed by a 1000-year transient climate simulation." Note that this removes more than enough words to meet the eds requirement too.
2. for this one, I'd simply state that the forcing is stronger in the Arctic than at lower lats (double check how much) and also add what Giff suggested.
3. agree, make the suggested clarification
4. important (!) and hopefully easy. I leave to whomever did the calculation to make sure

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any serial correlation bias was taken into account. Make sure all p values are thus corrected.

5. ditto, makes sense too

6. clarify

7. this reviewer knows what he/she is talking about - do what they suggest, and double check it's done well.

8. Don't delete the para. Instead point out that you've strengthened it and that it is important to place the new synthesis in a longer term Holocene context. It also clarifies to interdisciplinary readers why the Arctic is so sensitive (perhaps more sensitive than in models? - see above). That said, I would cite Kerwin et al 99 - I've attached it. It provides added detail and balance. Also, since you're responding to a reviewer comment and strengthening the ms, you can add the ref w/o hassle (or so I'm guessing on recent experience).

9. yep, delete all "attribution"s in the ms. On p 6, lone 129, can say "...support the connection between the Arctic summer cooling trend and a orbitally-driven reduction..."

10) reviewer is correct - see my response above for the general comment, and see if you can work with his/her ideas to improve. The outlier has to be just that?! Need an explanation before you can remove from any analysis, however.

11) makes sense - do it

12) yep - change text as suggested

13) agree, change p 7, line 153 to read "...1980s appears to have been the single..."

14) agree, change line 167 on p 8 to read "...trend. Our new synthesis suggests that the most recent 10-year..."

Other suggested changes....

P. 3 line 69 - change region to read regional

P 6 line 128 - "(-2600 to -1600AD) isn't going to make sense to readers. Please provide some context - SOM or ??

P 7 line 145 - insert "Arctic" before "summer"

P. 11 line 234 change to read "...century. Ten-year means (bold lines) were used..."

Because you don't really say what the bold and unbold lines are - this will help the reader make sure they have it right.

Fig 4 and caption - need to explain why the isolation axes are labeled differently - the numbers, and that both are still cover the same number of $wm-2$.

Didn't look at SOM, but make sure it's all bomber too, since there is a good chance it will get PICKED apart, and any errors thrown back in our face in a counter productive manner.

Thanks! Nice job. Best, Peck (probably w/o email for a while in the Amazon, although one never knows...)

On 5/26/09 1:08 PM, "Darrell Kaufman" <[1]Darrell.Kaufman@nau.edu> wrote:

Co-authors:

I just received the reviewers' comments and editor's decision on our SCIENCE manuscript

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(attached). The decision isn't final, but it looks like good news, with very reasonable revisions. Reviewer #1 had nothing substantial to suggest. Reviewer #2 was rather thorough. I think I can address his/her suggestions but could use some help with three:

(1) The reviewer challenged our assertion that, because climate change is amplified in the Arctic, the signal:noise ratio should be higher too. We don't have more than 1 sentence to expand on the assertion in the text. We could plead the case to editor and hope that it doesn't trip up the final acceptance, or we could omit it from the text.

Suggestions?

(2) The reviewer suggested that, if we are concerned about outliers influencing the mean values of the composite record, we should attempt a so-called "robust" regression procedure, such as median absolute deviation regression. Does anyone have experience with this?

(3) The reviewer was concerned that we overestimated the strength of the relation between temperature and insolation in the long CCSM simulation. Namely s/he criticized the leveraging effect of the one outlier in the model-generated insolation vs temperature plot (Fig. 4b), and suggested that we use 10-year means instead of 50 year.

Dave: you up for this, please?

Please forward any input to me and I'll compile them, and let you all have a look before

I submit the final revisions. I'm hoping we can turn this around this week.

Thanks.

Darrell

Begin forwarded message:

From: Lisa Johnson <[2]ljohnson@aaaas.org>
Date: May 26, 2009 12:25:40 PM GMT-07:00
To: Darrell S Kaufman <[3]Darrell.Kaufman@nau.edu>
Subject: Your Science manuscript 1173983 at revision

26 May 2009

Dr. Darrell S Kaufman
Department of Geology
Frier Hall Knoles Dr
Northern Arizona University
Box 4099
Flagstaff, AZ 86011
UserID: 1173983
Password: 307923

Dear Dr. Kaufman:

Thank you for sending us your manuscript "Recent Warming Reverses Long-Term Arctic Cooling." We are interested in publishing the paper as a Report, but we cannot accept it in its present form. Please revise your manuscript in accord with the referees' comments (pasted below) and as indicated on the attached editorial checklist

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and marked manuscript. I have also made some suggestions regarding shortening and clarification directly on the manuscript. Because of the nature of the reviewers' comments and revisions required, we may send the revised manuscript back for further review.

Please return your revised manuscript with a cover letter describing your response to the referees' comments. We prefer to receive your revision electronically via our [www](http://www.submit2science.org/revisionupload/) site ([4]<http://www.submit2science.org/revisionupload/>) using the User information above. In your letter, please also include your travel schedule for the next several weeks so we can contact you if necessary. The revised manuscript must reach us within four weeks if we are to preserve your original submission date; if you cannot meet this deadline, please let us know as soon as possible when we can expect the revision.

The cost of color illustrations is \$650 for the first color figure and \$450 for each additional color figure. In addition there is a comparable charge for use of color in reprints. We ask that you submit your payment with your reprint order, which you will receive with your galley proofs. We also now provide a free electronic reprint service; information will be sent by email immediately after your paper is published in Science Online.

Science allows authors to retain copyright of their work. You will be asked to grant Science an exclusive license to publish your paper when you return your manuscript via our revision [www](http://www.submit2science.org/revisionupload/) site. We must have your acceptance of this publication agreement in order to accept your paper. Additional information regarding the publication license is available in the instructions for authors on our [www](http://www.submit2science.org/revisionupload/) site.

I look forward to receiving your revised manuscript. Please let me know if I can be of assistance.

Please let me know that you have received this email and can read the attached files.

Sincerely,

Jesse Smith, Ph.D.
Senior Editor

[cid:3326358178_1079548]

[cid:3326358178_1100494]

Jonathan T. Overpeck
Co-Director, Institute for Environment and Society
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Embedded Content: image.png: 00000001,3e910253,00000000,00000000 Embedded

Content:

image1.png: 00000001,35902c45,00000000,00000000 Attachment Converted:
"c:\eudora\attach\kerwin_et_al&role&1999.pdf"

References

1. file://localhost/tmp/Darrell.Kaufman@nau.edu
2. file://localhost/tmp/ljohnson@aaas.org
3. file://localhost/tmp/Darrell.Kaufman@nau.edu
4. http://www.submit2science.org/revisionupload/
5. file://localhost/tmp/jto@u.arizona.edu
6. file://localhost/tmp/regalado@email.arizona.edu

979. 1244067818.txt

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From: David Schneider <dschneid@ucar.edu>
To: Darrell Kaufman <Darrell.Kaufman@nau.edu>
Subject: Re: spatial pattern
Date: Wed, 3 Jun 2009 18:23:38 -0600
Cc: Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, Bradley Ray <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Miller Giff <gmiller@colorado.edu>, Otto-Bleisner Bette <ottobli@ucar.edu>, Overpeck Jonathan <jto@u.arizona.edu>, Bo Vinther <bo@gfy.ku.dk>

I don't think we should go there. Any PC analysis on proxy data will be picked apart by the skeptics, even if it yields some useful insight, and I don't recall there being anything too exciting in the pattern given the limited amount of data.

Dave

On Wed, Jun 3, 2009 at 5:42 PM, Darrell Kaufman <[1]Darrell.Kaufman@nau.edu> wrote:

Dave and Nick:

I've been thinking about the remaining holes in the manuscript. Spatial patterns are

important. At one point we explored the spatial pattern of the PC scores. I think it

would be good to bring this up in the SOM. I could make a dot map showing the

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site locations and their correlations with PC1. The upshot would be that the proxy types are not uniformly distributed, and there are too few records to discern any spatial patterns from any geographical or proxy-type bias (e.g., high-elevation ice cores). Thoughts? Darrell

References

1. <mailto:Darrell.Kaufman@nau.edu>

980. 1245773909.txt

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From: Phil Jones <p.jones@uea.ac.uk>
To: adrian.simmons@ecmwf.int, Dick Dee <Dick.Dee@ecmwf.int>
Subject: Re: [Fwd: 2009JD012442 (Editor - Steve Ghan): Decision Letter]
Date: Tue Jun 23 12:18:29 2009
Cc: "Willett, Kate" <kate.willett@metoffice.gov.uk>, Peter Thorne <peter.w.thorne@googlegmail.com>

Adrian,

Emails to Kate yesterday were returned by the ECMWF server (for your email address) but not for Dick's?

I also found the two emails you sent last night in my spam list. No idea why this is happening. I found some other semi-important emails in my spam as well! Anyway - hope you get this email!

All three reviewers are positive, which is good, but there is still a lot of to do as you say.

Here are some initial thoughts. Before I begin - it seems as though Rev 2 comments have ended abruptly during #13. I'd suggest you ask if there is any more?

Rev 1

I would have thought that the second point (larger trends in full ERA-INTERIM fields) was

just an interesting aside, and not as important as the RH decline.

I'll need to go back to see if sections 5 and 6 can be reordered/restructured?

Both Reviewers 1 and 2 (they appear to be Kevin and Aiguo, but odd to have two people

who only live a few rooms apart!) make quite a few statements about GPCC. We're doing updating work on the higher resolution CRU-TS (0.5 by 0.5 degree lat/long) datasets. We're doing comparisons with GPCC and for the Giorgi type regions (as

in Fig 3.14 of Ch 3 of AR4) and the agreement is amazingly good. Maybe all you need to point to is this Figure and the previous one (Fig 3.12) to say that for

land regions at the continental scale, it doesn't matter which datasets are used (for

the period

from the 1970s). The key thing is that they just use gauges, with no satellites.

My view is that bringing in satellites as in CMAP and GPCP products can lead to same

satellite data products. The point to emphasize for precip is that GPCC is

totally independent from any ERA (40 or Interim) input.

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I've come across these issues about GPCC before. I've been haranguing Bruno Rudolf and now Tobias Fuchs of GPCC to write something up for a number of years within AOPC! I think their QC is likely the best of all the centres, but they will continue to get these doubts if they don't write anything up. They should at least explain how they do their interpolation - it can certainly be done better. GPCC is using so much more data that it has to be better than any other product. They can't release the raw station data, and it seems they can't release the numbers in each grid box. There will be an HC paper on the buoy/ship SST issue, but this isn't yet used operationally. It will come, but not before your paper goes back. I hope it is fairly straightforward to do RMSSs as well as correlations. We had SDS in the 2004 paper. I don't think RMSSs would show anything untoward, but would take up some more space. WRT Rev 2, I'm not that convinced by some of Aiguo's arguments. Between us, I'm not that convinced by some of his data analyses. The ones involving PDSI leave a lot to be desired (this is coming to light in other work we are doing). Rev 2 #6 Obviously not read the paper(s). CRUTEM3 is a simple average of stations within a grid box. There is no interpolation! If there are no stations, then there is no value! I think this is the same for HadCRUH as well. Rev 2 #13 Comment seems to end abruptly. I'd like to know what I might have said! I don't think I've ever said I doubt GPCC! I am around all the time except for the week of July 12-17, when I'll be at the IPCC Scoping meeting in Venice. Kevin will be there as well. Aiguo will be in CRU the first few days of the week after (July 20/21) Cheers Phil

At 22:53 22/06/2009, Adrian Simmons wrote:

Dick
It's a bit irritating getting a review one wants to nail just before leaving for Brussels for three days of EC-related meetings. I'm sure now that reviewer 2's comments on SYNOP numbers is easily answered. The number of GTS SYNOPS went up a lot, but that's not because there were a lot more stations installed - the existing one just started having their data transmitted more frequently than 6-hourly. But this should hardly have effected the RH2m analysis as it uses only the 0, 6, 12 and 18UTC obs that have been there pretty well all the time. It only uses off-time obs if the value for the main synoptic hour is missing. The 4D-Var does assimilate more data over time, but here we appeal to fig 8 and argue that the increment does not shift over time. We already argue in the Appendix that the extra obs over North America may well be part of the difficulty HadCRUHext has for that region. Anyway I'd like to confirm that the number of used SYNOPS does not change much

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over time
I don't know how to retrieve the job output from the logfiles stored in ECFS. I would only look at a few samples. I'd be grateful if you'd let me know how to do this.
In any case even if there was a problem with the numbers increasing sharply around 2000, this would manifest itself in a sudden drop in the RH time series, not a steady decline over the last few years.
After a bit of thinking I can find several things wrong with reviewer 2's argument why q over land is insensitive to variations in q over sea (think coastal mountain ranges, deserts, drought regions - moisture does not simply build up everywhere over land via onshore winds from the boundary-layer until it rains), and the response can draw attention to other points made in the paper, such as the coherence of changes in the vertical, and the similarity (but lag) of the q series over land and sea. Hard to believe the latter is all coincidence.
Also, there is a relationship between q and precip, not generally strong, but there's a high correlation for Australia.
Better stop for now.
Adrian

----- Original Message -----

Subject: 2009JD012442 (Editor - Steve Ghan): Decision Letter

Date: Mon, 22 Jun 2009 16:42:51 UT

From: jgr-atmospheres@agu.org

Reply-To: jgr-atmospheres@agu.org

To: adrian.simmons@ecmwf.int

Manuscript Number: 2009JD012442

Manuscript Title: Low-frequency variations in surface atmospheric humidity, temperature and precipitation: Inferences from reanalyses and monthly gridded observational datasets
Dear Dr. Simmons:
Attached below please find 3 reviews on your above-referenced paper. One of the reviewers has raised questions and made suggestions for important revisions, mostly involving organization and presentation. Please consider the Reviewer reports carefully, make the necessary changes in your manuscript and respond to me, explaining how you have addressed these comments. In your Response to Reviewer letter, please include a statement confirming that all authors listed on the manuscript concur with submission in its revised form.
The due date for your revised paper is July 20, 2009. If you will be unable to submit a revised manuscript by July 20, 2009, please notify my office and arrange for an extension (maximum two weeks). If we do not hear from you by the revision due date, your manuscript will be considered as withdrawn.
When you are ready to submit your revision, please use the link below.
*The link below will begin the resubmission of your manuscript, please Do Not click on the link until you are ready to upload your revised files. Any partial submission that

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sits for 3 days without files will be deleted.

<[1]http://jgr-atmospheres-submit.agu.org/cgi-bin/main.plex?el=A5Bc4EasP6A2oLJ3I6A9jNWgL

zbgfwly58nFGPxNeQZ>

(NOTE: The link above automatically submits your login name and password. If you wish to share this link with co-authors or colleagues, please be aware that they will have

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Please put

your color option in the cover letter.

[2]http://www.agu.org/pubs/e_publishing/AGU-publication-fees.pdf

Sincerely,

Steve Ghan

Editor, Journal of Geophysical Research - Atmospheres

-----Important JGR-Atmospheres Information-----

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on the If you need Adobe Acrobat Reader to download the forms, it is available, free,

internet at: [3]http://www.adobe.com/prodindex/acrobat/readstep.html

Reviewer Comments

Reviewer #2 (Comments):

Review of JGR Manuscript entitled

precipitation: Low-frequency variations in surface atmospheric humidity, temperature and

inferences from reanalyses and monthly gridded observational data sets

by A.J. Simmons, K.M. Willett, P.D. Jones, P.W. Thorne, and D. Dee

General comments:

This paper provides a nice and useful summary on how the ERA-40 and ERA-Interim surface

analysis products of temperature and humidity were derived, and a fairly comprehensive

evaluation/comparison with the HadCRU surface data sets derived purely from surface

observations, as well as with three other precipitation products. They found that in

general the ERA surface temperature and humidity data from 1973 onward are in close

agreement with the HadCRU data sets and that ERA precipitation also follows closely with

gauge-based products, although long-term changes differ. Furthermore, the study

reports

a significant and steady decline in surface relative humidity (RH) over land from ~1999-2008 and suggested that the recent steady SSTs might be responsible for this land RH decrease. The manuscript is well written, the analysis appears to be comprehensive, and the results are of interest to many readers in the climate community. I think the paper should be published after some relatively minor revisions.

My main concern is the interpretation of the recent RH decline over land. To me, the RH decreases shown in Fig. 4 and Fig. 7 look a bit spurious (non-climatic, e.g., lack of variations in Fig. 4 and stepwise changes in Fig.7) rather than realistic changes. They are also inconsistent with the RH changes during recent decades (up to 2004) reported in Dai (2006, JC), and this is not pointed out in the paper. As shown in Dai (2006), there was a 3-fold increase around the late 1990s in the number of surface humidity reports (mostly in North America but also over some other regions) included in the WMO SYNOP GTS reports. Furthermore, I personally found that there were other (undocumented) changes in the SYNOP reports around that time that led to shifts in derived precipitation and cloud frequencies over Euroasia and other places. Thus, there are reasons to suspect some non-climatic changes in the SYNOP reports around the late 1990s that might alter the RH trend over land.

I also was not convinced by the physical explanation of the RH decline (p.23). Even if the surface q stayed the same over the oceans during the 1999-2008 period when land air temperature has been increasing, this can not explain the RH decrease over land. This is because as long as the marine air contains more water vapor than continental surface air (which is still true even if marine sfc. q did not increase), advection of marine air onto land should cause land q to accumulate and RH to increase until the land q and RH reach certain levels so that precipitation kicks in to remove the moisture over land. Remember that the atmospheric moisture storage (PW) is very small compared with the annual P and E fluxes, thus any perturbation in RH is quickly (within days) restored through surface E, vertical mixing, or lateral advection/mixing. If the RH in the marine air had decreased, then land RH would likely to decrease too. Dai (2006) did not show RH decreases over oceans since the 1980s. I wish the authors of this paper would also show RH series over ocean, at least since the middle 1980s.

For the ERA humidity data, the large well-known inhomogeneities in radiosonde humidity

records will certainly propagate into the ERA background forecast and its analysis fields, making them not really suitable for long-term trend analyses. For example, all U.S.-operated radiosonde records (including many in the Pacific) before about Oct. 1993 report a dew point depression (DPD) of 30deg.C or a RH of 20% for any cases where RH is below 20%, which resulted in an abnormally higher frequency of reports of DPD=30deg.C and few reports below and no reports above DPD=30deg.C. This practice is also found in some Mexican, Canadian, Australian, and few other places (but stopped at different times from the late 1980s to the 1990s). In general, the newer humidity sensors during the last 10-15 years report more low RH or large DPD cases, whereas earlier ones had no measurements or incorrect values for these cases. One can see this shift in the histograms of daily DPD made by different humidity sensors. Thus, one needs to be very cautious when radiosonde humidity data are used in assessing trends, even if they are used indirectly (as in the ERA surface humidity analysis).

Some other comments:

1. Abstract: it gives the impression that even the long-term mean values for surface T, q and RH are the same between ERA and HadCRU data sets, which appears to be not the case as the respective means are removed in all plots. Please mention that the climatological mean may differ (if this is the case) even though the anomaly variations are similar.

2. Abstract, at the end: Please note that the mean precipitation amount and its change rate are not controlled by atmospheric water vapor amount (q), although higher q is often associated with higher P (e.g., tropical vs. high latitudes). Locally, you can have moist air passing by without any rain. Globally, annual P is controlled by how much moisture gets evaporated from ocean and land surfaces (i.e., $P=E$), and this surface E is primarily controlled by surface energy terms. In essence, P and E are water fluxes, and PW (or q) is the water storage in the atmosphere. People often link P to q because of the associated mentioned above (through low-level moisture convergence in a storm, etc.), and think that P change rates somehow should follow that of q or PW. However, and P (or E) and q are controlled by different processes and in general the flux terms are not coupled with the storage terms in a cycling system (e.g., no one would think P or E is controlled by water storage in the ocean).

3. p. 3, top: the net radiative effect of clouds is relatively small, when their effect on solar radiation is included. To include clouds in the natural greenhouse

warmth is a bit misleading because the higher surface temperature is maintained primarily by the greenhouse effect of water vapor and CO2.

4. p. 4, middle: Again, any sampling/reporting biases in WMO SYNOP reports could affect both ERA and HadCRUH humidity data. Thus caution is still needed.

5. pp.5-6, section 2a: So in essence, ERA-40 and ERA-Interim surface T, q, and RH are another analysis product based on surface observations, just like the HadCRU and other climate data sets. The only difference is in the analysis methodology (IO interpolation with the use of the ERA background forecast fields vs. other more conventional analysis methods). Like most users, I thought the ERA surface fields are more tightly coupled with the reanalysis model system. I think it would be helpful to point out the above at the beginning of this section or in the Introduction.

6. p. 7, top: Please briefly mention how the station anomalies were aggregated onto 5deg. grid in CRUTEMP3, e.g., by simply averaging station values within the grid box, or making use of correlated, nearby station data outside the box when sampling inside the box is sparse? I think most people would use the later to increase the coverage in the gridded products.

7. p. 7, bottom: Have any adjustments/corrections done for the most recent decades (1999-2008) in HadCRUH+ext? This is the period when RH decreases. Are there any homogeneity issues in combining the extended records with the homogenized HadCRUH?

8. p. 9, top: How could the fit of the ERA background forecasts capture multiple shifts induced by instrumental changes or reporting practices, especially when the future changes are needed to determine the timing and the size of a shift. Many statistical methods specifically designed to do these two tasks by analyzing the whole historical series still have difficulties in reliably detecting the locations of shifts and can only make a best guess regarding the real shift size. I wonder how one can do this in a reanalysis system when future records are not used yet, or nearby station series are combined together to form a grid box series that contain shifts from multiple stations (i.e., the stepwise patterns become very complex and look more like real variations).

9. p. 9, middle: I can't believe the GPCP people are still gridding precipitation total, not anomalies. This makes their products useless for long-term change analyses. Another land precipitation product from 1948-present that is derived from gauge records and the OI method is the PRECL from the NCEP Climate Prediction Center (CPC, ref: Chen et al. 2002, J. Hydrometeorol.). I think that is a better products for assessing long-term changes in land precipitation, although the gauge coverage for recent years

(after 1997)

may be not as good as that of the GPCC.

10. p. 11, middle and bottom: need to point out in Abstract or Summary that differences

in the mean exist between the ERA and HadCRU T and humidity data.

11. Fig. 1 and other Figures: I suspect that different mean values were removed in

computing the difference series. If that's the case, then need to point out this (i.e.,

the difference is between the anomalies relative to their respective mean).

12. Fig. 4: also show RH over the oceans for the last 25 years?

13. Fig. 11: with the changing gauge coverage and gridding precipitation total, one can

not trust the low-frequency variations in the GPCC products. Phil Jones and other have

Reviewer #3 (Comments):

Review of the paper entitled "Low-frequency variations in surface atmospheric humidity,

temperature and precipitation: Inferences from reanalyses and monthly gridded observational dataset" by A.J. Simmons, K. M. Willett, P. D. Thorne and D. Dee.

Recommendation: Accept with minor changes.

Summary of the paper:

This is an elaborate study examining trends in temperature, humidity and precipitation

from the latest ECMWF reanalysis, comparing with independent gridded analyses, which are

also performed with utmost care. The paper revealed that the commonly accepted assumption that the relative humidity stays the same under global warming

condition does

not necessarily holds over land. This is an important finding and should be of interest

to wide climate communities. There are several other important contributions, such as

the sensitivity of observation coverage on long term trend, which can only be studied by

the use of reanalysis that has full global coverage. This paper also presents that the

ERA-40 and ERA-Interim are of very high quality and useable for low frequency climate

studies.

Major comments:

1. I am particularly impressed with the way the work is performed. This is a very

elaborate work using a variety of datasets to present that there is a strong long time

trend in temperature and humidity. This thorough work made it possible to convince

readers these observed facts. Although the finding of the decrease in relative humidity

over land is credible, it may be more meteorologically interesting and convincing if

additional analysis is made to present the possible mechanisms of the absence of

increase in specific humidity over land. If reanalysis is used, it is not impossible to

estimate the change in the moisture transport into land areas (although this may involve

considerable amount of work). It may also possible to examine the change in large scale

mean land-ocean circulation that contributes to the transport of moisture. From heuristic point of view, stronger heating over land tends to strengthen upper

level high

and subsidence, which may prevent moisture to be

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transported inland, and such trend may be detectable from large scale reanalysis. In terms of the change in precipitation, moisture availability and relative humidity are important, but static stability and large scale convergence should also play an important role. If any of these additional analyses can be performed, or even discussed in qualitative manner, it will enhance the paper.

2. It is not very clear how the diurnal variations of temperature and humidity are handled in this study. It is helpful to state the time frequency of reanalysis output that is used to compute daily mean, and the way observed daily mean are obtained.

3. Are there any reason that the relative humidity or dew point depression is analyzed and not the specific humidity itself?

4. The paper is a little too long. One way to shorten it is to separating it into two parts by adding analysis suggested above, or separating the analysis of precipitation. This is just a suggestion and decision is up to the authors.

Minor comments:

1. Page 6 & 11. The authors claim that the use of anomaly will reduce the influence of surface elevation differences. Can this be true even the relation between elevation and relative humidity/specific humidity is very nonlinear?

2. It may be friendlier to the reader why relative humidity and specific humidity are both examined. Some introductory remarks on the different impact of relative and specific humidity will help.

3. Page 13. Lines 298-300. These lines just present why the ERA-40 and Interim are different but not the reason for the ERA-Interim worse than ERA-40 over Africa.

4. Page 14. Lines 316-328. Is it possible to separate the actual reduction in the number of observations and the reduction in data used by CRUTEM?

5. Page 15. Line 364. It seems that the difference in analysis between ERA-40 and ERA-Interim seems to be used as a measure of the reanalysis accuracy. Is this a good assumption?

6. Page 17. Lines 392-397. Can it be possible to mathematically estimate the relation between the correlation of specific humidity and relative humidity? Since relative humidity is a function of specific humidity, temperature and pressure, it seems natural that the correlation for relative humidity should be lower. However, this will depend on which parameters are analyzed in the first place.

--

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Norwich Email p.jones@uea.ac.uk
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References

1. <http://jgr-atmospheres-submit.agu.org/cgi-bin/main.plex?el=A5Bc4EasP6A2oLJ3I6A9jNWGLzbgfWly58nFGPxNeQZ>
2. http://www.agu.org/pubs/e_publishing/AGU-publication-fees.pdf
3. <http://www.adobe.com/prodindex/acrobat/readstep.html>

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#####

From: "Nick Pepin" <nicholas.pepin@port.ac.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Re: Fwd: CRU surface temperature dataset
Date: Thu, 25 Jun 2009 10:59:26 +0100

Phil

Thanks for this great detail. I am thinking that probably a raw radiosonde dataset may be better (I tried this before using the LKS dataset but station density was an issue and only ended up with around 20 station pairs) - it sounds as though things have improved dramatically in that area and will look at the sources you suggest. My hope is that at least I can find hundreds/thousands of stations near to my high elevation surface ones for comparison. If not I could interpolate spatially maybe between radiosondes to my surface sites since free-air climate (not meteorology) should be relatively smooth in space. I cannot interpolate between surface stations.

I agree that reanalyses can be a can of worms (esp NCEP/NCAR)!

As for the surface I'll also look at the site you suggest and get back if I have any Q/problems. I appreciate the time you have taken to answer some of my Q!

Best wishes

Nick

>>> Phil Jones <p.jones@uea.ac.uk> 24/06/2009 13:09 >>>

Nick,

I don't want to put off, but there is an awful lot of things wrong with NCEP/NCAR.

They are probably OK for month-to-month variability, but if you look at some of the figures in Simmons et al (2004) you'll see that for trends they are practically useless before 1979.

There is just so much wrong with the sondes which together with the introduction of satellite data in 1978/9 makes reanalyses awful.

The Simmons paper is about how much better ERA-40 is than NCEP/NCAR.

It is also telling you that you shouldn't be using NCEP/NCAR for trends - and ERA-40 is only OK in Europe and North America.

A group of us are hopeful of getting an EU project funded to go through the

Reanalysis input - surface and sonde. The aim is to put in all the homogenised

surface and sonde data, so giving reanalysis better data input -

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and putting back all the data that missed the real-time cut. I'm not sure you're aware that no back data have ever got into the reanalyses. If data doesn't make the cut in real time, it can never get in later. The reanalysis source input doesn't collect back data!

You'd be better off getting one of the newer sonde datasets. HadAT2 although developed in 2005 is beyond it's sell-by date. Have a look at the attached and this web site

<http://homepage.univie.ac.at/leopold.haimberger/leoweb/index.html>

Ra-ob core version 1.4 is the latest.

The drop off in surface data isn't the fault of GHCNV2. The folks in Asheville are doing all they can to get additional datasets. Currently about 2000 sites are exchanged in real time.

If the sites you want are not exchanged by Met Services in real time we can't get access to them except by asking each Met Service and/or waiting till the next volumes of the 10-year books (for 2001-2010) get released.

CRUTEM3 has some additional station data going in for Australia and Canada, but apart from this we will have nothing more than GHCNV2. We could get a load more from the US quite easily, but coverage is reasonable there compared to the rest of the world.

GHCNV2 and ourselves have lots of historic series, but these aren't updatable in real time, without continuous effort. Lots of projects were funded in the US and Europe in the 1980s and 1990s to get loads of data digitized, homogenized and accessible.

It is possible to do things with daily data (SYNOPS) but these are only generally good enough for the good countries.

http://www.dwd.de/bvbw/appmanager/bvbw/dwdwwwDesktop/?_nfpb=true&switchLang=en&_pageLabel=_dwdwww_klima_umwelt_datenzentren_gsnmc

This site has what is available in real time - since 2001. This site can be very annoying. There is a link back to NCDC.

Cheers
Phil

Cheers
Phil

At 17:48 23/06/2009, you wrote:

>Phil

>

>Many thanks for your reply. This is very helpful, esp the Simmons paper.
>I am aware there are issues with reanalyses although I do want to
>try and use data representative of free air (and not contaminated

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>with surface obs)- hence NCEP/NCAR rather than ERA-40 maybe, and use
>of pressure level data rather than 2 m or surface reanalysis temps
>(which I think the Simmons paper is about). I don't want the
>reanalysis to respond to surface issues and want it to be
>independent (purely based on radiosonde and satellite coupled with modelling).
>Of course this doesn't make the points irrelevant and I am looking
>at these while deciding what to use.

>
>As regards surface data, I am interested in the Tmean you mention
>used for CRUTEM3. Is this available and for how many stations?
>GHCNV2 is not good after 1990 since many stations stop! It is
>particularly dire after 2005 as you may realise? Please let me know
>what you think?

>Best wishes and thanks for your help re this.

>
>Nick

> >>> Phil Jones <p.jones@uea.ac.uk> 22/06/2009 10:38 >>>

> Nick,
> I was away when your earlier message came in March, and I must have
> forgotten it when I got back to Norwich.
> We generally only put the gridded data on the web site. The
>station data that
> goes into CRUTEM3 is only monthly mean temperature. It is only
>since the mid-1990s
> that countries have routinely exchanged monthly mean Tx and Tn
>data. Many countries
> don't use these data to calculate mean T, instead using their
>historical methods based
> on fixed hours.
> We do have an archive of historic Tx and Tn (monthly) but this
>is almost entirely
> based on GHCNV2 sources. We use these data in products like this paper

>
>Mitchell, T.D. and Jones, P.D., 2005: An improved method of
>constructing a database of monthly climate observations and
>associated high-resolution grids. Int. J. Climatol. 25, 693-712.

>
> When you compare with Reanalysis trends you want to consider
>looking at ERA-INTERIM
> available from 1989-2008. There are also longer reanalysis products
>developed by NOAA
> (Gil Compo) from surface station data only (i.e. no sondes and no
>satellites, so
> consistent through time).

>
> Are you aware of this paper? Basically reanalyses will be wrong
>before 1979 - except possibly
> in Europe and North America. This paper has the reasons why
>reanalyses will be wrong.

>
> Cheers
> Phil

>
>At 15:06 17/06/2009, you wrote:

> >Dear Prof. Jones
> >You maybe had forgotten that I e-mailed you a while ago (March)
> >asking about access to data for surface stations for work on

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> >temperature trends in complex topography (original e-mail and
> details below).
> >Since then I have been awarded a Royal Society Travel Grant to do
> >some work on this in the U.S. and I will be examining the GHCNV2
> >dataset in detail (which I have). I would really like to be able to
> >include a CRU dataset as well, since I did this in my original
> >research and these datasets are highly regarded.
> >If you are not the correct person to ask, maybe you could guide me
> >to the right person!
> >Many thanks for your reply.
> >Best wishes
> >Nick Pepin
> >
> >
> >>> Nick Pepin 09/03/2009 16:43 >>>
> >Dear Prof. Jones
> >You may remember that a few years ago (2005) I published a paper
> >with Dian Seidel looking at temperature trends at high elevation
> >surface stations and comparing them with reanalysis trends. I wish
> >to update this work as part of another project, and was looking on
> >the UEA website to see if any of the original stations have been
> >updated. It is important that they are homogeneity adjusted as much
> >as possible.
> >
> >It appears that nearly all of the datasets available on the web are
> >gridded and therefore interpolated (which I don't want since
> >interpolation influences what I am examining). Are any of the 3000
> >approx original stations available (mean monthly maxima and minima
> >are good enough) which are used to create CRUTEM3 etc?
> >
> >In my original analysis I combined data from the CRU station dataset
> >and GHCN (some stations were in both) and I would like to do the
> >same again if possible. This is part of work looking at the effect
> >of topography on temperature trend patterns on a global scale (it
> >will be more detailed than preliminary work on this in the attached paper).
> >
> >Many thanks for your help
> >Best wishes
> >Nick Pepin
> >
> >
> >
> >Prof. Phil Jones
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> >School of Environmental Sciences Fax +44 (0) 1603 507784
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>

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From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Skeptics
Date: Thu, 25 Jun 2009 11:19:45 -0400
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Hi Phil,

well put, it is a parallel universe. irony is as you note, often the contrarian arguments are such a scientific straw man, that an effort to address them isn't even worthy of the peer-reviewed literature!

mike

On Jun 25, 2009, at 10:58 AM, Phil Jones wrote:

Mike,

Just spent 5 minutes looking at watts up. Couldn't bear it any longer - had to stop!. Is there really such a parallel universe out there? I could understand all of the words some commenters wrote - but not in the context they used them.

It is a mixed blessing. I encouraged Tom Peterson to do the analysis with the limited number of USHCN stations. Still hoping they will write it up for a full journal article.

Problem might be though - they get a decent reviewer who will say there is nothing new in the paper, and they'd be right!

Cheers

Phil

At 15:53 24/06/2009, Michael Mann wrote:

Phil--thanks for the update on this. I think your read on this is absolutely correct. By the way, "watts up" has mostly put "ClimateAudit" out of business. a mixed blessing I suppose.

talk to you later,
mike

On Jun 24, 2009, at 8:32 AM, Phil Jones wrote:

Gavin,

Good to see you, if briefly, at NCAR on Friday. The day went well, as did the dinner in the evening.

It must be my week on Climate Audit! Been looking a bit and Mc said he has no interest in developing an alternative global T series. He'd also said earlier it would be easy to do. I'm 100% confident he knows how robust the land component is.

I also came across this on another thread. He obviously likes doing these sorts of things, as opposed to real science. They are going to have a real go at procedures when it comes to the AR5. They have lost on the science, now they are going for the process.

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Cheers
Phil
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<McIntyre_Submission_to_EPA.pdf>

--
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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
website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.htm>
"Dire Predictions" book site:
[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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University Park, PA 16802-5013
website: [7]<http://www.meteo.psu.edu/~mann/Mann/index.htm>
"Dire Predictions" book site:
[8]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. <mailto:p.jones@uea.ac.uk>
2. <mailto:mann@psu.edu>
3. <http://www.meteo.psu.edu/~mann/Mann/index.htm>
4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
5. <mailto:p.jones@uea.ac.uk>
6. <mailto:mann@psu.edu>
7. <http://www.meteo.psu.edu/~mann/Mann/index.htm>
8. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

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#####

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Luterbacher Jürg <juerg.luterbacher@giub.unibe.ch>
Subject: Re: IPCC Fig. 6.10
Date: Wed Jul 1 10:31:36 2009

Hi Juerg,
At 21:56 16/06/2009, you wrote:

I hope you are very well. Douglas arrived safely here and hopefully he will be starting officially soon. I am looking very much forward having him here and of course working together with you on different topics!

Yes, that sounds great to me too.

I have a chinese paleo climatology researcher (Zhinxin Hao) with me for a couple of weeks. She is working on the comparison with different chinese long temperature reconstructions and would like to present a similar figure as in the IPCC Fig 6.10. Keith told me that he might not be able to work for the next time, so I thought I could address this issue to you as you were also much involved.

That's fine. Indeed I designed and drew the figure.

She asked me if I could ask you whether you could have a look at the attachment where she tried to explain how she calculated and plotted the curves for China. As she did not fully understand the way it was done in the IPCC report, would you mind having a look at the text and let me know if she applied it correctly?

It is a little hard to follow (some symbols got replaced by squares -- perhaps a PDF file would work better than a Word Doc?) but I think that the method looks approximately right but not quite right. Some things that look a bit different:
Se: it appears that the same value is used for all 4 reconstructions (in the example, Se=1.3165 is used). Why would the uncertainty on one reconstruction be the same as the uncertainty on all the others? Perhaps she has used the standard deviation of the instrumental temperature rather than the standard error of each reconstruction? Did the authors actually publish estimated uncertainties along with their best-estimate reconstruction series? You should also note that reconstruction errors/uncertainties may depend on time scale -- the IPCC fig 6.10 showed variations on timescales of 30-yrs and longer, so I attempted to use uncertainties estimated for that timescale (or a similar multi-decadal timescale). IPCC wanted to mostly standardise on the 90% range (5%-95%), so for my scoring I awarded 100%/N to any temperature that falls within the +- 1 SE reconstruction range (the same as noted in her document) but awarded 0.5*100%/N to any temperature that falls within +- 1.6448 SE reconstruction range (this differs from the +-2 SE in her document). I

mail.2009

originally used ± 2 SE, but (under assumption of normality), ± 1.6448 SE should encompass 5%-95% range, while ± 2 SE is of course approx 2.5%-97.5%. Either is of course equally defensible, but if you want to reproduce IPCC, then its ± 1.6448 SE for the half score ($0.5 \cdot 100\% / N$). This is of course repeated for all N reconstructions. I was a little unsure about the actual plot produced too. When the Xu2003 curve is very low or very high, the brown shading extends in both directions (to very low *and* very high values at once). e.g. AD 650 (but there are others too). Also the range is very narrow at about AD 1050; although the 3 recons are quite similar here, it still looks too narrow, especially when you add on the reconstruction SE (and ± 1.6448 SE or ± 2 SE). Hope this helps,
Tim

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#####

From: Tim Osborn <t.osborn@uea.ac.uk>
To: haozx@igsnr.ac.cn
Subject: Re: =?gb2312?B?Rnc6IFRpbXMgQW5zd2Vy?=
Date: Wed Jul 1 16:17:28 2009
Cc: Luterbacher Jürg <juerg.luterbacher@giub.unibe.ch>

Dear Zhixin,
At 15:14 01/07/2009, you wrote:

Do you mean se should be the standard error from the individual reconstruction series

yes, that's what I mean.

(before I got your answer, I calculated the standard error for the 5 reconstruction data at one time point, e.g. 1470s, it is not from the original papers given by the authors)?

Ah. I understand what you've done now.

But my question is if the author did not publish the uncertainty, how can I deal with the value of se?

well, the original purpose of constructing IPCC Fig. 6.10c was to display the published uncertainty estimates of each study. If no uncertainties had been estimated by the original authors then we wouldn't have produced the figure in the first place! So, do you really want to produce such a figure to show the uncertainty ranges when the uncertainty ranges haven't been calculated before? If you do, then you'd need to somehow estimate the uncertainty. You could do this yourself, perhaps, e.g. from the differences between each reconstruction and the instrumental temperatures during some overlap (calibration, or independent

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verification)

period? But this wouldn't measure any increase in uncertainty during periods when each

reconstruction is perhaps based on less input proxy data.

Estimating the uncertainty from the spread of individual reconstruction values in a

particular year, like you've done, is open to criticism. Do you really think that in a

particular year when the three recons have very similar values that the uncertainty is much

less than other nearby years? If you had a high number of

And now I understood the meaning of 5%-95% range, I will follow this, and replot my

figures with $\pm 1.645SE$ for the half scores.

Thank you very much again, hopefully I can give the uncertainty of reconstruction

results over China region soon. After finished, may I send the manuscript to you and

give us comments and suggestions?

Best wishes,

Zhixin

----- Original Message -----

From: "Juerg Luterbacher"

To:

Subject: Tims Answer

Sent: Wed, 1 Jul 2009 12:27:44 +0200

here is the answer of Tim.

cheers maybe you can now email him directly to make things clear

cheers

Juerg

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file

would work better than a word Doc?) but I think that the method looks approximately

right

but not quite right. Some things that look a bit different:

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same as the Se=1.3165 is used). Why would the uncertainty on one reconstruction be the

the uncertainty on all the others? Perhaps she has used the standard deviation of

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Did the

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as

noted in her document) but awarded $0.5 \cdot 100\%/N$ to any temperature that falls within \pm

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 I originally used +- 2 SE, but (under assumption of normality), +- 1.6448 SE
 should encompass 5%-95% range, while +- 2 SE is of course approx 2.5%-97.5%. Either
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 and very high values at once). e.g. AD 650 (but there are others too). Also the range
 is very narrow at about AD 1050; although the 3 recons are quite similar here, it still
 looks too narrow, especially when you add on the reconstruction SE (and +- 1.6448 SE or
 +- 2 SE).
 Hope this helps,
 Tim

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From: Tim Osborn <t.osborn@uea.ac.uk>
 To: haozx@igsnr.ac.cn
 Subject: Re: =?gb2312?B?Rnc6IFRpbXMgQW5zd2Vy?=
 Date: Wed Jul 1 16:19:39 2009
 Cc: Luterbacher Jürg <juerg.luterbacher@giub.unibe.ch>

Dear Zhixin (cc Juerg),
 At 15:14 01/07/2009, you wrote:

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yes, that's what I mean.

(before I got your answer, I calculated the standard error for the 5 reconstruction data at one time point, e.g. 1470s, it is not from the original papers given by the authors)?

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But my question is if the author did not publish the uncertainty, how can I deal with the value of se?

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And now I understood the meaning of 5%-95% range, I will follow this, and replot my figures with $\pm 1.645SE$ for the half scores. Thank you very much again, hopefully I can give the uncertainty of reconstruction results over China region soon. After finished, may I send the manuscript to you and give us comments and suggestions?

Yes, that would be fine.
Tim

986. 1247199598.txt

#####

From: "Tim Osborn" <t.osborn@uea.ac.uk>
To: I.Harris@uea.ac.uk
Subject: cruts tmp to 2008
Date: Fri, 10 Jul 2009 00:19:58 +0100 (BST)
Reply-to: t.osborn@uea.ac.uk
Cc: "tim Osborn" <t.osborn@uea.ac.uk>

Hi Harry,

finally had time to take a look at the latest cruts3 run through to 2008 for tmp, picked up from /cru/cruts/

Two PDFs showing seasonal national means are attached.

Look at ...2008a_vs_2008b.pdf first. Black is your previous update to 2008, pink is the latest one. Many very similar, some small differences (presumably due to outlier 3/4 SD removal... note that as these are national/seasonal means, outliers might be quite large, yet only show up small in the means if many other stations contribute).

page 4. The hot spike in Guatemala SON has been removed in the new version. That looks much better.

page 6 & page 9: the hot spikes in France, Italy and Austria in JJA in 2003 have been reduce slightly too. Not sure if this is right or not, could ask Phil what he thinks. Could Jul & Aug 2003 have been so hot that

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some observations validly did exceed the +3SD outlier check? Or do you use a +4SD check for TMP? Anyway, this is one to ask Phil about.

There are various other erroneous hot spikes that have now been correctly removed, I won't list them all here.

However, there are some cold spikes in both previous and latest 2008 updates... see e.g. Mali SON on page 12. Have you turned on only outlier checking for +3SD, and not for -3SD? Some wrong-looking cold spikes are still present.

Now look at ...2005_vs_2008b.pdf. Black is last years CRUTS3 through to 2005 (I know the files went to mid 2006, but I stopped at last complete year). Note this isn't CRUTS2.1! :-). Pink is again the newest version of the update to 2008.

There are some early 20th century differences that I'm not too bothered about, though it would be nice to know why they arise. One concern is that the mean level is different between the versions... see e.g. JJA for various countries on pages 7 and 8. Seems to be a constant offset. It's too big to be a simple rounding error in my calculations (I may have changed from 1 dec. place to 2 dec. place, but some differences are about 0.5 deg C), and these are absolute values so there's no dependency on any anomalisation/reference period meaning as I'm not doing any.

Intriguing. Perhaps some normals have change in some regions/seasons?

So:

- (1) hot spikes have been corrected.
- (2) cold spikes still there.
- (3) some odd differences in mean level.

Progress!

Tim

--
 Dr. Tim Osborn
 RCUK Academic Fellow
 Climatic Research Unit
 School of Environmental Sciences
 University of East Anglia
 Norwich NR4 7TJ, UK
 www.cru.uea.ac.uk/~timo/

Attachment Converted: "c:\eudora\attach\idl_cruts3_2008a_vs_2008b.pdf"

Attachment Converted: "c:\eudora\attach\idl_cruts3_2005_vs_2008b.pdf"

987. 1248785856.txt

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From: Kevin Trenberth <trenbert@ucar.edu>
 To: Phil Jones <p.jones@uea.ac.uk>
 Subject: Re: ENSO blamed over warming - paper in JGR
 Date: Tue, 28 Jul 2009 08:57:36 -0600
 Cc: Michael Mann <mann@meteo.psu.edu>, Jim Salinger <j.salinger@auckland.ac.nz>, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>,
 Page 124

mail.2009

James Annan <jdannan@jamstec.go.jp>, Grant Foster <tamino_9@hotmail.com>

The leads and lags are analyzed in detail in this paper
Trenberth, K. E., J. M. Caron, D. P. Stepaniak, and S. Worley 2002: [1]The
evolution of
ENSO and global atmospheric surface temperatures J. Geophys. Res., 107, D8,
10.1029/2000JD000298.
and we were not able to reproduce Tom Wigley's result (we tried). It may depend
in indices
used. In this paper we also document the extent to which ENSO contributes to
warming
overall.
Kevin
Phil Jones wrote:

Mike,

See below for instructions.

Also, just because IPCC (2007, Ch 3) didn't point out the 6/7-month lag
between the SOI and global temperatures doesn't mean it hasn't been
known for years. IPCC is an assessment and not a review of everything
done. If they had even read Wigley (2001) they would have seen this
lag pointed out. I wasn't the first to do this in 1989 either. I don't
think Walker was either. I think the first was Hildebrandsson in the
1890s. Why does it always go back to a Swede!

file is at [2]ftp.cru.uea.ac.uk

login anonymously with emails as pw

then go to people/philjones

and you should find santeretal2001.pdf

Cheers

Phil

At 14:08 28/07/2009, Michael Mann wrote:

thanks Phil,

this is very helpful and reaffirms what we've identified as some of
the main points that need to be covered in a formal response. I've
taken the liberty of copying in a couple other colleagues who have
been looking into this. Grant Foster was the first author on a
response to a similarly bad paper by Schwartz that was published some
time ago, and has been doing a number of analyses aimed at
demonstrating the key problems in McClean et al.

I've suggested that Grant sent out a draft of the response when it is
ready to the broader group of people who have been included in these
exchanges for feedback and potential co-authorship,
mike

p.s. Santer et al paper still didn't come through in your followup
message. Can you post in on ftp where it can be downloaded?

On Jul 28, 2009, at 5:15 AM, Phil Jones wrote:

Jim et al,

Having now read the paper in a moment of peace and quiet, there
are a few things

to bear in mind. The authors of the original will have a right of
reply, so need to

ensure that they don't have anything to come back on. From doing
the attached a

year or so ago, there is a word limit and also it is important to
concentrate only

on a few key points. As we all know there is so much wrong with the
paper, it

won't be difficult to come up with a few, but it does need to be
just two or three.

The three aspects I would emphasize are

1. The first difference type filtering. Para 14 implies that they

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smooth the series
with a 12 month running mean, then subtract the value in Jan 1980
from that in
Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know this
removes
any long-term trend.

The running mean also probably distorts the phase, so this is
possibly why
they get different lags from others. Using running means also
enhances the
explained variance. Perhaps we should repeat the exercise without
the smoothing.

2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These
clearly have a
trend. Perhaps show the residual after extracting the ENSO part.

3. They do the same first difference on the smoothed SOI. The SOI
doesn't explain
the climate jump in the 1976/77 period. Their arguments in para 30
are all wrong.

A few minor points

- there are some negative R*R values just after equation 3.
- I'm sure Tom Wigley wouldn't have proposed El Nino events
occurring after volcanoes!

Attached this paper as well. From a quick read it doesn't say
what is purported - in fact
it seems to show clearly how the analysis should have been done.

- there is a paper by Ben Santer (more recent) where he applies
the same type
of extraction procedure to models. I'll send this separately as it
is large. In case it
is too large here is the reference.

Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen,
J.E., Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta, S. and
Taylor K.E., 2001: Accounting for the effects of volcanoes and ENSO
in comparisons of modeled and observed temperature trends. Journal
of Geophysical Research 106, 2803328059.

Finally I've attached a paper I wrote in 1990, where I did
something similar to
what they did. I looked at residuals from a Gaussian filter, and I
added
the smoothed data back afterwards. I was working at the annual
timescale
and I did have many more years.

Cheers

Phil

At 00:19 25/07/2009, Michael Mann wrote:

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sent him an email asking hi if we was interested in spearheading a
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discuss possible strategy for moving this forward,
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On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:

Kia orana All from the Tropical South Pacific
Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin
Tamino's bang up job is great, And good that you go up with stuff on

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Real Climate, Mike. As Kevin is preoccupied, for the scientific record we need a rebuttal somewhere pulled together. Who wants to join in on the multi-authored effort?? I am happy to coordinate it. Return to 'winter' this evening after enjoying a balmy south east trades and sunny dry 24 C in the Cook Islands.

Jim

Quoting Michael Mann [3]<mann@meteo.psu.edu>:

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Jim

Quoting Kevin Trenberth [4]<trenbert@ucar.edu>:

I am on vacation today and don't have the time. I have been on travel the past 4 weeks (including AR5 IPCC scoping mtg); the NCAR summer Colloquium is coming up in a week and then I am off to Oz and NZ for 3 weeks (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper to do.

Kevin

a formal comment to JGR seems like a worthwhile undertaking here. contrarians will continue to cite the paper regardless of whether or not its been rebutted, but for the purpose of future scientific assessments, its important that this be formally rebutted in the peer-reviewed literature.

mike

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Thanks for the pro-activeness. Is there an opportunity to write a letter to JGR pointing out the junk science in this??....if it is not rebutted, then all sceptics will use this to justify their position.

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Quoting Michael Mann [5]<mann@meteo.psu.edu>:

2nd email

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So the amplitude goes from A to Aw where w is the frequency $= 2\pi/L$ where

L is the period.

So the response to this procedure is to reduce periods of 10 years by a factor of 5 compared with periods of 2 years, or 20 and 50 years get reduced by factors of 10 and 25 relative to two year periods. i.e. Their procedure is designed to only analyse the interannual variability not the trends.

Kevin

hi Seth, you always seem to catch me at airports. only got a few minutes. took a cursory look at the paper, and it has all the worry

signs of extremely bad science and scholarship. JGR is a legitimate journal, but some extremely bad papers have slipped through the cracks

in recent years, and this is another one of them.

first of all, the authors use two deeply flawed datasets that

understate the warming trends: the Christy and Spencer MSU data and

uncorrected radiosonde temperature estimates. There were a series

of

three key papers published in science a few years ago, by

Mears

et al,

Santer et al, and Sherwood et al.

see Gavin's excellent RealClimate article on this:

[6]<http://www.realclimate.org/index.php/archives/2005/08/et-tu-lt/>

these papers collectively showed that both datasets were deeply

flawed

and understate actual tropospheric temperature trends. I

find it

absolutely remarkable that this paper could get through a

serious

review w/out referencing any 3 of these critical papers--

papers

whose findings render that conclusions of the current article completely invalid!
The Christy and Spencer MSU satellite-derived tropospheric temperature estimates contained two errors--a sign error and an algebraic error--that had the net effect of artificially removing the warming trend. Christy and Spencer continue to produce revised versions of the MSU dataset, but they always seem to show less warming than every other independent assessment, and their estimates are largely disregarded by serious assessments such as that done by the NAS and the IPCC. So these guys have taken biased estimates of tropospheric temperatures that have artificially too little warming trend, and then shown, quite unremarkably, that El Nino dominates much of what is left (the interannual variability). the paper has absolutely no implications that I can see at all for the role of natural variability on the observed warming trend of recent decades. other far more careful analyses (a paper by David Thompson of CSU, Phil Jones, and others published in Nature more than year ago) used proper, widely-accepted surface temperature data to estimate the influence of natural factors (El Nino and volcanos) on the surface temperature record. their analysis was so careful and clever that it detected a post-world war II error in sea surface temperature measurements (that yields artificial cooling during the mid 1940s) that had never before been discovered in the global surface temperature record. needless to say, they removed that error too. and the correct record, removing influences of ENSO, volcanoes, and even this newly detected error, reveal that a robust warming of global mean surface temperature over the past century of a little less than 1C which has nothing to do w/ volcanic influences or ENSO influences. the dominant source of the overall warming, as concluded in

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every
legitimate major scientific assessment, is anthropogenic
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this later paper provides absolutely nothing to cast that in
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its a joke! and the aptly named Mark "Morano" has fallen for
it!

m

On Jul 23, 2009, at 11:54 AM, Borenstein, Seth wrote:

Kevin, Gavin, Mike,
It's Seth again. Attached is a paper in JGR today that
Marc Morano
is hyping wildly. It's in a legit journal. whatchya think?

Seth

Seth Borenstein

Associated Press Science Writer

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<McLean2008JD011637.pdf>

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Precisely.

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local

fallout this will cause...oh dear.....

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Bye the way June was the warmest month on record for the oceans according to NOAA
Jim
Quoting Kevin Trenberth [8]<trenbert@ucar.edu>:

Exactly
They use 2 datasets that are deficient in the first place and then they use derivatives: differentiation is a high pass filter, and so they show what we have long known that ENSO accounts for a lot of high frequency variability. It should not have been published
Kevin

kia orana from Rarotonga
How the h... did this get accepted!!
Jim
Dominion today {24/7/09}
Nature blamed over warming - describing recently published paper in JGR by Chris de Freitas, Bob Carter and J McLean, and including comment by J Salinger "little new"
McLean J. D., C. R. de Freitas, R. M. Carter (2009), Influence of the Southern Oscillation on tropospheric temperature, J. Geophys. Res., 114, D14104, doi:10.1029/2008JD011637.
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MSU data and uncorrected radiosonde temperature estimates. There were a series of three key papers published in Science a few years ago, by Mears et al, Santer et al, and Sherwood et al. see Gavin's excellent RealClimate article on this: [27]<http://www.realclimate.org/index.php/archives/2005/08/et-tu-1t/> these papers collectively showed that both datasets were deeply flawed and understate actual tropospheric temperature trends. I find it absolutely remarkable that this paper could get through a serious review w/out referencing any 3 of these critical papers-- papers whose findings render that conclusions of the current article completely invalid! The Christy and Spencer MSU satellite-derived tropospheric temperature estimates contained two errors--a sign error and an algebraic error-- that had the net effect of artificially removing the warming trend. Christy and Spencer continue to produce revised versions of the MSU dataset, but they always seem to show less warming than every other independent assessment, and their estimates are largely disregarded by serious assessments such as that done by the NAS and the IPCC. So these guys have taken biased estimates of tropospheric temperatures that have artificially too little warming trend, and then shown, quite unremarkably, that El Nino dominates much of what is left (the interannual variability). the paper has absolutely no implications that I can see at all for the role of natural variability on the observed warming trend of recent decades. other far more careful analyses (a paper by David Thompson of CSU, Phil Jones, and others published in Nature more than year ago) used proper, widely-accepted surface temperature data to estimate the influence of natural factors (El Nino and volcanos) on the surface temperature record. their analysis was so careful and clever that it

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Prof. Phil Jones
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Norwich Email [45]p.jones@uea.ac.uk
NR4 7TJ
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<Parker-on-Pielke-2009.pdf><Jones_ENSO_1990.pdf><wigley2001.pdf>

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website: [47]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[48]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

thanks Phil,

this is very helpful and reaffirms what we've identified as some of the main
points that
need to be covered in a formal response. I've taken the liberty of copying in a
couple
other colleagues who have been looking into this. Grant Foster was the first
author on a
response to a similarly bad paper by Schwartz that was published some time ago,
and has
been doing a number of analyses aimed at demonstrating the key problems in
McClean et
al.
I've suggested that Grant sent out a draft of the response when it is ready to
the
broader group of people who have been included in these exchanges for feedback
and
potential co-authorship,
mike
p.s. Santer et al paper still didn't come through in your followup message. Can

mail.2009

you post

in on ftp where it can be downloaded?

On Jul 28, 2009, at 5:15 AM, Phil Jones wrote:

Jim et al,

Having now read the paper in a moment of peace and quiet, there are a few things to bear in mind. The authors of the original will have a right of reply, so need to ensure that they don't have anything to come back on. From doing the attached a year or so ago, there is a word limit and also it is important to concentrate only on a few key points. As we all know there is so much wrong with the paper, it won't be difficult to come up with a few, but it does need to be just two or three.

The three aspects I would emphasize are

1. The first difference type filtering. Para 14 implies that they smooth the series with a 12 month running mean, then subtract the value in Jan 1980 from that in Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know this removes any long-term trend.

The running mean also probably distorts the phase, so this is possibly why they get different lags from others. Using running means also enhances the explained variance. Perhaps we should repeat the exercise without the smoothing.

2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These clearly have a trend. Perhaps show the residual after extracting the ENSO part.

3. They do the same first difference on the smoothed SOI. The SOI doesn't explain the climate jump in the 1976/77 period. Their arguments in para 30 are all wrong.

A few minor points

- there are some negative R*R values just after equation 3.

- I'm sure Tom Wigley wouldn't have proposed El Nino events occurring after volcanoes!

Attached this paper as well. From a quick read it doesn't say what is purported - in fact

it seems to show clearly how the analysis should have been done.

- there is a paper by Ben Santer (more recent) where he applies the same type of extraction procedure to models. I'll send this separately as it is large.

In case it is too large here is the reference.

Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen, J.E., Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta, S. and Taylor K.E., 2001: Accounting for the effects of volcanoes and ENSO in comparisons of modeled and observed temperature trends. Journal of Geophysical Research 106, 28033-28059.

Finally I've attached a paper I wrote in 1990, where I did something similar to what they did. I looked at residuals from a Gaussian filter, and I added the smoothed data back afterwards. I was working at the annual timescale and I did have many more years.

Cheers

Phil

At 00:19 25/07/2009, Michael Mann wrote:

Hi Jim,

Grant Foster ('Tamino') did a nice job in a previous response

mail.2009

(attached) we wrote to a similarly bad article by Schwartz which got a lot of play in contrarian circles.

since he's already done some of the initial work in debunking this, I sent him an email asking hi if we was interested in spearheading a similar effort w/ this one.

let me get back to folks after I've heard back from him, and we can discuss possible strategy for moving this forward,

mike

On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:

Kia orana All from the Tropical South Pacific

Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin Tamino's bang up job is great, And good that you go up with stuff on Real Climate, Mike. As Kevin is preoccupied, for the scientific record we need a rebuttal somewhere pulled together. who wants to join in on the multiauthored effort?? I am happy to coordinate it. Return to 'winter' this evening after enjoying a balmy south east trades and sunny dry 24 C in the Cook Islands.

Jim

Quoting Michael Mann <[49]mann@meteo.psu.edu>:

folks, we're going to go up w/ something brief on RealClimate later today, mostly just linking to other useful deconstructions of the paper already up on other sites,

mike

On Jul 23, 2009, at 11:01 PM, Jim Salinger wrote:

I am tied up next week, but could frame something up the following week which , I hope would be multi-authored. It would be quite good to have a rebuttal from the same Department at Uni of Auckland (which Glenn McGregor of IJC is director of)! I haven't had the opportunity to download the text here in the Cook Islands, so this would give me the opportunity to do that. who else wants to join in??

Jim

Quoting Kevin Trenberth <[50]trenbert@ucar.edu>:

I am on vacation today and don't have the time. I have been on travel the past 4 weeks (including AR5 IPCC scoping mtg); the NCAR summer Colloquium is coming up in a week and then I am off to Oz and NZ for 3 weeks (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper to do. Kevin

a formal comment to JGR seems like a worthwhile undertaking here. contrarians will continue to cite the paper regardless of whether or

not its been rebutted, but for the purpose of future scientific assessments, its important that this be formally rebutted in the peer-reviewed literature.

mike

On Jul 23, 2009, at 9:05 PM, Jim Salinger wrote:

Hi All

Thanks for the pro-activeness. Is there an opportunity to write a letter to JGR pointing out the junk science in this??....if it is not rebutted, then all sceptics will use this to justify their position.

Jim

Quoting Michael Mann <[51]mann@meteo.psu.edu>:

2nd email

Thanks Kevin, hadn't even noticed that in my terse initial skim of it. yes--that makes things even worse than my initial impression. this is a truly horrible paper. one wonders who the editor was, and what he/she was thinking (or drinking),

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On Jul 23, 2009, at 3:51 PM, Kevin Trenberth wrote:

I just looked briefly at the paper. Their relationships use derivatives of the series. well derivatives are equivalent to a high pass filter, that is to say it filters out all the low frequency variability and trends.

If one takes $y = A \sin wt$ and does a differentiation one gets $dy = Aw \cos wt$. So the amplitude goes from A to Aw where w is the frequency $= 2\pi/L$ where L is the period.

So the response to this procedure is to reduce periods of 10 years by a factor of 5 compared with periods of 2 years, or 20 and 50 years get reduced by factors of 10 and 25 relative to two year periods. i.e. Their procedure is designed to only analyse the interannual variability not the trends.
Kevin

hi Seth, you always seem to catch me at airports. only got a few minutes. took a cursory look at the paper, and it has all the worry signs of extremely bad science and scholarship. JGR is a legitimate journal, but some extremely bad papers have slipped through the cracks in recent years, and this is another one of them. first of all, the authors use two deeply flawed datasets that understate the warming trends: the Christy and Spencer MSU data and uncorrected radiosonde temperature estimates. There were a series of three key papers published in Science a few years ago, by Mears et al, Santer et al, and Sherwood et al. see Gavin's excellent RealClimate article on this: [52]<http://www.realclimate.org/index.php/archives/2005/08/et-tu-1t/> these papers collectively showed that both datasets were deeply flawed and understate actual tropospheric temperature trends. I

find it absolutely remarkable that this paper could get through a serious review w/out referencing any 3 of these critical papers-- papers whose findings render that conclusions of the current article completely invalid!

The Christy and Spencer MSU satellite-derived tropospheric temperature estimates contained two errors--a sign error and an algebraic error-- that had the net effect of artificially removing the warming trend. Christy and Spencer continue to produce revised versions of the MSU dataset, but they always seem to show less warming than every other independent assessment, and their estimates are largely disregarded by serious assessments such as that done by the NAS and the IPCC. So these guys have taken biased estimates of tropospheric temperatures that have artificially too little warming trend, and then shown, quite unremarkably, that El Nino dominates much of what is left (the interannual variability). the paper has absolutely no implications that I can see at all for the role of natural variability on the observed warming trend of recent decades. other far more careful analyses (a paper by David Thompson of CSU, Phil Jones, and others published in Nature more than year ago) used proper, widely-accepted surface temperature data to estimate the influence of natural factors (El Nino and volcanos) on the surface temperature record. their analysis was so careful and clever that it detected a post-world war II error in sea surface temperature measurements (that yields artificial cooling during the mid 1940s) that had never before been discovered in the global surface temperature record. needless to say, they removed that error too. and the correct record, removing influences of ENSO, volcanoes, and even this newly detected error, reveal that a robust warming of global mean surface temperature over the past century of a little less than 1C which has nothing to do w/ volcanic influences or ENSO influences. the dominant source of the overall warming, as concluded in every legitimate major scientific assessment, is anthropogenic

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On Jul 23, 2009, at 11:54 AM, Borenstein, Seth wrote:

Kevin, Gavin, Mike,
It's Seth again. Attached is a paper in JGR today that
Marc Morano
is hyping wildly. It's in a legit journal. whatchya think?
Seth

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Precisely.
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Brett, myself and maybe others will have to deal with the
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fallout this will cause...oh dear.....
Bye the way June was the warmest month on record for the oceans
according to NOAA
Jim

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Quoting Kevin Trenberth <[54]trenbert@ucar.edu>:

Exactly
They use 2 datasets that are deficient in the first place and
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use derivatives: differentiation is a high pass filter, and so
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How the h... did this get accepted!!
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Dominion today {24/7/09}
Nature blamed over warming - describing recently published
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including
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Influence
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Seth Borenstein

Associated Press Science Writer

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Bye the way June was the warmest month on record for the oceans according to NOAA
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Exactly
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Kevin

kia orana from Rarotonga
How the h... did this get accepted!!
Jim
Dominion today {24/7/09}
Nature blamed over warming - describing recently published paper in JGR by Chris de Freitas, Bob Carter and J McLean, and including comment by J Salinger "little new"
McLean J. D., C. R. de Freitas, R. M. Carter (2009), Influence of the Southern Oscillation on tropospheric temperature, J. Geophys. Res., 114, D14104, doi:10.1029/2008JD011637.
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Page 151

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Prof. Phil Jones
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University of East Anglia
Norwich Email [91]p.jones@uea.ac.uk
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<Parker-on-Pielke-2009.pdf><Jones_ENSO_1990.pdf><wigley2001.pdf>

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References

1. <http://www.cgd.ucar.edu/cas/papers/2000JD000298.pdf>
2. <ftp://ftp.cru.uea.ac.uk/>
3. <mailto:mann@meteo.psu.edu>
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7. <mailto:sborenstein@ap.org>
8. <mailto:trenbert@ucar.edu>
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- 97. <http://www.cgd.ucar.edu/cas/trenbert.html>

988. 1248790545.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>, Jim Salinger <j.salinger@auckland.ac.nz>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Tue Jul 28 10:15:45 2009
Cc: trenbert@ucar.edu, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>

Jim et al,

Having now read the paper in a moment of peace and quiet, there are a few things to bear in mind. The authors of the original will have a right of reply, so need to ensure that they don't have anything to come back on. From doing the attached a year or so ago, there is a word limit and also it is important to concentrate only on a few key points. As we all know there is so much wrong with the paper, it won't be difficult to come up with a few, but it does need to be just two or three.

The three aspects I would emphasize are

1. The first difference type filtering. Para 14 implies that they smooth the series

with a 12 month running mean, then subtract the value in Jan 1980 from that in Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know this removes any long-term trend.

The running mean also probably distorts the phase, so this is possibly why they get different lags from others. Using running means also enhances the explained variance. Perhaps we should repeat the exercise without the smoothing.

2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These clearly have a trend. Perhaps show the residual after extracting the ENSO part.

3. They do the same first difference on the smoothed SOI. The SOI doesn't explain the climate jump in the 1976/77 period. Their arguments in para 30 are all wrong.

A few minor points

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- there are some negative R*R values just after equation 3.
- I'm sure Tom Wigley wouldn't have proposed El Nino events occurring after volcanoes!

Attached this paper as well. From a quick read it doesn't say what is purported - in fact

it seems to show clearly how the analysis should have been done.

- there is a paper by Ben Santer (more recent) where he applies the same type of extraction procedure to models. I'll send this separately as it is large. In case it

is too large here is the reference.

P.D.,
Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen, J.E., Jones,

Meehl, G.A., Roeckner, E., Sengupta, S. and Taylor K.E., 2001: Accounting for the effects

of volcanoes and ENSO in comparisons of modeled and observed temperature trends. Journal

of Geophysical Research 106, 2803328059.

Finally I've attached a paper I wrote in 1990, where I did something similar to what they did. I looked at residuals from a Gaussian filter, and I added the smoothed data back afterwards. I was working at the annual timescale and I did have many more years.

Cheers

Phil

At 00:19 25/07/2009, Michael Mann wrote:

Hi Jim,

Grant Foster ('Tamino') did a nice job in a previous response (attached) we wrote to a similarly bad article by Schwartz which got a lot of play in contrarian circles.

since he's already done some of the initial work in debunking this, I sent him an email asking hi if we was interested in spearheading a similar effort w/ this one.

let me get back to folks after I've heard back from him, and we can discuss possible strategy for moving this forward,
mike

On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:

Kia orana All from the Tropical South Pacific

Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin Tamino's bang up job is great, And good that you go up with stuff on Real Climate, Mike. As Kevin is preoccupied, for the scientific record we need a rebuttal somewhere pulled together. who wants to join in on the multiauthored effort?? I am happy to coordinate it. Return to 'winter' this evening after enjoying a balmy south east trades and sunny dry 24 C in the Cook Islands.

Jim

Quoting Michael Mann <mann@meteo.psu.edu>:

folks, we're going to go up w/ something brief on RealClimate later today, mostly just linking to other useful deconstructions of the paper already up on other sites,
mike

On Jul 23, 2009, at 11:01 PM, Jim Salinger wrote:

I am tied up next week, but could frame something up the following week which , I hope would be multi-authored. It would be quite good to have a rebuttal from the same Department at Uni of Auckland (which Glenn McGregor of IJC is director of)!

I haven't had the opportunity to download the text here in the Cook Islands, so this would give me the opportunity to do that. who else wants to join in??

Jim

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Quoting Kevin Trenberth <trenbert@ucar.edu>:

I am on vacation today and don't have the time. I have been on travel the past 4 weeks (including AR5 IPCC scoping mtg); the NCAR summer Colloquium is coming up in a week and then I am off to Oz and NZ for 3 weeks (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper to do.
Kevin

a formal comment to JGR seems like a worthwhile undertaking here. contrarians will continue to cite the paper regardless of whether or not its been rebutted, but for the purpose of future scientific assessments, its important that this be formally rebutted in the peer-reviewed literature.

mike

On Jul 23, 2009, at 9:05 PM, Jim Salinger wrote:

Hi All

Thanks for the pro-activeness. Is there an opportunity to write a letter to JGR pointing out the junk science in this??...if it is not rebutted, then all sceptics will use this to justify their position.

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Quoting Michael Mann <mann@meteo.psu.edu>:

2nd email

Thanks Kevin, hadn't even noticed that in my terse initial skim of it. yes--that makes things even worse than my initial impression. this is a truly horrible paper. one wonders who the editor was, and what he/she was thinking (or drinking),
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If one takes $y = A \sin wt$ and does a differentiation one gets $dy = Aw \cos wt$.

So the amplitude goes from A to Aw where w is the frequency $= 2\pi/L$ where L is the period.

So the response to this procedure is to reduce periods of 10 years by a factor of 5 compared with periods of 2 years, or 20 and 50 years get reduced by factors of 10 and 25 relative to two year periods. i.e. Their procedure is designed to only analyse the interannual variability not the trends.

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Kevin

hi Seth, you always seem to catch me at airports. only got a few minutes. took a cursory look at the paper, and it has all the worry signs of extremely bad science and scholarship. JGR is a legitimate journal, but some extremely bad papers have slipped through the cracks in recent years, and this is another one of them. first of all, the authors use two deeply flawed datasets that understate the warming trends: the Christy and Spencer MSU data and uncorrected radiosonde temperature estimates. There were a series of three key papers published in science a few years ago, by Mears et al, Santer et al, and Sherwood et al. see Gavin's excellent RealClimate article on this: [1]<http://www.realclimate.org/index.php/archives/2005/08/et-tu-1t/> these papers collectively showed that both datasets were deeply flawed and understate actual tropospheric temperature trends. I find it absolutely remarkable that this paper could get through a serious review w/out referencing any 3 of these critical papers-- papers whose findings render that conclusions of the current article completely invalid! The Christy and Spencer MSU satellite-derived tropospheric temperature estimates contained two errors--a sign error and an algebraic error-- that had the net effect of artificially removing the warming trend. Christy and Spencer continue to produce revised versions of the MSU dataset, but they always seem to show less warming than every other independent assessment, and their estimates are largely disregarded by serious assessments such as that done by the NAS and the IPCC. So these guys have taken biased estimates of tropospheric temperatures that have artificially too little warming trend, and then shown, quite unremarkably, that El Nino dominates much of what is left (the interannual variability). the paper has absolutely no implications that I can see at all for the role of natural variability on the observed warming trend of recent decades. other far more careful analyses (a paper by David Thompson of CSU,

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Phil Jones, and others published in Nature more than year ago) used proper, widely-accepted surface temperature data to estimate the influence of natural factors (El Nino and volcanos) on the surface temperature record. their analysis was so careful and clever that it detected a post-world war II error in sea surface temperature measurements (that yields artificial cooling during the mid 1940s) that had never before been discovered in the global surface temperature record. needless to say, they removed that error too. and the correct record, removing influences of ENSO, volcanoes, and even this newly detected error, reveal that a robust warming of global mean surface temperature over the past century of a little less than 1C which has nothing to do w/ volcanic influences or ENSO influences. the dominant source of the overall warming, as concluded in every legitimate major scientific assessment, is anthropogenic influences (human greenhouse gas concentrations w/ some offsetting cooling due to sulphate aerosols). this later paper provides absolutely nothing to cast that in doubt. it uses a flawed set of surface temperature measurements for which the trend has been artificially suppressed, to show that whats left over (interannual variability) is due to natural influences. duh! its a joke! and the aptly named Mark "Morano" has fallen for it!

m
On Jul 23, 2009, at 11:54 AM, Borenstein, Seth wrote:

Kevin, Gavin, Mike,
It's Seth again. Attached is a paper in JGR today that Marc Morano is hyping wildly. It's in a legit journal. whatchya think?
Seth
Seth Borenstein
Associated Press Science Writer
sborenstein@ap.org
The Associated Press, 1100 13th St. NW, Suite 700,
Washington, DC
20005-4076
202-641-9454

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msk dccc60c6d2c3a6438f0cf467d9a4938
<McLean2008JD011637.pdf>

On Jul 23, 2009, at 7:57 PM, Jim Salinger wrote:

Precisely.
Mike Mann: You better rush something up on RealClimate. Jim,
Brett, myself and maybe others will have to deal with the
local
fallout this will cause...oh dear.....
Bye the way June was the warmest month on record for the oceans
according to NOAA
Jim
Quoting Kevin Trenberth <trenbert@ucar.edu>:

Exactly
They use 2 datasets that are deficient in the first place and
then they
use derivatives: differentiation is a high pass filter, and so
they show
what we have long known that ENSO accounts for a lot of high
frequency
variability. It should not have been published
Kevin

kia orana from Rarotonga
How the h... did this get accepted!!
Jim
Dominion today {24/7/09}
Nature blamed over warming - describing recently published
paper
in
JGR by Chris de Freitas, Bob Carter and J McLean, and
including
comment by J Salinger "little new"
McLean J. D., C. R. de Freitas, R. M. Carter (2009),
Influence
of the
Southern Oscillation on tropospheric temperature, J. Geophys.
Res.,
114, D14104, doi:10.1029/2008JD011637.
paper at
[2]<http://www.agu.org/pubs/crossref/2009/2008JD011637.shtml>

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Associate Professor Jim Salinger
School of Geography and Environmental Science
University of Auckland
Private Bag 92 019

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Auckland, New Zealand
Tel: + 64 9 373 7599 ext 88473

This message was sent using IMP, the Internet Messaging Program.

Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
[3]<http://www.cgd.ucar.edu/cas/trenbert.htm>

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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
website: [4]<http://www.meteo.psu.edu/~mann/Mann/index.htm>
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mike
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Kia orana All from the Tropical South Pacific

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So the response to this procedure is to reduce periods of 10 years by a factor of 5 compared with periods of 2 years, or 20 and 50 years get reduced by factors of 10 and 25 relative to two year periods.

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in recent years, and this is another one of them.
first of all, the authors use two deeply flawed datasets that
understate the warming trends: the Christy and Spencer MSU data and
uncorrected radiosonde temperature estimates. There were a series
of
three key papers published in Science a few years ago, by Mears
et al,
Santer et al, and Sherwood et al.
see Gavin's excellent RealClimate article on this:
[16]<http://www.realclimate.org/index.php/archives/2005/08/et-tu-lt/>
these papers collectively showed that both datasets were deeply
flawed
and understate actual tropospheric temperature trends. I find it
absolutely remarkable that this paper could get through a serious
review w/out referencing any 3 of these critical papers--papers
whose
findings render that conclusions of the current article completely
invalid!
The Christy and Spencer MSU satellite-derived tropospheric
temperature
estimates contained two errors--a sign error and an algebraic
error--
that had the net effect of artificially removing the warming trend.
Christy and Spencer continue to produce revised versions of the MSU
dataset, but they always seem to show less warming than every other
independent assessment, and their estimates are largely
disregarded by
serious assessments such as that done by the NAS and the IPCC.
So these guys have taken biased estimates of tropospheric
temperatures
that have artificially too little warming trend, and then shown,
quite
unremarkably, that El Nino dominates much of what is left (the
interannual variability).
the paper has absolutely no implications that I can see at all
for the
role of natural variability on the observed warming trend of recent
decades.
other far more careful analyses (a paper by David Thompson of CSU,
Phil Jones, and others published in Nature more than year ago)
used
proper, widely-accepted surface temperature data to estimate the
influence of natural factors (El Nino and volcanos) on the surface
temperature record. their analysis was so careful and clever that
it
detected a post-world war II error in sea surface temperature
measurements (that yields artificial cooling during the mid 1940s)
that had never before been discovered in the global surface
temperature record. needless to say, they removed that error too.
and
the correct record, removing influences of ENSO, volcanoes, and
even
this newly detected error, reveal that a robust warming of
global mean
surface temperature over the past century of a little less than 1C
which has nothing to do w/ volcanic influences or ENSO
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dominant source of the overall warming, as concluded in every
legitimate major scientific assessment, is anthropogenic
influences
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Seth Borenstein

Associated Press Science Writer

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<McLean2008JD011637.pdf>

On Jul 23, 2009, at 7:57 PM, Jim Salinger wrote:

Precisely.

Mike Mann: You better rush something up on RealClimate. Jim, Brett, myself and maybe others will have to deal with the local fallout this will cause...oh dear.....

Bye the way June was the warmest month on record for the oceans according to NOAA

Jim

Quoting Kevin Trenberth <[18]trenbert@ucar.edu>:

Exactly

They use 2 datasets that are deficient in the first place and then they

use derivatives: differentiation is a high pass filter, and so they show

what we have long known that ENSO accounts for a lot of high frequency

variability. It should not have been published

Kevin

kia orana from Rarotonga

How the h... did this get accepted!!

Jim

mail.2009

Dominion today {24/7/09]
Nature blamed over warming - describing recently published paper
in
JGR by Chris de Freitas, Bob Carter and J McLean, and including
comment by J Salinger "little new"
McLean J. D., C. R. de Freitas, R. M. Carter (2009), Influence
of the
Southern Oscillation on tropospheric temperature, J. Geophys.
Res.,
114, D14104, doi:10.1029/2008JD011637.
paper at
[19]<http://www.agu.org/pubs/crossref/2009/2008JD011637.shtml>

Associate Professor Jim Salinger
School of Geography and Environmental Science
University of Auckland
Private Bag 92 019
Auckland, New Zealand
Tel: + 64 9 373 7599 ext 88473

This message was sent using IMP, the Internet Messaging Program.

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

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Michael E. Mann
Professor
Director, Earth System Science Center (ESSC)
Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
865-3663
The Pennsylvania State University email: [21]mann@psu.edu
University Park, PA 16802-5013
website: [22]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[23]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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Kevin Trenberth

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19. <http://www.agu.org/pubs/crossref/2009/2008JD011637.shtml>
20. <http://www.cgd.ucar.edu/cas/trenbert.html>
21. <mailto:mann@psu.edu>
22. <http://www.meteo.psu.edu/~mann/Mann/index.html>
23. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
24. <mailto:mann@psu.edu>
25. <http://www.meteo.psu.edu/~mann/Mann/index.html>
26. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
27. <http://www.cgd.ucar.edu/cas/trenbert.html>
28. <mailto:mann@psu.edu>
29. <http://www.meteo.psu.edu/~mann/Mann/index.html>
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32. <http://www.meteo.psu.edu/~mann/Mann/index.html>
33. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

989. 1248862973.txt

#####

From: Jim Salinger <j.salinger@auckland.ac.nz>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Wed, 29 Jul 2009 06:22:53 +1200
Cc: Phil Jones <p.jones@uea.ac.uk>, Michael Mann <mann@meteo.psu.edu>, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Annan <jdannan@jamstec.go.jp>, Grant Foster <tamino_9@hotmail.com>

<x-flowed>

Good morning all from tomorrowland (Wednesday!)

Gosh, you have all been very busy overnight here. Thank you, and Mike & I will start wordsmithing our section. We now have (in IPCC terms) a nice bunch of LA's and CAS for this commentary!

'Talk' to you later!

Jim

Quoting Kevin Trenberth <trenbert@ucar.edu>:

- > Phil
- > see also this:
- > Trenberth, K. E., and L. Smith, 2009: Variations in the three
- > dimensional structure of the atmospheric circulation with different
- > flavors of El Niño. /J. Climate/, *12*, No. 11, 2978-2991, doi:
- > 10.1175/2008JCLI2691.1. [PDF]
- >
- > <<http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/TrenberthSmithVTempJCI09.pdf>
- >
- > It has tables with relationships with Nino 3.4 and SOI and you can
- > see the differences in lead lag e.g. Table 1. SOI leads Nino 3.4 by
- > 1 or 2 months typically but as in the 2002 paper, the leads and
- > lags vary with Nino index, see also
- > Trenberth, K. E., and D. P. Stepaniak, 2001: Indices of El Niño
- > evolution. /J. Climate./, *14*, 1697-1701. [Paper(.pdf)]
- > <<http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/tniJC.pdf>>
- > ^*
- > <<http://www.cgd.ucar.edu/cas/Trenberth/trenberth-publish.html#amscr>
- >

> Kevin
>
> Phil Jones wrote:
>>
>> Kevin, Mike et al,
>> Figure 3 in what Kevin just sent is the sort of thing we need to show.
>> On the lags, I think the reason the lag with what Tom did was different is
>> that you used Nino3.4 SST and Tom used SOI. I know people think
>> they are the same thing, but I think SOI lags a little behind 3.4 SST.
>> It would be a useful bit of new science to look at the links between SOI
>> and 3.4 SST, but it shouldn't be part of a comment on what's wrong with the
>> awful paper For that you're going to have to use the Bureau Of Meteorology
>> version of the SOI. These are on this web site
>> <http://www.bom.gov.au/climate/current/soihtml.shtml>
>> I did check a few years ago and these numbers look pretty much the same
>> as the CRU ones (allowing for the BoM multiplier of 10).
>> when you calculate the SOI you normalize the Darwin
>> and Tahiti series. BoM change the base period with each new year, so
>> don't expect to get exactly the same results as McLean.
>> You have to smooth the SOI series in some way as it is noisy.
>> Their running mean
>> is a lousy filter. I'd recommend using the one we did in ch 3 of
>> IPCC. It is on
>> p336. The second filter will work fine, with all the months in
>> sequence. It will
>> approximate a 10-12 month filter and won't do anything to the phase. Maybe
>> doing this with SOI and Nino3.4 will show a slight lag between the
>> two - 3 months
>> maybe!
>>
>> Cheers
>> Phil
>>
>>
>> At 15:57 28/07/2009, Kevin Trenberth wrote:
>>> The leads and lags are analyzed in detail in this paper
>>> Trenberth, K. E., J. M. Caron, D. P. Stepaniak, and S. Worley
>>> 2002: The evolution of ENSO and global atmospheric surface
>>> temperatures <<http://www.cgd.ucar.edu/cas/papers/2000JD000298.pdf>>
>>> /J. Geophys. Res./, *107*, D8, 10.1029/2000JD000298.
>>> and we were not able to reproduce Tom wigley's result (we tried).
>>> It may depend in indices used. In this paper we also document the
>>> extent to which ENSO contributes to warming overall.
>>> Kevin
>>>
>>> Phil Jones wrote:
>>>> Mike,
>>>> See below for instructions.
>>>>
>>>> Also, just because IPCC (2007, Ch 3) didn't point out the 6/7-month lag
>>>> between the SOI and global temperatures doesn't mean it hasn't been
>>>> known for years. IPCC is an assessment and not a review of everything
>>>> done. If they had even read wigley (2001) they would have seen this
>>>> lag pointed out. I wasn't the first to do this in 1989 either. I don't
>>>> think walker was either. I think the first was Hildebrandsson in the
>>>> 1890s. why does it always go back to a Swede!
>>>>
>>>> file is at <ftp://ftp.cru.uea.ac.uk> <<ftp://ftp.cru.uea.ac.uk/>>
>>>>
>>>> login anonymously with emails as pw
>>>>
>>>> then go to people/philjones
>>>>

>>>> and you should find santeretal2001.pdf

>>>>

>>>> Cheers

>>>> Phil

>>>>

>>>>

>>>> At 14:08 28/07/2009, Michael Mann wrote:

>>>>> thanks Phil,

>>>>>

>>>>>> this is very helpful and reaffirms what we've identified as some
>>>>>> of the main points that need to be covered in a formal response.

>>>>>> I've taken the liberty of copying in a couple other colleagues

>>>>>> who have been looking into this. Grant Foster was the first

>>>>>> author on a response to a similarly bad paper by Schwartz that

>>>>>> was published some time ago, and has been doing a number of

>>>>>> analyses aimed at demonstrating the key problems in McClean et al.

>>>>>>

>>>>>> I've suggested that Grant sent out a draft of the response when

>>>>>> it is ready to the broader group of people who have been

>>>>>> included in these exchanges for feedback and potential

>>>>>> co-authorship,

>>>>>>

>>>>>> mike

>>>>>>

>>>>>> p.s. Santer et al paper still didn't come through in your

>>>>>> followup message. Can you post in on ftp where it can be

>>>>>> downloaded?

>>>>>> On Jul 28, 2009, at 5:15 AM, Phil Jones wrote:

>>>>>>

>>>>>>>

>>>>>>>> Jim et al,

>>>>>>>> Having now read the paper in a moment of peace and quiet,

>>>>>>>> there are a few things

>>>>>>>> to bear in mind. The authors of the original will have a right

>>>>>>>> of reply, so need to

>>>>>>>> ensure that they don't have anything to come back on. From

>>>>>>>> doing the attached a

>>>>>>>> year or so ago, there is a word limit and also it is important

>>>>>>>> to concentrate only

>>>>>>>> on a few key points. As we all know there is so much wrong with

>>>>>>>> the paper, it

>>>>>>>> won't be difficult to come up with a few, but it does need to

>>>>>>>> be just two or three.

>>>>>>>>

>>>>>>>> The three aspects I would emphasize are

>>>>>>>>

>>>>>>>>> 1. The first difference type filtering. Para 14 implies that

>>>>>>>>> they smooth the series

>>>>>>>>> with a 12 month running mean, then subtract the value in Jan

>>>>>>>>> 1980 from that in

>>>>>>>>> Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know

>>>>>>>>> this removes

>>>>>>>>> any long-term trend.

>>>>>>>>>

>>>>>>>>>> The running mean also probably distorts the phase, so this is

>>>>>>>>>> possibly why

>>>>>>>>>> they get different lags from others. Using running means also

>>>>>>>>>> enhances the

>>>>>>>>>> explained variance. Perhaps we should repeat the exercise

>>>>>>>>>> without the smoothing.

>>>>>>>>>>

>>>>>>>>>>> 2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These

>>>>>>>>>>> clearly have a

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>>>>> trend. Perhaps show the residual after extracting the ENSO part.
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>>>>> 3. They do the same first difference on the smoothed SOI. The
>>>>> SOI doesn't explain
>>>>> the climate jump in the 1976/77 period. Their arguments in para
>>>>> 30 are all wrong.
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>>>>> A few minor points
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>>>>> - there are some negative R*R values just after equation 3.
>>>>> - I'm sure Tom Wigley wouldn't have proposed El Nino events
>>>>> occurring after volcanoes!
>>>>> Attached this paper as well. From a quick read it doesn't
>>>>> say what is purported - in fact
>>>>> it seems to show clearly how the analysis should have been done.
>>>>> - there is a paper by Ben Santer (more recent) where he
>>>>> applies the same type
>>>>> of extraction procedure to models. I'll send this separately as
>>>>> it is large. In case it
>>>>> is too large here is the reference.
>>>>>
>>>>> Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S.,
>>>>> Hansen, J.E., Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta,
>>>>> S. and Taylor K.E., 2001: Accounting for the effects of
>>>>> volcanoes and ENSO in comparisons of modeled and observed
>>>>> temperature trends. Journal of Geophysical Research 106,
>>>>> 28033-28059.
>>>>>
>>>>> Finally I've attached a paper I wrote in 1990, where I did
>>>>> something similar to
>>>>> what they did. I looked at residuals from a Gaussian filter, and I added
>>>>> the smoothed data back afterwards. I was working at the annual timescale
>>>>> and I did have many more years.
>>>>>
>>>>> Cheers
>>>>> Phil
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>>>>> At 00:19 25/07/2009, Michael Mann wrote:
>>>>>> Hi Jim,
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>>>>>>> Grant Foster ('Tamino') did a nice job in a previous response
>>>>>>> (attached) we wrote to a similarly bad article by Schwartz which got a
>>>>>>> lot of play in contrarian circles.
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>>>>>>>> since he's already done some of the initial work in debunking this, I
>>>>>>>> sent him an email asking hi if we was interested in spearheading a
>>>>>>>> similar effort w/ this one.
>>>>>>>>
>>>>>>>>> let me get back to folks after I've heard back from him, and we can
>>>>>>>>> discuss possible strategy for moving this forward,
>>>>>>>>>
>>>>>>>>> mike
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>>>>>>>>>> On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:
>>>>>>>>>>
>>>>>>>>>>> Kia orana All from the Tropical South Pacific
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>>>>>>>>>>>> Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin
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>>>>>>>>>>>>>> Real Climate, Mike. As Kevin is preoccupied, for the scientific
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>>>>>>> join in on the multiauthored effort?? I am happy to coordinate it.
>>>>>>>
>>>>>>> Return to 'winter' this evening after enjoying a balmy south east
>>>>>>> trades and sunny dry 24 C in the Cook Islands.

>>>>>>> Jim

>>>>>>> Quoting Michael Mann <mann@meteo.psu.edu> <mailto:mann@meteo.psu.edu>:

>>>>>>>> folks, we're going to go up w/ something brief on RealClimate
>>>>>>>> later today, mostly just linking to other useful deconstructions
>>>>>>>> of the paper already up on other sites,

>>>>>>>> mike

>>>>>>>> On Jul 23, 2009, at 11:01 PM, Jim Salinger wrote:

>>>>>>>>> I am tied up next week, but could frame something up the
>>>>>>>>> following week which , I hope would be multi-authored. It would
>>>>>>>>> be quite good to have a rebuttal from the same Department at Uni
>>>>>>>>> of Auckland (which Glenn McGregor of IJC is director of)!

>>>>>>>>> I haven't had the opportunity to download the text here in the
>>>>>>>>> Cook Islands, so this would give me the opportunity to do that.
>>>>>>>>> Who else wants to join in??

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>>>>>>>>> Quoting Kevin Trenberth <trenbert@ucar.edu>
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>>>>>>>>>>> I am on vacation today and don't have the time. I have been on
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>>>>>>>>>>> Colloquium
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>>>>>>>>>>>>> a formal comment to JGR seems like a worthwhile undertaking here.
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>>>>>>>>> 2nd email

>>>>>>>>> _____
>>>>>>>>> Thanks Kevin, hadn't even noticed that in my terse
>>>>>>>>> initial skim of
>>>>>>>>> it. yes--that makes things even worse than my initial
>>>>>>>>> impression.

>>>>>>>>> this is a truly horrible paper. one wonders who the editor was,
>>>>>>>>> and what he/she was thinking (or drinking),

>>>>>>>>> m

>>>>>>>>> On Jul 23, 2009, at 3:51 PM, Kevin Trenberth wrote:

>>>>>>>>> I just looked briefly at the paper. Their relationships use
>>>>>>>>> derivatives
>>>>>>>>> of the series. well derivatives are equivalent to a high pass
>>>>>>>>> filter,
>>>>>>>>> that is to say it filters out all the low frequency
>>>>>>>>> variability and
>>>>>>>>> trends.

>>>>>>>>> If one takes $y = A \sin wt$
>>>>>>>>> and does a differentiation one gets
>>>>>>>>> $dy = Aw \cos wt$.

>>>>>>>>> So the amplitude goes from A to Aw where w is the
>>>>>>>>> frequency = $2\pi/L$
>>>>>>>>> L where
>>>>>>>>> L is the period.

>>>>
>>>>
>>>>
>>>>

>>>> thanks Phil,

>>>>

>>>> this is very helpful and reaffirms what we've identified as some
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>>>>>>> Quoting Michael Mann <mann@meteo.psu.edu <mailto:mann@meteo.psu.edu>>:
>>>>>>>
>>>>>>> folks, we're going to go up w/ something brief on
>>>>>>> RealClimate later today, mostly just linking to other
>>>>>>> useful deconstructions of the paper already up on other
>>>>>>> sites,
>>>>>>>
>>>>>>> mike
>>>>>>>
>>>>>>> On Jul 23, 2009, at 11:01 PM, Jim Salinger wrote:
>>>>>>>
>>>>>>> I am tied up next week, but could frame something up the
>>>>>>> following week which , I hope would be multi-authored. It
>>>>>>> would be quite good to have a rebuttal from the same
>>>>>>> Department at Uni of Auckland (which Glenn McGregor of IJC
>>>>>>> is director of)!>>>>>>>
>>>>>>> I haven't had the opportunity to download the text here in
>>>>>>> the Cook Islands, so this would give me the opportunity to
>>>>>>> do that. who else wants to join in??
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>>>>>>> Quoting Kevin Trenberth <trenbert@ucar.edu
>>>>>>> <mailto:trenbert@ucar.edu>>:
>>>>>>>
>>>>>>>> I am on vacation today and don't have the time. I have
>>>>>>>> been on travel the
>>>>>>>> past 4 weeks (including AR5 IPCC scoping mtg); the NCAR
>>>>>>>> summer Colloquium
>>>>>>>> is coming up in a week and then I am off to Oz and NZ for 3 weeks
>>>>>>>> (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper to do.
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>>>>>>>>> whether or
>>>>>>>>> not its been rebutted, but for the purpose of future scientific
>>>>>>>>> assessments, its important that this be formally rebutted
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>>>>>>>>>> Hi All
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>>>>>>>>>>> not rebutted, then all sceptics will use this to justify their
>>>>>>>>>>> position.
>>>>>>>>>>>
>>>>>>>>>>> Jim
>>>>>>>>>>>>

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>>>>>>

>>>>>> Hi Jim,

>>>>>>

>>>>>> Grant Foster ('Tamino') did a nice job in a previous response
>>>>>> (attached) we wrote to a similarly bad article by Schwartz
>>>>>> which got a lot of play in contrarian circles.

>>>>>>

>>>>>> since he's already done some of the initial work in debunking
>>>>>> this, I sent him an email asking hi if we was interested in
>>>>>> spearheading a similar effort w/ this one.

>>>>>>

>>>>>> let me get back to folks after I've heard back from him, and
>>>>>> we can discuss possible strategy for moving this forward,

>>>>>>

>>>>>> mike

>>>>>>

>>>>>> On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:

>>>>>>

>>>>>>> Kia orana All from the Tropical South Pacific

>>>>>>>

>>>>>>> Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin
>>>>>>> Tamino's bang up job is great, And good that you go up with
>>>>>>> stuff on Real Climate, Mike. As Kevin is preoccupied, for the
>>>>>>> scientific record we need a rebuttal somewhere pulled
>>>>>>> together. who wants to join in on the multiauthored effort??
>>>>>>> I am happy to coordinate it.

>>>>>>>

>>>>>>> Return to 'winter' this evening after enjoying a balmy south
>>>>>>> east trades and sunny dry 24 C in the Cook Islands.

>>>>>>>

>>>>>>> Jim

>>>>>>>

>>>>>>> Quoting Michael Mann <mann@meteo.psu.edu <mailto:mann@meteo.psu.edu>>:

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>>>>>>>>> Jim

>>>>>>>>> Quoting Michael Mann <mann@meteo.psu.edu
>>>>>>>>> <mailto:mann@meteo.psu.edu>:

>>>>>>>>> 2nd email

>>>>>>>>> _____
>>>>>>>>> Thanks Kevin, hadn't even noticed that in my terse
>>>>>>>>> initial skim of
>>>>>>>>> it. yes--that makes things even worse than my initial
>>>>>>>>> impression.

>>>>>>>>> this is a truly horrible paper. one wonders who the editor was,
>>>>>>>>> and what he/she was thinking (or drinking),

>>>>>>>>> m

>>>>>>>>> On Jul 23, 2009, at 3:51 PM, Kevin Trenberth wrote:

>>>>>>>>> I just looked briefly at the paper. Their relationships use
>>>>>>>>> derivatives
>>>>>>>>> of the series. well derivatives are equivalent to a high pass
>>>>>>>>> filter,
>>>>>>>>> that is to say it filters out all the low frequency
>>>>>>>>> variability and
>>>>>>>>> trends.

>>>>>>>>> If one takes $y = A \sin wt$
>>>>>>>>> and does a differentiation one gets
>>>>>>>>> $dy = Aw \cos wt$.

>>>>>>>>> So the amplitude goes from A to Aw where w is the
>>>>>>>>> frequency = $2\pi/L$
>>>>>>>>> where
>>>>>>>>> L is the period.

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>>>>>> 503 Walker Building FAX: (814) 865-3663
>>>>>> The Pennsylvania State University email: mann@psu.edu
>>>>>> <mailto:mann@psu.edu>
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>>>>>> website: <http://www.meteo.psu.edu/~mann/Mann/index.html>
>>>>>> <<http://www.meteo.psu.edu/%7Emann/Mann/index.html>>
>>>>>> "Dire Predictions" book site:
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>>>>>> Prof. Phil Jones
>>>>>> Climatic Research Unit Telephone +44 (0) 1603 592090
>>>>>> School of Environmental Sciences Fax +44 (0) 1603 507784
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>>>>>> UK

>>>>>> -----

>>>>>> <Parker-on-Pielke-2009.pdf><Jones_ENSO_1990.pdf><wigley2001.pdf>

>>>>>>
>>>>>> --

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

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>>>>
>>>> --

>>>> *****
>>> Kevin E.
>>> Trenberth e-mail: trenbert@ucar.edu
>>> <mailto:trenbert@ucar.edu>
>>> Climate Analysis
>>> Section, www.cgd.ucar.edu/cas/trenbert.html
>>> <http://www.cgd.ucar.edu/cas/trenbert.html>
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>>> Boulder, CO
>>> 80307 (303) 497 1333 (fax)
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>>> Street address: 1850 Table Mesa Drive, Boulder, CO 80305
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>> UK

>
> --

> *****
> 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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This message was sent using IMP, the Internet Messaging Program.
</x-flowed>

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#####

From: Kevin Trenberth <trenbert@ucar.edu>
Page 207

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To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Wed, 29 Jul 2009 10:23:09 -0600
Cc: Grant Foster <tamino_9@hotmail.com>, p.jones@uea.ac.uk, "J. Salinger" <j.salinger@auckland.ac.nz>, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Annan <jdannan@jamstec.go.jp>

Hi all
Wow this is a nice analysis by Grant et al. What we should do is turn this into a learning experience for everyone: there is often misuse of filtering. Obviously the editor and reviewers need to also be taken to task here. I agree with Mike Mann that a couple of other key points deserve to be made wrt this paper. Making sure that the important relationships and role of ENSO on interannual variability of global temperatures should also be pointed out with some select references (as in recent emails and the refs therein). In terms of the paper, I recommend consolidating the figures to keep them fewer in number if this is a comment: combine Figs 3 with 4, and 6 with 7. Make sure the plots of spectra have period prominently displayed as well as frequency and maybe even highlight with stipple some bands like >10 years. Glad to sign on: I would need an acknowledgment that NCAR is sponsored by NSF.
Regards
Kevin
Michael Mann wrote:

thanks Grant, the paper is starting to shape up well now. Jim and I (well, mostly Jim, w/ some input from me) are iterating on a blurb about past studies on ENSO/temperature relationships and should have something for you soon on that,

As James has pointed out, its important to stick to the key points and not get sidetracked with nonsense. I would avoid any commentary on their ignorant ramblings about the Hadley Cell, etc. We want to cut straight to the deep flaws in their analysis which are, in order of importance in my view,

1. indefensible use of a differencing filter, which has the effect of selectively damping low-frequency variability and renders any conclusions about factors underlying long-term trends completely spurious.
2. ignoring the fact that the influence of ENSO on global temperature has been known for decades, and much better quantified in past studies than in the current deeply flawed analysis.
3. the selective use of a flawed temperature data and curious splicing in of inappropriate recent data (UAH TMT) to further suppress trends. A bit of overkill given that they already eliminated the trends anyway. Guess they wanted to play it extra cautious

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just in
case some bit of warming trend tried to sneak in.
The other stuff is just a distraction.

mike

On Jul 29, 2009, at 10:51 AM, Grant Foster wrote:

Gentlemen,
Attached is a zip file with LaTeX and pdf for a first draft. I've included everybody's name (in alphabetical order after mine), but of course it should only include in submission those who give explicit consent.
There are a few other issues. One is that MFC have recently removed the pdf version of their paper from the "New Zealand Climate Coalition" website. They've replaced it with this:

[1]http://nzclimatescience.net/index.php?option=com_content&task=view&id=502&Itemid=1
which refers to a graph showing only part of figure 7, and suggests that there's not trend in GTTA so "nothing to worry about." Yet the plotted GTTA is from UAH TMT (*not* TLT) so of course it shows no trend, and the MT channel is contaminated by stratospheric cooling.
In figure 7 of the paper itself they compare the 50-year record of SOI and GTTA, but their graph of GTTA is made of RATPAC-A data until 1980 grafted onto UAH TMT data afterward -- hence the lack of an obvious trend. I think this too should be mentioned, especially as the entire RATPAC-A record shows a very pronounced trend.
One last thing: there's a lot of stuff in the paper about Hadley cells and heat transport and so forth. I suspect this is really a bunch of gobbledygook -- but I don't know. But I'll bet you guys do. Comments?
Sincerely,
Grant

Windows Live(TM) Hotmail®: Celebrate the moment with your favorite sports pics.
[2]Check it out. <comment.zip>

--
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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: [3]mann@psu.edu
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References

1. http://nzclimatescience.net/index.php?option=com_content&task=view&id=502&Itemid=1
2. http://www.windowlive.com/Online/Hotmail/Campaign/QuickAdd?ocid=TXT_TAGLM_WL_QA_HM_sports_photos_072009&cat=sports
3. <mailto:mann@psu.edu>
4. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
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6. <mailto:trenbert@ucar.edu>
7. <http://www.cgd.ucar.edu/cas/trenbert.htm>

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#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Thomas.C.Peterson@noaa.gov
Subject: Re: This and that
Date: Wed Jul 29 17:19:53 2009

Tom,

Good idea with that BAMS paper. There is also the KNMI web site, which tells that they have restricted data from Europe - on the ECA part. Both despite WMO-Res40!

On IPCC, I suggested Thomas to not get too many hangers on amongst the LAs. Chs 2 and 14 are prime candidates for upping the geographic spread. We had about half of ours not doing that much last time.

Isn't Tom Karl on the US nominating committee?
Away all day tomorrow - CRU barbecue - so will pour down.

Cheers
Phil

At 17:07 29/07/2009, you wrote:

Hi, Phil,

Yes, Friday-Saturday I noticed that ClimateFraudit had renewed their interest in you. I was thinking about sending an email of sympathy, but I was busy preparing for a quick trip to Hawaii - I left Monday morning and flew out Tuesday evening and am now in the Houston airport on my way home.

Data that we can't release is a tricky thing here at NCDC. Periodically, Tom Karl will twist my arm to release data that would violate agreements and therefore hurt us in the long run, so I would prefer that you don't specifically cite me or NCDC in this.

But I can give you a good alternative. You can point to the Peterson-Manton article on regional climate change workshops. All those workshops resulted in data being provided to the author of the peer-reviewed paper with a strict promise that none of the data would be released. So far as far as I know, we have all lived up to that agreement - myself with the Caribbean data (so that is one example of data I have that are not released by NCDC), Lucie and Malcolm for South

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America, Enric for Central America, Xuebin for Middle Eastern data, Albert for south/central Asian data, John Ceasar for SE Asia, Enric again for central Africa, etc. The point being that such agreements are common and are the only way that we have access to quantitative insights into climate change in many parts of the world. Many countries don't mind the release of derived products such as your gridded field or Xuebin's ETCCDI indices, but very much object to the release of actual data (which they might sell to potential users). Does that help? Regarding AR4, I would like to be part of it. I have no idea what role would be deemed appropriate. One thing I noticed with the CLAS in my old chapter is that if one isn't up to doing his part (too busy, or a different concept of timeliness, or ...) it can make for a difficult job. You and I have worked well together before (e.g., GSN) so I'd be delighted to work with you on it and I know you'd hold up your side of the tasks. We touched on this briefly at the AOPC meeting. If I get an opportunity, I would say yes. But I also don't know what the U.S. IPCC nominating approach would be or even who decides that. There is an upcoming IPCC report on extremes and impacts of extremes and I wasn't privy to any insights into the U.S. nominations other than when it was over it was announced in NCDC staff notes that the nominations had been made. However, Kumar had earlier asked if he could nominate me, so he did (I provided him with the details).
Regards,

Tom

Tom,

If you look on Climate Audit you will see that I'm all over it! Our ftp site is regularly trawled as I guess yours is. It seems that a Canadian along with two Americans copied some files we put there for MOHC in early 2003. So saying they have the CRU data is not quite correct. What they have is our raw data for CRUTEM2 which went into Jones and Moberg (2003) - data through end of 2002.

Anyway enough of my problems - I have a question for you. I'm going to write a small document for our web site to satisfy (probably the wrong word) the 50 or so FOI/EIR requests we've had over the weekend. I will put up the various agreements we have with Met Services.

The question - I think you told me one time that you had a file containing all the data you couldn't release (i.e. it's not in GHCN).

Presumably

this is not in your gridded datasets? Do you know off hand how much data is in this category? Would NCDC mind if I mentioned that you have such data - not the amount/locations/anything, just that there is some?

On something positive - attached is the outlines for the proposed Chs in

AR5/WG1.

Ch1 is something Thomas thinks he can write himself - well with Qin Dahe, so only 13 chapters. There are a lot of issues with overlaps between some of the data chapters 2 with 3, 2 with 5 and 2 with 14.

I'm still thinking about whether to get involved. It would be 2 if I decide.

At the

moment I'd say yes, but I might change my mind tomorrow! Nominations are from Nov09 thru Jan10 with the selection made in April 10. Are you considering getting involved?

I have got the IPCC Secretariat and Thomas to raise the FOI issues with the full IPCC Plenary, which meets in Bali in September or October. Thomas is fully aware of all the issues we've had here wrt Ch 6 last time, and others

in

the US have had.

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit

School of Environmental Sciences

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Page 211

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NR4 7TJ
UK

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#####

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Michael Mann" <mann@meteo.psu.edu>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: wed, 29 Jul 2009 21:15:39 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: "Jim Salinger" <j.salinger@auckland.ac.nz>, "James Renwick"
<j.renwick@niwa.co.nz>, gschmidt@giss.nasa.gov, tamino_9@hotmail.com,
jdannan@jamstec.go.jp, "Brett Mullan" <b.mullan@niwa.co.nz>, p.jones@uea.ac.uk

Content-Type: text/plain; charset="iso-8859-1"
X-MIME-Autoconverted: from 8bit to quoted-printable by ueamailgate02.uea.ac.uk id
n6U3Feqd018708

See some suggested mods

BTW the T et al 2002 paper was one that got horribly caught up in the JGR
transition to electronic publication and the doi etc was not properly set.

It was not published on time but delayed by some 6 months when about 10
issues came out all at once, and no one read it!

Kevin

> dear all,
>
> here's a revised intro based on a few iterations between Jim and me.
> Grant--please incorporate this into your next revision of the m.s.,
>
> others feel free to suggest changes/additions/etc.
>
> thanks,
>
> mike
>
> On Jul 29, 2009, at 4:26 PM, Jim Salinger wrote:
>
>> Kia ora all and Austral Jim
>>
>> Don't get sacked now (lol).....well you must be famous if he is
>> making a complaint...I guess he can't get at me here. Mike and I are
>> just putting some wee finishing touches to the intro bit then Mike
>> will circulate it more widely later.
>>
>> It seems that Hildebrandsson was the real originator of atmospheric
>> centres of action (see attached), and that walker was just using his

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>> ideas...interesting stuff - and perhaps it is time for a review by
>> someone....Kevin???

>>

>> I concur with Phil and Mike in that we don't critique their rather
>> bad knowledge of Hadley Cell and stuff and just cut to the chase.
>> Interesting that they are EVEN cherry picking their own paper. They
>> have whipped up a storm through farmers in NZ who are using this to
>> vehemently deny climate change, and therefore not address on farm
>> emissions from CH4 and N2O and leave it to all the rest of us (when
>> 60-70% of our electricity is renewable!) so I guess we all will be
>> walking and cycling very quickly as farmers keep their animals
>> burping out methane...that's my little sermon for this morning!

>>

>> Adios for now

>>

>> Not quite so Austral Jim

>>

>>

>> James Renwick wrote:

>>> Dear all:

>>> Great stuff, while I've sat back and watched... For info, I've just
>>> heard that Bob Carter has sent a formal complaint to NIWA, about
>>> comments I made, to a local reporter, on the paper. I'll be talking
>>> to
>>> our comms people tomorrow about a response (and I haven't actually
>>> seen
>>> the complaint yet).

>>> Regards,
>>> Jim R

>>> -----

>>> Dr James Renwick
>>> Principal Scientist, Climate Variability & Change
>>> NIWA
>>> Private Bag 14901, Wellington
>>> +64-4-386-0343 +64-21-178-5550

>>>>> Jim Salinger <j.salinger@auckland.ac.nz> 07/30/09 6:22 AM >>>

>>> Kia ora All from the Land of the Long White Cloud and Thursday
>>> Thanks all...Phil I found reference to the Hildrebrandsson stuff
>>> ibn 'Recent Researches on Climate by N N Dickson in The
>>> geographical Journal 10 (3) 1897 303-306. Good fun! Mike and I
>>> will finish iterating our bit this morning and then it can be
>>> added in to Grant's fine work!

>>> Talk to you later

>>> Jim

>>> Quoting Kevin Trenberth <trenbert@ucar.edu>:

>>>> Hi all

>>>> wow this is a nice analysis by Grant et al. what we should do is
>>>> turn this into a learning experience for everyone: there is often
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>>>>>> who give explicit consent.

>>>>>>

>>>>>> There are a few other issues. One is that MFC have recently
>>>>>> removed the pdf version of their paper from the "New Zealand
>>>>>> Climate Coalition" website. They've replaced it with this:

>>>>>>

>>>>>>

>>>>>>

>>>>>> http://nzclimatescience.net/index.php?option=com_content&task=view&id=502&Itemid=1

>>>>>>

>>>>>>> which refers to a graph showing only part of figure 7, and
>>>>>>> suggests that there's not trend in GTTA so "nothing to worry
>>>>>>> about." Yet the plotted GTTA is from UAH TMT (*not* TLT) so of
>>>>>>> course it shows no trend, and the MT channel is contaminated by
>>>>>>> stratospheric cooling.

>>>>>>>

>>>>>>> In figure 7 of the paper itself they compare the 50-year record
>>>>>>> of SOI and GTTA, but their graph of GTTA is made of RATPAC-A
>>>>>>> data until 1980 grafted onto UAH TMT data afterward -- hence
>>>>>>> the lack of an obvious trend. I think this too should be
>>>>>>> mentioned, especially as the entire RATPAC-A record shows a
>>>>>>> very pronounced trend.

>>>>>>>

>>>>>>> One last thing: there's a lot of stuff in the paper about
>>>>>>> Hadley cells and heat transport and so forth. I suspect this
>>>>>>> is really a bunch of gobbledygook -- but I don't know. But

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>>>>> I'll bet you guys do. Comments?

>>>>>

>>>>> Sincerely,

>>>>> Grant

>>>>>

>>>>>

>>>>>

>>> -----

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>>>>> sports pics. Check it out.

>>>>

<http://www.windowslive.com/Online/Hotmail/Campaign/QuickAdd?ocid=TXT_TAGLM_WL_QA_HM_sports_photos_072009&cat=sports

>>> >

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>>>>> <comment.zip>

>>>>> --

>>>>> Michael E. Mann

>>>>> 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

>>>>> <<mailto:mann@psu.edu>

>>>>> >

>>>>> University Park, PA 16802-5013

>>>>>

>>>>> website: <http://www.meteo.psu.edu/~mann/Mann/index.html>

>>>>> <<http://www.meteo.psu.edu/%7Emann/Mann/index.html>

>>>>> >

>>>>> "Dire Predictions" book site:

>>>>> http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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>>> This message was sent using IMP, the Internet Messaging Program.

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>>> NIWA is the trading name of the National Institute of water &
>>> Atmospheric Research Ltd.

>>

>> --

>> *****

>> Dr Jim Salinger
>> Honorary Research Fellow
>> School of Geography, Geology and Environmental Science
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>>
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>>
>> President,
>> World Meteorological Organization
>> Commission for Agricultural Meteorology
>> *****

>> <1774775.pdf>

>

> --

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> website: <http://www.meteo.psu.edu/~mann/Mann/index.html>
> "Dire Predictions" book site:
> http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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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>

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993. 1248979991.txt

#####

From: Grant Foster <tamino_9@hotmail.com>
To: Mike Mann <mann@meteo.psu.edu>, <p.jones@uea.ac.uk>
Subject: RE: ENSO blamed over warming - paper in JGR
Date: Thu, 30 Jul 2009 14:53:11 +0000
Cc: James Annan <jdannan@jamstec.go.jp>, <trenbert@ucar.edu>, "J. Salinger"
<j.salinger@auckland.ac.nz>, <j.renwick@niwa.co.nz>, Gavin Schmidt
<gschmidt@giss.nasa.gov>, <b.mullan@niwa.co.nz>

Gentlemen,
I've combined everything (I hope!) into the latest revision. I've probably made some glaring mistake somewhere, so read it critically. It's also necessary to ensure that it all fits together coherently, and that anything we claim we'll do is actually done. I want this to be airtight, let's not leave them any "wobble room."
Referring to the inappropriate application of filters, I have a feeling that saying "perhaps not an uncommon error" is too easy on them. I have no motivation to go easy on them. Perhaps I'm being too aggressive; I defer to the majority opinion. On a few technical details, I need altaffils and authoraddresses for everybody. And make sure I've got your name right!
Sincerely,
Grant

Bing brings you maps, menus, and reviews organized in one place. [1]Try it now.
Attachment
Converted: "c:\eudora\attach\comment.zip"

References

1.
http://www.bing.com/search?q=restaurants&form=MLOGEN&publ=WLHMTAG&crea=TXT_MLOGEN_Local_Local_Restaurants_1x1

994. 1248993704.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: "Thomas R. Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: Re: [Fwd: concerns about the Southeast chapter]]
Date: Thu, 30 Jul 2009 18:41:44 -0700
Reply-to: santer1@llnl.gov
Cc: Virginia Burkett <virginia_burkett@usgs.gov>, Thomas C Peterson <Thomas.C.Peterson@noaa.gov>, Michael wehner <mfwehner@lbl.gov>, Karl Taylor <taylor13@llnl.gov>, peter gleckler <gleckler1@llnl.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante

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<John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, "'Philip D. Jones'"
<p.jones@uea.ac.uk>, Carl Mears <mears@remss.com>, Gavin Schmidt
<gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz
<frank.wentz@remss.com>

<x-flowed>
Dear Tom,

Thanks for forwarding the message from John Christy. Excuse me for being so blunt, but John's message is just a load of utter garbage.

I got a laugh out of John's claim that Santer et al. (2008) was "poorly done". This was kind of ironic coming from a co-author of the Douglass et al. (2007) paper, which used a fundamentally flawed statistical test to compare modeled and observed tropospheric temperature trends. To my knowledge, John has NEVER acknowledged that Douglass et al. used a flawed statistical test to reach incorrect conclusions - despite unequivocal evidence from the "synthetic data" experiments in Santer et al. (2008) that the Douglass et al. "robust consistency" test was simply wrong. Unbelievably, Christy continues to assert that the results of Douglass et al. (2007) "still stand". I can only shake my head in amazement at such intellectual dishonesty. I guess the best form of defense is a "robust" attack.

So how does John support his contention that Santer et al. (2008) was "poorly done"? He begins by stating that:

"Santer et al. 2008 used ERSST data which I understand has now been changed in a way that discredits the conclusion there".

Maybe you or Tom Peterson or Dick Reynolds can enlighten me on this one. How exactly have NOAA ERSST surface data changed? Recall that Santer et al. (2008) actually used two different versions of the ERSST data (version 2 and version 3). We also used HadISST sea-surface temperature data, and combined SSTs and land 2m temperature data from HadCRUT3v. In other words, we used four different observational estimates of surface temperature changes. Our bottom-line conclusion (no significant discrepancy between modeled and observed lower-tropospheric lapse-rate trends) was not sensitive to our choice of observed surface temperature dataset.

John next asserts that:

"Haimberger's v1.2-1.4 (of the radiosonde data) are clearly spurious due to the error in ECMWF as published many places".

I'll let Leo Haimberger respond to that one. And if v1.2 of Leo's data is "clearly spurious", why did John Christy agree to be a co-author on the Douglass et al. paper which uses upper-air data from v1.2?

Santer et al. (2008) comprehensively examined structural uncertainties in the observed upper-air datasets. They looked at two different satellite and seven different radiosonde-based estimates of tropospheric temperature change. As in the case of the surface temperature data, getting the statistical test right was much more important (in terms of the bottom-line conclusions) than the choice of observational upper-air dataset.

Christy's next criticism of our IJoC paper is even more absurd. He states that:

"Santer et al. 2008 asked a very different question...than we did. Our question was "Does the IPCC BEST ESTIMATE agree with the Best Data

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(including RSS)?" Answer - No. Santer et al. asked, "Does ANY IPCC model agree with ANY data set?" ... I think you can see the difference.

Actually, we asked and answered BOTH of these questions. "Tests with individual model realizations" are described in Section 4.1 of Santer et al. (2008), while Section 4.2 covers "Tests with multi-model ensemble-mean trend". As should be obvious - even to John Christy - we did NOT just compare observations with results from individual models.

For both types of test ("individual model" and "multi-model average"), we found that, if one applied appropriate statistical tests (which Douglass et al. failed to do), there was no longer a serious discrepancy between modeled and observed trends in tropical lapse rates or in tropical tropospheric temperatures.

Again, I find myself shaking my head in amazement. How can John make such patently false claims about our paper? The kindest interpretation is that he is a complete idiot, and has not even bothered to read Santer et al. (2008) before making erroneous criticisms of it. The less kind interpretation is that he is deliberately lying.

A good scientist is willing to acknowledge the errors he or she commits (such as applying an inappropriate statistical test). John Christy is not a good scientist. I'm not a religious man, but I'm sure willing to thank some higher authority that Dr. John Christy is not the "gatekeeper" of what constitutes sound science.

I hope you don't mind, Tom, but I'm copying this email to some of the other co-authors of the Santer et al. (2008) IJoC paper. They deserve to know about the kind of disinformation Christy is spreading.

with best regards,

Ben

Thomas R. Karl wrote:

> FYI

>

> ----- Original Message -----

> Subject: Re: [Fwd: concerns about the Southeast chapter]

> Date: Mon, 27 Jul 2009 09:54:22 -0500

> From: John Christy <john.christy@nsstc.uah.edu>

> To: Thomas C Peterson <Thomas.C.Peterson@noaa.gov>

> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

> References: <4A534CF9.9080700@noaa.gov>

>

>

>

> Tom:

>

> I've been on a heavy travel schedule and just now getting to emails I've
> delayed. I was in Asheville briefly Thursday for a taping for the CDMP
> project at the Biltmore estates (don't know why that was the backdrop)
> while traveling between meetings in Chapel Hill, Atlanta and here.

>

> we disagree on the use of available climate information regarding the
> many things related to climate/climate change as I see by your responses
> below - that is not unexpected as climate is an ugly, ambiguous, and
> complex system studied by a bunch of prima donnas (me included) and
> which defies authoritative declarations. I base my views on hard-core,
> published literature (some of it mine, but most of it not), so saying
> otherwise is not helpful or true. The simple fact is that the opinions
> expressed in the CCSP report do not represent the real range of

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> scientific literature (the IPCC fell into the same trap - so running to
> the IPCC's corner doesn't move things forward).

>
> I think I can boil my objections to the CCSP Impacts report to this one
> idea for the SE (and US): The changes in weather variables (measured in
> a systematic settings) of the past 30 years are within the range of
> natural variability. That's the statement that should have been front
> and center of this whole document because it is
> mathematically/scientifically defensible. And, it carries more weight
> with planners so you can say to them, "If it happened before, it will
> happen again - so get ready now." By the way, my State Climatologist
> response to the CCSP was well-received by legislators and stakeholders
> (including many in the federal government) and still gets hits at
> http://*vortex.nsstc.uah.edu/aosc/.

>
> There also was a page or so on the tropical troposphere-surface issue
> that I didn't talk about on my response. It was wrong because it did
> not include all the latest research (i.e. since 2006) on the continuing
> and significant difference between the two trends. Someone was acting
> as a fierce gatekeeper on that one - citing only things that agreed with
> the opinion shown even if poorly done (e.g. Santer et al. 2008 used
> ERSST data which I understand has now been changed in a way that
> discredits the conclusion there, and Haimberger's v1.2-1.4 are clearly
> spurious due to the error in ECMWF as published many places, but
> analyzed in detail in Sakamoto and Christy 2009). The results of
> Douglass et al. 2007 (not cited by CCSP) still stand since Santer et al.
> 2008 asked a very different question (and used bad data to boot) than we
> did. Our question was "Does the IPCC BEST ESTIMATE agree with the Best
> Data (including RSS)?" Answer - No. Santer et al. asked, "Does ANY IPCC
> model agree with ANY data set?" ... I think you can see the difference.
> The fact my 2007 tropical paper (the follow-on papers in 2009 were
> probably too late, but they substantiate the 2007 paper) was not cited
> indicates how biased this section was. Christy et al. 2007 assessed the
> accuracy of the datasets (Santer et al. did not - they assumed all
> datasets were equal without looking at the published problems) and we
> came up with a result that defied the "consensus" of the CCSP report -
> so, it was doomed to not be mentioned since it would disrupt the
> storyline. (And, as soon as RSS fixes their spurious jump in 1992, our
> MSU datasets will be almost indistinguishable.)

>
> This gets to the issue that the "consensus" reports now are just the
> consensus of those who agree with the consensus. The
> government-selected authors have become gatekeepers rather than honest
> brokers of information. That is a real tragedy, because when someone
> becomes a gatekeeper, they don't know they've become a gatekeeper - and
> begin to (sincerely) think the non-consensus scientists are just nuts
> (... it's more comfortable that way rather than giving them credit for
> being skeptical in the face of a paradigm).

> Take care.

> John C.

> p.s. a few quick notes are interspersed below.

> Thomas C Peterson wrote:

>> Hi, John,
>> I didn't want this to catch you by surprise.
>> Tom

>> ----- Original Message -----

>> Subject: concerns about the Southeast chapter

mail.2009

>> Date: Tue, 07 Jul 2009 09:25:45 -0400
>> From: Thomas C Peterson <thomas.c.peterson@noaa.gov>
>> To: jim.obrien@coaps.fsu.edu
>> CC: Tom Karl <Thomas.R.Karl@noaa.gov>

>>
>>
>>

>> Dear Jim,

>>
>>

>> First off and most importantly, congratulations on your recent
>> marriage. Anthony said it was the most touching wedding he has ever
>> been to. I wish you and your bride all the best.

>>

>> Thank you for your comments and for passing on John Christy's detailed
>> concerns about the Southeast chapter of our report, /Global Climate
>> Change Impacts in the United States/. Please let me respond to the key
>> points he raised.

>>

>> In Dr. John Christy's June 23, 2009 document "Alabama climatologist
>> responds to U.S. government report on regional impacts of global
>> climate change", he primarily focused on 4 prime concerns:

>>

- >> 1. Assessing changes since 1970.
- >> 2. Statements on hurricanes.
- >> 3. Electrical grid disturbances (from the Energy section).
- >> 4. Using models to assess the future.

>>
>>
>>

>> /1. Assessing changes since 1970./

>>

>> The Southeast section has 5 figures and one table. One figure is on
>> changes in precipitation patterns from 1901-2007. The next figure is
>> on patterns of days per year over 90F with two maps, one 1961-1979,
>> the other 2080-2099. One figure is on the change in freezing days per
>> year, 1976-2007. The next figure is on changes to a barrier island
>> land from 2002 to 2005. And the last figure was on Sea Surface
>> Temperature from 1900 to the present. The table indicates trends in
>> temperature and precipitation over two periods, 1901-2008 and
>> 1970-2008. As Dr. Christy indicates in his paper, the full period and
>> the period since 1970 are behaving differently. To help explain this,
>> the table shows them both. Of the 5 figures, only one shows the
>> changes over this shorter period.

>>

>> Since, as the IPCC has indicated, the human impact on climate isn't
>> distinguishable from natural variability until about 1950, describing
>> the changes experienced in the majority of the time since 1950 would
>> be a more logical link to future anthropogenic climate change. In
>> most of the report, maps have shown the changes over the last 50
>> years. Because of the distinct behavior of time series of
>> precipitation and temperature in the Southeast, discussing the period
>> since 1970 seemed more appropriate. Though as the figures and table
>> indicate, this shorter period is not the sole or even major focus.

>

> See crux of the matter in email above - looking at the whole time series
> is demanded by science. Any 30 or 50-year period will give changes -
> blaming the most recent on humans ignores the similar (or even more
> rapid) changes that occurred before industrialization (e.g. western
> drought in 12th century). The period since 1970 WAS the major focus in

> the SE section (mentioned 6 times in two pages). And, OF COURSE any
> 30-year sub-period will have different characteristics than the 100-year
> population from which it is extracted ... that doesn't prove anything.

>>
>>
>>

>> /2. Statements on hurricanes./

>>

>> Dr. Christy takes issue with the report's statements about hurricanes
>> and quotes a line from the report and quotes an individual hurricane
>> expert who says that he disagrees with the conclusions. The line in
>> the report that Dr. Christy quotes comes almost word for word out of
>> CCSP SAP 3.3. While individual scientists may disagree with the
>> report's conclusions, this conclusion came directly out of the
>> peer-reviewed literature and assessments. Dr. Christy also complains
>> that "the report did not include a plot of the actual hurricane
>> landfalls". However, the section in the Southeast chapter discussing
>> landfalling hurricanes states "see /National Climate Change/ section
>> for a discussion of past trends and future projections" and sure
>> enough on page 35 there is a figure showing land falling hurricanes
>> along with a more in depth discussion of hurricanes.

>>

> You didn't read my State Climatologist response carefully - I mentioned
> page 35 and noted again it talked about the most recent decades (and
> even then, the graph still didn't go back to 1850). This hurricane
> storyline was hit hard by many scientists - hence is further evidence
> the report was generated by a gatekeeper mentality.

>>
>>

>> /3. Electrical grid disturbances (from the Energy section)./

>>

>> Moving out of the Southeast, Dr. Christy complains about one figure in
>> the Energy Chapter. Citing a climate skeptic's blog which cites an
>> individual described as the keeper of the data for the Energy
>> Information Administration (EIA), John writes that the rise in weather
>> related outages is largely a function of better reporting. Yet the
>> insert of weather versus non-weather-related outages shows a much
>> greater increase in weather-related outages than non-weather-related
>> outages. If all the increases were solely due to better reporting,
>> the differences between weather- and non-weather-related outages would
>> indicate a dramatic decrease over this time period in non-weather
>> related problems such as transmission equipment failures, earthquakes,
>> faults in line, faults at substations, relaying malfunctions, and
>> vandalism.

>>

>> Thanks to the efforts of EIA, after they took over the responsibility
>> of running the Department of Energy (DOE) data-collection process
>> around 1997, data collection became more effective. Efforts were made
>> in subsequent years to increase the response rate and upgrade the
>> reporting form. It was not until EIA's improvement of the data
>> collection that the important decoupling of weather- and
>> non-weather-related events (and a corresponding increase in the
>> proportion of all events due to weather extremes) became visible.

>>

>> To adjust for potential response-rate biases, we have separated
>> weather- and non-weather-related trends into indices and found an
>> upward trend only in the weather-related time series.

>>

>> As confirmed by EIA, *if there were a systematic bias one would expect
>> it to be reflected in both data series (especially since any given
>> reporting site would report both types of events).*

>>

>> As an additional precaution, we focused on trends in the number of

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>> events (rather than customers affected) to avoid fortuitous
>> differences caused by the population density where events occur. This,
>> however, has the effect of understating the weather impacts because of
>> EIA definitions (see survey methodology notes below).

>>
>> More details are available at:
>> http://*eetd.lbl.gov/emills/pubs/grid-disruptions.html

>
> The data were not systematically taken and should not have been shown
> .. basic rule of climate.

>>
>>
>>
>> /4. Using models to assess the future./

>> Can anyone say anything about the future of the southeast's climate?
>> Evidently according to John Christy, the answer is no. The basic
>> physics of the greenhouse effect and why increasing greenhouse gases
>> are warming and should be expected to continue to warm the planet are
>> well known and explained in the /Global Climate Change/ section of the
>> report. Climate models are used around the world to both diagnose the
>> observed changes in climate and to provide projections for the
>> future. There is a huge body of peer-reviewed literature, including a
>> large number of peer-reviewed climate change assessments, supporting
>> this use. But in Dr. Christy's "view," models should not be used for
>> projections of the future, especially for the Southeast. The report
>> based, and indeed must base, its results on the huge body of
>> peer-reviewed scientific literature rather than the view of one
>> individual scientist.

>
> No one has proven models are capable of long-range forecasting.
> Modelers write and review their own literature - there are millions of
> dollars going into these enterprises, so what would you expect?
> Publication volume shouldn't impress anyone. The simple fact is we
> demonstrated in a straightforward and reproducible way that the actual
> trends over the past 30, 20, and 10 years are outside of the envelop of
> model predictions ... no one has disputed that finding with an
> alternative analysis - even when presented before congressional hearings
> where the opportunity for disagreement was openly available.

>>
>> I hope this helps relieve some of your concerns.

>>
>> Regards,
>> Tom Peterson

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> --
> *****
> John R. Christy
> Director, Earth System Science Center voice: 256-961-7763
> Professor, Atmospheric Science fax: 256-961-7751
> Alabama State Climatologist
> University of Alabama in Huntsville
> http://*www.*nsstc.uah.edu/atmos/christy.html

>
> Mail: ESSC-Cramer Hall/University of Alabama in Huntsville, Huntsville AL 35899

> Express: Cramer Hall/ESSC, 320 Sparkman Dr., Huntsville AL 35805

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>
>
> --
>
> *Thomas R. Karl, L.H.D.*
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#####

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Grant Foster" <tamino_9@hotmail.com>
Subject: RE: ENSO blamed over warming - paper in JGR
Date: Thu, 30 Jul 2009 20:01:06 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: "J. Salinger" <j.salinger@auckland.ac.nz>, "Mike Mann" <mann@meteo.psu.edu>, p.jones@uea.ac.uk, "James Annan" <jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

You have a go from me. By all means clean up. I think you should argue that it should be expedited for the reasons of interest by the press. Key question is who was the editor who handled the original, because this is an implicit criticism of that person. May need to point this out and ensure that someone else handles it.

Thanks

Kevin

>
> Gentlemen,

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>
> I've added additional suggestions received today, and made a few minor
> changes myself. Here's the latest version. Enjoy!
>
> Sincerely,
> Grant
>
>
>
>

> Windows Live™ SkyDrive™: Store, access, and share your photos. See how.
> http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009

Kevin Trenberth
Climate Analysis Section, NCAR
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ph 303 497 1318
<http://www.cgd.ucar.edu/cas/trenbert.html>

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#####

From: Michael Mann <mann@meteo.psu.edu>
To: trenbert@ucar.edu
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Thu, 30 Jul 2009 22:26:32 -0400
Cc: "Grant Foster" <tamino_9@hotmail.com>, "J. Salinger"
<j.salinger@auckland.ac.nz>, p.jones@uea.ac.uk, "James Annan"
<jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, "Gavin Schmidt"
<gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

folks, I was thinking exactly the same thing. the problems are so unusually
fundamental and
obvious, as we lay them out, that it does immediately call into suspicion the
integrity of
the review process.

We probably need to take this directly to the chief editor at JGR, asking that
this not be
handled by the editor who presided over the original paper, as this would
represent a
conflict of interest. if we are told that is not possible, then we would at least
want the
chief editor himself to closely monitor the handling of the paper.
I too am happy to sign of at this point,
mike
On Jul 30, 2009, at 10:01 PM, Kevin Trenberth wrote:

You have a go from me. By all means clean up. I think you should argue
that it should be expedited for the reasons of interest by the press. Key
question is who was the editor who handled the original, because this is
an implicit criticism of that person. May need to point this out and
ensure that someone else handles it.
Thanks
Kevin

Gentlemen,

I've added additional suggestions received today, and made a few minor
Page 225

mail.2009

changes myself. Here's the latest version. Enjoy!

Sincerely,

Grant

windows Live SkyDrive: Store, access, and share your photos. See how.

[1]http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009

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

--
Michael E. Mann
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: [3]mann@psu.edu
University Park, PA 16802-5013
website: [4]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[5]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009
2. <http://www.cgd.ucar.edu/cas/trenbert.html>
3. <mailto:mann@psu.edu>
4. <http://www.meteo.psu.edu/~mann/Mann/index.html>
5. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

997. 1249042511.txt

#####

From: Susan Parham <sp@cagconsult.co.uk>
To: Karen Dyson <kd@cagconsult.co.uk>, Mick Denness <m.denness@btcv.org.uk>, Andrew Gouldson <a.gouldson@leeds.ac.uk>, Cara Busfield <C.L.Busfield@leeds.ac.uk>, "Adger Neil Prof ((ENV))" <N.Adger@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, c.l.busfield@see.leeds.ac.uk, Tim Osborn <t.osborn@uea.ac.uk>, Tom MacInnes <tom.macinnis@npi.org.uk>, Niall Machin <nm@cagconsult.co.uk>, Peter Kenway <peter.kenway@npi.org.uk>, Emma Cranidge <ec@cagconsult.co.uk>, Denny Gray <dg@cagconsult.co.uk>, Niamh Carey <ncarey@wwf.org.uk>, Mary Anderson <ma@cagconsult.co.uk>, amanda@cdx.org.uk, Helen Chalmers <hc@cagconsult.co.uk>
Subject: JRF social impacts CC - proposal and supporting documents - final versions
Date: Fri, 31 Jul 2009 08:15:11 +0100

Dear All My colleague Emma and I are submitting everything this morning. I'm
Page 226

mail.2009

doing the

email version, Emma the 4 hard copies to the office in York before 2pm. Peter provided a

very useful edit yesterday which has got the proposal down under 4,000 words. Please find

attached: 1. Proposal registration form (I have just put in CAG details as main proposer

but flagged up its a partnership bid) 2. Summary (just under 600 words as required) 3.

Proposal 4. Budget form (their's and an extra one they agreed I could do to show who does

what days - don't worry about days shown - its provisional - we can revise and rearrange it

if we get the job!) 5. Staff Costs forms (attached to the budget form but not filled in as

they agreed we didn't have to submit these - they don't work with day rates) 6. Full CVs

for all Proposers (Emma is adding in some final material she has but couldnt access

yesterday - we will send round the very final version for your records once done this

morning) 7. Three appendices as one word document (to go with the proposal but separately

so as not to increase the word count of the proposal) 8. A rather long covering letter to

go with email and hard copy versions. If you notice I've missed something please email me!

Thanks to everyone for the their work on this. Very much appreciated. I will let you know

as soon as I hear anything. best wishes Susan ÿ¼ÿ¼ÿ¼ÿ¼ÿ¼ÿ¼ÿ¼ÿ¼ÿ¼ÿ¼ÿ¼ Dr Susan Parham

Director - CAG Consultants Tel: 020 7704 0018 Mob: 07967 816 295

sp@cagconsult.co.uk

www.cagconsult.co.uk Office: 30 Aberdeen Road, London, N5 2UH HQ: Gordon House, 6 Lissenden

Gardens, London, NW5 1LX Dear All

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mail.2009

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best wishes

Susan

Content-Type: application/octet-stream; x-mac-type=5738424E; x-unix-mode=0644; x-mac-creator=4D535744; name=CAG and Partners Application Registration Form.doc Content-Disposition: attachment; filename="CAG and Partners Application

Registration

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Registration

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x-unix-mode=0644;

x-mac-creator=4D535744; name=Application summary CAG and partners.doc

Content-Disposition:

attachment; filename="Application summary CAG and partners.doc" Attachment

Converted:

"c:\eudora\attach\Application summary CAG and partners.doc" Content-Type: application/octet-stream; x-mac-type=5738424E; x-unix-mode=0644;

x-mac-creator=4D535744;

name=CAG and Partners Application Final.doc Content-Disposition: attachment;

filename="CAG

and Partners Application Final.doc" Attachment Converted: "c:\eudora\attach\CAG

and

Partners Application Final.doc" Content-Type: application/octet-stream;

x-mac-type=584C5338; x-unix-mode=0644; x-mac-creator=5843454C; name=CAG and

Partners Budget

Form.xls Content-Disposition: attachment; filename="CAG and Partners Budget Form.xls"

Form.xls"

Attachment Converted: "c:\eudora\attach\CAG and Partners Budget Form.xls"

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x-mac-creator=4D535744;

name=CAG and Partners Additional Budget Form and Explanatory Notes.doc

Content-Disposition:

attachment; filename*0="CAG and Partners Additional Budget Form and Explanatory Notes.do";

Notes.do";

filename*1=c Attachment Converted: "c:\eudora\attach\CAG.doc" Content-Type:

application/octet-stream; x-mac-type=5738424E; x-unix-mode=0644;

x-mac-creator=4D535744;

name=CAG and Partners CVs.doc Content-Disposition: attachment; filename="CAG and

Partners

CVs.doc" Attachment Converted: "c:\eudora\attach\CAG and Partners CVs.doc"

Content-Type:

application/octet-stream; x-mac-type=5738424E; x-unix-mode=0644;

x-mac-creator=4D535744;

name=CAG and Partners Application Appendices.doc Content-Disposition: attachment;

filename="CAG and Partners Application Appendices.doc" Attachment Converted:

"c:\eudora\attach\CAG and Partners Application Appendices.doc" Content-Type:

application/octet-stream; x-mac-type=5738424E; x-unix-mode=0644;

x-mac-creator=4D535744;

name=CAG and Partners covering letter final.doc Content-Disposition: attachment;

mail.2009

filename="CAG and Partners covering letter final.doc" Attachment Converted:
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Dr Susan Parham
Director - CAG Consultants
Tel: 020 7704 0018 Mob: 07967 816 295
[1]sp@cagconsult.co.uk
www.cagconsult.co.uk
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London, N5 2UH
HQ: Gordon House, 6 Lissenden Gardens, London, NW5 1LX

References

1. <mailto:sp@cagconsult.co.uk>

998. 1249045162.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Thorne, Peter (Climate Research)" <peter.thorne@metoffice.gov.uk>
Subject: See below
Date: Fri Jul 31 08:59:22 2009

Peter,

Don't know if you got this. There is a link below to something Tom P said.

Keith is fine - seems as though there nothing malignant or cancerous
in the post op tests. Just needs to ensure the scar heals OK, then
he can come back to the madhouse.

Cheers
Phil

X-Failed-Recipients: peter.thorne@metoffice.gov.uk
Auto-Submitted: auto-replied
From: Mail Delivery System <Mailer-Daemon@uea.ac.uk>
To: p.jones@uea.ac.uk
Subject: Mail delivery failed: returning message to sender
Date: Fri, 31 Jul 2009 08:31:08 +0100

This message was created automatically by mail delivery software.
A message that you sent could not be delivered to one or more of its
recipients. This is a permanent error. The following address(es) failed:
peter.thorne@metoffice.gov.uk

SMTP error from remote mail server after end of data:
host ueamailgate01.uea.ac.uk [139.222.131.184]:

554 5.7.1 Message rejected because of unacceptable content. For help,
please quote

incident ID 3442835.

----- This is a copy of the message, including all the headers. -----

Return-path: <p.jones@uea.ac.uk>

Received: from [139.222.104.75] (helo=crupdj2.uea.ac.uk)
by ueams02.uea.ac.uk with esmtps (TLSv1:AES256-SHA:256)
(Exim 4.69)

(envelope-from <p.jones@uea.ac.uk>)

id 1Mwma3-0007wd-KH

for peter.thorne@metoffice.gov.uk; Fri, 31 Jul 2009 08:31:07 +0100

X-Mailer: QUALCOMM windows Eudora Version 7.1.0.9

Date: Fri, 31 Jul 2009 08:31:19 +0100

To: "Thorne, Peter (Climate Research)" <peter.thorne@metoffice.gov.uk>

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

Subject: Fwd: did you get a chance to see

Page 229

mail.2009

Mime-Version: 1.0
Content-Type: multipart/alternative;
boundary="====_1878687==.ALT"
-----_1878687==.ALT
Content-Type: text/plain; charset="us-ascii"; format=flowed
>Date: wed, 29 Jul 2009 13:50:57 -0400
>From: Thomas.C.Peterson@noaa.gov
>Subject: did you get a chance to see
>To: Thomas.C.Peterson@noaa.gov
>Cc: Phil Jones <p.jones@uea.ac.uk>
>X-Mailer: iPlanet Messenger Express 5.2 HotFix 2.01 (built Aug 26 2004)
>X-Accept-Language: en
>Priority: normal
>X-Canit-CHI2: 0.00
>X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)
>X-Spam-Score: 1.00 (*) [Hold at 5.00]
>APOSTROPHE_OBFUSCATION,HTML_MESSAGE,SPF(none,0)
>X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
>X-Canit-Stats-ID: 26983044 - 2dc0798c114f
>X-Antispam-Training-Forget:
>[1]https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=f
>X-Antispam-Training-Nospam:
>[2]https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=n
>X-Antispam-Training-Spam:
>[3]https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=s
>X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.185
>

>[4]<http://climateprogress.org/2009/07/29/the-video-that-anthony-watts-does-not-want-you>

-to-see-the-sinclair-climate-denial-crock-of-the-week/
>

>----- Original Message -----

>From: <Thomas.C.Peterson@noaa.gov>
>Date: Wednesday, July 29, 2009 12:07 pm
>Subject: Re: This and that
>

> > Hi, Phil,

> >
> > Yes, Friday-Saturday I noticed that ClimateFraudit had renewed their
> > interest in you. I was thinking about sending an email of
> > sympathy, but
> > I was busy preparing for a quick trip to Hawaii - I left Monday
> > morning and flew out Tuesday evening and am now in the Houston
> > airport on my way
> > home.
> >

> > Data that we can't release is a tricky thing here at NCDC.
> > Periodically, Tom Karl will twist my arm to release data that would
> > violate agreements
> > and therefore hurt us in the long run, so I would prefer that you
> > don't specifically cite me or NCDC in this.
> >

> > But I can give you a good alternative. You can point to the
> > Peterson-Manton article on regional climate change workshops. All
> > those workshops resulted in data being provided to the author of the
> > peer-reviewed paper with a strict promise that none of the data
> > would be
> > released. So far as far as I know, we have all lived up to that
> > agreement - myself with the Caribbean data (so that is one example of
> > data I have that are not released by NCDC), Lucie and Malcolm for
> > South America, Enric for Central America, Xuebin for Middle Eastern
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mail.2009

> > Enricagain for central Africa, etc. The point being that such
> > agreements are
> > common and are the only way that we have access to quantitative
> > insights into climate change in many parts of the world. Many
> > countries don't
> > mind the release of derived products such as your gridded field or
> > Xuebin's ETCCDI indices, but very much object to the release of actual
> > data (which they might sell to potential users). Does that help?
> >
> > Regarding AR4, I would like to be part of it. I have no idea what
> > role would be deemed appropriate. One thing I noticed with the CLAS
> > in my
> > old chapter is that if one isn't up to doing his part (too busy, or a
> > different concept of timeliness, or ...) it can make for a difficult
> > job. You and I have worked well together before (e.g., GSN) so I'd be
> > delighted to work with you on it and I know you'd hold up your side of
> > the tasks. We touched on this briefly at the AOPC meeting. If I
> > get an
> > opportunity, I would say yes.
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> > But I also don't know what the U.S. IPCC nominating approach would
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> > extremes and
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> > notes that the nominations had been made. However, Kumar had earlier
> > asked if he could nominate me, so he did (I provided him with the
> > details).
> > Regards,
> > Tom
> >
> >
>> Tom,
> If you look on Climate Audit you will see that I'm all over it!
> Our ftp site is regularly trawled as I guess yours is. It seems that
> a Canadian along with two Americans copied some files we put there
> for MOHC in early 2003. So saying they have the CRU data is not
> quite correct. What they have is our raw data for CRUTEM2 which
> went into Jones and Moberg (2003) - data through end of 2002.
> Anyway enough of my problems - I have a question for you. I'm
> going to write a small document for our web site to satisfy (probably the
> wrong word) the 50 or so FOI/EIR requests we've had over the weekend.
> I will put up the various agreements we have with Met Services.
> The question - I think you told me one time that you had a file
> containing all the data you couldn't release (i.e. it's not in
> GHCN). Presumably
> this is not in your gridded datasets? Do you know off hand how much
> data is in this category? Would NCDC mind if I mentioned that you
> have such data - not the amount/locations/anything, just that there is some?
>
> On something positive - attached is the outlines for the
> proposed Chs in AR5/WG1.
> Ch1 is something Thomas thinks he can write himself - well with Qin Dahe, so
> only 13 chapters. There are a lot of issues with overlaps between
> some of the
> data chapters 2 with 3, 2 with 5 and 2 with 14.
> I'm still thinking about whether to get involved. It would be 2
> if I decide. At the
> moment I'd say yes, but I might change my mind tomorrow! Nominations are
> from Nov09 thru Jan10 with the selection made in April 10. Are you
> considering
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mail.2009

> I have got the IPCC Secretariat and Thomas to raise the FOI issues with
> the full IPCC Plenary, which meets in Bali in September or October. Thomas
> is fully aware of all the issues we've had here wrt Ch 6 last
> time, and others in
> the US have had.

> Cheers
> Phil

>
>
>Prof. Phil Jones
>Climatic Research Unit Telephone +44 (0) 1603 592090
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>Norwich Email p.jones@uea.ac.uk
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-----_1878687==.ALT
Content-Type: text/html; charset="iso-8859-1"
Content-Transfer-Encoding: quoted-printable

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Cc: Phil Jones <p.jones@uea.ac.uk>
X-Mailer: iPlanet Messenger Express 5.2 HotFix 2.01 (built Aug 26 2004)
X-Accept-Language: en
Priority: normal
X-Canit-CHI2: 0.00
X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)
X-Spam-Score: 1.00 (*) [Hold at 5.00]
APOSTROPHE_OBFUSCATION,HTML_MESSAGE,SPF(none,0)
X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
X-Canit-Stats-ID: 26983044 - 2dc0798c114f
X-Antispam-Training-Forget:
[5]<https://canit.uea.ac.uk/b.php?i=3D26983044&m=3D2dc0798c114f&c=3Df>
X-Antispam-Training-Nonspam:
[6]<https://canit.uea.ac.uk/b.php?i=3D26983044&m=3D2dc0798c114f&c=3Dn>
X-Antispam-Training-Spam:
[7]<https://canit.uea.ac.uk/b.php?i=3D26983044&m=3D2dc0798c114f&c=3Ds>
X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.185

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Phil

Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784

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Norwich &nb=

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nbsp; Email p.jones@uea.ac.uk

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Prof. Phil Jones
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Norwich Email p.jones@uea.ac.uk
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References

1. <https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=f>
2. <https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=n>
3. <https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=s>
4. <http://climateprogress.org/2009/07/29/the-video-that-anthony-watts-does-not-want-you-to-see-the-sinclair-climate-denial-crock-of-the-week/>
5. file://localhost/tmp/3D.htm
6. file://localhost/tmp/3D.htm
7. file://localhost/tmp/3D.htm
8. file://localhost/tmp/3D.htm

999. 1249052097.txt

#####

From: Grant Foster <tamino_9@hotmail.com>
To: <p.jones@uea.ac.uk>, "J. Salinger" <j.salinger@auckland.ac.nz>
Subject: RE: ENSO blamed over warming - paper in JGR
Date: Fri, 31 Jul 2009 10:54:57 +0000
Cc: <j.renwick@niwa.co.nz>, Mike Mann <mann@meteo.psu.edu>, <trenbert@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Annan <jdannan@jamstec.go.jp>, <b.mullan@niwa.co.nz>

Gentlemen,
we're very close to being ready for submission; here's the latest version. I suggest a close reading, and don't forget to point out all the typos you notice. James, since you can cover the page charges I suggest you handle the actual submission (when the time comes). would you be willing to write the cover letter? Any other volunteers?
So far I've produced versions in 2-column format with graphs inline (so we can all see what it'll look like), but when we're ready I'll create a draft version with all the

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figures at the end (or if you really want to James, you can do this as well). The 2-column version takes jpg files as input, but I've already created eps files for all the figures. I *think* I've got everybody's suggestions in here, but if I've missed anything or you have further suggestions send 'em along. We're still waiting for explicit consent (and affiliation info) from B. Mullan and G. Schmidt! If either of you fellas would rather opt out that's OK -- as far as I'm concerned you're completely welcome to join or to decline. If we're as close as I think, we may be ready by Monday. Thanks, Phil, for the link to the video; a good laugh! Maybe the most amusing blog post I've seen about MFC09 is this one:

<http://deepclimate.org/2009/07/30/is-enso-responsible-for-recent-global-warming-no/>
What amuses me most is that "in its original news item on the paper, the International Climate Science Coalition had actually substituted the title of the first press release for the actual title in its link to the paper ... Thats right according to the ICSC, the papers title was Nature, not Man, is responsible for global warming. Stop the presses!
<http://deepclimate.files.wordpress.com/2009/07/icsc-july-26-short-2.jpg>
Sincerely,
Grant

Bing brings you maps, menus, and reviews organized in one place. [1]Try it now.
Attachment
Converted: "c:\eudora\attach\comment2.zip"

References

1.
http://www.bing.com/search?q=restaurants&form=MLOGEN&publ=WLHMTAG&crea=TXT_MLOGEN_Local_Local_Restaurants_1x1

1000. 1249052848.txt

#####

From: Jim Salinger <j.salinger@auckland.ac.nz>
To: Grant Foster <tamino_9@hotmail.com>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Fri, 31 Jul 2009 11:07:28 +1200
Cc: trenbert@ucar.edu, Mike Mann <mann@meteo.psu.edu>, p.jones@uea.ac.uk, James Annan <jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

<x-flowed>
Grant el al

All good to me apart from adding in the IPCC 2007 WG1 Chap 3 reference.

I checked with IJC chief editor here (Glenn McGregor) and editors usually like to publish comments asap, and send them only to the

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original authors to respond to as soon as possible.

So once the USA contingent has signed it off 'today' (Friday) and submitted it, I will send a copy to our Australian colleagues for information.

All good stuff

Best

Auckland Jim

Grant Foster wrote:

> Gentlemen,
 >
 > I've added additional suggestions received today, and made a few minor
 > changes myself. Here's the latest version. Enjoy!
 >
 > Sincerely,
 > Grant
 >
 >
 > -----
 > windows Live™ SkyDrive™: Store, access, and share your photos. See how.
 > <http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009>

</x-flowed>

1001. 1249313699.txt

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#####
#####
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From: Kevin Trenberth <trenbert@ucar.edu>
 To: Grant Foster <tamino_9@hotmail.com>
 Subject: Re: ENSO blamed over warming - paper in JGR
 Date: Mon, 03 Aug 2009 11:34:59 -0600
 Cc: p.jones@uea.ac.uk, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, Mike Mann <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

Hi Grant,

I have been tied up with other things. In looking at the paper some questions.

- 1) In Fig 1, why is the scale zero to 2? Normally a filter would be scaled to have a response function zero to 1.
- 2) In Fig 2 and 3 what are the units of "power"? It is not in the caption. Are these normalized spectra so that the area under the curve is unity? My guess is that this is the case and hence the amplification at ENSO bands. But it is important to say this and perhaps point out. Maybe the captions are sufficient? Add something like: The spectra have been normalized to have unit variance, which relatively inflates the values in the 0.2 to 0.5 frequency band. In a couple of places in text add "normalized" before "power spectrum" such as 2 lines above Fig 3 in the JGR set version.
- 3) A minor point: in the $x = \sin(2\pi vt)$ I would be inclined to add an amplitude which would then be included also in eq (1) on RHS emphasizing how the amplitude is changed.

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[My own preference would be to call the amplitude A and the A you have R (for response function)]. However it is fine as is.

Thanks
Kevin

Grant Foster wrote:

Gentlemen,

Well, I got some free time and it didn't take as long as I expected. Attached are:

- comment.zip Comment in preprint form
- draft.zip Comment in draft form (for submission)
- freeform.zip Comment NOT as preprint or draft, with larger font and double-wide graphs

I suggest we don't circulate it until folks have had one further day to check. And

double check and triple-check. If we don't hear an objection by tomorrow morning, I

suggest we submit it to JGR and feel free to circulate it.

So -- this is your last chance to suggest changes before submission, or to suggest

restraint in circulation.

Sincerely,
Grant

windows Live(TM): Keep your life in sync. [1]Check it out.

--

Kevin E. Trenberth
Climate Analysis Section,
NCAR
P. O. Box 3000,
Boulder, CO 80307

e-mail: [2]trenbert@ucar.edu
[3]www.cgd.ucar.edu/cas/trenbert.html
(303) 497 1318
(303) 497 1333 (fax)

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

References

1.

http://windowslive.com/explore?ocid=PID23384::T:WLMTAGL:ON:WL:en-US:NF_BR_sync:082009

2. <mailto:trenbert@ucar.edu>

3. <http://www.cgd.ucar.edu/cas/trenbert.html>

1002. 1249326482.txt

#####

From: Jim Salinger <j.salinger@auckland.ac.nz>
To: James Annan <jdannan@jamstec.go.jp>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Mon, 03 Aug 2009 15:08:02 +1200
Cc: Grant Foster <tamino_9@hotmail.com>, p.jones@uea.ac.uk, "J. Salinger" <j.salinger@auckland.ac.nz>, j.renwick@niwa.co.nz, Mike Mann <mann@meteo.psu.edu>, trenbert@ucar.edu, Gavin Schmidt <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

<x-flowed>
Dear James

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From the Land of the Long White Cloud to the Land of the Rising Sun....

Should we not also inquire about their time line for publishing the comment, and on the basis that is so serious, and the implications of their flawed findings ask it to be expedited.

Perhaps

We also note that the paper is now being used as the basis of campaigns against climate change policy and, should you decide to go ahead and publish our comment, expedite its acceptance.

Best

Auckland James

James Annan wrote:

> Grant Foster wrote:

>> James, since you can cover the page charges I suggest you handle the
>> actual submission (when the time comes). Would you be willing to
>> write the cover letter? Any other volunteers?

>
> Sure, I propose something like the below. I don't think there is
> anything to be gained by being overly combative wrt JGR.

>
> I look forward to the next final version of the paper :-)

>
> Covering Letter:

>
> Dear Sir/Madam,

> Please consider the attached manuscript for publication in the Journal
> of Geophysical Research (Atmospheres). We consider that the errors in
> the analysis of McLean et al are so serious that the publication of a
> Comment to correct the public record is amply justified. In view of the
> high profile of the issue, we would prefer if one of the senior editors
> could take charge of the editorial process.

> Yours sincerely..

>
>
</x-flowed>

1003. 1249503274.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>, Grant Foster <tamino_9@hotmail.com>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Wed Aug 5 16:14:34 2009
Cc: "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, Mike Mann <mann@meteo.psu.edu>, j.renwick@niwa.co.nz

Hi all,

Agree with Kevin that Tom Karl has too much to do. Tom Wigley is semi retired and like Mike Wallace may not be responsive to requests from JGR. We have Ben Santer in common ! Dave Thompson is a good suggestion.

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I'd go for one of Tom Peterson or Dave Easterling.

To get a spread, I'd go with 3 US, One Australian and one in Europe. So Neville Nicholls and David Parker.

All of them know the sorts of things to say - about our comment and the awful original, without any prompting.

Cheers

Phil

At 15:50 05/08/2009, Kevin Trenberth wrote:

Hi all

I went to JGR site to look for index codes, and I see that the offending article has been downloaded 128 times in past week (second). All the more reason to get on with it.

see below

Kevin

Grant Foster wrote:

Gentlemen,

I've completed most of the submission to JGR, but there are three required entries I

hope you can help me with.

1) Keyword

Please provide 1 unique keyword

global temperatures, statistical methods, El Nino-Southern Oscillation, global warming

2) Index Terms

Please provide 3 unique index terms

1600 GLOBAL CHANGE
1616 Climate variability
3309 Climatology
1694 Instruments and techniques

3) Suggested Reviewers to Include

Please list the names of 5 experts who are knowledgeable in your area and could give an unbiased review of your work. Please do not list colleagues who are close associates, collaborators, or family members. (this requires name, email, and institution).

Tom Wigley [1]wigley@ucar.edu NCAR

Ben Santer [2]<santer1@llnl.gov> Lawrence Livermore

most Mike Wallace [3]<wallace@atmos.washington.edu> U Washington [May not be

responsive]

Dave Thompson [4]<davet@atmos.colostate.edu> Col State Univ

Dave Easterling [5]<David.Easterling@noaa.gov> NCDC

Sincerely,
Grant

windows Live: Keep your life in sync. [6]Check it out.

mail.2009

--

Kevin E. Trenberth
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Boulder, CO 80307

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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

References

1. <mailto:wigley@ucar.edu>
2. <mailto:santer1@llnl.gov>
3. <mailto:wallace@atmos.washington.edu>
4. <mailto:davet@atmos.colostate.edu>
5. <mailto:David.Easterling@noaa.gov>
- 6.

http://windowslive.com/explore?ocid=PID23384::T:WLMTAGL:ON:WL:en-US:NF_BR_sync:082009

7. <mailto:trenbert@ucar.edu>
8. <http://www.cgd.ucar.edu/cas/trenbert.html>

1004. 1249652050.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: "Thomas R. Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: Re: [Fwd: concerns about the Southeast chapter]]
Date: Fri, 07 Aug 2009 09:34:10 -0700
Reply-to: santer1@llnl.gov
Cc: Virginia Burkett <virginia_burkett@usgs.gov>, Thomas C Peterson <Thomas.C.Peterson@noaa.gov>, Michael wehner <mfwehner@lbl.gov>, Karl Taylor <taylor13@llnl.gov>, peter gleckler <gleckler1@llnl.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, "Philip D. Jones" <p.jones@uea.ac.uk>, carl mears <mears@remss.com>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

<x-flowed>
Dear Tom,

I'm inclined to agree with Mike. Some people are accessible to rational scientific debate. They are good Bayesians - when confronted with new scientific information, they are capable of modifying previously-held views. John Christy is not accessible to rational scientific debate. New evidence does not cause him to change his views. He simply claims that the new evidence is wrong. From John's perspective, any datasets in disagreement with UAH-based estimates of tropospheric temperature change constitute "bad data".

mail.2009

John is incapable of recognizing and admitting that Douglass et al. used a flawed statistical test to reach incorrect conclusions. He continues to misrepresent the analyses we performed in our response to Douglass et al. I don't see what useful purpose can be served by trying to engage him in reasonable scientific debate.

At the Hawaii IPCC meeting in March, John stood up in front of an audience of IPCC Working Group I Lead Authors and attempted to portray himself as a victim of scientific discrimination. He claimed that his "alternative" views on the nature and causes of climate change were being ignored by the mainstream scientific community. This claim is bogus. The "mainstream" scientific community has not ignored the "alternative" views of folks like John Christy. The sad reality is that we've wasted an inordinate amount of time responding to the flawed science and incorrect claims of John and his colleagues.

I'm hopeful that I won't have to waste much more time on the "great satellite debate". In my personal opinion, we're already well past the point of diminishing returns on this debate. The point of diminishing returns was reached three years ago, when you overcame great obstacles to lead a fractious bunch of scientists to the successful completion of the first CCSP Report.

With best regards,

Ben

Thomas R. Karl wrote:

> Ben,

>

> Just got to this. I wonder if it would be useful to directly respond to
> John, or would this be a time sink? Maybe a cleaned up version of this
> is a single response? Just thinking out loud.

>

> Thanks Ben

>

> P.S. I have no idea what he is talking about regarding ERST.

>

>

> Ben Santer said the following on 7/30/2009 9:41 PM:

>> Dear Tom,

>>

>> Thanks for forwarding the message from John Christy. Excuse me for
>> being so blunt, but John's message is just a load of utter garbage.

>>

>> I got a laugh out of John's claim that Santer et al. (2008) was
>> "poorly done". This was kind of ironic coming from a co-author of the
>> Douglass et al. (2007) paper, which used a fundamentally flawed
>> statistical test to compare modeled and observed tropospheric
>> temperature trends. To my knowledge, John has NEVER acknowledged that
>> Douglass et al. used a flawed statistical test to reach incorrect
>> conclusions - despite unequivocal evidence from the "synthetic data"
>> experiments in Santer et al. (2008) that the Douglass et al. "robust
>> consistency" test was simply wrong. Unbelievably, Christy continues to
>> assert that the results of Douglass et al. (2007) "still stand". I can
>> only shake my head in amazement at such intellectual dishonesty. I
>> guess the best form of defense is a "robust" attack.

>>

>> So how does John support his contention that Santer et al. (2008) was
>> "poorly done"? He begins by stating that:

>>

>> "Santer et al. 2008 used ERSST data which I understand has now been
>> changed in a way that discredits the conclusion there".

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>>
>> Maybe you or Tom Peterson or Dick Reynolds can enlighten me on this
>> one. How exactly have NOAA ERSST surface data changed? Recall that
>> Santer et al. (2008) actually used two different versions of the ERSST
>> data (version 2 and version 3). We also used HadISST sea-surface
>> temperature data, and combined SSTs and land 2m temperature data from
>> HadCRUT3v. In other words, we used four different observational
>> estimates of surface temperature changes. Our bottom-line conclusion
>> (no significant discrepancy between modeled and observed
>> lower-tropospheric lapse-rate trends) was not sensitive to our choice
>> of observed surface temperature dataset.

>>
>> John next asserts that:

>>
>> "Haimberger's v1.2-1.4 (of the radiosonde data) are clearly spurious
>> due to the error in ECMWF as published many places".

>>
>> I'll let Leo Haimberger respond to that one. And if v1.2 of Leo's data
>> is "clearly spurious", why did John Christy agree to be a co-author on
>> the Douglass et al. paper which uses upper-air data from v1.2?

>>
>> Santer et al. (2008) comprehensively examined structural uncertainties
>> in the observed upper-air datasets. They looked at two different
>> satellite and seven different radiosonde-based estimates of
>> tropospheric temperature change. As in the case of the surface
>> temperature data, getting the statistical test right was much more
>> important (in terms of the bottom-line conclusions) than the choice of
>> observational upper-air dataset.

>>
>> Christy's next criticism of our IJoC paper is even more absurd. He
>> states that:

>>
>> "Santer et al. 2008 asked a very different question...than we did. Our
>> question was "Does the IPCC BEST ESTIMATE agree with the Best Data
>> (including RSS)?" Answer - No. Santer et al. asked, "Does ANY IPCC
>> model agree with ANY data set?" ... I think you can see the difference.

>>
>> Actually, we asked and answered BOTH of these questions. "Tests with
>> individual model realizations" are described in Section 4.1 of Santer
>> et al. (2008), while Section 4.2 covers "Tests with multi-model
>> ensemble-mean trend". As should be obvious - even to John Christy - we
>> did NOT just compare observations with results from individual models.

>>
>> For both types of test ("individual model" and "multi-model average"),
>> we found that, if one applied appropriate statistical tests (which
>> Douglass et al. failed to do), there was no longer a serious
>> discrepancy between modeled and observed trends in tropical lapse
>> rates or in tropical tropospheric temperatures.

>>
>> Again, I find myself shaking my head in amazement. How can John make
>> such patently false claims about our paper? The kindest interpretation
>> is that he is a complete idiot, and has not even bothered to read
>> Santer et al. (2008) before making erroneous criticisms of it. The
>> less kind interpretation is that he is deliberately lying.

>>
>> A good scientist is willing to acknowledge the errors he or she
>> commits (such as applying an inappropriate statistical test). John
>> Christy is not a good scientist. I'm not a religious man, but I'm sure
>> willing to thank some higher authority that Dr. John Christy is not
>> the "gatekeeper" of what constitutes sound science.

>>
>> I hope you don't mind, Tom, but I'm copying this email to some of the
>> other co-authors of the Santer et al. (2008) IJoC paper. They deserve

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>> to know about the kind of disinformation Christy is spreading.

>>

>> with best regards,

>>

>> Ben

>>

>> Thomas R. Karl wrote:

>>> FYI

>>>

>>> ----- Original Message -----

>>> Subject: Re: [Fwd: concerns about the Southeast chapter]

>>> Date: Mon, 27 Jul 2009 09:54:22 -0500

>>> From: John Christy <john.christy@nsstc.uah.edu>

>>> To: Thomas C Peterson <Thomas.C.Peterson@noaa.gov>

>>> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

>>> References: <4A534CF9.9080700@noaa.gov>

>>>

>>>

>>>

>>> Tom:

>>>

>>> I've been on a heavy travel schedule and just now getting to emails
>>> I've delayed. I was in Asheville briefly Thursday for a taping for
>>> the CDMP project at the Biltmore estates (don't know why that was the
>>> backdrop) while traveling between meetings in Chapel Hill, Atlanta
>>> and here.

>>>

>>> We disagree on the use of available climate information regarding the
>>> many things related to climate/climate change as I see by your
>>> responses below - that is not unexpected as climate is an ugly,
>>> ambiguous, and complex system studied by a bunch of prima donnas (me
>>> included) and which defies authoritative declarations. I base my
>>> views on hard-core, published literature (some of it mine, but most
>>> of it not), so saying otherwise is not helpful or true. The simple
>>> fact is that the opinions expressed in the CCSP report do not
>>> represent the real range of scientific literature (the IPCC fell into
>>> the same trap - so running to the IPCC's corner doesn't move things
>>> forward).

>>>

>>> I think I can boil my objections to the CCSP Impacts report to this
>>> one idea for the SE (and US): The changes in weather variables
>>> (measured in a systematic settings) of the past 30 years are within
>>> the range of natural variability. That's the statement that should
>>> have been front and center of this whole document because it is
>>> mathematically/scientifically defensible. And, it carries more
>>> weight with planners so you can say to them, "If it happened before,
>>> it will happen again - so get ready now." By the way, my State
>>> Climatologist response to the CCSP was well-received by legislators
>>> and stakeholders (including many in the federal government) and still
>>> gets hits at http://**vortex.nsstc.uah.edu/aosc/.

>>>

>>> There also was a page or so on the tropical troposphere-surface issue
>>> that I didn't talk about on my response. It was wrong because it did
>>> not include all the latest research (i.e. since 2006) on the
>>> continuing and significant difference between the two trends.
>>> Someone was acting as a fierce gatekeeper on that one - citing only
>>> things that agreed with the opinion shown even if poorly done (e.g.
>>> Santer et al. 2008 used ERSST data which I understand has now been
>>> changed in a way that discredits the conclusion there, and
>>> Haimberger's v1.2-1.4 are clearly spurious due to the error in ECMWF
>>> as published many places, but analyzed in detail in Sakamoto and
>>> Christy 2009). The results of Douglass et al. 2007 (not cited by
>>> CCSP) still stand since Santer et al. 2008 asked a very different

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>>> question (and used bad data to boot) than we did. Our question was
>>> "Does the IPCC BEST ESTIMATE agree with the Best Data (including
>>> RSS)?" Answer - No. Santer et al. asked, "Does ANY IPCC model agree
>>> with ANY data set?" ... I think you can see the difference. The fact
>>> my 2007 tropical paper (the follow-on papers in 2009 were probably
>>> too late, but they substantiate the 2007 paper) was not cited
>>> indicates how biased this section was. Christy et al. 2007 assessed
>>> the accuracy of the datasets (Santer et al. did not - they assumed
>>> all datasets were equal without looking at the published problems)
>>> and we came up with a result that defied the "consensus" of the CCSP
>>> report - so, it was doomed to not be mentioned since it would disrupt
>>> the storyline. (And, as soon as RSS fixes their spurious jump in
>>> 1992, our MSU datasets will be almost indistinguishable.)

>>> This gets to the issue that the "consensus" reports now are just the
>>> consensus of those who agree with the consensus. The
>>> government-selected authors have become gatekeepers rather than
>>> honest brokers of information. That is a real tragedy, because when
>>> someone becomes a gatekeeper, they don't know they've become a
>>> gatekeeper - and begin to (sincerely) think the non-consensus
>>> scientists are just nuts (... it's more comfortable that way rather
>>> than giving them credit for being skeptical in the face of a paradigm).

>>> Take care.

>>> John C.

>>> p.s. a few quick notes are interspersed below.

>>> Thomas C Peterson wrote:

>>>> Hi, John,

>>>> I didn't want this to catch you by surprise.

>>>> Tom

>>>> ----- Original Message -----

>>>> Subject: concerns about the Southeast chapter

>>>> Date: Tue, 07 Jul 2009 09:25:45 -0400

>>>> From: Thomas C Peterson <thomas.c.peterson@noaa.gov>

>>>> To: jim.obrien@coaps.fsu.edu

>>>> CC: Tom Karl <Thomas.R.Karl@noaa.gov>

>>>> Dear Jim,

>>>> First off and most importantly, congratulations on your recent
>>>> marriage. Anthony said it was the most touching wedding he has ever
>>>> been to. I wish you and your bride all the best.

>>>> Thank you for your comments and for passing on John Christy's
>>>> detailed concerns about the Southeast chapter of our report, /Global
>>>> Climate Change Impacts in the United States/. Please let me respond
>>>> to the key points he raised.

>>>> In Dr. John Christy's June 23, 2009 document "Alabama climatologist
>>>> responds to U.S. government report on regional impacts of global
>>>> climate change", he primarily focused on 4 prime concerns:

>>>> 1. Assessing changes since 1970.

>>>> 2. Statements on hurricanes.

>>>>
>>>> 3. Electrical grid disturbances (from the Energy section).
>>>>
>>>> 4. Using models to assess the future.
>>>>
>>>>

>>>> /1. Assessing changes since 1970./
>>>>

>>>> The Southeast section has 5 figures and one table. One figure is on
>>>> changes in precipitation patterns from 1901-2007. The next figure is
>>>> on patterns of days per year over 90F with two maps, one 1961-1979,
>>>> the other 2080-2099. One figure is on the change in freezing days
>>>> per year, 1976-2007. The next figure is on changes to a barrier
>>>> island land from 2002 to 2005. And the last figure was on Sea
>>>> Surface Temperature from 1900 to the present. The table indicates
>>>> trends in temperature and precipitation over two periods, 1901-2008
>>>> and 1970-2008. As Dr. Christy indicates in his paper, the full
>>>> period and the period since 1970 are behaving differently. To help
>>>> explain this, the table shows them both. Of the 5 figures, only one
>>>> shows the changes over this shorter period.

>>>> Since, as the IPCC has indicated, the human impact on climate isn't
>>>> distinguishable from natural variability until about 1950,
>>>> describing the changes experienced in the majority of the time since
>>>> 1950 would be a more logical link to future anthropogenic climate
>>>> change. In most of the report, maps have shown the changes over the
>>>> last 50 years. Because of the distinct behavior of time series of
>>>> precipitation and temperature in the Southeast, discussing the
>>>> period since 1970 seemed more appropriate. Though as the figures and
>>>> table indicate, this shorter period is not the sole or even major
>>>> focus.

>>>>
>>>> See crux of the matter in email above - looking at the whole time
>>>> series is demanded by science. Any 30 or 50-year period will give
>>>> changes - blaming the most recent on humans ignores the similar (or
>>>> even more rapid) changes that occurred before industrialization (e.g.
>>>> western drought in 12th century). The period since 1970 WAS the
>>>> major focus in the SE section (mentioned 6 times in two pages). And,
>>>> OF COURSE any 30-year sub-period will have different characteristics
>>>> than the 100-year population from which it is extracted ... that
>>>> doesn't prove anything.

>>>>
>>>>
>>>> /2. Statements on hurricanes./
>>>>

>>>> Dr. Christy takes issue with the report's statements about
>>>> hurricanes and quotes a line from the report and quotes an
>>>> individual hurricane expert who says that he disagrees with the
>>>> conclusions. The line in the report that Dr. Christy quotes comes
>>>> almost word for word out of CCSP SAP 3.3. While individual
>>>> scientists may disagree with the report's conclusions, this
>>>> conclusion came directly out of the peer-reviewed literature and
>>>> assessments. Dr. Christy also complains that "the report did not
>>>> include a plot of the actual hurricane landfalls". However, the
>>>> section in the Southeast chapter discussing landfalling hurricanes
>>>> states "see /National Climate Change/ section for a discussion of
>>>> past trends and future projections" and sure enough on page 35 there
>>>> is a figure showing land falling hurricanes along with a more in
>>>> depth discussion of hurricanes.

>>>>
>>>> You didn't read my State Climatologist response carefully - I

>>> mentioned page 35 and noted again it talked about the most recent
>>> decades (and even then, the graph still didn't go back to 1850).
>>> This hurricane storyline was hit hard by many scientists - hence is
>>> further evidence the report was generated by a gatekeeper mentality.

>>>>
>>>>

>>>> /3. Electrical grid disturbances (from the Energy section)./
>>>>

>>>> Moving out of the Southeast, Dr. Christy complains about one figure
>>>> in the Energy Chapter. Citing a climate skeptic's blog which cites
>>>> an individual described as the keeper of the data for the Energy
>>>> Information Administration (EIA), John writes that the rise in
>>>> weather related outages is largely a function of better reporting.
>>>> Yet the insert of weather versus non-weather-related outages shows a
>>>> much greater increase in weather-related outages than
>>>> non-weather-related outages. If all the increases were solely due
>>>> to better reporting, the differences between weather- and
>>>> non-weather-related outages would indicate a dramatic decrease over
>>>> this time period in non-weather related problems such as
>>>> transmission equipment failures, earthquakes, faults in line, faults
>>>> at substations, relaying malfunctions, and vandalism.

>>>>

>>>> Thanks to the efforts of EIA, after they took over the
>>>> responsibility of running the Department of Energy (DOE)
>>>> data-collection process around 1997, data collection became more
>>>> effective. Efforts were made in subsequent years to increase the
>>>> response rate and upgrade the reporting form. It was not until EIA's
>>>> improvement of the data collection that the important decoupling of
>>>> weather- and non-weather-related events (and a corresponding
>>>> increase in the proportion of all events due to weather extremes)
>>>> became visible.

>>>>

>>>> To adjust for potential response-rate biases, we have separated
>>>> weather- and non-weather-related trends into indices and found an
>>>> upward trend only in the weather-related time series.

>>>>

>>>> As confirmed by EIA, *if there were a systematic bias one would
>>>> expect it to be reflected in both data series (especially since any
>>>> given reporting site would report both types of events).*

>>>>

>>>> As an additional precaution, we focused on trends in the number of
>>>> events (rather than customers affected) to avoid fortuitous
>>>> differences caused by the population density where events occur.
>>>> This, however, has the effect of understating the weather impacts
>>>> because of EIA definitions (see survey methodology notes below).

>>>>

>>>> More details are available at:
>>>> http://**eetd.lbl.gov/emills/pubs/grid-disruptions.html

>>>>

>>>> The data were not systematically taken and should not have been shown
>>>> .. basic rule of climate.

>>>>

>>>>

>>>>

>>>> /4. Using models to assess the future./
>>>>

>>>> Can anyone say anything about the future of the Southeast's climate?
>>>> Evidently according to John Christy, the answer is no. The basic
>>>> physics of the greenhouse effect and why increasing greenhouse gases
>>>> are warming and should be expected to continue to warm the planet
>>>> are well known and explained in the /Global Climate Change/ section
>>>> of the report. Climate models are used around the world to both
>>>> diagnose the observed changes in climate and to provide projections

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>>>> for the future. There is a huge body of peer-reviewed literature,
>>>> including a large number of peer-reviewed climate change
>>>> assessments, supporting this use. But in Dr. Christy's "view,"
>>>> models should not be used for projections of the future, especially
>>>> for the Southeast. The report based, and indeed must base, its
>>>> results on the huge body of peer-reviewed scientific literature
>>>> rather than the view of one individual scientist.

>>>>
>>> No one has proven models are capable of long-range forecasting.
>>> Modelers write and review their own literature - there are millions
>>> of dollars going into these enterprises, so what would you expect?
>>> Publication volume shouldn't impress anyone. The simple fact is we
>>> demonstrated in a straightforward and reproducible way that the
>>> actual trends over the past 30, 20, and 10 years are outside of the
>>> envelop of model predictions ... no one has disputed that finding
>>> with an alternative analysis - even when presented before
>>> congressional hearings where the opportunity for disagreement was
>>> openly available.

>>>>
>>>> I hope this helps relieve some of your concerns.

>>>>
>>>> Regards,
>>>>
>>>> Tom Peterson

>>>>
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>
>>>> --

>>>> *****
>>>> John R. Christy
>>>> Director, Earth System Science Center voice: 256-961-7763
>>>> Professor, Atmospheric Science fax: 256-961-7751
>>>> Alabama State Climatologist
>>>> University of Alabama in Huntsville
>>>> http://**www.**nsstc.uah.edu/atmos/christy.html
>>>>
>>>> Mail: ESSC-Cramer Hall/University of Alabama in Huntsville,
>>>> Huntsville AL 35899
>>>> Express: Cramer Hall/ESSC, 320 Sparkman Dr., Huntsville AL 35805
>>>>
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>>>>

>>>> --
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>>>> Thomas.R.Karl@noaa.gov <mailto:Thomas.R.Karl@noaa.gov>

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</x-flowed>

1005. 1249655311.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Grant Foster <tamino_9@hotmail.com>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Fri, 7 Aug 2009 10:28:31 -0400
Cc: <trenbert@ucar.edu>, <p.jones@uea.ac.uk>, "J. Salinger"
<j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>,
<b.mullan@niwa.co.nz>, Gavin Schmidt <gschmidt@giss.nasa.gov>,
<j.renwick@niwa.co.nz>

good news Grant, we can trust him to be professional.

on a related note, a few folks have expressed concern that the galley-formatting
of the article w/out any label such as "submitted to JGR" is a bit misleading. some

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people think
the paper has already gone to press!

we should add a clear label such as "sub judice" or "submitted" to any posted
and/or
circulating version of this,

mike

p.s. I've already had to correct both Andy Revkin and Joe Romm on this!

On Aug 6, 2009, at 7:19 PM, Grant Foster wrote:

Greetings,
I thought I'd let you all know that Steve Gahn has been assigned as editor for
the
submission.
Sincerely,
Grant

windows Live: keep your life in sync. [1]Check it out.

--
Michael E. Mann
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
website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.htm>
"Dire Predictions" book site:
[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

visible links

1. http://windowslive.com/explore?ocid=PID23384::T:WLMTAGL:ON:WL:en-US:NF_BR_sync:082009
2. <mailto:mann@psu.edu>
3. <http://www.meteo.psu.edu/~mann/Mann/index.htm>
4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

1006. 1250169233.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Nature Aug 12
Date: Thu Aug 13 09:13:53 2009

Mike, Gavin,

See the attached - odd quote by McIntyre in the middle of this
Page 250

mail.2009

.. he is not interested in challenging the science of climate change or in nit-picking, but is simply asking that the data be made available. "The only policy I want people to

change is their data-access policy"

I must have been in a parallel universe for the past 7-8 years!

The CRU web page referred to in the article is this one.

[1]<http://www.cru.uea.ac.uk/cru/data/availability/>

I'm off at noon today - back in on Aug 20. I'll be checking email once a day, but will not be looking at blog sites.

Olive Heffernan at Nature expects the Nature blog site to be hijacked by the deniers.

She also said she would put up an expanded article, but I can't see this.

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

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University of East Anglia

Norwich

Email p.jones@uea.ac.uk

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UK

References

- 1. <http://www.cru.uea.ac.uk/cru/data/availability/>

1007. 1250174764.txt

#####

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

To: "Niklaus Zimmermann" <niklaus.zimmermann@wsl.ch>

Subject: ECOCHANGE budget available to UEA

Date: Thu Aug 13 10:46:04 2009

Cc: k.briffa@uea.ac.uk

Nick,

Apologies if I've asked you this before, but I'm being asked about the ECOCHANGE budget that appears to be available to UEA.

With the UEA budget there is money in categories that UEA has not had money in before (in other EU projects). Do you know what this money is supposed to be for? We understand the budget for personnel and also travel, but it is the other categories - which seem to relate to more travel and costs for capital equipment.

Keith is still off work, but is recovering well from his operation. I'm off in the next few hours for 2 weeks away.

Cheers

Phil

Prof. Phil Jones

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Email p.jones@uea.ac.uk

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1008. 1251384906.txt

#####

From: P.Jones@uea.ac.uk
To: "Niklaus E. Zimmermann" <niklaus.zimmermann@wsl.ch>
Subject: Re: ECOCHANGE budget available to UEA - update
Date: Thu, 27 Aug 2009 10:55:06 +0100 (BST)
Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Emmanuel Muhr" <emuhr@vitamib.com>, k.briffa@uea.ac.uk

Nick,
Thanks. Perhaps I'll need to contact Keith
as to why some of the items are in the budget.
I understand about the salary money.

Cheers
Phil

> Dear Phil, Emmanuel,
>
> sorry for late reply, I undergo
> evaluation these days. I add Emmanuel, so that he
> can correct if my answers are wrong!!!
>
> - In general, you decide how much you spend where as long
> as you have open tasks you are expected to contribute
> (which is the case for UEA, you are still involved in A5).
>
> - This means that you spend the money by declaration on
> the project netboard, and not by the original budget.
>
> - You cannot spend more salary, should there be no open
> task left for you.
>
> - You can spend more salary months than expected from the
> budget for a specific position, but you cannot spend
> more total money than the budget is.
>
> - One major constraint is teaching activity, which can
> only be spent in ECOCHANGE teaching activities (summer
> school), but you did not list any here.
>
> best,
> Nick
>
> PS: Dear Keith, I wish you all the best for
> recovery! Hope to see you soon again.
>
> At 17:34 26.08.2009, Phil Jones wrote:
>
>> Nick,
>> I've now found out some more information.
>>
>> In the Consumables category, we had £5070 and
>> have left £4543. There is little, we are
>> generally able to buy in this category.
>>
>> In a new category to us (called Recurrent
>> costs) there is £7013, with nothing spent.
>>
>> In another new category to us (called
>> Equipment under £5000) there is £5766, again with nothing spent.

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>>
>> In another new category to us (called
>> Exceptional Non Payments) there is £3844, again with nothing spent.
>>
>> Finally in travel there was £22923 of which
>> we've spent (for meetings so far) £3445 so far, leaving £19477.
>>
>> These numbers were in Euros, but our accounts have them in UK pounds.
>> They have been converted using the official EU
>> rates eoros/pounds. This should be about
>> 1.2 Euros equals one UK pound.
>>
>> We are talking about 36 thousand pounds! We
>> are almost spent up on salaries.
>>
>> Cheers
>> Phil
>>
>> Nick,
>> Apologies if I've asked you this before, but I'm being asked about
>> the ECOCHANGE budget that appears to be available to UEA.
>>
>> With the UEA budget there is money in categories that UEA has not
>> had money in before (in other EU projects). Do you know what this money
>> is supposed to be for? We understand the budget for personnel and also
>> travel, but it is the other categories - which seem to relate to more
>> travel
>> and costs for capital equipment.
>>
>> Keith is still off work, but is recovering well from his operation.
>>
>> I'm off in the next few hours for 2 weeks away.
>>
>> Cheers
>> Phil
>>
>>
>>
>>Prof. Phil Jones
>>Climatic Research Unit Telephone +44 (0) 1603 592090
>>School of Environmental Sciences Fax +44 (0) 1603 507784
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> -----
> Dr. Niklaus E. Zimmermann
> Research Unit Head
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> Zuercherstrasse 111, CH-8903 Birmensdorf, Switzerland
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> phone: +41 (0)44-739-2337, fax: +41 (0)44-739-2215
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>
> email: niklaus.zimmermann@wsl.ch
> URL: <http://www.wsl.ch/staff/niklaus.zimmermann/>

> -----
>
>

1009. 1252090220.txt

#####

From: Ian Harris <i.harris@uea.ac.uk>
To: t.osborn@uea.ac.uk
Subject: Re: Hopefully fixed TMP
Date: Fri, 4 Sep 2009 14:50:20 +0100

<x-flowed>
Hi Tim

I've re-run with the same database used for the previous 2006 run
(tmp.0705101334.dtb).

/cru/cruts/version_3_0/update_top/gridded_finals/data/data.0909041051/
tmp/cru_ts_3_00.1901.2008.tmp.dat.nc.gz

Is that any better? If not please can you send the traditional multi-
page country plots for me to pore over?

Cheers

Harry

On 3 Sep 2009, at 17:04, Tim Osborn wrote:

> Hi Harry and Phil,
>
> the mean level of the "updated-to-2008" CRU TS 3.0 now looks good,
> matching closely with the 1961-1990 means of the earlier CRU TS 3.0
> and
> CRU TS 2.1.
>
> Please see the attached PDF of country mean time series, comparing
> last-year's CRU TS 3.0 (black, up to 2005) with the most-recent CRU
> TS 3.0
> (pink, up to 2008).
>
> Latest version matches last-year's version well for the most part, and
> where differences do occur I can't say that the new version is any
> worse
> than last-year's version (some may be better).
>
> One exception is the hot JJA in Europe in 2003. This is less
> extreme in
> the latest version. See attached PNG for a blow-up of France in JJA.
>
> I'm sure some people will use CRU TS 3.0 to look at 2003 in Europe,
> so we
> need to be happy with the version we release.
>
> Perhaps some hot stations have been dropped as outliers (more than 3
> standard deviations from the mean)?
>

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> But I'm not sure if that is the reason, since outlier checking was
> already
> used in last-year's version, wasn't it?
>
> Does the outlier checking always check +-3 SD from 61-90 mean (or
> normal),
> or does it check +-3 SD from the local mean (30-years centred on the
> value) which would allow for a gradual warming in both mean and
> outlier
> threshold?
>
> Cheers
>
> Tim
>
> On wed, September 2, 2009 6:08 pm, Ian Harris wrote:
>> Tim
>>
>> when you have the time and/or the inclination, please can you run the
>> new TMP output through your IDL thingummajig?
>>
>> /cru/cruts/version_3_0/update_top/gridded_finals/data/data.
>> 0909021348/
>> tmp/cru_ts_3_00.1901.2008.tmp.dat.nc.gz
>>
>> Please let me know if you can't access it. I do appreciate your help!
>>
>> Cheers
>>
>> Harry
>
> --
> Dr. Tim Osborn
> RCUK Academic Fellow
> Climatic Research Unit
> School of Environmental Sciences
> University of East Anglia
> Norwich NR4 7TJ, UK
> www.cru.uea.ac.uk/~timo/

Ian "Harry" Harris
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

</x-flowed>

1010. 1252154659.txt

#####

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
To: Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, David
Schneider <dschneid@ucar.edu>, Jonathan Overpeck <jto@email.arizona.edu>, "Bette L.
Otto-Bliesner" <ottobli@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Miller
Giff <gmiller@colorado.edu>, Bo Vinther <bo@gfy.ku.dk>, Keith Briffa
<k.briffa@uea.ac.uk>
Subject: Arctic2k update?
Date: Sat, 5 Sep 2009 08:44:19 -0700

Cc: <mann@psu.edu>

All:

I received my first hate mail this AM, which helped me to realize that I shouldn't be wasting time reading the blogs.

Regarding the "upside down man", as Nick's plot shows, when flipped, the Korttajarvi series has little impact on the overall reconstructions. Also, the series was not included in the calibration. Nonetheless, it's unfortunate that I flipped the Korttajarvi data. We used the density data as the temperature proxy, as recommended to me by Antii Ojala (co-author of the original work). It's weakly inversely related to organic matter content. I should have used the inverse of density as the temperature proxy. I probably got confused by the fact that the 20th century shows very high density values and I inadvertently equated that directly with temperature.

This is new territory for me, but not acknowledging an error might come back to bite us. I suggest that we nip it in the bud and write a brief update showing the corrected composite (Nick's graph) and post it to RealClimate. Do you all agree?

There's other criticisms that have come up by McIntyre's group:

(1) we cherry-picked the tree-ring series in Eurasia. Apparently this is old ground, but do we need to address why we chose the Yamal record over the Polar Urals? Apparently, there's also a record from the Indigirka River region, which might not have been published and doesn't seem to be included in Keith's recent summary. If we overlooked any record that met our criteria, I suggest that we explain why. Keith: are you back? Can Ray or Mike provide some advise?

(2) The correction for Dye-3 was criticized because the approach/rationale had not been reviewed independently on its own. Bo: has this procedure now been published anywhere?

(3) We didn't publish any error analysis (e.g., leave-one-out), but I recall that we did do some of that prior to publication. Would it be worthwhile including this in our update? The threshold-exceedence difference (O&B-style) does include a boot-strapped estimate of errors. That might suffice, but is not the record we use for the temperature calibration.

(4) We selected records that showed 20th century warming. The only records that I know of that go back 1000 years that we left out were from the Gulf of Alaska that are known to be related strongly to precipitation, not temperature, and we stated this upfront.

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Do we want to clarify that it would be inappropriate to use a record of precip to reconstruct temperature? Or do we want to assume that precip should increase with temperature and add those records in and show that the primary signals remain?

(5) McIntyre wrote to me to request the annual data series that we used to calculate the 10-year mean values (10-year means were up on the NOAA site the same AM as the paper was published). The only "non-published" data are the annual series from the ice cores (Agassiz, Dye-3, NGRIP, and Renland). We stated this in the footnote, but it does stretch our assertion that all of the data are available publicly. Bo: How do you want to proceed? Should I forward the annual data to McIntyre?

Please let me -- better yet, the entire group -- know whether you think we should post a revision on RealScience, and whether we should include a reply to other criticism (1 through 5 above). I'm also thinking that I should write to Ojala and Tiljander directly to apologize for inadvertently reversing their data.

Other thoughts or advise?

Darrell

On Sep 4, 2009, at 5:24 PM, Nick McKay wrote:

The Korttajarvi record was oriented in the reconstruction in the way that McIntyre said. I took a look at the original reference - the temperature proxy we looked at is x-ray density, which the author interprets to be inversely related to temperature. We had higher values as warmer in the reconstruction, so it looks to me like we got it wrong, unless we decided to reinterpret the record which I don't remember. Darrell, does this sound right to you? This dataset is truncated at 1800, so it doesn't enter the calibration, nor does it affect the recent warming trend. The attached plot (same as before) shows the effect of re-orienting the record on the reconstruction. It doesn't change any of our major or minor interpretations of course.
Nick

On Thu, Sep 3, 2009 at 4:45 PM, Nick McKay <[1]nmckay@email.arizona.edu> wrote:

Hi all,
I haven't checked the original reference for it's interpretation, but I checked the code and we did use it in the orientation that he stated. He's also right that flipping doesn't affect any of the conclusions. Actually, flipping it makes it fit in better with the 1900-year trend.

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I've attached a plot of the original, and another with Korttajarvi flipped.
Nick

[cid:2D818DBD-2A02-494E-B050-C1C5BACE9984@domain.actds!tmp] Embedded Content:
Effect of
flipping Korttajarvi.jpg: 00000001,0da94ca9,00000000,00000000

References

- 1. mailto:nmckay@email.arizona.edu

1011. 1252164302.txt

#####

From: Jonathan Overpeck <jto@email.arizona.edu>
 To: Darrell Kaufman <Darrell.Kaufman@nau.edu>, Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, David Schneider <dschneid@ucar.edu>, "Bette L. Otto-Bliesner" <ottobli@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Miller Giff <gmiller@colorado.edu>, Bo Vinther <bo@gfy.ku.dk>, Keith Briffa <k.briffa@uea.ac.uk>
 Subject: Re: Arctic2k update?
 Date: Sat, 05 Sep 2009 11:25:02 -0700
 Cc: <mann@psu.edu>

D et al - Please write all emails as though they will be made public.
 I would not rush and I would not respond to any of them until the best strategy is developed - don't want to waste anyone's time, including yours or Mc's. Since the recon in Science has an error, I think you do need to publish a correction in Science. In that, you can very briefly not it didn't affect the calibration, nor the final result. I don't think you have a choice here. And I don't think RealClimate alone is the place for this, although RC could be good for the bigger list of issues. Don't do it on Mc;s blog. But, it would be good to hear from Ray and Mike, since they have the most experience in getting it right.
 Here are some other QUICK thoughts - don't count on me for the next week.
 Proposal hell and traveling.
 Make sure you have Keith's feedback before saying anything about the dendro aspects.
 Don't know about Dye3 issue
 Error analysis should be done and be the topic of another paper - it wasn't included in this paper, so it's something that should be done outside the peer-review process. There is lots of new research to be done, and someone should do it as time allows. Don't get pushed into something too rushed or preliminary, and your defense is that you wrote a paper that reviewed well and was published. The goal wasn't to do everything in this paper.
 #4 - your are absolutely right and that could be in a blog someplace, or just let them go ahead and do a stupid thing. If this was a climate field recon it would be different, no?
 #5 is tricky. Giving him the data would be good, but only if it is yours to give. You can't give him data that you got from others and are not allowed to share. But, it

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would be nice

if he could have access to all the data that we used - that's the way science is supposed

to work. See what Mike and Ray say...

Be careful, very careful. But now you know why I advocated redoing all the analyses a few

months ago - to make sure we got it all right. We knew we'd get this scrutiny.

This paper has had great impact so far, so that's something to remember - its good work.

Thanks, peck

On 9/5/09 8:44 AM, "Darrell Kaufman" <[1]Darrell.Kaufman@nau.edu> wrote:

All:

I received my first hate mail this AM, which helped me to realize that I shouldn't be

wasting time reading the blogs.

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Apparently,

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published and doesn't seem to be included in Keith's recent summary. If we overlooked

any record that met our criteria, I suggest that we explain why. Keith: are you back?

Can Ray or Mike provide some advise?

(2) The correction for Dye-3 was criticized because the approach/rationale had not been

reviewed independently on its own. Bo: has this procedure now been published anywhere?

(3) We didn't publish any error analysis (e.g., leave-one-out), but I recall that we

did do some of that prior to publication. Would it be worthwhile including this in our

update? The threshold-exceedence difference (O&B-style) does include a boot-strapped

estimate of errors. That might suffice, but is not the record we use for the temperature

calibration.

(4) We selected records that showed 20th century warming. The only records that I know

of that go back 1000 years that we left out were from the Gulf of Alaska that

mail.2009

are known

to be related strongly to precipitation, not temperature, and we stated this upfront. Do

we want to clarify that it would be inappropriate to use a record of precip to reconstruct temperature? Or do we want to assume that precip should increase with

temperature and add those records in and show that the primary signals remain? (5) McIntyre wrote to me to request the annual data series that we used to calculate the

10-year mean values (10-year means were up on the NOAA site the same AM as the paper was published). The only "non-published" data are the annual series from the ice cores

(Agassiz, Dye-3, NGRIP, and Renland). We stated this in the footnote, but it does stretch our assertion that all of the data are available publicly. Bo: How do you want

to proceed? Should I forward the annual data to McIntyre? Please let me -- better yet, the entire group -- know whether you think we should post a

revision on RealScience, and whether we should include a reply to other criticism (1 through 5 above). I'm also thinking that I should write to Ojala and Tiljander directly

to apologize for inadvertently reversing their data.

Other thoughts or advise?

Darrell

On Sep 4, 2009, at 5:24 PM, Nick McKay wrote:

The Korttajarvi record was oriented in the reconstruction in the way that McIntyre said.

I took a look at the original reference - the temperature proxy we looked at is x-ray density, which the author interprets to be inversely related to temperature. We had

higher values as warmer in the reconstruction, so it looks to me like we got it wrong, unless we decided to reinterpret the record which I don't remember. Darrell,

does this sound right to you?

This dataset is truncated at 1800, so it doesn't enter the calibration, nor does it affect the recent warming trend.

The attached plot (same as before) shows the effect of re-orienting the record on the reconstruction. It doesn't change any of our major or minor interpretations of course.

Nick

On Thu, Sep 3, 2009 at 4:45 PM, Nick McKay <[2]nmckay@email.arizona.edu> wrote:

Hi all,

I haven't checked the original reference for it's interpretation, but I checked the code and we did use it in the orientation that he stated. He's also right that

flipping doesn't affect any of the conclusions. Actually, flipping it makes it fit in better with the 1900-year trend.

I've attached a plot of the original, and another with Korttajarvi flipped.

Nick

mail.2009

[cid:3334994702_4110695]

Jonathan T. Overpeck
 Co-Director, Institute of the Environment
 Professor, Department of Geosciences
 Professor, Department of Atmospheric Sciences
 Mail and Fedex Address:
 Institute of the Environment
 715 N. Park Ave. 2nd Floor
 University of Arizona
 Tucson, AZ 85721
 direct tel: +1 520 622-9065
 Email: [3]jto@u.arizona.edu
 PA Lou Regalado +1 520 792-8712
 [4]regalado@email.arizona.edu
 Embedded Content: image7.jpg: 00000001,780e1428,00000000,00000000

References

1. file:///localhost/tmp/Darrell.Kaufman@nau.edu
2. file:///localhost/tmp/nmckay@email.arizona.edu
3. file:///localhost/tmp/jto@u.arizona.edu
4. file:///localhost/tmp/regalado@email.arizona.edu

1012. 1252233095.txt

 #####

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
 To: Bo Vinther <bo@gfy.ku.dk>
 Subject: Re: Arctic2k update?
 Date: Sun, 6 Sep 2009 06:31:35 -0700
 Cc: Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, David Schneider <dschneid@ucar.edu>, Jonathan Overpeck <jto@email.arizona.edu>, "Bette L. Otto-Bliesner" <ottobli@ucar.edu>, "Raymond Bradley" <rbradley@geo.umass.edu>, Miller Giff <gmiller@colorado.edu>, "Keith Briffa" <k.briffa@uea.ac.uk>, "mann@psu.edu" <mann@psu.edu>

Bo and others:

Regarding the annual data: You're correct that we only use 10-year means throughout our calculations (Fig 2 shows annual values, but are not used in any calculation/conclusion). In his e-mail to me, McIntyre requested the annual data that we say are not publicly available as a footnote to Table S1.

Unless anyone has another suggestion, I will reply and send him the 10-year data (which is already posted at NOAA-Paleoclimate) and explain that they were the basis for all of the calculations. He might want the annual data that the mean values were based on. I suppose we'll cross that bridge when we get to it.

Darrell

On Sep 6, 2009, at 5:42 AM, Bo Vinther wrote:

Hi Darrell

mail.2009

Sorry to hear that you are getting trouble for doing such a nice paper....I by the way agree completely with Peck that we should not be rushed and that a correction probably should go into science.

Anyway, let me answer the two questions you had for me:

2) Correcting ice core data for upstream effects should not be controversial (while not correcting in areas of flow should be highly controversial indeed!).

Upstream correction of delta-180 was in fact already done 30 years ago for the Milcent ice

core - a quick quote from Hammer et al. 1978, page 14:

"The delta values are corrected for decreasing deltas up-slope at the site of formation of the individual layers"

Hammer, C. U., H. B. Clausen, W. Dansgaard, N. Gundestrup, S. J. Johnsen and N. Reeh, Dating of Greenland ice cores by flow models, isotopes, volcanic debris, and continental dust, J. Glaciol., 20, 326, 1978.

So upstream correction of delta data from ice cores using ice flow models has in fact been performed since the year I was born.....

5) I will suggest that we release the 1860-2000 section of the annually resolved ice core

data, as these are the data that go into figure 2 in the paper.

Such a limited release I can permit immediately.

Releasing everything is something different and I can't see the need - as far as I remember

we are not presenting/using the 1-1859 part of the series in annual resolution anywhere in

the paper - or am I wrong?

Cheers.....BO

Darrell Kaufman wrote:

All:

I received my first hate mail this AM, which helped me to realize that I shouldn't be wasting time reading the blogs.

Regarding the "upside down man", as Nick's plot shows, when flipped, the Korttajarvi series has little impact on the overall reconstructions. Also, the series was not included in the

calibration. Nonetheless, it's unfortunate that I flipped the Korttajarvi data. We used the

density data as the temperature proxy, as recommended to me by Antii Ojala (co-author of

the original work). It's weakly inversely related to organic matter content. I should have

used the inverse of density as the temperature proxy. I probably got confused by the fact

that the 20th century shows very high density values and I inadvertently equated that

directly with temperature.

This is new territory for me, but not acknowledging an error might come back to bite us. I

suggest that we nip it in the bud and write a brief update showing the corrected composite

(Nick's graph) and post it to RealClimate. Do you all agree?

There's other criticisms that have come up by McIntyre's group:

mail.2009

(1) We cherry-picked the tree-ring series in Eurasia. Apparently this is old ground, but do we need to address why we chose the Yamal record over the Polar Urals? Apparently, there's also a record from the Indigirka River region, which might not have been published and doesn't seem to be included in Keith's recent summary. If we overlooked any record that met our criteria, I suggest that we explain why. Keith: are you back? Can Ray or Mike provide some advise?

(2) The correction for Dye-3 was criticized because the approach/rationale had not been reviewed independently on its own. Bo: has this procedure now been published anywhere?

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mail.2009

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I've attached a plot of the original, and another with Korttajarvi flipped.
Nick

References

- 1. <mailto:nmckay@email.arizona.edu>

1013. 1252672219.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: claudia tebaldi <ctebaldi@climatecentral.org>
Subject: Re: Important: Input for Funding
Date: Fri, 11 Sep 2009 08:30:19 -0700
Reply-to: santer1@llnl.gov
Cc: Myles Allen <allen@atm.ox.ac.uk>, Knutti Reto <reto.knutti@env.ethz.ch>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, Gabi Hegerl <gabi.hegerl@ed.ac.uk>, "Zwiers, Francis [Ontario]" <francis.zwiers@ec.gc.ca>, Tim Barnett <tbarnett-ul@ucsd.edu>, Hans von Storch <hvonstorch@web.de>, Phil Jones <p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, Toru Nozawa <nozawa@nies.go.jp>, Daithi Stone <stoned@atm.ox.ac.uk>, Richard Smith <rls@email.unc.edu>, Nathan Gillett <n.gillett@uea.ac.uk>, Michael Wehner <MFWehner@lbl.gov>, Doug Nychka <nychka@ucar.edu>, Xuebin Zhang <Xuebin.Zhang@ec.gc.ca>, Tom Knutson <Tom.Knutson@noaa.gov>, Tim Delsole <delsole@cola.iges.org>, "Jones, Gareth S" <gareth.s.jones@metoffice.gov.uk>, Stephen Leroy <leroy@huarp.harvard.edu>, seung-ki.min@ec.gc.ca, dpierce@ucsd.edu

<x-flowed>
Dear Claudia,

mail.2009

The 13th session of the Working Group on Climate Modelling (WGCM) is going to be taking place in San Francisco at the end of this month. PCMDI is hosting this event. I just received an invitation to talk about IDAG at this meeting. I'd be very happy to do this, but would appreciate some guidance from you and others regarding what aspects of IDAG you'd like me to discuss.

with best regards,

Ben

claudia tebaldi wrote:

> Hi again

>

> I'm attaching the current version after some remessaging, especially of
> the task list.

> There is a need for a reference that I would like to get from David
> Karoly, and a general request for input having to do with the synthesis
> products that originally were described as instrumental to AR5 but Gabi
> thinks they would not be prepared in time for that. So I'm wondering if
> people have specific ideas for the next round of review papers that we
> could describe at the end of Section 3 of the document.

>

> MOST IMPORTANTLY:

> I need some very specific input from *all of you* (only exception,
> Francis's group).

>

> After asking Anjuli I can confirm that government employees cannot
> receive funding besides travel reimbursement. So for those of you that
> are GOVERNMENT EMPLOYEES, the only thing that remains to do is to go
> through the document once again, make sure your work (past and future)
> is not misrepresented, and then send me a note with an "OK" or your new
> comments, specifying that you are a government employee (please don't
> let me guess it).

>

> For those of you that are ACADEMICS WITH 12 MONTHS SALARY all that we
> can budget is a small amount of consulting fees, up to 2 weeks' worth.
> If you belong to this category please respond saying that you are or you
> are not interested. If you are, then include in the document at the end
> in the place already arranged for it a statement of work referring to
> specific tasks as they stand in Section 3 of the narrative, and a
> bio-sketch (see end of this email for specific instructions).

>

> For THOSE OF YOU THAT CAN GET FULL SUPPORT, please say if you want it or
> not, and if you do, then do as I requested above: include in the
> document at the end in the place already arranged for it a statement of
> work referring to specific tasks as they stand in Section 3 of the
> narrative, and a bio-sketch (see end of this email for specific
> instructions).

>

> Please shoot me an email and say something, esp. those of you abroad for
> whom I'm not familiar with affiliations/months of salary. Needless to
> say, if you don't send the bio and don't put yourself down in the
> Statements of Work session you won't be budgeted but for travel
> reimbursement.

>

> Can I ask you to do this at your earliest convenience, but at the latest
> before mid-week next week?

>

> Thanks

>

> c

>

mail.2009

> PS I received only 2 figures in response to my earlier request. If you
> take the time to read the narrative and have a good figure for it, send
> it along!

>
> #####
> Biographical Sketches: Instructions
> #####

>
> The biographical sketch is limited to a maximum of two pages. It must
> contain name and position title, organization, degree, years and field
> of study for each academic degree; a listing of research and
> professional positions, awards, and honors; and references to all
> publications for the past three years along with any earlier
> publications pertinent to this application. If this list causes the
> biographical sketch to exceed two pages, select the most pertinent
> publications to stay within the page limit.

>
> Current and Pending Support

>
> The PI/PD(s) are requested to list all their current and pending
> non-Federal and Federal support.

>
> Identification of Potential Conflicts of Interest/Bias in Selection of
> Reviewers

>
> Provide the following information:

>
> Collaborators and Co-editors: List in alphabetical order all
> persons, including their current organizational affiliation, who are, or
> who have been, collaborators or co-authors with you on a research
> project, book or book article, report, abstract, or paper during the 48
> months preceding the submission of this application. Also, list any
> individuals who are currently, or have been, co-editors with you on a
> special issue of a journal, compendium, or conference proceedings during
> the 24 months preceding the submission of this application. If there are
> no collaborators or co-editors to report, state 'none'.

>
> Graduate and Postdoctoral Advisors and Advisees: List the names and
> current organizational affiliations of your graduate advisor(s) and
> principal postdoctoral sponsor(s) during the last 5 years. Also, list
> the names and current organizational affiliations of your graduate
> students and postdoctoral associates during the past 5 years.

> --
> Claudia Tebaldi
> Research Scientist, Climate Central
> http://*www.*climatecentral.org
> & Adjunct Professor
> Department of Statistics - UBC Vancouver
> office 604 822 3595 (Canadian area code)
> cell 303 775 5365 (US area code)

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
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1014. 1253561029.txt

#####

From: Malcolm Hughes <mhughes@ltrr.arizona.edu>
To: Tom Melvin <t.m.melvin@uea.ac.uk>
Subject: Re: recent paper
Date: Mon, 21 Sep 2009 15:23:49 -0700
Cc: Keith <K.Briffa@uea.ac.uk>

<x-flowed>

Hi Tom - please find the Esper article in question attached. The so-called Indigirka River data set is not yet available because it has not been published. I am currently working on that with Russian colleagues, and was indeed in Switzerland the week before last to work with one of them on specifically this. All being well, there will be an accepted manuscript before next summer, and at that point I will make the data freely available. Once we get to that point, I'll let you know, of course. Cheers, Malcolm

Tom Melvin wrote:

> Malcolm,

>

> 1. There was a recent Esper Siberian paper I recall reading but I
> cannot find it at the moment (my comment was on the Divergence
> pitfalls paper). I will find the paper and see if there is an
> explanation.

>

> 2. For trend distortion to produce a "divergence" effect there needs
> to be a distinct increase (or decrease) over the last few decades of
> growth, e.g. at TTHH and curve fitting methods should be used. In the
> attached figure the Scandinavian site groups (red) have an increase at
> 1920 and are likely to show divergence using curve fitting methods.
> Some of the eastern most chronologies might also show divergence if
> 250+ year old trees were used.

>

> 3. RCS should not produce "divergence" over decades as an artifact if
> sub-fossil trees are used. RCS on modern chronologies has all sorts
> of bias. We have lots of ideas to test in the divergence project and
> lots of data to test them on.

>

> 4. Keith has been complained at by Climate Audit for cherry picking
> and not using your long Indigirka River data set. Not used because we
> did not have the data. Please, could we have the data? We will make
> proper acknowledgement/coauthorship if we use the data.

>

> Tom

>

>

>

> At 16:35 21/09/2009, you wrote:

>> Tom, I don't disagree with your take on the lack of originality of
>> much of what is in the paper. The question is: why is there apparently
>> divergence in ring width in some of this region in Briffa et al 98 but
>> not in this paper? Isn't espers failure to see divergence
>> counterintuitive when using RCS in his way?

mail.2009

>> Cheers, Malcolm

>>

>>

>> On Sep 21, 2009, at 2:11 AM, Tom Melvin <t.m.melvin@uea.ac.uk> wrote:

>>

>>> Malcolm,

>>>

>>> The Esper "Divergence pitfalls .." paper does not appear to add anything of significance. None of the figures show any form of the divergence discussed in papers e.g. a recent (last few decades) change in the slope of tree-ring growth indices compared to climate. Differences in overall slope, generally weak relationships, differences in variance, and the effects of using selected calibration periods are all problems to be addressed in reconstructions but are not divergence.

>>>

>>> I cannot foresee needing to reference this paper in discussions of divergence as all the suggestions have more detailed, earlier references.

>>>

>>> Tom

>>>

>>>

>>> At 22:33 18/09/2009, you wrote:

>>>> Hi Tom - I had a good talk with Keith on the phone the other day, mainly to wish him well. He did suggest I ask you for your take on the recent Esper et al paper on divergence (or rather the lack of it) in Siberia. Looks like the problem disappears. What do you think? Cheers, Malcolm

>>>

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

>>>

>>> Phone: +44-1603-593161

>>> Fax: +44-1603-507784

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1015. 1253631628.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: help
Date: Tue, 22 Sep 2009 11:00:28 -0400
Cc: Phil Jones <p.jones@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>

Hey Tom, thanks for checking w/ me on this. Re, Moberg: Yes, in fact we (me, Phil, Tim, Keith, Caspar, etc.) submitted a comment to Nature about the problem w/ the

mail.2009

variance

scaling used by Moberg. It can easily be shown to inflate the low- frequency variance in synthetic experiments. I've attached both the original comment (which they judged to be too technical to merit publication) and also a J. Climate paper where we discussed the same result (see Figure 5 and associated discussion). Re, von Storch et al. Yes, the paper you have in mind is Osborn et al Climate Dynamics '06. I only seem to have the preprint though

(attached), please let me know if I can be of any further help w/ an of this, mike p.s. you

can delete the U.Va email address--haven't been there for 4 years! On Sep 22, 2009, at

10:31 AM, Tom Wigley wrote: > Dear all, > > (Apologies Mike for email address confusion --

one of them will > get you I hope.) > > I need some help to finish a report I've had to write for EPRI -- > which is due in a few days. Hence the questions below ... > >

(1) The Moberg paper (2005 Nature) is used by the skeptics as evidence > that most of recent warming could still be natural. Has anyone > published a critique/criticism of this? It seems to me take this > work is fundamentally flawed. First, variance scaling is crap > statistics as it produces results with far less explained variance > than normal least-squares regression. Second, the paper seems to > have no independent validation.

Third, what happens if one just takes > his low-frequency (numbered in his Fig. 1) points and calculates > the area average? Surely this will have much greater variability > than the full global mean? (If no-one has done this please let me > know -- I can do it very easily myself.) But perhaps his scaling > method circumvents this "problem"? > >

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what is wrong with the von > Storch ECHO simulation? I think Tim Osborn did something on

this. > > Many thanks for your help, > Tom. > -- Michael E. Mann 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 website:

<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html Hey Tom,

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mail.2009

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mike

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Many thanks for your help,

Tom.

--

Michael E. Mann

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: [1]mann@psu.edu

University Park, PA 16802-5013

website: [2]<http://www.meteo.psu.edu/~mann/Mann/index.htm>

"Dire Predictions" book site:

[3]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Attachment Converted: "c:\eudora\attach\MRWA-JClimate05.pdf" Attachment Converted:

"c:\eudora\attach\62811_0_merged_1109271201.pdf" Attachment Converted:

"c:\eudora\attach\osbornetalClimDynInPress06.pdf"

References

visible links

mail.2009

1. <mailto:mann@psu.edu>
2. <http://www.meteo.psu.edu/~mann/Mann/index.html>
3. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

1016. 1254108338.txt

#####

From: Tom wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: 1940s
Date: Sun, 27 Sep 2009 23:25:38 -0600
Cc: Ben Santer <santer1@llnl.gov>

<x-flowed>
Phil,

Here are some speculations on correcting SSTs to partly explain the 1940s warming blip.

If you look at the attached plot you will see that the land also shows the 1940s blip (as I'm sure you know).

So, if we could reduce the ocean blip by, say, 0.15 degC, then this would be significant for the global mean -- but we'd still have to explain the land blip.

I've chosen 0.15 here deliberately. This still leaves an ocean blip, and i think one needs to have some form of ocean blip to explain the land blip (via either some common forcing, or ocean forcing land, or vice versa, or all of these). When you look at other blips, the land blips are 1.5 to 2 times (roughly) the ocean blips -- higher sensitivity plus thermal inertia effects. My 0.15 adjustment leaves things consistent with this, so you can see where I am coming from.

Removing ENSO does not affect this.

It would be good to remove at least part of the 1940s blip, but we are still left with "why the blip".

Let me go further. If you look at NH vs SH and the aerosol effect (qualitatively or with MAGICC) then with a reduced ocean blip we get continuous warming in the SH, and a cooling in the NH -- just as one would expect with mainly NH aerosols.

The other interesting thing is (as Foukal et al. note -- from MAGICC) that the 1910-40 warming cannot be solar. The Sun can get at most 10% of this with Wang et al solar, less with Foukal solar. So this may well be NADW, as Sarah and I noted in 1987 (and also Schlesinger later). A reduced SST blip in the 1940s makes the 1910-40 warming larger than the SH (which it currently is not) -- but not really enough.

So ... why was the SH so cold around 1910? Another SST problem? (SH/NH data also attached.)

This stuff is in a report I am writing for EPRI, so I'd appreciate any comments you (and Ben) might have.

Tom.

</x-flowed>

Attachment Converted: "c:\eudora\attach\TTHEMIS.xls"

Attachment Converted: "c:\eudora\attach\TTLVSO.XLS"

1017. 1254147614.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: 1940s
Date: Mon Sep 28 10:20:14 2009
Cc: Ben Santer <santer1@llnl.gov>

Tom,
A few thoughts

[1]http://ams.allenpress.com/archive/1520-0442/preprint/2009/pdf/10.1175_2009JCLI3089.1.pdf

f
This is a link to the longer Thompson et al paper. It isn't yet out in final form - Nov09 maybe?

[2]<http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle>

-matching-and-removal-of-natural-variables/
is a link to wattsupwiththat - not looked through this apart from a quick scan.

Dave
Thompson just emailed me this over the weekend and said someone had been busy! They seemed to have not fully understood what was done.

Have looked at the plots. I'm told that the HadSST3 paper is fairly near to being submitted, but I've still yet to see a copy. More SST data have been added for the WW2 and WW1 periods, but according to John Kennedy they have not made much difference to these periods.

Here's the two ppts I think I showed in Boulder in June. These were from April 09, so don't know what these would look like now. SH is on the left and adjustment there seems larger, for some reason - probably just British ships there?

Maybe I'm misinterpreting what you're saying, but the adjustments won't reduce the 1940s blip but enhance it. It won't change the 1940-44 period, just raise the 10 years after Aug 45.

I expect MOHC are looking at the NH minus SH series re the aerosols. My view is that a cooler temps later in the 1950s and 1960s it is easier to explain.

Land warming in the 1940s and late 1930s is mainly high latitude in NH. One other thing - MOHC are also revising the 1961-90 normals. This will likely have more effect in the SH.

With the SH around 1910s there is the issue of exposure problems in Australia -

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see

Neville's paper.

This shouldn't be an issue in NZ - except maybe before 1880, but could be in southern South America. New work in Spain suggest screens got renewed about 1900, so maybe this happened in Chile and Argentina, but Mossmann was head of the Argentine NMS so he may have got them to use Stevenson screens early.

Neville has never been successful getting any OZ funding to sort out pre-1910 temps everywhere except Qld.

Here's a paper in CC on European exposure problems. There is also one on Spanish series.

Cheers

Phil

At 06:25 28/09/2009, Tom Wigley wrote:

Phil,

Here are some speculations on correcting SSTs to partly explain the 1940s warming blip.

If you look at the attached plot you will see that the land also shows the 1940s blip (as I'm sure you know). So, if we could reduce the ocean blip by, say, 0.15 degC, then this would be significant for the global mean -- but we'd still have to explain the land blip.

I've chosen 0.15 here deliberately. This still leaves an ocean blip, and I think one needs to have some form of ocean blip to explain the land blip (via either some common forcing, or ocean forcing land, or vice versa, or all of these). When you look at other blips, the land blips are 1.5 to 2 times (roughly) the ocean blips -- higher sensitivity plus thermal inertia effects. My 0.15 adjustment leaves things consistent with this, so you can see where I am coming from. Removing ENSO does not affect this.

It would be good to remove at least part of the 1940s blip, but we are still left with "why the blip".

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So ... why was the SH so cold around 1910? Another SST problem? (SH/NH data also attached.)

This stuff is in a report I am writing for EPRI, so I'd appreciate any comments you (and Ben) might have.

Tom.

Prof. Phil Jones

Climatic Research Unit

School of Environmental Sciences

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Norwich

NR4 7TJ

UK

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Email p.jones@uea.ac.uk

References

- 1.
http://ams.allenpress.com/archive/1520-0442/preprint/2009/pdf/10.1175_2009JCLI3089.1.pdf
- 2.
<http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle-matching-and-removal-of-natural-variables/>

1018. 1254163301.txt

#####

From: Susan Parham <sp@cagconsult.co.uk>
 To: Peter Kenway <peter.kenway@npi.org.uk>, "Adger Neil Prof (ENV)" <N.Adger@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Mick Denness <m.denness@bctv.org.uk>, Andrew Gouldson <a.gouldson@leeds.ac.uk>, c.l.busfield@see.leeds.ac.uk, Tom MacInnes <tom.macinnes@npi.org.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Niamh Carey <ncarey@wwf.org.uk>, amanda@cdx.org.uk
 Subject: I am afraid we didn't get the JRF climate change research
 Date: Mon, 28 Sep 2009 14:41:41 +0100
 Cc: Denny Gray <dg@cagconsult.co.uk>, Emma Cranidge <ec@cagconsult.co.uk>, Tim Maiden <tm@cagconsult.co.uk>, Mary Anderson <ma@cagconsult.co.uk>, Helen Chalmers <hc@cagconsult.co.uk>, Niall Machin <nm@cagconsult.co.uk>, Gerard Couper <gc@cagconsult.co.uk>

Dear All

Im afraid its bad news on the JRF bid. We were not selected.

The gist of the letter I have now received says the problem was that it went over the £100,000 mark for a single bid and was therefore out of contention on those grounds - they accepted I'd rung to check about this and so said they reviewed the proposal given 'the potential confusion' about this.

They also said "it was unfortunate that the proposal did not more clearly demonstrate how it could build on the findings from the existing review of social impacts CAG has conducted". (No mention of the 3,000 word word limit for the whole proposal).

I just want to say thanks again for all you great work on this. I do think JRF wrote a confusing and difficult brief and we did a good job despite their strange requirements.

I hope this won't put people bidding again should other suitable work come up.

all the best

Susan

ps I will be scanning their letter and will send round tomorrow.
 Dr Susan Parham
 Director - CAG Consultants
 Tel: 020 7704 0018 Mob: 07967 816 295
 [1]sp@cagconsult.co.uk
 www.cagconsult.co.uk
 Office: 30 Aberdeen Road,
 London, N5 2UH

HQ: Gordon House, 6 Lissenden Gardens, London, NW5 1LX

References

- 1. mailto:sp@cagconsult.co.uk

1019. 1254163518.txt

#####

From: Kevin Trenberth <trenbert@ucar.edu>
 To: Grant Foster <tamino_9@hotmail.com>
 Subject: Re: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter
 Date: Mon, 28 Sep 2009 14:45:18 -0600
 Cc: Mike Mann <mann@meteo.psu.edu>, p.jones@uea.ac.uk, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, j.renwick@niwa.co.nz

Hi all

About time. Incidentally i gave a copy to Mike McPhaden and discussed it with him last week when we were together at the OceanObs'09 conference. Mike is President of AGU.

Basically this is an acceptance with a couple of suggestions for extras, and some suggestions for toning down the rhetoric. I had already tried that a bit. My reaction is

that the main thing is to expedite this. That means no extras unless it really makes

sense. And removal of a few unnecessary words like "absolutely".

In the abstract, we have a number of such adjectives that could be removed: I agree with

Rev 3 in this.

"greatly overstates" could be just "overstates" as it is reinforced better later.

"severely overestimates" could be just "overestimates"

"faulty analysis" maybe "flawed analysis"?

"extremely high" maybe "very high" or "unduly high"

I would leave last sentence alone though as the main comment.

A few more comments embedded below.

Grant Foster wrote:

> From: [1]jgr-atmospheres@agu.org
 > Date: Mon, 28 Sep 2009 15:54:05 +0000
 > To: [2]tamino_9@hotmail.com
 > Subject: 2009JD012960 (Editor - Steve Ghan):Decision Letter
 > CC: [3]twistor9@gmail.com

>
 > Manuscript Number: 2009JD012960
 > Manuscript Title: Comment on "Influence of the Southern Oscillation on tropospheric temperature" by J. D. McLean, C. R. de Freitas, and R. M. Carter

tropospheric

>
>

 >
 > Reviewer Comments
 >
 > Reviewer #1 (Comments):

>
 > This paper does an excellent job of showing the errors in the analytical methods used by McLean et al. and why their conclusions

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> about the influence of ENSO on global air temperature is incorrect.
> I have only a couple of suggestions to help clarify their analysis of the methods.
First, a little more explanation of the comment about the time derivative reduced to an additive constant would help. Second, in the analysis of the artificial time series I think it would be interesting to show the results of both steps of filtering (running mean and derivative) as separate time series. This would help the reader understand why the filtering creates false correlations. The only other suggestion is to find a better adjective than "faulty" in the abstract to characterize the analysis.
>

It is not so easy to see the result from the derivative owing to the phase shift. The spectrum actually does a better job. I would address this comment in this way and change "faulty".

>
> Reviewer #2 (Comments):
>
> I think this comment on McLean et al can be published more or less as is.
>
> I have two comments
>
> First, in the abstract (page 3, line 15), I'm not sure that "inflating" is quite the right verb - the paper itself does not make the point that the filter constructed by McLean et al inflates power in the 2-6 year window. Perhaps "isolating" would be a better verb.

Yes it should not be in abstract if not in text. Need to point out that the response function in Fig. 1 is greater than unity and does "inflate". So adjust the text.

>
> Secondly, I think the points that are being made with Figures 4 and 5 could be strengthened by adding to the right of each plot of a pair of time series, a scatter plot of the pairs of values available at each time. Such a scatter plot would help to clearly illustrate the absence (upper panels) or presence (lower panels) of correlation between red and black values.

I don't think this helps. There is nothing to be gained from a scatter plot that a correlation or regression value does not summarize.

>
>
> Reviewer #3 (Comments):
>
> Accept pending major changes (mainly in style not scientific comment)
>
> The real mystery here, of course, is how the McLean et al. paper ever made it

into

JGR. How that happened, I have no idea. I can't see it ever getting published through J

Climate. The analyses in McLean et al. are among the worst I have seen in the climate

literature. The paper is also a poorly guised attack on the integrity of the climate

community, and I guess that is why Foster et al. have taken the energy to contradict its

findings.

>

> So the current paper (Foster et al.) should certainly be accepted. Someone needs to address the science in the McLean et al paper in the peer-reviewed literature.

But the current paper could be - and should be - done better. That's why I am suggesting major

changes before the paper is accepted. All of my suggestions have to do more with the

tone and framing of the current paper, rather than its content.

>

> 1. As noted above, I agree McLean et al is problematic. But as it is written, the

current paper almost stoops to the level of "blog diatribe". The current paper does not

read like a peer-reviewed journal article. The tone is sometimes dramatic and sometimes

accusatory. It is inconsistent with the language one normally encounters in the objectively-based, peer-reviewed literature. For examples....

> - In the abstract: Do you really need all of these adjectives?... 'greatly overstates';

'severely overestimates'; 'faulty analysis'; 'extremely high'.

Agree, see above

> - In the introduction... 'Unfortunately, their conclusions are seriously in error...'

strikes me as overly subjective. Better to say: 'We will demonstrate that their conclusions are strongly dependent on' or something like that...

Don't go that far. Could drop "seriously" but they are "in error"

> - Page X-6: 'tell us absolutely nothing'. Surely it's enough to state 'tell us nothing'.

agree

> - Page X-9: 'it is misleading...' That's a strong word. It may be true. But I think we should rise above such accusations.

misleading is OK. I did a search (not sure I have latest) and found "grossly misleading" and the "grossly" could be removed.

>

> Anyway, I'm sure the lead author gets my point. I think the current paper will have a much greater impact (and can claim the high road) if it is rewritten in a more objective manner.

>

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> 2. Similarly, instead of framing the paper as "Taking down McLean et al.", why not focus more on interesting aspects of the science, such as the frequency dependence between ENSO and global-mean temperature (perhaps cross-correlation analysis would be useful); the importance of not extrapolating results from one timescale to another timescale; or the lack of trends in ENSO. That way, the current paper contributes to the peer-reviewed literature while also doing a service by highlighting the problems with McLean et al.

I think I tried to emphasize that this should be a teaching moment. Even more important given the time lapse.

>
> 3. In general, the current paper is sloppy and needs tightening. I don't think the lead author needs 10 pages of text to make the main points.
>
>

So over to you to generate the next draft.
Thanks
Kevin

Kevin E. Trenberth
Climate Analysis Section,
NCAR
P. O. Box 3000,
Boulder, CO 80307

e-mail: [4]trenbert@ucar.edu
[5]www.cgd.ucar.edu/cas/trenbert.html
(303) 497 1318
(303) 497 1333 (fax)

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

References

1. <mailto:jgr-atmospheres@agu.org>
2. mailto:tamino_9@hotmail.com
3. <mailto:twistor9@gmail.com>
4. <mailto:trenbert@ucar.edu>
5. <http://www.cgd.ucar.edu/cas/trenbert.html>

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#####

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: latest
Date: 28 Sep 2009 17:59:04 -0400

Hi Tim, I know Keith is out of commission for a while (give him my regards when you see him), but someone needs to at least give some context to the latest McIntyre meme.

<http://planetgore.nationalreview.com/post/?q=Y2Q5ZGEXZTC3ZTlMMTA5OTdhOGRjNzd1NmU4N2M4ZTg=>

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None of us at RC have any real idea what was done or why and so we are singularly unable to sensibly counter the flood of nonsense. Of course, most of the reaction is hugely overblown and mixed up but it would be helpful to have some kind of counterpoint to the main thrust. If you can point to someone else that could be helpful, please do!

Thanks

Gavin

1021. 1254179301.txt

#####

From: Grant Foster <tamino_9@hotmail.com>
To: <trenbert@ucar.edu>, Mike Mann <mann@meteo.psu.edu>, <p.jones@uea.ac.uk>, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, <b.mullan@niwa.co.nz>, Gavin Schmidt <gschmidt@giss.nasa.gov>, <j.renwick@niwa.co.nz>
Subject: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter
Date: Mon, 28 Sep 2009 19:08:21 +0000

> From: jgr-atmospheres@agu.org
> Date: Mon, 28 Sep 2009 15:54:05 +0000
> To: tamino_9@hotmail.com
> Subject: 2009JD012960 (Editor - Steve Ghan):Decision Letter
> CC: twistor9@gmail.com

> Manuscript Number: 2009JD012960
> Manuscript Title: Comment on "Influence of the Southern Oscillation on tropospheric temperature" by J. D. McLean, C. R. de Freitas, and R. M. Carter

>
>
> Dear Dr. Foster:

> 3 reviews of your above-referenced manuscript are attached below. Reviewer 3 is concerned with the tone on the writing; while I appreciate the value of "taking the high road", I do not object to emphatic statements that conclusions are incorrect. Strong language is needed

sometimes when errors must be corrected. Please carefully consider the Reviewers' recommendations for revisions, make the necessary changes, and respond to me with

a point-by-point response of how you have addressed each concern. In your cover letter,

please include a statement confirming that all authors listed on the manuscript concur with submission in its revised form.

>
>

> The due date for your revised paper is October 28, 2009. If you will be unable to submit a revised manuscript by this time, please notify my office and arrange for an extension (maximum two weeks). If we do not hear from you by the revision due date, your manuscript will be considered as withdrawn.

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>
> When you are ready to submit your revision, please use the link below.
>
> *The link below will begin the resubmission of your manuscript, please Do Not
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> the link until you are ready to upload your revised files. Any partial submission
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> for 3 days without files will be deleted.
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> Please see the AGU web site for more information about preparing text and art
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> (<http://www.agu.org/pubs/inf4aus.shtml>). If you have any questions, please
contact the
> editor's assistant.

>
> Sincerely,

>
> Steve Ghan
> Editor, Journal of Geophysical Research - Atmospheres

>
> -----Important JGR-Atmospheres Information-----

>
> Submission, Review and Publication Stages Chart
> Text Preparation and Formatting
> Manuscript Preparation
> Acceptable Electronic File Formats
> Editorial Style Guide for Authors
> Auxiliary Materials (Electronic Supplements)
>
> Artwork Preparation
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 on the internet at: <http://www.adobe.com/prodindex/acrobat/readstep.html>

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The analyses in McLean et al. are among the worst I have seen in the climate literature.
The paper is also a poorly guised attack on the integrity of the climate community, and I guess that is why Foster et al. have taken the energy to contradict its findings.
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a1.
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Hotmail® has ever-growing storage! Dont worry about storage limits. [1]Check it out.

References

1.
http://windowslive.com/Tutorial/Hotmail/Storage?ocid=TXT_TAGLM_WL_HM_Tutorial_Storage_062009

1022. 1254230232.txt
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From: Phil Jones <p.jones@uea.ac.uk>
 To: Tim Osborn <t.osborn@uea.ac.uk>, Michael Mann <mann@meteo.psu.edu>,Gavin Schmidt <gschmidt@giss.nasa.gov>
 Subject: Re: attacks against Keith
 Date: Tue Sep 29 09:17:12 2009

Mike, Gavin,
 AS Tim has said Keith is making a good recovery and hopes to be back in soon, gradually during October and hopefully full time from November.
 I talked to him by phone yesterday and sent him and Tom Melvin the threads on CA. As you're fully aware, trying to figure out what McIntyre has done is going to be difficult.
 It would be so much easier if they followed normal procedure and wrote up a comment and submitted it to a journal. I looked through the threads yesterday trying to make sense of what he's done. My suspicion is that he's brought in other tree ring series from more distant sites, some of which may not even be larch. There are two chronologies that have been used - one called the Polar Urals and one called Yamal. PU is a Schweingruber site with density as well as ring width. The PU reconstruction is therefore not a chronology, but a regression based reconstruction from both MXD and TRW. Yamal is just a ring width series (with lots of sub-fossil material, so much older) from an area some distance (at

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least 500km) north of PU. It was developed by Hantemirov and Shiyatov and was poorly standardized - corridor method. I also don't think McIntyre understands the RCS method even though he claims to have a program. The ends and the age structure of the samples are crucial in all this, but I think he just throws series in.

I totally agree that these attacks (for want of a better word) are getting worse.

Comments on the thread are snide in the extreme, with many saying they see no need to submit the results to a journal. They have proved Keith has manipulated the data, so job done.

Hadn't thought of Senate debates. I'd put this down to the build up to Copenhagen, which is sort of the same.

[1]<http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle-matching-and-removal-of-natural-variables/>

is a complete reworking of Dave Thompson's paper which is in press in J. Climate (online). Looked at this, but they have made some wrong assumptions, but someone has put a lot of work into it.

[2]<http://wattsupwiththat.com/2009/09/24/ooops-dutch-meteorological-institute-caught-in-wea-ther-station-siting-failure-moved-station-and-told-nobody/>

This one is a complete red herring - nothing wrong with De Bilt measurements. This is what

it is about according to someone at KNMI. The issue you refer to is causing a lot of noise in the Netherlands (even MP's asking questions to the minister). It seems this is not at all about the observational series (nothing strange is going on), but more related to the "Law on KNMI" and the division of

tasks between commercial providers and KNMI to be discussed by parliament soon.

Cheers

Phil

At 08:46 29/09/2009, Tim Osborn wrote:

Hi Mike and Gavin,
thanks for your emails re McIntyre, Yamal and Keith.
I'll pass on your best wishes for his recovery when I next speak to Keith.
He's been off almost 4 months now and won't be back for at least another month (barring a couple of lectures that he's keen to do in October as part of a gradual return).

Hopefully he'll be properly back in November.

Regarding Yamal, I'm afraid I know very little about the whole thing -- other than that I am 100% confident that "The tree ring data was hand-picked to get the desired result"

is complete crap. Having one's integrity questioned like this must make your blood boil (as I'm sure you know, with both of you having been the target of numerous such attacks). Though it would be nice to shield Keith from this during his

mail.2009

recovery, I think Keith will already have heard about this because he had recently been asked to look at CA in relation to the Kaufman threads (Keith was a co-author on that and Darrell had asked Keith to help with a response to the criticisms). Apart from Keith, I think Tom Melvin here is the only person who could shed light on the McIntyre criticisms of Yamal. But he can be a rather loose cannon and shouldn't be directly contacted about this (also he wasn't involved in the Yamal chronology being discussed, though he has been involved in a regional reconstruction that we've recently been working towards that uses these -- and more -- data). Perhaps Phil and I should talk with Tom and also see if Keith is already considering a response.

Off to lecture for a couple of hours now...

Cheers

Tim

Dr Timothy J Osborn, Academic Fellow

Climatic Research Unit

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University of East Anglia

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1.

<http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle-matching-and-removal-of-natural-variables/>

2.

<http://wattsupwiththat.com/2009/09/24/ooops-dutch-meteorological-institute-caught-in-weather-station-siting-failure-moved-station-and-told-nobody/>

3. <http://www.cru.uea.ac.uk/~timo/>

4. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

1023. 1254232855.txt

#####

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

To: Kevin Trenberth <trenbert@ucar.edu>, Grant Foster <tamino_9@hotmail.com>

Subject: Re: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter

Date: Tue Sep 29 10:00:55 2009

Cc: Mike Mann <mann@meteo.psu.edu>, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, b.mullan@niwa.co.nz, Gavin Schmidt

mail.2009

<gschmidt@giss.nasa.gov>, j.renwick@niwa.co.nz

Grant, Kevin,

Agree on the responses. It does just seem a case of removing a number of the adjectives. It is important to keep the moral high ground in this, if just to show how a comment on a paper should be written and submitted to the same journal that had the poor paper in the first instance. Might be worth reiterating this if any of us get called when the comment does come out. There does seem a trend these days to slam a paper on blogs with no attempt to submit a comment to a journal.

Agree on the running mean/derivative issue - the spectral diagram is better. Scatter plots aren't that useful unless they might help with the (a) parts, but it's

obvious from the time series plots and the r-squareds are so different!

Finally - there was this comment via Jim S from Neville Nicholls. I vaguely recall Angell

and Korshover papers

from that time. The attached refers to some of them - also found Newell and Weare. This

isn't the first, but it might be worth adding. Attached this one from Science as well.

Neville Nicholls wrote:

Hi Jim.

I hope things are going well with you.

Thanks for being part of this robust response to the latest silliness. You have certainly gathered an illustrious group of co-authors.

I am disappointed that you didn't cite the very early (1970s) work by Newell and Weare,

and by Angell and Korshover. I think you should squeeze these in, to demonstrate that

the climate community did not have to wait for McLean et al to understand the influence

of ENSO on global temperatures. In fact, our colleagues in the 1970s understood this,

and demonstrated it much more scientifically than does the McLean et al paper.

Cheers,

Neville

Cheers

Phil

At 21:45 28/09/2009, Kevin Trenberth wrote:

Hi all

About time. Incidentally I gave a copy to Mike McPhaden and discussed it with him last

week when we were together at the OceanObs'09 conference. Mike is President of AGU.

Basically this is an acceptance with a couple of suggestions for extras, and some

suggestions for toning down the rhetoric. I had already tried that a bit. My reaction

is that the main thing is to expedite this. That means no extras unless it really makes

sense. And removal of a few unnecessary words like "absolutely".

In the abstract, we have a number of such adjectives that could be removed: I agree

with Rev 3 in this.

"greatly overstates" could be just "overstates" as it is reinforced better later.

mail.2009
"severely overestimates" could be just "overestimates"
"faulty analysis" maybe "flawed analysis"?
"extremely high" maybe "very high" or "unduly high"
I would leave last sentence alone though as the main comment.
A few more comments embedded below.
Grant Foster wrote:

> From: [1]jgr-atmospheres@agu.org
> Date: Mon, 28 Sep 2009 15:54:05 +0000
> To: [2]tamino_9@hotmail.com
> Subject: 2009JD012960 (Editor - Steve Ghan):Decision Letter
> CC: [3]twistor9@gmail.com
>
> Manuscript Number: 2009JD012960
> Manuscript Title: Comment on "Influence of the Southern Oscillation on
tropospheric
temperature" by J. D. McLean, C. R. de Freitas, and R. M. Carter
>
>

>
> Reviewer Comments
>
> Reviewer #1 (Comments):
>
> This paper does an excellent job of showing the errors in the analytical
methods used
by McLean et al. and why their conclusions
> about the influence of ENSO on global air temperature is incorrect.
> I have only a couple of suggestions to help clarify their analysis of the
methods.
First, a little more explanation of the comment about the time derivative
reduced to an
additive constant would help. Second, in the analysis of the artificial time
series I
think it would be interesting to show the results of both steps of filtering
(running
mean and derivative) as separate time series. This would help the reader
understand why
the filtering creates false correlations. The only other suggestion is to find
a better
adjective than "faulty" in the abstract to characterize the analysis.
>

It is not so easy to see the result from the derivative owing to the phase
shift. The
spectrum actually does a better job. I would address this comment in this way
and
change "faulty".

>
> Reviewer #2 (Comments):
>
> I think this comment on McLean et al can be published more or less as is.
>
> I have two comments
>
> First, in the abstract (page 3, line 15), I'm not sure that "inflating" is
quite the
right verb - the paper itself does not make the point that the filter
constructed by
McLean et al inflates power in the 2-6 year window. Perhaps "isolating" would
be a

better verb.

Yes it should not be in abstract if not in text. Need to point out that the response function in Fig. 1 is greater than unity and does "inflate". So adjust the text.

>
> Secondly, I think the points that are being made with Figures 4 and 5 could be strengthened by adding to the right of each plot of a pair of time series, a scatter plot of the pairs of values available at each time. Such a scatter plot would help to clearly illustrate the absence (upper panels) or presence (lower panels) of correlation between red and black values.

I don't think this helps. There is nothing to be gained from a scatter plot that a correlation or regression value does not summarize.

>
>
> Reviewer #3 (Comments):
>
> Accept pending major changes (mainly in style not scientific comment)
>
> The real mystery here, of course, is how the McLean et al. paper ever made it into JGR. How that happened, I have no idea. I can't see it ever getting published through J Climate. The analyses in McLean et al. are among the worst I have seen in the climate literature. The paper is also a poorly guised attack on the integrity of the climate community, and I guess that is why Foster et al. have taken the energy to contradict its findings.

>
> So the current paper (Foster et al.) should certainly be accepted. Someone needs to address the science in the McLean et al paper in the peer-reviewed literature. But the current paper could be - and should be - done better. That's why I am suggesting major changes before the paper is accepted. All of my suggestions have to do more with the tone and framing of the current paper, rather than its content.

>
> 1. As noted above, I agree McLean et al is problematic. But as it is written, the current paper almost stoops to the level of "blog diatribe". The current paper does not read like a peer-reviewed journal article. The tone is sometimes dramatic and sometimes accusatory. It is inconsistent with the language one normally encounters in the objectively-based, peer-reviewed literature. For examples....

> - In the abstract: Do you really need all of these adjectives?... 'greatly overstates'; 'severely overestimates'; 'faulty analysis'; 'extremely high'.

Agree, see above

mail.2009

> - In the introduction... 'Unfortunately, their conclusions are seriously in error...'
strikes me as overly subjective. Better to say: 'We will demonstrate that their conclusions are strongly dependent on' or something like that...

Don't go that far. Could drop "seriously" but they are "in error"

us > - Page X-6: 'tell us absolutely nothing'. Surely it's enough to state 'tell nothing'.

agree

think we > - Page X-9: 'it is misleading...' That's a strong word. It may be true. But I should rise above such accusations.

misleading is OK. I did a search (not sure I have latest) and found "grossly misleading" and the "grossly" could be removed.

will have a > > Anyway, I'm sure the lead author gets my point. I think the current paper much greater impact (and can claim the high road) if it is rewritten in a more objective manner.

why not > > 2. Similarly, instead of framing the paper as "Taking down McLean et al.", focus more on interesting aspects of the science, such as the frequency dependence between ENSO and global-mean temperature (perhaps cross-correlation analysis would be useful); the importance of not extrapolating results from one timescale to another timescale; or the lack of trends in ENSO. That way, the current paper contributes to the peer-reviewed literature while also doing a service by highlighting the problems with McLean et al.

important I think I tried to emphasize that this should be a teaching moment. Even more given the time lapse.

think the > > 3. In general, the current paper is sloppy and needs tightening. I don't lead author needs 10 pages of text to make the main points.

>
>

So over to you to generate the next draft.

Thanks
Kevin

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1. <mailto:jgr-atmospheres@agu.org>
2. mailto:tamino_9@hotmail.com
3. <mailto:twistor9@gmail.com>
4. <mailto:trenbert@ucar.edu>
5. <http://www.cgd.ucar.edu/cas/trenbert.html>

1024. 1254235516.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: attacks against Keith
Date: Tue, 29 Sep 2009 10:45:16 -0400
Cc: "gschmidt@giss.nasa.gov" <gschmidt@giss.nasa.gov>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

Thanks for the clarification Tim, doesn't change the fact the the attack was inappropriate and unfair of course, but perhaps not as despicable as at first might appear,
M

--

Michael E. Mann
Professor
Director, Earth System Science Center (ESSC)
<http://www.meteo.psu.edu/~mann/Mann/index.html>
http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

On Sep 29, 2009, at 9:50 AM, Tim Osborn <t.osborn@uea.ac.uk> wrote:

> At 14:30 29/09/2009, Gavin Schmidt wrote:
>> The fact is that they launched an assault on Keith knowing full
>> well he isn't in a position to respond. This is despicable.
>
> Gavin,
>
> be careful here, I think it more likely that McIntye only learned of
> Keith's absence after he started posting about Yamal and the real
> reason for the timing of all this is that we made the Yamal tree-
> core measurements available about 2-3 weeks ago (in fact Keith had
> thought they had been made available before he fell ill, and only
> realised in early September that they weren't -- and asked for that
> to be rectified).
>
> Cheers

mail.2009

>
> Tim
>
>
>
>
>
> Dr Timothy J Osborn, Academic Fellow
> Climatic Research Unit
> School of Environmental Sciences
> University of East Anglia
> Norwich NR4 7TJ, UK
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> web: http://www.cru.uea.ac.uk/~timo/
> sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm
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1025. 1254258663.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Andrew Revkin <anrevk@nytimes.com>
Subject: Re: mcintyre's latest...
Date: Tue, 29 Sep 2009 17:11:03 -0400

p.s. Tim Osborn ([1]t.osborn@uea.ac.uk) is probably the best person to contact for further details, in Keith's absence,

mike

On Sep 29, 2009, at 5:08 PM, Michael Mann wrote:

Hi Andy,
I'm fairly certain Keith is out of contact right now recovering from an operation, and is not in a position to respond to these attacks. However, the preliminary information I have from others familiar with these data is that the attacks are bogus. It is unclear that this particular series was used in any of our reconstructions (some of the underlying chronologies may be the same, but I'm fairly certain the versions of these data we have used are based on a different composite and standardization method), let alone any of the dozen other reconstructions of Northern Hemisphere mean temperature shown in the most recent IPCC report, which come to the conclusion that recent warming is anomalous in a long-term context. So, even if there were a problem w/ these data, it wouldn't matter as far as the key conclusions regarding past warmth are concerned. But I don't think there is any problem with these data, rather it appears that McIntyre has greatly distorted the actual information content of these data. It will take folks a few days to get to the bottom of

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this, in Keith's absence.

if McIntyre had a legitimate point, he would submit a comment to the journal in question.

of course, the last time he tried that (w/ our '98 article in Nature), his comment was

rejected. For all of the noise and bluster about the Steig et al Antarctic warming, its now

nearing a year and nothing has been submitted. So more likely he won't submit for peer-reviewed scrutiny, or if it does get his criticism "published" it will be in the

discredited contrarian home journal "Energy and Environment". I'm sure you are aware that

McIntyre and his ilk realize they no longer need to get their crap published in legitimate

journals. All they have to do is put it up on their blog, and the contrarian noise machine

kicks into gear, pretty soon Druge, Rush Limbaugh, Glenn Beck and their ilk (in this case,

The Telegraph were already on it this morning) are parroting the claims. And based on what?

some guy w/ no credentials, dubious connections with the energy industry, and who hasn't

submitted his claims to the scrutiny of peer review.

Fortunately, the prestige press doesn't fall for this sort of stuff, right?

mike

I'm sure you're aware that you will dozens of bogus, manufactured distortions of the

science in the weeks leading up to the vote on cap & trade in the U.S. senate. This is no

On Sep 29, 2009, at 4:30 PM, Andrew Revkin wrote:

needless to say, seems the 2008 pnas paper showing that without tree rings still solid

picture of unusual recent warmth, but McIntyre is getting wide play for his statements

about Yamal data-set selectivity.

Has he communicated directly to you on this and/or is there any indication he's seeking

journal publication for his deconstruct?

--

Andrew C. Revkin

The New York Times / Environment

620 Eighth Ave., NY, NY 10018

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[2]<http://www.nytimes.com/revkin>

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"Dire Predictions" book site:

[5]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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7. http://www.meteo.psu.edu/~mann/Mann/index.html
8. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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

1026. 1254259645.txt

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From: Michael Mann <mann@meteo.psu.edu>
To: Andrew Revkin <anrevk@nytimes.com>
Subject: Re: mcintyre's latest....
Date: Tue, 29 Sep 2009 17:27:25 -0400
Cc: t.osborn@uea.ac.uk

HI Andy,

Yep, what was written below is all me, but it was purely on background, please don't quote anything I said or attribute to me w/out checking specifically--thanks.

Re, your point at the end--you've taken the words out of my mouth. Skepticism is essential for the functioning of science. It yields an erratic path towards eventual truth. But legitimate scientific skepticism is exercised through formal scientific circles, in particular the peer review process. A necessary though not in general sufficient condition for taking a scientific criticism seriously is that it has passed through the legitimate scientific peer review process. those such as McIntyre who operate almost entirely outside of this system are not to be trusted.

mike

On Sep 29, 2009, at 5:19 PM, Andrew Revkin wrote:

thanks heaps.
tom crowley has sent me a direct challenge to mcintyre to start contributing to the reviewed lit or shut up. i'm going to post that soon.

mail.2009

just want to be sure that what is spliced below is from YOU ... a little unclear
? I'm copying this to Tim, in hopes that he can shed light on the specific data
assertions made over at climateaudit.org.....
I'm going to blog on this as it relates to the value of the peer review process
and not on the merits of the mcintyre et al attacks.
peer review, for all its imperfections, is where the herky-jerky process of
knowledge building happens, would you agree?

p.s. Tim Osborn ([1]t.osborn@uea.ac.uk) is probably the best person to contact
for further details, in Keith's absence,
mike

On Sep 29, 2009, at 5:08 PM, Michael Mann wrote:

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

1027. 1254323180.txt

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From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: attacks against Keith
Date: wed, 30 Sep 2009 11:06:20 -0400
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>

Hi Phil,

mail.2009

lets not get into the topic of hate mail. I promise you I could fill your inbox w/ a very long list of vitriolic attacks, diatribes, and threats I've received.

Its part of the attack of the corporate-funded attack machine, i.e. its a direct and highly intended outcome of a highly orchestrated, heavily-funded corporate attack campaign. We saw it over the summer w/ the health insurance industry trying to defeat Obama's health plan, we'll see it now as the U.S. Senate moves on to focus on the cap & trade bill that passed congress this summer. It isn't coincidental that the original McIntyre and MCKitrick E&E paper w/ press release came out the day before the U.S. senate was considering the McCain Lieberman climate bill in '05.

we're doing the best we can to expose this. I hope our Realclimate post goes some ways to exposing the campaign and pre-emptively deal w/ the continued onslaught we can expect over the next month.

thanks for alerting us to that detail of Kaufman et al which I'd overlooked. We'd already asked Darrell if he could compute a Yamal-less version of his series, but as you point out he's really already done this! And Osborn and Briffa '06 is also immune to this issue, as it eliminated any combination of up to 3 of the proxies and showed the result was essentially the same (fair to say this Tim?).

Also, is it fair to say that this particular version of Keith's Yamal series was not what we used in Mann and Jones '03 (we reference Briffa et al '01)?

thanks for the help! We're hoping to have something up tomorrow at the latest, and any updates at your end will be extremely helpful to the case,

mike

On Sep 30, 2009, at 10:30 AM, Phil Jones wrote:

Mike, Gavin,

The short note may not say much. As you're aware Kaufman et al have a plot without trees - their plots shows trees, lakes and ice separately.

Another issue is science by blog sites - and the then immediate response mode. Science ought to work through the peer-review system..... sure you've said all these things before.

we're getting a handful of nasty emails coming and requests for comments on other blog sites. One email has gone to the University Registrar because of the language used. Keith had one that said he was responsible for millions of deaths! Even one reading far too much into his off ill message.

Even though I've had loads of FOIs and nasty emails, a few in the last 2 days have been

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the worst yet. I'm realizing more what those working on animal experiments must have gone through.
Cheers
Phil

At 14:56 30/09/2009, Michael Mann wrote:

great--thanks Tim, sounds like we have a plan. in our post, which we'll target for tomorrow as well, we'll simply link to whatever CRU puts up and re-iterate the sentiment of the temporary short response (i.e. that there was no cherry-picking, a careful and defensible selection procedure was used) and we'll mostly focus on the broader issues, i.e. that any impact of this one series in the vast array of paleoclimate reconstructions (and the importance of the paleoclimate reconstructions themselves) has been over-stated, why these sorts of attacks are not legitimate science, etc.
mike

On Sep 30, 2009, at 9:51 AM, Gavin Schmidt wrote:

of course. we're preparing a 'bigger picture' response and will link directly to CRU and maybe quote from it directly.

=====

Gavin Schmidt
NASA/Goddard Institute for Space Studies
2880 Broadway
New York, NY 10025
Tel: (212) 678 5627

Email: [1]gschmidt@giss.nasa.gov URL: [2]http://www.giss.nasa.gov/~gavin

On wed, 30 Sep 2009, Tim Osborn wrote:

Hi Mike and Gavin,
Keith's temporarily come in to get a handle on all this, but it will take time.
Likely outcome is (1) brief holding note that no cherry-picking was done and demonstrating data selection is defensible by our time tomorrow; (2) longer piece with more evaluation etc. in around a week. No point is posting something that turns out to be wrong. Keith may post them on the CRU website, but presumably they could be linked to from a RealClimate page or, if Keith agrees, be reproduced on RealClimate?
Cheers
Tim

At 14:16 30/09/2009, Michael Mann wrote:

Hi Tim,
Just checking if there are any further developments here, i.e. some more info from either Tom or Keith.
Gavin and I feel we need to do something on RealClimate on this quickly, probably by later today.
thanks in advance for any help you can offer,
mike

On Sep 29, 2009, at 3:46 AM, Tim Osborn wrote:

Hi Mike and Gavin,
thanks for your emails re McIntyre, Yamal and Keith.
I'll pass on your best wishes for his recovery when I next speak to Keith. He's

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been off almost 4 months now and won't be back for at least another month (barring a couple of lectures that he's keen to do in October as part of a gradual return). Hopefully he'll be properly back in November. Regarding Yamal, I'm afraid I know very little about the whole thing -- other than that I am 100% confident that "The tree ring data was hand-picked to get the desired result" is complete crap. Having one's integrity questioned like this must make your blood boil (as I'm sure you know, with both of you having been the target of numerous such attacks). Though it would be nice to shield Keith from this during his recovery, I think Keith will already have heard about this because he had recently been asked to look at CA in relation to the Kaufman threads (Keith was a co-author on that and Darrell had asked Keith to help with a response to the criticisms). Apart from Keith, I think Tom Melvin here is the only person who could shed light on the McIntyre criticisms of Yamal. But he can be a rather loose cannon and shouldn't be directly contacted about this (also he wasn't involved in the Yamal chronology being discussed, though he has been involved in a regional reconstruction that we've recently been working towards that uses these -- and more -- data). Perhaps Phil and I should talk with Tom and also see if Keith is already considering a response.

Off to lecture for a couple of hours now...

Cheers

Tim

Dr Timothy J Osborn, Academic Fellow

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School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ, UK

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Michael E. Mann

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Prof. Phil Jones
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References

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Hidden links:

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

1028. 1254345174.txt

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From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: attacks against Keith
Date: Wed Sep 30 17:12:54 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>

Mike,

I realized you'd have many more bad emails!

As for MJ2003 what we used was an average of Fennoscan, Yamal and Taymir (as one of the series).

Briffa et al (2001) was just referred to in that as a ref to RCS. The paper also talks about N Eurasia, so the sites get a mention.

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27. http://www.meteo.psu.edu/~mann/Mann/index.html
28. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

1029. 1254345329.txt

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>, Phil Jones <p.jones@uea.ac.uk>
Subject: Re: attacks against Keith
Date: Wed Sep 30 17:15:29 2009

mail.2009

Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

At 16:06 30/09/2009, Michael Mann wrote:

And Osborn and Briffa '06 is also immune to this issue, as it eliminated any combination of up to 3 of the proxies and showed the result was essentially the same (fair to say this Tim?).

Mike,
yes, you're right: figs S4-S6 in our supplementary information do indeed show results leaving out individual, groups of two, and groups of three proxies, respectively. It's attached.

I wouldn't say we were immune to the issue -- results are similar for these leave 1, 2 or 3

out cases, but they certainly are not as strong as the case with all 14 proxies. Certainly

in figure S6, there are some cases with 3 omitted (i.e. some sets of 11) where modern

results are comparable with intermittent periods between 800 and 1100. Plus there is the additional uncertainty, discussed on the final page of the supplementary

information, associated with linking the proxy records to real temperatures (remember we

have no formal calibration, we're just counting proxies -- I'm still amazed that Science

agreed to publish something where the main analysis only involves counting from 1 to 14!

:--)). But this is fine, since the IPCC AR4 and other assessments are not saying the evidence is

100% conclusive (or even 90% conclusive) but just "likely" that modern is warmer than MWP.

So, yes, it should be possible to find some subsets of data where MWP and Modern are

comparable and similarly for some seasons and regions. And as you've pointed out before,

if any season/region is comparable (or even has MWP>Modern) then it will probably be the

northern high latitudes in summer time (I think you published on this, suggesting that

combination of orbital forcing, land-use change and sulphate aerosols could cause this for

that season/region, is that right?). So, this Yamal thing doesn't damage Osborn & Briffa (2006), but important to note

that O&B (2006) and others support the "likely" statement rather than being conclusive.

Cheers
Tim

1030. 1254364959.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Malcom Hughes <mhughes@ltrr.arizona.edu>
Subject: draft of Yamal RealClimate post
Date: wed, 30 sep 2009 22:42:39 -0400

mail.2009

Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Dear Tim, Phil, Malcolm, I've enclosed a draft of our article, which we'd like to go online w/ tomorrow (attached as a word file--unfortunately this distorts the post relative to the way it will actually look on the website, but it was the easiest way to send w/ hyperlinks and figures intact). Please let us know if there is anything that you think is either erroneous, unclear, etc. in the piece. we'll link to whatever CRU puts up tomorrow as soon as a link is available. thanks in advance for your help, mike -- Michael E. Mann
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 website: <http://www.meteo.psu.edu/~mann/Mann/index.html> "Dire Predictions" book site: http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html Dear Tim, Phil, Malcolm,

I've enclosed a draft of our article, which we'd like to go online w/ tomorrow (attached as a word file--unfortunately this distorts the post relative to the way it will actually look on the website, but it was the easiest way to send w/ hyperlinks and figures intact).

Please let us know if there is anything that you think is either erroneous, unclear, etc. in the piece.

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

References

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1. <mailto:mann@psu.edu>
2. <http://www.meteo.psu.edu/~mann/Mann/index.html>
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Mike, Gavin,

Here are a few important mods to your piece. Don't mention Keith has been off ill.

Remove the bit about provenance and about access to more data. We'll go into the latter in the longer bit next week.

We'll send the piece we're putting up later - or give you the link.

Rest of your piece is great - especially the bit on how science should be done. Keith has

also picked up in the bit we'll post that McIntyre has put in the caveats but lets others

say the outrageous things in comments or on other blogs.

Cheers

Phil

At 03:42 01/10/2009, Michael Mann wrote:

Dear Tim, Phil, Malcolm,
I've enclosed a draft of our article, which we'd like to go online w/ tomorrow (attached as a word file--unfortunately this distorts the post relative to the way it will actually look on the website, but it was the easiest way to send w/ hyperlinks and figures intact). Please let us know if there is anything that you think is either erroneous, unclear, etc. in the piece. We'll link to whatever CRU puts up tomorrow as soon as a link is available.

thanks in advance for your help,
mike

--

Michael E. Mann
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
website: [1]<http://www.meteo.psu.edu/~mann/Mann/index.htm>
"Dire Predictions" book site:
[2]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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as a word file--unfortunately this distorts the post relative to the way it will

actually look on the website, but it was the easiest way to send w/ hyperlinks and figures intact).

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etc. in the piece.

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website: [4]http://www.meteo.psu.edu/~mann/Mann/index.html
"Dire Predictions" book site:
[5]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.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

References

1. http://www.meteo.psu.edu/~mann/Mann/index.html
2. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
3. mailto:mann@psu.edu
4. http://www.meteo.psu.edu/~mann/Mann/index.html
5. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

1032. 1254501801.txt

#####

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.m.melvin@uea.ac.uk
Subject: Fwd: Re: URGENT
Date: Fri, 02 Oct 2009 12:43:21 +0100

<x-flowed>

>Date: Fri, 02 Oct 2009 13:43:50 +0200
>From: Anders Moberg <anders.moberg@natgeo.su.se>
>User-Agent: Thunderbird 2.0.0.16 (X11/20080720)
>To: Keith Briffa <k.briffa@uea.ac.uk>
>Subject: Re: URGENT
>X-Virus-Scanned: by amavisd-new at smtp.su.se
>X-Spam-Status: No, hits=-2.202 tagged_above=-99 required=7 tests=[AWL=0.110,
> BAYES_00=-2.312]
>X-Spam-Level:
>X-Canit-CHI2: 0.00
>X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f023)
>X-Spam-Score: 0.00 () [Tag at 5.00] SPF(none,0)
>X-CanItPRO-Stream: UEA:f023 (inherits from
>UEA:10_Tag_Only,UEA:default,base:default)
>X-Canit-Stats-ID: 32039918 - 2186b9c79b71
>X-Antispam-Training-Forget:
>https://canit.uea.ac.uk/b.php?i=32039918&m=2186b9c79b71&c=f
>X-Antispam-Training-Nonspam:
>https://canit.uea.ac.uk/b.php?i=32039918&m=2186b9c79b71&c=n
>X-Antispam-Training-Spam:

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><https://canit.uea.ac.uk/b.php?i=32039918&m=2186b9c79b71&c=s>
>X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.184
>
>Yes, of course! It is attached here. As you might perhaps imagine,
>the little corrigendum in Nature 2006 which led me to produce this
>data file was a consequence of requests from McIntyre to get the data.
>
>Actually, Phil has already got the data from me (but he might have
>forgotten it). I don't have any raw data, just the data sent here.
>
>cheers,
>Anders
>
>
>
>Keith Briffa skrev:
>>Anders
>>now I must ask a favour - could you send me the data for the long
>>Russian chronology that was produced by Sidorova et al.
>>At the very least I need the numbers representing their final
>>chronology straight away - I need to include them in a reworking of
>>a recent science paper (rather than trying to digitise them from a
>>scan). I would also like the raw data but understand if you are not able
>>to release these .
>>thanks
>>Keith
>>
>>14:56 01/10/2009, you wrote:
>>>Dear Keith,
>>>
>>>Thanks for the support letter. It is perfect for our case!
>>>
>>>Anders
>>
>>--
>>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/>
>
>--
>
>Anders Moberg
>Bert Bolin Centre for Climate Research
>Department of Physical Geography and Quaternary Geology
>Stockholm University, SE-106 91 Stockholm, Sweden
>
>Phone: +46 (0)8 6747814, Fax: +46 (0)8 164818
>anders.moberg@natgeo.su.se
>www.ink.su.se www.bbcc.su.se
><http://people.su.se/~amobe>
>
>
>

--
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Climatic Research Unit

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Phone: +44-1603-593909
Fax: +44-1603-507784

<http://www.cru.uea.ac.uk/cru/people/briffa/>
</x-flowed>

Attachment Converted: "c:\eudora\attach\indigirka_moberg05.dat"

1033. 1254505571.txt

#####

From: Malcolm Hughes <mhughes@ltrr.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: IN STRICTEST CONFIDENCE
Date: Fri, 02 Oct 2009 13:46:11 -0700
Cc: t.m.melvin@uea.ac.uk

<x-flowed>

Keith - is there a time in the next few days when you could stand talking briefly about this on the phone? I think the fog about the status of the Indigirka/Yakutua data could be cleared really quickly that way. Once again, I'm really sorry it has been necessary to bother you with this. Cheers, Malcolm

Keith Briffa wrote:

> Malcolm
> honestly just a cross thread between Tom and I. I had been asked by
> Darrell whether we should use the Sidorova chronology - because of
> hassle by you know who - so asked Tom a while ago to ask you. I did
> not see your answer - sorry if you cc'd me in as I have not been
> checking emails. I fully accept and would NEVER go behind your back to
> ask for the data. I understood that the chronology was published and
> so thought to compare our RCS version with it if we could produce it
> in time . we are being accused of not using that chronology in the
> Science paper- so then asked Anders for it. I am happy to send Darrell
> the single chronology if that is what Anders has sent. I am having to
> start thinking about the Yamal crap and then this Darrell stuff
> suddenly arises. I just wanted him to consider including the Polar
> Urals reconstruction and the Sidorova series in his analysis before
> publishing a correction in Science- apparently the selection criterion
> for inclusion of series was anything published north of 60 degrees and
> longer than 1000 years. I could do without all this now - don't really
> understand what Climate Audit are getting so hysterical about but feel
> that I can not ignore it this time - but don't feel up to getting
> involved. I fully admit to being out of the loop as regards all this
> and having trouble getting back to it.

>
> To restate - this was a confusion. I fully accept your point (as you
> know I would). Sorry if you thought I was doing anything without your
> knowledge - TO BE HONEST ALSO - I actually was not really aware that
> the data you were producing and that used by Sidorova were one and the
> same. Best wishes hopefully all ok
> I assume that we are allowed to use the chronolgy as published - are
> we? I have not contacted Sidorova. Can you cc answer to Tom as I have
> no email at present. (this coming from someone elses computer)
> Keith
>
>

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> At 16:50 02/10/2009, you wrote:

>> Dear Keith - I do hope your recovery continues apace, in spite of the
>> recent nonsense. I really have had no intention to bother you with
>> work stuff, and had strongly encouraged Mike and Gavin to contact
>> Tim and/or Tom putting a response on R1C1. So, I'm really reticent to
>> raise something else, but must.

>> What's going on? 21st September I got an email from Tom M that
>> contained the following para, among other more general discussion:
>> "Keith has been complained at by Climate Audit for cherry picking and
>> not using your long Indigirka River data set. Not used because we did
>> not have the data. Please, could we have the data? We will make
>> proper acknowledgement/coauthorship if we use the data."

>> I replied pretty much straight away thus: "Hi Tom - please find the
>> Esper article in question attached. The so-called Indigirka River
>> data set is not yet available because it has not been published. I
>> am currently working on that with Russian colleagues, and was indeed
>> in Switzerland the week before last to work with one of them on
>> specifically this. All being well, there will be an accepted
>> manuscript before next summer, and at that point I will make the data
>> freely available. Once we get to that point, I'll let you know, of
>> course. Cheers, Malcolm"

>> So far, no direct response to this email from Tom.

>> This morning I get an email from Anders Moberg, telling me that you
>> had asked him for the "Indigirka data". I've waited a couple of hours
>> before writing this email so as to try to be constructive. To be sure
>> that you understand what that dataset is and is not, please read the
>> attached 2006 Moberg corrigendum.

>> Once again, the actual data are unpublished, in spite of having been
>> discussed in the Russian literature by Siderova et al. A large
>> proportion of the raw data are not yet in the public domain, and so
>> you would not be able to critically evaluate the chronology as a
>> possible climate proxy. Why can that not be said - adequate metadata
>> not available, please see Moberg corrigendum? By the way, a 600-year
>> reconstruction is available (Hughes et al 1999, also attached), and
>> all those raw data are at the ITRDB.

>> As you know, it is my intention to friendly, cooperative and open,
>> but I'm determined to get some scientific value from all the years of
>> work I've invested in the Yakutia work, and in cooperation with
>> Russia in general. Releasing these data now would be too much.
>> Cheers, Malcolm

>>

>>

>> --

>> Malcolm K Hughes
>> Regents' Professor
>> Laboratory of Tree-Ring Research
>> The University of Arizona
>> 105 W Stadium
>> Tucson, AZ 85721
>> USA

>>

>> tel: +1-520-621-6470

>> fax: +1-520-621-8229

>>

>> mhughes@ltrr.arizona.edu

>>

>> <http://www.ltrr.arizona.edu/people/8>

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> --
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> Climatic Research Unit
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> Norwich, NR4 7TJ, U.K.
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> Fax: +44-1603-507784
>
> <http://www.cru.uea.ac.uk/cru/people/briffa/>
</x-flowed>

1034. 1254517566.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Malcolm Hughes <mhughes@ltrr.arizona.edu>
Subject: Re: [Fwd: IN STRICTEST CONFIDENCE]
Date: Fri Oct 2 17:06:06 2009

Malcolm,
Keith should be reading emails. Probably been a misunderstanding. I've only glanced at the nonsense but didn't see anything related to Indigirka. I see they are now getting at the Taimyr site, so Keith/Tom having to look at that one too. They have some extra data from Vlad which CA won't have, so whatever they say there will get more emails about keeping hold of more data. All the issues seem to relate to canopy closed sites like Fritz would have likely sampled and more open sites. They are trying to contact the Russians to get site pictures or anything else. Keith is on +44 1953 851013 if you fancy calling at the weekend. They get at us for keeping hold of data, but they have no intention of publishing in the peer-review literature!
Cheers
Phil
At 16:56 02/10/2009, you wrote:

Phil - just in case Keith is not opening email and Tom is helping him out by taking initiative, here's an email I just sent Keith. Unfortunately, I really had to respond to this. I hope all is going well for you. Cheers, Malcolm

--
Malcolm K Hughes
Regents' Professor
Laboratory of Tree-Ring Research
The University of Arizona
105 W Stadium
Tucson, AZ 85721
USA
tel: +1-520-621-6470
fax: +1-520-621-8229
mhughes@ltrr.arizona.edu
[1]<http://www.ltrr.arizona.edu/people/8>
Message-ID: <4AC6212D.7070401@ltrr.arizona.edu>
Date: Fri, 02 Oct 2009 08:50:05 -0700
From: Malcolm Hughes <mhughes@ltrr.arizona.edu>

mail.2009

User-Agent: Thunderbird 2.0.0.23 (windows/20090812)

MIME-Version: 1.0

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

Subject: IN STRICTEST CONFIDENCE

Content-Type: multipart/mixed;

boundary="-----090305040400060007010009"

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As you know, it is my intention to be friendly, cooperative and open, but I'm determined to get some scientific value from all the years of work I've invested in the Yakutia work, and in cooperation with Russia in general. Releasing these data now would be too much.
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--
Malcolm K Hughes

mail.2009

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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

References

- 1. <http://www.ltrr.arizona.edu/people/8>
- 2. <http://www.ltrr.arizona.edu/people/8>

1035. 1254518902.txt

#####

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Malcolm Hughes <mhughes@ltrr.arizona.edu>
Subject: Re: IN STRICTEST CONFIDENCE
Date: Fri, 02 Oct 2009 17:28:22 +0100
Cc: t.m.melvin@uea.ac.uk

<x-flowed>
Malcolm

honestly just a cross thread between Tom and I. I had been asked by Darrell whether we should use the Sidorova chronology - because of hassle by you know who - so asked Tom a while ago to ask you. I did not see your answer - sorry if you cc'd me in as I have not been checking emails. I fully accept and would NEVER go behind your back to ask for the data. I understood that the chronology was published and so thought to compare our RCS version with it if we could produce it in time . We are being accused of not using that chronology in the Science paper- so then asked Anders for it. I am happy to send Darrell the single chronology if that is what Anders has sent. I am having to start thinking about the Yamal crap and then this Darrell stuff suddenly arises. I just wanted him to consider including the Polar Urals reconstruction and the Sidorova series in his analysis before publishing a correction in Science- apparently the selection criterion for inclusion of series was anything published north of 60 degrees and longer than 1000 years. I could do without all this now - don't really understand what Climate Audit are getting so hysterical about but feel that I can not ignore it this time - but don't feel up to getting involved. I fully admit to being out of the loop as regards all this and having trouble getting back to it.

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>in Switzerland the week before last to work with one of them on
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>manuscript before next summer, and at that point I will make the
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>proportion of the raw data are not yet in the public domain, and so
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>Cheers, Malcolm

>

>

>---

>Malcolm K Hughes
>Regents' Professor
>Laboratory of Tree-Ring Research
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>105 W Stadium
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>mhughes@ltrr.arizona.edu
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>http://www.ltrr.arizona.edu/people/8
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Fax: +44-1603-507784

<http://www.cru.uea.ac.uk/cru/people/briffa/>

</x-flowed>

1036. 1254746802.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: thanks and one question
Date: Mon, 5 Oct 2009 08:46:42 -0400
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

thanks Phil,

I wondered where this completely false claim was coming with. Are these people really so clueless that they don't even understand that I have nothing to do with this whatsoever. Pretty much tells you everything you need to do.

I never acknowledge emails from people I don't know, about topics that are in any way sensitive. this is a perfect example of something that goes right to the trash bin,

mike

On Oct 5, 2009, at 5:55 AM, Phil Jones wrote:

Gavin, Mike,
Thanks for this!
I assume you are both aware of this prat - Neil Craig, see below. Keith won't be responding.
Checking facts doesn't seem important these days. As CA threads aren't publications this is difficult for non scientists.
I am going further over one email I got at the weekend - see also below.
Typical of Sonia
- although she now seems to only be an emeritus reader!

Cheers
Phil

Return-path: <[1]CrgN143@aol.com>
From: [2]CrgN143@aol.com
Full-name: CrgN143

mail.2009

Message-ID: <[3]d03.64b01875.37f87aa4@aol.com>

Date: Sat, 3 Oct 2009 06:00:04 EDT

Subject: Tree rings - accusation that you were solely responsible.

To: [4]k.briffa@uea.ac.uk

MIME-Version: 1.0

Content-Type: multipart/alternative;

boundary="-----1254564004"

X-Mailer: 9.0 SE for windows sub 5045

Professor Briffa,

I have written a couple of blogs on the current report
by Steve
McIntyre that the data used by Mann to "prove" the hockey stick was fabricated.
This & the
following day's

[5]<http://a-place-to-stand.blogspot.com/2009/10/global-warming-proven-deliberate-fraud.html>

As a result I have received this email from somebody I am not acquainted
with throwing
the entire blame on you. This seems improbable to me & possibly an alarmist
damage
limitation exercise. If you wish to comment I would be happy for you to do so.

"Please note: Steve McIntyre's post concerns work by climate scientist Keith
Briffa and
not Michael Mann. You will probably wish to correct your post.
Cheers
Avisame"

I have posted this as an update with my reply:

"My understanding is that while Briffa did the tree ring measurement, Mann, in
his paper,
chose to choose 12 atypical tree rings out of at least 34 to fabricate the global
warming
trend. My assumption is that Mann is responsible for fabrications in his own
paper & that
this is a damage limitation exercise. I am open to correction on this & indeed
have emailed
Mr Briffa to see. "

Neil Craig

You may be interested in my political blog

[6]<http://a-place-to-stand.blogspot.com/>

We received this through our enquiries desk. I assume that you are aware of this
person,
including those copied on the message.

If we are to respond, it would be to indicate that there are multiple sources of
supporting
evidence and that we continue to place our confidence in the international
scientific
assessment process. This confidence has proven to be well placed.
Roger

From: Sonja A Boehmer-Christiansen <[7]Sonja.B-C@hull.ac.uk>

Date: 2 October 2009 18:09:39 GMT+01:00

To: Stephanie Ferguson <[8]stephanie.ferguson@ukcip.org.uk>

Cc: "Peiser, Benny" <[9]B.J.Peiser@lmu.ac.uk>, Patrick David Henderson
<[10]pdhenderson18@googlemail.com>, Christopher Monckton <[11]monckton@mail.com>

Subject: RE: Please take note of potetially serious allegations of

scientific

'fraud' by CRU and Met Office

Dear Stephanie

I expect that a great deal of UKCIP work is based on the data provided by CRU (as does the work of the IPCC and of course UK climate policy). Some of this, very fundamentally, would now seem to be open to scientific challenge, and may even face future legal enquiries. It may be in the interest of UKCIP to inform itself in good time and become a little more 'uncertain' about its policy advice.

Perhaps you can comment on the following and pass the allegations made on to the relevant people.

It is beyond my expertise to assess the claims made, but they would fit into my perception of the whole 'man-made global warming' cum energy policy debate. I know several of the people involved personally and have no reason to doubt their sincerity and honour as scientists, though I am also aware of their highly critical (of IPCC science) policy positions.

I could also let you have statements by Steve McIntyre and Ross MCKitrick. Ross MCKitrick currently teaches at Westminster Business School and who is fully informed about the relevant issues. He recently addressed a meeting of about 50 people in London.

Best wishes

Sonja B-C

Dr.Sonja A.Boehmer-Christiansen
Reader Emeritus, Department of Geography
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TWO copied pieces follow, both relate to CRU and UK climate policy

a. THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

" Jennifer Marohasy <[13]jennifermarohasy@jennifermarohasy.com>

Leading UK Climate Scientists Must Explain or Resign, Jennifer Marohasy
< <[14]

<http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists ->>
[15]<http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists - must-explain-or-resign/>>

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"Dire Predictions" book site:
[19]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. <mailto:CrgN143@aol.com>
2. <mailto:CrgN143@aol.com>
3. <mailto:d03.64b01875.37f87aa4@aol.com>
4. <mailto:k.briffa@uea.ac.uk>
- 5.

<http://a-place-to-stand.blogspot.com/2009/10/global-warming-proven-deliberate-fraud.html>

6. <http://a-place-to-stand.blogspot.com/>
7. <mailto:Sonja.B-C@hull.ac.uk>
8. <mailto:stephanie.ferguson@ukcip.org.uk>
9. <mailto:B.J.Peiser@ljmu.ac.uk>
10. <mailto:pdhenderson18@gmail.com>
11. <mailto:monckton@mail.com>
12. <http://www.multi-science.co.uk/>
13. <mailto:jennifermarohasy@jennifermarohasy.com>
14. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>
15. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>
16. <mailto:p.jones@uea.ac.uk>
17. <mailto:mann@psu.edu>
18. <http://www.meteo.psu.edu/~mann/Mann/index.html>
19. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

1037. 1254751382.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [geo] Re: CCNet: A Scientific Scandal Unfolds
Date: Mon Oct 5 10:03:02 2009

Tom,
Thanks for trying to clear the air with a few people. Keith is still working on a response. Having to contact the Russians to get some more site details takes time.

mail.2009

Several things in all this are ludicrous as you point out. Yamal is one site and isn't

in most of the millennial reconstructions. It isn't in MBH, Crowley, Moberg etc. Also

picking trees for a temperature response is not done either.

The other odd thing is that they seem to think that you can reconstruct the last

millennium from a few proxies, yet you can't do this from a few instrumental series for the

last 150 years! Instrumental data are perfect proxies, after all.

[1]http://www.americanthinker.com/2009/10/un_climate_reports_they_lie.html

This one is wrong as well. IPCC (1995) didn't use that silly curve that Chris Folland or

Geoff Jenkins put together.

Cheers

Phil

At 02:59 05/10/2009, you wrote:

David,

This is entirely off the record, and I do not want this shared with anyone. I hope you will respect this. This issue is not my problem, and I await further developments.

However, Keith Briffa is in the Climatic Research Unit (CRU), and I was Director of CRU for many years so I am quite familiar with Keith and with his work. I have also done a lots of hands on tree ring work, both in the field and in developing and applying computer programs for climate reconstruction from tree rings. On the other hand, I have not been involved in any of this work since I left CRU in 1993 to move to NCAR. But I do think I can speak with some modicum of authority.

You say, re dendroclimatologists, "they rely on recent temperature data by which to

select recent tree data" (my emphasis). I don't know where you get this idea, but I

can assure you that it is entirely wrong.

Further, I do not know the basis for your claim that "Dendrochonology is a bankrupt approach". It is one of the few proxy data areas where rigorous multivariate statistical tools are used and where reconstructions are carefully tested

on independent data.

Finally, the fact that scientists (in any field) do not willingly share their hard-earned primary data implies that they have something to hide has no logical basis.

Tom.

+++++

David Schnare wrote:

Tom:

Briffa has already made a preliminary response and he failed to explain his selection

procedure. Further, he refused to give up the data for several years, and was forced to

do so only when he submitted to a journal that demanded data archiving and actually

enforced the practice.

More significantly, Briffa's analysis is irrelevant. Dendrochonology is a bankrupt

approach. They admit that they cannot distiguish causal elements contributing to tree

ring size. Further, they rely on recent temperature data by which to select recent tree

data (excluding other data) and then turn around and claim that the tree ring

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data

explains the recent temperature data. If you can give a principled and reasoned defense of Briffa (see the discussion on watt's website) then go for it. I'd be fascinated, as would a rather large number of others.

None of this, of course, detracts for the need to do research on geoengineering. David Schnare

On Sun, Oct 4, 2009 at 8:50 PM, Tom Wigley <wigley@ucar.edu>
<mailto:wigley@ucar.edu>>
wrote:

Dear all,
I think it would be wise to let Briffa respond to these accusations before compounding them with unwarranted extrapolations.
With regard to the Hockey Stick, it is highly unlikely that a single site can be very important. M&M have made similar accusations in the past and they have been shown, in the peer-reviewed literature, to be ill-founded.
Two recent papers you should read are those in the attached word document (first pages only).
Tom.

++++
Eugene I. Gordon wrote:
David:

I concede all of your points but add one other thought. It is my grandchildren I worry about and I suspect their grand children will find it exceedingly warm because sunspots will return and carbon abatement is only a game; It wont happen significantly in their lifetime AND IT WONT BE ENOUGH IN ANY CASE. HENCE _WE WILL NEED A GEOENGINEERING SOLUTION_ COME WHAT MAY!
-gene

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From: geoengineering@googlegroups.com
<[5]mailto:geoengineering@googlegroups.com>
[mailto:geoengineering@googlegroups.com
<[6]mailto:geoengineering@googlegroups.com>] *On Behalf Of *David Schnare
Sent: Sunday, October 04, 2009 10:49 AM
Cc: Alan white; geoengineering@googlegroups.com
<[7]mailto:geoengineering@googlegroups.com>
Subject: [geo] Re: CCNet: A Scientific Scandal Unfolds

Gene:

I've been following this issue closely and this is what I take away from it:

1) Tree ring-based temperature reconstructions are fraught with so much uncertainty, they have no value whatever. It is impossible to tease out the relative contributions of rainfall, nutrients, temperature and access to sunlight. Indeed a single tree can, and apparently has, skewed the entire 20th century

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temperature reconstruction.

2) The IPCC peer review process is fundamentally flawed if a lead author is able to both disregard and ignore criticisms of his own work, where that work is the critical core of the chapter. It not only destroys the credibility of the core assumptions and data, it destroys the credibility of the larger work - in this case, the IPCC summary report and the underlying technical reports. It also destroys the utility and credibility of the modeling efforts that use assumptions on the relationship of CO2 to temperature that are based on Britta's work, which is, of course, the majority of such analyses.

As Corcoran points out, "the IPCC has depended on 1) computer models, 2) data collection, 3) long-range temperature forecasting and 4) communication. None of these efforts are sitting on firm ground."

Nonetheless, and even if the UNEP thinks it appropriate to rely on wikipedia as their scientific source of choice, greenhouse gases may (at an ever diminishing probability) cause a significant increase in global temperature. Thus, research, including field trials, on the leading geoengineering techniques are appropriate as a backstop in case our children find out that the current alarmism is justified.

David Schnare

On Sun, Oct 4, 2009 at 8:35 AM, Eugene I. Gordon

<euggordon@comcast.net <[8]mailto:euggordon@comcast.net>

<[9]mailto:euggordon@comcast.net <mailto:euggordon@comcast.net>>>

wrote:

Alan:

Thanks for the extensive and detailed e-mail. This is terrible but not surprising. Obviously I do not know what gives with these guys. However, I have my own suspicions and hypothesis. I dont think they are scientifically inadequate or stupid. I think they are dishonest and members of a club that has much to gain by practicing and perpetuating global warming scare tactics. That is not to say that global warming is not occurring to some extent since it would be even without CO2 emissions. The CO2 emissions only accelerate the warming and there are other factors controlling climate. As a result, the entire process may be going slower than the powers that be would like. Hence, (I postulate) the global warming contingent has substantial motivation to be dishonest or seriously biased, and to be loyal to their equally dishonest club members. Among the motivations are increased and continued grant funding, university advancement, job advancement, profits and payoffs from carbon control advocates such as Gore, being in the limelight, and other motivating factors I am too inexperienced to identify.

Alan, this is nothing new. You and I experienced similar behavior from some of our colleagues down the hall, the Bell Labs research people, in the good old days. Humans are hardly perfect creations. I am never surprised at what they can do. _I am perpetually grateful for those who are honest and fair and thankfully there is a goodly share of those._

-gene

From: Alan white [mailto:adwhite99@comcast.net

<[10]mailto:adwhite99@comcast.net> <[11]mailto:adwhite99@comcast.net>

mail.2009

<[12]mailto:adwhite99@comcast.net>>]

Sent: Saturday, October 03, 2009 8:28 PM

To: Gene Gordon

Subject: Fw: CCNet: A Scientific Scandal Unfolds

more of the same. what gives with these guys?

----- Original Message -----

From: Peiser, Benny <[13]mailto:B.J.Peiser@ljmu.ac.uk>

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To: CCNetMedia <[15]mailto:CCNetMedia@livjm.ac.uk>

<[16]mailto:CCNetMedia@livjm.ac.uk>>

Sent: Friday, October 02, 2009 6:36 AM

Subject: CCNet: A Scientific Scandal Unfolds

CCNet 153/2009 - 2 October 2009 -- Audiatur et altera pars
CRU'S HIDDEN DATA AND THE IPCC: A SCIENTIFIC SCANDAL UNFOLDS

A scientific scandal is casting a shadow over a number of recent peer-reviewed climate papers. The scandal has serious implications for public trust in science. The IPCC's mission is to reflect the science, not create it. As the IPCC states, its duty is "assessing the scientific, technical and socioeconomic information relevant for the understanding of the risk of human-induced climate change. It does not carry out new research nor does it monitor climate-related data." But as IPCC lead author, Briffa was a key contributor in shaping the assessment. When the IPCC was alerted to peer-reviewed research that refuted the idea, it declined to include it. This leads to the more general, and more serious issue: what happens when peer-review fails - as it did here?

--Andrew Orłowski, The Register, 29 September 2009
Over the next nine years, at least one paper per year appeared in prominent journals using Briffa's Yamal composite to support a hockey stick-like result. The IPCC relied on these studies to defend the Hockey Stick view, and since it had appointed Briffa himself to be the IPCC Lead Author for this topic, there was no chance it would question the Yamal data. Despite the fact that these papers appeared in top journals like Nature and Science, none of the journal reviewers or editors ever required Briffa to release his Yamal data. Steve McIntyre's repeated requests for them to uphold their own data disclosure rules were ignored.

--Ross MCKitrick, Financial Post, 1 October 2009
The official United Nation's global warming agency, the Intergovernmental Panel on Climate Change, is a four-legged stool that is fast losing its legs. To carry the message of man-made global warming theory to the world, the IPCC has depended on 1) computer models, 2) data collection, 3) long-range temperature forecasting and 4) communication. None of these efforts are sitting on firm ground.

--Terence Corcoran, National Post, 1 October 2009
Media reaction to the Yamal story has been rather limited so

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far. I'm not sure whether this is because people are trying to digest what it means or whether it's "too hot to handle". None of the global warming supporters in the mainstream media have gone near it. The reaction of the Guardian - to delete any mention of the affair from their comment threads - has been extraordinary.

--Bishop Hill, 1 October 2009

Britain will have to stop building airports, switch to electric cars and shut down coal-fired power stations as part of a 'planned recession' to avoid dangerous climate change. A new report from the Tyndall Centre for Climate Change Research says the only way to avoid going beyond the dangerous tipping point is to double the target to 70 per cent by 2020. This would mean reducing the size of the economy through a "planned recession".

--Louise Gray, The Daily Telegraph, 30 September 2009

Tokyo governor Shintaro Ishihara warned on Wednesday the 2016 Olympics could be the last Games, with global warming an immediate threat to mankind. "It could be that the 2016 Games are the last Olympics in the history of mankind," Ishihara told reporters at a Tokyo 2016 press event ahead of the vote. "Global warming is getting worse. We have to come up with measures without which Olympic Games could not last long. "Scientists have said we have passed the point of no return," said Ishihara.

--Karolos Grohmann, Reuters, 30 September 2009

- (1) TREEMOMETERS: A NEW SCIENTIFIC SCANDAL
Andrew Orłowski, The Register, 29 September 2009
- (2) ANALYSIS: DEFECTS IN KEY CLIMATE DATA ARE UNCOVERED
Ross Mckittrick, Financial Post, 1 October 2009
- (3) OPINION: CLIMATE DATA BUSTER
Terence Corcoran, National Post, 1 October 2009
- (4) OPINION: COOLING DOWN THE CASSANDRAS
George F. Will, The Washington Post, 1 October 2009
- (5) U.S. THROWS SPANNER INTO CLIMATE TALKS
Times of India, 2 October 2009
- (6) CAP AND TRADE MAY SINK OPPOSITION LEADER DOWN UNDER
Lenore Taylor, The Australian, 2 October 2009
- (7) THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN
Jennifer Marohasy <jennifermarohasy@jennifermarohasy.com>

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(10) COPENHAGEN SUMMIT: DO SCIENCE AND ECONOMICS SUPPORT GOVERNMENT ACTION ON GLOBAL WARMING?

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(12) AND FINALLY: 'PLANNED RECESSION' COULD AVOID CATASTROPHIC CLIMATE CHANGE

Louise Gray, The Daily Telegraph, 30 September 2009

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(1) TREEMOMETERS: A NEW SCIENTIFIC SCANDAL

The Register, 29 September 2009

<[32]http://www.theregister.co.uk/2009/09/29/yamal_scandal/>

By Andrew Orłowski

A scientific scandal is casting a shadow over a number of recent peer-reviewed climate papers.

At least eight papers purporting to reconstruct the historical temperature record times may need to be revisited, with significant implications for contemporary climate studies, the basis of the IPCC's

assessments. A number of these involve senior climatologists at the British climate research centre CRU at the University East Anglia. In

every case, peer review failed to pick up the errors.

At issue is the use of tree rings as a temperature proxy, or dendrochronology. Using statistical techniques, researchers take the ring data to create a "reconstruction" of historical temperature anomalies. But trees are a highly controversial indicator of temperature, since the rings principally record CO₂, and also record humidity, rainfall, nutrient intake and other local factors.

Picking a temperature signal out of all this noise is problematic, and a

dendrochronology can differ significantly from instrumented data. In dendro jargon, this disparity is called "divergence". The process of creating a raw data set also involves a selective use of samples - a choice open to a scientist's biases.

Yet none of this has stopped paleoclimatologists from making bold claims using tree ring data.

In particular, since 2000, a large number of peer-reviewed climate papers have incorporated data from trees at the Yamal Peninsula in Siberia. This dataset gained favour, curiously superseding a

newer and larger data set from nearby. The older Yamal trees indicated pronounced

and dramatic uptick in temperatures.

How could this be? Scientists have ensured much of the measurement data

used in the reconstructions remains a secret - failing to fulfill procedures to archive the raw data. Without the raw data, other scientists could not reproduce the results. The most prestigious peer

reviewed journals, including Nature and Science, were reluctant to demand the data from contributors. Until now, that is.

At the insistence of editors of the Royal Society's Philosophical Transactions B the data has leaked into the open - and Yamal's mystery

is no more.

>From this we know that the Yamal data set uses just 12 trees from a

larger set to produce its dramatic recent trend. Yet many more were

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cored, and a larger data set (of 34) from the vicinity shows no dramatic recent warming, and warmer temperatures in the middle ages. In all there are 252 cores in the CRU Yamal data set, of which ten were alive 1990. All 12 cores selected show strong growth since the mid-19th century. The implication is clear: the dozen were cherry-picked. Controversy has been raging since 1995, when an explosive paper by Keith Briffa at the Climate Research Unit at the University of East Anglia asserted that that the medieval warm period was actually really cold, and recent warming is unusually warm. Both archaeology and the historical accounts, Briffa was declaring, were bunk. Briffa relied on just three cores from Siberia to demonstrate this. Three years later Nature published a paper by Mann, Bradley and Hughes based on temperature reconstructions which showed something similar: warmer now, cooler then. With Briffa and Mann as chapter editors of the UN's Intergovernmental Panel on Climate Change (IPCC), this distinctive pattern became emblematic - the "Logo of Global warming". IPCC's Assessment Report from 2001 - with the error bars in grey emphasised Hokey hockey sticks Mann too used dendrochronology to chill temperatures, and rebuffed attempts to publish his measurement data. Initially he said he had forgotten where he put it, then declined to disclose it. (Some of Mann's data was eventually discovered, by accident, on his ftp server in a directory entitled 'BACKTO_1400-CENSORED'.) Tree data was secondary in importance to Mann's statistical technique, which would produce a dramatic modern upturn in temperatures - which became nicknamed the "Hockey Stick" - even using red noise. Similarly, all the papers that used the Yamal data have the same point to make. All suggest recent dramatic warming. Having scored a global hit with a combination of flawed statistics and dubious dendrochronology, the acts repeated the formula. "Late 20th century warmth is unprecedented for at least roughly the past two millennia for the Northern Hemisphere," wrote the two authors of Global Surface Temperatures over the Past Two Millennia published in Geophysical Research Letters in 2003 - Mann, and Phil Jones of CRU. For example, Briffa's 2008 paper concludes that: "The extent of recent widespread warming across northwest Eurasia, with respect to 100- to 200-year trends, is unprecedented in the last 2000 years." The same authors in 2004: It continues to this day. A study purporting to show the Arctic was warmer now than for 2,000 years received front-page attention last month. Led by Northern Arizona University professor Darrell S Kaufman, and including dendro veteran Mann, this too relied heavily on Yamal, and produced the signature shape. Now here's Yamal.

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And when Yamal is plotted against the wider range of cores, the implications of the choice is striking:

A comparison of Yamal RCS chronologies. red - as archived with 12 picked

cores; black - including Schweingruber's Khadyta River, Yamal (russ035w)

archive and excluding 12 picked cores. Both smoothed with 21-year gaussian smooth. y-axis is in dimensionless chronology units centered on

1 (as are subsequent graphs (but represent age-adjusted ring width).

"The majority of these trees (like the Graybill bristlecones) have a prolonged growth pulse (for whatever reason) starting in the 19th century," wrote Canadian mathematician Steve McIntyre on his blog on Sunday. "When a one-size fits all age profile is applied to these particular trees, the relatively vigorous growth becomes monster growth

- 8 sigma anomalies in some of them."

McIntyre's determination to reproduce the reconstructions has resulted

in the Yamal data finally coming to light.

All the papers come from a small but closely knit of scientists who mutually support each other's work. All use Yamal data.

What went wrong?

The scandal has serious implications for public trust in science. The

IPCC's mission is to reflect the science, not create it.

As the panel states, its duty is "assessing the scientific, technical

and socioeconomic information relevant for the understanding of the risk

of human-induced climate change. It does not carry out new research nor

does it monitor climate-related data." But as lead author, Briffa was a

key contributor in shaping (no pun intended) the assessment.

When the IPCC was alerted to peer-reviewed research that refuted the idea, it declined to include it. This leads to the more general, and more serious issue: what happens when peer-review fails - as it did here?

The scandal has only come to light because of the dogged persistence of

a Canadian mathematician who attempted to reproduce the results. Steve

McIntyre has written dozens of letters requesting the data and methodology, and over 7,000 blog posts. Yet Yamal has remained elusive

for almost a decade. (r)

Footnote

The Royal Society's motto from the enlightenment era is Nullius in verba. "On nobody's authority" or colloquially, "take nobody's word for

it". In 2007, the Society's then president suggested this be changed to

"respect the facts".

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(2) ANALYSIS: DEFECTS IN KEY CLIMATE DATA ARE UNCOVERED
Financial Post, 1 October 2009

<[33]<http://network.nationalpost.com/np/blogs/fpcomment/archive/2009/10/01/ross-mckitrick-defects-in-key-climate-data-are-uncovered.aspx>>

By Ross Mckitrick

Beginning in 2003, I worked with Stephen McIntyre to replicate a

famous result in paleoclimatology known as the Hockey Stick graph. Developed by a U.S. climatologist named Michael Mann, it was a statistical compilation of tree ring data supposedly proving that air temperatures had been stable for 900 years, then soared off the charts in the 20th century. Prior to the publication of the Hockey Stick, scientists had held that the medieval-era was warmer than the present, making the scale of 20th century global warming seem relatively unimportant. The dramatic revision to this view occasioned by the Hockey Stick's publication made it the poster child of the global warming movement. It was featured prominently in a 2001 report of the U.N. Intergovernmental Panel on Climate Change (IPCC), as well as government websites and countless review reports. Steve and I showed that the mathematics behind the Mann Hockey Stick were badly flawed, such that its shape was determined by suspect bristlecone tree ring data. Controversies quickly piled up: Two expert panels involving the U.S. National Academy of Sciences were asked to investigate, the U.S. Congress held a hearing, and the media followed the story around the world. The expert reports upheld all of our criticisms of the Mann Hockey Stick, both of the mathematics and of its reliance on flawed bristlecone pine data. One of the panels, however, argued that while the Mann Hockey Stick itself was flawed, a series of other studies published since 1998 had similar shapes, thus providing support for the view that the late 20th century is unusually warm. The IPCC also made this argument in its 2007 report. But the second expert panel, led by statistician Edward Wegman, pointed out that the other studies are not independent. They are written by the same small circle of authors, only the names are in different orders, and they reuse the same few data climate proxy series over and over. Most of the proxy data does not show anything unusual about the 20th century. But two data series have reappeared over and over that do have a hockey stick shape. One was the flawed bristlecone data that the National Academy of Sciences panel said should not be used, so the studies using it can be set aside. The second was a tree ring curve from the Yamal Peninsula in Siberia, compiled by UK scientist Keith Briffa. Briffa had published a paper in 1995 claiming that the medieval period actually contained the coldest year of the millennium. But this claim depended on just three tree ring records (called cores) from the Polar Urals. Later, a colleague of his named F. H. Schweingruber produced a

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much larger sample from the Polar Urals, but it told a very different story: The medieval era was actually quite warm and the late 20th century was unexceptional. Briffa and Schweingruber never published those data, instead they dropped the Polar Urals altogether from their climate reconstruction papers. In its place they used a new series that Briffa had calculated from tree ring data from the nearby Yamal Peninsula that had a pronounced Hockey Stick shape: relatively flat for 900 years then sharply rising in the 20th century. This Yamal series was a composite of an undisclosed number of individual tree cores. In order to check the steps involved in producing the composite, it would be necessary to have the individual tree ring measurements themselves. But Briffa didn't release his raw data. Over the next nine years, at least one paper per year appeared in prominent journals using Briffa's Yamal composite to support a hockey stick-like result. The IPCC relied on these studies to defend the Hockey Stick view, and since it had appointed Briffa himself to be the IPCC Lead Author for this topic, there was no chance it would question the Yamal data. Despite the fact that these papers appeared in top journals like Nature and Science, none of the journal reviewers or editors ever required Briffa to release his Yamal data. Steve McIntyre's repeated requests for them to uphold their own data disclosure rules were ignored. Then in 2008 Briffa, Schweingruber and some colleagues published a paper using the Yamal series (again) in a journal called the Philosophical Transactions of the Royal Society, which has very strict data-sharing rules. Steve sent in his customary request for the data, and this time an editor stepped up to the plate, ordering the authors to release their data. A short while ago the data appeared on the Internet. Steve could finally begin to unpack the Yamal composite. It turns out that many of the samples were taken from dead (partially fossilized) trees and they have no particular trend. The sharp uptrend in the late 20th century came from cores of 10 living trees alive as of 1990, and five living trees alive as of 1995. Based on scientific standards, this is too small a sample on which to produce a publication-grade proxy composite. The 18th and 19th century portion of the sample, for instance, contains at least 30 trees per year. But that portion doesn't show a warming spike. The only segment that does is the late 20th century, where the sample size collapses. Once again a dramatic hockey stick shape turns out to depend on the least

reliable portion of a dataset. But an even more disquieting discovery soon came to light. Steve searched a paleoclimate data archive to see if there were other tree ring cores from at or near the Yamal site that could have been used to increase the sample size. He quickly found a large set of 34 up-to-date core samples, taken from living trees in Yamal by none other than Schweingruber himself! Had these been added to Briffa's small group the 20th century would simply be flat. It would appear completely unexceptional compared to the rest of the millennium. Combining data from different samples would not have been an unusual step. Briffa added data from another Schweingruber site to a different composite, from the Taimyr Peninsula. The additional data were gathered more than 400 km away from the primary site. And in that case the primary site had three or four times as many cores to begin with as the Yamal site. why did he not fill out the Yamal data with the readily-available data from his own coauthor? why did Briffa seek out additional data for the already well-represented Taimyr site and not for the inadequate Yamal site? Thus the key ingredient in most of the studies that have been invoked to support the Hockey Stick, namely the Briffa Yamal series, depends on the influence of a woefully thin subsample of trees and the exclusion of readily-available data for the same area. whatever is going on here, it is not science. I have been probing the arguments for global warming for well over a decade. In collaboration with a lot of excellent coauthors I have consistently found that when the layers get peeled back, what lies at the core is either flawed, misleading or simply non-existent. The surface temperature data is a contaminated mess with a significant warm bias, and as I have detailed elsewhere the IPCC fabricated evidence in its 2007 report to cover up the problem. Climate models are in gross disagreement with observations, and the discrepancy is growing with each passing year. The often-hyped claim that the modern climate has departed from natural variability depended on flawed statistical methods and low-quality data. The IPCC review process, of which I was a member last time, is nothing at all like what the public has been told: Conflicts of interest are endemic, critical evidence is systematically ignored and there are no effective checks and balances against bias or distortion. I get exasperated with fellow academics, and others who ought to know better, who pile on to the supposed global warming consensus without bothering to investigate any of the glaring scientific discrepancies and

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procedural flaws. Over the coming few years, as the costs of global warming policies mount and the evidence of a crisis continues to collapse, perhaps it will become socially permissible for people to start thinking for themselves again. In the meantime I am grateful for those few independent thinkers, like Steve McIntyre, who continue to ask the right questions and insist on scientific standards of openness and transparency.

Ross McKittrick is a professor of environmental economics at the University of Guelph, and coauthor of Taken By Storm: The Troubled Science, Policy and Politics of Global Warming.

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EDITOR'S NOTE: More on the CRU's Yamal scandal and its impact, see: <[34]<http://www.climateaudit.org/>>

<[35]<http://wattsupwiththat.com/2009/10/01/response-from-briffa-on-the-yamal-tree-ring-affair-plus-rebuttal/>>

<[36]<http://bishophill.squarespace.com/blog/2009/9/29/the-yamal-implosion.html>>

<[37]<http://bishophill.squarespace.com/blog/2009/10/1/yamal-the-debate-continues.html>>

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(3) OPINION: CLIMATE DATA BUSTER
National Post, 1 October 2009

<[38]<http://network.nationalpost.com/np/blogs/fullcomment/archive/2009/10/01/terence-corcoran-climate-data-buster.aspx>>

By Terence Corcoran

The official United Nation's global warming agency, the Intergovernmental Panel on Climate Change, is a four-legged stool that

is fast losing its legs. To carry the message of man-made global warming theory to the world, the IPCC has depended on 1) computer models, 2) data collection, 3) long-range temperature forecasting and 4)

communication. None of these efforts are sitting on firm ground.

Over the past month, one of the IPCC's top climate scientists, Mojib Latif, attempted to explain that even if global temperatures were to cool over the next 10 to 20 years, that would not mean that man-made global warming is no longer catastrophic. It was a tough case to make,

and it is not clear Mr. Latif succeeded. In a presentation to a world

climate conference in early September, Mr. Latif rambled somewhat and

veered off into inscrutable language that is now embedded in a million

blog posts attempting to prove one thing or another.

A sample: "It may well happen that you enter a decade, or maybe even two, you know, when the temperature cools, all right, relative to the

present level...And then, you know, I know what's going to happen. You

know, I will get, you know, millions of phone calls, you know - 'what's

going on?' 'So is global warming disappearing, you know?' 'Have you lied

on us, you know?' So, and, therefore, this is the reason why we need to

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address this decadal prediction issue." The decadal prediction issue appears to be a combination of computer model problems, the unpredictability of natural climate variation, and assorted uncertainties. Making all this clear to the average global citizen will not be easy and climate scientists need to be able to make it clear, said Mr. Latif. "We have to ask the nasty questions ourselves, all right, or some other people will do it." All this is still swirling around the global climate issue today. But now along comes another problem. Canadian data buster Steve McIntyre has spend most of the last three years deconstructing the IPCC's famous claim that the last couple of decades of the 20th century were the hottest in a thousand years. Using what was called The Hockey Stick graph, the IPCC claimed to have the smoking gun that showed a sharp run up in global temperatures through to 1997. The validity of the IPCC data began to crumble when Mr. McIntyre and Ross Mckitrick of Guelph University found serious data problems that raised doubts about the graph and the claims of record high temperatures. As Ross Mckitrick explains in his op-ed, Steve McIntyre has uncovered another data distortion that further undermines the original graphic claim that the world has set temperature records in recent years. If world temperatures may have been just as hot in the past as they have been recently, and if the the next two decades could be cooler than they have been recently, the theory of climate change becomes an even tougher case to make. The IPCC is now on wobbly legs at all four corners. Its models are inadequate and need overhaul, data integrity is at issue, the climate is not quite following the script, and the communication program for the whole campaign is a growing struggle. Copyright 2009, NP

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(4) OPINION: COOLING DOWN THE CASSANDRAS
The Washington Post, 1 October 2009

<[39]http://www.washingtonpost.com/wp-dyn/content/article/2009/09/30/AR2009093003569.html>

By George F. Will
"Plateau in Temperatures Adds Difficulty to Task Of Reaching a Solution"
--New York Times, Sept. 23
In this headline on a New York Times story about the difficulties confronting people alarmed about global warming, note the word "plateau." It dismisses the unpleasant -- to some people -- fact that global warming is maddeningly (to the same people) slow to vindicate their apocalyptic warnings about it. The "difficulty" -- the "intricate challenge," the Times says -- is "building momentum" for carbon reduction "when global temperatures have been relatively stable for a decade and may even drop in the next few

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years." That was in the Times's first paragraph. In the fifth paragraph, a "few years" became "the next decade or so," according to Mojib Latif, a German "prize-winning climate and ocean scientist" who campaigns constantly to promote policies combating global warming. Actually, Latif has said he anticipates "maybe even two" decades in which temperatures cool. But stay with the Times's "decade or so."

By asserting that the absence of significant warming since 1998 is a mere "plateau," not warming's apogee, the Times assures readers who are

alarmed about climate change that the paper knows the future and that

warming will continue: Do not despair, bad news will resume.

The Times reported that "scientists" -- all of them? -- say the 11 years

of temperature stability has "no bearing," none, on long-term warming.

Some scientists say "cool stretches are inevitable." Others say there

may be growth of Arctic sea ice, but the growth will be "temporary."

According to the Times, however, "scientists" say that "trying to communicate such scientific nuances to the public -- and to policymakers

-- can be frustrating."

The Times says "a short-term trend gives ammunition to skeptics of climate change." Actually, what makes skeptics skeptical is the accumulating evidence that theories predicting catastrophe from man-made

climate change are impervious to evidence. The theories are unfalsifiable, at least in the "short run." And the "short run" is defined as however many decades must pass until the evidence begins to

fit the hypotheses.

The Post recently reported the theory of a University of Virginia professor emeritus who thinks that, many millennia ago, primitive agriculture -- burning forests, creating methane-emitting rice paddies,

etc. -- produced enough greenhouse gases to warm the planet at least a

degree. The theory is interesting. Even more interesting is the reaction

to it by people such as the Columbia University professor who says it

makes him "really upset" because it might encourage opponents of legislation combating global warming.

Warnings about cataclysmic warming increase in stridency as evidence of

warming becomes more elusive. A recent report from the United Nations

Environment Program predicts an enormous 6.3 degrees Fahrenheit increase

by the end of the century even if nations fulfill their most ambitious

pledges concerning reduction of carbon emissions. The U.S. goal is an 80

percent reduction by 2050. But Steven Hayward of the American Enterprise

Institute says that would require reducing greenhouse gas emissions to

the 1910 level. On a per capita basis, it would mean emissions

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approximately equal to those in 1875.
That will not happen. So, we are doomed. So, why try?
America needs a national commission appointed to assess the evidence
about climate change. Alarmists will fight this because the first
casualty would be the carefully cultivated and media-reinforced
myth of
consensus -- the bald assertion that no reputable scientist
doubts the
gravity of the crisis, doubts being conclusive evidence of
disreputable
motives or intellectual qualifications. The president, however,
could
support such a commission because he is sure "there's finally
widespread
recognition of the urgency of the challenge before us." So he
announced
last week at the U.N. climate change summit, where he said the
threat is
so "serious" and "urgent" that unless all nations act "boldly,
swiftly
and together" -- "time . . . is running out" -- we risk
"irreversible
catastrophe." Prince Charles agrees. In March, seven months ago,
he said
humanity had 100 months -- until July 2017 -- to prevent
"catastrophic
climate change and the unimaginable horrors that this would bring."
Evidently humanity will prevent this.
Charles Moore of the Spectator notes that in July, the prince
said that
by 2050 the planet will be imperiled by the existence of 9 billion
people, a large portion of them consuming as much as western
people now
do. Environmental Cassandras must be careful with their
predictions lest
they commit what climate alarmists consider the unpardonable
faux pas of
denying that the world is coming to an end.
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(5) U.S. THROWS SPANNER INTO CLIMATE TALKS
Times of India, 2 October 2009

<[40]<http://timesofindia.indiatimes.com/news/environment/global-warming/US-trows-spanner-into-climate-talks/articleshow/5079332.cms>>

Nitin Sethi, TNN

NEW DELHI: The promise of a deal at Copenhagen seem to be
turning into a
pipedream as the US has refused to put down hard numbers for
mitigation
under the second phase of Kyoto Protocol at the ongoing climate
negotiations at Bangkok. EU too seems to be taking a deal-breaking
condition saying, "environmental integrity" was central to the
UN treaty
and "equity" of different countries' rights was just one element.
The negotiations at various levels seem to be grinding into a logjam
with US determined not to sign on to the Kyoto Protocol. The US
negotiators fought hard at different forums within the UN talks
to block
any progress on industrialized countries' commitments to reduce
emissions in the mid-term under the second phase of Kyoto Protocol.
India stood steadfast in demanding that the rich countries put
up their

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offers in terms of hard numbers for emission reductions over 2012-2020 under the existing protocol. But, US and many other developed countries seemed determined to do away with the Kyoto Protocol entirely. This is not the first time that US has voiced its opposition to the Kyoto Protocol which demands quantified targets from rich countries. US had not signed on to Kyoto earlier and it continues to oppose the only tool the global treaty has for making measurable and comparable reductions in the dangerous greenhouse gases. The protocol is also seen by a select band of industrialized countries such as US and Japan as a wall of differentiation constructed in the convention. The parent treaty -- UN Framework Convention on Climate Change -- lays most of the burden of mitigation on the industrialized countries that caused it in the first place. The Kyoto Protocol activates this principle of burden sharing into hard actions and targets. The protocol in its first phase sets fixed percentages by which countries reduce their emissions by 2012 below 1990 levels. Many of the industrialized countries have not moved on a trajectory to achieve the targets for 2012. Part of the discussions in the UN talks have been to set a higher level targets for the second phase of Kyoto Protocol between 2012-2020. But the US, not keen to take on any commitments in the mid-term, has always shown interest in disbanding with Kyoto Protocol and instead taking on a series of actions that are decided by countries on their own -- say energy efficiency targets -- and merely presented to the UN forum. India and developing countries have pointed out that would make the targets incomparable and render it impossible to figure out if any significant reductions have been made in emissions to prevent a climate calamity. Other industrialized countries too have so far shown little interest in offering credible and robust targets for the second phase of the protocol. The offers so far on the table from the industrialized countries, if implemented, would only bring in reductions in the range of 11-18% by 2020 below 1990 levels. India and other developing countries have demanded that the industrialized countries follow the recommendations of the UN climate science panel -- IPCC -- and take cuts in the range of 25-40% below 1990 levels by 2020 which would put the world on a trajectory to avoid temperatures reaching dangerous levels in the decades to come.

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(6) CAP AND TRADE MAY SINK OPPOSITION LEADER DOWN UNDER
The Australian, 2 October 2009

<[41]http://www.theaustralian.news.com.au/story/0,25197,26153820-2702,00.htm>
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Lenore Taylor, National correspondent | October 02, 2009
MALCOLM Turnbull is on a collision course with his own back bench after staking his leadership on a demand that they back his climate change strategy. Several MPs immediately refused to do so. If the partyroom refused to back his strategy of negotiating amendments to the government's emissions trading scheme, Mr Turnbull said yesterday, the Coalition would "literally be a party with nothing to say ... a party with no ideas", and that was "not the party I am prepared to lead".
Throwing down the gauntlet to his internal critics, Mr Turnbull said: "I am asserting my authority as the leader of the Liberal Party and the Leader of the Opposition."
"If the partyroom were to reject my recommendation to them, that would obviously be a leadership issue. That's perfectly plain, perfectly clear," he told ABC Radio in Adelaide.
"I could not possibly lead a party that was on a do-nothing-on-climate-change platform."
His critics were not cowed, despite the fact that both mooted leadership alternatives -- Joe Hockey and Tony Abbott -- support Mr Turnbull's stance.
West Australian backbencher Wilson Tuckey said: "Mr Turnbull has made the ETS a leadership issue and we will now treat it as such." His leader's ultimatum did not alter his "total opposition to an ETS and to the suggestion that we might amend it".
Victorian Liberal senator Julian McGauran said he stood by his vow to vote against the ETS in November, no matter what amendments were negotiated.
Nationals senators also remain implacably opposed to the scheme. "He hasn't got the partyroom with him on this one ... we are going to stand up for what we believe in," said senator Ron Boswell.
"This is not just another issue. This is not one we can let go through to the keeper," said senator Barnaby Joyce.
Mr Tuckey appeared to suggest Mr Turnbull's deputy, Julie Bishop, as an alternative leader, saying there were "many good potential leaders in the Liberal Party ... and perhaps some people who have had their reputations tarnished by backgrounding from our side now deserve reconsideration for the top job".
FULL STORY at

<[42]http://www.theaustralian.news.com.au/story/0,25197,26153820-2702,00.htm>
1>

===== e-mails to the editor =====

(7) THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

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Leading UK Climate Scientists Must Explain or Resign, Jennifer Marohasy

mail.2009

<[46]<http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-must-explain-or-resign/>>

MOST scientific sceptics have been dismissive of the various reconstructions of temperature which suggest 1998 is the warmest year of the past millennium. Our case has been significantly bolstered over the last week with statistician Steve McIntyre finally getting access to data used by Keith Briffa, Tim Osborn and Phil Jones to support the idea that there has been an unprecedented upswing in temperatures over the last hundred years - the infamous hockey stick graph. Mr McIntyre's analysis of the data - which he had been asking for since 2003 - suggests that scientists at the Climate Research Unit of the United Kingdom's Bureau of Meteorology have been using only a small subset of the available data to make their claims that recent years have been the hottest of the last millennium. When the entire data set is used, Mr McIntyre claims that the hockey stick shape disappears completely. [1] Mr McIntyre has previously showed problems with the mathematics behind the 'hockey stick'. But scientists at the Climate Research Centre, in particular Dr Briffa, have continuously republished claiming the upswing in temperatures over the last 100 years is real and not an artifact of the methodology used - as claimed by Mr McIntyre. However, these same scientists have denied Mr McIntyre access to all the data. Recently they were forced to make more data available to Mr McIntyre after they published in the Philosophical Transactions of the Royal Society - a journal which unlike Nature and Science has strict policies on data archiving which it enforces. This week's claims by Steve McIntyre that scientists associated with the UK Meteorology Bureau have been less than diligent are serious and suggest some of the most defended building blocks of the case for anthropogenic global warming are based on the indefensible when the methodology is laid bare. This sorry saga also raises issues associated with how data is archived at the UK Meteorological Bureau with in complete data sets that spuriously support the case for global warming being promoted while complete data sets are kept hidden from the public - including from scientific sceptics like Steve McIntyre. It is indeed time leading scientists at the Climate Research Centre associated with the UK Meteorological Bureau explain how Mr McIntyre is in error or resign.

[1] Yamal: A "Divergence" Problem, by Steve McIntyre, 27 September 2009

[47]<http://www.climateaudit.org/?p=7168>
Jennifer Marohasy BSc PhD

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(8) COOLING?

Rodney Chilton <maberrd@hotmail.com <<mailto:maberrd@hotmail.com>>

mail.2009

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Dear Benny:

Recently, there has been considerable discussion concerning the slight

cooling of the earth's overall climate since about 2005. The result of

the cooling has brought some scientists into the forefront to be openly

critical of the still prominent view that climate changes over the century or so have predominately been man caused. The proponents of human initiated climate changes are of the opinion that the recent cooling is but a temporary interruption in what soon again will be a rapid climate warming.

I think one of the keys to alleviate some of this discussion is to attempt to determine the triggers for two other climate shifts in earlier times. The first of these, the "Little Ice Age" is generally regarded by most scientists as resulting from a reduced output

of energy

from the sun. Coinciding as it did with an interval of very

little to

almost no sunspot activity, a time known as the "Maunder

Minimum", many

solar scientists suggest that as little as 0.25% decrease in solar output initiated this cold climate period. Similarly, during

the mid

20th Century during the years from the end of the 1940's to

about the

mid 1970's, the sun was in one of its quiet modes (very few

sunspots).

The cause for what was a slightly cooler interval could logically be linked to decreased energy from the sun. However, the quite recent thirty year period is more commonly linked to increased dust in the earth's atmosphere. Consistent with this view is the idea that perhaps

the Little Ice Age too, was forced not by a decrease in the sun's output, but by an increase in dust, not that produced by man, but by extraterrestrial dust from a comet encounter. More details of this particular scenario can be seen at the following website:

<[49]<http://www.bcclimate.com> <[50]<http://www.bcclimate.com/>>

<[51]<http://www.bcclimate.com/>>>

All of this raises the questions, what drove both the Little Ice Age and

the thirty year interval in the middle of the last century? It is

possible that they were driven by the two different causes

outlined. It

is vital I think that the reason(s) for the two climate shifts be determined. This would go along way to settle the recent debate

as to

the importance of solar minima in initiating climate changes over more

than just a few years. Further to this, the picture of the future will

be clarified. If for example, decreases in solar output is

proven to be

of less importance during the past, then surely the present climate downturn will be likely only a temporary respite from the inexorable upward trend in temperatures worldwide. If on the other hand the

solar

cycles accompanied by low sun activity over decades and even

longer can

be proven as significant, then I believe we must re-examine the increased carbon dioxide scenario.

Rodney Chilton

mail.2009

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(9) RESOURCES DEPLETION WORRIES

Steven Zoraster <szoraster@szoraster.com

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Benny,

Certainly someone with access to the hard numbers and more knowledge than I can do better proving or disproving the following argument about

the EROEI of nuclear power in the United States:

Today, 104 nuclear reactors supply 20% of the electricity used each year

in the United States. [1]They have been doing this for approximately 25

years. [2] Many existing reactors have now been approved to operate for

60 years. While the initial costs measured in energy use 25 years ago

were high and construction often took 5 years, I doubt that the construction process for all 104 reactors, required greater energy than

the equivalent of 20% of annual electricity used 25 years ago over a 5

year period. (I include the cost of design, obtaining permits, fighting

environmental lawsuits, manufacturing parts, and actual construction,

etc., in the total energy cost.)

Today the annual operating costs of maintaining, fuelling, and repairing

existing reactors are low compared to alternate sources of electricity

except hydroelectric. The nuclear waste from these reactors has been safely stored at the reactor sites without causing a single human death.

Conclusions: Assuming the generation of electric energy in the US since

about 1985 has been and will be constant, the EROEI of nuclear power using 25 year old technology is greater than 12. (Twenty percent of all

electric energy generated over 60 years divided by 20% of the same amount of pre-atomic electricity generated over 5 years.) Given that total electricity use in the US has almost doubled in the last 25 years

[3], the EROEI may be greater than 24. More modern proposed reactor designs, with greater standardization, simpler fuel cycles, fail safe

features, and increased automation, can be expected to have higher EROEI.

(I have not included the cost of decommissioning reactors. Numbers I found online are often estimates and seldom given in terms of energy.

Because fuel costs today and to be expected in the future are low, ignoring the option of recycling used fuel is not a significant factor

in my calculations.)

Steven Zoraster

[1]

[55]http://www.nei.org/resourcesandstats/nuclear_statistics/usnuclearpowerplants/

[2]

mail.2009

[56]<http://www.eia.doe.gov/cneaf/nuclear/page/analysis/nuclearpower.html>
Reactors were being completed between 1957 and 1996. The first large commercial reactors date to 1968. The longest "build time" is 24 years.
Some reactors have been closed after being built and have been ignored in my argument. My use of 25 years in these calculations is certainly a suspect approximation or average.
[3] [57]<http://www.eia.doe.gov/emeu/aer/eh/frame.html> (Then click on "Electricity" on the left side of the page.)

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(10) COPENHAGEN SUMMIT: DO SCIENCE AND ECONOMICS SUPPORT GOVERNMENT ACTION ON GLOBAL WARMING?

Peter Kidson <peterdkidson@googlemail.com

<[58]mailto:peterdkidson@googlemail.com>

<[59]mailto:peterdkidson@googlemail.com>

<[60]mailto:peterdkidson@googlemail.com>>]

Hi Benny

You might perhaps want to publicise this public debate

<[61]http://www.iea.org.uk/record.jsp?type=event&ID=217>

<[62]http://www.iea.org.uk/record.jsp?type=event&ID=217>

<[63]http://www.iea.org.uk/record.jsp?type=event&ID=217>

<[64]http://www.iea.org.uk/record.jsp?type=event&ID=217>>>

Note that you need to reserve seats.

Regards

-Peter

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(11) A DEATH SPIRAL FOR CLIMATE ALARMISM?

Robert Bradley <rbradley@iertx.org <[65]mailto:rbradley@iertx.org>

<[66]mailto:rbradley@iertx.org <mailto:rbradley@iertx.org>>>

Ken Green's post at MasterResource today should be of interest.

<[67]http://masterresource.org/?p=5036>

Things are getting very shrill from the Climate Industry, but there is a

rethink going on starting with the physical science.

Robert L. Bradley Jr.

CEO & Founder, Institute for Energy Research

Houston, Texas 77057-3527

IER website: [68]www.energyrealism.org

<[69]http://www.energyrealism.org/> <[70]http://www.energyrealism.org/>

Political Capitalism website: [71]www.politicalcapitalism.org

<[72]http://www.politicalcapitalism.org/>

<[73]http://www.politicalcapitalism.org/>

Energy Blog: [74]www.MasterResource.org

<[75]http://www.masterresource.org/>

<[76]http://www.masterresource.org/>

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(12) AND FINALLY: 'PLANNED RECESSION' COULD AVOID CATASTROPHIC CLIMATE CHANGE

The Daily Telegraph, 30 September 2009

<[77]http://www.telegraph.co.uk/earth/earthnews/6248257/Planned-recession-could-avoid-catastrophic-climate-change.html>

By Louise Gray, Environment Correspondent

Britain will have to stop building airports, switch to electric cars and

shut down coal-fired power stations as part of a 'planned recession' to

avoid dangerous climate change.

At the moment the UK is committed to cutting greenhouse gases by a third

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by 2020.

However a new report from the Tyndall Centre for Climate Change Research said these targets are inadequate to keep global warming below two degrees C above pre-industrial levels.

The report says the only way to avoid going beyond the dangerous tipping

point is to double the target to 70 per cent by 2020.

This would mean reducing the size of the economy through a "planned recession".

Kevin Anderson, director of the research body, said the building of new

airports, petrol cars and dirty coal-fired power stations will have to

be halted in the UK until new technology provides an alternative to burning fossil fuels.

"To meet [Government] targets of not exceeding two degrees C, there would have to be a moratorium on airport expansion, stringent measures

on the type of vehicle being used and a rapid transition to low carbon

technology," he said.

Prof Anderson also said individuals will have to consume less.

"For most of the population it would mean fairly modest changes to how

they live, maybe they will drive less, share a car to work or take more

holidays in Britain."

More than 190 countries are due to meet in Copenhagen in December to decide a new international deal on climate change.

Speaking at an Oxford University conference on the threat of climate change, Prof. Anderson said rich countries will have to make

much more ambitious cuts to have any chance of keeping temperature rise below four

degrees C.

"If we do everything we can do then we might have a chance," he said.

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CCNet is a science policy network edited by Benny Peiser. To subscribe,

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<[78]mailto:listserver@ljmu.ac.uk> <[79]mailto:listserver@ljmu.ac.uk>
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-- David W. Schnare

Center for Environmental Stewardship

-- David W. Schnare

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For more options, visit this group at

[85]<http://groups.google.com/group/geoengineering?hl=en>

Prof. Phil Jones

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3. <http://www.germgardlighting.com/>
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5. <mailto:geoengineering@googlegroups.com>
6. <mailto:geoengineering@googlegroups.com>
7. <mailto:geoengineering@googlegroups.com>
8. <mailto:euggordon@comcast.net>
9. <mailto:euggordon@comcast.net%20%3Cmailto:euggordon@comcast.net>
10. <mailto:adwhite99@comcast.net>
11. <mailto:adwhite99@comcast.net>
12. <mailto:adwhite99@comcast.net>
13. <mailto:B.J.Peiser@ljmu.ac.uk%3E%A0%A0%A0%A0%A0%A0%A0%A0>
14. <mailto:B.J.Peiser@ljmu.ac.uk>
15. <mailto:CCNetMedia@livjm.ac.uk%3E%A0%A0%A0%A0%A0%A0%A0%A0>
16. <mailto:CCNetMedia@livjm.ac.uk>
17. <mailto:jennifermarohasy@jennifermarohasy.com>
18. <mailto:jennifermarohasy@jennifermarohasy.com>
19. <mailto:jennifermarohasy@jennifermarohasy.com>
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32. http://www.theregister.co.uk/2009/09/29/yamal_scandal/
33. <http://network.nationalpost.com/np/blogs/fpcomment/archive/2009/10/01/r%3E%A0%A0%A0%A0%A0%A0%A0%A0>
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35. <http://wattsupwiththat.com/2009/10/01/response-from-briffa-on-the-yamal%3E%A0%A0%A0%A0%A0%A0%A0%A0>
36. <http://bishophill.squarespace.com/blog/2009/9/29/the-yamal-implosion.ht>
- 37.

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From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: thanks and one question
Date: Mon Oct 5 10:55:36 2009
Cc: Michael Mann <mann@meteo.psu.edu>

Gavin, Mike,

Thanks for this!

I assume you are both aware of this prat - Neil Craig, see below. Keith won't be responding.

Checking facts doesn't seem important these days. As CA threads aren't publications this is difficult for non scientists.

I am going further over one email I got at the weekend - see also below. Typical of Sonia

- although she now seems to only be an emeritus reader!

Cheers

Phil

Return-path: <CrgN143@aol.com>

From: CrgN143@aol.com

Full-name: CrgN143

Message-ID: <d03.64b01875.37f87aa4@aol.com>

Date: Sat, 3 Oct 2009 06:00:04 EDT

Subject: Tree rings - accusation that you were solely responsible.

To: k.briffa@uea.ac.uk

MIME-Version: 1.0

Content-Type: multipart/alternative;

boundary="-----1254564004"

X-Mailer: 9.0 SE for windows sub 5045

Professor Briffa,

I have written a couple of blogs on the current report by Steve McIntyre that the data used by Mann to "prove" the hockey stick was fabricated. This & the following day's

[1]<http://a-place-to-stand.blogspot.com/2009/10/global-warming-proven-deliberate-fraud.html>

As a result I have received this email from somebody I am not acquainted with throwing the entire blame on you. This seems improbable to me & possibly an alarmist damage limitation exercise. If you wish to comment I would be happy for you to do so.

"Please note: Steve McIntyre's post concerns work by climate scientist Keith Briffa and not Michael Mann. You will probably wish to correct your post.

Cheers

Avisame"

I have posted this as an update with my reply:

"My understanding is that while Briffa did the tree ring measurement, Mann, in his paper, chose to choose 12 atypical tree rings out of at least 34 to fabricate the global warming trend. My assumption is that Mann is responsible for fabrications in his own paper & that this is a damage limitation exercise. I am open to correction on this & indeed have emailed

mail.2009

Mr Briffa to see. "

Neil Craig

You may be interested in my political blog

[2]<http://a-place-to-stand.blogspot.com/>

We received this through our enquiries desk. I assume that you are aware of this person, including those copied on the message.

If we are to respond, it would be to indicate that there are multiple sources of supporting evidence and that we continue to place our confidence in the international scientific assessment process. This confidence has proven to be well placed.

Roger

From: Sonja A Boehmer-Christiansen <Sonja.B-C@hull.ac.uk>

Date: 2 October 2009 18:09:39 GMT+01:00

To: Stephanie Ferguson <stephanie.ferguson@ukcip.org.uk>

Cc: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>, Patrick David Henderson <pdhenderson18@googlemail.com>, Christopher Monckton <monckton@mail.com>

Subject: RE: Please take note of potetially serious allegations of

scientific

'fraud' by CRU and Met Office

Dear Stephanie

I expect that a great deal of UKCIP work is based on the data provided by CRU (as does the work of the IPCC and of course UK climate policy). Some of this, very fundamentally, would now seem to be open to scientific challenge, and may even face future legal enquiries. It may be in the interest of UKCIP to inform itself in good time and become a little more 'uncertain' about its policy advice.

Perhaps you can comment on the following and pass the allegations made on to the relevant people.

It is beyond my expertise to assess the claims made, but they would fit into my perception of the whole 'man-made global warming' cum energy policy debate. I know several of the people involved personally and have no reason to doubt their sincerity and honour as scientists, though I am also aware of their highly critical (of IPCC science) policy positions.

I could also let you have statements by Steve McIntyre and Ross Mckitrick. Ross

Mckitrick currently teaches at Westminster Business School and who is fully informed about

the relevant issues. He recently addressed a meeting of about 50 people in London.

Best wishes

Sonja B-C

Dr.Sonja A.Boehmer-Christiansen

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mail.2009
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Multi-Science ([3]www.multi-science.co.uk)
HULL HU6 7RX
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Fax: (0044) 1482 466340

TWO copied pieces follow, both relate to CRU and UK climate policy

a. THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

" Jennifer Marohasy <jennifermarohasy@jennifermarohasy.com>

Leading UK Climate Scientists Must Explain or Resign, Jennifer Marohasy
<

<[4]http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists->
[5]http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-
must-explain-or-resign/>

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2. <http://a-place-to-stand.blogspot.com/>
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4. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>
5. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>

1039. 1254756944.txt

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From: Phil Jones <p.jones@uea.ac.uk>
To: k.briffa@uea.ac.uk
Subject: Message from Tom Wigley
Date: Mon Oct 5 11:35:44 2009

Keith,

Here's a message from Tom. It might be worth sending anything you've got to him to have a look through. Shorter responses are probably better. Detail can go in a poster.

Pointing out how often or not Yamal is used is useful. I don't think they have done this. I think many people confuse this with the polar urals chronology. That is different and it is based on density.

M&M rely on people not checking.

Cheers
Phil

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Date: Mon, 05 Oct 2009 03:57:57 -0600
From: Tom Wigley <wigley@ucar.edu>
User-Agent: Thunderbird 2.0.0.14 (windows/20080421)
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [geo] Re: CCNet: A Scientific Scandal Unfolds
X-Canit-CHI2: 0.00
X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)
X-Spam-Score: 0.30 () [Hold at 5.00] PORN_RP_NASTY,SPF(none,0)
X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
X-Canit-Stats-ID: 32219749 - e7f62debf1d6
X-Antispam-Training-Forget:
[1]<https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=f>
X-Antispam-Training-Nonspam:
[2]<https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=n>
X-Antispam-Training-Spam:
[3]<https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=s>
X-Scanned-By: CanIt (www.roaringpenguin.com) on 139.222.131.184

Phil,

It is distressing to read that American Stinker item. But Keith does seem to have got himself into a mess. As I pointed out in emails, Yamal is insignificant. And you say that (contrary to what M&M say) Yamal is *not* used in MBH, etc. So these facts alone are enough to shoot down M&M in a few sentences (which surely is the only way to go -- complex and wordy responses will be counter productive).

But, more generally, (even if it *is* irrelevant) how does Keith explain the McIntyre plot that compares Yamal-12 with Yamal-all? And how does he explain the apparent "selection" of the less well-replicated chronology rather than the later (better replicated) chronology?

Of course, I don't know how often Yamal-12 has really been used in recent, post-1995, work. I suspect from what you say it is much less often than M&M say -- but where did they get their information? I presume they went thru papers to see if Yamal was cited, a pretty foolproof

method if

you ask me. Perhaps these things can be explained clearly and concisely -- but

I am not

sure Keith is able to do this

as he is too close to the issue and probably quite pissed of.

And the issue of with-holding data is still a hot potato, one that affects both you and Keith (and Mann). Yes, there are reasons -- but many *good* scientists appear to be unsympathetic to these. The trouble here is that with-holding data looks like hiding something, and hiding means (in some eyes) that it is bogus science that is being hidden.

I think Keith needs to be very, very careful in how he handles this. I'd be willing to check over anything he puts together.

Tom.

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University of East Anglia
Norwich Email p.jones@uea.ac.uk
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3. <https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=s>

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1040. 1254760537.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <>wigley@ucar.edu>
Subject: Re: NCDC data
Date: Mon Oct 5 12:35:37 2009
Cc: Ben Santer <santer1@llnl.gov>

Tom,

I can't see why the data become ERSSTv3b. b seems all that you can download.

I reviewed the 2008 paper. The version that I reviewed had something in for the problem of

SST data now re drifters and ships, but they pulled that section. I recall saying it needed

to be watertight and they needed to explain the spatial pattern to the ship minus drifter

field. Maybe that version was a?

I was never that keen on their infilling. It biases the values before the 1920s when you

infill with anomalies that are nearer to zero. You can see this in their Fig6.

This version

is better than their previous one.

I always assumed they still had gaps - as it would be impossible to infill the

Antarctic and some parts of the Southern Oceans. Have you tried looking at their Antarctic

average - 65-90s for example?

Their globe should be one domain, so not $(NH+SH)/2$ but for an infilled dataset this

shouldn't make any difference.

I wonder if they downweight the infilled values in some way? They have their error

field?

The 2008 paper doesn't say how they compute Global and NH and SH. Are NH and SH the same

as you get?

Cheers

Phil

At 06:56 05/10/2009, Tom Wigley wrote:

Phil, Ben,

Have you looked at the latest NCDC global data? It seems odd.

The data on their site is ERSSTv3 (Smith et al. 2008). As far as I know, this is an infilled data set with no gaps. As such, $(NH+SH)/2$ should be the same as their global mean. For monthly data, this is not the case. There are actually some big differences, even recently.

Any idea why?

Tom.

Prof. Phil Jones

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#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Andrew Manning <a.manning@uea.ac.uk>
Subject: Re: Fwd: Co2 Data
Date: Tue Oct 6 08:38:04 2009

Andrew,

Getting a bit fed up with these baseless allegations.

You could point out several things to Martin.

1. Projections aren't made with observed data - instrumental or paleo. They are made with climate models.

2. The initial seed for all these allegations is made on Climate Audit. Here they are quite clever and don't go over the top. They leave it to others like the National Review,

the American Thinker to make the ridiculous ones.

Here is what Stephen McIntyre says on Climate Audit.

"While there is much to criticise in the handling of this data by the authors and the journals, the results do not in any way show that 'AGW is a fraud' nor that this particular study was a 'fraud'.

McIntyre has no interest in publishing his results in the peer-review literature. IPCC

won't be able to assess any of it unless he does.

You dad and Susan Solomon have had runs in with him and others

3. You might like to send him this pdf and its Figure 2. Three different groups get much the same result.

Here are the two web pages we have put up so far. Keith is working on the tree one and

put much more later in the week.

[1]<http://www.cru.uea.ac.uk/cru/data/availability/>

So other groups around the world have also entered into agreements. I know this doesn't

make it right, but it is the way of the world with both instrumental and paleo data. I

frequently try and get data from other people without success, sometimes from people who

send me the pdf of their paper then tell me they can't send me the series in their plots.

[2]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2000/>

It is the right wing web sites doing all this, presumably in the build up to Copenhagen.

At 00:13 06/10/2009, Andrew Manning wrote:

Hi Phil,

is this another witch hunt (like Mann et al.)? How should I respond to the below? (I'm

in the process of trying to persuade Siemens Corp. (a company with half a million

employees in 190 countries!) to donate me a little cash to do some CO2 measurements here

in the UK - looking promising, so the last thing I need is news articles calling into

question (again) observed temperature increases - I thought we'd moved the debate beyond

this, but seems that these sceptics are real die-hards!!).

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mail.2009

Kind regards,
Andrew

Date: Mon, 5 Oct 2009 15:50:38 +0100
Subject: Co2 Data
From: Martin Lutyens <martinlutyens@googlemail.com>
To: Andrew Manning <a.manning@uea.ac.uk>

Dear Andrew,

I just came across an article in The Week, called "The case of the vanishing data". It writes in a rather wry and sceptical way about your UEA colleagues Phil Jones and Tom Wigley, saying that only their "homogenised" or "adjusted" historical data is available, and the original, raw data has gone missing. Apparently some other environmental gurus now want to look at the original data and were "fobbed off".

According to the article, the adjusted data forms the basis for much of the climate change debate and, because others now want to look at the source data, it is "at the centre of an academic spat that could have major implications for the climate change debate". The author of the original article is Patrick Michaels in The National Review, who may just be stirring it.

The article concludes "In short, the data invoked to verify the most significant forecasts about the world's future, have simply vanished." Could you comment on this please, as someone (eg Siemens Corp.) may pick this up and I think we should all be forearmed by knowing what really happened and what to say if asked.

Many thanks, Martin

--
Martin Lutyens
+44 (0) 207 938 2387
+44 (0) 796 646 2661

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
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1042. 1254850534.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>, Ben Santer <santer1@llnl.gov>
Subject: Re: help please
Date: Tue Oct 6 13:35:34 2009

Tom,

mail.2009

Agreed that NCDC must have some data gaps - but this isn't very clear from the web site.

GISS is inferior - not just because it doesn't use back data. They also impose some urbanization adjustment which is based on population/night lights which I don't think is very good. Their gridding also smooths things out. Plotting all three together for land only though they look similar at decadal timescales. GISS does have less year-to-year variability - when I last looked.

I assume NCDC should add the back data in - although there isn't the need if infilling is going on OK.

I've never looked to see if NCDC changes from year to year.

I think you can say that GISS is inferior to CRUTEM3. In Ch 3 of AR4 I put the station number counts in.

GISS and NCDC have more, but almost all of this is more data in the US. Their non-use of a base period (GISS using something very odd and NCDC first differences) means they can use very short series that we can't (as they don't have base periods) but with short series it is impossible to assess for homogeneity. So some of their extra series may be very short ones as well. As you know the more important thing is where the stations are (and in time).

The paper I sent you by Adrian Simmons shows great agreement with CRUTEM3 when subsampled according to CRU grid boxes. Also shows that ERA-INTERIM is very good. ERA-INTERIM's absolute is also within 0.2 deg C of the CRU 14 deg C value. It would give about 13.8 for 1961-90. Sometime I should write this up as more and more people seem to be using 15 deg C.

Away from tomorrow till next Tuesday.

Cheers

Phil

At 23:23 05/10/2009, Tom Wigley wrote:

Phil,

Thanks again.

Re ENSO/volcs, it was me who did this first ...

Wigley, T.M.L., 2000: ENSO, volcanoes and record breaking temperatures. Geophysical Research Letters 27, 41014104.

Then in a paper with Ben (with you as a co-author) ...

Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen, J.E., Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta, S. and Taylor K.E., 2001: Accounting for the effects of volcanoes and ENSO in comparisons of modeled and observed temperature trends. Journal of Geophysical Research 106, 2803328059.

I think my iterative method is better than Thompson's method. He has some weird volcano results. Removing the dynamic bit is not much use in my view.

So I have all these series with volc and ENSO removed (or just ENSO removed, but accounting for volcano obfuscation). I also use running approx. 20-year

regressions usually -- as you know, the ENSO-global link breaks down in the 1930s, so

using a relationship that comes from a (e.g.) 100-year regression would impose a spurious anti-ENSO signal

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on the data in the 1930s. I think this is important -- ignored by Thompson. The reason for this breakdown is obscure, but I think it is because, for some reason, the N34/SOI link (i.e., really the SST/walker circulation link) weakens in the 1930s. We need to look at this more fully in models. I also have these series for different regions of the globe. I need to revise and update these. It is tricky to get the regional volc signal because of SNR problems at the smaller spatial scale. I wrote all this up more than 10 years ago, but have not got around to finalizing it to submit for publication. (I have a number of other papers like this. Once I get done with an issue to a certain level I get sidetracked on other issues.) The amplification *does* work for warming and cooling. Theory says about +30% for TLT/surface. This works for overall variability, and for RSS trend. But oddly the ENSO and volc amplification seems to be greater than this.

I've asked Ben for his thoughts on why. Re NCDC, it seems that there *must* be data gaps. This is the only way that global can differ from (N+S)/2. It also seems that the NCDC data must be ERSST3b. But their web site is not clear on this. Perhaps Ben knows. Thanks for the GISS info. So this means that their series does not change from year to year, whereas HadCRU does (albiet by only small amounts). Does NCDC change each year? The GISS thing means that it must be inferior to HadCRU and NCDC. Should I say this in my report to EPRI?

Tom.

+++++

Phil Jones wrote:

Tom,

I don't think AR4 (Ch 3) went into the TLT/surface amplification issue. You can get the pdf of the chapter from here [1]<http://ipcc-wg1.ucar.edu/wg1/wg1-report.html> . This amplification issue is only addressed in some recent papers - mainly Ben's. The timescale argument is quite convincing. It is a pity that there is only Pinatubo that you can test it on. El Chichon ought to work but it is confused by ENSO. Does the amplification work well for the 1997/98 El Nino? Did you pick up that Thompson et al paper due out in J. Climate soon? Factoring out ENSO and volcanoes might help in isolating this. [2]<http://www.atmos.colostate.edu/faculty/thompson.php> where there is a link to the paper and also the data [3]<http://www.atmos.colostate.edu/~davet/ThompsonWallaceJonesKennedy/> It seems as though you can get all the extraction parts. No need for the dynamic bit. Anyway my thought is as Pinatubo gives the amplification then ENSO ought to as well. A thought might be to take Dave Thompson's ENSO and volcanic subtraction series, then scale them by thermodynamic theory value then subtract these from RSS and UAH. Small issue of base periods to sort out and assume there is no lag. This Need to do this with NCDC surface as well - have to use Dave T's numbers here.

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can't do the 20N-20S - just the globe.

It would of course, at this and any other time, be very nice to show that UAH is wrong.

A couple of minor things in the paper

- the amplification should work for a cooling as well - not just warming trends?

In Fig 5 in your legend LOUAH should be UAHLO. This is in Fig 4 as well.

By the way - meant to add this to the earlier email.

(NH+SH)/2 NCDC ERSST3 side does talk about missing data, so any of this would mean the

won't equal the global average that NCDC calculate.

they have a I recall you asking about GISS. One thing I have learned about GISS is that

the cut off date of the 8th of each month. After this date nothing is changed for

any back previous month and nothing earlier either. This means they never incorporate

16th of data and they don't get the second tranche of CLIMAT data which comes about the

way, plus the following month. Countries like Paraguay and Bolivia mostly come in this

some in Africa.

I'll see Tom Peterson later in the week. I'll ask him about their cut offs. I think

Tom who they don't change a month later. This won't lose you much data though. It was

told me about the data they can't use.

Cheers

Phil

At 05:25 04/10/2009, Tom wigley wrote:

Hi Phil,

I'm writing a report for EPRI where I have to discuss the instrumental temperature record. what they are particularly concerned with is/are the criticisms that have been leveled at the surface record, especially differences from MSU data.

I think CCSP 1.1 does a good job on this -- not sure about AR4 (which I need to re-check). But things have changed since CCSP 1.1 and AR4, and I think I can make a better case against UAH than either of these reports.

Could you please look at the attached and give me your opinion and comments (tracked if that makes it easier)? In my view, the evidence that the UAH data are flawed is overwhelming -- but I want to make the case in a logical and balanced way. Have I succeeded? The audience level for this is IPCC report level, perhaps a bit lower. So I need to be relatively simple, but authoritative. The MSU issue also comes up later in my report where I discuss the IJOC Santer et al. paper -- which is only mentioned briefly in the attached extract.

One thing I thought I might add is more about the other two surface data sets. A key point may be that 1998 is not the warmest year in the GISS record -- do you trust GISS? I've not looked at NOAA. Perhaps this still has 1998 as warmest? Thanks for your help.

By the way, this report was due to EPRI last week. I'm hoping to get it to them by Friday (9 Oct.)

Best wishes,

Tom

Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia

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

References

1. <http://ipcc-wg1.ucar.edu/wg1/wg1-report.html%A0>
2. <http://www.atmos.colostate.edu/faculty/thompson.php>
3. <http://www.atmos.colostate.edu/~davet/ThompsonWallaceJonesKennedy/>

1043. 1255027691.txt

#####

From: Viva Banzon <Viva.Banzon@noaa.gov>
To: Thomas.C.Peterson@noaa.gov
Subject: Re: ERSST
Date: Thu, 08 Oct 2009 14:48:11 -0400
Cc: Tom Wigley <wigley@ucar.edu>, Tom Karl <Thomas.R.Karl@noaa.gov>, Phil Jones <p.jones@uea.ac.uk>, Ben Santer <santer1@llnl.gov>, Richard.W.Reynolds@noaa.gov, Derek.Arndt@noaa.gov

Hello, everyone,
Additional info provided below.-Viva

ERSST refers only to the ocean temperature fields. Smith et al. (2008) described the updates to create ERSST version 3. This included the use of in situ and satellite data. The paper also presented updates to the Land Surface Temperature (LST) product and culminated in the computation of the Merged Land-Ocean Surface Temperature product. However, since ca. Nov 2008, satellite data was removed from the analysis, and was called v3b, but the methodology is essentially the same as in the paper. The reason was that there was a residual cold bias in the satellite data. This caused problems for users concerned with rankings. We do not handle the page for the LST and Merged ST product, and perhaps there should be more coordination among these webpages. We have noticed the confusion about the ERSST v3 and v3b in several articles, are in the process of updating the webpage. The in situ data used for the ERSSTv3b is ICOADS. The current v3b was computed using ICOADS release 2.4 (1784-2007). In July 2009, a new release was made with additional data pre-1900's and during the war years, but we have no plans yet to reprocess. It is during

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such a reprocessing that we will include any missed data. Operationally, we run the code

on the 3rd of each month using the available GTS data.

The baseline for the ERSST anomalies is 1971-2000. For the LST, the GHCN box averages are

provided to us as anomalies already, so I am not sure what the baseline is (I just started

3 months ago so I have not worked a lot on the Merged product codes yet). In the programs,

there is an adjustment of the LST anomaly to a 1971-2000 base. So the final merged ST

anomaly has a 1971-2000 base period. The best practice would be to reconstruct the

original ST by adding the 1971-2000 base. Then compare or adjust or change baselines as

you please.

BTW, my last name is BANZON, no R. Alas I am not related to the 261st richest person.

--

[NOTE: The opinions expressed in this email are those of the author alone, and do not necessarily reflect official NOAA, Department of Commerce, or US government policy.]

Patria Viva F. Banzon

Physical Scientist, Remote Sensing & Applications Division

National Climatic Data Center (NOAA-NESDIS)

151 Patton Avenue, Asheville, NC 28801-5001

(828) 271 4453 (Tel.) 828-271-4328 (FAX)

[1]Viva.Banzon@noaa.gov

[2]Thomas.C.Peterson@noaa.gov wrote:

Dear Tom,

Phil Jones, who is sitting next to me here in southern Spain and also checking email, explained what you are working on and it sounds like a potentially very insightful analysis. I wish you luck.

Viva Branson (cc'd) is our new/improved keeper of ERSST. We sometimes refer to her as Dick Reynolds version b (Dick is cc'd as well). She will be able to answer your questions more accurately than I. But if I recall correctly from talking to them Monday, to avoid confusion they are trying to only make the latest (and therefore best) version of ERSST available. So the version you downloaded should be 3b.

But Viva can verify this for you. I don't know which reference is currently the recommended one to use.

A decade ago, NCDC did a global land analysis and a global ocean

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analysis and then combined them with a weighting of 30/70. This could also arguably be the most accurate way to combine spatially incomplete data so that the world is not inappropriately weighted more towards the ocean than land (which tends to have larger gaps). Once we used Tom Smith's more spatially complete analysis, we went with a simple global average. While the data are more spatially complete, they are not complete. Data are set to missing over sea ice, much of the world north of 75N and Antarctica (Viva and I are currently reevaluating options for those last two).

ERSST is updated monthly. The SST portion is already updated for September and the land portion will wait another week or so for more data to come in. (I realize I've been assuming you are using ERSST as shorthand for NCDC's merged land/ocean data set, equivalent to HadCRU - if you're only asking about SSTs, Viva and Dick are the people to ask.)

The base period used for calculation of anomalies from the grid box mean of ERSST is, I believe, the 30 years 1961-90 (as that had the most data). So if you are using a gridded field, that is the relevant number - though Viva can verify my memory on the dates). But when we make global averaged temperature time series, we adjust the time series up or down so that the zero line is the mean of 1900-1999.

Viva, Dick, do you have anything to add (or correct)?

Tom, I've also cc'd Deke Arndt, the head of our Climate Monitoring Branch because if you find this confusing, he will probably want to make sure the web pages you read are made clearer.

Regards,
Tom P.

----- Original Message -----

From: Tom Wigley [3]<wigley@ucar.edu>
Date: Thursday, October 8, 2009 2:16 am
Subject: ERSST

Dear Toms,

Could you please clarify a few things for me ...

(1) Is the currently downloadable ERSST data version 3, or 3b? It seems to be 3b -- but the web page is not entirely clear. In one place it says that v.3 will be used from July, but elsewhere it says 3b will be used from July.

If it is v.3b, then does this mean that the Smith et al. reference is not (quite) appropriate?

(2) Is ERSST spatially complete? I think not. If it were, then $(NH+SH)/2$ should equal GL, but this is not the case. I'm sure you know that HadCRU uses $(NH+SH)/2$ for the global mean (arguably superior to a straight global area average). It seems odd that this issue has been glossed over.

(3) How often will ERSST be updated? I presume you are aware that HadCRU updates annually to get the late data in. It seems that ERSST only updates with new numbered versions -- so it misses late data. (GISS is worse.)

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(4) what is the reference period? I think I saw somewhere on the web page that it is 1900-99? But methodologically perhaps it is difficult to define a reference period?

Thanks,
Tom

References

1. mailto:Viva.Banzon@noaa.gov
2. mailto:Thomas.C.Peterson@noaa.gov
3. mailto:wigley@ucar.edu

1044. 1255095172.txt

#####

From: Ben Santer <santer1@llnl.gov>
 To: Stephen H Schneider <shs@stanford.edu>
 Subject: [Fwd: Re: CEI formal petition to derail EPA GHG endangerment finding with charge that destruction of CRU raw data undermines integrity of global temperature record]
 Date: Fri, 09 Oct 2009 09:32:52 -0700
 Reply-to: santer1@llnl.gov
 Cc: "'Kevin E. Trenberth'" <trenbert@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, mann <mann@psu.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Tom Wigley <>wigley@cgd.ucar.edu>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>

<x-flowed>
Dear Steve,

I was made aware of this yesterday (see forwarded email).

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) 422-3840
 FAX: (925) 422-7675
 email: santer1@llnl.gov

</x-flowed>
X-Account-Key: account1
X-Mozilla-Keys:

Return-Path: <santer1@llnl.gov>
 Received: from mail-2.llnl.gov ([unix socket])
 by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
 Thu, 08 Oct 2009 18:28:44 -0700
 Received: from nspiron-1.llnl.gov (nspiron-1.llnl.gov [128.115.41.81])
 by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.7 \$) with ESMTTP id
 n991sh62016185;
 Thu, 8 Oct 2009 18:28:43 -0700
 X-Attachments: None

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Received: from dione.llnl.gov ([128.115.57.29])
by nspiron-1.llnl.gov with ESMTP; 08 Oct 2009 18:28:44 -0700
Message-ID: <4ACE91CA.700006@llnl.gov>
Date: Thu, 08 Oct 2009 18:28:42 -0700
From: Ben Santer <santer1@llnl.gov>
Reply-To: santer1@llnl.gov
Organization: LLNL
User-Agent: Thunderbird 2.0.0.22 (X11/20090605)
MIME-Version: 1.0
To: Rick Piltz <piltz@comcast.net>
CC: Tom Wigley <wigley@ucar.edu>, Tom Karl <Thomas.R.Karl@noaa.gov>,
Jim Hansen <jeh1@columbia.edu>,
Bob Watson <robert.watson@defra.gsi.gov.uk>,
Mike MacCracken <mmaccrac@comcast.net>,
"John F. B. Mitchell" <john.f.mitchell@metoffice.gov.uk>
Subject: Re: CEI formal petition to derail EPA GHG endangerment finding with
charge that destruction of CRU raw data undermines integrity of global temperature
record
References: <80955b\$27nkli@smtp.llnl.gov>
In-Reply-To: <80955b\$27nkli@smtp.llnl.gov>
Content-Type: text/plain; charset=ISO-8859-1; format=flowed
Content-Transfer-Encoding: 7bit

<x-flowed>
Dear Rick,

I am prepared to help in any way that I can.

As I see it, there are two key issues here.

First, the CEI and Pat Michaels are arguing that Phil Jones and colleagues at the Climatic Research Unit (CRU) willfully and intentionally "destroyed" some of the raw surface temperature data used in the construction of the gridded surface temperature datasets.

Second, the CEI and Pat Michaels contend that the CRU surface temperature datasets provided the sole basis for IPCC "discernible human influence" conclusions.

Both of these arguments are factually incorrect. First, there was no intentional destruction of the primary source data. I am sure that, over 20 years ago, Phil could not have foreseen that the raw station data might be the subject of legal proceedings by the CEI and Pat Michaels. Raw data were NOT secretly destroyed to avoid efforts by other scientists to replicate the CRU and Hadley Centre-based estimates of global-scale changes in near-surface temperature. In fact, a key point here is that other groups (primarily at NCDC and at GISS, but also in Russia) WERE able to replicate the major findings of the CRU and Hadley Centre groups. The NCDC and GISS groups performed this replication completely independently. They made different choices in the complex process of choosing input data, adjusting raw station data for known inhomogeneities (such as urbanization effects, changes in instrumentation, site location, and observation time), and gridding procedures. NCDC and GISS-based estimates of global surface temperature changes are in good accord with the HadCRUT results.

I'm sure that Pat Michaels does not have the primary source data used in his Ph.D. thesis. Perhaps one of us should request the datasets used in Michaels' Ph.D. work, and then ask the University of Wisconsin to withdraw Michaels' Ph.D. if he fails to produce every dataset and computer program used in the course of his thesis research.

I'm equally sure that John Christy and Roy Spencer have not preserved

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every single version of their MSU-based estimates of tropospheric temperature change. Nor is it likely that Christy and Spencer have preserved for posterity each and every computer program they used to generate UAH tropospheric temperature datasets.

[One irony here is that the Christy/Spencer claim that the troposphere had cooled over the satellite era did not stand up to rigorous scientific scrutiny. Christy and Spencer have made a scientific career out of being wrong. In contrast, CRU's claim of a pronounced increase in global-mean surface temperature over the 20th century HAS withstood the test of time.]

The CEI and Michaels are applying impossible legal standards to science. They are essentially claiming that if we do not retain - and make available to self-appointed auditors - every piece of information about every scientific paper we have ever published, we are perpetrating some vast deception on the American public. I think most ordinary citizens understand that few among us have preserved every bank statement and every utility bill we've received in the last 20 years.

The second argument - that "discernible human influence" findings are like a house of cards, resting solely on one observational dataset - is also invalid. The IPCC Third Assessment Report (TAR) considers MULTIPLE observational estimates of global-scale near-surface temperature changes. It does not rely on HadCRUT data alone - as is immediately obvious from Figure 2.1b of the TAR, which shows CRU, NCDC, and GISS global-mean temperature changes.

As pointed out in numerous scientific assessments (e.g., the IPCC TAR and Fourth Assessment Reports, the U.S. Climate Change Science Program Synthesis and Assessment Report 1.1, and the CCSP "State of Knowledge" Report), rigorous statistical fingerprint studies have now been performed with a whole range of climate variables - and not with surface temperature only. Examples include variables like ocean heat content, atmospheric water vapor, surface specific humidity, continental river runoff, sea-level pressure patterns, stratospheric and tropospheric temperature, tropopause height, zonal-mean precipitation over land, and Arctic sea-ice extent. The bottom-line message from this body of work is that natural causes alone CANNOT plausibly explain the climate changes we have actually observed. The climate system is telling us an internally- and physically-consistent story. The integrity and reliability of this story does NOT rest on a single observational dataset, as Michaels and the CEI incorrectly claim.

Michaels should and does know better. I can only conclude from his behavior - and from his participation in this legal action - that he is being intentionally dishonest. His intervention seems to be timed to influence opinion in the run-up to the Copenhagen meeting, and to garner publicity for himself. In my personal opinion, Michaels should be kicked out of the AMS, the University of Virginia, and the scientific community as a whole. He cannot on the one hand engage in vicious public attacks on the reputations of individual scientists (in the past he has attacked Tom Karl, Tom Wigley, Jim Hansen, Mike Mann, myself, and numerous others), and on the other hand expect to be treated as a valued member of our professional societies.

The sad thing here is that Phil Jones is one of the true gentlemen of our field. I have known Phil for most of my scientific career. He is the antithesis of the secretive, "data destroying" character the CEI and Michaels are trying to portray to the outside world. Phil and Tom Wigley have devoted significant portions of their scientific careers to the construction of the land surface temperature component of the HadCRUT dataset. They have conducted this research in a very open and

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transparent manner - examining sensitivities to different gridding algorithms, different ways of adjusting for urbanization effects, use of various subsets of data, different ways of dealing with changes in spatial coverage over time, etc. They have thoroughly and comprehensively documented all of their dataset construction choices. They have done a tremendous service to the scientific community - and to the planet - by making gridded surface temperature datasets available for scientific research. They deserve medals as big as soup plates - not the kind of crap they are receiving from Pat Michaels and the CEI.

The bottom line, Rick, is that I am incensed at the "data destruction" allegations that are being unfairly and incorrectly leveled against Phil and Tom by the CEI and Pat Michaels. Please let me know how you think I can be most effective in rebutting such allegations. Whatever you need from me - you've got it.

I hope you don't mind, but I'm also copying my email to John Mitchell at the Hadley Centre. I know that John also feels very strongly about these issues.

With best regards,

Ben

Rick Piltz wrote:

> Gentlemen--

>

> I expect that you have already been made aware of the petition to EPA
> from the Competitive Enterprise Institute (and Pat Michaels) calling for
> a re-opening of public comment on EPA's prospective "endangerment"
> finding on greenhouse gases. CEI is charging that the CRU at East Anglia
> has destroyed the raw data for a portion of the global temperature
> record, thus destroying the integrity of the IPCC assessments and any
> other work that treats the UK Jones-Wigley global temperature data
> record as scientifically legitimate. I have attached the petition in
> PDF, with a statements by CEI and Michaels.

>

> The story was reported in Environment & Energy Daily yesterday (below).
> They called me for it, presumably because I am on their call list as
> someone who gets in the face of the global warming disinformation
> campaign, among other things. I hit CEI, but I don't have a technical
> response to their allegations.

>

> Who is responding to this charge on behalf of the science community?
> Surely someone will have to, if only because EPA will need to know
> exactly what to say. And really I believe all of you, as the
> authoritative experts, should be prepared to do that in a way that has
> some collective coherence.

>

> I am going to be writing about this on my Climate Science Watch website
> as soon as I think I can do so appropriately. I am most interested in
> what you have to say to set the record straight and put things in
> perspective -- either on or off the record, whichever you wish. Will
> someone please explain this to me?

>

> Best regards,
> Rick

>

>

> *1. CLIMATE: Free-market group attacks data behind EPA
> 'endangerment' proposal (E&E News PM, 10/07/2009)

>

> *

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>
>
> *Robin Bravender, E&E reporter*
>
> A free-market advocacy group has launched another attack on the science
> behind U.S. EPA's proposed finding that greenhouse gases endanger human
> health and welfare.
>
> The Competitive Enterprise Institute -- a vocal foe of EPA's efforts to
> finalize its "endangerment finding" -- *petitioned*
> <http://www.eenews.net/features/documents/2009/10/07/document_pm_02.pdf>
> the agency this week to reopen the public comment period on the
> proposal, arguing that critical data used to formulate the plan have
> been destroyed and that the available data are therefore unreliable.
>
> *At issue is a set of raw data from the Climatic Research Unit at the
> University of East Anglia in Norwich, England, that includes surface
> temperature averages from weather stations around the world. *According
> to CEI, the data provided a foundation for the 1996 second assessment
> report by the Intergovernmental Panel on Climate Change, which EPA used
> when drafting its endangerment proposal.
>
> According to the web site for East Anglia's research unit, "Data storage
> availability in the 1980s meant that we were not able to keep the
> multiple sources for some sites, only the station series after
> adjustment for homogeneity issues. We, therefore, do not hold the
> original raw data but only the value-added (i.e. quality controlled and
> homogenized) data."
>
> CEI general counsel Sam Kazman said this lack of raw data calls the
> endangerment finding into question. *"EPA is resting its case on
> international studies that in turn relied on CRU data. But CRU's
> suspicious destruction of its original data, disclosed at this late
> date, makes that information totally unreliable," he said.* "If EPA
> doesn't re-examine the implications of this, it's stumbling blindly into
> the most important regulatory issue we face."
>
> *In a statement filed with CEI's petition, Cato Institute senior fellow
> Patrick Michaels called the development a "totally new element" in the
> endangerment debate. "It violates basic scientific principles and throws
> even more doubt onto the contention that anthropogenic greenhouse gas
> emissions endanger human welfare," he wrote.
>
> *Michaels is a University of Virginia professor and author of the book,
> "The Satanic Gases: Clearing the Air about Global Warming." He stepped
> down from his post as Virginia's state climatologist in 2007 after he
> came under fire for publicly doubting global warming while taking money
> from the utility industry (/ Greenwire/
> <<http://www.eenews.net/Greenwire/2007/09/27/archive/9>>, Sept. 27, 2007).
>
> Representatives of East Anglia University's Climatic Research Unit were
> not available to comment on the CEI petition.
>
> EPA spokeswoman Adora Andy said the agency will evaluate the petition.
> "But after initial review of the statement their position rests upon,"
> Andy added, "it certainly does not appear to justify upheaval."
>
> The petition is the latest in a string of CEI challenges to the
> proceedings surrounding the endangerment finding and other Obama
> administration climate policies. Last week, the group threatened to sue
> the administration over documents related to the costs of a federal
> cap-and-trade program to curb greenhouse gas emissions. And in June, the
> group accused EPA officials of suppressing dissenting views from an EPA

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> environmental economist during the run-up to the release of the
> endangerment proposal.
>
> Rick Piltz, director of the watchdog group Climate Science Watch and a
> former official at the U.S. Climate Change Science Program, said that
> although the research unit's data are among key data sets used by the
> IPCC, "it's not the only data set that they use." He also said EPA drew
> on "multifaceted, robust" data in the technical support document
> underlying the finding.
>
> EPA's endangerment finding relies most heavily on IPCC's 2007 fourth
> assessment; synthesis and assessment products of the U.S. Climate Change
> Science Program; National Research Council reports under the U.S.
> National Academy of Sciences; the EPA annual report on U.S. greenhouse
> gas emission inventories; and the EPA assessment of the effects of
> global change on regional U.S. air quality, according to the agency's
> technical support document.
>
> "You do not need to reopen the IPCC reports and the technical support
> document on the EPA endangerment finding because of something having to
> do with the raw data from the temperature record from East Anglia
> University in the 1980s," Piltz said, adding that the IPCC carefully
> vets its data.
>
> Piltz said CEI is on an ideological mission to head off EPA attempts to
> finalize the endangerment finding and is "grasping at straws" by
> challenging the IPCC data.
>
> "Their bottom line is an antiregulatory ideology," Piltz said. "When
> they use science, they use it tactically, and they will go to war with
> the mainstream science community."
>
> Republican senators also weighed in yesterday, urging EPA to reopen the
> public comment period on the endangerment finding to investigate the
> scientific merit of the research data.
>
> "It's astonishing that EPA, so confident in the scientific integrity of
> its work, refuses to be transparent with the public about the most
> consequential rulemaking of our time," said Sen. James Inhofe (R-Okla.),
> ranking member of the Environment and Public Works Committee. Inhofe
> sent a joint press release with Sen. John Barrasso (R-Wyo.) accusing EPA
> of relying upon flawed data.
>
> "Now the evidence shows that scientists interested in testing some of
> EPA's assertions can't engage in basic scientific work, such as assuring
> reproducibility and objectivity, because the data they seek have been
> destroyed," Inhofe said. "In order to conform to federal law and basic
> standards of scientific integrity, EPA must reopen the record so the
> public can judge whether EPA's claims are based on the best available
> scientific information."
>
> Rick Piltz
> Director, Climate Science Watch
> 301-807-2472
> www.*climatesciencewatch.org
>
> <http://*www.*climatesciencewatch.org/>Climate Science Watch is a
> sponsored project of the Government Accountability Project, Washington,
> DC, dedicated to holding public officials accountable for using climate
> science and related research effectively and with integrity in
> responding to the challenges posed by global climate disruption.
>
> The right to search for truth implies also a duty; one must not conceal

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> any part of what one has recognized to be true.
> --Albert Einstein
>

--

Benjamin D. Santer
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Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

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1045. 1255100876.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: CEI formal petition to derail EPA GHG endangerment finding with charge that destruction of CRU raw data undermines integrity of global temperature record
Date: Fri, 09 Oct 2009 11:07:56 -0700
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

I've known Rick Piltz for many years. He's a good guy. I believe he used to work with Mike MacCracken at the U.S. Global Change Research Program.

I'm really sorry that you have to go through all this stuff, Phil. Next time I see Pat Michaels at a scientific meeting, I'll be tempted to beat the crap out of him. Very tempted.

I'll help you to deal with Michaels and the CEI in any way that I can. The only reason these guys are going after you is because your work is of crucial importance - it changed the way the world thinks about human effects on climate. Your work mattered in the 1980s, and it matters now.

with best wishes,

Ben

P.Jones@uea.ac.uk wrote:

> Ben,
> Thanks for backing me up with whoever Rick is. I forwarded the message
> to Rick. So if you want to add anything else feel free to do so.
> We have more stations going into the latest CRU data than we did in the
> 1980s.
>
> In Lecce next week for 2 days at a GKSS summer school led by Hans VS!
>
> Cheers
> Phil

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>
>> Dear Rick,
>>
>> I am prepared to help in any way that I can.
>>
>> As I see it, there are two key issues here.
>>
>> First, the CEI and Pat Michaels are arguing that Phil Jones and
>> colleagues at the Climatic Research Unit (CRU) willfully and
>> intentionally "destroyed" some of the raw surface temperature data used
>> in the construction of the gridded surface temperature datasets.
>>
>> Second, the CEI and Pat Michaels contend that the CRU surface
>> temperature datasets provided the sole basis for IPCC "discernible human
>> influence" conclusions.
>>
>> Both of these arguments are factually incorrect. First, there was no
>> intentional destruction of the primary source data. I am sure that, over
>> 20 years ago, Phil could not have foreseen that the raw station data
>> might be the subject of legal proceedings by the CEI and Pat Michaels.
>> Raw data were NOT secretly destroyed to avoid efforts by other
>> scientists to replicate the CRU and Hadley Centre-based estimates of
>> global-scale changes in near-surface temperature. In fact, a key point
>> here is that other groups (primarily at NCDC and at GISS, but also in
>> Russia) WERE able to replicate the major findings of the CRU and Hadley
>> Centre groups. The NCDC and GISS groups performed this replication
>> completely independently. They made different choices in the complex
>> process of choosing input data, adjusting raw station data for known
>> inhomogeneities (such as urbanization effects, changes in
>> instrumentation, site location, and observation time), and gridding
>> procedures. NCDC and GISS-based estimates of global surface temperature
>> changes are in good accord with the HadCRUT results.
>>
>> I'm sure that Pat Michaels does not have the primary source data used in
>> his Ph.D. thesis. Perhaps one of us should request the datasets used in
>> Michaels' Ph.D. work, and then ask the University of Wisconsin to
>> withdraw Michaels' Ph.D. if he fails to produce every dataset and
>> computer program used in the course of his thesis research.
>>
>> I'm equally sure that John Christy and Roy Spencer have not preserved
>> every single version of their MSU-based estimates of tropospheric
>> temperature change. Nor is it likely that Christy and Spencer have
>> preserved for posterity each and every computer program they used to
>> generate UAH tropospheric temperature datasets.
>>
>> [One irony here is that the Christy/Spencer claim that the troposphere
>> had cooled over the satellite era did not stand up to rigorous
>> scientific scrutiny. Christy and Spencer have made a scientific career
>> out of being wrong. In contrast, CRU's claim of a pronounced increase in
>> global-mean surface temperature over the 20th century HAS withstood the
>> test of time.]
>>
>> The CEI and Michaels are applying impossible legal standards to science.
>> They are essentially claiming that if we do not retain - and make
>> available to self-appointed auditors - every piece of information about
>> every scientific paper we have ever published, we are perpetrating some
>> vast deception on the American public. I think most ordinary citizens
>> understand that few among us have preserved every bank statement and
>> every utility bill we've received in the last 20 years.
>>
>> The second argument - that "discernible human influence" findings are
>> like a house of cards, resting solely on one observational dataset - is
>> also invalid. The IPCC Third Assessment Report (TAR) considers MULTIPLE

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>> observational estimates of global-scale near-surface temperature
>> changes. It does not rely on HadCRUT data alone - as is immediately
>> obvious from Figure 2.1b of the TAR, which shows CRU, NCDC, and GISS
>> global-mean temperature changes.
>>
>> As pointed out in numerous scientific assessments (e.g., the IPCC TAR
>> and Fourth Assessment Reports, the U.S. Climate Change Science Program
>> Synthesis and Assessment Report 1.1, and the CCSP "State of Knowledge"
>> Report), rigorous statistical fingerprint studies have now been
>> performed with a whole range of climate variables - and not with surface
>> temperature only. Examples include variables like ocean heat content,
>> atmospheric water vapor, surface specific humidity, continental river
>> runoff, sea-level pressure patterns, stratospheric and tropospheric
>> temperature, tropopause height, zonal-mean precipitation over land, and
>> Arctic sea-ice extent. The bottom-line message from this body of work is
>> that natural causes alone CANNOT plausibly explain the climate changes
>> we have actually observed. The climate system is telling us an
>> internally- and physically-consistent story. The integrity and
>> reliability of this story does NOT rest on a single observational
>> dataset, as Michaels and the CEI incorrectly claim.
>>
>> Michaels should and does know better. I can only conclude from his
>> behavior - and from his participation in this legal action - that he is
>> being intentionally dishonest. His intervention seems to be timed to
>> influence opinion in the run-up to the Copenhagen meeting, and to garner
>> publicity for himself. In my personal opinion, Michaels should be kicked
>> out of the AMS, the University of Virginia, and the scientific community
>> as a whole. He cannot on the one hand engage in vicious public attacks
>> on the reputations of individual scientists (in the past he has attacked
>> Tom Karl, Tom Wigley, Jim Hansen, Mike Mann, myself, and numerous
>> others), and on the other hand expect to be treated as a valued member
>> of our professional societies.
>>
>> The sad thing here is that Phil Jones is one of the true gentlemen of
>> our field. I have known Phil for most of my scientific career. He is the
>> antithesis of the secretive, "data destroying" character the CEI and
>> Michaels are trying to portray to the outside world. Phil and Tom Wigley
>> have devoted significant portions of their scientific careers to the
>> construction of the land surface temperature component of the HadCRUT
>> dataset. They have conducted this research in a very open and
>> transparent manner - examining sensitivities to different gridding
>> algorithms, different ways of adjusting for urbanization effects, use of
>> various subsets of data, different ways of dealing with changes in
>> spatial coverage over time, etc. They have thoroughly and
>> comprehensively documented all of their dataset construction choices.
>> They have done a tremendous service to the scientific community - and to
>> the planet - by making gridded surface temperature datasets available
>> for scientific research. They deserve medals as big as soup plates - not
>> the kind of crap they are receiving from Pat Michaels and the CEI.
>>
>> The bottom line, Rick, is that I am incensed at the "data destruction"
>> allegations that are being unfairly and incorrectly leveled against Phil
>> and Tom by the CEI and Pat Michaels. Please let me know how you think I
>> can be most effective in rebutting such allegations. Whatever you need
>> from me - you've got it.
>>
>> I hope you don't mind, but I'm also copying my email to John Mitchell at
>> the Hadley Centre. I know that John also feels very strongly about these
>> issues.
>>
>> With best regards,
>>
>> Ben

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>>

>> Rick Piltz wrote:

>>> Gentlemen--

>>>

>>> I expect that you have already been made aware of the petition to EPA
>>> from the Competitive Enterprise Institute (and Pat Michaels) calling for
>>> a re-opening of public comment on EPA's prospective "endangerment"
>>> finding on greenhouse gases. CEI is charging that the CRU at East Anglia
>>> has destroyed the raw data for a portion of the global temperature
>>> record, thus destroying the integrity of the IPCC assessments and any
>>> other work that treats the UK Jones-Wigley global temperature data
>>> record as scientifically legitimate. I have attached the petition in
>>> PDF, with a statements by CEI and Michaels.

>>>

>>> The story was reported in Environment & Energy Daily yesterday (below).
>>> They called me for it, presumably because I am on their call list as
>>> someone who gets in the face of the global warming disinformation
>>> campaign, among other things. I hit CEI, but I don't have a technical
>>> response to their allegations.

>>>

>>> who is responding to this charge on behalf of the science community?
>>> Surely someone will have to, if only because EPA will need to know
>>> exactly what to say. And really I believe all of you, as the
>>> authoritative experts, should be prepared to do that in a way that has
>>> some collective coherence.

>>>

>>> I am going to be writing about this on my Climate Science Watch website
>>> as soon as I think I can do so appropriately. I am most interested in
>>> what you have to say to set the record straight and put things in
>>> perspective -- either on or off the record, whichever you wish. Will
>>> someone please explain this to me?

>>>

>>> Best regards,

>>> Rick

>>>

>>>

>>> *1. CLIMATE: Free-market group attacks data behind EPA
>>> 'endangerment' proposal (E&E News PM, 10/07/2009)

>>>

>>> *

>>>

>>>

>>>

>>> *Robin Bravender, E&E reporter*

>>>

>>> A free-market advocacy group has launched another attack on the science
>>> behind U.S. EPA's proposed finding that greenhouse gases endanger human
>>> health and welfare.

>>>

>>> The Competitive Enterprise Institute -- a vocal foe of EPA's efforts to
>>> finalize its "endangerment finding" -- *petitioned*
>>> <http://**www.**eenews.net/features/documents/2009/10/07/document_pm_02.pdf>
>>> the agency this week to reopen the public comment period on the
>>> proposal, arguing that critical data used to formulate the plan have
>>> been destroyed and that the available data are therefore unreliable.

>>>

>>> *At issue is a set of raw data from the Climatic Research Unit at the
>>> University of East Anglia in Norwich, England, that includes surface
>>> temperature averages from weather stations around the world. *According
>>> to CEI, the data provided a foundation for the 1996 second assessment
>>> report by the Intergovernmental Panel on Climate Change, which EPA used
>>> when drafting its endangerment proposal.

>>>

>>> According to the web site for East Anglia's research unit, "Data storage

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>>> availability in the 1980s meant that we were not able to keep the
>>> multiple sources for some sites, only the station series after
>>> adjustment for homogeneity issues. We, therefore, do not hold the
>>> original raw data but only the value-added (i.e. quality controlled and
>>> homogenized) data."

>>>
>>> CEI general counsel Sam Kazman said this lack of raw data calls the
>>> endangerment finding into question. *"EPA is resting its case on
>>> international studies that in turn relied on CRU data. But CRU's
>>> suspicious destruction of its original data, disclosed at this late
>>> date, makes that information totally unreliable," he said.* "If EPA
>>> doesn't re-examine the implications of this, it's stumbling blindly into
>>> the most important regulatory issue we face."

>>>
>>> *In a statement filed with CEI's petition, Cato Institute senior fellow
>>> Patrick Michaels called the development a "totally new element" in the
>>> endangerment debate. "It violates basic scientific principles and throws
>>> even more doubt onto the contention that anthropogenic greenhouse gas
>>> emissions endanger human welfare," he wrote.

>>>
>>> *Michaels is a University of Virginia professor and author of the book,
>>> "The Satanic Gases: Clearing the Air about Global Warming." He stepped
>>> down from his post as Virginia's state climatologist in 2007 after he
>>> came under fire for publicly doubting global warming while taking money
>>> from the utility industry (/ Greenwire/
>>> <http://**eenews.net/Greenwire/2007/09/27/archive/9>, Sept. 27, 2007).

>>>
>>> Representatives of East Anglia University's Climatic Research Unit were
>>> not available to comment on the CEI petition.

>>>
>>> EPA spokeswoman Adora Andy said the agency will evaluate the petition.
>>> "But after initial review of the statement their position rests upon,"
>>> Andy added, "it certainly does not appear to justify upheaval."

>>>
>>> The petition is the latest in a string of CEI challenges to the
>>> proceedings surrounding the endangerment finding and other Obama
>>> administration climate policies. Last week, the group threatened to sue
>>> the administration over documents related to the costs of a federal
>>> cap-and-trade program to curb greenhouse gas emissions. And in June, the
>>> group accused EPA officials of suppressing dissenting views from an EPA
>>> environmental economist during the run-up to the release of the
>>> endangerment proposal.

>>>
>>> Rick Piltz, director of the watchdog group Climate Science Watch and a
>>> former official at the U.S. Climate Change Science Program, said that
>>> although the research unit's data are among key data sets used by the
>>> IPCC, "it's not the only data set that they use." He also said EPA drew
>>> on "multifaceted, robust" data in the technical support document
>>> underlying the finding.

>>>
>>> EPA's endangerment finding relies most heavily on IPCC's 2007 fourth
>>> assessment; synthesis and assessment products of the U.S. Climate Change
>>> Science Program; National Research Council reports under the U.S.
>>> National Academy of Sciences; the EPA annual report on U.S. greenhouse
>>> gas emission inventories; and the EPA assessment of the effects of
>>> global change on regional U.S. air quality, according to the agency's
>>> technical support document.

>>>
>>> "You do not need to reopen the IPCC reports and the technical support
>>> document on the EPA endangerment finding because of something having to
>>> do with the raw data from the temperature record from East Anglia
>>> University in the 1980s," Piltz said, adding that the IPCC carefully
>>> vets its data.

>>> Piltz said CEI is on an ideological mission to head off EPA attempts to
>>> finalize the endangerment finding and is "grasping at straws" by
>>> challenging the IPCC data.

>>> "Their bottom line is an antiregulatory ideology," Piltz said. "When
>>> they use science, they use it tactically, and they will go to war with
>>> the mainstream science community."

>>> Republican senators also weighed in yesterday, urging EPA to reopen the
>>> public comment period on the endangerment finding to investigate the
>>> scientific merit of the research data.

>>> "It's astonishing that EPA, so confident in the scientific integrity of
>>> its work, refuses to be transparent with the public about the most
>>> consequential rulemaking of our time," said Sen. James Inhofe (R-Okla.),
>>> ranking member of the Environment and Public Works Committee. Inhofe
>>> sent a joint press release with Sen. John Barrasso (R-Wyo.) accusing EPA
>>> of relying upon flawed data.

>>> "Now the evidence shows that scientists interested in testing some of
>>> EPA's assertions can't engage in basic scientific work, such as assuring
>>> reproducibility and objectivity, because the data they seek have been
>>> destroyed," Inhofe said. "In order to conform to federal law and basic
>>> standards of scientific integrity, EPA must reopen the record so the
>>> public can judge whether EPA's claims are based on the best available
>>> scientific information."

>>> Rick Piltz
>>> Director, Climate Science Watch
>>> 301-807-2472
>>> www.**climatesciencewatch.org

>>> <http://**www.**climatesciencewatch.org/>Climate Science Watch is a
>>> sponsored project of the Government Accountability Project, Washington,
>>> DC, dedicated to holding public officials accountable for using climate
>>> science and related research effectively and with integrity in
>>> responding to the challenges posed by global climate disruption.

>>> The right to search for truth implies also a duty; one must not conceal
>>> any part of what one has recognized to be true.

>>> --Albert Einstein

>>>

>>>

>>> --

>>> -----

>> Benjamin D. Santer
>> Program for Climate Model Diagnosis and Intercomparison
>> Lawrence Livermore National Laboratory
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>> Livermore, CA 94550, U.S.A.
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#####

From: P.Jones@uea.ac.uk
To: "Rick Piltz" <piltz@comcast.net>
Subject: Re: Your comments on the latest CEI/Michaels gambit
Date: Sun, 11 Oct 2009 18:03:13 +0100 (BST)
Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Ben Santer" <santer1@llnl.gov>

Rick,
What you've put together seems fine from a quick read. I'm in Lecce in the heel of Italy till Tuesday. I should be back in the UK by Wednesday.

The original raw data are not lost either. I could reconstruct what we had from some DoE reports we published in the mid-1980s. I would start with the GHCN data. I know that the effort would be a complete waste of time though. I may get around to it some time. As you've said, the documentation of what we've done is all in the literature.

I think if it hadn't been this issue, the CEI would have dreamt up something else!

Cheers
Phil

> Phil and Ben--
>
> Thanks for writing. I appreciate very much what you're saying.
>
> I'm going to be posting some entries on this matter on the Climate
> Science Watch web site. I'm sure others will weigh in on it in
> various venues (Steve Schneider has supplied me with an on-the-record
> quote), and I suppose that a more formal response by the relevant
> scientists is likely eventually to become part of the EPA docket as
> part of their rejection of the CEI petition. But that will drag on,
> and meanwhile CEI and Michaels will demagogue their allegations, as
> they do with everything. No way to prevent that. But I would like to
> expedite documenting some immediate pushback, helping to set the
> record straight and put what CEI and Michaels are up to in perspective.
>
> I have taken the liberty of editing what you wrote just a bit (and
> adding some possible URL links and writing-out of acronyms), in the
> hope that, with your permission and with any revisions or additions
> you might care to make, we could post your comments. This requires
> no clearance other than you and me. I would draft appropriate text to
> provide context. Please take a look at this and RSVP:

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>
> Ben's comment:
>
> As I see it, there are two key issues here.
>
> First, the Competitive Enterprise Institute (CEI) and Pat Michaels
> are arguing that Phil Jones and colleagues at the CRU [Climatic
> Research Unit at the University of East Anglia, UK] willfully,
> intentionally, and suspiciously "destroyed" some of the raw surface
> temperature data used in the construction of the gridded surface
> temperature datasets.
>
> Second, the CEI and Pat Michaels contend that the CRU surface
> temperature datasets provided the sole basis for IPCC "discernible
> human influence" conclusions.
>
> Both of these arguments are incorrect. First, there was no
> intentional destruction of the primary source data. I am sure that,
> over 20 years ago, the CRU could not have foreseen that the raw
> station data might be the subject of legal proceedings by the CEI and
> Pat Michaels. Raw data were NOT secretly destroyed to avoid efforts
> by other scientists to replicate the CRU and Hadley Centre-based
> estimates of global-scale changes in near-surface temperature. In
> fact, a key point here is that other groups -- primarily at the NCDC
> [NOAA National Climatic Data Center] and at GISS [NASA Goddard
> Institute for Space Studies], but also in Russia -- WERE able to
> replicate the major findings of the CRU and UK Hadley Centre groups.
> The NCDC and GISS groups performed this replication completely
> independently. They made different choices in the complex process of
> choosing input data, adjusting raw station data for known
> inhomogeneities (such as urbanization effects, changes in
> instrumentation, site location, and observation time), and gridding
> procedures. NCDC and GISS-based estimates of global surface
> temperature changes are in good accord with the HadCRUT data results.
>
> The second argument -- that "discernible human influence" findings
> are like a house of cards, resting solely on one observational
> dataset -- is also invalid. The IPCC Third Assessment Report (TAR)
> considers MULTIPLE observational estimates of global-scale
> near-surface temperature changes. It does not rely on HadCRUT data
> alone - as is immediately obvious from Figure 2.1b of the TAR, which
> shows CRU, NCDC, and GISS global-mean temperature changes.
>
> As pointed out in numerous scientific assessments (e.g., the IPCC TAR
> and Fourth Assessment Reports, the U.S. Climate Change Science
> Program Synthesis and Assessment Report 1.1 (Temperature trends in
> the lower atmosphere: steps for understanding and reconciling
> differences), and the state of knowledge report, Global Climate
> Change Impacts on the United States, rigorous statistical fingerprint
> studies have now been performed with a whole range of climate
> variables -- and not with surface temperature only. Examples include
> variables like ocean heat content, atmospheric water vapor, surface
> specific humidity, continental river runoff, sea-level pressure
> patterns, stratospheric and tropospheric temperature, tropopause
> height, zonal-mean precipitation over land, and Arctic sea-ice
> extent. The bottom-line message from this body of work is that
> natural causes alone CANNOT plausibly explain the climate changes we
> have actually observed. The climate system is telling us an
> internally- and physically-consistent story. The integrity and
> reliability of this story does NOT rest on a single observational
> dataset, as Michaels and the CEI incorrectly claim.
>
> I have known Phil for most of my scientific career. He is the

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> antithesis of the secretive, "data destroying" character the CEI and
> Michaels are trying to portray to the outside world. Phil and Tom
> Wigley have devoted significant portions of their scientific careers
> to the construction of the land surface temperature component of the
> HadCRUT dataset. They have conducted this research in a very open and
> transparent manner -- examining sensitivities to different gridding
> algorithms, different ways of adjusting for urbanization effects, use
> of various subsets of data, different ways of dealing with changes in
> spatial coverage over time, etc. They have thoroughly and
> comprehensively documented all of their dataset construction choices.
> They have done a tremendous service to the scientific community --
> and to the planet -- by making gridded surface temperature datasets
> available for scientific research. They deserve medals -- not the
> kind of deliberately misleading treatment they are receiving from Pat
> Michaels and the CEI.

>
>
> Phil's comment:

>
> No one, it seems, cares to read what we put up on the CRU web page.
> These people just make up motives for what we might or might not have
> done.

>
> <<http://www.cru.uea.ac.uk/cru/data/temperature/>><http://www.cru.uea.ac.uk/cru/data/temperature/>

>
> Almost all the data we have in the CRU archive is exactly the same as
> in the GHCN archive [Global Historical Climatology Network, used by
> the NOAA National Climate Data Center].

> <http://www.ncdc.noaa.gov/oa/climate/ghcn-monthly/index.php>
> <http://www.ncdc.noaa.gov/oa/climate/research/ghcn/ghcngrid.html>

> If we have lost any data it is the following:

>
> 1. Station series for sites that in the 1980s we deemed then to be
> affected by either urban biases or by numerous site moves, that were
> either not correctable or not worth doing as there were other series
> in the region.

>
> 2. The original data for sites that we adjusted the temperature data
> [Phil: for known inhomogeneities, or what?] in the 1980s. We still
> have our adjusted data, of course, and these along with all other
> sites that didn't need adjusting.

>
> 3. Since the 1980s as colleagues and NMSs [National Meteorological
> Services] have produced adjusted series for regions and or countries,
> then we replaced the data we had with the better series.

> http://www.wmo.int/pages/members/index_en.html

>
> In the papers, I've always said that homogeneity adjustments are best
> produced by NMSs. A good example of this is the work by Lucie Vincent
> in Canada. Here we just replaced what data we had for the 200+ sites
> she sorted out.

>
> The CRUTEM3 data for land look much like the GHCN and GISS [NASA
> Goddard Institute for Space Studies] data for the same domains.
> <http://data.giss.nasa.gov/gistemp/>

>
> Apart from a figure in the IPCC AR4 [Fourth Assessment Report, 2007]
> showing this, there is also this paper from Geophysical Research
> Letters in 2005 by Russ Vose et al. Figure 2 is similar to the AR4 plot.
> [Vose et al paper]

>

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> All best,
 > Rick
 >
 >
 > Rick Piltz
 > Director, Climate Science Watch
 > 301-807-2472
 > www.climate-science-watch.org
 >
 > Climate Science Watch is a sponsored project of the Government
 > Accountability Project, Washington, DC, dedicated to holding public
 > officials accountable for using climate science and related research
 > effectively and with integrity in responding to the challenges posed
 > by global climate disruption.
 >
 > The right to search for truth implies also a duty; one must not
 > conceal any part of what one has recognized to be true.
 > --Albert Einstein
 >

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 #####

From: Stephen H Schneider <shs@stanford.edu>
 To: Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, trenbert <trenbert@ucar.edu>, Michael Mann <mann@meteo.psu.edu>, Michael Oppenheimer <omichael@Princeton.EDU>
 Subject: Fwd: BBC U-turn on climate
 Date: Sun, 11 Oct 2009 23:32:11 -0700 (PDT)

Hi all. Any of you want to explain decadal natural variability and signal to noise and sampling errors to this new "IPCC Lead Author" from the BBC? As we enter an El Nino year and as soon, as the sunspots get over their temporary--presumed--vacation worth a few tenths of a watt per meter squared reduced forcing, there will likely be another dramatic upward spike like 1992-2000. I heard someone--Mike Schlesinger maybe??--was willing to bet alot of money on it happening in next 5 years?? Meanwhile the past 10 years of global mean temperature trend stasis still saw what, 9 of the warmest in reconstructed 1000 year record and Greenland and the sea ice of the North in big retreat?? Some of you observational folks probably do need to straighten this out as my student suggests below. Such "fun",
 Cheers,
 Steve
 Stephen H. Schneider
 Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies,
 Professor, Department of Biology and
 Senior Fellow, Woods Institute for the Environment
 Mailing address:
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Ph: 650 725 9978
F: 650 725 4387
Websites: climatechange.net
patientfromhell.org

----- Forwarded Message -----
From: "Narasimha D. Rao" <ndrao@stanford.edu>
To: "Stephen H Schneider" <shs@stanford.edu>
Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada Pacific
Subject: BBC U-turn on climate

Steve,

You may be aware of this already. Paul Hudson, BBC's reporter on climate change, on Friday wrote that there's been no warming since 1998, and that pacific oscillations will force cooling for the next 20-30 years. It is not outrageously biased in presentation as are other skeptics's views.

<http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

<http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

BBC has significant influence on public opinion outside the US.

Do you think this merits an op-ed response in the BBC from a scientist?

Narasimha

PhD Candidate,

Emmett Interdisciplinary Program in Environment and Resources (E-IPER)
Stanford University

Tel: 415-812-7560

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#####

From: Kevin Trenberth <trenbert@ucar.edu>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: BBC U-turn on climate
Date: Mon, 12 Oct 2009 08:57:37 -0600
Cc: Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Tom Wigley <>wigley@ucar.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <omichael@Princeton.EDU>

Hi all

mail.2009

well I have my own article on where the heck is global warming? we are asking that here in Boulder where we have broken records the past two days for the coldest days on record. we had 4 inches of snow. The high the last 2 days was below 30F and the normal is 69F, and it smashed the previous records for these days by 10F. The low was about 18F and also a record low, well below the previous record low. This is January weather (see the Rockies baseball playoff game was canceled on Saturday and then played last night in below freezing weather).

Trenberth, K. E., 2009: An imperative for climate change planning: tracking Earth's global energy. Current Opinion in Environmental Sustainability, 1, 19-27, doi:10.1016/j.cosust.2009.06.001. [1][PDF] (A PDF of the published version can be obtained from the author.)

The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't. The CERES data published in the August BAMS 09 supplement on 2008 shows there should be even more warming: but the data are surely wrong. Our observing system is inadequate.

That said there is a LOT of nonsense about the PDO. People like CPC are tracking PDO on a monthly basis but it is highly correlated with ENSO. Most of what they are seeing is the change in ENSO not real PDO. It surely isn't decadal. The PDO is already reversing with the switch to El Nino. The PDO index became positive in September for first time since Sept 2007. see

[2]http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt
Kevin
Michael Mann wrote:

extremely disappointing to see something like this appear on BBC. its particularly odd, since climate is usually Richard Black's beat at BBC (and he does a great job). from what I can tell, this guy was formerly a weather person at the Met Office.

We may do something about this on RealClimate, but meanwhile it might be appropriate for the Met Office to have a say about this, I might ask Richard Black what's up here?

mike

On Oct 12, 2009, at 2:32 AM, Stephen H Schneider wrote:

Hi all. Any of you want to explain decadal natural variability and signal to noise and sampling errors to this new "IPCC Lead Author" from the BBC? As we enter an El Nino year and as soon, as the sunspots get over their temporary--presumed--vacation worth a few

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tenths of a watt per meter squared reduced forcing, there will likely be another dramatic upward spike like 1992-2000. I heard someone--Mike Schlesinger maybe??--was willing to bet a lot of money on it happening in next 5 years?? Meanwhile the past 10 years of global mean temperature trend stasis still saw what, 9 of the warmest in reconstructed 1000 year record and Greenland and the sea ice of the North in big retreat?? Some of you observational folks probably do need to straighten this out as my student suggests below. Such "fun", Cheers,

Steve

Stephen H. Schneider

Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies,
Professor, Department of Biology and
Senior Fellow, Woods Institute for the Environment

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Websites: climatechange.net

patientfromhell.org

----- Forwarded Message -----

From: "Narasimha D. Rao" <[\[3\]ndrao@stanford.edu](mailto:[3]ndrao@stanford.edu)>

To: "Stephen H Schneider" <[\[4\]shs@stanford.edu](mailto:[4]shs@stanford.edu)>

Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada Pacific

Subject: BBC U-turn on climate

Steve,

You may be aware of this already. Paul Hudson, BBC's reporter on climate change, on Friday wrote that there's been no warming since 1998, and that pacific oscillations will force cooling for the next 20-30 years. It is not outrageously biased in presentation as are other skeptics' views.

[5]<http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

[6]<http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

BBC has significant influence on public opinion outside the US.

Do you think this merits an op-ed response in the BBC from a scientist?

Narasimha

PhD Candidate,

Emmett Interdisciplinary Program in Environment and Resources (E-IPER)

Stanford University

Tel: 415-812-7560

--

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Michael E. Mann
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: [7]mann@psu.edu
University Park, PA 16802-5013
website: [8]http://www.meteo.psu.edu/~mann/Mann/index.html
"Dire Predictions" book site:
[9]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

--

Kevin E. Trenberth e-mail: [10]trenbert@ucar.edu
Climate Analysis Section, [11]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

1. <http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/EnergyDiagnostics09final.pdf>
2. http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt
3. <mailto:ndrao@stanford.edu>
4. <mailto:shs@stanford.edu>
5. <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>
6. <http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>
7. <mailto:mann@psu.edu>
8. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
9. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
10. <mailto:trenbert@ucar.edu>
11. <http://www.cgd.ucar.edu/cas/trenbert.html>

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#####

From: Michael Mann <mann@meteo.psu.edu>
To: Stephen H Schneider <shs@stanford.edu>
Subject: Re: BBC U-turn on climate
Date: Mon, 12 Oct 2009 09:00:44 -0400
Cc: Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, trenbert <trenbert@ucar.edu>, Michael Oppenheimer <omichael@Princeton.EDU>

extremely disappointing to see something like this appear on BBC. its particularly odd, since climate is usually Richard Black's beat at BBC (and he does a great job). from what I can tell, this guy was formerly a weather person at the Met Office.

We may do something about this on RealClimate, but meanwhile it might be appropriate for

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the Met Office to have a say about this, I might ask Richard Black what's up here?

mike

On Oct 12, 2009, at 2:32 AM, Stephen H Schneider wrote:

Hi all. Any of you want to explain decadal natural variability and signal to noise and sampling errors to this new "IPCC Lead Author" from the BBC? As we enter an El Nino year and as soon, as the sunspots get over their temporary--presumed--vacation worth a few tenths of a watt per meter squared reduced forcing, there will likely be another dramatic upward spike like 1992-2000. I heard someone--Mike Schlesinger maybe??--was willing to bet a lot of money on it happening in next 5 years?? Meanwhile the past 10 years of global mean temperature trend stasis still saw what, 9 of the warmest in reconstructed 1000 year record and Greenland and the sea ice of the North in big retreat?? Some of you observational folks probably do need to straighten this out as my student suggests below. Such "fun",
Cheers,

Steve

Stephen H. Schneider
Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies,
Professor, Department of Biology and
Senior Fellow, Woods Institute for the Environment

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473 Via Ortega

Ph: 650 725 9978

F: 650 725 4387

Websites: climatechange.net

patientfromhell.org

----- Forwarded Message -----

From: "Narasimha D. Rao" <[\[1\]ndrao@stanford.edu](mailto:[1]ndrao@stanford.edu)>

To: "Stephen H Schneider" <[\[2\]shs@stanford.edu](mailto:[2]shs@stanford.edu)>

Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada Pacific

Subject: BBC U-turn on climate

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[4]<http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

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website: [6]http://www.meteo.psu.edu/~mann/Mann/index.html
"Dire Predictions" book site:
[7]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. <mailto:ndrao@stanford.edu>
2. <mailto:shs@stanford.edu>
3. <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>
- 4.

<http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

5. <mailto:mann@psu.edu>
6. <http://www.meteo.psu.edu/~mann/Mann/index.html>
7. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

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#####

From: Tom Wigley <wigley@ucar.edu>
To: Rick Piltz <piltz@comcast.net>
Subject: Re: FYI--"Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on temperature data record"
Date: Tue, 13 Oct 2009 19:45:45 -0600
Cc: Thomas.R.Karl@noaa.gov, Jim Hansen <jhansen@giss.nasa.gov>, Steve Schneider <shs@stanford.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Kevin Trenberth <trenbert@ucar.edu>, Michael Mann <mann@meteo.psu.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Phil Jones <P.Jones@uea.ac.uk>, Ben Santer <santer1@llnl.gov>

<x-flowed>
Dear folks,

You may be interesting in this snippet of information about Pat Michaels. Perhaps the University of Wisconsin ought to open up a public comment period to decide whether Pat Michaels, PhD needs re-assessing?

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Michaels' PhD was, I believe, supervised by Reid Bryson. It dealt with statistical (regression-based) modeling of crop-climate relationships. In his thesis, Michaels claims that his statistical model showed that weather/climate variations could explain 95% of the inter-annual variability in crop yields. Had this been correct, it would have been a remarkable results. Certainly, it was at odds with all previous studies of crop-climate relationships, which generally showed that weather/climate could only explain about 50% of inter-annual yield variability.

How did result come about? The answer is simple. In Michaels' regressions he included a trend term. This was at the time a common way to account for the effects of changing technology on yield. It turns out that the trend term accounts for 90% of the variability, so that, in Michaels' regressions, weather/climate explains just 5 of the remaining 10%. In other words, Michaels' claim that weather/climate explains 95% of the variability is completely bogus.

Apparently, none of Michaels' thesis examiners noticed this. We are left with wondering whether this was deliberate misrepresentation by Michaels, or whether it was simply ignorance.

As an historical note, I discovered this many years ago when working with Dick Warrick and Tu Qipu on crop-climate modeling. We used a spatial regression method, which we developed for the wheat belt of southwestern Western Australia. We carried out similar analyses for winter wheat in the USA, but never published the results.

Wigley, T.M.L. and Tu Qipu, 1983: Crop-climate modelling using spatial patterns of yield and climate: Part 1, Background and an example from Australia. *Journal of Climate and Applied Meteorology* 22, 1831-1841.

There never was a "Part 2".

Tom

+++++

Rick Piltz wrote:

> Just posted on Climate Science Watch website.

> --RP

>

>

> <http://www.climate-science-watch.org/index.php/csw/details/phil-jones-and-ben-santer-comment-on-cei/>

>

>

> *Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on temperature data record*

>

> /Posted on Tuesday, October 13, 2009

>

> /Prof. Phil Jones, Director of the Climatic Research Unit at the University of East Anglia in the UK and Ben Santer at Lawrence Livermore National Laboratory comment in response to a petition to EPA by the Competitive Enterprise Institute and Pat Michaels, which misleadingly seeks to obstruct EPA's process in making an "endangerment" finding on greenhouse gases. This new CEI tactic is to call into question the

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> integrity of the global temperature data record and, by implication, the
> integrity of leading climate scientists.

>
> /E&E News PM/ reported on October 7 (“CLIMATE: Free-market group attacks
> data behind EPA ‘endangerment’ proposal”):

>
> The Competitive Enterprise Institute—a vocal foe of EPA’s efforts to
> finalize its “endangerment finding”—petitioned the agency this week
> to reopen the public comment period on the proposal, arguing that
> critical data used to formulate the plan have been destroyed and
> that the available data are therefore unreliable.

> At issue is a set of raw data from the Climatic Research Unit at the
> University of East Anglia in Norwich, England, that includes surface
> temperature averages from weather stations around the world...

> Republican senators also weighed in yesterday, urging EPA to reopen
> the public comment period on the endangerment finding to investigate
> the scientific merit of the research data...

>
> We talked with E&E News on this latest maneuver by the ideologues at CEI
> and contrarian scientist Pat Michaels and posted on October 8

>
> <<http://www.climate-science-watch.org/index.php/csw/details/cei-epa-endangerment-petition-oct09/>>:

> “CEI global warming denialists try another gambit seeking to derail EPA
> ‘endangerment’ finding”

>
> The process initiated by the CEI petition will, we suppose, produce an
> appropriate response for the record from EPA and relevant members of the
> science community. And while that process drags on, CEI and Michaels no
> doubt will use their petition as a basis for attempting to muddy the
> waters of scientific discourse, while slinging leaders of the
> international climate science community and questioning their motives.

>
> A few of those leaders have begun to comment on this attempt. We post
> below comments Climate Science Watch has received from Ben Santer at
> Lawrence Livermore National Laboratory and Prof. Phil Jones, Director of
> the Climatic Research Unit at the University of East Anglia in the UK:

>
> Comment by Benjamin D. Santer
> <<http://www-pcmdi.llnl.gov/about/staff/Santer/index.php>>, Program for
> Climate Model Diagnosis and Intercomparison, Lawrence Livermore National
> Laboratory:

>
> As I see it, there are two key issues here.
> First, the Competitive Enterprise Institute (CEI) and Pat Michaels
> are arguing that Phil Jones and colleagues at the Climatic Research
> Unit at the University of East Anglia (CRU) willfully,
> intentionally, and suspiciously “destroyed” some of the raw surface
> temperature data used in the construction of the gridded surface
> temperature datasets.

> Second, the CEI and Pat Michaels contend that the CRU surface
> temperature datasets provided the sole basis for IPCC “discernible
> human influence” conclusions.

> Both of these arguments are incorrect. First, there was no
> intentional destruction of the primary source data. I am sure that,
> over 20 years ago, the CRU could not have foreseen that the raw
> station data might be the subject of legal proceedings by the CEI
> and Pat Michaels. Raw data were NOT secretly destroyed to avoid
> efforts by other scientists to replicate the CRU and Hadley
> Centre-based estimates of global-scale changes in near-surface
> temperature. In fact, a key point here is that other
> groups—primarily at the NOAA National Climatic Data Center (NCDC)
> and at the NASA Goddard Institute for Space Studies (GISS), but also

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> in Russia-WERE able to replicate the major findings of the CRU and
> UK Hadley Centre groups. The NCDC and GISS groups performed this
> replication completely independently. They made different choices in
> the complex process of choosing input data, adjusting raw station
> data for known inhomogeneities (such as urbanization effects,
> changes in instrumentation, site location, and observation time),
> and gridding procedures. NCDC and GISS-based estimates of global
> surface temperature changes are in good accord with the HadCRUT data
> results.

> The second argument-that "discernible human influence" findings are
> like a house of cards, resting solely on one observational
> dataset-is also invalid. The IPCC Third Assessment Report (TAR)
> considers MULTIPLE observational estimates of global-scale
> near-surface temperature changes. It does not rely on HadCRUT data
> alone-as is immediately obvious from Figure 2.1b of the TAR, which
> shows CRU, NCDC, and GISS global-mean temperature changes.

> As pointed out in numerous scientific assessments (e.g., the IPCC
> TAR and Fourth Assessment Reports, the U.S. Climate Change Science
> Program Synthesis and Assessment Report 1.1 (Temperature trends in
> the lower atmosphere: Steps for understanding and reconciling
> differences), and the state of knowledge report, Global Climate
> Change Impacts on the United States, rigorous statistical
> fingerprint studies have now been performed with a whole range of
> climate variables-and not with surface temperature only. Examples
> include variables like ocean heat content, atmospheric water vapor,
> surface specific humidity, continental river runoff, sea-level
> pressure patterns, stratospheric and tropospheric temperature,
> tropopause height, zonal-mean precipitation over land, and Arctic
> sea-ice extent. The bottom-line message from this body of work is
> that natural causes alone CANNOT plausibly explain the climate
> changes we have actually observed. The climate system is telling us
> an internally- and physically-consistent story. The integrity and
> reliability of this story does NOT rest on a single observational
> dataset, as Michaels and the CEI incorrectly claim.

> I have known Phil for most of my scientific career. He is the
> antithesis of the secretive, "data destroying" character the CEI and
> Michaels are trying to portray to the outside world. Phil and Tom
> Wigley have devoted significant portions of their scientific careers
> to the construction of the land surface temperature component of the
> HadCRUT dataset. They have conducted this research in a very open
> and transparent manner-examining sensitivities to different gridding
> algorithms, different ways of adjusting for urbanization effects,
> use of various subsets of data, different ways of dealing with
> changes in spatial coverage over time, etc. They have thoroughly and
> comprehensively documented all of their dataset construction
> choices. They have done a tremendous service to the scientific
> community-and to the planet-by making gridded surface temperature
> datasets available for scientific research. They deserve medals-not
> the kind of deliberately misleading treatment they are receiving
> from Pat Michaels and the CEI.

> (Santer has received several honors, awards and fellowships including
> the Department of Energy Distinguished Scientist Fellowship
> <https://publicaffairs.llnl.gov/news/news_releases/2005/NR-05-10-01.html>,
> the E.O. Lawrence Award, and the "Genius Award" by the MacArthur
> Foundation.)

> Comment by Prof. Phil Jones
> <<http://www.cru.uea.ac.uk/cru/people/pjones/>>, Director, Climatic
> Research Unit (CRU), and Professor, School of Environmental Sciences,
> University of East Anglia, Norwich, UK:

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> No one, it seems, cares to read what we put up
> <<http://www.cru.uea.ac.uk/cru/data/temperature/>> on the CRU web
> page. These people just make up motives for what we might or might
> not have done.
> Almost all the data we have in the CRU archive is exactly the same
> as in the Global Historical Climatology Network (GHCN) archive used
> by the NOAA National Climatic Data Center [see here
> <<http://www.ncdc.noaa.gov/oa/climate/ghcn-monthly/index.php>> and
> here <<http://www.ncdc.noaa.gov/oa/climate/research/ghcn/ghcngrid.html>>].
> The original raw data are not "lost." I could reconstruct what we
> had from U.S. Department of Energy reports we published in the
> mid-1980s. I would start with the GHCN data. I know that the effort
> would be a complete waste of time, though. I may get around to it
> some time. The documentation of what we've done is all in the
> literature.
> If we have "lost" any data it is the following:
> 1. Station series for sites that in the 1980s we deemed then to be
> affected by either urban biases or by numerous site moves, that were
> either not correctable or not worth doing as there were other series
> in the region.
> 2. The original data for sites for which we made appropriate
> adjustments in the temperature data in the 1980s. We still have our
> adjusted data, of course, and these along with all other sites that
> didn't need adjusting.
> 3. Since the 1980s as colleagues and National Meteorological
> Services <http://www.wmo.int/pages/members/index_en.html> (NMSS)
> have produced adjusted series for regions and or countries, then we
> replaced the data we had with the better series.
> In the papers, I've always said that homogeneity adjustments are
> best produced by NMSS. A good example of this is the work by Lucie
> Vincent in Canada. Here we just replaced what data we had for the
> 200+ sites she sorted out.
> The CRUTEM3 data for land look much like the GHCN and NASA Goddard
> Institute for Space Studies data
> <<http://data.giss.nasa.gov/gistemp/>> for the same domains.
> Apart from a figure in the IPCC Fourth Assessment Report (AR4)
> showing this, there is also this paper from Geophysical Research
> Letters in 2005 by Russ Vose et al.
>
> <<http://www.climate-sciencewatch.org/file-uploads/Vose-et-al-TempTrends-GRL2005.pdf>>
> Figure 2 is similar to the AR4 plot.
>
> I think if it hadn't been this issue, the Competitive Enterprise
> Institute would have dreamt up something else!
>

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From: Tom Wigley <wigley@ucar.edu>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: BBC U-turn on climate
Date: Wed, 14 Oct 2009 01:01:24 -0600
Cc: Michael Mann <manna@meteo.psu.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <g Schmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <omichael@Princeton.EDU>

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<x-flowed>
Dear all,

At the risk of overload, here are some notes of mine on the recent lack of warming. I look at this in two ways. The first is to look at the difference between the observed and expected anthropogenic trend relative to the pdf for unforced variability. The second is to remove ENSO, volcanoes and TSI variations from the observed data.

Both methods show that what we are seeing is not unusual. The second method leaves a significant warming over the past decade.

These sums complement Kevin's energy work.

Kevin says ... "The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't". I do not agree with this.

Tom.

+++++

Kevin Trenberth wrote:

> Hi all
> Well I have my own article on where the heck is global warming? We are
> asking that here in Boulder where we have broken records the past two
> days for the coldest days on record. We had 4 inches of snow. The high
> the last 2 days was below 30F and the normal is 69F, and it smashed the
> previous records for these days by 10F. The low was about 18F and also
> a record low, well below the previous record low. This is January
> weather (see the Rockies baseball playoff game was canceled on Saturday
> and then played last night in below freezing weather).
>
> Trenberth, K. E., 2009: An imperative for climate change planning:
> tracking Earth's global energy. /Current Opinion in Environmental
> Sustainability/, *1*, 19-27, doi:10.1016/j.cosust.2009.06.001. [PDF]
>
<<http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/EnergyDiagnostics09final.pdf>
>
> (A PDF of the published version can be obtained from the author.)
>
> The fact is that we can't account for the lack of warming at the moment
> and it is a travesty that we can't. The CERES data published in the
> August BAMS 09 supplement on 2008 shows there should be even more
> warming: but the data are surely wrong. Our observing system is inadequate.
>
> That said there is a LOT of nonsense about the PDO. People like CPC are
> tracking PDO on a monthly basis but it is highly correlated with ENSO.
> Most of what they are seeing is the change in ENSO not real PDO. It
> surely isn't decadal. The PDO is already reversing with the switch to
> El Nino. The PDO index became positive in September for first time
> since Sept 2007. see
>
http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt
>
> Kevin
>

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> Michael Mann wrote:

>> extremely disappointing to see something like this appear on BBC. its
>> particularly odd, since climate is usually Richard Black's beat at BBC
>> (and he does a great job). from what I can tell, this guy was formerly
>> a weather person at the Met Office.

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>> We may do something about this on RealClimate, but meanwhile it might
>> be appropriate for the Met Office to have a say about this, I might
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>>

>> mike

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>>>

>>>

>>> Stephen H. Schneider
>>> Melvin and Joan Lane Professor for Interdisciplinary Environmental
>>> Studies,

>>> Professor, Department of Biology and
>>> Senior Fellow, Woods Institute for the Environment

>>> Mailing address:

>>> Yang & Yamazaki Environment & Energy Building - MC 4205

>>> 473 Via Ortega

>>> Ph: 650 725 9978

>>> F: 650 725 4387

>>> Websites: climatechange.net

>>> patientfromhell.org

>>>

>>>

>>> ----- Forwarded Message -----

>>> From: "Narasimha D. Rao" <ndrao@stanford.edu <mailto:ndrao@stanford.edu>>

>>> To: "Stephen H Schneider" <shs@stanford.edu <mailto:shs@stanford.edu>>

>>> Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada Pacific

>>> Subject: BBC U-turn on climate

>>>

>>> Steve,

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>>>

>>>

>>>

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>>>

>>> <http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

>>>

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

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#####

From: Kevin Trenberth <trenbert@ucar.edu>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: BBC U-turn on climate
Date: wed, 14 Oct 2009 08:36:36 -0600
Cc: Tom Wigley <wigley@ucar.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <omichael@Princeton.EDU>

Mike

Here are some of the issues as I see them:

Saying it is natural variability is not an explanation. What are the physical processes?
Where did the heat go? We know there is a build up of ocean heat prior to El Nino, and a discharge (and sfc T warming) during late stages of El Nino, but is the observing system sufficient to track it? Quite aside from the changes in the ocean, we know there are major changes in the storm tracks and teleconnections with ENSO, and there is a LOT more rain on land during La Nina (more drought in El Nino), so how does the albedo change overall (changes in cloud)? At the very least the extra rain on land means a lot more heat goes into evaporation rather than raising temperatures, and so that keeps land temps down: and should generate cloud. But the resulting evaporative cooling means the heat goes into atmosphere and should be radiated to space: so we should be able to track it with CERES data. The CERES data are unfortunately wanting and so too are the cloud data. The ocean data are also lacking although some of that may be related to the ocean current changes and burying heat at depth where it is not picked up. If it is sequestered at depth then it comes back to haunt us later and so we should know about it.

Kevin

Michael Mann wrote:

Kevin, that's an interesting point. As the plot from Gavin I sent shows, we can easily account for the observed surface cooling in terms of the natural variability seen in the CMIP3 ensemble (i.e. the observed cold dip falls well within it). So in that sense, we can "explain" it. But this raises the interesting question, is there something going

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thanks Kevin, yes, it's a matter of what question one is asking. to argue that the observed global mean temperature anomalies of the past decade falsifies the model projections of global mean temperature change, as contrarians have been fond of claiming, is clearly wrong. but that doesn't mean we can explain exactly what's going on. there is always the danger of falling a bit into the "we don't know everything, so we know nothing" fallacy. hence, I wanted to try to clarify where we all agree, and where there may be disagreement,

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Here are some of the issues as I see them:

Saying it is natural variability is not an explanation. what are the physical processes? where did the heat go? we know there is a build up of ocean heat prior to El Nino, and a discharge (and sfc T warming) during late stages of El Nino, but is the observing system sufficient to track it? Quite aside from the changes in the ocean, we know there are major changes in the storm tracks and teleconnections with ENSO, and there is a LOT more rain on land during La Nina (more drought in El Nino), so how does the albedo change overall (changes in cloud)? At the very least the extra rain on land means a lot more heat goes into evaporation rather than raising temperatures, and so that keeps land temps down: and should generate cloud. But the resulting evaporative cooling means the heat goes into atmosphere and should be radiated to space: so we should be able to track it with CERES data. The CERES data are unfortunately wanting and so too are the cloud data. The ocean data are also lacking although some of that may be related to the ocean current changes and burying heat at depth where it is not picked up. If it is sequestered at depth then it comes back to haunt us later and so we should know about it.

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presentation as
You may be aware of this already. Paul Hudson, BBCs reporter on climate change,
Friday wrote that theres been no warming since 1998, and that pacific
force cooling for the next 20-30 years. It is not outrageously biased in
are other skeptics views.

[7]<http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

[8][http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-tur
n-on-climate-change/](http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/)

BBC has significant influence on public opinion outside the US.

Do you think this merits an op-ed response in the BBC from a scientist?

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Narasimha

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<[12]<http://www.meteo.psu.edu/%7Emann/Mann/index.html>>

"Dire Predictions" book site:
[13]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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"Dire Predictions" book site:
[25]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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2. http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt
3. <mailto:ndrao@stanford.edu>
4. <mailto:ndrao@stanford.edu>
5. <mailto:shs@stanford.edu>
6. <mailto:shs@stanford.edu>
7. <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>
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9. <mailto:mann@psu.edu>
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12. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
13. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
14. <mailto:trenbert@ucar.edu>
15. <http://www.cgd.ucar.edu/cas/trenbert.html>
16. <mailto:trenbert@ucar.edu>
17. <http://www.cgd.ucar.edu/cas/trenbert.html>
18. <mailto:mann@psu.edu>
19. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
20. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
21. <mailto:trenbert@ucar.edu>
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23. <mailto:mann@psu.edu>

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- 24. <http://www.meteo.psu.edu/~mann/Mann/index.html>
- 25. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

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

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From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: FYI--"Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on temperature data record"
Date: Wed Oct 14 12:41:21 2009
Cc: Ben Santer <santer1@llnl.gov>

Tom,

what you'd need to point this out is a pdf of his thesis! Or is there a paper where the thesis is referred to?

I recall Pat wasn't very good at writing stuff up. There was one paper about warming in Alaska that I recall either you or me reviewing. It related to surface warming in Alaska

and the borehole from Lachenbruch/Marshall (?) from about 1986.

With the pdf you wouldn't need to say that much, as it is as you say stupid to leave the

Trend in with the rest of the variance.

Did the NCDC info help you sort out that data. Tom P told me that they don't infill

certain areas in early decades, so there is missing data. Tom P isn't that keen on the

method. He rightly thinks that it discourages them from looking for early data or including

any new stuff they get - as they have infilled it, so it won't make a difference. It won't

make a difference, but that isn't the point.

Cheers

Phil

At 02:45 14/10/2009, Tom wigley wrote:

Dear folks,

You may be interesting in this snippet of information about Pat Michaels. Perhaps the University of Wisconsin ought to open up a public comment period to decide whether Pat Michaels, PhD needs re-assessing?

Michaels' PhD was, I believe, supervised by Reid Bryson. It dealt with statistical (regression-based) modeling of crop-climate relationships. In his thesis, Michaels claims that his statistical model showed that weather/climate variations could explain 95% of the inter-annual variability in crop yields. Had this been correct, it would have been a remarkable results. Certainly, it was at odds with all previous studies of crop-climate relationships, which generally showed that weather/climate could only explain about 50% of inter-annual yield variability.

How did result come about? The answer is simple. In Michaels' regressions he included a trend term. This was at the time a common way to account for the effects of changing technology on yield. It turns out that the trend term accounts for 90% of the variability, so that, in Michaels' regressions, weather/climate explains just 5 of the remaining 10%. In other words, Michaels' claim that

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weather/climate explains 95% of the variability is completely bogus.

Apparently, none of Michaels' thesis examiners noticed this. We are left with wondering whether this was deliberate misrepresentation by Michaels, or whether it was simply ignorance.

As an historical note, I discovered this many years ago when working with Dick Warrick and Tu Qipu on crop-climate modeling. We used a spatial regression method, which we developed for the wheat belt of southwestern Western Australia. We carried out similar analyses for winter wheat in the USA, but never published the results.

Wigley, T.M.L. and Tu Qipu, 1983: Crop-climate modelling using spatial patterns of yield and climate: Part 1, Background and an example from Australia. Journal of Climate and Applied Meteorology 22, 1831-1841. There never was a "Part 2".

Tom

+++++

Rick Piltz wrote:

Just posted on Climate Science Watch website.

--RP

[1]<http://www.climate-science-watch.org/index.php/csw/details/phil-jones-and-ben-santer-co>

comment-on-cei/

Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on temperature data record

/Posted on Tuesday, October 13, 2009

/Prof. Phil Jones, Director of the Climatic Research Unit at the University of

East

Anglia in the UK and Ben Santer at Lawrence Livermore National Laboratory

comment in

response to a petition to EPA by the Competitive Enterprise Institute and Pat

Michaels,

which misleadingly seeks to obstruct EPA's process in making an endangerment

finding on

greenhouse gases. This new CEI tactic is to call into question the integrity

of the

global temperature data record and, by implication, the integrity of leading

climate

scientists.

/E&E News PM/ reported on October 7 (CLIMATE: Free-market group attacks data

behind EPA

endangerment proposal):

The Competitive Enterprise Institute, a vocal foe of EPA's efforts to finalize its endangerment finding, petitioned the agency this week to reopen the public comment period on the proposal, arguing that critical data used to formulate the plan have been destroyed and that the available data are therefore unreliable.

At issue is a set of raw data from the Climatic Research Unit at the University of East Anglia in Norwich, England, that includes surface temperature averages from weather stations around the world.

Republican senators also weighed in yesterday, urging EPA to reopen the public comment period on the endangerment finding to investigate the scientific merit of the research data.

We talked with E&E News on this latest maneuver by the ideologues at CEI and

contrarian

scientist Pat Michaels and posted on October 8

<[2]<http://www.climate-science-watch.org/index.php/csw/details/cei-epa-endangerment-petition>

on-oct09/>: CEI global warming denialists try another gambit seeking to derail

EPA

endangerment finding

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The process initiated by the CEI petition will, we suppose, produce an appropriate response for the record from EPA and relevant members of the science community.

And while that process drags on, CEI and Michaels no doubt will use their petition as a basis for attempting to muddy the waters of scientific discourse, while sliming leaders of the international climate science community and questioning their motives. A few of those leaders have begun to comment on this attempt. We post below

comments
Climate Science Watch has received from Ben Santer at Lawrence Livermore National

Laboratory and Prof. Phil Jones, Director of the Climatic Research Unit at the University of East Anglia in the UK:

Comment by Benjamin D. Santer

<[\[3\]http://www-pcmdi.llnl.gov/about/staff/Santer/index.php](http://www-pcmdi.llnl.gov/about/staff/Santer/index.php)>, Program for Climate Model

Diagnosis and Intercomparison, Lawrence Livermore National Laboratory:

As I see it, there are two key issues here.

First, the Competitive Enterprise Institute (CEI) and Pat Michaels are arguing that Phil Jones and colleagues at the Climatic Research Unit at the University of East Anglia (CRU) willfully, intentionally, and suspiciously destroyed some of the raw surface temperature data used in the construction of the gridded surface temperature datasets.

Second, the CEI and Pat Michaels contend that the CRU surface temperature datasets provided the sole basis for IPCC discernible human influence conclusions.

Both of these arguments are incorrect. First, there was no intentional destruction of the primary source data. I am sure that, over 20 years ago, the CRU could not have foreseen that the raw station data might be the subject of legal proceedings by the CEI and Pat Michaels. Raw data were NOT secretly destroyed to avoid efforts by other scientists to replicate the CRU and Hadley Centre-based estimates of global-scale changes in near-surface temperature. In fact, a key point here is that other groups primarily at the NOAA National Climatic Data Center (NCDC) and at the NASA Goddard Institute for Space Studies (GISS), but also in Russia were able to replicate the major findings of the CRU and UK Hadley Centre groups. The NCDC and GISS groups performed this replication completely independently. They made different choices in the complex process of choosing input data, adjusting raw station data for known inhomogeneities (such as urbanization effects, changes in instrumentation, site location, and observation time), and gridding procedures. NCDC and GISS-based estimates of global surface temperature changes are in good accord with the HadCRUT data results.

The second argument that discernible human influence findings are like a house of cards, resting solely on one observational dataset is also invalid. The IPCC Third Assessment Report (TAR) considers MULTIPLE observational estimates of global-scale near-surface temperature changes. It does not rely on HadCRUT data alone as is immediately obvious from Figure 2.1b of the TAR, which shows CRU, NCDC, and GISS global-mean temperature changes.

As pointed out in numerous scientific assessments (e.g., the IPCC TAR and Fourth Assessment Reports, the U.S. Climate Change Science Program Synthesis and Assessment Report 1.1 (Temperature trends in the lower atmosphere: Steps for understanding and reconciling differences), and the state of knowledge report, Global Climate Change Impacts on the United States, rigorous statistical fingerprint studies have now been performed with a whole range of climate variables and not with surface temperature only. Examples

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include variables like ocean heat content, atmospheric water vapor, surface specific humidity, continental river runoff, sea-level pressure patterns, stratospheric and tropospheric temperature, tropopause height, zonal-mean precipitation over land, and Arctic sea-ice extent. The bottom-line message from this body of work is that natural causes alone CANNOT plausibly explain the climate changes we have actually observed. The climate system is telling us an internally- and physically-consistent story. The integrity and reliability of this story does NOT rest on a single observational dataset, as Michaels and the CEI incorrectly claim.

I have known Phil for most of my scientific career. He is the antithesis of the secretive, data destroying character the CEI and Michaels are trying to portray to the outside world. Phil and Tom Wigley have devoted significant portions of their scientific careers to the construction of the land surface temperature component of the HadCRUT dataset. They have conducted this research in a very open and transparent manner examining sensitivities to different gridding algorithms, different ways of adjusting for urbanization effects, use of various subsets of data, different ways of dealing with changes in spatial coverage over time, etc. They have thoroughly and comprehensively documented all of their dataset construction choices. They have done a tremendous service to the scientific community and to the planet by making gridded surface temperature datasets available for scientific research. They deserve medals not the kind of deliberately misleading treatment they are receiving from Pat Michaels and the CEI.

(Santer has received several honors, awards and fellowships including the Department of

Energy Distinguished Scientist Fellowship

<[4]https://publicaffairs.llnl.gov/news/news_releases/2005/NR-05-10-01.html>, the E.O.

Lawrence Award, and the Genius Award by the MacArthur Foundation.)

Comment by Prof. Phil Jones <[5]<http://www.cru.uea.ac.uk/cru/people/pjones/>>, Director,

Climatic Research Unit (CRU), and Professor, School of Environmental Sciences, University of East Anglia, Norwich, UK:

No one, it seems, cares to read what we put up

<[6]<http://www.cru.uea.ac.uk/cru/data/temperature/>> on the CRU web page. These people just make up motives for what we might or might not have done.

Almost all the data we have in the CRU archive is exactly the same as in the Global Historical Climatology Network (GHCN) archive used by the NOAA National Climatic Data Center [see here

<[7]<http://www.ncdc.noaa.gov/oa/climate/ghcn-monthly/index.php>> and here <[8]<http://www.ncdc.noaa.gov/oa/climate/research/ghcn/ghcngrid.html>>].

The original raw data are not lost. I could reconstruct what we had from U.S. Department of Energy reports we published in the mid-1980s. I would start with the GHCN data. I know that the effort would be a complete waste of time, though. I may get around to it some time. The documentation of what we've done is all in the literature.

If we have lost any data it is the following:

1. Station series for sites that in the 1980s we deemed then to be affected by either urban biases or by numerous site moves, that were either not correctable or not worth doing as there were other series in the region.

2. The original data for sites for which we made appropriate adjustments in the temperature data in the 1980s. We still have our adjusted data, of course, and these along with all other sites that didn't need adjusting.

3. Since the 1980s as colleagues and National Meteorological Services <[9]http://www.wmo.int/pages/members/index_en.html> (NMSS) have produced adjusted series for regions and or countries, then we

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replaced the data we had with the better series.

In the papers, Ive always said that homogeneity adjustments are best produced by NMSS. A good example of this is the work by Lucie Vincent in Canada. Here we just replaced what data we had for the 200+ sites she sorted out.

The CRUTEM3 data for land look much like the GHCN and NASA Goddard Institute for Space Studies data

<[10]http://data.giss.nasa.gov/gistemp/> for the same domains. Apart

from a

figure in the IPCC Fourth Assessment Report (AR4)

showing this, there is also this paper from Geophysical Research Letters in 2005 by Russ Vose et al.

<[11]http://www.climate-science-watch.org/file-uploads/Vose-et-al-TempTrends-GRL2005.pdf>

Figure 2 is similar to the AR4 plot.

I think if it hadnt been this issue, the Competitive Enterprise Institute would have dreamt up something else!

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

References

1.

http://www.climate-science-watch.org/index.php/csw/details/phil-jones-and-ben-santer-comment-on-cei/

2.

http://www.climate-science-watch.org/index.php/csw/details/cei-epa-endangerment-petition-oct09/

3. http://www-pcmdi.llnl.gov/about/staff/Santer/index.php

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9. http://www.wmo.int/pages/members/index_en.html

10. http://data.giss.nasa.gov/gistemp/

11.

http://www.climate-science-watch.org/file-uploads/Vose-et-al-TempTrends-GRL2005.pdf%3E%A0%A0%A0%A0

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From: Tom Wigley <wigley@ucar.edu>

To: Kevin Trenberth <trenbert@ucar.edu>

Subject: Re: BBC U-turn on climate

Date: wed, 14 Oct 2009 16:09:35 -0600

Cc: Michael Mann <mann@meteo.psu.edu>, Stephen H Schneider <shs@stanford.edu>, Myles

Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D.

Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl

<Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen

<jhansen@giss.nasa.gov>, Michael Oppenheimer <omichael@Princeton.EDU>

<x-flowed>
Kevin,

I didn't mean to offend you. But what you said was "we can't account for the lack of warming at the moment". Now you say "we are no where close to knowing where energy is going". In my eyes these are two different things -- the second relates to our level of understanding, and I agree that this is still lacking.

Tom.

+++++

Kevin Trenberth wrote:

> Hi Tom
> How come you do not agree with a statement that says we are no where
> close to knowing where energy is going or whether clouds are changing to
> make the planet brighter. We are not close to balancing the energy
> budget. The fact that we can not account for what is happening in the
> climate system makes any consideration of geoengineering quite hopeless
> as we will never be able to tell if it is successful or not! It is a
> travesty!
> Kevin

>
> Tom Wigley wrote:

>> Dear all,
>>
>> At the risk of overload, here are some notes of mine on the recent
>> lack of warming. I look at this in two ways. The first is to look at
>> the difference between the observed and expected anthropogenic trend
>> relative to the pdf for unforced variability. The second is to remove
>> ENSO, volcanoes and TSI variations from the observed data.
>>
>> Both methods show that what we are seeing is not unusual. The second
>> method leaves a significant warming over the past decade.
>>
>> These sums complement Kevin's energy work.

>>
>> Kevin says ... "The fact is that we can't account for the lack of
>> warming at the moment and it is a travesty that we can't". I do not
>> agree with this.

>> Tom.

>>
>> +++++

>> Kevin Trenberth wrote:

>>> Hi all
>>> Well I have my own article on where the heck is global warming? We
>>> are asking that here in Boulder where we have broken records the past
>>> two days for the coldest days on record. We had 4 inches of snow.
>>> The high the last 2 days was below 30F and the normal is 69F, and it
>>> smashed the previous records for these days by 10F. The low was
>>> about 18F and also a record low, well below the previous record low.
>>> This is January weather (see the Rockies baseball playoff game was
>>> canceled on Saturday and then played last night in below freezing
>>> weather).

>>>
>>> Trenberth, K. E., 2009: An imperative for climate change planning:
>>> tracking Earth's global energy. /Current Opinion in Environmental
>>> Sustainability/, *1*, 19-27, doi:10.1016/j.cosust.2009.06.001. [PDF]
>>>

<<http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/EnergyDiagnostics09final.pdf>

>
>>> (A PDF of the published version can be obtained from the author.)

>>>
>>> The fact is that we can't account for the lack of warming at the
>>> moment and it is a travesty that we can't. The CERES data published
>>> in the August BAMS 09 supplement on 2008 shows there should be even
>>> more warming: but the data are surely wrong. Our observing system is
>>> inadequate.

>>>
>>> That said there is a LOT of nonsense about the PDO. People like CPC
>>> are tracking PDO on a monthly basis but it is highly correlated with
>>> ENSO. Most of what they are seeing is the change in ENSO not real
>>> PDO. It surely isn't decadal. The PDO is already reversing with the
>>> switch to El Nino. The PDO index became positive in September for
>>> first time since Sept 2007. see

>>>
>>> http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt

>>>
>>> Kevin

>>>
>>> Michael Mann wrote:

>>>> extremely disappointing to see something like this appear on BBC.
>>>> its particularly odd, since climate is usually Richard Black's beat
>>>> at BBC (and he does a great job). from what I can tell, this guy was
>>>> formerly a weather person at the Met Office.
>>>> We may do something about this on RealClimate, but meanwhile it
>>>> might be appropriate for the Met Office to have a say about this, I
>>>> might ask Richard Black what's up here?

>>>>
>>>> mike

>>>>
>>>> On Oct 12, 2009, at 2:32 AM, Stephen H Schneider wrote:

>>>>>
>>>>> Hi all. Any of you want to explain decadal natural variability and
>>>>> signal to noise and sampling errors to this new "IPCC Lead Author"
>>>>> from the BBC? As we enter an El Nino year and as soon, as the
>>>>> sunspots get over their temporary--presumed--vacation worth a few
>>>>> tenths of a watt per meter squared reduced forcing, there will
>>>>> likely be another dramatic upward spike like 1992-2000. I heard
>>>>> someone--Mike Schlesinger maybe??--was willing to bet alot of money
>>>>> on it happening in next 5 years?? Meanwhile the past 10 years of
>>>>> global mean temperature trend stasis still saw what, 9 of the
>>>>> warmest in reconstructed 1000 year record and Greenland and the sea
>>>>> ice of the North in big retreat?? Some of you observational folks
>>>>> probably do need to straighten this out as my student suggests
>>>>> below. Such "fun", Cheers, Steve

>>>>>
>>>>> Stephen H. Schneider
>>>>> Melvin and Joan Lane Professor for Interdisciplinary Environmental
>>>>> Studies,
>>>>> Professor, Department of Biology and
>>>>> Senior Fellow, Woods Institute for the Environment
>>>>> Mailing address:
>>>>> Yang & Yamazaki Environment & Energy Building - MC 4205
>>>>> 473 Via Ortega
>>>>> Ph: 650 725 9978
>>>>> F: 650 725 4387
>>>>> websites: climatechange.net
>>>>> patientfromhell.org

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>>>>

>>>>

>>>> ----- Forwarded Message -----

>>>> From: "Narasimha D. Rao" <ndrao@stanford.edu

>>>> <mailto:ndrao@stanford.edu>>

>>>> To: "Stephen H Schneider" <shs@stanford.edu <mailto:shs@stanford.edu>>

>>>> Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada

>>>> Pacific

>>>> Subject: BBC U-turn on climate

>>>>

>>>> Steve,

>>>> You may be aware of this already. Paul Hudson, BBC's reporter on

>>>> climate change, on Friday wrote that there's been no warming since

>>>> 1998, and that pacific oscillations will force cooling for the next

>>>> 20-30 years. It is not outrageously biased in presentation as are

>>>> other skeptics' views.

>>>>

>>>>

>>>>

>>>> <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

>>>>

>>>> <http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

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>>>> BBC has significant influence on public opinion outside the US.

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>>>> Do you think this merits an op-ed response in the BBC from a

>>>> scientist?

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>>>> Narasimha

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>>>> Emmett Interdisciplinary Program in Environment and Resources (E-IPER)

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>>>> --
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>>>>

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>>>> website: <http://www.meteo.psu.edu/~mann/Mann/index.html>

>>>> <<http://www.meteo.psu.edu/%7Emann/Mann/index.html>>

>>>> "Dire Predictions" book site:

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> suited to a comparison with a short period of observations. MAGICC is
> great at the longer time scale, but its neglect of unforced variability
> does not make it useful for these kinds of comparison.

>
> The kind of things we are hearing "no model showed a cooling", the "data
> is outside the range of the models" need to be addressed directly.

>
> Gavin

> On wed, 2009-10-14 at 18:06, Michael Mann wrote:

>> Hi Tom,

>>
>> thanks for the comments. well, ok. but this is the full CMIP3
>> ensemble, so at least the plot is sampling the range of choices
>> regarding if and how indirect effects are represented, what the cloud
>> radiative feedback & sensitivity is, etc. across the modeling
>> community. I'm not saying that these things necessarily cancel out
>> (after all, there is an interesting and perhaps somewhat disturbing
>> compensation between indirect aerosol forcing and sensitivity across
>> the CMIP3 models that defies the assumption of independence), but if
>> showing the full spread from CMIP3 is deceptive, its hard to imagine
>> what sort of comparison wouldn't be deceptive (your point re MAGICC
>> notwithstanding),

>>
>> perhaps Gavin has some further comments on this (it is his plot after
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>>
>> mike

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>>>>> Environmental Sustainability/, *1*, 19-27,
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>>>>>>> Stephen H. Schneider
>>>>>>> Melvin and Joan Lane Professor for Interdisciplinary
>>>>>>> Environmental Studies,
>>>>>>> Professor, Department of Biology and
>>>>>>> Senior Fellow, Woods Institute for the Environment
>>>>>>> Mailing address:
>>>>>>> Yang & Yamazaki Environment & Energy Building - MC 4205
>>>>>>> 473 Via Ortega
>>>>>>> Ph: 650 725 9978
>>>>>>> F: 650 725 4387

>>>>>>> Websites: climatechange.net
>>>>>>> patientfromhell.org

>>>>>>> ----- Forwarded Message -----

>>>>>>> From: "Narasimha D. Rao" <ndrao@stanford.edu>
>>>>>>> <mailto:ndrao@stanford.edu>
>>>>>>> To: "Stephen H Schneider" <shs@stanford.edu>
>>>>>>> <mailto:shs@stanford.edu>
>>>>>>> Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00
>>>>>>> US/Canada Pacific
>>>>>>> Subject: BBC U-turn on climate

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>>>>>>> been no warming since 1998, and that pacific oscillations
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>>>>>>> outrageously biased in presentation as are other skeptic's
>>>>>>> views.
>>>>>>> <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

>>>>>>> <http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

>>>>>>> BBC has significant influence on public opinion outside
>>>>>>> the US.
>>>>>>> Do you think this merits an op-ed response in the BBC from
>>>>>>> a scientist?
>>>>>>> Narasimha

>>>>>>> -----
>>>>>>> PhD Candidate,
>>>>>>> Emmett Interdisciplinary Program in Environment and
>>>>>>> Resources (E-IPER)
>>>>>>> Stanford University
>>>>>>> Tel: 415-812-7560

>>>>>>> --
>>>>>>> Michael E. Mann
>>>>>>> Professor
>>>>>>> Director, Earth System Science Center (ESSC)
>>>>>>> Department of Meteorology Phone: (814)
>>>>>>> 863-4075
>>>>>>> 503 Walker Building FAX:
>>>>>>> (814) 865-3663

>>

</x-flowed>

1058. 1255558867.txt

#####

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: BBC U-turn on climate
Date: 14 Oct 2009 18:21:07 -0400
Cc: Tom Wigley <wigley@ucar.edu>, Kevin Trenberth <trenbert@ucar.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, Peter Stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Jim Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <omichael@Princeton.EDU>

Tom, with respect to the difference between the models and the data, the fundamental issue on short time scales is the magnitude of the internal variability. Using the full CMIP3 ensemble at least has multiple individual realisations of that internal variability and so is much more suited to a comparison with a short period of observations. MAGICC is great at the longer time scale, but its neglect of unforced variability does not make it useful for these kinds of comparison.

The kind of things we are hearing "no model showed a cooling", the "data is outside the range of the models" need to be addressed directly.

Gavin

On wed, 2009-10-14 at 18:06, Michael Mann wrote:

> Hi Tom,
>
> thanks for the comments. well, ok. but this is the full CMIP3
> ensemble, so at least the plot is sampling the range of choices
> regarding if and how indirect effects are represented, what the cloud
> radiative feedback & sensitivity is, etc. across the modeling
> community. I'm not saying that these things necessarily cancel out
> (after all, there is an interesting and perhaps somewhat disturbing
> compensation between indirect aerosol forcing and sensitivity across
> the CMIP3 models that defies the assumption of independence), but if
> showing the full spread from CMIP3 is deceptive, its hard to imagine
> what sort of comparison wouldn't be deceptive (your point re MAGICC
> notwithstanding),
>
> perhaps Gavin has some further comments on this (it is his plot after
> all),
>
> mike

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> > > > > > outrageously biased in presentation as are other skeptics

mail.2009

>Marcus has just received this message below from the EDP
>environment correspondent. He is telling her he knows nothing
>about it (true, as he has just returned from China).
>
>I have just dropped a note to the solicitor asking if she sees
>any problem in our warning her to be very cautious in how
>anything is phrased and issuing a statement along the
>following lines. (I think the last line would have to come
>directly from you Keith)
>
>For info, still no response from the Spectator to the letter.
>I have rung three times (first time PA told me message had been
>opened) and emailed. Solicitor is now looking closely at the
>piece in the Spectator to judge whether to send a solicitor's letter.
>Best, Annie
>
>
>Draft statement
>Any implication that Professor Keith Briffa deliberately
>selected tree-ring data in order to manufacture evidence of
>recent dramatic warming in the Yamal region of northern Russia
>is completely false. A full rebuttal is published on the
>Climatic Research Unit's website.
>
>This stems from a report on the Climate Audit blog site - a
>site for climate change sceptics. The blog's editor, Steve
>McIntyre, has produced an alternative history of tree-growth
>changes in the Yamal region by substituting some of the data
>used in Prof Briffa's published and peer-reviewed analysis,
>with recent data from a more localised origin than the data
>analysed by Prof Briffa. While McIntyre's selection produces
>a different result, it cannot be considered to be more authoritative.
>
>This appears to be an attempt to discredit the work of the
>Intergovernmental Panel of Climate Change in the run-up to the
>Copenhagen climate talks.
>
>
>-----
>Annie Ogden, Head of Communications,
>University of East Anglia,
>Norwich, NR4 7TJ.
>Tel:+44 (0)1603 592764
>www.uea.ac.uk/comm
>.....
>
>
>
>-----Original Message-----
>From: Armes Marcus Mr (VCO)
>Sent: Tuesday, October 20, 2009 2:40 PM
>To: Ogden Annie Ms (MAC)
>Subject: FW: Climate Research Centre crisis spreads
>
> Here it is Annie
>
>-----Original Message-----
>From: Greaves, Tara [mailto:Tara.Greaves@archant.co.uk]
>Sent: Tuesday, October 20, 2009 12:11 PM
>To: Armes Marcus Mr (VCO)
>Subject: FW: Climate Research Centre crisis spreads
>
>Also, do you know anything about this?

mail.2009

>
>-----Original Message-----
>From: David_Robinson [mailto:darobin@netcomuk.co.uk]
>Sent: 19 October 2009 22:45
>To: newsdesk@archant.co.uk
>Subject: Climate Research Centre crisis spreads
>
>Sir,
>I draw your attention to the growing international climate
>change scandal that is engulfing the CRU and dragging the
>reputation of it, and Norfolk, through the mud.
>
>After several weeks of open criticism of the use of a
>particular, allegedly flawed, CRU dataset there has been no
>attempted rebuttle by the CRU. Latest information suggests
>that dozens of 'peer reviewed' scientific papers that relied
>on the same dataset are now 'similarly flawed' and should be
>withdrawn. This, unfortunately, draws into question a
>fundamental part of the IPCC conclusion - namely, whether the
>recent global warming is in fact abnormal and hence
>attributable to man.
>
>I think the continued silence by the CRU on this subject
>profoundly worrying given the importance of the topic.
>
>Any light you can shed on this whole sorry story would be
>greatly in the public interest, especially given the
>Copenhagen summit fast approaching.
>
>David Robinson
>
><http://www.climateaudit.org/?p=7374#comments>
>----
>Sent via BlackBerry
>David Robinson MSC
>Blacklock and Bowers Limited
>
>This email and any attachments to it are confidential and
>intended solely for the individual or organisation to whom
>they are addressed.
>You must not copy or retransmit this e-mail or its attachments
>in whole or in part to anyone else without our permission. The
>views expressed in them are those of the individual author and
>do not necessarily represent the views of this Company.
>
>Whilst we would never knowingly transmit anything containing a
>virus we cannot guarantee that this e-mail is virus-free and
>you should take all steps that you can to protect your systems
>against viruses.
>
>Archant Regional Limited, is registered in England under
>Company Registration Number 19300, and the Registered Office
>is Prospect House, Rouen Road, Norwich NR1 1RE.
>
>

1060. 1256302524.txt

#####

From: Kevin Trenberth <trenbert@ucar.edu>
To: James Annan <jdannan@jamstec.go.jp>

mail.2009

Subject: Re: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter
Date: Fri, 23 Oct 2009 08:55:24 -0600
Cc: Jim Salinger <j.salinger@auckland.ac.nz>, Grant Foster <tamino_9@hotmail.com>, Mike Mann <mann@meteo.psu.edu>, p.jones@uea.ac.uk, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, j.renwick@niwa.co.nz

<x-flowed>

Hi James

Thanks for doing this and let's keep it moving as fast as possible. Yes the formatting in places is disconcerting and the line numbering is a bit on and off.

I have suggestions for changing two words.

Line 13 "severely" to "greatly"

Line 79 "more dramatic" to "greater"

As they stand, words like those used carry a lot of extra subjective tone that implies "bad" or has a commentary that is not desirable as per Rev 3. I wonder if you should not be a bit more specific in responding to Rev 3 and say what other words were changed in the abstract at least? If it were "word" I would send in a version of the abstract with tracking on. It might make the difference between having the editor approve it and sending it back to Rev 3.

Best regards
Kevin

James Annan wrote:

> Dear All,
>
> I had a reply from Grant, and have made some changes to the paper -
> very little of substance, but I've lightly edited the wording
> throughout. I also added refs to Newell and Weare, and Angell (not
> A+Korshover), which seem relevant. Despite this, I've managed to cut a
> few lines off in total. I have also drafted replies to the reviewers
> (with their comments appended for reference).
>
> We do have a 2 week extension agreed, to 11 Nov. However it doesn't
> really seem like there is much more that needs doing. More suggestions
> are welcome, however, and before resubmitting, *I need an explicit OK
> from each author*.
>
> James

--

Kevin E. Trenberth
Climate Analysis Section,
NCAR
P. O. Box 3000,
Boulder, CO 80307

e-mail: trenbert@ucar.edu
www.cgd.ucar.edu/cas/trenbert.html
(303) 497 1318
(303) 497 1333 (fax)

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

</x-flowed>

1061. 1256353124.txt

#####

mail.2009

From: Mike Salmon <m.salmon@uea.ac.uk>
To: Mike Salmon <m.salmon@uea.ac.uk>
Subject: Re: Yamal 2009
Date: Fri, 23 Oct 2009 22:58:44 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Tom Melvin <t.m.melvin@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>
I'm not thinking straight. It makes far more sense to have password-protection rather than IP-address protection. So, to access those pages

Username: steve
Password: tosser

Have a good weekend!

Mike

Mike Salmon wrote:

> Figure E added; figure F updated. I still need "ALT" tags for each
> figure. Data page needs a lot of work.
>
> Tim: I understand you're providing a whole new page?
>
> Tom: I definitely don't have the list of references for sensit.htm.
> Please send me the word file or tell me where to look on your PC.
>
> Briffa et al 1996 added to <http://www.cru.uea.ac.uk/cru/people/briffa/>
>
> Access to the Yamal 2009 pages is currently restricted by IP address.
> Try to access them from home, then tell me the time at which you tried.
> I'll pick your IP address out of the logs and add it to the "permitted"
> list.
>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009-temporary/main.htm>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009-temporary/sensit.htm>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009-temporary/data/>
>
> Mike
>
>

</x-flowed>

1062. 1256735067.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: The web page is up about the Yamal tree-ring chronology
Date: Wed Oct 28 09:04:27 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Mike,
Sept 2009 isn't up yet. I expect it in the next day or so. I'll check again tomorrow.
Away Friday and Mon/Tues next week. Our web site will update on Sunday if the HC have

mail.2009

updated theirs.

Seems nothing yet on Keith's Yamal.

One of the Russians has a reason why Khad hasn't grown so much. All the sites in the region have permafrost at depth. Those nearer the rivers have the permafrost at a greater depth, partly due to the rivers. Warmth in the 20th century has meant greater depths for the roots. Khad is a walk from the river and slightly higher, so possibly has less available soil depth above the permafrost. All the sites are sampled through river transport. When the coring was done in the 1980s and early 1990s the fieldwork teams ate a lot of fish!

Permafrost idea is impossible to prove without going back to the sites and drilling down.

The Russians plan to do this when they revisit the area, but that depends on resources.

Cheers

Phil

Cheers

Phil

At 17:07 27/10/2009, Michael Mann wrote:

Hi Phil,

Thanks--we know that. The point is simply that if we want to talk about a meaningful "2009" anomaly, every additional month that is available from which

to

calculate an annual mean makes the number more credible. We already have this

for

GISTEMP, but have been awaiting HadCRU to be able to do a more decisive update

of the

status of the disingenuous "globe is cooling" contrarian talking point,

Mike

you

p.s. be a bit careful about what information you send to Andy and what emails

copy

him in on. He's not as predictable as we'd like

On Oct 27, 2009, at 1:04 PM, Phil Jones wrote:

Mike,

Yes a link will be fine.

months.

I'll look into Sept numbers, but you shouldn't be looking at individual

Cheers

Phil

At 16:54 27/10/2009, Michael Mann wrote:

thanks Phil,

Perhaps we'll do a simple update to the Yamal post, e.g. linking Keith's new page--Gavin t?

series,

As to the issues of robustness, particularly w.r.t. inclusion of the Yamal

in our

actually emphasized that (including the Osborn and Briffa '06 sensitivity test)

plausibly

original post! As we all know, this isn't about truth at all, it's about

deniable accusations,

m

p.s. any word on HadCRU Sep numbers yet???

On Oct 27, 2009, at 12:37 PM, Phil Jones wrote:

Gavin, Mike, Andy,

mail.2009

It has taken Keith longer than he would have liked, but it is up. There is a lot to read and understand. It is structured for different levels. The link goes to the top level. There is more detail below this and then there are the data below that.

You can either go to our main page
[1]<http://www.cru.uea.ac.uk/> then click on the link
or directly here

[2]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

I'll let you make up your own minds! It seems to me as though McIntyre cherry picked for effect.

There is an additional part that shows how many series from Ch 6 of AR4 used Yamal - most didn't! Also there is a sensitivity test of omitting it - which comes from the Supplementary Info with Osborn and Briffa (2006). As expected omitting it makes very little difference. To get to this follow the links from the above link.

McIntyre knows that the millennial temperature record is pretty robust, otherwise he

would produce his own series. Similarly the instrumental temperature is even more robust, which he also knows.

Cheers

Phil

Prof. Phil Jones

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--
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email: [4]mann@psu.edu

University Park, PA 16802-5013

website: [5]<http://www.meteo.psu.edu/~mann/Mann/index.htm>

"Dire Predictions" book site:

[6]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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--
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Page 427

mail.2009
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The Pennsylvania State University email: [8]mann@psu.edu
University Park, PA 16802-5013
website: [9]http://www.meteo.psu.edu/~mann/Mann/index.html
"Dire Predictions" book site:
[10]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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References

1. <http://www.cru.uea.ac.uk/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>
3. <mailto:p.jones@uea.ac.uk>
4. <mailto:mann@psu.edu>
5. <http://www.meteo.psu.edu/~mann/Mann/index.html>
6. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
7. <mailto:p.jones@uea.ac.uk>
8. <mailto:mann@psu.edu>
9. <http://www.meteo.psu.edu/~mann/Mann/index.html>
10. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

1063. 1256747199.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Mitchell, John FB (Director of Climate Science)"
<john.f.mitchell@metoffice.gov.uk>
Subject: Yamal response from Keith
Date: Wed Oct 28 12:26:39 2009

John,

[1]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

This went up last night about 5pm. There is a lot to read at various levels. If you get time just the top level is necessary. There is also a bit from Tim Osborn showing that

Yamal was used in 3 of the 12 millennial reconstructions used in Ch 6.

Also McIntyre had the Yamal data in Feb 2004 - although he seems to have forgotten this.

Keith succeeding in being very restrained in his response. McIntyre knew what he was doing when he replaced some of the trees with those from another site.

Cheers
Phil

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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

1064. 1256760240.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: k.briffa@uea.ac.uk
Subject: FW: Yamal and paleoclimatology
Date: wed Oct 28 16:04:00 2009

Keith,

There is a lot more there on CA now. I would be very wary about responding to this

person now having seen what McIntyre has put up.

You and Tim talked about Yamal. why have the bristlecones come in now.

[1]<http://www.climateaudit.org/?p=7588#comments>

This is what happens - they just keep moving the goalposts.

Maybe get Tim to redo OB2006 without a few more series.

Cheers

Phil

X-Authentication-warning: ueamailgate02.uea.ac.uk: defang set sender to

<Don.Keiller@anglia.ac.uk> using -f

Subject: FW: Yamal and paleoclimatology

Date: wed, 28 Oct 2009 15:39:48 -0000

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: Yamal and paleoclimatology

Thread-Index: AcpDQ2sqWC+z2djuSqC1Ax4HdHoH1wUn10cw

From: "Keiller, Donald" <Don.Keiller@anglia.ac.uk>

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

Cc: <p.jones@uea.ac.uk>

X-ARU-HELO: CAMEXCH.ANGLIA.LOCAL

X-ARU-sender-host: cambe01.ad.anglia.ac.uk (CAMEXCH.ANGLIA.LOCAL)

[193.63.55.171]:25427

X-ARU-Mailhub: yes

X-ARU-Exchange: yes

X-ARU-MailFilter: message scanned

X-Spam-Status: no

Reply-to: Don.Keiller@anglia.ac.uk

X-Canit-CHI2: 0.00

X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)

X-Spam-Score: 0.00 () [Hold at 5.00] SPF(none,0)

X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)

X-Canit-Stats-ID: 34330416 - 89bde843c4e5 (trained as not-spam)

X-Antispam-Training-Forget:

[2]<https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=f>

X-Antispam-Training-Nospam:

[3]<https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=n>

X-Antispam-Training-Spam:

[4]<https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=s>

X-Scanned-By: CanIt (www.roaringpenguin.com) on 127.0.0.1

Dear Professor Briffa, I am pleased to hear that you appear to have recovered from your recent illness sufficiently to post a response to the controversy surrounding the use of the Yamal chronology;

([5]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/cautious/cautious.htm>)

mail.2009

and the chronology itself;

([6]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>)

Unfortunately I find your explanations lacking in scientific rigour and I am more inclined to believe the analysis of McIntyre

([7]<http://www.climateaudit.org/?p=7588>)

Can I have a straightforward answer to the following questions

1) Are the reconstructions sensitive to the removal of either the Yamal data and Strip pine bristlecones, either when present singly or in combination?

2) why these series, when incorporated with white noise as a background, can still produce a Hockey-Stick shaped graph if they have, as you suggest, a low individual weighting?

And once you have done this, please do me the courtesy of answering my initial email.

Dr. D.R. Keiller

-----Original Message-----

From: Keiller, Donald

Sent: 02 October 2009 10:34

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

Cc: 'p.jones@uea.ac.uk'

Subject: Yamal and paleoclimatology

Dear Professor Briffa, my apologies for contacting you directly, particularly since I hear that you are unwell.

However the recent release of tree ring data by CRU has prompted much discussion and indeed disquiet about the methodology and conclusions of a number of key papers by you and co-workers.

As an environmental plant physiologist, I have followed the long debate starting with Mann et al (1998) and through to Kaufman et al (2009).

As time has progressed I have found myself more concerned with the whole scientific basis of dendroclimatology. In particular;

1) The appropriateness of the statistical analyses employed

2) The reliance on the same small datasets in these multiple studies

3) The concept of "teleconnection" by which certain trees respond to the "Global Temperature Field", rather than local climate

4) The assumption that tree ring width and density are related to temperature in a linear manner.

Whilst I would not describe myself as an expert statistician, I do use inferential statistics routinely for both research and teaching and find difficulty in understanding the statistical rationale in these papers.

As a plant physiologist I can say without hesitation that points 3 and 4 do not agree with the accepted science.

There is a saying that "extraordinary claims require extraordinary proof".

Given the scientific, political and economic importance of these papers, further detailed explanation is urgently required.

Yours sincerely,

Dr. Don Keiller.

--

30% of EMERGING EXCELLENCE: In the Research Assessment Exercise (RAE) 2008, more than

our submissions were rated as 'Internationally Excellent' or 'World-leading'. Among the

academic disciplines now rated 'World-leading' are Allied Health Professions & Studies;

Art & Design; English Language & Literature; Geography & Environmental Studies; History;

Music; Psychology; and Social Work & Social Policy & Administration. Visit

[8]www.anglia.ac.uk/rae for more information.

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[9]www.altman.co.uk/emailsystems

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References

1. <http://www.climateaudit.org/?p=7588#comments>
2. <https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=f>
3. <https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=n>
4. <https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=s>
5. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/cautious/cautious.htm>
6. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>
7. <http://www.climateaudit.org/?p=7588>
8. <http://www.anglia.ac.uk/rae>
9. <http://www.altman.co.uk/emailsystems>

1065. 1256765544.txt

#####

From: "Graham F Haughton" <G.F.Haughton@hull.ac.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: Dr Sonja BOEHMER-CHRISTIANSEN
Date: wed, 28 Oct 2009 17:32:24 -0000

Content-class: urn:content-classes:message
Content-Type: text/plain;
charset="iso-8859-1"

I know, I feel for you being in that position. If its any consolation we've had it here for years, very pointed commentary at all external seminars and elsewhere, always coming back to the same theme. Since Sonja retired I am a lot more free to push my environmental interests without ongoing critique of my motives and supposed misguidedness - I've signed my department up to 10:10 campaign and have a taskforce of staff and students involved in it.... Every now and then people say to me sotto voce with some bemusement, 'and when Sonja finds out, how will you explain it to her...!'

Graham

-----Original Message-----

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: 28 October 2009 16:39

mail.2009

To: Graham F Haughton
Subject: RE: Dr Sonja BOEHMER-CHRISTIANSEN

Dear Graham,

Thanks for the speedy reply. Just like you are, we are trying here to do bits of research mostly related to the current set of contracts we have. Trying to respond to blogs is just not part of the deadlines we have entered into with the Research Councils, the EU and DEFRA.

You are probably aware of this, but the journal Sonja edits is at the very bottom of almost all climate scientists lists of journals to read. It is the journal of choice of climate change skeptics and even here they don't seem to be bothering with journals at all recently.

I don't think there is anything more you can do. I have vented my frustration and have had a considered reply from you.

Cheers
Phil

At 18:45 27/10/2009, you wrote:

>Content-class: urn:content-classes:message
>Content-Type: text/plain;
> charset="iso-8859-1"

>
>Dear Phil, sorry to hear this. I don't see much
>of her these days, but when I do see Sonja next
>I'll try and have a quiet word with her about
>the way the affiliation to us is used, but at
>the moment in fairness she is entitled to use it
>in the way she does. Fortunately I don't get to
>see many of these email exchanges but I do
>occasionally hear about them or see them and
>frankly am rarely convinced by what I read. But
>as with all academics, I'd want to protect
>another academic's freedom to be contrary and
>critical, even if I personally believe she is
>probably wrong. I agree with you that it'd be
>better for these exchanges to be conducted
>through the peer review process but these forms
>of e-communication are now part of the public
>debate and its difficult to do much about it
>other than to defend your position in this and
>other fora, or just ignore it as being, in your words, malicious.

>
>I can understand your frustration and I am
>pretty sure I'd be feeling exactly the same in
>your shoes, but I am not sure at the moment that
>I can do much more. If you think I can and
>should do more then feel free to ring and I am happy to discuss the matter.

>
>Graham

>
>
>-----Original Message-----
>From: Phil Jones [mailto:p.jones@uea.ac.uk]
>Sent: 27 October 2009 17:05
>To: Graham F Haughton
>Subject: Dr Sonja BOEHMER-CHRISTIANSEN

mail.2009

>
>
> Dear Professor Haughton,
> The email below was brought to my attention
>by the help desk of UKCP09 - the new set of UK
>climate scenarios developed for DEFRA. It was
>sent by the person named in the header of this
>email. I regard this email as very malicious. Dr
>Boehmer-Christiansen states that it is beyond her
>expertise to assess the claims made. If this is
>the case then she shouldn't be sending malicious
>emails like this. The two Canadians she refers
>to have never developed a tree-ring chronology in
>their lives and McIntyre has stated several times
>on his blog site that he has no aim to write up
>his results for publication in the peer-review literature.
> I'm sure you will be of the same opinion as
>me that science should be undertaken through the
>peer-review literature as it has been for over
>300 years. The peer-review system is the
>safeguard science has developed to stop bad science being published.
>
> In case you want to read more about the
>subject my colleague Keith Briffa has just put this up on his web site.
>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>
>
> It has taken him some time, partly as he has
>been off after a serious operation in June. He
>has had to return early to respond to this. He
>has also had some difficulty contacting our Russian colleagues.
>
> The claims on the Climate Audit site are
>exaggerated, but get taken completely out of
>context by the other blog sites that get referred
>to in Dr Boehmer-Christiansen's email. I will
>draw your attention to two things
>
> 1. The Yamal chronology is only used in 3 of
>the 12 millennial temperature reconstructions in Ch 6 of the 2007 IPCC Report.
>
> 2. McIntyre was sent the data for Yamal by our
>Russian colleagues on Feb 2, 2004.
>
> I realize Dr Boehmer-Christensen no longer
>works for you, but she is still using your affiliation.
>
> Best Regards
> Phil Jones
>
>
> From: Sonja A Boehmer-Christiansen <Sonja.B-C@hull.ac.uk>
> Date: 2 October 2009 18:09:39 GMT+01:00
> To: Stephanie Ferguson <stephanie.ferguson@ukcip.org.uk>
> Cc: "Peiser, Benny"
><B.J.Peiser@ljmu.ac.uk>, Patrick David Henderson
><pdhenderson18@googlemail.com>, Christopher Monckton <monckton@mail.com>
> Subject: RE: Please take note of
>potentially serious allegations of scientific 'fraud' by CRU and Met Office
>
>
>
>

mail.2009

> Dear Stephanie

>
> I expect that a great deal of UKCIP work
>is based on the data provided by CRU (as does the
>work of the IPCC and of course UK climate
>policy). Some of this, very fundamentally, would
>now seem to be open to scientific challenge, and
>may even face future legal enquiries. It may be
>in the interest of UKCIP to inform itself in good
>time and become a little more 'uncertain' about its policy advice.

>
> Perhaps you can comment on the following
>and pass the allegations made on to the relevant people.

>
> It is beyond my expertise to assess the
>claims made, but they would fit into my
>perception of the whole 'man-made global warming'
>cum energy policy debate. I know several of
>the people involved personally and have no
>reason to doubt their sincerity and honour as
>scientists, though I am also aware of their
>highly critical (of IPCC science) policy positions.

>
> I could also let you have statements by
>Steve McIntyre and Ross MCKitrick. Ross MCKitrick
>currently teaches at Westminster Business School
>and who is fully informed about the relevant
>issues. He recently addressed a meeting of about 50 people in London.

> Best wishes

> Sonja B-C

> Dr.Sonja A.Boehmer-Christiansen
> Reader Emeritus, Department of Geography
> Hull University
> Editor, Energy&Environment
> Multi-Science (www.multi-science.co.uk)
> HULL HU6 7RX
> Phone:(0044)1482 465369/465385
> Fax: (0044) 1482 466340

> TWO copied pieces follow, both relate to CRU and UK climate policy

> a. THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

> " Jennifer Marohasy <jennifermarohasy@jennifermarohasy.com>

> Leading UK Climate Scientists Must
>Explain or Resign, Jennifer Marohasy

> <
><[http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists->](http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-)
>[http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-
>must-explain-or-resign/>](http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-must-explain-or-resign/)

> MOST scientific sceptics have been
>dismissive of the various reconstructions of
>temperature which suggest 1998 is the warmest
>year of the past millennium. Our case has been
>significantly bolstered over the last week with
>statistician Steve McIntyre finally getting
>access to data used by Keith Briffa, Tim Osborn

mail.2009

>and Phil Jones to support the idea that there has
>been an unprecedented upswing in temperatures
>over the last hundred years - the infamous hockey stick graph.

>
> Mr McIntyre's analysis of the data -
>which he had been asking for since
> 2003 - suggests that scientists at the
>Climate Research Unit of the United Kingdom's
>Bureau of Meteorology have been using only a
>small subset of the available data to make their
>claims that recent years have been the hottest of
>the last millennium. When the entire data set is
>used, Mr McIntyre claims that the hockey stick shape disappears
> completely. [1]

>
> Mr McIntyre has previously showed
>problems with the mathematics behind the 'hockey
>stick'. But scientists at the Climate Research
>Centre, in particular Dr Briffa, have
>continuously republished claiming the upswing in
>temperatures over the last 100 years is real and
>not an artifact of the methodology used - as
>claimed by Mr McIntyre. However, these same
>scientists have denied Mr McIntyre access to all
>the data. Recently they were forced to make more
>data available to Mr McIntyre after they
>published in the Philosophical Transactions of
>the Royal Society - a journal which unlike Nature
>and Science has strict policies on data archiving which it
> enforces.

>
> This week's claims by Steve McIntyre that
>scientists associated with the UK Meteorology
>Bureau have been less than diligent are serious
>and suggest some of the most defended building
>blocks of the case for anthropogenic global
>warming are based on the indefensible when the
> methodology is laid bare.

>
> This sorry saga also raises issues
>associated with how data is archived at the UK
>Meteorological Bureau with in complete data sets
>that spuriously support the case for global
>warming being promoted while complete data sets
>are kept hidden from the public - including from
>scientific sceptics like Steve McIntyre.

>
> It is indeed time leading scientists at
>the Climate Research Centre associated with the
>UK Meteorological Bureau explain how Mr McIntyre is in error or resign.

>
> [1] Yamal: A "Divergence" Problem, by
>Steve McIntyre, 27 September 2009
> <http://www.climateaudit.org/?p=7168>

> Jennifer Marohasy BSc PhD

>
> b. National Review Online, 23 September 2009

><<http://article.nationalreview.com/?q=ZTBiMTRlMDQxNzEyMmRhZjU3ZmYzODI1MGY4ZWl5OWM=>>
By

>
>Patrick J. Michaels

>
>
> Imagine if there were no reliable
>records of global surface temperature. Raucous
>policy debates such as cap-and-trade would have
>no scientific basis, Al Gore would at this point
>be little more than a historical footnote, and
>President Obama would not be spending this U.N.
>session talking up a (likely unattainable)
>international climate deal in Copenhagen in
>December. Steel yourself for the new reality,
>because the data needed to verify the
>gloom-and-doom warming forecasts have disappeared.

>
> Or so it seems. Apparently, they were
>either lost or purged from some discarded
>computer. Only a very few people know what really
>happened, and they aren't talking much. And what
>little they are saying makes no sense.

> In the early 1980s, with funding from
>the U.S. Department of Energy, scientists at the
>United Kingdom's University of East Anglia
>established the Climate Research Unit (CRU) to
>produce the world's first comprehensive history
>of surface temperature. It's known in the trade
>as the "Jones and Wigley" record for its authors,
>Phil Jones and Tom Wigley, and it served as the
>primary reference standard for the U.N.
>Intergovernmental Panel on Climate Change (IPCC)
>until 2007. It was this record that prompted the
>IPCC to claim a "discernible human influence on global climate."

> Putting together such a record isn't at
>all easy. Weather stations weren't really
>designed to monitor global climate. Long-standing
>ones were usually established at points of
>commerce, which tend to grow into cities that
>induce spurious warming trends in their records.
>Trees grow up around thermometers and lower the
>afternoon temperature. Further, as documented by
>the University of Colorado's Roger Pielke Sr.,
>many of the stations themselves are placed in
>locations, such as in parking lots or near heat
>vents, where artificially high temperatures are bound to be recorded.

> So the weather data that go into the
>historical climate records that are required to
>verify models of global warming aren't the
>original records at all. Jones and Wigley,
>however, weren't specific about what was done to
>which station in order to produce their record,
>which, according to the IPCC, showed a warming of
>0.6Å° +/- 0.2Å°C in the 20th century.

>
> Now begins the fun. Warwick Hughes, an
>Australian scientist, wondered where that "+/-"
>came from, so he politely wrote Phil Jones in
>early 2005, asking for the original data. Jones's
>response to a fellow scientist attempting to
>replicate his work was, "we have 25 years or so
>invested in the work. Why should I make the data
>available to you, when your aim is to try and find something wrong with it?"
> Reread that statement, for it is

mail.2009

>brehtaking in its anti-scientific thrust. In
>fact, the entire purpose of replication is to
>"try the data because he wasn't an "academic." So
>objective of science is to do things so well that, indeed, nothing is wrong.

>
> Then the story changed. In June 2009,
>Georgia Tech's Peter Webster told Canadian
>researcher Stephen McIntyre that he had requested
>raw data, and Jones freely gave it to him. So
>McIntyre promptly filed a Freedom of Information
>Act request for the same data. Despite having
>been invited by the National Academy of Sciences
>to present his analyses of millennial
>temperatures, McIntyre was told that he couldn't
>have the data because he wasn't an "academic." So
>his colleague Ross Mckitrick, an economist at the
>University of Guelph, asked for the data. He was turned down, too.

> Faced with a growing number of such
>requests, Jones refused them all, saying that
>there were "confidentiality" agreements regarding
>the data between CRU and nations that supplied
>the data. McIntyre's blog readers then requested
>those agreements, country by country, but only a
>handful turned out to exist, mainly from Third
>world countries and written in very vague language.

> It's worth noting that Mckitrick and I
>had published papers demonstrating that the
>quality of land-based records is so poor that the
>warming trend estimated since 1979 (the first
>year for which we could compare those records to
>independent data from satellites) may have been
>overestimated by 50 percent. Webster, who
>received the CRU data, published studies linking
>changes in hurricane patterns to warming (while others have found otherwise).

> Enter the dog that ate global warming.
>
> Roger Pielke Jr., an esteemed professor
>of environmental studies at the University of
>Colorado, then requested the raw data from Jones. Jones responded:
> Since the 1980s, we have merged the data
>we have received into existing series or begun
>new ones, so it is impossible to say if all
>stations within a particular country or if all of
>an individual record should be freely available.
>Data storage availability in the 1980s meant that
>we were not able to keep the multiple sources for
>some sites, only the station series after
>adjustment for homogeneity issues. We, therefore,
>do not hold the original raw data but only the
>value-added (i.e., quality controlled and homogenized) data.

> The statement about "data storage" is
>balderdash. They got the records from somewhere.
>The files went onto a computer. All of the
>original data could easily fit on the 9-inch tape
>drives common in the mid-1980s. I had all of the
>world's surface barometric pressure data on one such tape in 1979.
> If we are to believe Jones's note to the
>younger Pielke, CRU adjusted the original data
>and then lost or destroyed them over twenty years
>ago. The letter to Warwick Hughes may have been
>an outright lie. After all, Peter Webster
>received some of the data this year. So the
>question remains: what was destroyed or lost,

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>when was it destroyed or lost, and why?

>
> All of this is much more than an
>academic spat. It now appears likely that the
>U.S. Senate will drop cap-and-trade climate
>legislation from its docket this fall - whereupon
>the Obama Environmental Protection Agency is
>going to step in and issue regulations on
>carbon-dioxide emissions. Unlike a law, which
>can't be challenged on a scientific basis, a
>regulation can. If there are no data, there's no
>science. U.S. taxpayers deserve to know the
>answer to the question posed above. (Patrick J.
>Michaels is a senior fellow in environmental
>studies at the Cato Institute and author of
>Climate of Extremes: Global Warming Science They Don't Want You to Know.) "

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>
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1066. 1257532857.txt

#####

From: Tom wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Revised CC text
Date: Fri, 06 Nov 2009 13:40:57 -0700

<x-flowed>
Thanks, Phil.

A bunch of us are putting something together on the latest Lindzen and Choi crap (GRL). Not a comment, but a separate paper to avoid giving Lindzen the last word.

Tom.

+++++

Phil Jones wrote:

>
>> Tom,
>
> Got to this sooner than I thought. I've responded to your points by
> saying things in comments and also responding to some points at the end
> of the references.
>
> Over the weekend I'll get the references into the same format. Can
> you have another look through? I think we are there on almost everything.
>
> Keith should be replying about the trees - a possible reason why KHAD
> is anomalous relates to permafrost depth. Impossible to prove and it's
> likely much more complicated. Difficult to detail with MM when they
> won't publish anything. They also know the global temperature record is
> robust, the millennial records less so. Taking one or two records out
> makes no difference and they know that. They go on about issues that
> have no effect.
>
> The CC article explains why the global T record is robust, so
> something to refer to. I don't think it is going to help our H-Indexes
> though!
>
> Have a good weekend!

> Phil

>
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> -----

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</x-flowed>

1067. 1257546975.txt

#####

From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: LAND vs OCEAN
Date: Fri, 06 Nov 2009 17:36:15 -0700

<x-flowed>

We probably need to say more about this. Land warming since 1980 has been twice the ocean warming -- and skeptics might claim that this proves that urban warming is real and important.

See attached note.

Comments?

Tom

</x-flowed>

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

1068. 1257847147.txt

#####

From: "IPCC WGI TSU" <wg1@ipcc.unibe.ch>
Subject: IPCC Draft Good Practice Guidance Paper on Detection and Attribution for Review
Date: Tue, 10 Nov 2009 04:59:07 +0100 (CET)
Reply-to: wg1@ipcc.unibe.ch
Cc: stocker@climate.unibe.ch, qdh@cma.gov.cn, barros@at.fcen.uba.ar, cfield@ciw.edu, plattner@ipcc.unibe.ch, krisebi@ipcc-wg2.gov, midgley@ipcc.unibe.ch, tignor@ipcc.unibe.ch, wg1@ipcc.unibe.ch, tsu@ipcc-wg2.gov

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

X-MIME-Autoconverted: from 8bit to quoted-printable by ueamailgate02.uea.ac.uk id nAA3xK1S014515

Dear Participants of the IPCC Expert Meeting on Detection & Attribution,
dear Colleagues,

Please find attached the draft version of the Good Practice Guidance Paper (GPGP) which has been prepared by the Core Writing Team (CWT) following the IPCC joint WGI/II Expert Meeting on Detection and Attribution. Gabi, Ove, Camille, David, Gino, Marty, Peter, and Sari, have been working very hard to meet the TSU deadline and have managed to provide the Co-Chairs with the attached draft version right in time for presentation at the IPCC Plenary in Bali the last week of October. We all owe them our sincere thanks for the efforts put into the preparation of this document.

Logistics:

We would now like to invite all participants of the Geneva Expert Meeting to review the GPGP and to provide comments and suggestions on the attached

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draft within 2 weeks from today (i.e. by *November 24*). If you do plan to provide your inputs, please prepare your comments in a separate document (word or plain text) in order to facilitate the handling of the comments from potentially ~30 participants. Submission of the files will be by email to the WGI TSU at wg1@ipcc.unibe.ch. We will collect all the reviews, combine them into an easily manageable format and will then forward them to the CWT. The task of the CWT will then be to consider all your comments and revise the GPGP accordingly. We do not plan to send the Guidance Paper out for a second round of comments, but trust that the CWT will make every effort to take your suggestions into account as much as possible.

Changes to terminology discussed in Geneva:

Please note that the CWT, after intense discussions, had to make a few changes to the language used in the "approved" documents from the last day's final plenary. One of the changes is the change from "direct" to "single step" attribution. Given the level of discussion created within the CWT and also during the meeting, the CWT felt it was more constructive NOT to insinuate which methods are better or stronger and so strived for neutral language, particularly as the views about what constitutes a strong method differed between groups (not only IPCC WGs). Note that the word "direct" already had created discussion during the final plenary of the Geneva meeting and was flagged as unresolved in the material sent to the CWT by the WGI TSU. As a consequence, the CWT has then changed "sequential" to "multi-step" to keep language consistent. The CWT has highlighted in the text by brackets where language was changed in order to maintain maximum transparency.

Material to be included in the Expert Meeting Report:

The GPGP will be part of the full meeting report which we are currently preparing at the WGI TSU. The full meeting report will include all the materials from the conference documentation, i.e. abstracts, participants list, agenda, etc. In Geneva, we also discussed to include additional science background material going along with the Guidance Paper. In light of the substantial GPGP we currently have, it seems sufficient to add a few (2-3) practical examples of D&A to the report which would illustrate and clarify in concrete terms the different points raised in the GPGP. As such examples are of a different nature than the GPGP text, we propose to present them in separate boxes. Our proposal is that the CWT will work on these D&A examples while the participants are commenting on the GPGP, and while the WGI TSU works on preparing the full meeting report. A further science element to be included in the full meeting report would be a non-comprehensive bibliography of D&A literature added at the end of the report (see separate email following).

I hope this way forward is acceptable to you. Thank you very much for your continued efforts and contributions to this important IPCC activity. We are looking forward to your inputs,

Cheers, Kasper
IPCC WGI TSU

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#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Gil Compo <compo@colorado.edu>
Subject: Re: Twentieth Century Reanalysis preliminary version 2 data - One other thing!
Date: Tue Nov 10 12:40:26 2009

Gil,

One other good plot to do is this. Plot land minus ocean. as a time series. This should stay relatively close until the 1970s. Then the land should start moving away

from the ocean.

This departure is part of AGW. The rest is in your Co2 increases.

Cheers

Phil

Gil,

These will do for my purpose. I won't pass them on. I am looking forward to the draft paper. As you're fully aware you're going to have to go some ways to figuring out what's causing the differences.

You will have to go down the sub-sampling, but I don't think it is going to make much difference. The agreement between CRU and GISS is amazing good, as already know. You ought to include the NCDC dataset as well.

[1]<http://www.ncdc.noaa.gov/oa/climate/research/anomalies/index.html> the ERSST3b dataset.

In the lower two plots there appear to be two types of differences, clearer in the NH20-70 land domain.

The first is when rean120v2 differs for a single year (like a year in the last 1960s, 1967 or 1968) and then when it differs for about 10 years or so. It is good that it keeps coming back. For individual years there are a couple of years in the first decade of the 20th century (the 1900s).

The longer periods are those you've noticed - the 1920s and the 1890s. There is also something up with the period 1955-65 and the 1970s. The 1920s seems to get back then go off

again from about 1935 to early 1940s. Best thing to try and isolate some of the reasons would be maps for decades or individual years. For the 1920s I'd expect the differences to be coming from Siberia as opposed to Canada. I think the 1890s might be just down to sparser coverage. The 1890s is the only period where the difference brings your pink line back towards the long-term zero. All the others have the pink line more extreme than the

HadCRUT3/GISS average.

Rob Allan just called. I briefly mentioned this to him. He suggested maps of

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data input

during these times. He also suggested looking at the spread of the ensembles.

Your grey

spread is sort of this, but this is a different sort of ensemble to what Rob implied you might have?

One final thing - don't worry too much about the 1940-60 period, as I think we'll be

changing the SSTs there for 1945-60 and with more digitized data for 1940-45.

There is also

a tendency for the last 10 years (1996-2005) to drift slightly low - all 3 lines.

This may

be down to SST issues.

Once again thanks for these! Hoping you'll send me a Christmas Present of the draft!

Cheers

Phil

At 20:45 09/11/2009, you wrote:

Phil,

1. I didn't get the attached.

Both version1 and version2 use HadISST1.1 for SST and sea ice.

2. time-varying CO2, volcanic aerosols, and solar variability (11-year cycle until 1949,

"observed" after that) are specified.

Attached is a research figure. Please do not share.

In it, I have plotted the annual average (top panel) 50S to 70N global average 2m

temperature from 20CRV2, SST/2m temperature from HadCRU3, SST/2m temperature

from GISTEMP 1200km, and the 90% range of 2m air temperature from 25 CMIP3 models

that can be extended beyond their 20C3M runs with SRESA1B. The ensemble mean is the thick

gray curve. Averages are July-June.

(middle panel) 50S to 70N land-only 2m temperature from 20CRV2, 2m temperature

from CRUTEM3, 2m temperature from GISTEMP land-only 1200km. CMIP3 data is the same.

(bottom panel) same as middle panel but for Northern Hemisphere land-only (20N

to 70N). Anomalies are with respect to 1901-2000. period is July 1891 to June 2005. The

CRU (HadCRU) curves are supposed to be black.

missing No data has been masked by another dataset's observational availability, but

values are not included in that dataset's area-weighted average.

Your ERA-Interim finding about it being warmer seems to be the case in the late

19th century but not the early 1920's.

Note that the only thermometer data in the magenta curve (20CRV2) is the

HadISST1.1 over

oceans. The two landonly panels are independent of thermometers, aside from the specified SSTs.

There are some very interesting differences, particularly late-19th century,

1920s, and

WWII.

linearly Correlations (I told you this was research, right?). The second pair is for

detrended data.

GLOBE (70N-50S)

rean120v2.70n50s.landocean.juljun

hadcru3.70n50s.landocean.juljun 0.94370

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reanl20v2.70n50s.landocean.juljun	
hadcru3.70n50s.landocean.juljun	0.82017
reanl20v2.70n50s.landocean.juljun	
gistemp_combined1200.70n50s.landocean.juljun	0.95284
reanl20v2.70n50s.landocean.juljun	
gistemp_combined1200.70n50s.landocean.juljun	0.85808
hadcru3.70n50s.landocean.juljun	
gistemp_combined1200.70n50s.landocean.juljun	0.99088
hadcru3.70n50s.landocean.juljun	
gistemp_combined1200.70n50s.landocean.juljun	0.97383
GLOBAL LAND (70N-50S)	
reanl20v2.70n50s.landonly.juljun	
cru3.70n50s.landonly.juljun	0.85167
reanl20v2.70n50s.landonly.juljun	
cru3.70n50s.landonly.juljun	0.68755
reanl20v2.70n50s.landonly.juljun	
gistemp_land1200.70n50s.landonly.juljun	0.81469
reanl20v2.70n50s.landonly.juljun	
gistemp_land1200.70n50s.landonly.juljun	0.60152
cru3.70n50s.landonly.juljun	
gistemp_land1200.70n50s.landonly.juljun	0.98050
cru3.70n50s.landonly.juljun	
gistemp_land1200.70n50s.landonly.juljun	0.95316
NH Land (20N-70N)	
reanl20v2.nh_nohigh.landonly.juljun	
cru3.nh_nohigh.landonly.juljun	0.82956
reanl20v2.nh_nohigh.landonly.juljun	
cru3.nh_nohigh.landonly.juljun	0.67989
reanl20v2.nh_nohigh.landonly.juljun	
gistemp_land1200.nh_nohigh.landonly.juljun	0.79247
reanl20v2.nh_nohigh.landonly.juljun	
gistemp_land1200.nh_nohigh.landonly.juljun	0.59900
cru3.nh_nohigh.landonly.juljun	
gistemp_land1200.nh_nohigh.landonly.juljun	0.98001
cru3.nh_nohigh.landonly.juljun	
gistemp_land1200.nh_nohigh.landonly.juljun	0.95880

this I thought that correlations of 0.8 to 0.85 were high for an independent dataset

long. I think that these are higher than the proxies?
The global isn't that fair because we have the HadISST.
The correlations are about the same as for AMIP runs, though. See
variability in Hoerling M., A. Kumar, J. Eischeid, B. Jha (2008), what is causing the

global mean land temperature?, Geophys. Res. Lett., 35, L23712,
doi:10.1029/2008GL035984.

It will be interesting to see if the masked numbers change.

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Let me know if you need anything else on this for your essay material.
best wishes,
gil
Phil Jones wrote on 11/9/09 2:55 AM:

Gil,

A couple of questions.

1. See the attached. Is this paper providing the SST input to 20CRV2?
2. Do you change greenhouse gases in the run?

Apologies if these are answered elsewhere.

Do you have any pre-draft plots without subsampling to get some idea of how good the agreement?

I'm asking these questions as I'm writing an essay for Climate Change. There are no diagrams in this, but showing the agreement with 20CRV2 will be a nice way to finish the paper.

Paper briefly documents the magnitude of all the problems in global temperature data - such as SST biases, exposure issues, urbanization and site changes (in order of importance). Site changes for global averages are the least important. Trying to point to a few home truths to skeptics who keep on going on about the land data.

Cheers

Phil

At 15:39 03/11/2009, Gil Compo wrote:

Phil,

Already calculated. We don't suffer from some of the issues that you and Adrian raised because we use only surface pressure.

In the Northern Hemisphere extratropics, the agreement with the various (yours, GISTEMP, NOAA) thermometer-based near surface T is high, but in the Tropics and Southern Hemisphere, there are discrepancies, particularly over Africa and South America. The

20CRV2 does not have the intensity of the Siberia warming.

There are also discrepancies in the WWII period. I have not subset the reanalysis to correspond to a particular dataset's missing mask as all 3 have different coverages.

I'll be making plots for the paper (with a draft coming) soon.

best wishes,

gil

[2]P.Jones@uea.ac.uk wrote on 11/3/09 3:37 AM:

Gil,

I'm sitting in a meeting in Bristol with Rob Allan. We've had a thought. When you finish v2 will you be quickly calculating the global T average for the 1891-2006 period? Do you expect this to look like the real global T, or do you expect it to not show the longer timescale change that NCEP from 1948 showed?

I can send a paper with Adrian Simmons from JGR in 2004 on this when I'm back in Norwich tomorrow.

Cheers

Phil

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Dear Colleagues,

Courtesy of the NOAA Earth System Research Laboratory Physical Sciences Division and University of Colorado CIRES Climate Diagnostics Center, at

[3]ftp://ftp.cdc.noaa.gov/Datasets/20thC_Rean/provisionalV2/ , please find temporary netCDF files from the 20th Century Reanalysis version 2 (1891-2006). These yearly files are for the ensemble mean analysis (means) and ensemble standard deviation (spreads) of selected variables. Colleagues from organizations contributing to the 20th Century Reanalysis version 2 or the International Surface Pressure Databank version 2.2, the observational input dataset, are welcome to investigate these preliminary files. Colleagues on the Atmospheric Circulation Reconstructions over the Earth Working Group 3 Verification and Validation of reanalyses are also welcome to begin working with these files.

We are working with our distribution partners at the National Center for Atmospheric Research, the National Oceanic and Atmospheric Administration (NOAA) Earth System Research Laboratory and NOAA's National Climatic Data Center on wider availability and documentation. A rough draft of important documentation is attached.

Also, please see our new homepage at

[4]http://www.esrl.noaa.gov/psd/data/20thC_Rean/ which includes access to images of 6-hourly sea level pressure and 500 geopotential maps generated from the version 2 data.

When production is complete, the 20CR version 2 will span 1871 to present.

The references for the dataset are

Compo, G.P., J.S. Whitaker, P.D. Sardeshmukh, N. Matsui, R.J. Allan, X. Yin, B.E. Gleason, R.S. Vose, G. Rutledge, P. Bessemoulin, S. Brönnimann, M. Brunet, R.I. Crouthamel, A.N. Grant, P.Y. Groisman, P.D. Jones, M. Kruk, A.C. Kruger, G.J. Marshall, M. Maugeri, H.Y. Mok, Ø. Nordli, T.F. Ross, R.M. Trigo, X.L. Wang, S.D. Woodruff, S.J. Worley, 2009: The Twentieth Century Reanalysis Project. Quarterly J. Roy. Met. Soc., in preparation.

Compo, G.P., J.S. Whitaker, P.D. Sardeshmukh, 2008: The 20th Century Reanalysis Project. Third WCRP International Conference on Reanalysis, 28 January 2008, Tokyo, Japan

<

[5]http://wcrp.ipsl.jussieu.fr/workshops/Reanalysis2008/Documents/V5-511_ea.pdf

>

Compo, G.P., J.S. Whitaker, and P.D. Sardeshmukh, 2006: Feasibility of a 100 year reanalysis using only surface pressure data. Bull. Amer. Met. Soc., 87, 175-190.

Whitaker, J.S., G.P. Compo, X. Wei, and T.M. Hamill 2004: Reanalysis without radiosondes using ensemble data assimilation. Mon. Wea. Rev., 132, 1190-1200.

Please let us know of any questions about the dataset. And, thank you for your contributions to its development.

Best wishes,

Gil Compo

[6]<compo@colorado.edu>

Jeffrey S. Whitaker

[7]

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<Jeffrey.S.whitaker@noaa.gov>
20th Century Reanalysis Project leads

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[9]<http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>
++++
"Stop and consider the wondrous works of God."
Job 37:34

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[14]http://www.esrl.noaa.gov/psd/people/gilbert.p.compo
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References

1. <http://www.ncdc.noaa.gov/oa/climate/research/anomalies/index.html%A0>
2. <mailto:P.Jones@uea.ac.uk>
3. ftp://ftp.cdc.noaa.gov/Datasets/20thC_Rean/provisionalV2/
4. http://www.esrl.noaa.gov/psd/data/20thC_Rean/
5. http://wcrp.ipsl.jussieu.fr/workshops/Reanalysis2008/Documents/V5-511_ea.pdf
6. <mailto:compo@colorado.edu>
7. <mailto:Jeffrey.S.Whitaker@noaa.gov>
8. <mailto:compo@colorado.edu>
9. <http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>
10. <mailto:compo@colorado.edu>
11. <http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>
12. <mailto:p.jones@uea.ac.uk>
13. <mailto:compo@colorado.edu>
14. <http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>

1070. 1257881012.txt

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From: AGU Atmospheric Sciences Section <Section@AGU.ORG>
To: <AS-SECTION_D@listserv.agu.org>
Subject: Letter to Atmospheric Sciences members
Date: Tue, 10 Nov 2009 14:23:32 -0500
Reply-to: AGU Atmospheric Sciences Section <Section@AGU.ORG>

Dear Atmospheric Sciences Section members,

First, I would like to remind you of three very close deadlines:

Nov. 12, Discounted registration for AGU Fall Meeting. Register at
[1]https://www.associationsciences.org/agu/meet_demog.jsp, and sign up for our
Atmospheric
Sciences banquet on Dec. 15.

Nov. 13, Vote yes on AGU governance changes,
[2]<http://www.agu.org/governancevote/>

Nov. 13, Please respond to [3]stacyjackson@berkeley.edu if you are willing to
volunteer
your expertise to help answer questions during the Copenhagen Conference of the
Parties of

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the Framework Convention on Climate Change (see below).

Second, I would like to give you some information about where your contributions to AGU go. Last year, members of the Atmospheric Sciences Section contributed \$43,410 to AGU's Voluntary Contribution Campaign. In 2008, due largely to member donations like these, AGU facilitated career development events attended by 600 students, hosted 75 K-12 teachers at Fall Meeting workshops, and sponsored 31 members' visits with U.S. policy makers. Additionally, voluntary contributions allowed AGU to provide travel grants to 135 deserving students to present their research for the first time at an AGU meeting. These programs are essential for AGU's relevance and vitality. I know Atmospheric Science members want AGU to do more. Please join me in supporting AGU's efforts to strengthen our scientific society by making a gift to the 2010 voluntary Contribution Campaign. Unrestricted contributions are used to support AGU's greatest needs, but you can directly support students pursuing Atmospheric Sciences by making a gift to the David Hofmann Travel Grant, Holton-Kaufman Grant, or Namias Travel Grant. You can make your gift when you renew your AGU membership, or you can give today at:

[4]https://www.agu.org/givingtoagu/making_your_gift.php

Sincerely,

Alan Robock

President, Atmospheric Sciences Section, AGU [5]robock@envsci.rutgers.edu

AGU Climate Scientists,

We are writing to encourage hundreds of you to participate in a unique opportunity to improve the public's climate knowledge during the week before and the week of this year's AGU Fall Meeting.

As you know, the Copenhagen negotiations (Dec. 7-18) are attracting hundreds of journalists and will result in a proliferation of media articles about climate change. Recently, the American public's "belief" in climate change has waned (36% think humans are warming the earth according to the Pew Center's October poll), and December's media blitz provides an opportunity to reverse the trend.

Your participation is needed to ensure that climate science coverage across media

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channels

is accurate, fact-based, and nuanced. Provided that enough AGU members sign up to participate, we will be offering the opportunity for journalists reporting during the Copenhagen conference to submit their questions on-line and receive a response from a climate expert before an article goes to press.

We are asking each of you to sign up for two hours over the course of those two weeks (12/7-18) to respond to questions from journalists. You will be able to choose which queries to answer based on your expertise, and there will be an option to double-team when questions span multiple areas of expertise. We will be setting up the appropriate logistics to enable both virtual participation and a central work area at the AGU meeting. If you have any questions, feel free to email Stacy Jackson at the email address below.

If you are willing to participate, please respond in the affirmative by Friday November 13th to [6]stacyjackson@berkeley.edu. Given the magnitude of the media coverage, we are seeking several hundred willing climate scientists. More details will be forthcoming.

Thanks in advance,

Alan Robock, President, AGU Atmospheric Sciences Section

Anne Thompson, President-Elect, AGU Atmospheric Sciences Section

References

1. https://www.associationsciences.org/agu/meet_demog.jsp
2. <http://www.agu.org/governancevote/>
3. <mailto:stacyjackson@berkeley.edu>
4. https://www.agu.org/givingtoagu/making_your_gift.php
5. <mailto:robock@envsci.rutgers.edu>
6. <mailto:stacyjackson@berkeley.edu>

1071. 1257888920.txt

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From: Phil Jones <p.jones@uea.ac.uk>
To: c.harpham@uea.ac.uk
Subject: FW: Helpdesk query 1489: Hourly data have discontinuities at day joins
Date: Tue Nov 10 16:35:20 2009

Colin,
I thought that this didn't happen.
Cheers
Phil

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From: C G Kilsby <c.g.kilsby@newcastle.ac.uk>
To: "p.jones@uea.ac.uk" <p.jones@uea.ac.uk>
Date: Tue, 10 Nov 2009 15:35:37 +0000
Subject: FW: Helpdesk query 1489: Hourly data have discontinuities at day joins
Thread-Topic: Helpdesk query 1489: Hourly data have discontinuities at day joins
Thread-Index: AcpiFAtfZVu2N5gLTBW4NaA+k/QJowAB1zVA
Accept-Language: en-GB
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
acceptlanguage: en-GB
X-smtpf-Report: sid=1A9Fze094454569100; tid=1A9Fze0944545691XL;
client=lan,relay,white,ipv6; mail=; rcpt=; nrcpt=1:0; fails=0
X-Canit-CHI2: 0.00
X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)
X-Spam-Score: 0.00 () [Hold at 5.00] HTML_MESSAGE,SPF(pass,0)
X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
X-Canit-Stats-ID: 35355645 - b33bcd1c960c (trained as not-spam)
X-Antispam-Training-Forget:
[1]<https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=f>
X-Antispam-Training-Nonspam:
[2]<https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=n>
X-Antispam-Training-Spam:
[3]<https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=s>
X-Scanned-By: CanIt (www.roaringpenguin.com) on 139.222.131.184
Interesting one for you....

From: Lyndsey Middleton [[4]mailto:lyndsey.middleton@ukcip.org.uk]
Sent: 10 November 2009 2:43 PM
To: C G Kilsby
Subject: Helpdesk query 1489: Hourly data have discontinuities at day joins

Hi Chris,

Manchester Another weather Generator query for you. It was raised by Richard Watkins of University (and COPSE project) following a visit from Roger yesterday.

Can you let me know your response please?

Cheers,
Lyndsey

Long Description=The hourly data from the weather Generator have discontinuities at each midnight join. The e.g. temperature jumps, may be as high as 9½°C. The hourly data seem to have been generated independently for each day, rather than fitting a curve from the maximum of one day to the minimum of the next. The minimum to maximum curve, i.e. within each day, is fine.

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Could the weather Generator be altered to produce more realistic hourly data by fitting from Tmax to Tmin the following day, please? This would be helpful particularly for any use of the data for building simulation with plant controls.

Thanks,

Richard Watkins

Lyndsey Middleton
Enquiries Officer

UK Climate Impacts Programme
School of Geography and Environment
OUCE
South Parks Road
Oxford OX1 3QY

[5]www.ukcip.org.uk

Tel: 01865 285 718 (direct) or 01865 285717 (switchboard)

My working days are: Tuesday and Wednesday 9am to 5pm and Friday 9 am to 12.30pm

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

References

1. <https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=f>
2. <https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=n>
3. <https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=s>
4. <mailto:lyndsey.middleton@ukcip.org.uk>
5. <http://www.ukcip.org.uk/>

1072. 1258039134.txt

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From: Phil Jones <p.jones@uea.ac.uk>
To: Sandy Tudhope <sandy.tudhope@ed.ac.uk>
Subject: Latest draft of WP1
Date: Thu Nov 12 10:18:54 2009
Cc: "Wolff, Eric W" <ewwo@bas.ac.uk>, Rob Wilson <rjsw@st-andrews.ac.uk>, "Bass, Catherine" <C.J.Bass@exeter.ac.uk>, "Turney, Christian" <C.Turney@exeter.ac.uk>, Rob Allan <rob.allan@metoffice.gov.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "t.osborn@uea.ac.uk" <t.osborn@uea.ac.uk>

Dear All (especially Chris/Catherine),
Here's the latest draft of WP1. All in the group have now commented and amended this.
You should have the 3 supporting letters from Tree partners. Eric was contacting Eric Steig and Sandy (see below) is contacting 3 coral people.
There is an issue about a Map. Rob W put one in his PhD page. This shows the corals. If we were to add the tree-ring sites we would mainly get a splodge of points in South America and NZ. Ice cores would just be over the AP and in the low-lat Andes. Issue is one of space. We already have 3pp fo this WP. Refs will reduce to about 0.5pp once we go to et al for 3 or more authors. A map would be useful for presentation to NERC, but is it essential for the submission?
I'm away from tomorrow lunchtime for the weekend. Back in on Monday. Hope we'll be looking through more complete drafts next week!
Cheers
Phil
At 19:02 11/11/2009, Sandy Tudhope wrote:

Dear Phil et al,
Good to speak to you earlier Phil and Rob W..
Please find attached a slightly modified version for WP1 ... I've just changed the coral section a bit. Briefly, I've identified the new coral coring sites (rather than get bogged down trying to describe how we will use analysis of model output to prioritise), plus I've added back in some references and details that I think help, but don't add too much length.
I've written to Janice Lough, Julie Cole and Kim Cobb re being Project Partners (I actually spoke to Kim and she is keen).
FIGURE: I still think it might be useful to have a map in the main proposal ... basically like the one Rob has in the PhD proposal ... we can simply have boxes around the tree ring and ice core regions. This map needn't be any larger than Rob already has it ... but it does help illustrate where we will get/have data. What do you all think?
Cheers,
Sandy

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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
UK

1073. 1258053464.txt

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From: "Thorne, Peter (Climate Research)" <peter.thorne@metoffice.gov.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Letter draft
Date: Thu, 12 Nov 2009 14:17:44 -0000

Phil, attached is a draft letter. We were keen to keep it as short, sweet and uncomplicated as possible without skipping over important details. Shorter, simpler, requests are more likely to get read and acted upon was the specific advice from international relations.

--
Peter Thorne, Climate Research scientist
Met Office Hadley Centre, FitzRoy Road, Exeter, EX1 3PB.
tel. +44 1392 886552 fax. +44 1392 885681
<http://www.hadobs.org>

Attachment Converted: "c:\eudora\attach\Phil_letter_draft_091109.doc"