

CRU CORRESPONDENCE 2008

#####

835. 1199286511.txt

#####

From: Peter Thorne <peter.thorne@metoffice.gov.uk>
To: Susan Solomon <Susan.Solomon@noaa.gov>
Subject: Re: Douglass et al. paper
Date: wed, 02 Jan 2008 10:08:31 +0000
Cc: Tom Wigley <wigley@ucar.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, Dian Seidel <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Ben Santer <santer1@llnl.gov>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>, Myles Allen <m.allen1@physics.ox.ac.uk>, Bill Fulkerson <wfulk@utk.edu>

Susan et al.,

I had also seen the Forster et al paper and was glad to see he had followed up on work and ideas we had discussed some years ago when he was at Reading and from the Exeter workshop. At the time I had done some simple research on whether the stratosphere could affect the tropical troposphere - possibly through convection modification or radiative cooling. I'd done a simple timeseries regression of $T2LT = a * T_{surf} + b * T4 + c$ and got some regression coefficients out that suggested an influence. Now, this was with old and now discredited data and the Fu et al. technique has since superseded it to some extent (or at least cast considerable doubt upon its efficacy) ... it would certainly be hard to prove in a regression what was cause and effect with such broad weighting functions even using T2LT which still isn't *really* independent from T4.

But one thing I did do to try to "prove" the regression result was real is take the composite differences between QBO phases on 45 years of detrended (can't remember exactly how but I think I took differences from decadal filtered data) data from radiosondes (HadAT1 at the time). This showed a really very interesting result and suggested that this communication if it was real went quite far down in to the troposphere and was statistically significant, particularly in those seasons when the ITCZ and QBO were geographically coincident. I attach the slide for interest. I think this is the only scientifically valid part of the analysis that I would stand by today given the rather massive developments since. I doubt that raobs inhomogeneities could explain the plot result as they project much more onto the trend than they would onto this type of analysis.

The cooling stratosphere may really have an influence even quite low down if this QBO composite technique is a good analogue for a cooling startosphere's impact, and timeseries regression analysis supports it in some obs (it would be interesting to repeat such an analysis with the newer obs but I don't have time). A counter, however, is that surely the models do radiation so those with ozone loss should do a good job of this effect. This could be checked in Ben's ensemble in a poor man's sense at least because some have ozone depletion and some don't.

The only way this could be a real factor not picked by the models, I

mail.2008

concluded at the time, is if models are far too keen to trigger convection and that any real-world increased radiative cooling efficiency effect is masked in the models because they convect far too often and regain CAPE closure as a condition.

On another matter, we seem to be concentrating entirely on layer-average temperatures. This is fine, but we know from CCSP these show little in the way of differences. The key, and much harder test is to capture the differences in behaviour between layers / levels - the "amplification" behaviour. This was the focus of Santer et al. and I still believe is the key scientific question given that each model realisation is inherently so different but that we believe the physics determining the temperature profile to be the key test that has to be answered. Maybe we need to step back and rephrase the question in terms of the physics rather than aiming solely to rebutt Douglass et al? In this case the key physical questions in my view would be:

1. Why is there such strong evidence from sondes for a minima at c. 500? Is this because it is near the triple point of water in the tropics? Or at the top of the shallow convection? Or simply an artefact? [I don't have any good ideas how we would answer the first two of these questions]

2. Is there really a stratospheric radiative influence? If so, how low does it go? What is the cause? Are the numbers consistent with the underlying governing physics or simply an artefact of residual obs errors?

3. Can any models show trend behaviour that deviates from a SALR on multi-decadal timescales? If so, what is it about the model that causes this effect? Physics? Forcings? Phasing of natural variability? Is it also true on shorter timescales in this model?

It seems to me that trying to do an analysis based upon such physical understanding / questions will clarify things far better than simply doing another set of statistical analysis. I'm still particularly interested if #2 is really true in the raobs (its not possible to do with satellites I suspect, but if it is true it means we need to massively rethink Fu et al. type analysis at least in the tropics) and would be interested in helping someone follow up on that ... I think in the future the Forster et al paper may be seen as the more scientifically significant result when Douglass et al is no longer cared about ...

Happy new year to you all.

Peter

--

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

Attachment Converted: "c:\eudora\attach\qbo_slide.ppt"

836. 1199303943.txt

#####

From: Susan Solomon <Susan.Solomon@noaa.gov>
To: P.Jones@uea.ac.uk, Kevin Trenberth <trenbert@ucar.edu>

mail.2008

Subject: Re: urban stuff
Date: Wed, 02 Jan 2008 14:59:03 -0700
Cc: Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

Phil

Thanks for the Benestad reference, which I hadn't seen and will read with interest.

Please keep me in the loop on your reprints.

I'm aware of the work with Dave Thompson, which is very interesting.

Happy new year to you too.

We can all look back on 2007 as a year in which we, the scientists, did a fantastic job.

best
Susan

At 8:59 PM +0000 1/2/08, P.Jones@uea.ac.uk wrote:

> Kevin, Susan,
> working on several things at the moment, so won't
> have much time for a few weeks. Rasmus Benestad of
> the Norwegian Met Service wrote a paper on a very similar
> earlier version of this Mckittrick/Michaels paper (both
> were in Climate Research). There is nothing new in this
> paper in JGR.
> The only thing new in both this JGR paper and the
> Douglass et al one in IJC is the awful reviewing!!!!
> Rebuttals help, but often the damage is done once the
> paper comes out. The MM paper is bad, but the reviewing
> is even worse. Why did MM refer to an erratum on their
> paper which is essentially the same? Any reviewer worth
> any salt should have spotted that and then they would have
> seen the Benestad comment, which MM surprisingly don't refer to.

>
> I'm hoping to submit a paper on urbanization soon -
> based on work with Chinese series - this relates to the
> fraud allegation against Wei-Chyung Wang that Kevin knows
> about.

>
> Also should be a press release tomorrow or Friday about
> the forecast for 2008 temperatures. La Nina looks like making
> it coolish - cooler just than all years since 2001 (including
> 2001) and 1998. Pointing out that 2001-2007 is 0.21 warmer
> than 1991-2000 which is exactly as it should be with ghg-related
> warming of 0.2 per decade.

>
> [Also working on something with Dave Thompson (Dave's laeding)
> that will have an ENSO-factored out (and COWL) global T series.]

>
> We're (with the Met Office) extending the press release
> due to the silly coverage in mid-December about global warming
> ending, as all years since 1998 are cooler than it. Mostly this
> was by people just parroting the same message from the same
> people. It is a case of people who should know better (and check
> their sources) just copying from people who don't know any
> better.

>

mail.2008

> Oh - forgot - Happy New Year!
>
> Any pictures on the IPCC web site of Oslo on Dec 10 !
>
> Patchy is on the front cover of the last issue of the 2007 in Nature.
>
> Cheers
> Phil
>
>
> Susan
>> Not me. Phil has been involved in various stuff related to this but I
>> am not up to speed. I'll cc him.
>> I recall some exchanges a while ago now.
>> Kevin
>>
>> Susan Solomon wrote:
>>> Kevin
>>> Happy new year to you. All's well here. Have you or other
>>> colleagues organized a rebuttal to the Mckitrick and Michaels JGR 2007
>>> material on urbanization? It's getting exposure, along with the
>>> Douglass et al. paper. On the latter, you probably know Ben Santer is
>>> preparing one.
>>> best
>>> Susan
>>
>> --
>> *****
>> Kevin E. Trenberth e-mail: trenbert@ucar.edu
>> Climate Analysis Section, www.cgd.ucar.edu/cas/trenbert.html
>> NCAR
>> P. O. Box 3000, (303) 497 1318
>> Boulder, CO 80307 (303) 497 1333 (fax)
>>
>> Street address: 1850 Table Mesa Drive, Boulder, CO 80305
>>
>>
>>

</x-flowed>

837. 1199325151.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: More significance testing stuff
Date: wed, 02 Jan 2008 20:52:31 -0800
Reply-to: santer1@llnl.gov
Cc: John.Lanzante@noaa.gov, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Sherwood Steven <steven.sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, 'Susan Solomon' <Susan.Solomon@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <>wigley@cgd.ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>

mail.2008

Dear Tom,

In the end, I decided to test the significance of trends in the $O(t)$ minus $M(t)$ difference time series, as you and John Lanzante have suggested. I still think that this "difference series test" is more appropriate when one is operating on a pair of time series with correlated variability (for example, if you wished to test whether an observed tropical T2LT trend was significantly different from the T2LT trend simulated in an AMIP experiment). But you and John convinced me that our response to Douglass et al. would be strengthened by using several different approaches to address the statistical significance of differences between modeled and observed temperature trends.

The Tables given below show the results from two different types of test. You've already seen the "TYPE1" or "PAIRED TREND" results. These involve $b\{O\}$ and $b\{M\}$, which represent any single pair of Observed and Modeled trends, with standard errors $s\{bO\}$ and $s\{bM\}$ (which are adjusted for temporal autocorrelation effects). As in our previous work (and as in related work by John Lanzante), we define the normalized trend difference d as:

$$d1 = (b\{O\} - b\{M\}) / \text{sqrt}[(s\{bO\})^2 + (s\{bM\})^2]$$

Under the assumption that $d1$ is normally distributed, values of $d1 > +1.96$ or < -1.96 indicate observed-minus-model trend differences that are significant at the 5% level, and one can easily calculate a p-value for each value of d . These p-values for the 98 pairs of trend tests (49 involving UAH data and 49 involving RSS data) are what we use for determining the total number of "hits", or rejections of the null hypothesis of no significant difference between modeled and observed trends. I note that each test is two-tailed, since we have no information a priori about the "direction" of the model trend (i.e., whether we expect the simulated trend to be significantly larger or smaller than observed).

The "TYPE2" results are the "DIFFERENCE SERIES" tests. These involve $O(t)$ and $M(t)$, which represent any single pair of modeled and observed layer-averaged temperature time series. One first defines the difference time series $D(t) = O(t) - M(t)$, and then calculates the trend $b\{D\}$ in $D(t)$ and its adjusted standard error, $s\{bD\}$. The test statistic is then simply $d2 = b\{D\} / s\{bD\}$. As in the case of the "PAIRED TREND" tests, we assume that $d2$ is normally distributed, and then calculate p-values for the 98 pairs of difference series tests.

As I mentioned in a previous email, the interpretation of the "DIFFERENCE SERIES" tests is a little complicated. Over half (35) of the 49 model simulations examined in the CCSP report include some form of volcanic forcing. In these 35 cases, differencing the $O(t)$ and $M(t)$ time series reduces the amplitude of this externally-forced component in $D(t)$. This will tend to reduce the overall temporal variability of $D(t)$, and hence reduce $s\{bD\}$, the standard error of the trend in $D(t)$. Such noise reduction should make it easier to identify true differences in the anthropogenically-forced components of $b\{O\}$ and $b\{D\}$. But since the internally-generated variability in $O(t)$ and $M(t)$ is uncorrelated, differencing $O(t)$ and $M(t)$ has the opposite effect of amplifying the noise, thus inflating $s\{bD\}$ and making it more difficult to identify model-versus-observed trend differences.

The results given below show that the "PAIRED TREND" and "DIFFERENCE SERIES" tests yield very similar rejection rates of the null hypothesis. The bottom line is that, regardless of which test we use, which significance level we stipulate, which observational dataset we use, or which atmospheric layer we focus on, there is no evidence to support

mail.2008

Douglass et al.'s assertion that all "UAH and RSS satellite trends are inconsistent with model results".

REJECTION RATES FOR STIPULATED 5% SIGNIFICANCE LEVEL				
Test type	No. of tests	T2 "Hits"	T2LT "Hits"	
1. OBS-vs-MODEL (TYPE1)	49 x 2 (98)	2 (2.04%)	1	(1.02%)
2. OBS-vs-MODEL (TYPE2)	49 x 2 (98)	2 (2.04%)	2	(2.04%)

REJECTION RATES FOR STIPULATED 10% SIGNIFICANCE LEVEL				
Test type	No. of tests	T2 "Hits"	T2LT "Hits"	
1. OBS-vs-MODEL (TYPE1)	49 x 2 (98)	4 (4.08%)	2	(2.04%)
2. OBS-vs-MODEL (TYPE2)	49 x 2 (98)	3 (3.06%)	3	(3.06%)

REJECTION RATES FOR STIPULATED 20% SIGNIFICANCE LEVEL				
Test type	No. of tests	T2 "Hits"	T2LT "Hits"	
1. OBS-vs-MODEL (TYPE1)	49 x 2 (98)	7 (7.14%)	5	(5.10%)
2. OBS-vs-MODEL (TYPE2)	49 x 2 (98)	10 (10.20%)	7	(7.14%)

As I've mentioned in previous emails, I think it's a little tricky to figure out the null distribution of rejection rates - i.e., the distribution that might be expected by chance alone. My gut feeling is that this is easiest to do by generating distributions of the d1 and d2 statistics using model control run data only. Use of Monte Carlo procedures gets into issues of whether one should use "block resampling", and attempt to preserve the characteristic decorrelation times of the model and observational data being tested, etc., etc.

Thanks very much to all of you for your advice and comments. I still believe that there is considerable merit in a brief response to Douglass et al. I think this could be done relatively quickly. From my perspective, this response should highlight four issues:

- 1) It should identify the flaws in the statistical approach used by Douglass et al. to compare modeled and observed trends.
- 2) It should do the significance testing properly, and report on the results of "PAIRED TREND" and "DIFFERENCE SERIES" tests.
- 3) It should show something similar to the figure that Leo recently distributed (i.e., zonal-mean trend profiles in various versions of the RAOBCORE data), and highlight the fact that the structural uncertainty in sonde-based estimates of tropospheric temperature change is much larger than was claimed in Douglass et al.
- 4) It should note and discuss the considerable body of "complementary evidence" supporting the finding that the tropical lower troposphere has warmed over the satellite era.

with best regards,

Ben

Thomas.R.Karl wrote:

- > Thanks Ben,
- >
- > You have been busy! I sent Tom an email before reading the last
- > paragraph of this note. Recognizing the "random" placement of ENSO in
- > the models and volcanic effects (in a few) and the known impact of the
- > occurrence of these events on the trends, I think it is appropriate that
- > the noise and related uncertainty about the trend differences be

mail.2008

> increased. Amplifying the noise could be argued as an appropriate
> conservative approach, since we know that these events are confounding
> our efforts to see differences between models and obs w/r to greenhouse
> forcing.

>
> I know it is more work, but I think it does make sense to calculate
> $O(1)-M(1)$, $O(2)-M(2)$ $O(n)-M(n)$ for all combinations of observed
> data sets and model simulations. You could test for significance by
> using a Monte Carlo bootstrap approach by randomizing the years for both
> models and data.

>
> Regards, Tom

>
>
> Ben Santer said the following on 12/26/2007 9:50 PM:
>> Dear John,

>>
>> Thanks for your email. As usual, your comments were constructive and
>> thought-provoking. I've tried to do some of the additional tests that
>> you suggested, and will report on the results below.

>>
>> But first, let's have a brief recap. As discussed in my previous
>> emails, I've tested the significance of differences between trends in
>> observed MSU time series and the trends in synthetic MSU temperatures
>> in a multi-model "ensemble of opportunity". The "ensemble of
>> opportunity" comprises results from 49 realizations of the CMIP-3
>> "20c3m" experiment, performed with 19 different A/OGCMs. This is the
>> same ensemble that was analyzed in Chapter 5 of the CCSP Synthesis and
>> Assessment Product 1.1.

>> I've used observational results from two different groups (RSS and
>> UAH). From each group, we have results for both T2 and T2LT. This
>> yields a total of 196 different tests of the significance of
>> observed-versus-model trend differences (2 observational datasets x 2
>> layer-averaged temperatures x 49 realizations of the 20c3m
>> experiment). Thus far, I've tested the significance of trend
>> differences using T2 and T2LT data spatially averaged over oceans only
>> (both 20N-20S and 30N-30S), as well as over land and ocean (20N-20S).
>> All results described below focus on the land and ocean results, which
>> facilitates a direct comparison with Douglass et al.

>>
>> Here was the information that I sent you on Dec. 14th:

>>
>> COMBINED LAND/OCEAN RESULTS (WITH STANDARD ERRORS ADJUSTED FOR
>> TEMPORAL AUTOCORRELATION EFFECTS; SPATIAL AVERAGES OVER 20N-20S;
>> ANALYSIS PERIOD 1979 TO 1999)

>>
>> T2LT tests, RSS observational data: 0 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.

>> T2LT tests, UAH observational data: 1 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.

>>
>> T2 tests, RSS observational data: 1 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.

>> T2 tests, UAH observational data: 1 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.

>>
>> In other words, at a stipulated significance level of 5% (for a
>> two-tailed test), we rejected the null hypothesis of "No significant
>> difference between observed and simulated tropospheric temperature
>> trends" in only 1 out of 98 cases (1.02%) for T2LT and 2 out of 98
>> cases (2.04%) for T2.

>>
>> You asked, John, how we might determine a baseline for judging the

mail.2008

>> likelihood of obtaining the 'observed' rejection rate by chance alone.
>> You suggested use of a bootstrap procedure involving the model data
>> only. In this procedure, one of the 49 20c3m realizations would be
>> selected at random, and would constitute the "surrogate observations".
>> The remaining 48 members would be randomly sampled (with replacement)
>> 49 times. The significance of the difference between the surrogate
>> "observed" trend and the 49 simulated trends would then be assessed.
>> This procedure would be repeated many times, yielding a distribution
>> of rejection rates of the null hypothesis.

>>
>> As you stated in your email, "The actual number of hits, based on the
>> real observations could then be referenced to the Monte Carlo
>> distribution to yield a probability that this could have occurred by
>> chance."

>>
>> One slight problem with your suggested bootstrap approach is that it
>> convolves the trend differences due to internally-generated
>> variability with trend differences arising from inter-model
>> differences in both climate sensitivity and in the forcings applied in
>> the 20c3m experiment. So the distribution of "hits" (as you call it;
>> or "rejection rates" in my terminology) is not the distribution that
>> one might expect due to chance alone.

>>
>> Nevertheless, I thought it would be interesting to generate a
>> distribution of "rejection rates" based on model data only. Rather
>> than implementing the resampling approach that you suggested, I
>> considered all possible combinations of trend pairs involving model
>> data, and performed the paired difference test between the trend in
>> each 20c3m realization and in each of the other 48 realizations. This
>> yields a total of 2352 (49 x 48) non-identical pairs of trend tests
>> (for each layer-averaged temperature time series).

>> Here are the results:

>>
>> T2: At a stipulated 5% significance level, 58 out of 2352 tests
>> involving model data only (2.47%) yielded rejection of the null
>> hypothesis of no significant difference in trend.

>>
>> T2LT: At a stipulated 5% significance level, 32 out of 2352 tests
>> involving model data only (1.36%) yielded rejection of the null
>> hypothesis of no significant difference in trend.

>>
>> For both layer-averaged temperatures, these numbers are slightly
>> larger than the "observed" rejection rates (2.04% for T2 and 1.02% for
>> T2LT). I would conclude from this that the statistical significance of
>> the differences between the observed and simulated MSU tropospheric
>> temperature trends is comparable to the significance of the
>> differences between the simulated 20c3m trends from any two CMIP-3
>> models (with the proviso that the simulated trend differences arise
>> not only from internal variability, but also from inter-model
>> differences in sensitivity and 20th century forcings).

>>
>> Since I was curious, I thought it would be fun to do something a
>> little closer to what you were advocating, John - i.e., to use model
>> data to look at the statistical significance of trend differences that
>> are NOT related to inter-model differences in the 20c3m forcings or in
>> climate sensitivity. I did this in the following way. For each model
>> with multiple 20c3m realizations, I tested each realization against
>> all other (non-identical) realizations of that model - e.g., for a
>> model with an 20c3m ensemble size of 5, there are 20 paired trend
>> tests involving non-identical data. I repeated this procedure for the
>> next model with multiple 20c3m realizations, etc., and accumulated
>> results. In our CCSP report, we had access to 11 models with multiple

mail.2008

>> 20c3m realizations. This yields a total of 124 paired trend tests for
>> each layer-averaged temperature time series of interest.

>> For both T2 and T2LT, NONE of the 124 paired trend tests yielded
>> rejection of the null hypothesis of no significant difference in trend
>> (at a stipulated 5% significance level).

>> You wanted to know, John, whether these rejection rates are sensitive
>> to the stipulated significance level. As per your suggestion, I also
>> calculated rejection rates for a 20% significance level. Below, I've
>> tabulated a comparison of the rejection rates for tests with 5% and
>> 20% significance levels. The two "rows" of "MODEL-vs-MODEL" results
>> correspond to the two cases I've considered above - i.e., tests
>> involving 2352 trend pairs (Row 2) and 124 trend pairs (Row 3). Note
>> that the "OBSERVED-vs-MODEL" row (Row 1) is the combined number of
>> "hits" for 49 tests involving RSS data and 49 tests involving UAH data:

>> REJECTION RATES FOR STIPULATED 5% SIGNIFICANCE LEVEL:

Test type	No. of tests	T2 "Hits"	T2LT "Hits"
Row 1. OBSERVED-vs-MODEL	49 x 2	2 (2.04%)	1 (1.02%)
Row 2. MODEL-vs-MODEL	2352	58 (2.47%)	32 (1.36%)
Row 3. MODEL-vs-MODEL	124	0 (0.00%)	0 (0.00%)

>> REJECTION RATES FOR STIPULATED 20% SIGNIFICANCE LEVEL:

Test type	No. of tests	T2 "Hits"	T2LT "Hits"
Row 1. OBSERVED-vs-MODEL	49 x 2	7 (7.14%)	5 (5.10%)
Row 2. MODEL-vs-MODEL	2352	176 (7.48%)	100 (4.25%)
Row 3. MODEL-vs-MODEL	124	8 (6.45%)	6 (4.84%)

>> So what can we conclude from this?

>> 1) Irrespective of the stipulated significance level (5% or 20%), the
>> differences between the observed and simulated MSU trends are, on
>> average, substantially smaller than we might expect if we were
>> conducting these tests with trends selected from a purely random
>> distribution (i.e., for the "Row 1" results, 2.04 and 1.02% << 5%, and
>> 7.14% and 5.10% << 20%).

>> 2) Why are the rejection rates for the "Row 3" results substantially
>> lower than 5% and 20%? Shouldn't we expect - if we are only testing
>> trend differences between multiple realizations of the same model,
>> rather than trend differences between models - to obtain rejection
>> rates of roughly 5% for the 5% significance tests and 20% for the 20%
>> tests? The answer is clearly "no". The "Row 3" results do not involve
>> tests between samples drawn from a population of randomly-distributed
>> trends! If we were conducting this paired test using randomly-sampled
>> trends from a long control simulation, we would expect (given a
>> sufficiently large sample size) to eventually obtain rejection rates
>> of 5% and 20%. But our "Row 3" results are based on paired samples
>> from individual members of a given model's 20c3m experiment, and thus
>> represent both signal (response to the imposed forcing changes) and
>> noise - not noise alone. The common signal component makes it more
>> difficult to reject the null hypothesis of no significant difference
>> in trend.

>> 3) Your point about sensitivity to the choice of stipulated
>> significance level was well-taken. This is obvious by comparing "Row
>> 3" results in the 5% and 20% test cases.

>> 4) In both the 5% and 20% cases, the rejection rate for paired tests
>> involving model-versus-observed trend differences ("Row 1") is

mail.2008

>> comparable to the rejection rate for tests involving inter-model trend
>> differences ("Row 2") arising from the combined effects of differences
>> in internal variability, sensitivity, and applied forcings. On
>> average, therefore, model versus observed trend differences are not
>> noticeably more significant than the trends between any given pair of
>> CMIP-3 models. [N.B.: This inference is not entirely justified, since,
>> "Row 2" convolves the effects of both inter-model differences and
>> "within model" differences arising from the different manifestations
>> of natural variability superimposed on the signal. We would need a
>> "Row 4", which involves 19 x 18 paired tests of model results, using
>> only one 20c3m realization from each model. I'll generate "Row 4"
>> tomorrow.]

>>
>> John, you also suggested that we might want to look at the statistical
>> significance of trends in time series of differences - e.g., in $O(t)$
>> minus $M(t)$, or in $M_1(t)$ minus $M_2(t)$, where "O" denotes observations,
>> and "M" denotes model, and t is an index of time in months. While I've
>> done this in previous work (for example in the Santer et al. 2000 JGR
>> paper, where we were looking at the statistical significance of trend
>> differences between multiple observational upper air temperature
>> datasets), I don't think it's advisable in this particular case. As
>> your email notes, we are dealing here with A/OGCM results in which the
>> phasing of El Ninos and La Ninas (and the effects of ENSO variability
>> on T2 and T2LT) differs from the phasing in the real world. So
>> differencing $M(t)$ from $O(t)$, or $M_2(t)$ from $M_1(t)$, probably actually
>> amplifies rather than damps noise, particularly in the tropics, where
>> the externally-forced component of $M(t)$ or $O(t)$ over 1979 to 1999 is
>> only a relatively small fraction of the overall variance of the time
>> series. I think this amplification of noise is a disadvantage in
>> assessing whether trends in $O(t)$ and $M(t)$ are significantly different.

>>
>> Anyway, thanks again for your comments and suggestions, John. They
>> gave me a great opportunity to ignore the hundreds of emails that
>> accumulated in my absence, and instead do some science!

>>
>> With best regards,

>>
>> Ben

>>
>> John Lanzante wrote:

>>> Ben,

>>>
>>> Perhaps a resampling test would be appropriate. The tests you have
>>> performed
>>> consist of pairing an observed time series (UAH or RSS MSU) with each
>>> one
>>> of 49 GCM times series from your "ensemble of opportunity". Significance
>>> of the difference between each pair of obs/GCM trends yields a certain
>>> number of "hits".

>>>
>>> To determine a baseline for judging how likely it would be to obtain the
>>> given number of hits one could perform a set of resampling trials by
>>> treating one of the ensemble members as a surrogate observation. For
>>> each
>>> trial, select at random one of the 49 GCM members to be the
>>> "observation".
>>> >From the remaining 48 members draw a bootstrap sample of 49, and
>>> perform
>>> 49 tests, yielding a certain number of "hits". Repeat this many times to
>>> generate a distribution of "hits".

>>>
>>> The actual number of hits, based on the real observations could then be
>>> referenced to the Monte Carlo distribution to yield a probability

mail.2008

>>> that this
>>> could have occurred by chance. The basic idea is to see if the observed
>>> trend is inconsistent with the GCM ensemble of trends.
>>>
>>> There are a couple of additional tweaks that could be applied to your
>>> method.
>>> You are currently computing trends for each of the two time series in
>>> the
>>> pair and assessing the significance of their differences. Why not first
>>> create a difference time series and assess the significance of its
>>> trend?
>>> The advantage of this is that you would reduce somewhat the
>>> autocorrelation
>>> in the time series and hence the effect of the "degrees of freedom"
>>> adjustment. Since the GCM runs are based on coupled model runs this
>>> differencing would help remove the common externally forced variability,
>>> but not internally forced variability, so the adjustment would still be
>>> needed.
>>>
>>> Another tweak would be to alter the significance level used to assess
>>> differences in trends. Currently you are using the 5% level, which
>>> yields
>>> only a small number of hits. If you made this less stringent you
>>> would get
>>> potentially more weaker hits. But it would all come out in the wash
>>> so to
>>> speak since the number of hits in the Monte Carlo simulations would
>>> increase
>>> as well. I suspect that increasing the number of expected hits would
>>> make the
>>> whole procedure more powerful/efficient in a statistical sense since you
>>> would no longer be dealing with a "rare event". In the current
>>> scheme, using
>>> a 5% level with 49 pairings you have an expected hit rate of $0.05 \times$
>>> $49 = 2.45$.
>>> For example, if instead you used a 20% significance level you would
>>> have an
>>> expected hit rate of $0.20 \times 49 = 9.8$.
>>>
>>> I hope this helps.
>>>
>>> On an unrelated matter, I'm wondering a bit about the different
>>> versions of
>>> Leo's new radiosonde dataset (RAOBCORE). I was surprised to see that the
>>> latest version has considerably more tropospheric warming than I
>>> recalled
>>> from an earlier version that was written up in JCLI in 2007. I have a
>>> couple of questions that I'd like to ask Leo. One concern is that if
>>> we use
>>> the latest version of RAOBCORE is there a paper that we can reference --
>>> if this is not in a peer-reviewed journal is there a paper in
>>> submission?
>>> The other question is: could you briefly comment on the differences
>>> in methodology used to generate the latest version of RAOBCORE as
>>> compared to the version used in JCLI 2007, and what/when/where did
>>> changes occur to
>>> yield a stronger warming trend?
>>>
>>> Best regards,
>>>
>>> _____John
>>>
>>>

mail.2008

>>>
>>> On Saturday 15 December 2007 12:21 pm, Thomas.R.Karl wrote:
>>>> Thanks Ben,
>>>>
>>>> You have the makings of a nice article.
>>>>
>>>> I note that we would expect to 10 cases that are significantly
>>>> different by chance (based on the 196 tests at the .05 sig level).
>>>> You found 3. With appropriately corrected Leopold I suspect you
>>>> will find there is indeed stat sig. similar trends incl.
>>>> amplification. Setting up the statistical testing should be
>>>> interesting with this many combinations.

>>>> Regards, Tom
>>>

>>
>>
>

> --
>

> *Dr. Thomas R. Karl, L.H.D.*
>
> */Director/*//
>
> NOAA's National Climatic Data Center
>
> Veach-Baley Federal Building
>
> 151 Patton Avenue
>
> Asheville, NC 28801-5001
>
> Tel: (828) 271-4476
>
> Fax: (828) 271-4246
>
> Thomas.R.Karl@noaa.gov <mailto:Thomas.R.Karl@noaa.gov>
>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

838. 1199458641.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Thanks for the photos of Nick !
Date: Fri, 04 Jan 2008 09:57:21 -0800
Reply-to: santer1@llnl.gov

mail.2008

<x-flowed>
Dear Phil,

I was very sorry to hear of Hannah's health problems. I hope she makes a speedy recovery. Please give her my best wishes, and tell her that there is life and love after divorce!

My Mom's cataract surgery did not go very well, and it looks like she won't be able to drive any longer. Nick and I are best placed to take care of her, so I'm trying to persuade her to move to California. So there could be some big changes in our lives in 2008.

Nick has turned into a fine young man. It's going to be tough to see him leave for college in three and a half years.

I share your frustration about having to devote valuable time to the rebuttal of crappy papers. Douglass et al. is truly awful. It should never have been published. Any residual respect I might have had for John Christy has now vanished. I can't believe that he's a coauthor on this garbage.

Best wishes to all of you from rainy Livermore,

Ben

Phil Jones wrote:

>
>> Ben,
> Thanks for the card and photos of Nick and your caving exploits
> with Tom and Karl !
> Had a quiet Christmas and New Year. We did get to see Poppy
> at Hannah's house in Deal in Kent. Matthew and Miranda came as well
> along with Ruth's mum - so she saw her great granddaughter.
> We were there as Hannah had to have another cyst removed from around
> her ovary - all is well and she's recovering. Ruth has been with her since
> mid-December. Hannah had an earlier cyst when she was 12, but this time
> they managed to save the ovary. She still needs to see a gynaecologist to
> see if the ovary is still working OK.
> 2007 hasn't been a great year for Hannah, as she has started divorce
> proceedings from her husband (Gordon). They only married in 2005. He
> seemed fine initially, but has had at least 2 affairs.
>
> Keep up the good work on the Douglass et al comment. I'm trying to
> finish
> a few things in the next couple of months. I will comment on drafts if
> you want.
> Susan Solomon is trying to encourage me to respond to this piece of
> rubbish. I'll try and encourage Rasmus Benestad of DNMI to respond. He did
> so last time to a very similar paper in Climate Research. MM don't
> refer to
> that and MM don't use RSS data! Their analysis is flawed anyway, but it
> would
> all go away if they had used RSS instead of UAH!
>
> What gets me is who are the reviewers of these two awful papers. I know
> editors have a hard time finding reviewers, but they must have known that
> both papers were likely awful. It seems that editors (even of these
> two used-to-be OK
> journals) just want more papers.
>
> Sad day - coming in to hear of Bert Bolin's death.
>
> Cheers

mail.2008

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

>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

839. 1199466465.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Humphrey, Kathryn (CEOSA)" <kathryn.humphrey@DEFRA.GSI.GOV.UK>, "Stephens, A (Ag)" <A.Stephens@rl.ac.uk>
Subject: RE: Questions on the weather generator
Date: Fri Jan 4 12:07:45 2008
Cc: "David Sexton" <david.sexton@metoffice.gov.uk>, <C.G.Kilsby@newcastle.ac.uk>, "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>

Kathryn,

will I did talk to the Metro yesterday - no idea what they used. Maybe a few
have read it - before copies are tossed around on the tube!
Added Geoff on this email.

Ag has answered the second question. I may come back to that after
trying to answer the first part.

There are two aspects to the WG work we're doing. The first, which I've
mentioned
on a number of occasions, is to prove that the perturbation process used with
the WG
works. Colin Harpham sent around a load of plots to Chris/Ag/David/Geoff just
before
Christmas. I have a rough draft of a paper on this which I sent to Chris
yesterday. This
involves the UKCIP08 WG, but is totally independent of the change factors David
is
developing for UKCIP08. This uses some earlier HadRM3 model runs. The WG is fit
to

10 grid box series across the UK and then perturbed according to the differences between the future model integrations and the control runs. We then generate future weather and show that its characteristics are similar to what HadRM3 got directly. This has used the same change factors (same variables) but from a different set of RCM runs. The whole purpose of this exercise is to show that the perturbation process works. The only way we can test this is to use RCM model runs - because they have future runs with a big climate change. We can't use past weather data as it doesn't have enough of a climate change. This is validation of the perturbation process. We can additionally validate the WG using observational data - which we've done earlier.

Return to Q2. Ag has said how the model variants get chosen. The model variants used have a variety of ways of being chosen. Let's say we start with the 50th percentile for rainfall. We select all model variants between 45 and 55%. Then we want temperature at the 90th percentile. We then do a second selection of the variants already selected that have temperature changes between 85 and 95%. As we had initially 10,000 variants, the first selection reduced this to a 1000 (as we chose 10% of them). The second selection reduced this to 100 (as we've again chosen only 10% of them). Now with these 100 variants, most users will average the change factors (from David) across these 100. These average change factors (which will approximately be at the 50% and 90% value for precipitation and temperature respectively) get passed to the WG. The WG then simulates 100 runs of 30 years - for the already pre-selected location (small area) and future period.

There are obviously loads of permutations as we will be allowing users to select all percentile levels (singly for temperature or precipitation) or jointly for both from 5 to 95 % in steps of 5. The percentile levels can be chosen based on seasons (4) and years (1). If you select summer say, users will also get the rest of the year - using the change factors that go along with those for the selected model variants.

Another possibility is to select one model variant within the chosen percentile bands and pass these change factors to the WG.

There are other possibilities, but I think we've limited the choices to these two. The other possibility was a variant (can't think of a better word here - but not related to the model variants) to the first. As you have 100 chosen model variants in this example, you could chose one at random or allow each of the 100 WG generated integrations to be based on a different one of the model variants. These sequences will likely have greater variability than that based on the average of the 100 or that based on the single model variant.

I think this may open up a can of worms with Ag when he reads it !

mail.2008

whichever of these are chosen, the use should still run the WG for 100 30-year sequences.

I think I've made the last bit on model variant selection complicated and haven't gone back to look at what Ag has written in the User Guidance. It ought to tell you how the change factors that the WG needs will get selected.

Cheers
Phil

At 10:07 04/01/2008, Humphrey, Kathryn (CEOSA) wrote:

Hi Ag,

Yes that makes perfect sense in terms of selecting one/several model variant/s, thanks.

I'm still a bit confused about the utility of random sampling though as this won't give you results for a particular probability level (will it?). I think Phil was going to get back to me on this as well as the change factors question.

Phil, I liked your quote in the Metro this morning!

Kathryn

From: Stephens, A (Ag) [[1]mailto:A.Stephens@rl.ac.uk]
Sent: 04 January 2008 08:56
To: Humphrey, Kathryn (CEOSA)
Cc: Phil Jones; David Sexton; C.G.Kilsby@newcastle.ac.uk
Subject: RE: Questions on the weather generator
Hi Kathryn,

I can comment on your second question. Here is my understanding:

run. Firstly, users must run a minimum of 100 WG runs regardless of which ones they This is to enforce the use of a "probabilistic" approach.

Selection by model variant will only make sense once a user has produced some runs.

After any run they will have access to the model variant IDs that were used. The use case that gave rise to us including "selection by model variant ID" was as follows:

1. Person X does some WG runs (sampling by whatever method she chooses).
2. She uses/analyses a set of runs to produce some interesting results.
3. She is keen to do more/different analyses using the model variants that represented that part of parameter space.
4. She has the list of model variant IDs so she can publish these so that others can use them or she can re-use them herself in other experiments.
5. Person Y can read about what Person X did and re-produce exactly her results, or use the same set of interesting model variants for some other experiments.

Does that make sense?

Cheers,

Ag

From: Humphrey, Kathryn (CEOSA) [[2]mailto:kathryn.humphrey@DEFRA.GSI.GOV.UK]
Sent: 03 January 2008 16:58
To: Stephens, A (Ag)
Subject: FW: Questions on the weather generator

From: Humphrey, Kathryn (CEOSA)
Sent: 03 January 2008 16:55
To: 'Phil Jones'; 'Chris Kilsby'; 'Stephens, Ag'
Subject: Questions on the weather generator
Phil/Chris/Ag,

I'm putting together a "quick and easy" presentation on the UKCIP08 methodology for Defra officials to give them some idea of how it's all done so they can better appreciate what's it's potential uses may, and may not, be. However I'm getting stuck still on some of the WG methodology! Can you help? (I'm not planning on telling them this level of detail about the WG but am just bothered by the issues below).

I'm firstly confused about the RCM change factors; are you using these to validate the WG runs (which I do understand) or to generate them (which I don't as I thought they were being generated using the data in final PDFs themselves)? And I'm still confused about the reasons for allowing users to select runs by model variant. I think by model variant you mean each perturbed version of HadCM3 or other single model run or emulator result that creates a point in parameter space. Is this right? If so then I understand why you can't run your WG on all model variants (too many) so selecting a random sample is a representation of parameter space. But my initial understand of how the WG works is that you pick a point on the PDF (say 50th percentile) with a given probability and run the WG for that point. But this doesn't make sense if you are allowing users to select random/ single model variants seasons etc. because these won't reflect a particular percentile. Maybe it's the case that you don't need a particular percentile for whatever use the WG data is for, but if you don't know, how do you know how likely your WG output is and therefore what to do with the result in terms of planning?

Apologies for my ignorance and assistance would be gratefully received!

Kind Regards,

Kathryn

Kathryn Humphrey

Climate Change Impacts and Adaptation Team, Defra
Zone 3F Ergon House, Horseferry Road, London, SW1P 3JR
tel 0207 238 3362 fax 0207 238 3341

Department for Environment, Food and Rural Affairs (Defra)

This email and any attachments is intended for the named recipient only. If you have received it in error you have no authority to use, disclose, store or copy any of its contents and you should destroy it and inform the sender.

mail.2008

whilst this email and associated attachments will have been checked for known viruses whilst within Defra systems we can accept no responsibility once it has left our systems. Communications on Defra's computer systems may be monitored and/or recorded to secure the effective operation of the system and for other lawful purposes.

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:A.Stephens@rl.ac.uk>
2. <mailto:kathryn.humphrey@DEFRA.GSI.GOV.UK>

840. 1199926335.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>
Subject: Update on response to Douglass et al.
Date: Wed, 09 Jan 2008 19:52:15 -0800
Reply-to: santer1@llnl.gov

<x-flowed>
Dear folks,

I just wanted to update you on my progress in formulating a response to the Douglass et al. paper in the International Journal of Climatology (IJC). There have been several developments.

First, I contacted Science to gauge their level of interest in publishing a response to Douglass et al. I thought it was worthwhile to "test the water" before devoting a lot of time to the preparation of a manuscript for submission to Science. I spoke with Jesse Smith, who handles most of the climate-related papers at Science magazine.

The bottom line is that, while science is interested in this issue (particularly since Douglass et al. are casting doubt on the findings of the 2005 Santer et al. Science paper), Jesse Smith thought it was highly unlikely that Science would carry a rebuttal of work published in a different journal (IJC). Regretfully, I agree. Our response to Douglass et al. does not contain any fundamentally new science - although it does contain some new and interesting work (see below).

mail.2008

It's an unfortunate situation. Singer is promoting the Douglass et al. paper as a startling "new scientific evidence", which undercuts the key conclusions of the IPCC and CCSP Reports. Christy is using the Douglass et al. paper to argue that his UAH group is uniquely positioned to perform "hard-nosed" and objective evaluation of model performance, and that it's dangerous to leave model evaluation in the hands of biased modelers. Much as I would like to see a high-profile rebuttal of Douglass et al. in a journal like Science or Nature, it's unlikely that either journal will publish such a rebuttal.

So what are our options? Personally, I'd vote for GRL. I think that it is important to publish an expeditious response to the statistical flaws in Douglass et al. In theory, GRL should be able to give us the desired fast turnaround time. Would GRL accept our contribution, given that the Douglass et al. paper was published in IJC? I think they would - we've done a substantial amount of new work (see below), and can argue, with some justification, that our contribution is more than just a rebuttal of Douglass et al.

Why not go for publication of a response in IJC? According to Phil, this option would probably take too long. I'd be interested to hear any other thoughts you might have on publication options.

Now to the science (with a lower-case "s"). I'm appending three candidate Figures for a GRL paper. The first Figure was motivated by discussions I've had with Karl Taylor and Tom Wigley. It's an attempt to convey the differences between our method of comparing observed and simulated trends (panel A) and the approach used by Douglass et al. (panel B).

In our method, we account for both statistical uncertainties in fitting least-squares linear trends to noisy, temporally-autocorrelated data and for the effects of internally-generated variability. As I've described in previous emails, we compare each of the 49 simulated T2 and T2LT trends (i.e., the same multi-model ensemble used in our 2005 Science paper and in the 2006 CCSP Report) with observed T2 and T2LT trends obtained from the RSS and UAH groups. Our 2-sigma confidence intervals on the model and observed trends are estimated as in Santer et al. (2000). [Santer, B.D., T.M.L. Wigley, J.S. Boyle, D.J. Gaffen, J.J. Hnilo, D. Nychka, D.E. Parker, and K.E. Taylor, 2000: Statistical significance of trends and trend differences in layer-average atmospheric temperature time series, *J. Geophys. Res.*, 105, 7337-7356]

The method that Santer et al. (2000) used to compute "adjusted" trend confidence intervals accounts for the fact that, after fitting a trend to T2 or T2LT data, the regression residuals are typically highly autocorrelated. If this autocorrelation is not accounted for, one could easily reach incorrect decisions on whether the trend in an individual time series is significantly different from zero, or whether two time series have significantly different trends. Santer et al. (2000) accounted for temporal autocorrelation effects by estimating $r\{1\}$, the lag-1 autocorrelation of the regression residuals, using $r\{1\}$ to calculate an effective sample size $n\{e\}$, and then using $n\{e\}$ to determine an adjusted standard error of the least-squares linear trend. Panel A of Figure 1 shows the 2-sigma "adjusted" standard errors for each individual trend. Models with excessively large tropical variability (like FGOALS-g1.0 and GFDL-CM2.1) have large adjusted standard errors. Models with coarse-resolution OGCMs and low-amplitude ENSO variability (like the GISS-AOM) have smaller than observed adjusted standard errors. Neglect of volcanic forcing (i.e., absence of El Chichon and Pinatubo-induced temperature variability) can also contribute to smaller than observed standard errors, as in CCCma-CGCM3.1(T47).

The dark and light grey bars in Panel A show (respectively) the 1- and 2-sigma standard errors for the RSS T2LT trend. As is visually obvious, 36 of the 49 model trends are within 1 standard error of the RSS trend, and 47 of the 49 model trends are within 2 standard errors of the RSS trend.

I've already explained our "paired trend test" procedure for calculating the statistical significance of the model-versus-observed trend differences. This involves the normalized trend difference $d1$:

$$d1 = (b\{O\} - b\{M\}) / \text{sqrt}[(s\{bO\})^2 + (s\{bM\})^2]$$

where $b\{O\}$ and $b\{M\}$ represent any single pair of Observed and Modeled trends, with adjusted standard errors $s\{bO\}$ and $s\{bM\}$.

Under the assumption that $d1$ is normally distributed, values of $d1 > +1.96$ or < -1.96 indicate observed-minus-model trend differences that are significant at some stipulated significance level, and one can easily calculate a p-value for each value of $d1$. These p-values for the 98 pairs of trend tests (49 involving UAH data and 49 involving RSS data) are what we use for determining the total number of "hits", or rejections of the null hypothesis of no significant difference between modeled and observed trends. I note that each test is two-tailed, since we have no information a priori about the "direction" of the model trend (i.e., whether we expect the simulated trend to be significantly larger or smaller than observed).

REJECTION RATES FOR "PAIRED TREND TESTS, OBS-VS-MODEL

Stipulated sign. level	No. of tests	T2 "Hits"	T2LT "Hits"
5%	49 x 2 (98)	2 (2.04%)	1 (1.02%)
10%	49 x 2 (98)	4 (4.08%)	2 (2.04%)
15%	49 x 2 (98)	7 (7.14%)	5 (5.10%)

Now consider Panel B of Figure 1. It helps to clarify the differences between the Douglass et al. comparison of model and observed trends and our own comparison. The black horizontal line ("Multi-model mean trend") is the T2LT trend in the 19-model ensemble, calculated from model ensemble mean trends (the colored symbols). Douglass et al.'s "consistency criterion", $\sigma\{SE\}$, is given by:

$$\sigma\{SE\} = \sigma / \text{sqrt}(N - 1)$$

where σ is the standard deviation of the 19 ensemble-mean trends, and N is 19. The orange and yellow envelopes denote the 1- and 2- $\sigma\{SE\}$ regions.

Douglass et al. use $\sigma\{SE\}$ to decide whether the multi-model mean trend is consistent with either of the observed trends. They conclude that the RSS and UAH trends lie outside of the yellow envelope (the 2- $\sigma\{SE\}$ region), and interpret this as evidence of a fundamental inconsistency between modeled and observed trends. As noted previously, Douglass et al. obtain this result because they fail to account for statistical uncertainty in the estimation of the RSS and UAH trends. They ignore the statistical error bars on the RSS and UAH trends (which are shown in Panel A). As is clear from Panel A, the statistical error bars on the RSS and UAH trends overlap with the Douglass et al. 2- $\sigma\{SE\}$ region. Had Douglass et al. accounted for statistical uncertainty in estimation of the observed trends, they would have been unable to conclude that all "UAH and RSS satellite trends are inconsistent with model trends".

The second Figure plots values of our test statistic ($d1$) for the

mail.2008

"paired trend test". The grey histogram is based on the values of d_1 for the 49 tests involving the RSS T2LT trend and the simulated T2LT trends from 20c3m runs. The green histogram is for the 49 paired trend tests involving model 20c3m data and the UAH T2LT trend. Note that the d_1 distribution obtained with the UAH data is negatively skewed. This is because the numerator of the d_1 test statistic is $b\{0\} - b\{M\}$, and the UAH tropical T2LT trend over 1979-1999 is smaller than most of the model trends (see Figure 1, panel A).

The colored dots are values of the d_1 test statistic for what I referred to previously as "TYPE2" tests. These tests are limited to the M models with multiple realizations of the 20c3m experiment. Here, $M = 11$. For each of these M models, I performed paired trend tests for all C unique combinations of trends pairs. For example, for a model with 5 realizations of the 20c3m experiment, like GISS-EH, $C = 10$. The significance of trend differences is solely a function of "within-model" effects (i.e., is related to the different manifestations of natural internal variability superimposed on the underlying forced response). There are a total of 62 paired trend tests. Note that the separation of the colored symbols on the y-axis is for visual display purposes only, and facilitates the identification of results for individual models.

The clear message from Figure 2 is that the values of d_1 arising from internal variability alone are typically as large as the d_1 values obtained by testing model trends against observational data. The two negative "outlier" values of d_1 for the model-versus-observed trend tests involve the large positive trend in CCCma-CGCM3.1(T47). If you have keen eagle eyes, you'll note that the distribution of colored symbols is slightly skewed to the negative side. If you look at Panel A of Figure 1, you'll see that this skewness arises from the relatively small ensemble sizes. Consider results for the 5-member ensemble of 20c3m trends from the MRI-CGCM2.3.2. The trend in realization 1 is close to zero; trends in realizations 2, 3, 4, and 5 are large, positive, and vary between 0.27 to 0.37 degrees C/decade. So d_1 is markedly negative for tests involving realization 1 versus realizations 2, 3, 4, and 5. If we showed non-unique combinations of trend pairs (e.g., realization 2 versus realization 1, as well as 1 versus 2), the distribution of colored symbols would be symmetric. But I was concerned that we might be accused of "double counting" if we did this....

The third Figure is the most interesting one. You have not seen this yet. I decided to examine how the Douglass et al. "consistency test" behaves with synthetic data. I did this as a function of sample size N , for N values ranging from 19 (the number of models we used in the CCSP report) to 100. Consider the $N = 19$ case first. I generated 19 synthetic time series using an AR-1 model of the form:

$$xt(i) = a_1 * (xt(i-1) - a_m) + zt(i) + a_m$$

where a_1 is the coefficient of the AR-1 model, $zt(i)$ is a randomly-generated noise term, and a_m is a mean (set to zero here). Here, I set a_1 to 0.86, close to the lag-1 autocorrelation of the UAH T2LT anomaly data. The other free parameter is a scaling term which controls the amplitude of $zt(i)$. I chose this scaling term to yield a temporal standard deviation of $xt(i)$ that was close to the temporal standard deviation of the monthly-mean UAH T2LT anomaly data. The synthetic time series had the same length as the observational and model data (252 months), and monthly-mean anomalies were calculated in the same way as we did for observations and models.

For each of these 19 synthetic time series, I first calculated least-squares linear trends and adjusted standard errors, and then performed the "paired trends". The test involves all 171 unique pairs of

mail.2008

trends: $b_{\{1\}}$ versus $b_{\{2\}}$, $b_{\{1\}}$ versus $b_{\{3\}}$,... $b_{\{1\}}$ versus $b_{\{19\}}$, $b_{\{2\}}$ versus $b_{\{3\}}$, etc. I then calculate the rejection rates of the null hypothesis of "no significant difference in trend", for stipulated significance levels of 5%, 10%, and 20%. This procedure is repeated 1000 times, with 1000 different realizations of 19 synthetic time series. We can therefore build up a distribution of rejection rates for $N = 19$, and then do the same for $N = 20$, etc.

The "paired trend" results are plotted as the blue lines in Figure 3. Encouragingly, the percentage rejections of the null hypothesis are close to the theoretical expectations. The 5% significance tests yield a rejection rate of a little over 6%; 10% tests have a rejection rate of over 11%, and 20% tests have a rejection rate of 21%. I'm not quite sure why this slight positive bias arises. This bias does show some small sensitivity (1-2%) to choice of the a_1 parameter and the scaling term. Different choices of these parameters can give rejection rates that are closer to the theoretical expectation. But my parameter choices for the AR-1 model were guided by the goal of generating synthetic data with roughly the same autocorrelation and variance properties as the UAH data, and not by a desire to get as close as I possibly could to the theoretical rejection rates.

So why is there a small positive bias in the empirically-determined rejection rates? Perhaps Francis can provide us with some guidance here. Karl believes that the answer may be partly linked to the skewness of the empirically-determined rejection rate distributions. For example, for the $N = 19$ case, and for 5% tests, values of rejection rates in the 1000-member distribution range from a minimum of 0 to a maximum of 24%, with a mean value of 6.7% and a median of 6.4%. Clearly, the minimum value is bounded by zero, but the maximum is not bounded, and in rare cases, rejection rates can be quite large, and influences the mean. This inherent skewness must make some contribution to the small positive bias in rejection rates in the "paired trends" test.

What happens if we naively perform the paired trends test WITHOUT adjusting the standard errors of the trends for temporal autocorrelation effects? Results are shown by the black lines in Figure 3. If we ignore temporal autocorrelation, we get the wrong answer. Rejection rates for 5% tests are 60%!

We did not publish results from any of these synthetic data experiments in our 2000 JGR paper. In retrospect, this is a bit of a shame, since Figure 3 nicely shows that the adjustment for temporal autocorrelation effects works reasonably well, while failure to adjust yields completely erroneous results.

Now consider the red lines in Figure 3. These are the results of applying the Douglass et al. "consistency test" to synthetic data. Again, let's consider the $N = 19$ case first. I calculate the trends in all 19 synthetic time series. Let's consider the first of these 19 time series as the surrogate observations. The trend in this time series, $b_{\{1\}}$, is compared with the mean trend, $b_{\{\text{Synth}\}}$, computed from the remaining 18 synthetic time series. The Douglass $\sigma_{\{\text{SE}\}}$ is also computed from these 18 remaining trends. We then form a test statistic $d_2 = (b_{\{1\}} - b_{\{\text{Synth}\}}) / \sigma_{\{\text{SE}\}}$, and calculate rejection rates for the null hypothesis of no significant difference between the mean trend and the trend in the surrogate observations. This procedure is then repeated with the trend in time series 2 as the surrogate observations, and $b_{\{\text{Synth}\}}$ and $\sigma_{\{\text{SE}\}}$ calculated from time series 1, 3, 4,...19. This yields 19 different tests of the null hypothesis. Repeat 1,000 times, and build up a distribution of rejection rates, as in the "paired trends" test.

mail.2008

The results are truly alarming. Application of the Douglass et al. "consistency test" to synthetic data - data generated with the same underlying AR-1 model! - leads to rejection of the above-stated null hypothesis at least 65% of the time (for N = 19, 5% significance tests).

As expected, rejection rates for the Douglass consistency test rise as N increases. For N = 100, rejection rates for 5% tests are nearly 85%. As my colleague Jim Boyle succinctly put it when he looked at these results, "This is a pretty hard test to pass".

I think this nicely illustrates the problems with the statistical approach used by Douglass et al. If you want to demonstrate that modeled and observed temperature trends are fundamentally inconsistent, you devise a fundamentally flawed test is very difficult to pass.

I hope to have a first draft of this stuff written up by the end of next week. If Leo is agreeable, Figure 4 of this GRL paper would show the vertical profiles of tropical temperature trends in the various versions of the RAOBCORE data, plus model results.

Sorry to bore you with all the gory details. But as we've seen from Douglass et al., details matter.

with best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

Attachment Converted: "c:\eudora\attach\santer_fig01.pdf"

Attachment Converted: "c:\eudora\attach\santer_fig02.pdf"

Attachment Converted: "c:\eudora\attach\santer_fig03.pdf"

841. 1199972428.txt

#####

From: dian.seidel@noaa.gov
To: santer1@llnl.gov
Subject: Re: Update on response to Douglass et al.
Date: Thu, 10 Jan 2008 08:40:28 -0500
Cc: Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack,

mail.2008

James J." <jhack@ornl.gov>

Dear Ben,

Thank you for this detailed update of your work. A few thoughts for your consideration ...

Where to submit this: Although I understand your and Phil's reluctance to try IJC, it seems to me that, despite the new work presented, this is really a comment on Douglass et al. and so rightly belongs in IJC. If you suspect the review and publication process there is unacceptably long, perhaps this should be confirmed by inquiring with the editor, as a professional courtesy. Decide in advance what you'd consider a reasonable turn-around time, and if the editor says it will take longer, going with another journal makes sense.

Figures: They look great. As usual, you've done a super job telling the story in pictures. One suggestion would be to indicate in Fig. 3 which test, or trio of tests, is the most appropriate. Now it is shown as the blue curves, but I'd suggest making these black (and the black ones blue) and thicker than the rest. That way those readers who just skim the paper and look at the figures will get the message quickly.

Observations: Have you considered including results from HadAT and RATPAC as well as RAOBCOR? For even greater completeness, a version of RATPAC pared down based on the results of Randel and Wu could be added, as could Steve Sherwood's adjusted radiosonde data. I'd suggest adding results from these datasets to your Fig. 1, not the planned Fig 4, which I gather is meant to show the differences in versions of RAOBCOR and the impact of Douglass et al.'s choice to use and early version.

With best wishes,
Dian

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

From: Ben Santer <santer1@ornl.gov>
Date: Wednesday, January 9, 2008 10:52 pm
Subject: Update on response to Douglass et al.

> Dear folks,
>
> I just wanted to update you on my progress in formulating a
> response to
> the Douglass et al. paper in the International Journal of
> Climatology
> (IJC). There have been several developments.
>
> First, I contacted Science to gauge their level of interest in
> publishing a response to Douglass et al. I thought it was
> worthwhile to
> "test the water" before devoting a lot of time to the preparation
> of a
> manuscript for submission to Science. I spoke with Jesse Smith,
> who
> handles most of the climate-related papers at Science magazine.
>
> The bottom line is that, while Science is interested in this issue
> (particularly since Douglass et al. are casting doubt on the
> findings of
> the 2005 Santer et al. Science paper), Jesse Smith thought it was
> highly

mail.2008

> unlikely that Science would carry a rebuttal of work published in
> a
> different journal (IJC). Regretfully, I agree. Our response to
> Douglass
> et al. does not contain any fundamentally new science - although
> it does
> contain some new and interesting work (see below).
>
> It's an unfortunate situation. Singer is promoting the Douglass et
> al.
> paper as a startling "new scientific evidence", which undercuts
> the key
> conclusions of the IPCC and CCSP Reports. Christy is using the
> Douglass
> et al. paper to argue that his UAH group is uniquely positioned to
> perform "hard-nosed" and objective evaluation of model
> performance, and
> that it's dangerous to leave model evaluation in the hands of
> biased
> modelers. Much as I would like to see a high-profile rebuttal of
> Douglass et al. in a journal like Science or Nature, it's unlikely
> that
> either journal will publish such a rebuttal.
>
> So what are our options? Personally, I'd vote for GRL. I think
> that it
> is important to publish an expeditious response to the statistical
> flaws
> in Douglass et al. In theory, GRL should be able to give us the
> desired
> fast turnaround time. Would GRL accept our contribution, given
> that the
> Douglass et al. paper was published in IJC? I think they would -
> we've
> done a substantial amount of new work (see below), and can argue,
> with
> some justification, that our contribution is more than just a
> rebuttal
> of Douglass et al.
>
> Why not go for publication of a response in IJC? According to
> Phil, this
> option would probably take too long. I'd be interested to hear any
> other
> thoughts you might have on publication options.
>
> Now to the science (with a lower-case "s"). I'm appending three
> candidate Figures for a GRL paper. The first Figure was motivated
> by
> discussions I've had with Karl Taylor and Tom Wigley. It's an
> attempt to
> convey the differences between our method of comparing observed
> and
> simulated trends (panel A) and the approach used by Douglass et
> al.
> (panel B).
>
> In our method, we account for both statistical uncertainties in
> fitting
> least-squares linear trends to noisy, temporally-autocorrelated
> data and
> for the effects of internally-generated variability. As I've
> described

mail.2008

> in previous emails, we compare each of the 49 simulated T2 and
> T2LT
> trends (i.e., the same multi-model ensemble used in our 2005
> Science
> paper and in the 2006 CCSP Report) with observed T2 and T2LT
> trends
> obtained from the RSS and UAH groups. Our 2-sigma confidence
> intervals
> on the model and observed trends are estimated as in Santer et al.
> (2000). [Santer, B.D., T.M.L. Wigley, J.S. Boyle, D.J. Gaffen,
> J.J.
> Hnilo, D. Nychka, D.E. Parker, and K.E. Taylor, 2000: Statistical
> significance of trends and trend differences in layer-average
> atmospheric temperature time series, J. Geophys. Res., 105, 7337-
> 7356]
>
> The method that Santer et al. (2000) used to compute "adjusted"
> trend
> confidence intervals accounts for the fact that, after fitting a
> trend
> to T2 or T2LT data, the regression residuals are typically highly
> autocorrelated. If this autocorrelation is not accounted for, one
> could
> easily reach incorrect decisions on whether the trend in an
> individual
> time series is significantly different from zero, or whether two
> time
> series have significantly different trends. Santer et al. (2000)
> accounted for temporal autocorrelation effects by estimating $r\{1\}$,
> the
> lag-1 autocorrelation of the regression residuals, using $r\{1\}$ to
> calculate an effective sample size $n\{e\}$, and then using $n\{e\}$ to
> determine an adjusted standard error of the least-squares linear
> trend.
> Panel A of Figure 1 shows the 2-sigma "adjusted" standard errors
> for
> each individual trend. Models with excessively large tropical
> variability (like FGOALS-g1.0 and GFDL-CM2.1) have large adjusted
> standard errors. Models with coarse-resolution OGCMs and low-
> amplitude
> ENSO variability (like the GISS-AOM) have smaller than observed
> adjusted
> standard errors. Neglect of volcanic forcing (i.e., absence of El
> Chichon and Pinatubo-induced temperature variability) can also
> contribute to smaller than observed standard errors, as in
> CCCma-CGCM3.1(T47).
>
> The dark and light grey bars in Panel A show (respectively) the 1-
> and
> 2-sigma standard errors for the RSS T2LT trend. As is visually
> obvious,
> 36 of the 49 model trends are within 1 standard error of the RSS
> trend,
> and 47 of the 49 model trends are within 2 standard errors of the
> RSS
> trend.
>
> I've already explained our "paired trend test" procedure for
> calculating
> the statistical significance of the model-versus-observed trend
> differences. This involves the normalized trend difference $d1$:
>
> $d1 = (b\{0\} - b\{M\}) / \text{sqrt}[(s\{b0\})^{**2} + (s\{bM\})^{**2}]$

>
 > where $b\{O\}$ and $b\{M\}$ represent any single pair of Observed and
 > Modeled
 > trends, with adjusted standard errors $s\{bO\}$ and $s\{bM\}$.
 >
 > Under the assumption that $d1$ is normally distributed, values of $d1$
 > >
 > +1.96 or < -1.96 indicate observed-minus-model trend differences
 > that
 > are significant at some stipulated significance level, and one can
 > easily calculate a p-value for each value of $d1$. These p-values
 > for the
 > 98 pairs of trend tests (49 involving UAH data and 49 involving
 > RSS
 > data) are what we use for determining the total number of "hits",
 > or
 > rejections of the null hypothesis of no significant difference
 > between
 > modeled and observed trends. I note that each test is two-tailed,
 > since
 > we have no information a priori about the "direction" of the model
 > trend
 > (i.e., whether we expect the simulated trend to be significantly
 > larger
 > or smaller than observed).

>
 > REJECTION RATES FOR "PAIRED TREND TESTS, OBS-VS-MODEL

Stipulated sign. level	No. of tests	T2	"Hits"	T2LT
"Hits" 5%	49 x 2	(98)	2	(2.04%)
1 (1.02%)				
10%	49 x 2	(98)	4	(4.08%)
(2.04%)15%	49 x 2	(98)	7	(7.14%)
5 (5.10%)				

>
 > Now consider Panel B of Figure 1. It helps to clarify the
 > differences
 > between the Douglass et al. comparison of model and observed
 > trends and
 > our own comparison. The black horizontal line ("Multi-model mean
 > trend")
 > is the T2LT trend in the 19-model ensemble, calculated from model
 > ensemble mean trends (the colored symbols). Douglass et al.'s
 > "consistency criterion", $\sigma\{SE\}$, is given by:

>
 >
$$\sigma\{SE\} = \sigma / \sqrt{N - 1}$$

>
 > where σ is the standard deviation of the 19 ensemble-mean
 > trends,
 > and N is 19. The orange and yellow envelopes denote the 1- and
 > 2- $\sigma\{SE\}$ regions.

>
 > Douglass et al. use $\sigma\{SE\}$ to decide whether the multi-model
 > mean
 > trend is consistent with either of the observed trends. They
 > conclude
 > that the RSS and UAH trends lie outside of the yellow envelope
 > (the
 > 2- $\sigma\{SE\}$ region), and interpret this as evidence of a
 > fundamental
 > inconsistency between modeled and observed trends. As noted
 > previously,
 > Douglass et al. obtain this result because they fail to account
 > for

mail.2008

> statistical uncertainty in the estimation of the RSS and UAH
> trends.
> They ignore the statistical error bars on the RSS and UAH trends
> (which
> are shown in Panel A). As is clear from Panel A, the statistical
> error
> bars on the RSS and UAH trends overlap with the Douglass et al.
> 2-sigma{SE} region. Had Douglass et al. accounted for statistical
> uncertainty in estimation of the observed trends, they would have
> been
> unable to conclude that all "UAH and RSS satellite trends are
> inconsistent with model trends".
>
> The second Figure plots values of our test statistic (d1) for the
> "paired trend test". The grey histogram is based on the values of
> d1 for
> the 49 tests involving the RSS T2LT trend and the simulated T2LT
> trends
> from 20c3m runs. The green histogram is for the 49 paired trend
> tests
> involving model 20c3m data and the UAH T2LT trend. Note that the
> d1
> distribution obtained with the UAH data is negatively skewed. This
> is
> because the numerator of the d1 test statistic is $b_{\{0\}} - b_{\{M\}}$, and
> the
> UAH tropical T2LT trend over 1979-1999 is smaller than most of the
> model
> trends (see Figure 1, panel A).
>
> The colored dots are values of the d1 test statistic for what I
> referred
> to previously as "TYPE2" tests. These tests are limited to the M
> models
> with multiple realizations of the 20c3m experiment. Here, $M = 11$.
> For
> each of these M models, I performed paired trend tests for all C
> unique
> combinations of trends pairs. For example, for a model with 5
> realizations of the 20c3m experiment, like GISS-EH, $C = 10$. The
> significance of trend differences is solely a function of "within-
> model"
> effects (i.e., is related to the different manifestations of
> natural
> internal variability superimposed on the underlying forced
> response).
> There are a total of 62 paired trend tests. Note that the
> separation of
> the colored symbols on the y-axis is for visual display purposes
> only,
> and facilitates the identification of results for individual models.
>
> The clear message from Figure 2 is that the values of d1 arising
> from
> internal variability alone are typically as large as the d1 values
> obtained by testing model trends against observational data. The
> two
> negative "outlier" values of d1 for the model-versus-observed
> trend
> tests involve the large positive trend in CCCma-CGCM3.1(T47). If
> you
> have keen eagle eyes, you'll note that the distribution of colored
> symbols is slightly skewed to the negative side. If you look at

mail.2008

> Panel A
> of Figure 1, you'll see that this skewness arises from the
> relatively
> small ensemble sizes. Consider results for the 5-member ensemble
> of
> 20c3m trends from the MRI-CGCM2.3.2. The trend in realization 1 is
> close
> to zero; trends in realizations 2, 3, 4, and 5 are large,
> positive, and
> vary between 0.27 to 0.37 degrees C/decade. So d1 is markedly
> negative
> for tests involving realization 1 versus realizations 2, 3, 4, and
> 5. If
> we showed non-unique combinations of trend pairs (e.g.,
> realization 2
> versus realization 1, as well as 1 versus 2), the distribution of
> colored symbols would be symmetric. But I was concerned that we
> might be
> accused of "double counting" if we did this....
>
> The third Figure is the most interesting one. You have not seen
> this
> yet. I decided to examine how the Douglass et al. "consistency
> test"
> behaves with synthetic data. I did this as a function of sample
> size N,
> for N values ranging from 19 (the number of models we used in the
> CCSP
> report) to 100. Consider the N = 19 case first. I generated 19
> synthetic
> time series using an AR-1 model of the form:
>
>
$$xt(i) = a1 * (xt(i-1) - am) + zt(i) + am$$

>
> where a1 is the coefficient of the AR-1 model, zt(i) is a
> randomly-generated noise term, and am is a mean (set to zero
> here).
> Here, I set a1 to 0.86, close to the lag-1 autocorrelation of the
> UAH
> T2LT anomaly data. The other free parameter is a scaling term
> which
> controls the amplitude of zt(i). I chose this scaling term to
> yield a
> temporal standard deviation of xt(i) that was close to the
> temporal
> standard deviation of the monthly-mean UAH T2LT anomaly data. The
> synthetic time series had the same length as the observational and
> model
> data (252 months), and monthly-mean anomalies were calculated in
> the
> same way as we did for observations and models.
>
> For each of these 19 synthetic time series, I first calculated
> least-squares linear trends and adjusted standard errors, and then
> performed the "paired trends". The test involves all 171 unique
> pairs of
> trends: b{1} versus b{2}, b{1} versus b{3},... b{1} versus b{19},
> b{2}
> versus b{3}, etc. I then calculate the rejection rates of the null
> hypothesis of "no significant difference in trend", for stipulated
> significance levels of 5%, 10%, and 20%. This procedure is
> repeated 1000
> times, with 1000 different realizations of 19 synthetic time

mail.2008

> series. We
> can therefore build up a distribution of rejection rates for $N =$
> 19, and
> then do the same for $N = 20$, etc.
>
> The "paired trend" results are plotted as the blue lines in Figure
> 3.
> Encouragingly, the percentage rejections of the null hypothesis
> are
> close to the theoretical expectations. The 5% significance tests
> yield a
> rejection rate of a little over 6%; 10% tests have a rejection
> rate of
> over 11%, and 20% tests have a rejection rate of 21%. I'm not
> quite sure
> why this slight positive bias arises. This bias does show some
> small
> sensitivity (1-2%) to choice of the a_1 parameter and the scaling
> term.
> Different choices of these parameters can give rejection rates
> that are
> closer to the theoretical expectation. But my parameter choices
> for the
> AR-1 model were guided by the goal of generating synthetic data
> with
> roughly the same autocorrelation and variance properties as the
> UAH
> data, and not by a desire to get as close as I possibly could to
> the
> theoretical rejection rates.
>
> So why is there a small positive bias in the empirically-
> determined
> rejection rates? Perhaps Francis can provide us with some guidance
> here.
> Karl believes that the answer may be partly linked to the skewness
> of
> the empirically-determined rejection rate distributions. For
> example,
> for the $N = 19$ case, and for 5% tests, values of rejection rates
> in the
> 1000-member distribution range from a minimum of 0 to a maximum of
> 24%,
> with a mean value of 6.7% and a median of 6.4%. Clearly, the
> minimum
> value is bounded by zero, but the maximum is not bounded, and in
> rare
> cases, rejection rates can be quite large, and influences the
> mean. This
> inherent skewness must make some contribution to the small
> positive bias
> in rejection rates in the "paired trends" test.
>
> what happens if we naively perform the paired trends test WITHOUT
> adjusting the standard errors of the trends for temporal
> autocorrelation
> effects? Results are shown by the black lines in Figure 3. If we
> ignore
> temporal autocorrelation, we get the wrong answer. Rejection rates
> for
> 5% tests are 60%!
>
> we did not publish results from any of these synthetic data

mail.2008

> experiments
> in our 2000 JGR paper. In retrospect, this is a bit of a shame,
> since
> Figure 3 nicely shows that the adjustment for temporal
> autocorrelation
> effects works reasonably well, while failure to adjust yields
> completely
> erroneous results.
>
> Now consider the red lines in Figure 3. These are the results of
> applying the Douglass et al. "consistency test" to synthetic data.
> Again, let's consider the N = 19 case first. I calculate the
> trends in
> all 19 synthetic time series. Let's consider the first of these 19
> time
> series as the surrogate observations. The trend in this time
> series,
> $b\{1\}$, is compared with the mean trend, $b\{\text{Synth}\}$, computed from the
> remaining 18 synthetic time series. The Douglass $\sigma\{SE\}$ is also
> computed from these 18 remaining trends. We then form a test
> statistic
> $d2 = (b\{1\} - b\{\text{Synth}\}) / \sigma\{SE\}$, and calculate rejection rates
> for
> the null hypothesis of no significant difference between the mean
> trend
> and the trend in the surrogate observations. This procedure is
> then
> repeated with the trend in time series 2 as the surrogate
> observations,
> and $b\{\text{Synth}\}$ and $\sigma\{SE\}$ calculated from time series 1, 3,
> 4,...19.
> This yields 19 different tests of the null hypothesis. Repeat
> 1,000
> times, and build up a distribution of rejection rates, as in the
> "paired
> trends" test.
>
> The results are truly alarming. Application of the Douglass et al.
> "consistency test" to synthetic data - data generated with the
> same
> underlying AR-1 model! - leads to rejection of the above-stated
> null
> hypothesis at least 65% of the time (for N = 19, 5% significance
> tests).
> As expected, rejection rates for the Douglass consistency test
> rise as
> N increases. For N = 100, rejection rates for 5% tests are nearly
> 85%.
> As my colleague Jim Boyle succinctly put it when he looked at
> these
> results, "This is a pretty hard test to pass".
>
> I think this nicely illustrates the problems with the statistical
> approach used by Douglass et al. If you want to demonstrate that
> modeled
> and observed temperature trends are fundamentally inconsistent,
> you
> devise a fundamentally flawed test is very difficult to pass.
>
> I hope to have a first draft of this stuff written up by the end
> of next
> week. If Leo is agreeable, Figure 4 of this GRL paper would show
> the

mail.2008

> vertical profiles of tropical temperature trends in the various
> versions
> of the RAOCORE data, plus model results.

>
> Sorry to bore you with all the gory details. But as we've seen
> from
> Douglass et al., details matter.

>
> with best regards,
>
> Ben

> -----
> -----

> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-2486
> FAX: (925) 422-7675
> email: santer1@llnl.gov

> -----
> -----
>
>

842. 1199984805.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: Re: John Christy's latest ideas]
Date: Thu, 10 Jan 2008 12:06:45 -0800
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Phil,

If you get a chance, could you call me up at work (+1 925 423-3364) to talk about the "IJC publication" option? I'd really like to discuss that with you.

with best regards,

Ben
Phil Jones wrote:

>
> Ben,
> Almost said something about this in the main email about the diagrams!
> Other emails and a couple of phone calls distracting me - have to make
> sure
> I'm sending the right email to the right list/person!
> He's clearly biased, but he gets an audience unfortunately. There are
> enough people out there who think we're wrong to cause me to worry at
> times.
> I'd like the world to warm up quicker, but if it did, I know that
> the sensitivity
> is much higher and humanity would be in a real mess!

>
> I'm getting people misinterpreting my comment that went along with
> Chris Folland's press release about the 2008 forecast. It says we're

mail.2008

> warming at 0.2 degC/decade and that is exactly what we should be.
> The individual years don't matter.
>
> CA are now to send out FOIA requests for the Review Editor comments
> on the AR4 Chapters. For some reason they think they exist!
>
> Cheers
> Phil
>
>
> At 16:52 09/01/2008, you wrote:
>> Dear Phil,
>>
>> I can't believe John is now arguing that he's the only guy who can
>> provide unbiased assessments of model performance. After all the
>> mistakes he's made with MSU, and after the Douglass et al. fiasco, he
>> should have acquired a little humility. But I guess "humility" isn't
>> in his dictionary...
>>
>> With best regards,
>>
>> Ben
>> Phil Jones wrote:
>>> Ben,
>>> I'll give up on trying to catch him on the road to Damascus -
>>> he's beyond redemption.
>>> Glad to see that someone's rejected something he's written.
>>> Jim Hack's good, so I'm confident he won't be fooled.
>>> Cheers
>>> Phil
>>>
>>> At 17:28 07/01/2008, you wrote:
>>>> Dear Phil,
>>>>
>>>> More Christy stuff... The guy is just incredible...
>>>>
>>>> With best regards,
>>>>
>>>> Ben
>>>> -----
>>>> Benjamin D. Santer
>>>> Program for Climate Model Diagnosis and Intercomparison
>>>> Lawrence Livermore National Laboratory
>>>> P.O. Box 808, Mail Stop L-103
>>>> Livermore, CA 94550, U.S.A.
>>>> Tel: (925) 422-2486
>>>> FAX: (925) 422-7675
>>>> email: santer1@llnl.gov
>>>> -----
>>>>
>>>>
>>>> X-Account-Key: account1
>>>> Return-Path: <santer1@llnl.gov>
>>>> Received: from mail-2.llnl.gov ([unix socket])
>>>> by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
>>>> Mon, 07 Jan 2008 09:00:41 -0800
>>>> Received: from nspiron-2.llnl.gov (nspiron-2.llnl.gov [128.115.41.82])
>>>> by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with
>>>> ESMTMP id m07H0edp031523;
>>>> Mon, 7 Jan 2008 09:00:40 -0800
>>>> X-Attachments: None

mail.2008

>>>> X-IronPort-AV: E=McAfee;i="5100,188,5200"; a="5944377"
>>>> X-IronPort-AV: E=Sophos;i="4.24,254,1196668800";
>>>> d="scan'208";a="5944377"
>>>> Received: from dione.llnl.gov (HELO [128.115.57.29]) ([128.115.57.29])
>>>> by nspiron-2.llnl.gov with ESMTTP; 07 Jan 2008 09:00:40 -0800
>>>> Message-ID: <47825AB8.5000608@llnl.gov>
>>>> Date: Mon, 07 Jan 2008 09:00:40 -0800
>>>> From: Ben Santer <santer1@llnl.gov>
>>>> Reply-To: santer1@llnl.gov
>>>> Organization: LLNL
>>>> User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
>>>> MIME-Version: 1.0
>>>> To: "Hack, James J." <jhack@ornl.gov>
>>>> Subject: Re: John Christy's latest ideas
>>>> References:
>>>> <537C6C0940C6C143AA46A88946B854170B9FAF74@ORNLEXCHANGE.ornl.gov>
>>>> In-Reply-To:
>>>> <537C6C0940C6C143AA46A88946B854170B9FAF74@ORNLEXCHANGE.ornl.gov>
>>>> Content-Type: text/plain; charset=ISO-8859-1; format=flowed
>>>> Content-Transfer-Encoding: 7bit

>>>>

>>>> Dear Jim,

>>>>

>>>> I'm well aware of this paper, and am currently preparing a reply
>>>> (together with many others who were involved in the first CCSP
>>>> report). To put it bluntly, the Douglass paper is a piece of
>>>> worthless garbage. It has serious statistical flaws. Christy should
>>>> be ashamed that he's a co-author on this. His letter to Dr. Strayer
>>>> is deplorable and offensive. For over a decade, Christy has
>>>> portrayed himself as the only guy who is smart enough to develop
>>>> climate-quality data records from MSU. Recently, he's also portrayed
>>>> himself as the only guy who's smart enough to develop
>>>> climate-quality data records from radiosonde data. And now he's the
>>>> only scientist who is capable of performing "hard-nosed",
>>>> independent assessments of climate model performance.

>>>>

>>>> John Christy has made a scientific career out of being wrong. He's
>>>> not even a third-rate scientist. I'd be happy to discuss Christy's
>>>> "unique ways of validating climate models" with you.

>>>>

>>>> with best regards,

>>>>

>>>> Ben

>>>> Hack, James J. wrote:

>>>>> Dear Ben,

>>>>>

>>>>> Happy New Year. Hope all is well. I was wondering if you're
>>>>> familiar with the attached paper? I thought that you had recently
>>>>> published something that concludes something quite different. Is
>>>>> that right? If yes, could you forward me a copy? And, any
>>>>> comments are also welcome.

>>>>> He's coming to ORNL next week to under the premise that he has some
>>>>> unique ways to validate climate models (this time with regard to
>>>>> the lower thermodynamic structure). I'd be happy to chat with you
>>>>> about this as well if you would like. I'm appending what I know to
>>>>> the bottom of this note.

>>>>>

>>>>> Best regards ...

>>>>>

>>>>> Jim

>>>>>

>>>>> James J. Hack Director, National Center for Computational Sciences
>>>>> Oak Ridge National Laboratory

mail.2008

>>>> One Bethel Valley Road
>>>> P.O. Box 2008, MS-6008
>>>> Oak Ridge, TN 37831-6008

>>>> email: jhack@ornl.gov <mailto:jhack@ornl.gov>
>>>> voice: 865-574-6334
>>>> fax: 865-241-9578
>>>> cell: 865-206-9001

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

>>>>> >> From: John Christy [<mailto:john.christy@nsstc.uah.edu>]

>>>>> >> Sent: Tuesday, October 23, 2007 9:16 AM

>>>>> >> To: Strayer, Michael

>>>>> >> Cc: Salmon, Jeffrey

>>>>> >> Subject: Climate Model Evaluation

>>>>> >>

>>>>> >> Dr. Strayer:

>>>>> >>

>>>>> >> Jeff salmon is aware of a project we at UAHuntsville believe is

>>>>> >> vital and that you may provide a way to see it accomplished.

>>>>> >> As you

>>>>> >> know, our nation's energy and climate change policies are being

>>>>> >> driven by output from global climate models. However, there has

>>>>> >> never been a true "red team" assessment of these model

>>>>> >> projections

>>>>> >> in the way other government programs are subjected to hard-nosed,

>>>>> >> independent evaluations. To date, most of the "evaluation" of

>>>>> >> these

>>>>> >> models has been left in the hands of the climate modelers

>>>>> >> themselves. This has the potential of biasing the entire process.

>>>>> >>

>>>>> >> It is often a climate modeler's claim (and promoted in IPCC

>>>>> >> documents - see attached) that the models must be correct because

>>>>> >> the global surface

>>>>> >> temperature variations since 1850 are reproduced (somewhat) by

>>>>> >> the

>>>>> >> models when run in hindcast mode. However, this is not a

>>>>> >> scientific

>>>>> >> experiment for the simple reason that every climate modeler

>>>>> >> saw the

>>>>> >> answer ahead of time. It is terribly easy to get the right answer

>>>>> >> for the wrong reason, especially if you already know the answer.

>>>>> >>

>>>>> >> A legitimate experiment is to test the models' output against

>>>>> >> variables to which modelers did not have access ... a true blind

>>>>> >> test of the models.

>>>>> >>

>>>>> >> I have proposed and have had rejected a model evaluation

>>>>> >> project to

>>>>> >> DOE based on the utilization of global datasets we build here at

>>>>> >> UAH. We have published many of these datasets (most are

>>>>> >> satellite-based) which document the complexity of the climate

>>>>> >> system and which we think models should replicate in some way,

>>>>> >> and

>>>>> >> to aid in model development where shortcomings are found.

>>>>> >> These are

>>>>> >> datasets of quantities that modelers in general were not aware of

>>>>> >> when doing model testing. We have performed

>>>>> >> a few of these tests and have found models reveal serious

>>>>> >> shortcomings in some of the most fundamental aspects of energy

>>>>> >> distribution. We believe a rigorous test of climate models is in

>>>>> >> order as the congress starts considering energy reduction

>>>>> >> strategies which can have significant consequences on our
>>>>> economy.
>>>>> >> Below is an abstract of a retooled proposal I am working on.
>>>>> >>
>>>>> >> If you see a possible avenue for research along these lines,
>>>>> please
>>>>> >> let me know. Too, we have been considering some type of
>>>>> partnership
>>>>> >> with Oakridge since the facility is nearby, and this may be a way
>>>>> >> to do that.
>>>>> >>
>>>>> >> John C.

>>>>> >> Understanding the vertical energy distribution of the Earth's
>>>>> atmosphere
>>>>> >> and its expression in global climate model simulations
>>>>> >>
>>>>> >> John R. Christy, P.I., University of Alabama in Huntsville
>>>>> >>
>>>>> >> Abstract
>>>>> >>
>>>>> >> Sets of independent observations indicate, unexpectedly, that the
>>>>> >> warming of the tropical atmosphere since 1978 is proceeding at a
>>>>> >> rate much less than that anticipated from climate model
>>>>> simulations.
>>>>> >> Specifically, while the surface has warmed, the lower troposphere
>>>>> >> has experienced less warming. In contrast, all climate models we
>>>>> >> and others have examined indicate the lower tropical atmosphere
>>>>> >> should be warming at a rate 1.2 to 1.5 times greater than the
>>>>> >> surface when forced with increasing greenhouse gases within the
>>>>> >> context of other observed forcings (the so-called "negative lapse
>>>>> >> rate feedback".) We propose to diagnose this curious phenomenon
>>>>> >> with several satellite-based datasets to document its relation to
>>>>> >> other climate variables. We shall do the same for climate model
>>>>> >> output of the same simulated variables. This will
>>>>> >> enable us to propose an integrated conceptual framework of the
>>>>> >> phenomenon for further testing. Tied in with this research are
>>>>> potential
>>>>> >> answers to fundamental questions such as the following: (1) In
>>>>> >> response to increasing surface temperatures, is the lower
>>>>> >> atmosphere reconfiguring the way heat energy is transported which
>>>>> >> allows for an increasing amount of heat to more freely escape to
>>>>> >> space? (2) Could there be a natural thermostatic effect in the
>>>>> >> climate system which acts in a different way than parameterized
>>>>> >> convective-adjustment schemes dependent upon current
>>>>> assumptions of
>>>>> >> heat deposition and retention? (3)
>>>>> >> If observed atmospheric heat retention is considerably less than
>>>>> >> model projections, what impact will lower retention rates have on
>>>>> >> anticipated increases in surface temperatures in the 21st
>>>>> century?
>>>>> >>

>>>> >>
>>>> >>
>>>> --
>>>> -----
>>>> Benjamin D. Santer
>>>> Program for Climate Model Diagnosis and Intercomparison
>>>> Lawrence Livermore National Laboratory
>>>> P.O. Box 808, Mail Stop L-103

mail.2008

>>>> Livermore, CA 94550, U.S.A.
>>>> Tel: (925) 422-2486
>>>> FAX: (925) 422-7675
>>>> email: santer1@llnl.gov

>>>> -----

>>>> 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

>>>> -----

>>>>
>>>>
>>>> --

>>>> -----

>>> Benjamin D. Santer
>>> Program for Climate Model Diagnosis and Intercomparison
>>> Lawrence Livermore National Laboratory
>>> P.O. Box 808, Mail Stop L-103
>>> Livermore, CA 94550, U.S.A.
>>> Tel: (925) 422-2486
>>> FAX: (925) 422-7675
>>> email: santer1@llnl.gov

>>>> -----

>>> 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

>>>> -----

>>>>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

843. 1199988028.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>

mail.2008

Subject: Re: Update on response to Douglass et al.
Date: Thu, 10 Jan 2008 13:00:28 -0800
Reply-to: santer1@llnl.gov
Cc: "'Philip D. Jones'" <p.jones@uea.ac.uk>

<x-flowed>
Dear Tim,

Thanks very much for your email. I greatly appreciate the additional information that you've given me. I am a bit conflicted about what we should do.

IJC published a paper with egregious statistical errors. Douglass et al. was essentially a commentary on work by myself and colleagues - work that had been previously published in Science in 2005 and in Chapter 5 of the first U.S. CCSP Report in 2006. To my knowledge, none of the authors or co-authors of the Santer et al. Science paper or of CCSP 1.1 Chapter 5 were used as reviewers of Douglass et al. I am assuming that, when he submitted his paper to IJC, Douglass specifically requested that certain scientists should be excluded from the review process. Such an approach is not defensible for a paper which is largely a comment on previously-published work.

It would be fair and reasonable to give IJC the opportunity to "set the record straight", and correct the harm they have done by publication of Douglass et al. I use the word "harm" advisedly. The author and coauthors of the Douglass et al. IJC paper are using this paper to argue that "Nature, not CO₂, rules the climate", and that the findings of Douglass et al. invalidate the "discernible human influence" conclusions of previous national and international scientific assessments.

Quick publication of a response to Douglass et al. in IJC would go some way towards setting the record straight. I am troubled, however, by the very real possibility that Douglass et al. will have the last word on this subject. In my opinion (based on many years of interaction with these guys), neither Douglass, Christy or Singer are capable of admitting that their paper contained serious scientific errors. Their "last word" will be an attempt to obfuscate rather than illuminate. They are not interested in improving our scientific understanding of the nature and causes of recent changes in atmospheric temperature. They are solely interested in advancing their own agendas. It is telling and troubling that Douglass et al. ignored radiosonde data showing substantial warming of the tropical troposphere - data that were in accord with model results - even though such data were in their possession. Such behaviour constitutes intellectual dishonesty. I strongly believe that leaving these guys the last word is inherently unfair.

If IJC are interested in publishing our contribution, I believe it's fair to ask for the following:

1) Our paper should be regarded as an independent contribution, not as a comment on Douglass et al. This seems reasonable given i) The substantial amount of new work that we have done; and ii) The fact that the Douglass et al. paper was not regarded as a comment on Santer et al. (2005), or on Chapter 5 of the 2006 CCSP Report - even though Douglass et al. clearly WAS a comment on these two publications.

2) If IJC agrees to 1), then Douglass et al. should have the opportunity to respond to our contribution, and we should be given the chance to reply. Any response and reply should be published side-by-side, in the same issue of IJC.

I'd be grateful if you and Phil could provide me with some guidance on

mail.2008

1) and 2), and on whether you think we should submit to IJC. Feel free to forward my email to Glenn McGregor.

with best regards,

Ben

Tim Osborn wrote:

> At 03:52 10/01/2008, Ben Santer wrote:

>> ...Much as I would like to see a high-profile rebuttal of Douglass et al. in a journal like Science or Nature, it's unlikely that either journal will publish such a rebuttal.

>>

>> So what are our options? Personally, I'd vote for GRL. I think that it is important to publish an expeditious response to the statistical flaws in Douglass et al. In theory, GRL should be able to give us the desired fast turnaround time...

>>

>> Why not go for publication of a response in IJC? According to Phil, this option would probably take too long. I'd be interested to hear any other thoughts you might have on publication options.

>

> Hi Ben and Phil,

>

> as you may know (Phil certainly knows), I'm on the editorial board of IJC. Phil is right that it can be rather slow (though faster than certain other climate journals!). Nevertheless, IJC really is the preferred place to publish (though a downside is that Douglass et al. may have the opportunity to have a response considered to accompany any comment).

>

> I just contacted the editor, Glenn McGregor, to see what he can do. He promises to do everything he can to achieve a quick turn-around time (he didn't quantify this) and he will also "ask (the publishers) for priority in terms of getting the paper online asap after the authors have received proofs". He genuinely seems keen to correct the scientific record as quickly as possible.

>

> He also said (and please treat this in confidence, which is why I emailed to you and Phil only) that he may be able to hold back the hardcopy (i.e. the print/paper version) appearance of Douglass et al., possibly so that any accepted Santer et al. comment could appear alongside it. Presumably depends on speed of the review process.

>

> If this does persuade you to go with IJC, Glenn suggested that I could help (because he is in Kathmandu at present) with achieving the quick turn-around time by identifying in advance reviewers who are both suitable and available. Obviously one reviewer could be someone who is already familiar with this discussion, because that would enable a fast review - i.e., someone on the email list you've been using - though I don't know which of these people you will be asking to be co-authors and hence which won't be available as possible reviewers. For objectivity the other reviewer would need to be independent, but you could still suggest suitable names.

>

> Well, that's my thoughts... let me know what you decide.

>

> Cheers

>

> Tim

>

>

> Dr Timothy J Osborn, Academic Fellow
> Climatic Research Unit

mail.2008

> School of Environmental Sciences
> University of East Anglia
> Norwich NR4 7TJ, UK
>
> e-mail: t.osborn@uea.ac.uk
> phone: +44 1603 592089
> fax: +44 1603 507784
> web: http://www.cru.uea.ac.uk/~timo/
> sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm
>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

844. 1199994210.txt

#####

From: Peter Thorne <peter.thorne@metoffice.gov.uk>
To: Dian Seidel <dian.seidel@noaa.gov>
Subject: Dian, something like this?
Date: Thu, 10 Jan 2008 14:43:30 +0000
Cc: Ben Santer <santer1@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "Francis W. Zwiers" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

All,

as it happens I am preparing a figure precisely as Dian suggested. This has only been possible due to substantial efforts by Leo in particular, but all the other dataset providers also. I wanted to give a feel for where we are at although I want to tidy this substantially if we were to use it. To do this I've taken every single scrap of info I have in my possession that has a status of at least submitted to a journal. I have considered the common period of 1979-2004. So, assuming you are all sitting comfortably:

Grey shading is a little cheat from Santer et al using a trusty ruler. See Figure 3.B in this paper, take the absolute range of model scaling factors at each of the heights on the y-axis and apply this scaling to HadCRUT3 tropical mean trend denoted by the star at the surface. So, if we assume HadCRUT3 is correct then we are aiming for the grey shading or not depending upon one's pre-conceived notion as to whether the models are correct.

mail.2008

Red is HadAT2 dataset.

black dashed is the raw data used in Titchner et al. submitted (all tropical stations with a 81-2000 climatology)

Black whiskers are median, inter-quartile range and max / min from Titchner et al. submission. We know, from complex error-world assessments, that the median under-cooks the required adjustment here and that the truth may conceivably lie (well) outside the upper limit.

Bright green is RATPAC

Then, and the averaging and trend calculation has been done by Leo here and not me so any final version I'd want to get the raw gridded data and do it exactly the same way. But for the raw raobs data that Leo provided as a sanity check it seems to make a miniscule (<0.05K/decade even at height) difference:

Lime green: RICH (RAOBCORE 1.4 breaks, neighbour based adjustment estimates)

Solid purple: RAOBCORE 1.2
Dotted purple: RAOBCORE 1.3
Dashed purple: RAOBCORE 1.4

I am also in possession of Steve's submitted IUK dataset and will be adding this trend line shortly.

I'll be adding a legend in the large white space bottom left.

My take home is that all datasets are heading the right way and that this reduces the probability of a discrepancy. Compare this with Santer et al. Figure 3.B.

I'll be using this in an internal report anyway but am quite happy for it to be used in this context too if that is the general feeling. Or for Leo's to be used. whatever people prefer.

Peter

--
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

Attachment Converted: "c:\eudora\attach\trend_profiles_dogs_dinner.png"

845. 1199999668.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@lnl.gov
Subject: An issue/problem with Tim's idea !!!!!!!
Date: Thu Jan 10 16:14:28 2008

Ben,
Tim's idea is a possibility. I've not always got on that well great with Glenn McGregor, but Tim seems to have a reasonable rapport with him. Dian has suggested that this would be the best route - it

mail.2008

is the logical one. I also think that Glenn would get quick reviews, as Tim thinks he realises he's made a mistake.

Tim has let me into part of secret. Glenn said the paper had two reviews - one positive, the other said it wasn't great, but would leave it up to the editor's discretion. This is why Glenn knows he made the wrong choice.

The problem !! The person who said they would leave it to the editor's discretion is on your email list! I don't know who it is - Tim does - maybe they have told you? I don't want to put pressure on Tim. He doesn't know I'm sending this. It isn't me by the way - nor Tim ! Tim said it was someone who hasn't contributed to the discussion - which does narrow the possibilities down!

Tim/Glenn discussed getting quick reviews. whoever this person is they could be the familiar reviewer - and we could then come up with another reasonable name (Kevin - he does everything at the speed of light) as the two reviewers.

Colour in IJC costs a bit, but I'm sure we can lean on Glenn.

Also we can just have colour in the pdf.

I'll now send a few thoughts on the figures!

Cheers

Phil

Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@a1.noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

X-Mailer: QUALCOMM Windows Eudora Version 7.1.0.9

Date: Thu, 10 Jan 2008 13:00:39 +0000

To: santer1@llnl.gov, "'Philip D. Jones'" <p.jones@uea.ac.uk>

From: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: Update on response to Douglass et al.

At 03:52 10/01/2008, Ben Santer wrote:

...Much as I would like to see a high-profile rebuttal of Douglass et al. in a journal like Science or Nature, it's unlikely that either journal will publish such a rebuttal.

So what are our options? Personally, I'd vote for GRL. I think that it is important to

publish an expeditious response to the statistical flaws in Douglass et al. In theory,

GRL should be able to give us the desired fast turnaround time...

Phil would why not go for publication of a response in IJC? According to Phil, this option

have on probably take too long. I'd be interested to hear any other thoughts you might publication options.

Hi Ben and Phil,

Phil is as you may know (Phil certainly knows), I'm on the editorial board of IJC.

right that it can be rather slow (though faster than certain other climate

mail.2008

journals!).

Nevertheless, IJC really is the preferred place to publish (though a downside is that

Douglass et al. may have the opportunity to have a response considered to accompany any comment).

I just contacted the editor, Glenn McGregor, to see what he can do. He promises to do everything he can to achieve a quick turn-around time (he didn't quantify this) and he

will also "ask (the publishers) for priority in terms of getting the paper online asap

after the authors have received proofs". He genuinely seems keen to correct the scientific record as quickly as possible.

He also said (and please treat this in confidence, which is why I emailed to you and

Phil only) that he may be able to hold back the hardcopy (i.e. the print/paper version)

appearance of Douglass et al., possibly so that any accepted Santer et al. comment could

appear alongside it. Presumably depends on speed of the review process.

If this does persuade you to go with IJC, Glenn suggested that I could help (because he

is in Kathmandu at present) with achieving the quick turn-around time by identifying in

advance reviewers who are both suitable and available. Obviously one reviewer could be

someone who is already familiar with this discussion, because that would enable a fast

review - i.e., someone on the email list you've been using - though I don't know which

of these people you will be asking to be co-authors and hence which won't be available

as possible reviewers. For objectivity the other reviewer would need to be independent,

but you could still suggest suitable names.

Well, that's my thoughts... let me know what you decide.

Cheers

Tim

Dr Timothy J Osborn, Academic Fellow

Climatic Research Unit

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089

fax: +44 1603 507784

web: [1]<http://www.cru.uea.ac.uk/~timo/>

suncllock: [2]<http://www.cru.uea.ac.uk/~timo/suncllock.htm>

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

References

1. <http://www.cru.uea.ac.uk/~timo/>

2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

846. 1200003656.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Peter Thorne <peter.thorne@metoffice.gov.uk>, Dian Seidel <dian.seidel@noaa.gov>
Subject: Re: Dian, something like this?
Date: Thu Jan 10 17:20:56 2008
Cc: Ben Santer <santer1@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "Francis W. Zwiers" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

Ben et al,

As Dian has said Ben's diagrams are as usual great! I also like the one that Peter has just sent around as that illustrates the issue with the various RAOBCORE versions. Although I still think they should have used HadCRUT3v for the surface, I know HadCRUT2v shows much the same. What this figure shows is the differences between the various sonde datasets. Dian/Peter also make the point that there are other new datasets to be added - so the sondes are very much still work in progress. I know you will point out all the analytical/statistical issues see the series brings home the issues better. I know you could add the values to your Fig1, a plot like this is much better.

In the email Ben, you seem to have written much of the response! whichever route you go down (GRL/IJC) the text can't be too long. I would favour copious captions, and even an Appendix, to get the main points across quickly.

Cheers
Phil

At 14:43 10/01/2008, Peter Thorne wrote:

All,
as it happens I am preparing a figure precisely as Dian suggested. This has only been possible due to substantial efforts by Leo in particular, but all the other dataset providers also. I wanted to give a feel for where we are at although I want to tidy this substantially if we were to use it. To do this I've taken every single scrap of info I have in my possession that has a status of at least submitted to a journal. I have considered the common period of 1979-2004. So, assuming you are all sitting comfortably:

Grey shading is a little cheat from Santer et al using a trusty ruler. See Figure 3.B in this paper, take the absolute range of model scaling factors at each of the heights on the y-axis and apply this scaling to HadCRUT3 tropical mean trend denoted by the star at the surface. So, if we assume HadCRUT3 is correct then we are aiming for the grey shading or not depending upon one's pre-conceived notion as to whether the models are correct.

Red is HadAT2 dataset.
black dashed is the raw data used in Titchner et al. submitted (all tropical stations with a 81-2000 climatology)

Black whiskers are median, inter-quartile range and max / min from Titchner et al. submission. We know, from complex error-world

mail.2008

assessments, that the median under-cooks the required adjustment here and that the truth may conceivably lie (well) outside the upper limit. Bright green is RATPAC

Then, and the averaging and trend calculation has been done by Leo here and not me so any final version I'd want to get the raw gridded data and do it exactly the same way. But for the raw raobs data that Leo provided as a sanity check it seems to make a miniscule (<0.05K/decade even at height) difference:

Lime green: RICH (RAOBCORE 1.4 breaks, neighbour based adjustment estimates)

Solid purple: RAOBCORE 1.2

Dotted purple: RAOBCORE 1.3

Dashed purple: RAOBCORE 1.4

I am also in possession of Steve's submitted IUK dataset and will be adding this trend line shortly.

I'll be adding a legend in the large white space bottom left.

My take home is that all datasets are heading the right way and that this reduces the probability of a discrepancy. Compare this with Santer et al. Figure 3.B.

I'll be using this in an internal report anyway but am quite happy for it to be used in this context too if that is the general feeling. Or for Leo's to be used. Whatever people prefer.

Peter

--

Peter Thorne Climate Research Scientist
Met Office Hadley Centre, FitzRoy Road, Exeter, EX1 3PB
tel. +44 1392 886552 fax +44 1392 885681
[1]www.metoffice.gov.uk/hadobs

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.metoffice.gov.uk/hadobs>

847. 1200010023.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Leopold Haimberger <leopold.haimberger@univie.ac.at>
Subject: Re: Update on response to Douglass et al., Dian, something like this?
Date: Thu, 10 Jan 2008 19:07:03 -0800
Reply-to: santer1@llnl.gov
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>, Dian Seidel <dian.seidel@noaa.gov>, Tom Wigley <>wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "Francis W. Zwiers" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

<x-flowed>
Dear Leo,

Thanks very much for your email. I can easily make the observations a bit more prominent in Figure 1. As you can see from today's (voluminous!) email traffic, I've received lots of helpful suggestions regarding improvements to the Figures. I'll try to produce revised versions of the Figures tomorrow.

On the autocorrelation issue: The models have a much larger range of lag-1 autocorrelation coefficients (0.66 to 0.95 for T2LT, and 0.69 to 0.95 for T2) than the UAH or RSS data (which range from 0.87 to 0.89). I was concerned that if we used the model lag-1 autocorrelations to guide the choice of AR-1 parameter in the synthetic data analysis, Douglass and colleagues would have an easy opening for criticising us ("Aha! Santer et al. are using model results to guide them in their selection of the coefficients for their AR-1 model!") I felt that it was much more difficult for Douglass et al. to criticize what we've done if we used UAH data to dictate our choice of the AR-1 parameter and the "scaling factor" for the amplitude of the temporal variability.

As you know, my personal preference would be to include in our response to Douglass et al. something like the Figure 4 that Peter has produced. While inclusion of a Figure 4 is not essential for the purpose of illuminating the statistical flaws in the Douglass et al. "consistency test", such a Figure would clearly show the (currently large) structural uncertainties in radiosonde-based estimates of the vertical profile of atmospheric temperature changes. I think this is an important point, particularly in view of the fact that Douglass et al. failed to discuss versions 1.3 and 1.4 of your RAOBCORE data - even though they had information from those datasets in their possession.

However, I fully agree with Tom's comment that we don't want to do anything to "steal the thunder" from ongoing efforts to improve sonde-based estimates of atmospheric temperature change, and to better quantify structural uncertainties in those estimates. Your group, together with the groups at the Hadley Centre, Yale, NOAA ARL and NOAA GFDL, deserve great credit for making significant progress on a difficult, time-consuming, yet important problem.

I guess the best solution is to leave this decision up to all of you (the radiosonde dataset developers). I'm perfectly happy to include a version of Figure 4 in our response to Douglass et al. If we do go with inclusion of a Figure 4, you, Peter, Dian, Melissa, Steve Sherwood and John should decide whether you feel comfortable providing radiosonde data for such a Figure. I will gladly abide by your decisions. As you note in your email, our use of a Figure 4 would not preclude a more detailed and thorough comparison of simulated and observed amplification in some later publication.

Once again, thanks for all your help with this project, Leo.

with best regards,

Ben

Leopold Haimberger wrote:

> All,

>

> These three figures are really very clear and leave no doubts that the
> Douglass et al analysis is flawed. This is true especially for Fig. 1.
> In Fig. 1 one has to look carefully to find the RSS and UAH "observed"
> trends to the right of all the model trends. Maybe one can make their
> symbols more prominent.

mail.2008

>
> Concerning Fig. 3 I wonder whether the UAH autocorrelation is the lowest
> of all available data. .86 is quite substantial autocorrelation. Maybe
> it is a good idea to be on the safe side and use the lowest
> autocorrelation of all datasets (models, RSS, UAH) for this analysis.
>
> Concerning Fig. 4, I like Peter's and Dian's idea to include RAOBCORE,
> HadAT2, RATPAC and Steve's data and compare it in one plot with model
> output. While I agree that the first three figures and the corresponding
> text are already sufficient for the reply, they target mainly to the
> right panel of Fig. 1 in Douglass et al's paper. The trend profile plot
> of Fig. 4 is complementary as a counterpart to the left panel of their
> plot. To see the trend amplification in in some of the vertical profiles
> is much more suggestive than seeing the LT trends being larger than
> surface trends, at least for me. Showing all available profiles adds
> value beyond the RAOBCORE v1.2 vs RAOBCORE v1.4 issue. Yes, it is work
> in progress and such a plot as drafted by Peter makes that very clear.
> In this paper it is sufficient to show that the uncertainty of
> radiosonde trends is much larger than suggested by Douglass et al. and
> we do not need to have the final answer yet. I have nothing against
> Peter doing the drawing of the figure, since he has most of the
> necessary data. The plot would be needed for 1979-1999, however. Peter,
> I will send you the trend profiles for this period a bit later.
>
> Publishing the reply in either IJC or GRL including Fig. 4 is fine for me.
> When we first discussed a follow up of the Santer et al paper in
> October, we had in mind to publish post-FAR climate model data up to
> present (not just 1999) and also new radiosonde data up to present in a
> highest ranking journal. I am confident that this is still possible even
> if some of the new material planned for such a paper is submitted
> already now. What do you think?
>
> With best Regards,
>
> Leo
>
> Peter Thorne wrote:
>> All,
>>
>> as it happens I am preparing a figure precisely as Dian suggested. This
>> has only been possible due to substantial efforts by Leo in particular,
>> but all the other dataset providers also. I wanted to give a feel for
>> where we are at although I want to tidy this substantially if we were to
>> use it. To do this I've taken every single scrap of info I have in my
>> possession that has a status of at least submitted to a journal. I have
>> considered the common period of 1979-2004. So, assuming you are all
>> sitting comfortably:
>>
>> Grey shading is a little cheat from Santer et al using a trusty ruler.
>> See Figure 3.B in this paper, take the absolute range of model scaling
>> factors at each of the heights on the y-axis and apply this scaling to
>> HadCRUT3 tropical mean trend denoted by the star at the surface. So, if
>> we assume HadCRUT3 is correct then we are aiming for the grey shading or
>> not depending upon one's pre-conceived notion as to whether the models
>> are correct.
>>
>> Red is HadAT2 dataset.
>>
>> black dashed is the raw data used in Titchner et al. submitted (all
>> tropical stations with a 81-2000 climatology)
>>
>> Black whiskers are median, inter-quartile range and max / min from
>> Titchner et al. submission. we know, from complex error-world

mail.2008

>> assessments, that the median under-cooks the required adjustment here
>> and that the truth may conceivably lie (well) outside the upper limit.
>>
>> Bright green is RATPAC
>>
>> Then, and the averaging and trend calculation has been done by Leo here
>> and not me so any final version I'd want to get the raw gridded data and
>> do it exactly the same way. But for the raw raobs data that Leo provided
>> as a sanity check it seems to make a miniscule (<0.05K/decade even at
>> height) difference:
>>
>> Lime green: RICH (RAOBCORE 1.4 breaks, neighbour based adjustment
>> estimates)
>>
>> Solid purple: RAOBCORE 1.2
>> Dotted purple: RAOBCORE 1.3
>> Dashed purple: RAOBCORE 1.4
>>
>> I am also in possession of Steve's submitted IUK dataset and will be
>> adding this trend line shortly.
>>
>> I'll be adding a legend in the large white space bottom left.
>>
>> My take home is that all datasets are heading the right way and that
>> this reduces the probability of a discrepancy. Compare this with Santer
>> et al. Figure 3.B.
>>
>> I'll be using this in an internal report anyway but am quite happy for
>> it to be used in this context too if that is the general feeling. Or for
>> Leo's to be used. whatever people prefer.
>>
>> Peter
>>
>>
>> -----
>>
>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

848. 1200059003.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Potential reviewers
Date: Fri, 11 Jan 2008 08:43:23 -0800
Reply-to: santer1@llnl.gov
Cc: "'Philip D. Jones'" <p.jones@uea.ac.uk>

<x-flowed>
Dear Tim,

Here are some suggestions for potential reviewers of a Santer et al. IJoC submission on issues related to the consistency between modeled and observed atmospheric temperature trends. None of the suggested reviewers have been involved in the recent "focus group" that has discussed problems with the Douglass et al. IJoC paper.

1. Mike Wallace, University of Washington. U.S. National Academy member. Expert on atmospheric dynamics. Chair of National Academy of Sciences committee on "Reconciling observations of global temperature change" (2000). Email: wallace@atmos.washington.edu

2. Qiang Fu, University of Washington. Expert on atmospheric radiation, dynamics, radiosonde and satellite data. Published 2004 Nature paper and 2005 GRL paper dealing with issues related to global and tropical temperature trends. Email: qfu@atmos.washington.edu

3. Gabi Hegerl, University of Edinburgh. Expert on detection and attribution of externally-forced climate change. Co-Convening Lead Author of "Understanding and Attributing Climate Change" chapter of IPCC Fourth Assessment Report. Email: Gabi.Hegerl@ed.ac.uk

4. Jim Hurrell, National Center for Atmospheric Research (NCAR). Former Director of Climate and Global Dynamics division at NCAR. Expert on climate modeling, observational data. Published a number of papers on MSU-related issues. Email: jhurrell@cgd.ucar.edu

5. Myles Allen, Oxford University. Expert in Climate Dynamics, detection and attribution, application of statistical methods in climatology. Email: allen@atm.ox.ac.uk

6. Peter Stott, Hadley Centre for Climate Prediction and Research. Expert in climate modeling, detection and attribution. Email: peter.stott@metoffice.gov.uk

with best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

849. 1200076878.txt

#####

From: Tim Osborn <t.osborn@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: Update on response to Douglass et al.
Date: Fri, 11 Jan 2008 13:41:18 +0000
Cc: "'Philip D. Jones'" <p.jones@uea.ac.uk>

mail.2008

<x-flowed>
Hi Ben (cc Phil),

just heard back from Glenn. He's prepared to treat it as a new submission rather than a comment on Douglass et al. and he also reiterates that "Needless to say my offer of a quick turn around time etc still stands".

So basically this makes the IJC option more attractive than if it were treated as a comment. But whether IJC is still a less attractive option than GRL is up to you to decide :-) (or feel free to canvas your potential co-authors [the only thing I didn't want to make more generally known was the suggestion that print publication of Douglass et al. might be delayed... all other aspects of this discussion are unrestricted]).

Cheers

Tim

At 21:00 10/01/2008, Ben Santer wrote:

>Dear Tim,

>

>Thanks very much for your email. I greatly appreciate the additional information that you've given me. I am a bit conflicted about what we should do.

>

>IJC published a paper with egregious statistical errors. Douglass et al. was essentially a commentary on work by myself and colleagues - work that had been previously published in Science in 2005 and in Chapter 5 of the first U.S. CCSP Report in 2006. To my knowledge, none of the authors or co-authors of the Santer et al. Science paper or of CCSP 1.1 Chapter 5 were used as reviewers of Douglass et al. I am assuming that, when he submitted his paper to IJC, Douglass specifically requested that certain scientists should be excluded from the review process. Such an approach is not defensible for a paper which is largely a comment on previously-published work.

>

>It would be fair and reasonable to give IJC the opportunity to "set the record straight", and correct the harm they have done by publication of Douglass et al. I use the word "harm" advisedly. The author and coauthors of the Douglass et al. IJC paper are using this paper to argue that "Nature, not CO2, rules the climate", and that the findings of Douglass et al. invalidate the "discernible human influence" conclusions of previous national and international scientific assessments.

>

>Quick publication of a response to Douglass et al. in IJC would go some way towards setting the record straight. I am troubled, however, by the very real possibility that Douglass et al. will have the last word on this subject. In my opinion (based on many years of interaction with these guys), neither Douglass, Christy or Singer are capable of admitting that their paper contained serious scientific errors. Their "last word" will be an attempt to obfuscate rather than illuminate. They are not interested in improving our scientific understanding of the nature and causes of recent changes in atmospheric temperature. They are solely interested in advancing their own agendas. It is telling and troubling that Douglass et al. ignored radiosonde data showing substantial warming of the tropical troposphere - data that were in accord with model results - even though such data were in their possession. Such behaviour constitutes intellectual dishonesty. I strongly believe that leaving

mail.2008

>these guys the last word is inherently unfair.

>

>If IJC are interested in publishing our contribution, I believe it's
>fair to ask for the following:

>

>1) Our paper should be regarded as an independent contribution, not
>as a comment on Douglass et al. This seems reasonable given i) The
>substantial amount of new work that we have done; and ii) The fact
>that the Douglass et al. paper was not regarded as a comment on
>Santer et al. (2005), or on Chapter 5 of the 2006 CCSP Report - even
>though Douglass et al. clearly WAS a comment on these two publications.

>

>2) If IJC agrees to 1), then Douglass et al. should have the
>opportunity to respond to our contribution, and we should be given
>the chance to reply. Any response and reply should be published
>side-by-side, in the same issue of IJC.

>

>I'd be grateful if you and Phil could provide me with some guidance
>on 1) and 2), and on whether you think we should submit to IJC. Feel
>free to forward my email to Glenn McGregor.

>

>With best regards,

>

>Ben

>Tim Osborn wrote:

>>At 03:52 10/01/2008, Ben Santer wrote:

>>>...Much as I would like to see a high-profile rebuttal of Douglass
>>>et al. in a journal like Science or Nature, it's unlikely that
>>>either journal will publish such a rebuttal.

>>>

>>>So what are our options? Personally, I'd vote for GRL. I think
>>>that it is important to publish an expeditious response to the
>>>statistical flaws in Douglass et al. In theory, GRL should be able
>>>to give us the desired fast turnaround time...

>>>

>>>Why not go for publication of a response in IJC? According to
>>>Phil, this option would probably take too long. I'd be interested
>>>to hear any other thoughts you might have on publication options.

>>Hi Ben and Phil,

>>as you may know (Phil certainly knows), I'm on the editorial board
>>of IJC. Phil is right that it can be rather slow (though faster
>>than certain other climate journals!). Nevertheless, IJC really is
>>the preferred place to publish (though a downside is that Douglass
>>et al. may have the opportunity to have a response considered to
>>accompany any comment).

>>I just contacted the editor, Glenn McGregor, to see what he can
>>do. He promises to do everything he can to achieve a quick
>>turn-around time (he didn't quantify this) and he will also "ask
>>(the publishers) for priority in terms of getting the paper online
>>asap after the authors have received proofs". He genuinely seems
>>keen to correct the scientific record as quickly as possible.
>>He also said (and please treat this in confidence, which is why I
>>emailed to you and Phil only) that he may be able to hold back the
>>hardcopy (i.e. the print/paper version) appearance of Douglass et
>>al., possibly so that any accepted Santer et al. comment could
>>appear alongside it. Presumably depends on speed of the review process.
>>If this does persuade you to go with IJC, Glenn suggested that I
>>could help (because he is in Kathmandu at present) with achieving
>>the quick turn-around time by identifying in advance reviewers who
>>are both suitable and available. Obviously one reviewer could be
>>someone who is already familiar with this discussion, because that
>>would enable a fast review - i.e., someone on the email list you've
>>been using - though I don't know which of these people you will be

mail.2008

>>asking to be co-authors and hence which won't be available as
>>possible reviewers. For objectivity the other reviewer would need
>>to be independent, but you could still suggest suitable names.
>>well, that's my thoughts... let me know what you decide.

>>Cheers

>>Tim

>>

>>Dr Timothy J Osborn, Academic Fellow

>>Climatic Research Unit

>>School of Environmental Sciences

>>University of East Anglia

>>Norwich NR4 7TJ, UK

>>e-mail: t.osborn@uea.ac.uk

>>phone: +44 1603 592089

>>fax: +44 1603 507784

>>web: http://www.cru.uea.ac.uk/~timo/

>>sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm

>

>

>--

>-----

>Benjamin D. Santer

>Program for Climate Model Diagnosis and Intercomparison

>Lawrence Livermore National Laboratory

>P.O. Box 808, Mail Stop L-103

>Livermore, CA 94550, U.S.A.

>Tel: (925) 422-2486

>FAX: (925) 422-7675

>email: santer1@llnl.gov

>-----

Dr Timothy J Osborn, Academic Fellow

Climatic Research Unit

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089

fax: +44 1603 507784

web: http://www.cru.uea.ac.uk/~timo/

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

</x-flowed>

850. 1200090166.txt

#####

From: Tim Osborn <t.osborn@uea.ac.uk>

To: santer1@llnl.gov

Subject: Re: Potential reviewers

Date: Fri Jan 11 17:22:46 2008

I didn't know about the link between John and Kevin. Sounds like Qiang or Myles,
plus

Francis, would be best combination of expertise and speediness.

By the way, for online submission you'll just need to convert the Latex to a PDF
file and

submit that.

Have a good weekend,

mail.2008

Tim

At 17:07 11/01/2008, you wrote:

Dear Phil and Tim,
I did leave Kevin's name off because of concerns that he might be extremely upset by Christy's involvement in Douglass et al. I guess you know that John was a Ph.D. student of Kevin's. It must be tough to have a student who's the antithesis of everything you stand for and care about - careful, thorough science. Qiang Fu would be great, since he's so knowledgeable about MSU-related issues. I think he would be fast, too. Myles reviewed one of the GRL versions of Douglass et al., so he's very familiar with this territory.
with best regards,
Ben
Phil Jones wrote:

Ben,

I briefly discussed this with Tim a few minutes ago. with IDAG coming up, it is probably best not on to use Gabi and Myles. I also suggested that Mike Wallace might be slow - as Myles would have been. Peter S might not be right for the IDAG reason and he does work for the HC - where Peter T does.

If Jim is back working he would be good. So would Fu. If Tim can just persuade them to do it - and quickly.

I did suggest Kevin - he would do it quickly - but it may be a read rag to a bull with John Christy on the other paper.

Glad to see you've gone down his route!

Have a good weekend!

Ruth says hello!

Cheers

Phil

At 16:43 11/01/2008, Ben Santer wrote:

Dear Tim,

Here are some suggestions for potential reviewers of a Santer et al. IJoC submission on issues related to the consistency between modeled and observed atmospheric temperature trends. None of the suggested reviewers have been involved in the recent "focus group"

that has discussed problems with the Douglass et al. IJoC paper.

1. Mike Wallace, University of Washington. U.S. National Academy member. Expert on atmospheric dynamics. Chair of National Academy of Sciences committee on "Reconciling observations of global temperature change" (2000). Email: wallace@atmos.washington.edu

2. Qiang Fu, University of Washington. Expert on atmospheric radiation, dynamics, radiosonde and satellite data. Published 2004 Nature paper and 2005 GRL paper dealing with issues related to global and tropical temperature trends. Email: qfu@atmos.washington.edu

3. Gabi Hegerl, University of Edinburgh. Expert on detection and attribution of externally-forced climate change. Co-Convening Lead Author of "Understanding and

Attributing Climate Change" chapter of IPCC Fourth Assessment Report. Email: Gabi.Hegerl@ed.ac.uk

4. Jim Hurrell, National Center for Atmospheric Research (NCAR). Former

mail.2008

Director of
Climate and Global Dynamics division at NCAR. Expert on climate modeling,
observational
data. Published a number of papers on MSU-related issues. Email:
jhurrell@cgd.ucar.edu

5. Myles Allen, Oxford University. Expert in Climate Dynamics, detection and
attribution, application of statistical methods in climatology. Email:
allen@atm.ox.ac.uk

6. Peter Stott, Hadley Centre for Climate Prediction and Research. Expert in
climate
modeling, detection and attribution. Email: peter.stott@metoffice.gov.uk
with best regards,
Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

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

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

851. 1200112408.txt

#####

From: Leopold Haimberger <leopold.haimberger@univie.ac.at>
To: santer1@llnl.gov
Subject: Re: IJoC and Figure 4
Date: Fri, 11 Jan 2008 23:33:28 +0100
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>, Dian Seidel
<dian.seidel@noaa.gov>, Tom Wigley <>wigley@cgd.ucar.edu>, Karl Taylor
<taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante
<John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader"
<bader2@llnl.gov>, "Francis W. Zwiers" <francis.zwiers@ec.gc.ca>, Frank Wentz
<frank.wentz@remss.com>, Melissa Free <melissa.free@noaa.gov>, "Michael C.
MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Steve Sherwood
<Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon'
<ssolomon@al.noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt

mail.2008

<gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

<x-flowed>
Dear folks,

I believe Ben's suggestion is very good compromise and we should prepare a Fig. 4 with three RAOBCORE versions, RICH, HadAT and RATPAC. As I have understood Ben in his first description of Fig. 4, also the range of model trend profiles should be included.

Who will actually draw the figure? I can do this but I do not have the model data and I do not have the RATPAC profiles so far. It would be easiest to remove the Titchner et al. profiles and Steves profiles from Peter's plot. Or should we send our profile data to you, Ben? What do you think?

Concerning the possible reaction of Douglass et al.: RAOBCORE v1.2 and v1.3 are both published in the Haimberger(2007) RAOBCORE paper (where they were labeled differently). Thus they have at least omitted v1.3. RAOBCORE v1.4 time series have published in the May 2007 BAMS climate state of 2006 supplement.

Peter, myself, Dian and probably a few others will meet in Japan by the end of January and a few weeks later in Germany, where we can discuss the latest developments and plan the publishing strategy.

Thanks a lot Ben for moderating this Fig. 4 issue.

Regards,

Leo

Ben Santer wrote:

> Dear folks,
>
> Just a quick update. With the assistance of Tim Osborn, Phil Jones, and
> Dian, I've now come to a decision about the disposition of our response
> to Douglass et al. I've decided to submit to IJoC. I think this is a
> fair and reasonable course of action. The IJoC editor (and various IJoC
> editorial board members and Royal Meteorological Society members) now
> recognize that the Douglass et al. paper contains serious statistical
> flaws, and that its publication in IJoC reflects poorly on the IJoC and
> Royal Meteorological Society. From my perspective, IJoC should be given
> the opportunity to set the record straight.
>
> The editor of IJoC, Glenn McGregor, has agreed to treat our paper as an
> independent submission rather than as a comment on Douglass et al. This
> avoids the situation that I was afraid of - that our paper would be
> viewed as a comment, and Douglass et al. would have the "last word" in
> this exchange. In my opinion (based on many years of interaction with
> these guys), neither Douglass, Christy or Singer are capable of
> admitting that their paper contained serious scientific errors. Their
> "last word" would have been an attempt to obfuscate rather than
> illuminate. That would have been very unfortunate.
>
> If our contribution is published in IJoC, Douglass et al. will have the
> opportunity to comment on it, and we will have the right to reply.
> Ideally, any comment and reply should be published side-by-side in the
> same issue of IJoC.
>
> The other good news is that IJoC is prepared to handle our submission
> expeditiously. My target, therefore, is to finalize our submission by
> the end of next week. I hope to have a first draft to send you by no

mail.2008

> later than next Tuesday.
>
> Now on to the "Figure 4" issue. Thanks to many of you for very helpful
> discussions and advice. Here are some comments:
>
> 1) I think it is important to have a Figure 4. We need to provide
> information on structural uncertainties in radiosonde-based estimates of
> profiles of atmospheric temperature change. Douglass et al. did not
> accurately portray the full range of structural uncertainties.
>
> 2) I do not want our submission to detract from other publications
> dealing with recent progress in the development of sonde-based
> atmospheric temperature datasets. I am aware of at least four such
> publications which are "in the pipeline".
>
> 3) So here is my suggestion for a compromise.
>
> o If Leo is agreeable, I would like to show results from his three
> RAOBCORE versions (v1.2, v1.3, and v1.4) in Figure 4. I'd also like to
> include results from the RATPAC and HadAT datasets used by Douglass et
> al. This allows us to illustrate that Douglass et al. were highly
> selective in their choice of radiosonde data. They had access to results
> from all three versions of RAOBCORE, but chose to show results from v1.2
> only - the version that provided the best support for their "models are
> inconsistent with observations" argument.
>
> o I suggest that we do NOT show the most recent radiosonde results
> from the Hadley Centre (described in the Titchner et al. paper) or from
> Steve Sherwood's group. This leaves more scope for a subsequent paper
> along the lines suggested by Leo, which would synthesize the results
> from the very latest sonde- and satellite-based temperature datasets,
> and compare these results with model-based estimates of atmospheric
> temperature change. I think that someone from the sonde community should
> take the lead on such a paper.
>
> 4) As Melissa has pointed out, Douglass et al. may argue that v1.2 was
> published at the time they wrote their paper, while v1.3 and v1.4 were
> unpublished (but submitted). I'm sure this is how Douglass et al. will
> actually respond. Nevertheless, I strongly believe that Douglass et al.
> should have at least mentioned the existence of the v1.3 and v1.4 results.
>
> Do these suggested courses of action (submission to IJoC and inclusion
> of a Figure 4 with RAOBCOREv1.2,v1.3,v1.4/RATPAC/HadAT data) sound
> reasonable to you?

> With best regards,

> Ben

> -----
>
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-2486
> FAX: (925) 422-7675
> email: santer1@llnl.gov
> -----

--
Ao. Univ. Prof. Dr. Leopold Haimberger

mail.2008

Institut für Meteorologie und Geophysik, Universität wien
Althanstraße 14, A - 1090 wien
Tel.: +43 1 4277 53712
Fax.: +43 1 4277 9537
<http://mailbox.univie.ac.at/~haimbel7/>
</x-flowed>

852. 1200162026.txt

#####

From: John Lanzante <John.Lanzante@noaa.gov>
To: santer1@llnl.gov, John Lanzante <John.Lanzante@noaa.gov>
Subject: Re: Updated Figures
Date: Sat, 12 Jan 2008 13:20:26 -0500
Reply-to: John.Lanzante@noaa.gov
Cc: Melissa Free <Melissa.Free@noaa.gov>, Peter Thorne <peter.thorne@metoffice.gov.uk>, Dian Seidel <dian.seidel@noaa.gov>, Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "Francis W. Zwiers" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, Susan Solomon <Susan.Solomon@noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

Dear Ben and All,

After returning to the office earlier in the week after a couple of weeks off during the holidays, I had the best of intentions of responding to some of the earlier emails. Unfortunately it has taken the better part of the week for me to shovel out my avalanche of email. [This has a lot to do with the remarkable progress that has been made -- kudos to Ben and others who have made this possible]. At this point I'd like to add my 2 cents worth (although with the declining dollar I'm not sure it's worth that much any more) on several issues, some from earlier email and some from the last day or two.

I had given some thought as to where this article might be submitted. Although that issue has been settled (IJC) I'd like to add a few related thoughts regarding the focus of the paper. I think Ben has brokered the best possible deal, an expedited paper in IJC, that is not treated as a comment. But I'm a little confused as to whether our paper will be titled "Comments on ... by Douglass et al." or whether we have a bit more latitude.

While I'm not suggesting anything beyond a short paper, it might be possible to "spin" this in more general terms as a brief update, while at the same time addressing Douglass et al. as part of this. We could begin in the introduction by saying that this general topic has been much studied and debated in the recent past [e.g. NRC (2000), the Science (2005) papers, and CCSP (2006)] but that new developments since these works warrant revisiting the issue. We could consider Douglass et al. as one of several new developments. We could perhaps title the paper something like "Revisiting temperature trends in the atmosphere". The main conclusion will be that, in stark contrast to Douglass et al., the new evidence from the last couple of years has strengthened the conclusion of CCSP (2006) that there is no meaningful discrepancy between models and observations.

In an earlier email Ben suggested an outline for the paper:

- 1) Point out flaws in the statistical approach used by Douglass et al.

mail.2008

- 2) Show results from significance testing done properly.
- 3) Show a figure with different estimates of radiosonde temperature trends illustrating the structural uncertainty.
- 4) Discuss complementary evidence supporting the finding that the tropical lower troposphere has warmed over the satellite era.

I think this is fine but I'd like to suggest a couple of other items. First, some mention could be made regarding the structural uncertainty in satellite datasets. We could have 3a) for sondes and 3b) for satellite data. The satellite issue could be handled in as briefly as a paragraph, or with a bit more work and discussion a figure or table (with some trends). The main point to get across is that it's not just UAH vs. RSS (with an implied edge to UAH because its trends agree better with sondes) it's actually UAH vs all others (RSS, UMD and Zou et al.). There are complications in adding UMD and Zou et al. to the discussion, but these can be handled either qualitatively or quantitatively. The complication with UMD is that it only exists for T2, which has stratospheric influences (and UMD does not have a corresponding measure for T4 which could be used to remove the stratospheric effects). The complication with Zou et al. is that the data begin in 1987, rather than 1979 (as for the other satellite products).

It would be possible to use the Fu method to remove the stratospheric influences from UMD using T4 measures from either or both UAH and RSS. It would be possible to directly compare trends from Zou et al. with UAH, RSS & UMD for a time period starting in 1987. So, in theory we could include some trend estimates from all 4 satellite datasets in apples vs. apples comparisons. But perhaps this is more work than is warranted for this project. Then at very least we can mention that in apples vs. apples comparisons made in CCSP (2006) UMD showed more tropospheric warming than both UAH and RSS, and in comparisons made by Zou et al. their dataset showed more warming than both UAH and RSS. Taken together this evidence leaves UAH as the "outlier" compared to the other 3 datasets. Furthermore, better trend agreement between UAH and some sonde data is not necessarily "good" since the sonde data in question are likely to be afflicted with considerable spurious cooling biases.

The second item that I'd suggest be added to Ben's earlier outline (perhaps as item 5) is a discussion of the issues that Susan raised in earlier emails. The main point is that there is now some evidence that inadequacies in the AR4 model formulations pertaining to the treatment of stratospheric ozone may contribute to spurious cooling trends in the troposphere.

Regarding Ben's Fig. 1 -- this is a very nice graphical presentation of the differences in methodology between the current work and Douglass et al. However, I would suggest a cautionary statement to the effect that while error bars are useful for illustrative purposes, the use of overlapping error bars is not advocated for testing statistical significance between two variables following Lanzante (2005).

Lanzante, J. R., 2005: A cautionary note on the use of error bars.
Journal of Climate, 18(17), 3699-3703.

This is also motivation for application of the two-sample test that Ben has implemented.

Ben wrote:

> So why is there a small positive bias in the empirically-determined
> rejection rates? Karl believes that the answer may be partly linked to
> the skewness of the empirically-determined rejection rate distributions.
[NB: this is in regard to Ben's Fig. 3 which shows that the rejection rate
in simulations using synthetic data appears to be slightly positively biased
compared to the nominal (expected) rate].

I would note that the distribution of rejection rates is like the distribution
Page 58

mail.2008

of precipitation in that it is bounded by zero. A quick-and-dirty way to explore this possibility using a "trick" used with precipitation data is to apply a square root transformation to the rejection rates, average these, then reverse transform the average. The square root transformation should yield data that is more nearly Gaussian than the untransformed data.

Ben wrote:

> Figure 3: As Mike suggested, I've removed the legend from the interior
> of the Figure (it's now below the Figure), and have added arrows to
> indicate the theoretically-expected rejection rates for 5%, 10%, and
> 20% tests. As Dian suggested, I've changed the colors and thicknesses
> of the lines indicating results for the "paired trends". Visually,
> attention is now drawn to the results we think are most reasonable -
> the results for the paired trend tests with standard errors adjusted
> for temporal autocorrelation effects.

I actually liked the earlier version of Fig. 3 better in some regards. The labeling is now rather busy. How about going back to dotted, thin and thick curves to designate 5%, 10%, and 20%, and also placing labels (5%/10%/20%) on or near each curve? Then using just three colors to differentiate between Douglass, paired/no_SE_adj, and paired/with_SE_adj it will only be necessary to have 3 legends: one for each of the three colors. This would eliminate most of the legends.

Another topic of recent discussion is what radiosonde datasets to include in the trend figure. My own personal preference would be to have all available datasets shown in the figure. However, I would defer to the individual dataset creators if they feel uncomfortable about including sets that are not yet published.

Peter also raised the point about trends being derived differently for different datasets. To the extent possible it would be desirable to have things done the same for all datasets. This is especially true for using the same time period and the same method to perform the regression. Another issue is the conversion of station data to area-averaged data. It's usually easier to insure consistency if one person computes the trends from the raw data using the same procedures rather than having several people provide the trend estimates.

Karl Taylor wrote:

> The lower panel <of Figure 2> ...
> ... By chance the mean of the results is displaced negatively ...
> ... I contend that the likelihood of getting a difference of x is equal
> to the likelihood of getting a difference of $-x$...
> ... I would like to see each difference plotted twice, once with a positive
> sign and again with a negative sign ...
> ... One of the unfortunate problems with the asymmetry of the current figure
> is that to a casual reader it might suggest a consistency between the
> intra-ensemble distributions and the model-obs distributions that is not real
> Ben and I have already discussed this point, and I think we're both
> still a bit unsure on what's the best thing to do here. Perhaps others
> can provide convincing arguments for keeping the figure as is or making
> it symmetric as I suggest.

I agree with Karl in regard to both his concern for misinterpretation as well as his suggested solution. In the limit as N goes to infinity we expect the distribution to be symmetric since we're comparing the model data with itself. The problem we are encountering is due to finite sample effects. For simplicity Ben used a limited number of unique combinations -- using full bootstrapping the problem should go away. Karl's suggestion seems like a simple and effective way around the problem.

Karl Taylor wrote:

mail.2008

> It would appear that if we believe FGOALS or MIROC, then the
> differences between many of the model runs and obs are not likely to be
> due to chance alone, but indicate a real discrepancy ... This would seem
> to indicate that our conclusion depends on which model ensembles we have
> most confidence in.

Given the tiny sample sizes, I'm not sure one can make any meaningful statements regarding differences between models, particularly with regard to some measure of variability such as is implied by the width of a distribution. This raises another issue regarding Fig. 2 -- why show the results separately for each model? This does not seem to be relevant to this project. Our objective is to show that the models as a collection are not inconsistent with the observations -- not that any particular model is more or less consistent with the observations. Furthermore showing results for different models tempts the reader to make such comparisons. Why not just aggregate the results over all models and produce a histogram? This would also simplify the figure.

Best regards,

____John

853. 1200319411.txt

#####

From: Kevin Trenberth <trenbert@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Draft paper on Chinese temperature trends
Date: Mon, 14 Jan 2008 09:03:31 -0700
Cc: david.parker@metoffice.gov.uk, Thomas.C.Peterson@noaa.gov, Reinhard Boehm <Reinhard.Boehm@zamg.ac.at>, Susan Solomon <Susan.Solomon@noaa.gov>, Adrian Simmons <adrian.simmons@ecmwf.int>

Hi Phil

I'll read it more thoroughly later. My quick impression, more from the abstract than the main text, is that you are defensive and it almost seems that there is a denial of the UHI in part. Yet later in the abstract and nicely in the first two sentences of the conclusions, you recognize that the UHI is real and the climate is different in cities.

The point is that the homogenization takes care of this wrt the larger scale record and that UHI is essentially constant at many sites so that it does not alter trends.

So I urge you to redo the abstract and be especially careful of the wording. You might even start with:

The Urban Heat Island (UHI) is a real phenomenon in urban settings that generally makes cities warmer than surrounding rural areas. However, UHIs are evident at both London and Vienna, but do not contribute to the warming trends over the 20th century because the city influences have not changed much over that time. Similarly, ...

Regards

Kevin

Phil Jones wrote:

Dear All,

I have mentioned to you all that I've been working on a paper on

mail.2008

Chinese temperature trends. This partly started because of allegations about Jones et al. (1990). This shows, as expected, that these claims were groundless.

Anyway - I'd appreciate if you could have a look at this draft. I have spelt things out in some detail at times, but I'm expecting if it is published that it will get widely read and all the words dissected. I know you're all very busy and I could have been doing something more useful, but it hasn't taken too long.

The European examples are just a simple way to illustrate the difference between UHIs and urban-related warming trends, and an excuse to reference Luke Howard.

Cheers

Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email [1]p.jones@uea.ac.uk
NR4 7TJ
UK

--

Kevin E. Trenberth e-mail: [2]trenbert@ucar.edu
Climate Analysis Section, [3]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. mailto:p.jones@uea.ac.uk
- 2. mailto:trenbert@ucar.edu
- 3. http://www.cgd.ucar.edu/cas/trenbert.html

854. 1200421039.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: James Hansen <jhansen@giss.nasa.gov>
Subject: Differences in our series (GISS/HadCRUT3)
Date: Tue Jan 15 13:17:19 2008
Cc: gschmidt@giss.nasa.gov

Jim, Gavin,

Thanks for the summary about 2007. We're saying much the same things about recent temps, and probably when it comes to those idiots saying global warming is stopping - in some recent RC and CA threads. Gavin has gone to town on this with 6,7, 8 year trends etc.

What I wanted to touch base on is the issue in this figure I got yesterday. This is more of the same. You both attribute the differences to your extrapolation over the Arctic (as does Stefan). I've gone along with this, but have you produced an NH series excluding the Arctic? Do these agree better?

I reviewed a paper from NCDC (Tom Smith et al) about issues with recent SSTs and the greater number of buoy type data since the late-90s (now about 70%) cf ships. The paper shows ships are very slightly warmer

mail.2008

cf buoys (~0.1-0.2 for all SST). I don't think they have implemented an adjustment for this yet, but if done it would raise global T by about 0.1 for the recent few years. The paper should be out in J. Climate soon.

The HC folks are not including SST data appearing in the Arctic for regions where their climatology (61-90) includes years which had some sea ice. I take it you and NCDC are not including Arctic SST data where the climatology isn't correct? You get big positive anomalies if you do.

Some day we will have to solve both these issues. Both are difficult, especially the latter!

Cheers
Phil

At 21:39 14/01/2008, you wrote:

To be removed from Jim Hansen's e-mail list respond with REMOVE as subject
Discussion of 2007 GISS global temperature analysis is posted at Solar and
Southern
Oscillations
[1]http://www.columbia.edu/~jeh1/mailings/20080114_GISTEMP.pdf
Jim

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.columbia.edu/~jeh1/mailings/20080114_GISTEMP.pdf

855. 1200425298.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: trenbert@ucar.edu
Subject: Re: Draft paper on Chinese temperature trends
Date: Tue Jan 15 14:28:18 2008
Cc: david.parker@metoffice.gov.uk, thomas.c.peterson@noaa.gov, "Reinhard Boehm" <reinhard.boehm@zang.ac.at>, "Susan Solomon" <susan.solomon@noaa.gov>, "Adrian Simmons" <adrian.simmons@ecmwf.int>

Kevin,
Homogeneity only done on mean T. Lots of sites just measure this. A lot will measure max and min, but I haven't got the data. I also didn't want to get into max/min as what is relevant to urban-related warming in the global land series (or China) is the effects on mean T. I can't then look at max or min against a rural series.

I would expect max to have changed less than min, but I can't really look at that.

Also I don't want to confuse readers by saying there is an urban-related temp influence, but it is to a lower DTR. I guess I could refer to Vose et al (our Fig 3.11) which does show a decrease in DTR for 79-04 over China (mostly blues).

I'll work on the text.

Cheers
Phil

At 04:50 15/01/2008, Kevin Trenberth wrote:

mail.2008

Phil

I looked at the paper in more detail. It obviously needs a bit of polishing throughout.

I have a couple of fairly major comments. The first is that you only deal with the mean temperature and nothing on the max and min temperatures. Are those available? It would be much more powerful if those could be included. The second is the special situation in China associated with urbanization and that is air pollution. You do not mention aerosols and their effects. We have some on that in AR4 that may be of value: refer to our chapter.

In China, there has been so much increase in coal fired power and pollution (11 out of the top worst ten polluted cities in the world are in China, or something like that). So you do not see the sun for long periods of time. Presumably that greatly cuts down on the max temp but may also increase the min through a sort of greenhouse effect? Effects of urban runoff tend to warm and space heating also warms but should mainly affect the min. Pollution may not be in the inner city but concentrated more near the sites of industry and power stations; but also may not be that local owing to winds? Pollution may also change fog or smog conditions, and may also change drizzle and precip. Looking at other variables could help with whether the changes are local or linked to atmospheric circulation.

The unique aspect of urbanization related to air pollution should make China different, but may not be easily untangled without max and min temps (and DTR).

Anyway, given these aspects, you may want to at least assemble the expectations somewhere altogether and discuss max (day) vs night (min) effects?

Hope this helps

Kevin

>

>> Dear All,

> I have mentioned to you all that I've been working on a paper on
> Chinese temperature trends. This partly started because of allegations
> about Jones et al. (1990). This shows, as expected, that these claims
> were groundless.

> Anyway - I'd appreciate if you could have a look at this draft. I
> have

> spelt things out in some detail at times, but I'm expecting if it
> is published

> that it will get widely read and all the words dissected. I know you're
> all

> very busy and I could have been doing something more useful, but it
> hasn't

> taken too long.

> The European examples are just a simple way to illustrate the
> difference

> between UHIs and urban-related warming trends, and an excuse to
> reference Luke Howard.

>

> Cheers

> Phil

>

>

> Prof. Phil Jones

> Climatic Research Unit Telephone +44 (0) 1603 592090

> School of Environmental Sciences Fax +44 (0) 1603 507784

> University of East Anglia

> Norwich

Email p.jones@uea.ac.uk

> NR4 7TJ

> UK

>

mail.2008

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

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

References

1. <http://www.cgd.ucar.edu/cas/trenbert.html>

856. 1200426564.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@psu.edu
Subject: Re: Edouard Bard
Date: Tue Jan 15 14:49:24 2008
Cc: gschmidt@giss.nasa.gov

Mike,
Good triumphs over bad - eventually!
It does take a long time though!
Maybe Ray P. wants to do something. He is more up to speed
on all this - and reads French!
Cheers
Phil

At 14:33 15/01/2008, Michael Mann wrote:

Phil,
thanks for sending on, I've sent to Ray P. The Passoti piece is remarkably bad
for a Science "news" piece, it would be worth discussing this w/ the editor, Donald
Kennedy who is quite reasonable, and probably a bit embarrassed by this.
My french isn't great, but I could see there was something also about the
Moberg reconstructions, Courtilot obviously trying to use that to arge that the recent
warming isn't anomalous (even though the Moberg recon actually supports that it is).
I'll need to read over all of this and try to digest when I have a chance later
today.
Keep up the good fight, the attacks are getting more and more desparate as the
public contrarians are increasingly losing the battle (both scientifically, and in the
sphere). one thing I've learned is that the best way to deal w/ these attacks
is just to go on doing good science, something I learned from Ben...
talk to you later,
mike
well, the Phil Jones wrote:

mail.2008

Gavin, Mike,

Some emails within this and an attachment. Send on to Ray Pierrehumbert.

Maybe you're aware but things in France are getting bad.

One thing might be a letter to Science re the diagram in an editorial in Science.

I did talk to the idiot who wrote this, but couldn't persuade him it was rubbish. This

isn't the worst - see this email below from Jean Jouzel and Edouard Bard. My French is poor

at the best of times, but this all seems unfair pressure on Edouard.

See also this in French about me - lucky I can't follow it that well !

I know all this is a storm in a teacup - and I hope I'd show your resilience Mike if

this was directed at me. I'm just happy I'm in the UK, and our Royal Society knows

who and why it appoints its fellows!

In the Science piece, the two Courtillot papers are rejected. I have the journal

rejection emails - the other reviewer wasn't quite as strong as mine, but they were

awful.

Cheers

Phil

From: Jean Jouzel [1]<jean.jouzel@lsce.ipsl.fr>

Subject: Re: Fwd: Re: Fwd: FYI: Daggers Are Drawn

X-Greylist: Sender IP whitelisted, not delayed by milter-greylis-3.0

(shiva.jussieu.fr

[134.157.0.166]); Tue, 15 Jan 2008 00:07:14 +0100 (CET)

X-Virus-Scanned: ClamAV 0.92/5483/Mon Jan 14 15:45:01 2008 on shiva.jussieu.fr

X-Virus-Status: Clean

X-Miltered: at shiva.jussieu.fr with ID 478BEB15.002 by Joe's j-chkmail (

[2]http://j-chkmail.ensmp.fr)!

X-UEA-Spam-Score: 0.3

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Dear Phil,

Yes the situation is very bad in and I was indeed going to write you to ask somewhat for

your help in getting some support to Edouard, which is really needed.

Certainly one

thing you could do would be to write to the editor of Science at least pointing to the

fact that the figure is misleading using again the seasonal above 20°N Briffa

et al. data set as global.

May be also at some point write something supporting the answer of Edouard and Gilles

Delaygue, to EPSL (or in answering the letter Courtillot has recently written see

attached in which he is very critical with respect to your work). I don't know

....

Yes I will be in Vienna , this will be a pleasure to meet you with my best Jean

At 15:29 +0000 14/01/08, Phil Jones wrote:

Jean,

Will you be going to the EGU in Vienna this April?

Page 65

mail.2008

This disagreement with Courtillot seems to be getting out of hand. Edouard isn't having a great time at the moment. The data Courtillot used is not on the CRU web site. We did produce it, but for a paper Keith worked on in 2002. Courtillot's global is CRU data, but not the globe - it is land north of 20N and April to September only! The French Academy is looking a bit of a laughing stock! I did meet Courtillot in March last year - he was courteous, but he should read the

literature!

Cheers
Phil

X-Virus-Scanned: amavisd-new at arbois.cerege.fr
Date: Mon, 14 Jan 2008 12:20:00 +0100
To: Phil Jones [3]<p.jones@uea.ac.uk>
From: Edouard BARD [4]<bard@cerege.fr>
Subject: Re: Fwd: FYI: Daggers Are Drawn
X-UEA-Spam-Score: 1.4
X-UEA-Spam-Level: +
X-UEA-Spam-Flag: NO

>Courtillot says he will soon publish two studies arguing that methods used to measure global T need to be revised. Wonder if these are the two that I rejected!

Maybe one day he'll realise that there is oceanic data!

Cheers
Phil

Hello Phil,

These are indeed the papers submitted to EPSL. Courtillot has control on other journals and I'm sure he will manage to publish them somewhere else ...

As you can read below, Courtillot accused me publicly of scientific misconduct in a

written message sent in copy to the president of the Academy of Science, to the president of the CNRS and to the Director of the Cabinet of the Ministry of

Higher Education and Research. According to Courtillot, my misconduct is that I have acted as a

hacker, introducing a "note added in proof" in my EPSL paper without the editor and the publisher even knowing it !

Courtillot even requested the organization this week of a secret meeting at the Academy

in order to expose the case (yes, you've read it correctly, this is officially called

"un comité secret"). I am not a member of the Academy and nobody is there to defend my

case. Hence, I was obliged to write this long email to explain my position to some

academicians.

I'm not really planning for sending soon something to Science as my next week will be

hectic with this "inquisition" committee against me and the impact of the "droit de

réponse" in newspaper(s). I am sure that Courtillot will even use Pasotti's

poor paper against me during the audit of the case at the Academy. As I am the main author for the

Comment, sending a rebuttal to Science may even be counterproductive. Do you plan to

send something to Science about the fact that the Figure misrepresent Tglobe ? I'm quite depressed because this is taking a lot of my time and energy.

Everybody at

mail.2008

home is mad at me, children and wife, because I spend hours and days in the lab writing and checking emails and answering phone calls.
Best wishes, Edouard

Date: Sat, 12 Jan 2008 22:13:14 +0100
To: [5]bard@cerege.fr
From: Edouard BARD [6]<bard@cerege.fr>
Subject: Accusations de M. Courtillot
Destinataires:

Madame la Présidente du CNRS, Monsieur le Président de l'Académie des Sciences, Monsieur le Directeur de Cabinet de Madame la Ministre de l'Enseignement Supérieur et de la

Recherche,
Mesdames et Messieurs, Membres de l'Académie (incluant M. Courtillot).
Chers Collègues,

Je reviens à l'instant d'une tournée de conférences en Angleterre (Royal Geographical Society de Londres et Université de Cambridge). J'apprends avec stupeur que

Monsieur Courtillot m'attaque personnellement et publiquement d'avoir eu un comportement contraire à l'éthique scientifique ("contrairement aux règles déontologiques, la note de M. Bard a été envoyée APRES acceptation de son commentaire critique", cf. plus bas le message envoyé hier le 11 janvier et dont vous êtes destinataires). Cette accusation surprenante est totalement infondée.

Je rappelle que dans mon Commentaire qui vient d'être publié par la revue Earth & Planetary Science Letters (EPSL, pdf attaché), je n'ai proféré aucune accusation à l'égard de M. Courtillot. J'évite justement d'avoir un ton polémique en me cantonnant à des discussions dans l'arène scientifique, par exemple mes interventions lors du colloque organisé par M. Courtillot à l'Académie des Sciences (conférence et débat disponibles sur le site internet de l'Académie:

[7]http://www.academie-sciences.fr/conferences/seances_publicques/html/debat_13_03_07.htm

[8]<http://www.canalacademie.com/Modelisation-du-climat-et-role-du.html> ou mes publications, notamment ce 'Commentaire' Bard & Delaygue (2008 EPSL). Sur cette affaire, je n'ai accepté de faire aucun commentaire dans la presse et j'ai refusé toutes les demandes d'interview par les media audiovisuels. Même si je ne le voulais pas, je suis maintenant forcé de sortir de ma réserve et de me défendre publiquement contre les accusations de M. Courtillot.

La "note added in proof" dont vous parle M. Courtillot a été soumise normalement pour approbation au rédacteur d'EPSL (editor en anglais), M. Rob van de Hilst du MIT, comme le demande classiquement l'éditeur Elsevier (publisher) lorsqu'il envoie les épreuves d'un article à son auteur. Vous trouverez ci-dessous la copie de mon dernier échange à ce sujet avec M. van der Hilst qui explique clairement que je n'ai absolument

rien à me
rechercher. M. van der Hilst écrit lui-même "INDEED, YOU DID THE RIGHT THING IN
ASKING MY
APPROVAL.". Le fait que ma "note added in proof" ait été incluse dans la
version sous
presse de notre Commentaire est simplement dû à une erreur technique de
l'éditeur
Elsevier. Il est évidemment IMPOSSIBLE pour un auteur de modifier lui-même quoi
que ce
soit sur le site web d'Elsevier !
La meilleure preuve que cette fameuse note a été CRUCIALE pour clarifier
l'origine des
données utilisées par M. Courtillot est que justement le rédacteur, M. van der
Hilst, a
finalement décidé de la publier in extenso pour expliquer aux lecteurs son
importance
(sa note éditoriale complète est copiée plus bas). M. van der Hilst écrit ainsi
"Bard
and Delaygue noticed inconsistencies in the citation of data sources in
Courtillot et
al. (2008). and Courtillot et al. (2007)..." "instead of global, annual means
they are
seasonal estimates from land regions north of 20°N. With access to the correct
data
files readers can form their own opinion on the analysis of and conclusions by
Courtillot et al. (2007)."
Il aura donc fallu une année (voire plus, depuis Le Mouël et al. 2005 EPSL) et
de
nombreux courriers et publications, pour que l'on sache enfin quelles sont les
températures représentées par Courtillot et al. (2007) et Le Mouël et al.
(2005). La
réalité est que la courbe de température utilisée par Courtillot & Le Mouël
provient
d'un calcul de moyenne régionale et saisonnière (Briffa et al. JGR 2001) fondé
sur les
séries de températures de Jones et al. (1999 Rev. Geophys.). Le fichier cité
par
Courtillot et al. (2007) n'est donc pas un de ceux distribués par M. Philip D.
Jones
(University of East Anglia & Hadley Center), ni même tiré directement de
l'article de
Jones et al. (1999). La citation correcte aurait dû être l'article de Briffa et
al.
(2001) dont Jones est coauteur. Ceci étant dit, le problème CRUCIAL est qu'il
ne s'agit
pas de moyenne annuelle mondiale (Tglobe) comme l'ont écrit Courtillot et al.
(2007) et
Le Mouël et al. (2005), mais en fait de données régionales ET saisonnières
(latitudes
>20°N ET seulement sur les continents ET seulement pendant la saison chaude
d'Avril à
Septembre). Les courbes de la température moyenne annuelle mondiale (de MM.
Phil Jones
d'UEA ou de Jim Hansen de la NASA) ne présentent pas de corrélation marquée
avec
l'éclairement solaire et les indices géomagnétiques, en particulier au niveau
des années
70 (voir la Figure 1 de notre Commentaire publié par EPSL qui représente la
VERITABLE
courbe de température globale distribuée par M. Phil Jones).
Pour ce qui concerne les données d'irradiance solaire totale, les lecteurs
d'EPSL sont
maintenant pleinement informés du fait que les données utilisées par Courtillot

et al. (2007) et Le Mouël et al. (2005) NE sont PAS des données d'irradiance totale (Solanki 2002), mais seulement de la petite composante ultraviolette (Tobiska 2001). Dans leur Réponse publiée par EPSL, M. Courtillot et ses collègues accompagnent la nouvelle citation d'une note très surprenante (page 2, colonne 1): "(Tobiska, 2001; note that in Le Mouël et al., (2005), this data set was erroneously attributed to Solanki, 2002, although resulting changes are negligible)". Dans leurs deux articles Le Mouël et al. (2005) et Courtillot et al. (2007) auraient donc fait la même erreur de citation (Solanki 2002 au lieu de Tobiska 2001). Le problème est que justement les changements qui en résultent NE sont PAS du tout négligeables. Si Courtillot et al. (2007) et Le Mouël et al. (2005) avaient effectivement utilisé Solanki (2002), ils auraient inévitablement représenté la courbe d'irradiance $S(t)$ sur tout le 20e siècle car leur figure est focalisée sur tout ce siècle et que l'analyse de Solanki (2002) porte précisément sur TOUT le 20e siècle. Pour que l'utilisation de la courbe de Solanki n'entraîne que des changements négligeables, comme ils l'écrivent, il faudrait que ces auteurs tronquent délibérément la courbe de Solanki pour n'en montrer que la moitié (les derniers 50 ans). C'est une accusation grave que je ne fais bien évidemment pas. Par conséquent, l'utilisation de la courbe de Solanki (2002) devant être faite pour tout le 20e siècle, ceci entraîne des changements importants comme les lecteurs de notre Commentaire peuvent le constater (voir la Figure 1 qui représente la VERITABLE courbe d'irradiance solaire totale distribuée par M. S. Solanki pour TOUT le 20e siècle). En particulier, il apparaît clairement que les deux courbes géomagnétiques ESK et SIT proposées par Courtillot et al. (2007) et Le Mouël et al. (2005) sont en DESACCORD FLAGRANT vers les années 70 avec les VERITABLES courbes de la température moyenne annuelle mondiale et de l'irradiance solaire totale (dans notre Commentaire, nous soulignons au passage que l'index géomagnétique AA est en bien meilleur accord, fait connu et publié depuis dix ans, e.g. Cliver et al. 1998 GRL). Monsieur Courtillot n'apporte aucune réponse à ces nombreux problèmes. Par ailleurs, il est navrant de constater que dans ses conférences publiques récentes (voir celle donnée lors du 125e anniversaire de l'ESPCI avec fichier powerpoint disponible sur le site web [9]<http://www.espci.fr/actu/espci125/pgm0011.htm>), M. Courtillot continue encore de montrer le même diagramme erroné, avec une courbe de température "Tglobe" qui n'est pas une courbe de moyenne annuelle mondiale de la température et une courbe "S(t)" qui n'est

mail.2008

pas une courbe d'irradiance solaire totale. De plus, M. Courtillot montre encore cette courbe sur 50 ans seulement, alors même qu'il a pleinement connaissance de travaux qui donnent l'irradiance sur le tout le 20e siècle (Solanki 2002, article qu'il a lui-même cité depuis 2005).

Je vous prie de croire, Chers Collègues, à l'assurance de mes sentiments respectueux et dévoués.
Edouard Bard

Edouard BARD
Professeur au Collège de France
Chaire de l'évolution du climat et de l'océan
Directeur adjoint du CEREGE, UMR-6635
Le Trocadéro, Europole de l'Arbois BP80
13545 Aix-en-Provence cdx 4
Tel 04 42 50 74 18, 04 42 50 74 20 (secr.)
Fax 04 42 50 74 21, email [10]bard@cerege.fr
[11]http://www.college-de-france.fr/default/EN/all/evo_cli/

De : Vincent Courtillot [[12]courtil@ipgp.jussieu.fr]

Envoyé : vendredi 11 janvier 2008 12:13

À : MALAUSSENA Béatrice

Cc : [13]lemouel@ipgp.jussieu.fr; [14]fluteau@ipgp.jussieu.fr;

[15]gallet@ipgp.jussieu.fr; [16]zerbib@ipgp.jussieu.fr;

[17]dyon@ipgp.jussieu.fr;

[18]catherine.brechignac@lac.u-psud.fr; [19]catherine.brechignac@cnrs-dir.fr;

[20]J.Hoffmann@ibmc.u-strasbg.fr; [21]jules.hoffmann@academie-sciences.fr;

COURTOIS

Gérard; [22]laurentjoffrin@yahoo.fr; [23]smarchand@lefigaro.fr;

[24]pgillet@geologie.ens-lyon.fr; [25]philippe.gillet@recherche.gouv.fr

Objet : Droit de réponse

Importance : Haute

Madame,

étant donné le contenu du message que j'ai reçu de vous hier, je préfère continuer notre échange en en gardant des traces écrites et en le communiquant à des tiers en tant que de besoin, en cas de suites juridiques.

Un résumé sans doute un peu simplifié mais me semble t'il non faux de notre dernière conversation et de votre message sur ma boîte vocale est le suivant.

"Votre journaliste ne m'a pas calomnié; il a simplement retranscrit des accusations formulées dans une publication scientifique. En revanche, ma réponse vous poserait des problèmes juridiques et vous auriez des amendements à m'y faire apporter."

Je vous demande tout simplement de me renvoyer mon texte avec les modifications qui vous paraissent nécessaires en rouge, naturellement maintenues à leur minimum, puisqu'il s'agit d'une lettre que j'écris et dont j'endosse la responsabilité, avec toute la force que me donne l'évident droit de réponse (pas un lecteur que j'ai rencontré ne l'a nié,

quelque soir par ailleurs la réalité du fond) que vous me devez et que Gérard Courtois a reconnu a plusieurs reprises depuis le 21 décembre dernier (tout cela traîne de façon

étonnante...).

En ce qui concerne vos échanges avec M. Foucart tels que vous me les rapportez dans le

message vocal (que j'ai conservé), juste deux commentaires:

1) vous me dites que le fond du débat entre nous deux est scientifique. M.

Foucart est

un journaliste pas un scientifique (ce sont deux métiers différents, a priori

également

estimables). Seuls les propos qu'il peut valablement citer avec leurs sources

provenant

de scientifiques qui en portent alors la responsabilité sont des débats

scientifiques.

2) Vous me dites que M. Foucart a tout fait pour me joindre. Il est exact (je

l'ai

vérifié) qu'il a téléphoné à M. Dyon à l'IPGP, une demi-heure après que je sois

rentré

chez moi victime d'une grippe qui m'a tenu au lit trois jours avec 38°5. Une

fois guéri,

je suis passé à la garde de mes enfants et petits enfants qui étaient balayés

par le

virus. Rien de grave et nombreux sont ceux qui y sont passés à Noël. Donc

j'étais

réellement souffrant (et pas entre guillemets) ce soir là. Mais je n'ai fait

barrage à

aucune demande qui m'aurait été faite: mon numéro de téléphone est public, dans

les

pages planches du bottin, pas sur liste rouge, il était facile de m'appeler

chez moi. Je

n'ai reçu aucun coup de téléphone de M. Foucart, ni d'ailleurs d'aucun des

autres

journalistes. Et faire son travail en l'occurrence, alors que rien ne

justifiait

l'urgence de la publication de ce sujet, c'était attendre un ou deux jours et

faire

l'effort de me téléphoner et d'avoir mon témoignage. L'effort fait s'est limité

à ce

coup de fil à l'IPGP...

J'ai eu la possibilité de démontrer hier pendant deux heures devant un

auditoire

scientifique de plus de 150 personnes que non seulement les accusations portées

contre

nous, notamment par votre journaliste, étaient purement et simplement sans

fondement,

mais que de plus les critiques scientifiques formulées par Edouard Bard et

Gilles

Delaygue étaient pour l'essentiel fausses ou fondées sur des témoignages faux

que leur

avaient fourni certains de leurs collègues. Nos conclusions scientifiques

restent donc

dans leur totalité. mais ceci est la partie scientifique et ce qui m'intéresse

en ce qui

vous concerne c'est de faire savoir à vos lecteurs que les allégations de M.

Foucart

dans l'article incriminé étaient fausses, et j'ai le droit de le faire et

rapidement et

sans censure!

Dans ce séminaire hier, étaient présentes de très nombreuses personnes qui

pourront

porter témoignage. C'est le cas de l'ancienne directrice de la recherche, Mme

Giacobino,

de la présidente du CNRS Madame Catherine Bréchnignac qui m'a publiquement

assuré de son

soutien et m'a assuré qu'elle était totalement convaincue par ma démonstration

mail.2008

(sa compétence scientifique ne devrait pas être trop inférieure à celle de M. Foucart).
Etaient également présents un journaliste de l'AFP et un journaliste de science et avenir. Cette conférence, comme toutes celles de l'IPGP, sera bientôt disponible sur cd et intranet.
Mardi prochain, je suis invité par le président de l'académie des sciences à exposer pendant 20mn l'ensemble de nos arguments. Je pense que le résultat sera le même qu'hier à l'IPGP. A cette occasion, puisqu'aucune n'est encore sortie, je remettrai à l'ensemble des académiciens une copie des trois lettres de droit de réponse envoyées aux journaux, dont le vôtre. J'aurais préféré qu'elles soient publiées avant cette date (je note que ni Libération ni le Figaro ne m'ont à ce jour donné la moindre indication sur la publication de mes droits de réponse; le Monde aura pour l'instant été le plus réactif.
Je mets en copie les trois personnes à qui j'ai originellement envoyé mon droit de réponse dans ces trois journaux).
J'attends donc la version amendée que vous souhaitez me voir accepter.
Sincèrement, VC
PS J'ai eu copie, comme M. Foucart l'évoquait, d'une réponse qu'il a fournie à un de mes amis qui lui avait écrit; je la reproduis ci dessous. Je ne souhaite y relever qu'une seule phrase: "Le blog RealClimate cité dans l'article n'est que la façade d'une polémique qui se joue dans une revue savante, EPSL en l'occurrence. " Cette présentation est inexacte. L'échange dans EPSL est un échange scientifique, sans polémique et surtout sans diffamation. Il est cependant désormais établi que, contrairement aux règles déontologiques, la note de M. Bard a été envoyée APRES acceptation de son commentaire critique. Le rédacteur de la revue vient de nous envoyer copie de l'éditorial qu'il va faire publier en en-tête de nos deux articles dès la publication papier (qui fait autorité et doit avoir lieu très prochainement). Je vous joins également copie de cet éditorial. La ligne jaune est celle qui sépare le débat scientifique de la diffamation.
La diffamation n'apparaît pas dans EPSL mais dans le blog Real Climate et elle est reprise activement, sous leur signature et non pas seulement en citation entre guillemets) par les journalistes. Là est la faute juridiquement répréhensible qu'ils ont commise. Là est la base de mon droit de réponse. La journaliste neutre et semblable au lecteur de base du Monde que vous m'avez dit être doit facilement s'en convaincre si elle est impartiale.

Date: Thu, 08 Nov 2007 09:44:17 -0500

Page 72

mail.2008

From: Rob van der Hilst [26]<hilst@MIT.EDU>
Organization: MIT-EAPS
To: Edouard BARD [27]<bard@cerege.fr>
Subject: Re: ESPL comment & reply

Dear Edouard, a very quick response - I cannot do anything until I hear from Elsevier's production office that changes can or cannot be made. But I want to avoid misunderstandings between us. I do not ACCUSE you of adding material and hide it from me - indeed, you did the right thing in asking my approval. However, if you now go on line and check your "comment" you will see that it does have the 'note added in proof'. So by returning the proofs with the addition it did make it to the public domain REGARDLESS of me approving it or not. The EPSL production staff should have picked up on this. So I am not pointing fingers here - I just have to deal with an unfortunate situation in which a significant addition to an already accepted text may make it into the literature even if the other party has no chance to repond or clarify the issue.
OK? Cheers, Rob

Date: Thu, 8 Nov 2007 15:27:37 +0100
To: Rob van der Hilst [28]<hilst@MIT.EDU>
From: Edouard BARD [29]<bard@cerege.fr>
Subject: ESPL comment & reply
Dear Rob,

>In principle, after approval of a 'comment' the other party is given the opportunity to respond, and approval of the 'reply' closes the process. To avoid going-back-and-forth, in my view the material should not appear on line until after approval of the corrected proofs.

I agree.

>In this case you added material to the 'comment' after seeing the 'reply', and without my consent.

I disagree with your accusation. I did NOT try to add anything and hide it from you. Indeed, I immediately sent an email to you in order to propose our 'note added in proof'. I did this because I knew very well that such a note could not be published without your consent (during 4 years I also served EPSL). Indeed, the Elsevier message accompanying the uncorrected proofs is very clear on this issue "Significant changes to the article as accepted for publication will only be considered at this stage with permission from the Editor." This was exactly the purpose of my email to you.

>In my view the sole purpose of your addition is - or, at least, should be -
> to help clarify an important issue for the readers.

This is precisely my goal.

>(NB I am sure you realize that your 'note added in proof' could be perceived by readers as an accusation that Courtillot et al are not honest about the source of the data, in particulare related to the Tglobe file, and that would be quite a serious matter.)

I am open for revision of the note if you think it could be misinterpreted. whatever the reason for the discrepancy, it is important that the reader can identify exactly the source of these important data (published paper or valid URL). This is clearly a problem with many source of data cited by Courtillot et al (and Le Mouel et al.). For example the (flawed) TSI SOLAR2000 curve now cited in the Reply by Courtillot et al. should be accompanied by its URL and/or its reference (Tobiska 2001). It is even worse for the Tglobe curve which source is still completely unclear: it does not correspond to the cited reference and the code file cited in the Reply is not available. As previously stated, it is even possible to see that it does not correspond to the cited Tglobe curve just by looking at their shapes (see Fig. 1 of our Comment that provides the two very similar Tglobe curves developed at UEA by the group of Phil Jones and at NASA by the group of Jim Hansen). If you compare these two Tglobe curves with the one represented on Fig. 3 of Courtillot et al. (2007), you will immediately see that there are important differences in the shape of the maximum in the 40s, the pause (or minimum) in the 60-70s and the phase lag in the 30s. Comparison with the Tglobe curve is obviously central to all climate-related discussions in these papers.
Best regards,
Edouard

Editorial Note

The paper entitled "Are there connections between the Earth's magnetic field and climate?" published in Earth and Planetary Science Letters (Courtillot et al., 2007) triggered a "comment" (Bard and Delaygue, 2008) and a "reply" (Courtillot et al, 2008). These publications, and EPSL's handling of the "comment" and "reply" (hereinafter C08), have received significant attention in electronic and printed news media. In a "comment-reply" exchange, standard editorial policy gives the responder the last word and requires that the "comment" is not changed once accepted by the Editor and replied to by the authors whose work is being criticized. In this case, Bard and Delaygue noticed inconsistencies in the citation of data sources in C08 and Courtillot et al. (2007) after the (accepted) "comment" and "reply" had appeared online

(but before they received galley proofs). They pointed this out in a "Note added in Proof" to their "comment". Being against EPSL's policy this modification was disapproved (and removed). However, properly reporting data is an essential aspect of scientific communication in that it enables independent evaluations of the analysis presented by authors. Therefore, Courtillot et al. were asked to clarify (in C08) the source of the data used.

For full disclosure, the note by Bard and Delaygue is reproduced here:
"In their Response to our Comment, Courtillot et al. state that for the total irradiance curve $S(t)$ they had used the SOLAR2000 model product by Tobiska (2001) instead of the century-long record by Solanki (2002) cited in their original paper (Courtillot et al. 2007). However, the SOLAR2000 model is restricted to the UV component and their total solar irradiance is severely flawed as pointed out by Lean (2002). For the global temperature T_{globe} curve cited from Jones et al. (1999) in Courtillot et al. (2007), these authors now state in their response that they had used the following data file:

monthly_land_and_ocean_90S_90N_df_1901-2001mean_dat.txt. We were unable to find this file even by contacting its putative author who specifically stated to us that it is not one of his files (Dr. Philip D. Jones, written communication dated Oct. 23, 2007)."

In response, Courtillot et al. (2007) provided two modifications (in italics) in C08:
"The solar irradiance daily time series we used is that from the SOLAR2000 research grade model upgraded to v1.23A (file Five_cycle_v1_23a.txt dated 23 April 2003) which covers the time period from 14 February 1947 to 31 May 2002 (Tobiska, 2001; note that in Le Mouél et al, 2005, this data set was erroneously attributed to Solanki, 2002, although resulting changes are negligible)." and
"The temperature series we actually used is obtained from Briffa et al. (2001)

- specifically, column 7 of

[30]ftp://ftp.ncdc.noaa.gov/pub/data/paleo/treering/reconstructions/n_hem_temp/briffa2001jgr3.txt , that is, years 1871 to 1997 - which is, originally, from Jones et al (1999) as quoted. All we did was to average it over an 11yr sliding window."

The ftp link shows that the temperatures used are indeed from Jones and co-workers, but instead of global, annual means they are seasonal estimates from land regions north of 20°N. With access to the correct data files readers can form their own opinion on the analysis of and conclusions by Courtillot et al. (2007).

Robert D. van der Hilst

Editor for Earth and Planetary Science Letters

Bard, E., and Delaygue, G., 'Comment on "Are there connections between the

mail.2008

Earth's

magnetic field and climate?" by V. Courtillot, Y. Gallet, J.-L. Le Mouél, F. Fluteau, A.

Genevey', Earth Planet. Sci. Lett. 265, 302-307, 2008

Courtillot, V., Gallet, Y., Le Mouél, J.-L., Fluteau, F., and Genevey, A., Are there

connections between the Earth's magnetic field and climate?, Earth Planet. Sci. Lett.,

253, 328-339, 2007

Courtillot, V., Gallet, Y., Le Mouél, J.-L., Fluteau, F., and Genevey, A.,

'Response to

comment on "Are there connections between Earth's magnetic field and climate"

by Bard,

E., and Delaygue, G., Earth Planet. Sci. Lett., 265, 302-307, 2008

--

Edouard BARD

Professeur au Collège de France

Chaire de l'évolution du climat et de l'océan

Directeur adjoint du CEREGE, UMR-6635

Le Trocadéro, Europole de l'Arbois BP80

13545 Aix-en-Provence cdx 4

Tel 04 42 50 74 18, 04 42 50 74 20 (secre.)

Fax 04 42 50 74 21, email [31]bard@cerege.fr

[32]http://www.college-de-france.fr/default/EN/all/evo_cli/

--

Edouard BARD

Professeur au Collège de France

Chaire de l'évolution du climat et de l'océan

Directeur adjoint du CEREGE, UMR-6635

Le Trocadéro, Europole de l'Arbois BP80

13545 Aix-en-Provence cdx 4

Tel 04 42 50 74 18, 04 42 50 74 20 (secre.)

Fax 04 42 50 74 21, email [33]bard@cerege.fr

[34]http://www.college-de-france.fr/default/EN/all/evo_cli/

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 [35]p.jones@uea.ac.uk

NR4 7TJ

UK

--

Attention new mail address : [36]jean.jouzel@lsce.ipsl.fr

Directeur de l'Institut Pierre Simon Laplace, Université de Versailles

Saint-Quentin

Bâtiment d'Alembert, 5 Boulevard d'Alembert, 78280 Guyancourt, FRANCE

tél : 33 (0) 1 39 25 58 16, fax : 33 (0) 1 39 25 58 22, Portable : 33 (0) 684759682

- Université Pierre et Marie Curie, Tour 45-46, 3ème étage, 303, 4 Place Jussieu,

mail.2008

75252 Paris Cedex 05, e-mail : [37]jzips1@ips1.jussieu.fr, 01 44 27 49 92
- Laboratoire des Sciences du Climat et de l'Environnement, UMR
CEA-CNRS-UVSQ

CE Saclay, Orme des Merisiers, 91191 Gif sur Yvette, tél : 33 (0) 1 69 08 77

13,

fax : 33 (0) 1 69 08 77 16, Bt 701, Pièce 9a, e-mail :

[38]jean.jouzel@lsce.ips1.fr

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email [39]p.jones@uea.ac.uk

NR4 7TJ

UK

--

Michael E. Mann

Associate Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology

Phone: (814) 863-4075

503 Walker Building

FAX: (814) 865-3663

The Pennsylvania State University

email: [40]mann@psu.edu

University Park, PA 16802-5013

[41]<http://www.met.psu.edu/dept/faculty/mann.htm>

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

References

1. <mailto:jean.jouzel@lsce.ips1.fr>

2. <http://j-chkmail.ensmp.fr/>

3. <mailto:p.jones@uea.ac.uk>

4. <mailto:bard@cerege.fr>

5. <mailto:bard@cerege.fr>

6. <mailto:bard@cerege.fr>

7.

http://www.academie-sciences.fr/conferences/seances_publicques/html/debat_13_03_07.htm

m

8. <http://www.canalacademie.com/Modelisation-du-climat-et-role-du.html>

9. <http://www.espci.fr/actu/espci125/pgm0011.htm>

10. <mailto:bard@cerege.fr>

11. http://www.college-de-france.fr/default/EN/all/evo_cli/

12. <mailto:courtil@ipgp.jussieu.fr>

13. <mailto:lemouel@ipgp.jussieu.fr>

14. <mailto:fluteau@ipgp.jussieu.fr>

15. <mailto:gallet@ipgp.jussieu.fr>

16. <mailto:zerbib@ipgp.jussieu.fr>

17. <mailto:dyon@ipgp.jussieu.fr>

18. <mailto:catherine.brechignac@lac.u-psud.fr>

mail.2008

- 19. mailto:catherine.brechignac@cnrs-dir.fr
- 20. mailto:J.Hoffmann@ibmc.u-strasbg.fr
- 21. mailto:jules.hoffmann@academie-sciences.fr
- 22. mailto:laurentjoffrin@yahoo.fr
- 23. mailto:smarchand@lefigaro.fr
- 24. mailto:pgillet@geologie.ens-lyon.fr
- 25. mailto:philippe.gillet@recherche.gouv.fr
- 26. mailto:hilst@MIT.EDU
- 27. mailto:bard@cerege.fr
- 28. mailto:hilst@MIT.EDU
- 29. mailto:bard@cerege.fr
- 30.

ftp://ftp.ncdc.noaa.gov/pub/data/paleo/treering/reconstructions/n_hem_temp/briffa2001jgr3.txt

- 31. mailto:bard@cerege.fr
- 32. http://www.college-de-france.fr/default/EN/all/evo_cli/
- 33. mailto:bard@cerege.fr
- 34. http://www.college-de-france.fr/default/EN/all/evo_cli/
- 35. mailto:p.jones@uea.ac.uk
- 36. mailto:jean.jouzel@lsce.ipsl.fr
- 37. mailto:jzipsl@ipsl.jussieu.fr
- 38. mailto:jean.jouzel@lsce.ipsl.fr
- 39. mailto:p.jones@uea.ac.uk
- 40. mailto:mann@psu.edu
- 41. http://www.met.psu.edu/dept/faculty/mann.htm

857. 1200493432.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
 To: Raymond P. <rtp1@geosci.uchicago.edu>
 Subject: [Fwd: Re: [Fwd: Edouard Bard]]
 Date: Wed Jan 16 09:23:52 2008
 Cc: Michael Mann <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Ray,

Glad to see you're onto this. Obviously anything shouldn't make it even worse for Edouard, but you're in contact with him.

I'd be happy to sign onto any letter from Science, but this isn't essential.

I know

the series Courtillot has used (and Pasotti re-uses) came from here, but it isn't

what he and the authors says it was. I also know it doesn't make much difference if the correct one was used - given the smoothing. It is just sloppy and a principle thing. The correct data are sitting on our web site and have been

since

Brohan et al (2006) appeared in JGR. Even the earlier version (HadCRUT2v) would have been OK, but not a specially produced series for a tree-ring reconstruction paper back in 2001/2 and not on our web site.

Then there are all the science issues you and Edouard have raised in RC and the EPSL comment.

I have had a couple of exchanges with Courtillot. This is the last of them from March 26, 2007. I sent him a number of papers to read. He seems incapable of grasping the concept of spatial degrees of freedom, and how this number can change according to timescale. I also told him where he can get station data at NCDC and GISS (as I took a decision ages ago not to release our station data, mainly because of McIntyre). I told him all this as well when we met at a meeting of

the French Academy in early March.

What he understands below is my refusal to write a paper for the proceedings

mail.2008

of

the French Academy for the meeting in early March. He only mentioned this requirement afterwards and I said I didn't have the time to rewrite was already in the literature.

It took me several more months of emails to get my expenses for going to Paris!

Cheers

Phil

From Courtillot 26 March 2007

Dear Phil,

Sure I understand. Now research wise I would like us to remain in contact.

Unfortunately, I

have too little time to devote to what is in principle not in my main stream of research

and has no special funding. But still I intend to try and persist. I find these temperature

and pressure series fascinating. I have two queries:

1) how easy is it for me (not a very agile person computer wise) to obtain the files of

data you use in the various global or non global averages of T (I mean the actual monthly

data in each 5° box prior to any processing, including computation of the "temperature

anomaly")? How do I do it? What I would like to be able to extract is for instance all of

the data within a given 5° by 5° box with their dates (so: lat, lon, time, value). I

understand these are monthly means, though we find that there may be some quite important

information in the daily values which is likely lost on monthly averaging, but this is

another question...

2) I know you answered my question but still I have trouble grasping the answer. Could you

explain how the global T average for periods say before 1900 can have a total uncertainty

under 0.2°C back to 1850. This can only be true, given the data distribution in the Rayner

et al paper, if T is an incredibly smooth function of location. Did you really answer me

that by extracting from the recent (post 1950) database data with the same geographical and

temporal distributions as the 1850-1900 data you get almost the same result as with the

full modern data (with an uncertainty just above 0.1°C). This seems truly amazing, and

would never work with the global magnetic field data I am accustomed to work on. Yet it

does not seem to me that climate varies as slowly and with as long spatial scales as the

magnetic field...

I will very much appreciate your comments and help on those.

Thank you again for having come to our meeting.

Yours very sincerely,

Vincent

--

Vincent Courtillot

Professor of Geophysics University Paris 7,
Director Institut de Physique du Globe de Paris,

Member Institut Universitaire de France,

Member Academia Europaea and French Academy of Sciences

President, Geomagnetism and Paleomagnetism, American Geophysical Union

President, Scientific Council, City of Paris

mail.2008

Date: Tue, 15 Jan 2008 12:20:57 -0500
From: Michael Mann <mann@meteo.psu.edu>
Reply-To: mann@psu.edu
Organization: Penn State University
User-Agent: Thunderbird 2.0.0.9 (windows/20071031)
To: Phil Jones <p.jones@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: [Fwd: Re: [Fwd: Edouard Bard]]
X-UEA-Spam-Score: 0.3
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO
update from Ray P...
mike

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

Subject: Re: [Fwd: Edouard Bard]
Date: Tue, 15 Jan 2008 10:20:59 -0600
From: Raymond P. [1]<rtp1@geosci.uchicago.edu>
To: Group RealClimate [2]<group@realclimate.org>
References: [3]<478CC27D.1040900@meteo.psu.edu>

--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [4]mann@psu.edu
University Park, PA 16802-5013

[5]<http://www.met.psu.edu/dept/faculty/mann.htm>

Content-Type: text/enriched;
 name="[6]file:///C:/DOCUME~1/MICHAE~1/LOCALS~1/TEMP/nsmail.1"
Content-Disposition: inline;
 filename="[7]file:///C:/DOCUME~1/MICHAE~1/LOCALS~1/TEMP/nsmail.1"
X-MIME-Autoconverted: from 8bit to quoted-printable by f05n05.cac.psu.edu id
m0FHkxKM050156

Yes indeed. I am writing a letter to Science today regarding Pasotti's
ridiculous
article. If anybody things the rest of RC should sign on to that as well, just
let
me know.

I will also have to write a Part III, covering all the junk mentioned by
Edouard and
by Phil Jones. Courtillot's response (published via a legal device activated
where
there is the possibility of threatening a libel suit) appeared in Le Monde
today. I
may give it a week or so for new developments to settle down before writing.
For example, Foucart may get a chance to write a response in Le Monde.
While I'll wait a bit before doing the RC piece, I plan to send off the letter
to

Science this week.

--Ray

On Jan 15, 2008, at 8:26 AM, Michael Mann wrote:

fyi,
mike

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

Subject:
Edouard Bard
Date:

mail.2008

Tue, 15 Jan 2008 12:59:44 +0000

From:

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

To:

gschmidt@giss.nasa.gov

CC:

Michael E. Mann <mann@meteo.psu.edu>

References:

<46E534DD.30206@met.no> <4756A519.4090906@met.no> <4757EFB1.1000608@met.no>

<477CB5FA.609@met.no> <Pine.LNX.4.61.0801030902200.1581@isotope.giss.nasa.gov>

Some emails within this and an attachment. Send on to Ray

Pierrehumbert.

Maybe you're aware but things in France are getting bad.

One thing might be a letter to Science re the diagram in an editorial in Science.

I did talk to the idiot who wrote this, but couldn't persuade him it was rubbish. This

isn't the worst - see this email below from Jean Jouzel and Edouard Bard. My French is

poor

at the best of times, but this all seems unfair pressure on Edouard.

See also this in French about me - lucky I can't follow it that well !

I know all this is a storm in a teacup - and I hope I'd show your resilience

Mike if

this was directed at me. I'm just happy I'm in the UK, and our Royal Society

knows

who and why it appoints its fellows!

journal

In the Science piece, the two Courtillot papers are rejected. I have the

were

rejection emails - the other reviewer wasn't quite as strong as mine, but they

awfiul.

Cheers

Phil

From: Jean Jouzel <jean.jouzel@lsce.ipsl.fr>

Subject: Re: Fwd: Re: Fwd: FYI: Daggers Are Drawn

X-Greylist: Sender IP whitelisted, not delayed by milter-greylis-3.0

(shiva.jussieu.fr

[134.157.0.166]); Tue, 15 Jan 2008 00:07:14 +0100 (CET)

X-Virus-Scanned: ClamAV 0.92/5483/Mon Jan 14 15:45:01 2008 on shiva.jussieu.fr

X-Virus-Status: Clean

X-Miltered: at shiva.jussieu.fr with ID 478BEB15.002 by Joe's j-chkmail (

[8]http://j-chkmail.ensmp.fr)!

X-UEA-Spam-Score: 0.3

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Dear Phil,

somewhat for Yes the situation is very bad in and I was indeed going to write you to ask

your help in getting some support to Edouard, which is really needed.

Certainly one

thing you could do would be to write to the editor of Science at least pointing

to the

fact that the figure is misleading using again the seasonal above 20°N Briffa

et al.

data set as global.

Gilles May be also at some point write something supporting the answer of Edouard and

Delaygue, to EPSL (or in answering the letter Courtillot has recently written

see

attached in which he is very critical with respect to your work). I don't know

....

Yes I will be in Vienna , this will be a pleasure to meet you with my best

Jean

</blockquote></x-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. <mailto:rtp1@geosci.uchicago.edu>
2. <mailto:group@realclimate.org>
3. <mailto:478CC27D.1040900@meteo.psu.edu>
4. <mailto:mann@psu.edu>
5. <http://www.met.psu.edu/dept/faculty/mann.htm>
6. <file:///C:/DOCUME~1/MICHAEL~1/LOCALS~1/TEMP/nsmail.1/>
7. <file:///C:/DOCUME~1/MICHAEL~1/LOCALS~1/TEMP/nsmail.1/>
8. <http://j-chkmail.ensmp.fr/>

858. 1200651426.txt

 #####

From: "James Hansen" <jhansen@giss.nasa.gov>
 To: "Phil Jones" <p.jones@uea.ac.uk>
 Subject: Re: [Fwd: RE: Dueling climates]
 Date: Fri, 18 Jan 2008 05:17:06 -0500
 Cc: "Kevin Trenberth" <trenbert@ucar.edu>, "Karl, Tom" <Thomas.R.Karl@noaa.gov>, "Reto Ruedy" <rruedy@giss.nasa.gov>

Thanks, Phil. Here is a way that Reto likes to list the rankings that come out of our version of land-ocean index.

rank		LOTI
1	2005	0.62C
2	1998	0.57C
	2007	0.57C
	2002	0.56C
	2003	0.55C
	2006	0.54C
7	2004	0.49C

i.e., the second through sixth are in a statistical tie for second in our analysis. This seems useful, and most reporters are sort of willing to accept it. Given differences in treating the Arctic etc., there will be substantial differences in rankings. I would be a bit surprised is #7 (2004) jumped ahead to be #2 in someone else's analysis, but perhaps even that is possible, given the magnitude of these differences.
 Jim

On Jan 18, 2008 5:03 AM, Phil Jones <[1]p.jones@uea.ac.uk> wrote:

Kevin,
 when asked I always say the differences are due to the cross-Arctic

mail.2008

extrapolation.

Also

as you say there is an issue of SST/MAT coming in from ships/buoys in the Arctic. HadCRUT3

(really HadSST2) doesn't use these where there isn't a 61-90 climatology - a lot of areas

with sea ice in most/some years in the base period. Using fixed SST values of -1.8C is

possible for months with sea ice, but is likely to be wrong. MAT would be impossible to

develop 61-90 climatologies for when sea ice was there. This is an issue that will have to

addressed at some point as the sea ice disappears. Maybe we could develop possible

approaches using some AMIP type Arctic RCM simulations?

Agreeing on the ranks is the hardest of all measures. Uncertainties in global averages

are of the order of +/- 0.05 for one sigma, so any difference between years of less than

0.1

isn't significant. We (MOHC/CRU) put annual values in press releases, but we also put

errors. UK newspapers quote these, and the journalists realise about uncertainties, but

prefer

to use the word accuracy.

We only make the press releases to get the numbers out at one time, and focus

all the calls. We do this through WMO, who want the release in mid-Dec.

There is absolutely no sense of duelling in this. We would be criticised if there

were just

one analysis. The science is pushing for multiple analyses of the same measure - partly

to make sure people remember RSS and not just believe UAH. As we all know,

NOAA/NASA

and HadCRUT3 are all much closer than RSS and UAH!

I know we all know all the above. I try to address this when talking to journalists, but

they generally ignore this level of detail.

I'll be in Boulder the week after next at the IDAG meeting (Jan 28-30) and another

meeting Jan30/Feb 1. Tom will be also.

Cheers

Phil

At 02:12 18/01/2008, Kevin Trenberth wrote:

FYI

See the discussion below. Looks like clarification is called for when these statements

are made that consider the other announcements.

Kevin

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

Subject: RE: Dueling climates

Date: Thu, 17 Jan 2008 18:51:13 -0500

From: Ryan, Bob (NBC Universal) [2]<Bob.Ryan@nbcuni.com>

To: Kevin Trenberth [3]<trenbert@ucar.edu>, [4]<anthes@ucar.edu>

CC: [5]<kseitter@ametsoc.org>

References:

[6]<7C368A942599A944A0C43774DE6412EE044C9964@DCNMLVEM01.e2k.ad.ge.com>

[7]<478F89E4.10405@ucar.edu> [8]<478FBF64.1020500@ucar.edu>

mail.2008

Rick, Kevin,
Attached is the NOAA release. I believe I had read that the discrepancy with the NASA ("Second hottest year") data/release was also related to how NOAA adjusts for heat island effects and resiting of climate stations. In any event I don't think climate data serves the broad goals of informing/educating the public and decision makers about climate change. I can hear some saying, "If NOAA and NASA can't even agree what the temperature was last year, how can we believe what they are saying about the future climate".
Bob

From: Kevin Trenberth [[9]mailto:trenbert@ucar.edu]
Sent: Thursday, January 17, 2008 3:50 PM
To: [10]anthes@ucar.edu
Cc: Ryan, Bob (NBC Universal); [11]kseitter@ametsoc.org
Subject: Re: Dueling climates
Hi Rick

My understanding is that the biggest source of this discrepancy is the way the Arctic is analyzed. We know that the sea ice was at record low values, 22% lower than the previous low in 2005. Some sea temperatures and air temperatures were as much as 7C above normal. But most places there is no conventional data. In NASA they extrapolate and build in the high temperatures in the Arctic. In the other records they do not. They use only the data available and the rest is missing. In most cases the values from recent years are about statistically tied and the ranking is one that separates values by hundredths of a degree. There is no correct way to do this (especially the treatment of missing data), and different groups do it differently. You typically get different answers if you compute the hemispheric means and average them vs computing the global mean, because more data are missing in the southern hemisphere. Although this can be addressed using remote sensing in recent times, the climatologies differ. Ideally one should have a global analysis with no missing data, and this occurs in the global analyses, but they have other problems.

Hope this helps
Kevin
Rick Anthes wrote:

Bob-
I saw the NASA one (GISS) but not the NOAA release. Could you point me toward
Page 84

mail.2008

it?

I see your point. These preliminary analyses may change with time and the press releases have not been peer-reviewed. I am surprised the two estimates disagree this much, but the difference is probably well within the uncertainty of the estimate of annual global temperatures. I'd be interested in Kevin's take on this.

Rick
Ryan, Bob (NBC Universal) wrote:

Rick, Keith,
Don't know if this will come up in the Council or if there is time to even discuss but I'm sure you've seen the NOAA/NASA press releases and the news stories about the 2007 global temperatures. NASA says tied for "2nd hottest". . . NOAA says 5th warmest global and only 10th in US. who does this serve but create confusion and add to the skeptics/denialists argument. . ."They can't even agree on last year's temperatures. why should we believe them?"
Science by press release doesn't serve anyone and certainly not a curious public.
Role for the AMS?
See you soon.

Bob
Subject:
NASA SCIENTISTS RELEASE 2007 TEMPERATURE DATA
From:
"Maria Frostic" [12]<mfrostic@pop100.gsfc.nasa.gov>
Date:
Tue, 15 Jan 2008 18:26:13 -0500
To:
"Maria Frostic" [13]<mfrostic@pop100.gsfc.nasa.gov>
To:
"Maria Frostic" [14]<mfrostic@pop100.gsfc.nasa.gov>

Maria Frostic

1/15/08
(301) 286-9017
2007 Among Hottest Years on Record:
NASA Scientists Release Global Temperature Analysis
An analysis of 2007 global temperature data undertaken by scientists at Goddard Institute for Space Studies (GISS), New York, reveals that 2007 is

tied with 1998 as the second hottest year on record. The unusual warmth of 2007 is noteworthy because it occurs at a time when solar irradiance is at a minimum and the equatorial Pacific Ocean has entered the cool phase of its

El Niño-La Niña cycle.
The greatest warming in 2007 occurred in the Arctic. Global warming has a

mail.2008

larger affect in polar areas, as the loss of snow and ice leads to more open water, which absorbs more sunlight and warmth. The large Arctic warm anomaly of 2007 is consistent with observations of record low Arctic sea ice in September 2007. The eight warmest years in the GISS record have all occurred since 1998, with 2005 ranking as the hottest. Barring a large volcanic eruption, NASA scientists predict that a record global temperature exceeding that of 2005 can be expected within the next two to three years. A NASA TV Video File on this topic will run January 16th at 9 A.M., 12, 4, 8, and 10 P.M. EDT on the NASA TV media channel (#103).
Video Highlights:

- * Colorful Visualizations of Global Temperature Data from 1880-2007
- * Animations of Unique Perspectives on Ice Albedo
- * Animated Earth Displaying Seasonal Landcover and Arctic Sea Ice
- * Select Interview Clips with NASA Scientist Dr. James Hansen

For high definition video downloads, print resolution still images, and a short web video on taking Earth's temperature, visit:

[15]http://www.nasa.gov/topics/earth/features/earth_temp.html

NASA Television is carried on an MPEG-2 digital signal accessed via satellite AMC-6, at 72 degrees west longitude, transponder 17C, 4040 MHz, vertical polarization. A Digital Video Broadcast (DVB) - compliant Integrated Receiver Decoder (IRD) with modulation of QPSK/DBV, data rate of 36.86 and FEC =< is needed for reception. NASA TV Multichannel Broadcast includes Public Services Channel (#101), the Education Channel (#102) and the Media Services Channel (#103).
For NASA TV information and schedules on the web, visit: [16]www.nasa.gov/ntv

Subject:

NOAA: 2007 Was Tenth Warmest for U.S., Fifth Warmest Worldwide

From:

"NOAA News Releases" [17]<Press.Releases@noaa.gov>

Date:

Tue, 15 Jan 2008 15:00:00 -0500

To:

"Ryan, Bob (NBC Universal)" [18]<bob.ryan@nbc.com>

To:

"Ryan, Bob (NBC Universal)" [19]<bob.ryan@nbc.com>

TO: Ryan, Bob; WRC-TV

FOR IMMEDIATE RELEASE January 15, 2008

*** NEWS FROM NOAA ***

NATIONAL OCEANIC & ATMOSPHERIC ADMINISTRATION

U. S. DEPARTMENT OF COMMERCE

WASHINGTON, DC

Contact: John Leslie, 301-713-2087, ext. 174

NOAA: 2007 Was Tenth Warmest for U.S., Fifth Warmest Worldwide

The average temperature for the contiguous U.S. in 2007 is officially the tenth warmest on record, according to data from scientists at

NOAA's National Climatic Data Center in

mail.2008

Asheville, N.C. The agency also determined the global surface temperature last year was the fifth warmest on record.

U.S. Temperature Highlights

- * The average U.S. temperature for 2007 was 54.2 degrees F; 1.4 degrees F warmer than the 20th century mean of 52.8 degrees F. NCDC originally estimated in mid-December that 2007 would end as the eighth warmest on record, but below-average temperatures in areas of the country last month lowered the annual ranking. For Alaska, 2007 was the 15th warmest year since statewide records began in 1918.
- * Six of the 10 warmest years on record for the contiguous U.S. have occurred since 1998, part of a three decade period in which mean temperatures for the contiguous U.S. have risen at a rate near 0.6 degrees F per decade.
- * For the contiguous U.S., the December 2007 mean temperature was 33.6 degrees F, near the 20th century average of 33.4 degrees F. The Southeast was much warmer than average, while 11 states, from the Upper Midwest to the West Coast, were cooler than average.
- * Warmer-than-average temperatures for December 2007 in large parts of the more heavily populated eastern U.S. resulted in temperature related energy demand about 1.9 percent below average for

the nation as a whole, based on NOAA's

Residential Energy Demand Temperature Index. For the year, the REDTI estimates that national residential energy consumption was about 2.5 percent below average.

U.S. Precipitation Highlights December 2007

- * December 2007 was wetter than normal for the contiguous U.S., the 18th wettest December since national records began in 1895. Thirty-seven states were wetter, or much wetter, than average. Only Texas, Louisiana, Mississippi, and North Dakota were drier than average.
- * Precipitation was much above average in Washington state, due to a powerful storm that struck the Pacific Northwest in early December. Heavy rain and wind gusts greater than 100 mph caused widespread damage and the worst flooding in more than a decade in parts of western Oregon and Washington. Many locations received more than 10 inches of rainfall during the first three days of the month.
- * While above-average precipitation in late November and December led to improving drought conditions in parts of the Southwest, Southeast, and New England, more than three-fourths of the Southeast and half of the West remained in some stage of drought.

Global Highlights

- * For December 2007, the combined global land and ocean surface temperature was the 13th warmest on record (0.72 degrees F or 0.40 degrees C above the 20th century mean). Separately, the global December land-surface temperature was the eighth warmest on record. The most anomalously warm temperatures occurred from Scandinavia to central Asia.

- * La Niña continued to strengthen as ocean

surface temperatures in large areas of the central and eastern equatorial Pacific were more than 3 degrees F (1.7 degrees C) below average.

mail.2008

The continuation of cooler-than-average temperatures dampened the global ocean average, which was the 18th warmest on record for December.

* For 2007, the global land and ocean surface temperature was the fifth warmest on record. Separately, the global land surface temperature was warmest on record while the global ocean temperature was 9th warmest since records began in 1880. Seven of the eight warmest years on record have occurred since 2001, part of a rise in temperatures of more than 1 degree F (0.6 degrees C) since 1900. Within the past three decades, the rate of warming in global temperatures has been approximately three times greater than the century scale trend.

Note to Editors: Additional information on U.S. climate conditions in December and for 2007 is available online at:

[20]<http://www.ncdc.noaa.gov/oa/climate/research/2007/dec/dec07.html> and [21]<http://www.ncdc.noaa.gov/oa/climate/research/2007/ann/ann07.html>.

- 30 -

--

Dr. Richard A. Anthes
Phone: 303-497-1652

President
University Corporation for Atmospheric Research
P.O. Box 3000
Boulder, CO 80307-3000

For delivery via express mail, please use:

1850 Table Mesa Drive
Boulder, CO 80305

--

Kevin E. Trenberth
e-mail: [22]trenbert@ucar.edu
Climate Analysis
Section,
[23]
www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box
3000,
(303) 497 1318
Boulder, CO
80307
(303) 497 1333 (fax)

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

--

Kevin E. Trenberth

mail.2008

e-mail:

[24]trenbert@ucar.edu
Climate Analysis Section,
NCAR

[25]www.cgd.ucar.edu/cas/
P. O. Box
3000,
(303) 497 1318
Boulder, CO
80307
(303) 497 1333 (fax)

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

X-MimeOLE: Produced By Microsoft Exchange V6.5
Received: from [26]rkfmlef01.e2k.ad.ge.com ([[27]3.159.183.51]) by
[28]DCNMLVEM01.e2k.ad.ge.com with Microsoft SMTPSVC(6.0.3790.2499); Tue, 15 Jan

2008

14:59:24 -0500

MIME-Version: 1.0

Content-Type: multipart/alternative;

boundary="----_=_NextPart_003_01C857B1.23BF5550"

Received: from [29]useclpexw213.nbcuni.ge.com ([[30]3.44.150.24]) by
[31]rkfmlef01.e2k.ad.ge.com with Microsoft SMTPSVC(6.0.3790.2499); Tue, 15 Jan

2008

14:59:24 -0500

Received: from [32]int-ch1gw-3.online-age.net ([[33]3.159.232.67]) by

[34]useclpexw213.nbcuni.ge.com (SonicWALL 6.0.1.9157) with ESMTTP; Tue, 15 Jan

2008

14:59:24 -0500

Received: from [35]ext-ch1gw-9.online-age.net (int-ch1gw-3 [[36]3.159.232.67])

by

[37]int-ch1gw-3.online-age.net (8.13.6/8.13.6/20050510-SVVS) with ESMTTP id
m0FJxNgI021683 for <[38]bob.ryan@nbc.com>; Tue, 15 Jan 2008 14:59:23 -0500

(EST)

Received: from [39]mmp2.nems.noaa.gov ([40]mmp2.nems.noaa.gov

[[41]140.90.121.157]) by

[42]ext-ch1gw-9.online-age.net (8.13.6/8.13.6/20051111-SVVS-TLS-DNSBL) with

ESMTTP id

m0FJxKss007414 for <[43]bob.ryan@nbc.com>; Tue, 15 Jan 2008 14:59:23 -0500

Received: from [44]HCHB-WIRNS.noaa.gov ([[45]170.110.255.148]) by

[46]mmp2.nems.noaa.gov (Sun Java System Messaging Server 6.2-6.01 (built Apr 3
2006))

with ESMTTPSA id <[47]0JUP00MVJBIAQ7B0@mmp2.nems.noaa.gov> for

[48]bob.ryan@nbc.com; Tue,

15 Jan 2008 14:59:16 -0500 (EST)

Content-class: urn:content-classes:message

Subject: NOAA: 2007 was Tenth warmest for U.S., Fifth warmest worldwide

Date: Tue, 15 Jan 2008 15:00:00 -0500

Message-ID: <[49]0JUP00MZVBISQ7B0@mmp2.nems.noaa.gov>

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: NOAA: 2007 was Tenth warmest for U.S., Fifth warmest worldwide

Thread-Index: AchXSSO/aYafvboCRgCNpqPHISHPg==

From: "NOAA News Releases" <[50]Press.Releases@noaa.gov>

To: "Ryan, Bob (NBC Universal)" <[51]Bob.Ryan@nbcuni.com>

mail.2008

TO: Ryan, Bob; WRC-TV
FOR IMMEDIATE RELEASE January 15, 2008
*** NEWS FROM NOAA ***

NATIONAL OCEANIC & ATMOSPHERIC ADMINISTRATION
U. S. DEPARTMENT OF COMMERCE
WASHINGTON, DC

Contact: John Leslie, 301-713-2087, ext. 174
NOAA: 2007 was Tenth warmest for U.S., Fifth warmest worldwide

The average temperature for the contiguous U.S. in 2007 is officially the tenth warmest on record, according to data from scientists at NOAA's National Climatic Data Center in Asheville, N.C. The agency also determined the global surface temperature last year was the fifth warmest on record.

U.S. Temperature Highlights

* The average U.S. temperature for 2007 was 54.2 degrees F; 1.4 degrees F warmer than the 20th century mean of 52.8 degrees F. NCDC originally estimated in mid-December that 2007 would end as the eighth warmest on record, but below-average temperatures in areas of the country last month lowered the annual ranking. For Alaska, 2007 was the 15th warmest year since statewide records began in 1918.

* Six of the 10 warmest years on record for the contiguous U.S. have occurred since 1998, part of a three decade period in which mean temperatures for the contiguous U.S. have risen at a rate near 0.6 degrees F per decade.

* For the contiguous U.S., the December 2007 mean temperature was 33.6 degrees F, near the 20th century average of 33.4 degrees F. The Southeast was much warmer than average, while 11 states, from the Upper Midwest to the West Coast, were cooler than average.

* Warmer-than-average temperatures for December 2007 in large parts of the more heavily populated eastern U.S. resulted in temperature related energy demand about 1.9 percent below average for the nation as a whole, based on NOAA's Residential Energy Demand Temperature Index. For the year, the REDTI estimates that national residential energy consumption was about 2.5 percent below average.

U.S. Precipitation Highlights December 2007

* December 2007 was wetter than normal for the contiguous U.S., the 18th wettest December since national records began in 1895. Thirty-seven states were wetter, or much wetter, than average. Only Texas, Louisiana, Mississippi, and North Dakota were drier than average.

* Precipitation was much above average in Washington state, due to a powerful storm that struck the Pacific Northwest in early December. Heavy rain and wind gusts greater than 100 mph caused widespread damage and the worst flooding in more than a decade in parts of western Oregon and Washington. Many locations received more than 10 inches of rainfall during the first three days of the month.

* While above-average precipitation in late November and December led to improving drought conditions in parts of the Southwest, Southeast, and New England, more than three-fourths of the Southeast and half of the West remained in some stage of drought.

Global Highlights

* For December 2007, the combined global land and ocean surface temperature was the 13th warmest on record (0.72 degrees F or 0.40 degrees C above

mail.2008

the 20th century mean). Separately, the global December land-surface temperature was the eighth warmest on record. The most anomalously warm temperatures occurred from Scandinavia to central Asia.

* La Niña continued to strengthen as ocean surface temperatures in large areas of the central and eastern equatorial Pacific were more than 3 degrees F (1.7 degrees C) below average. The continuation of cooler-than-average temperatures dampened the global ocean average, which was the 18th warmest on record for December.

* For 2007, the global land and ocean surface temperature was the fifth warmest on record. Separately, the global land surface temperature was warmest on record while the global ocean temperature was 9th warmest since records began in 1880. Seven of the eight warmest years on record have occurred since 2001, part of a rise in temperatures of more than 1 degree F (0.6 degrees C) since 1900. Within the past three decades, the rate of warming in global temperatures has been approximately three times greater than the century scale trend.

Note to Editors: Additional information on U.S. climate conditions in December and for 2007 is available online at:

[52]<http://www.ncdc.noaa.gov/oa/climate/research/2007/dec/dec07.html> and [53]<http://www.ncdc.noaa.gov/oa/climate/research/2007/ann/ann07.html> .

- 30 -

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

References

1. <mailto:p.jones@uea.ac.uk>
2. <mailto:Bob.Ryan@nbcuni.com>
3. <mailto:trenbert@ucar.edu>
4. <mailto:anthes@ucar.edu>
5. <mailto:kseitter@ametsoc.org>
6. <mailto:7C368A942599A944A0C43774DE6412EE044C9964@DCNMLVEM01.e2k.ad.ge.com>
7. <mailto:478F89E4.10405@ucar.edu>
8. <mailto:478FBF64.1020500@ucar.edu>
9. <mailto:trenbert@ucar.edu>
10. <mailto:anthes@ucar.edu>
11. <mailto:kseitter@ametsoc.org>
12. <mailto:mfrostic@pop100.gsfc.nasa.gov>
13. <mailto:mfrostic@pop100.gsfc.nasa.gov>
14. <mailto:mfrostic@pop100.gsfc.nasa.gov>
15. http://www.nasa.gov/topics/earth/features/earth_temp.html
16. <http://www.nasa.gov/ntv>
17. <mailto:Press.Releases@noaa.gov>
18. <mailto:bob.ryan@nbc.com>
19. <mailto:bob.ryan@nbc.com>
20. <http://www.ncdc.noaa.gov/oa/climate/research/2007/dec/dec07.html>
21. <http://www.ncdc.noaa.gov/oa/climate/research/2007/ann/ann07.html>
22. <mailto:trenbert@ucar.edu>

mail.2008

- 23. <http://www.cgd.ucar.edu/cas/trenbert.html>
- 24. <mailto:trenbert@ucar.edu>
- 25. <http://www.cgd.ucar.edu/cas/>
- 26. <http://rkfmlef01.e2k.ad.ge.com/>
- 27. <http://3.159.183.51/>
- 28. <http://DCNMLVEM01.e2k.ad.ge.com/>
- 29. <http://useclpexw213.nbcuni.ge.com/>
- 30. <http://3.44.150.24/>
- 31. <http://rkfmlef01.e2k.ad.ge.com/>
- 32. <http://int-chlgw-3.online-age.net/>
- 33. <http://3.159.232.67/>
- 34. <http://useclpexw213.nbcuni.ge.com/>
- 35. <http://ext-chlgw-9.online-age.net/>
- 36. <http://3.159.232.67/>
- 37. <http://int-chlgw-3.online-age.net/>
- 38. <mailto:bob.ryan@nbc.com>
- 39. <http://mmp2.nems.noaa.gov/>
- 40. <http://mmp2.nems.noaa.gov/>
- 41. <http://140.90.121.157/>
- 42. <http://ext-chlgw-9.online-age.net/>
- 43. <mailto:bob.ryan@nbc.com>
- 44. <http://HCHB-WIRNS.noaa.gov/>
- 45. <http://170.110.255.148/>
- 46. <http://mmp2.nems.noaa.gov/>
- 47. <mailto:0JUP00MVJBIAQ7B0@mmp2.nems.noaa.gov>
- 48. <mailto:bob.ryan@nbc.com>
- 49. <mailto:0JUP00MZVBISQ7B0@mmp2.nems.noaa.gov>
- 50. <mailto:Press.Releases@noaa.gov>
- 51. <mailto:Bob.Ryan@nbcuni.com>
- 52. <http://www.ncdc.noaa.gov/oa/climate/research/2007/dec/dec07.html>
- 53. <http://www.ncdc.noaa.gov/oa/climate/research/2007/ann/ann07.html>
- 54. <mailto:p.jones@uea.ac.uk>

859. 1201561936.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
 To: Jean Jouzel <jean.jouzel@lsce.ipsl.fr>
 Subject: Re: [Fwd: EGU 2008]
 Date: Mon, 28 Jan 2008 18:12:16 -0500
 Reply-to: mann@psu.edu
 Cc: Phil Jones <p.jones@uea.ac.uk>

Hi Jean,
 no problem, I think Phil and I have it all sorted out. Sorry I won't be there to
 see you
 this time,
 mike
 Jean Jouzel wrote:

Dear Phil, Dear Mike,

I feel that I come too late in the discussion, but it's really fine for me.

Thanks a lot Jean

At 14:24 +0000 18/01/08, Phil Jones wrote:

Mike,
 I didn't read it properly! I see the Jan 25 deadline. I was looking
 at a Feb date which is for room and scheduling options.

mail.2008

So I will let you enter the session on Monday. I'll send something over the weekend or first thing Monday, once I've been through them. There a number of issues which relate to last year and who got orals/posters then.

The other thing is for a room for 250+ people. If we have a medallist we want more. We had 500 last year (due to Ray) but we did keep most for the next few talks. We still had about ~200 for the session after Ray's.

Cheers
Phil

At 14:01 18/01/2008, Michael Mann wrote:

Hi Phil,
thanks--sounds fine, I'll let you enter the session then.
I thought they wanted it sooner though (before Jan 25). I'm forwarding that email, maybe
I misunderstood it,
mike
Phil Jones wrote:

Mike,
Have printed out the abstracts. Looks like many reasonable ones. Pity we only have the limited numbers. I can put the session in once we're agreed. It seems as though we can't do that till mod-Feb.
I've contacted Gerrit and Gerard to see if we have to accommodate a medalist talk for the Hans Oeschger prize.
Cheers
Phil

At 13:15 18/01/2008, Michael Mann wrote:

Hi Phil,
thanks, that sounds fine to me. I'll await further word from you after you look this over again, and I'll await feedback from Jean. No rush, I'm hoping to finalize the session on Monday.
The Vinther et al stuff sounds very interesting--I'm looking forward to hearing more,
sorry I won't actually be at EGU.
talk to you later,
mike
Phil Jones wrote:

Mike, Jean
Thanks. I'll probably go with Vinther et al for the third invited. Not just as I'm on the author list, but because he'll show (will submit soon) that the Greenland borehole records (Dorthe Dahl Jensen) are winter proxies. Has implications for the Norse Vikings - as the summer isotopes (which unfortunately respond much to Icelandic than SW Greenland temps) don't show any Medieval warming.
Jean probably knew all this. The bottom line is that annual isotopes are essentially winter isotopes as they vary 2-3 times as much as summer ones. If the squeezing of the layers doesn't distort anything this implies longer series
are very winter half year dominant.

I mostly agree with the other orals, but I have to look at a few. There is one on the Millennium project (EU funded) which Jean knows about. Might have to give this an oral slot.

mail.2008

Jean - any thoughts? I assume you're happy to chair a session.
I also need to check whether we will have to talk a medallist talk? No
idea who?

Cheers

Phil

At 17:05 17/01/2008, Michael Mann wrote:

Content-Type: text/html; charset=ISO-8859-15

X-MIME-Autoconverted: from 8bit to quoted-printable by f05n05.cac.psu.edu id
m0HH5gQ6025372

Dear Phil and Jean,

We got an impressive turnout this year for our session, 37 total submitted
abstracts.

Please see attached word document. Based on the rules described by EGU below, I
suggest

we have 2 oral sessions (consisting of morning and afternoon), with a total of
10 oral

presentations w/ 7 of those being regular 15 minutes slots and 3 of those
invited 25

minute slots. The other 27 abstracts will be posters, conforming w/ the fairly
harsh

limits imposed by EGU on oral presentations.

My suggestions would be as follow:

Invited Presentations (25 minutes):

1 Ammann et al

2 Hughes et al

3 either Emile Geay et al OR Vinther et al OR Crespín et al (preferences?)

Other Oral (15 minutes):

4. 3 other of either Emile Geay et al OR Vinther et al OR Crespín et al

5. 3 other of either Emile Geay et al OR Vinther et al OR Crespín et al

6. Riedwyl et al

7. Graham et al

8. Smerdon et al

9. Kleinen et al

10. Jungklaus et al

Posters:

All others

Please let me know what you think. If these sound good to you, I'll go ahead
and arrange

the session online,

Mike

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

Subject: EGU 2008

Date: Thu, 17 Jan 2008 10:03:43 +0100

From: Andrea Bleyer [1]<Andrea.Bleyer@awi.de>

To: [2]Denis.Rousseau@lmd.ens.fr, [3]thomas.wagner@ncl.ac.uk,

[4]f.doblas-reyes@ecmwf.int, [5]tilmes@ucar.edu, [6]p.wadhams@damtp.cam.ac.uk,

[7]jbstuut@marum.de, [8]harz@gfz-potsdam.de, [9]w.hoek@geo.uu.nl, Johann

Jungklaus

[10]<johann.jungklaus@zmaw.de>, Heiko Paeth

[11]<heiko.paeth@mail.uni-wuerzburg.de>,

[12]piero.lionello@unile.it, [13]boc@dmi.dk, [14]helge.dränge@nersc.no,

[15]chris.d.jones@metoffice.com, [16]martin.claussen@zmaw.de,

[17]gottfried.kirchengast@uni-graz.at, [18]matthew.collins@metoffice.gov.uk,

[19]martin.beniston@unige.ch, [20]d.stainforth@physics.ox.ac.uk,

[21]rwarritt@bruce.agron.iastate.edu, Seneviratne Sonia Isabelle

[22]<sonia.seneviratne@env.ethz.ch>, Wild Martin [23]<martin.wild@env.ethz.ch>,

Nanne

weber [24]<weber@knmi.nl>, [25]Hubertus.Fischer@awi.de,

[26]rahmstorf@pik-potsdam.de,

[27]azakey@ictp.it, [28]mann@psu.edu, [29]steig@u.washington.edu,

[30]nalan.koc@npolar.no, [31]florindo@ingv.it, [32]ggd@aber.ac.uk,

[33]oromero@ugr.es,
[34]v.rath@geophysik.rwth-aachen.de, [35]awinguth@uta.edu,
[36]l.haass@mx.uni-saarland.de, [37]Gilles.Ramstein@cea.fr, Andre Paul
[38]<apau@palmod.uni-bremen.de>, [39]lucarini@adgb.df.unibo.it, Martin Trauth
[40]<trauth@geo.uni-potsdam.de>, [41]nathalie.fagel@ulg.ac.be,
[42]hans.renszen@geo.falw.vu.nl, [43]Xiaolan.wang@ec.gc.ca,
[44]Marie-Alexandrine.Sicre@lsce.cnrs-gif.fr, alessandra negri
[45]<a.negri@univpm.it>,
[46]ferretti@unimore.it, [47]Mark.Liniger@meteoswiss.ch, Geert Jan van
Oldenborgh
[48]<oldenborgh@knmi.nl>, [49]pjr@ucar.edu, [50]keith@ucalgary.ca,
[51]piacsek@nr1ssc.navy.mil, [52]kiefer@pages.unibe.ch,
[53]hatte@lsce.cnrs-gif.fr,
[54]peter.kershaw@arts.monash.edu.au, [55]icacho@ub.edu,
[56]kiefer@pages.unibe.ch,
Thomas Felis [57]<tfelis@uni-bremen.de>, [58]olander@gfy.ku.dk,
[59]karenluise.knudsen@geo.au.dk, [60]aku@geus.dk,
[61]Marie-Alexandrine.Sicre@lsce.cnrs-gif.fr, [62]reichart@geo.uu.nl,
[63]M.N.Tsimplis@soton.ac.uk, [64]c.goodess@uea.ac.uk,
[65]r.sutton@reading.ac.uk,
[66]valexeev@iarc.uaf.edu, [67]victor.brovkin@pik-potsdam.de,
[68]zeng@atmos.umd.edu,
[69]terray@cerfacs.fr, [70]dufresne@lmd.jussieu.fr,
[71]Burkhardt.Rockel@gkss.de,
[72]hurkvd@knmi.nl, [73]philippe.ciais@lsce.ipsl.fr,
[74]rolf.philipona@meteoswiss.ch,
[75]Masa.Kageyama@lsce.ipsl.fr, [76]jules@jamstec.go.jp, [77]ewwo@bas.ac.uk,
[78]raynaud@lgge.obs.ujf-grenoble.fr, [79]omarchal@who1.edu,
[80]claire.waelbroeck@lsce.cnrs-gif.fr, Phil Jones [81]<p.jones@uea.ac.uk>,
[82]jouzel@dsm-mail.saclay.cea.fr, [83]Jeff.Blackford@Manchester.ac.uk,
[84]gerardv@nioz.nl, [85]dharwood1@un1.edu, [86]lang@liv.ac.uk, Irka Hajdas
[87]<hajdas@phys.ethz.ch>, [88]x.crosta@epoc.u-bordeaux1.fr,
[89]pascal.claquin@unicaen.fr, Gonzalez-Rouco [90]<fidelgr@fis.ucm.es>,
[91]jsa@ig.cas.cz, [92]dankd@atmos.umd.edu, [93]kbice@who1.edu, "Brinkhuis, dr.
H.
(Henk)" [94]<H.Brinkhuis@bio.uu.nl>, [95]andy@seao2.org, [96]kbillups@udel.edu,
[97]anita.roth@uni-tuebingen.de, Gerrit Lohmann [98]<Gerrit.Lohmann@awi.de>,
[99]P.J.Valdes@bristol.ac.uk, [100]strecker@geo.uni-potsdam.de,
[101]mmaslin@geog.ucl.ac.uk, [102]marie-france.loutre@uclouvain.be,
[103]aurelia.ferrari@oma.be, [104]j.bamber@bristol.ac.uk, Torsten Bickert
[105]<bickert@rcom-bremen.de>, [106]chris.d.jones@metoffice.gov.uk,
[107]elsa.cortijo@lsce.ipsl.fr, [108]gerald.ganssen@falw.vu.nl,
[109]arne.richter@copernicus.org, Andrea Bleyer [110]<Andrea.Bleyer@awi.de>,
"Amelung B
(ICIS)" [111]<B.Amelung@ICIS.unimaas.nl>, [112]spn@env.ethz.ch,
[113]bgomez@ub.edu,
[114]wmson@ucar.edu, [115]d.vance@bristol.ac.uk

Dear convener and co-convener,

Thanks a lot for your effort for successful sessions at the EGU 2008.
>From our experience of the last years, there will be an
oral-to-poster ratio of about 1:2 (e.g. ~33% of the contributions can
get

a talk). This means that for a complete session, you need 18
contributions. 18:3 * 15min = 1.5h = 1 block

For those of you who are under the number of 18, there are several
options:

- 1) a pure poster session
- 2) merging with a related session
- 3) the contributions will go to the open session (CL0)
- 4) if you are just below 18, you may manage to get late contributions
within the next days (please no dummy posters)

Please tell me which option do you like most (email to

mail.2008

[116]andrea.bleyer@awi.de).
In case 2), please contact the respective conveners in advance.
The session could be also from other divisions (BG, OS, AS, IS, ..).
In case of merging, you may speak with the persons whether it would be appropriate to modify the title of the new session or to have a combined name with both titles.
I think the general rule is that the convener of the merged session is the person with the bigger session.
Kind regards
Gerrit

Prof. Dr. Gerrit Lohmann
Alfred Wegener Institute for Polar and Marine Research
Bussestr. 24
D-27570 Bremerhaven
Germany
Email: [117]Gerrit.Lohmann@awi.de
Telephone: +49(471)4831-1758 / 1760
Fax: +49(471)4831-1797
[118]<http://www.awi-bremerhaven.de/CurriculumVitae/glohmann.html>
[119]<http://www.awi.de/en/go/paleo>

--

Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)
Department of
Meteorology
Phone: (814) 863-4075
503 Walker
Building
FAX: (814) 865-3663
The Pennsylvania State University
email: [120]mann@psu.edu
University Park, PA 16802-5013
[121]<http://www.met.psu.edu/dept/faculty/mann.htm>

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email [122]p.jones@uea.ac.uk
NR4 7TJ
UK

--

Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)
Department of
Meteorology
Phone: (814) 863-4075
503 Walker
Building
FAX: (814) 865-3663
The Pennsylvania State University
email: [123]mann@psu.edu
University Park, PA 16802-5013
[124]<http://www.met.psu.edu/dept/faculty/mann.htm>

mail.2008

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

--
Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)
Department of
Meteorology
Phone: (814) 863-4075
503 Walker
Building
FAX: (814) 865-3663
The Pennsylvania State University
email: [126]mann@psu.edu
University Park, PA 16802-5013
[127]
<http://www.met.psu.edu/dept/faculty/mann.htm>

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email [128]p.jones@uea.ac.uk
NR4 7TJ
UK

--

Attention new mail address : [129]jean.jouzel@lsce.ipsl.fr
Directeur de l'Institut Pierre Simon Laplace, Université de Versailles
Saint-Quentin
Bâtiment d'Alembert, 5 Boulevard d'Alembert, 78280 Guyancourt, FRANCE
tél : 33 (0) 1 39 25 58 16, fax : 33 (0) 1 39 25 58 22, Portable : 33 (0)
684759682
- Université Pierre et Marie Curie, Tour 45-46, 3ème étage, 303, 4 Place
Jussieu,
75252 Paris Cedex 05, e-mail : [130]jzipsl@ipsl.jussieu.fr, 01 44 27 49 92
- Laboratoire des Sciences du Climat et de l'Environnement, UMR CEA-CNRS-UVSQ
CE Saclay, Orme des Merisiers, 91191 Gif sur Yvette, tél : 33 (0) 1 69 08 77 13,
fax : 33 (0) 1 69 08 77 16, Bt 701, Pièce 9a, e-mail :
[131]jean.jouzel@lsce.ipsl.fr

--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [132]mann@psu.edu
University Park, PA 16802-5013

[133]<http://www.met.psu.edu/dept/faculty/mann.htm>
Page 97

References

1. <mailto:Andrea.Bleyer@awi.de>
2. <mailto:Denis.Rousseau@lmd.ens.fr>
3. <mailto:thomas.wagner@ncl.ac.uk>
4. <mailto:f.doblas-reyes@ecmwf.int>
5. <mailto:tilmes@ucar.edu>
6. <mailto:p.wadhams@damtp.cam.ac.uk>
7. <mailto:jbstuut@marum.de>
8. <mailto:harz@gfz-potsdam.de>
9. <mailto:w.hoek@geo.uu.nl>
10. <mailto:johann.jungclaus@zmaw.de>
11. <mailto:heiko.paeth@mail.uni-wuerzburg.de>
12. <mailto:piero.lionello@unile.it>
13. <mailto:boc@dmi.dk>
14. <mailto:helge.drange@nersc.no>
15. <mailto:chris.d.jones@metoffice.com>
16. <mailto:martin.claussen@zmaw.de>
17. <mailto:gottfried.kirchengast@uni-graz.at>
18. <mailto:matthew.collins@metoffice.gov.uk>
19. <mailto:martin.beniston@unige.ch>
20. <mailto:d.stainforth1@physics.ox.ac.uk>
21. <mailto:rwarritt@bruce.agron.iastate.edu>
22. <mailto:sonia.seneviratne@env.ethz.ch>
23. <mailto:martin.wild@env.ethz.ch>
24. <mailto:weber@knmi.nl>
25. <mailto:Hubertus.Fischer@awi.de>
26. <mailto:rahmstorf@pik-potsdam.de>
27. <mailto:azakey@ictp.it>
28. <mailto:mann@psu.edu>
29. <mailto:steig@u.washington.edu>
30. <mailto:nalan.koc@npolar.no>
31. <mailto:florindo@ingv.it>
32. <mailto:ggd@aber.ac.uk>
33. <mailto:oromero@ugr.es>
34. <mailto:v.rath@geophysik.rwth-aachen.de>
35. <mailto:awinguth@uta.edu>
36. <mailto:l.haass@mx.uni-saarland.de>
37. <mailto:Gilles.Ramstein@cea.fr>
38. <mailto:apau@palmod.uni-bremen.de>
39. <mailto:lucarini@adgb.df.unibo.it>
40. <mailto:trauth@geo.uni-potsdam.de>
41. <mailto:nathalie.fagel@ulg.ac.be>
42. <mailto:hans.rensen@geo.falw.vu.nl>
43. <mailto:Xiaolan.Wang@ec.gc.ca>
44. <mailto:Marie-Alexandrine.Sicre@lsce.cnrs-gif.fr>
45. <mailto:a.negri@univpm.it>
46. <mailto:ferretti@unimore.it>
47. <mailto:Mark.Liniger@meteoswiss.ch>
48. <mailto:oldenborgh@knmi.nl>
49. <mailto:pjr@ucar.edu>
50. <mailto:keith@ucalgary.ca>
51. <mailto:piacsek@nr1ssc.navy.mil>
52. <mailto:kiefer@pages.unibe.ch>
53. <mailto:hatte@lsce.cnrs-gif.fr>
54. <mailto:peter.kershaw@arts.monash.edu.au>
55. <mailto:icacho@ub.edu>
56. <mailto:kiefer@pages.unibe.ch>
57. <mailto:tfelis@uni-bremen.de>
58. <mailto:olander@gfy.ku.dk>
59. <mailto:karenluise.knudsen@geo.au.dk>
60. <mailto:aku@geus.dk>

mail.2008

61. <mailto:Marie-Alexandrine.Sicre@lsce.cnrs-gif.fr>
62. <mailto:reichart@geo.uu.nl>
63. <mailto:M.N.Tsimplis@soton.ac.uk>
64. <mailto:c.goodess@uea.ac.uk>
65. <mailto:r.sutton@reading.ac.uk>
66. <mailto:valexeev@iarc.uaf.edu>
67. <mailto:victor.brovkin@pik-potsdam.de>
68. <mailto:zeng@atmos.umd.edu>
69. <mailto:terray@cerfacs.fr>
70. <mailto:dufresne@lmd.jussieu.fr>
71. <mailto:Burkhardt.Rockel@gkss.de>
72. <mailto:hurkvd@knmi.nl>
73. <mailto:philippe.ciais@lsce.ipsl.fr>
74. <mailto:rolf.philipona@meteoswiss.ch>
75. <mailto:Masa.Kageyama@lsce.ipsl.fr>
76. <mailto:jules@jamstec.go.jp>
77. <mailto:ewwo@bas.ac.uk>
78. <mailto:raynaud@lgge.obs.ujf-grenoble.fr>
79. <mailto:omarchal@whoi.edu>
80. <mailto:claire.waelbroeck@lsce.cnrs-gif.fr>
81. <mailto:p.jones@uea.ac.uk>
82. <mailto:jouzel@dsm-mail.saclay.cea.fr>
83. <mailto:Jeff.Blackford@Manchester.ac.uk>
84. <mailto:gerardv@nioz.nl>
85. <mailto:dharwood1@unl.edu>
86. <mailto:lang@liv.ac.uk>
87. <mailto:hajdas@phys.ethz.ch>
88. <mailto:x.crosta@epoc.u-bordeaux1.fr>
89. <mailto:pascal.claquin@unicaen.fr>
90. <mailto:fidelgr@fis.ucm.es>
91. <mailto:jsa@ig.cas.cz>
92. <mailto:dankd@atmos.umd.edu>
93. <mailto:kbice@whoi.edu>
94. <mailto:H.Brinkhuis@bio.uu.nl>
95. <mailto:andy@seao2.org>
96. <mailto:kbillups@udel.edu>
97. <mailto:anita.roth@uni-tuebingen.de>
98. <mailto:Gerrit.Lohmann@awi.de>
99. <mailto:P.J.Valdes@bristol.ac.uk>
100. <mailto:strecker@geo.uni-potsdam.de>
101. <mailto:mmaslin@geog.ucl.ac.uk>
102. <mailto:marie-france.loutre@uclouvain.be>
103. <mailto:aurelia.ferrari@oma.be>
104. <mailto:j.bamber@bristol.ac.uk>
105. <mailto:bickert@rcom-bremen.de>
106. <mailto:chris.d.jones@metoffice.gov.uk>
107. <mailto:elsa.cortijo@lsce.ipsl.fr>
108. <mailto:gerald.ganssen@falw.vu.nl>
109. <mailto:arne.richter@copernicus.org>
110. <mailto:Andrea.Bleyer@awi.de>
111. <mailto:B.Amelung@ICIS.unimaas.nl>
112. <mailto:spn@env.ethz.ch>
113. <mailto:bgomez@ub.edu>
114. <mailto:wmson@ucar.edu>
115. <mailto:d.vance@bristol.ac.uk>
116. <mailto:andrea.bleyer@awi.de>
117. <mailto:Gerrit.Lohmann@awi.de>
118. <http://www.awi-bremerhaven.de/CurriculumVitae/glohmann.html>
119. <http://www.awi.de/en/go/paleo>
120. <mailto:mann@psu.edu>
121. <http://www.met.psu.edu/dept/faculty/mann.htm>
122. <mailto:p.jones@uea.ac.uk>
123. <mailto:mann@psu.edu>

mail.2008

- 124. <http://www.met.psu.edu/dept/faculty/mann.htm>
- 125. <mailto:p.jones@uea.ac.uk>
- 126. <mailto:mann@psu.edu>
- 127. <http://www.met.psu.edu/dept/faculty/mann.htm>
- 128. <mailto:p.jones@uea.ac.uk>
- 129. <mailto:jean.jouzel@lsce.ipsl.fr>
- 130. <mailto:jzipsl@ipsl.jussieu.fr>
- 131. <mailto:jean.jouzel@lsce.ipsl.fr>
- 132. <mailto:mann@psu.edu>
- 133. <http://www.met.psu.edu/dept/faculty/mann.htm>

860. 1201724331.txt

#####

From: Caspar Ammann <ammann@ucar.edu>
To: P.Jones@uea.ac.uk
Subject: Re: pdf
Date: Wed, 30 Jan 2008 15:18:51 -0700

Phil,

will do. And regarding TSI, it looks like that 1361 or 1362 (+/-) are going to be the new consensus. All I hear is that this seems to be quite robust. Fodder for the critics: all these modelers, they always put in too much energy - no wonder it was warming - and now they want to reduce the natural component? The SORCE meeting is going to be on that satellite stuff but also about climate connections : Sun-Earth. Tom Crowley is going to be there, Gavin Schmidt, David Rind, and a few others; of course Judith.

Thanks for Bo Vinther's manuscript!

Caspar

On Jan 30, 2008, at 3:12 PM, [1]P.Jones@uea.ac.uk wrote:

Caspar,

OK. Keep me informed. Also I'd like to know more the conclusions of the meeting you're going to on the solar constant. Just that it can change from 1366.5 to 1361!!

Cheers

Phil

Phil,

we should hook together on this 1257 event (I call it 1257 because of the timings but its just a bit better than an informed guess). We now have these simulations of contemporary high-lat eruptions and can

mail.2008

compare them with low-lat ones.

Just a couple thoughts

pro high-lat:

- climate signal looks better in short and longer term
- potential for in-ice-core migration of some sulfur species ... some new work that has been done ...

con:

- deposition duration
- old fingerprints
- no high-lat calderas/flows of appropriate size : compare it to Eldgja or Laki, this thing is bigger!
- no large ash layers

What we need is fingerprinting. I'm participating in a project Icelandic volcanism and climate in the last 2000 years. There we have money to do some chemical fingerprinting. I'm pursuing to get somebody to run these samples. That will be the deciding thing. Remember, instrumentation has dramatically increased in sensitivity, so I think it should be possible. It's not that one would have to go dig around too much in the ice cores as the depth/location of that monster sulfate spikes are well known.

Should be interesting.

Caspar

On Jan 30, 2008, at 2:57 PM, [2]P.Jones@uea.ac.uk wrote:

Caspar,

The meeting I'm at is less interesting than IDAG.

I'll send the Greenland isotope data when I get back.

536 is a good story. 1258/9 needs to be good story too...

I think it isn't at the moment.

Cheers

Phil

Thanks Phil,

will have a look. I certainly like it, and I only was a bit picky on

mail.2008

the "largest eruption" versus "largest volcanic signal in trees". I like the isotope work very much and will now look if I can pick on something more substantial ;-)

Caspar

On Jan 30, 2008, at 1:24 PM, [3]P.Jones@uea.ac.uk wrote:

<2007GL032450.pdf>

Caspar M. Ammann

National Center for Atmospheric Research

Climate and Global Dynamics Division - Paleoclimatology

1850 Table Mesa Drive

Boulder, CO 80307-3000

email: [4]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

Caspar M. Ammann

National Center for Atmospheric Research

Climate and Global Dynamics Division - Paleoclimatology

1850 Table Mesa Drive

Boulder, CO 80307-3000

email: [5]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

Caspar M. Ammann

National Center for Atmospheric Research

Climate and Global Dynamics Division - Paleoclimatology

1850 Table Mesa Drive

Boulder, CO 80307-3000

email: [6]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

References

1. mailto:P.Jones@uea.ac.uk
2. mailto:P.Jones@uea.ac.uk
3. mailto:P.Jones@uea.ac.uk
4. mailto:ammann@ucar.edu
5. mailto:ammann@ucar.edu
6. mailto:ammann@ucar.edu

861. 1202939193.txt

#####

From: J Shukla <shukla@cola.iges.org>
To: IPCC-Sec <IPCC-Sec@wmo.int>
Subject: Future of the IPCC:
Date: wed, 13 Feb 2008 16:46:33 -0500
Cc: Ian.allison@aad.gov.au, neville.nicholls@arts.monash.edu.au,

mail.2008

fichefet@astr.ucl.ac.be, mati@at.fcen.uba.ar, randall@atmos.colostate.edu, philip@atmos.washington.edu, peltier@atmosphysics.utoronto.ca, arinke@awi-potsdam.de, peter.lemke@awi.de, bojariu@b.astral.ro, martin.heimann@bgc-jena.mpg.de, r.colman@bom.gov.au, xiaoye_02@cams.cma.gov.cn, yukihiro.nojiri@cao.go.jp, artale@casaccia.enea.it, sumi@ccsr.u-tokyo.ac.jp, hauglustaine@cea.fr, pasb@cea.fr, pierre.friedlingstein@cea.fr, schulz@cea.fr, t.k.berntsen@cicero.uio.no, menendez@cima.fcen.uba.ar, joos@climate.unibe.ch, stocker@climate.unibe.ch, derzhang@cma.gov.cn, pmzhai@cma.gov.cn, qdh@cma.gov.cn, zhaozc@cma.gov.cn, marengo@cptec.inpe.br, Ian.Watterson@csiro.au, penny.whetton@csiro.au, unni@darya.nio.org, jhc@dmi.dk, robted@eas.gatech.edu, anny.cazenave@easynet.fr, francis.zwiers@ec.gc.ca, Greg.Flato@ec.gc.ca, john.fyfe@ec.gc.ca, ken.denman@ec.gc.ca, hewitson@egs.uct.ac.za, ulrike.lohmann@env.ethz.ch, piers@env.leeds.ac.uk, P.M.Cox@exeter.ac.uk, djacob@fas.harvard.edu, eystein.jansen@geo.uib.no, gunnar.myhre@geo.uio.no, heinze@gfi.uib.no, drind@giss.nasa.gov, jouni.raisanen@helsinki.fi, cdccc@hotmail.com, thomas@hotmail.com, yluo@hotmail.com, zongci_zhao@hotmail.com, gaouxj@ictp.trieste.it, artaxo@if.usp.br, jwillebrand@ifm-geomar.de, scw@io.as.harvard.edu, matsuno@jamstec.go.jp, amnat_c@jgsee.kmutt.ac.th, Albert.Klein.Tank@knmi.nl, dorlandv@knmi.nl, ricardo@lab.cricyt.edu.ar, raynaud@lgge.obs.ujf-grenoble.fr, taylor13@llnl.gov, letreut@lmd.jussieu.fr, Sandrine.Bony@lmd.jussieu.fr, Jean-Claude.Duplessy@lsce.cnrs-gif.fr, ciais@dsm-mail.saclay.cea.fr, jouzel@dsm-mail.saclay.cea.fr, masson@dsm-mail.saclay.cea.fr, kattsov@main.mgo.rssi.ru, jayes@mecheng.iisc.ernet.in, c.mauritzen@met.no, jknganga@meteo.go.ke, jorge.carrasco@meteochile.cl, j.m.gregory@metoffice.gov.uk, james.murphy@metoffice.gov.uk, jim.haywood@metoffice.gov.uk, peter.stott@metoffice.gov.uk, richard.betts@metoffice.gov.uk, richard.jones@metoffice.gov.uk, richard.wood@metoffice.gov.uk, wontk@metri.re.kr, rprinn@mit.edu, s.raper@mmu.ac.uk, pldsdias@model.iag.usp.br, kitoh@mri-jma.go.jp, nada@mri-jma.go.jp, derzhang@msn.com, mokssit@mtpnet.gov.ma, hegerl@nc.rr.com, layesarr@netscape.net, fujii@nipr.ac.jp, d.lowe@niwa.co.nz, j.renwick@niwa.co.nz, d.wratt@niwa.cri.nz, david.Easterling@noaa.gov, david.w.fahey@noaa.gov, Isaac.Held@noaa.gov, martin.manning@noaa.gov, Ronald.Stouffer@noaa.gov, Susan.Solomon@noaa.gov, Sydney.Levitus@noaa.gov, thomas.c.peterson@noaa.gov, v.ramaswamy@noaa.gov, tzhang@nsidc.org, ckshum@osu.edu, rahmstorf@ozean-klima.de, apitman@penman.es.mq.edu.au, rahmstorf@pik-potsdam.de, hanawa@pol.geophys.tohoku.ac.jp, ram@prl.ernet.in, ralley@psu.edu, dingyh@public.bta.net.cn, jwren@public.lz.gs.cn, b.j.hoskins@rdg.ac.uk, bsoden@rsmas.miami.edu, gul@sail.msk.ru, raga@servidor.unam.mx, victormr@servidor.unam.mx, jlean@ssd5.nrl.navy.mil, jto@u.arizona.edu, atgaye@ucad.sn, brasseur@ucar.edu, eholland@ucar.edu, knutti@ucar.edu, lindam@ucar.edu, meehl@ucar.edu, ottobli@ucar.edu, trenbert@ucar.edu, wcollins@ucar.edu, mprather@uci.edu, ltalley@ucsd.edu, mjmolina@ucsd.edu, rsomerville@ucsd.edu, c.lequere@uea.ac.uk, k.briffa@uea.ac.uk, n.gillett@uea.ac.uk, p.jones@uea.ac.uk, georg.kaser@uibk.ac.at, penner@umich.edu, laprise.rene@uqam.ca, n.bindoff@utas.edu.au, weaver@uvic.ca, anthony.chen@uwimona.edu.jm, cubasch@vr-web.de, Rupa Kumar Kolli <RKolli@wmo.int>, r.ramesh@yahoo.co.in, dolago@yahoo.co.uk, ambenje@yahoo.com, busuioc@yahoo.com, david.parker@yahoo.com, jorcar59@yahoo.com, rahim_f@yahoo.com, solomina@yandex.ru

<x-flowed>

Dear All,

I would like to respond to some of the items in the attached text on issues etc. in particular to the statement in the section 3.1.1 (sections 3: Drivers of required change in the future).

"There is now greater demand for a higher level of policy relevance in the work of IPCC, which could provide policymakers a robust scientific basis for action".

1. while it is true that a vast majority of the public and the

mail.2008

policymakers have accepted the reality of human influence on climate change (in fact many of us were arguing for stronger language with a higher level of confidence at the last meetings of the LAs), how confident are we about the projected regional climate changes?

I would like to submit that the current climate models have such large errors in simulating the statistics of regional (climate) that we are not ready to provide policymakers a robust scientific basis for "action" at regional scale. I am not referring to mitigation, I am strictly referring to science based adaptation.

For example, we can not advise the policymakers about re-building the city of New Orleans - or more generally about the habitability of the Gulf-Coast - using climate models which have serious deficiencies in simulating the strength, frequency and tracks of hurricanes.

We will serve society better by enhancing our efforts on improving our models so that they can simulate the statistics of regional climate fluctuations; for example: tropical (monsoon depressions, easterly waves, hurricanes, typhoons, Madden-Julian oscillations) and extratropical (storms, blocking) systems in the atmosphere; tropical instability waves, energetic eddies, upwelling zones in the oceans; floods and droughts on the land; and various manifestations (ENSO, monsoons, decadal variations, etc.) of the coupled ocean-land-atmosphere processes.

It is inconceivable that policymakers will be willing to make billion-and trillion-dollar decisions for adaptation to the projected regional climate change based on models that do not even describe and simulate the processes that are the building blocks of climate variability. Of course, even a hypothetical, perfect model does not guarantee accurate prediction of the future regional climate, but at the very least, our suggestion for action will be based on the best possible science.

It is urgently required that the climate modeling community arrive at a consensus on the required accuracy of the climate models to meet the "greater demand for a higher level of policy relevance".

2. Is "model democracy" a valid scientific method? The "I" in the IPCC desires that all models submitted by all governments be considered equally probable. This should be thoroughly discussed, because it may have serious implications for regional adaptation strategies. AR4 has shown that model fidelity and model sensitivity are related. The models used for IPCC assessments should be evaluated using a consensus metric.

3. Does dynamical downscaling for regional climate change provide a robust scientific basis for action?

Is there a consensus in the climate modeling community on the validity of regional climate prediction by dynamical downscaling? A large number of dynamical downscaling efforts are underway worldwide. This is not necessarily because it is meaningful to do it, but simply because it is possible to do it. It is not without precedent that quite deficient climate models are used by large communities simply because it is convenient to use them. It is self-evident that if a coarse resolution IPCC model does not correctly capture the large-scale mean and transient response, a high-resolution regional model, forced by the lateral boundary conditions from the coarse model, can not improve the response. Considering the important role of multi-scale interactions and feedbacks in the climate system, it is essential that the IPCC-class global models themselves be run at sufficiently high resolution.

mail.2008

Regards,
Shukla

IPCC-Sec wrote:

> Dear LAS & CLAs,

>

> Please find attached a letter and issues related to the future of the
> IPCC.

>

> With kind regards,

>

> Annie

>

> IPCC Secretariat

> WMO

> 7bis, Avenue de la Paix

> P.O. Box 2300

> 1211 Geneva 2

> SWITZERLAND

> Tel: +41 22 730 8208/8254/8284

> Fax: +41 22 730 8025/8013

> Email: IPCC-Sec@wmo.int

> Website: <http://www.ipcc.ch>

>

> * * * * *

>

>

>

</x-flowed>

862. 1203620834.txt

#####

From: David Thompson <davet@atmos.colostate.edu>

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

Subject: Re: Your ENSO series

Date: Thu, 21 Feb 2008 14:07:14 +0000

Phil,

If it works, let's plan on me visiting for the day April 30 (I'll come out April
29; leave

May 1). I'll put the date on my calendar and assume it works unless I hear
otherwise. If

there is a better day that week, please let me know.

Thanks,

Dave

Dave,

will send on your details to the seminar organizer here. The week
of April 28 - May 2 is OK for me. I hope this is what you meant by
last week.

A few thoughts on the plots.

1. There isn't a drop off in land data around 1945 - nor during WW2.
So this is different from the ocean data. Most series are complete

mail.2008

or have been slightly infilled during the period in Europe. Berlin for example only missed one day's T obs in April 45.

2. Fuego could be underestimated.

3. It could also be that sulphate emissions were very high at this time - late 60s, early 70s.

I'll await the text !

Cheers

Phil

At 16:18 19/02/2008, you wrote:

Hi Phil,

I'd enjoy visiting.... how does the first or last week of April look to you?

As for some new results:

I've attached two figures. Both focus on the land data.

The first figure includes 4 time series. From top to bottom: the global-mean land data (CRUTEM 3); the ENSO fit; the COWL fit; the residual global-mean time series. There is nothing here you haven't seen before - the residual land time series is identical to the one in the Nature paper.

As we've discussed, the residual land time series highlights the signature of the volcanos. And as far as low frequency variability goes: the residual land time series supports the IPCC contention that the global warmed from ~1900-1940; did not warm from ~1940-1980; and warmed substantially from 1980 to present.

OK.... so now I'm going to play with removing the volcanic signal. There are a lot of ways to do this, and I haven't settled on the best method. For now, I am driving the simple climate model I've been using for ENSO with the Ammann et al. volcanic forcing time series. I get identical results using Crowley's estimate and Sato's estimate. The figure on page 2 shows the effect of removing the volcanic signal. From top to bottom: the the global-mean residual land time series (repeated from the previous figure); the volcanic fit; the 'ENSO/COWL/Volcano' residual land time series.

Some key points:

1. the volcanic fit isn't perfect, but captures most of the volcanic signal.

2. the residual time series (bottom of Fig 2) is interesting. If you look closely, it suggests the globe has warmed continuously since 1900 with two exceptions: a 'bite' in the 1970s, and a downwards 'step' in 1945. The step in 1945 is not as dramatic as the step in the ocean data. But it's there. (I'm guessing the corresponding change in variance is due to a sudden increase in data coverage).

3. the volcanic fit highlights the fact that the lack of warming in the middle part of the century comes from only two features: the step in 45 and Agung. When Agung is removed, land temperatures march upwards from 1945-1970 (Fig 2 bottom).

4. the bite in the 1970s could be due to an underestimate of the impact of Fuego (the bite is also evident in the SST data).

What do you think? The step in 1945 is not as dramatic as the step in the SST data. But it's certainly there. It's evident in the COWL/ENSO residual time series (top of Fig 2): removing Agung simply clarifies that without the step temperatures marched steadily upwards from 1900-1970.

-Dave

ï¿¼

On Feb 19, 2008, at 1:28 PM, Phil Jones wrote:

Dave,

Thanks.

Before seeing what you send, I think I'll find it harder to believe something is wrong with the land data. I can be convinced though....

So you're in Reading now. Do you still want to come up to

mail.2008

distant Norwich
at some point and also give a talk?
Cheers
Phil

At 16:55 18/02/2008, you wrote:

Phil,
I'm really sorry for the delay; my family and I have been in transit from the US to the UK this past week, and it's taken a bit for us to get settled.

I've attached the ENSO index I've been using. The first month is Jan 1850; the last is Dec 2006. The time series has a silly number of sig figures - that's just how Matlab wanted to save it.

The data are in K and are scaled as per the fit to the global-mean (as in the paper).

I've got some new results regarding the land data... I'll think you'll find them interesting. I'll pass them along in the next day or so... the main point is that I suspect the land data might also have some spurious cooling in the middle part of the century. More to come....

-Dave

ÃÃÃ

On Feb 14, 2008, at 12:35 PM, Phil Jones wrote:

David,

For a presentation I'm due to make in a few months, can you send me the ENSO and the COWL series that are in Figure 1 in the paper.

I'm not sure what I will do with COWL, but I want to compare your ENSO

with some of the ENSO-type indices I have.

These seem monthly from about the 1860s or maybe earlier.

Cheers

Phil

At 16:49 07/02/2008, you wrote:

So it made it past the first hurdle, which is good. My hunch is that the paper will fare OK in review, but you never know with Nature. And it's possible a reviewer will insist on our providing a correction... anyway, we'll see...

-Dave

Begin forwarded message:

From: [1]j.thorpe@nature.com
Date: February 7, 2008 3:44:07 AM PST
To: [2]davet@atmos.colostate.edu
Subject: Nature 2008-01-00939 out to review

Dear Professor Thompson,

Thank you for submitting your manuscript entitled "A discontinuity in the time series of global-mean surface temperature" to Nature. I am pleased to tell you that we are sending your paper out for review.

We will be in touch again as soon as we have received comments from our reviewers.

Yours sincerely

Nichola O'Brien

Staff

Nature

For Dr. Joanna Thorpe

Associate Editor, Nature

Nature Publishing Group -- [3]<http://www.nature.com/nature>

The Macmillan Building, 4 Crinan Street, London N1 9XW, UK

Tel +44 20 7833 4000; Fax +44 20 7843 4596; [4]nature@nature.com

Page 107

mail.2008
968 National Press Building, Washington DC 20045-1938, USA
Tel +1 202 737 2355; Fax +1 202 628 1609; [5]nature@naturedc.com
* Please see NPG's author and referees' website
([6]www.nature.com/ authors) for information about and links to
policies, services
and author benefits. See also [7]http://blogs.nature.com/nautilus,
our blog for authors, and [8]http://blogs.nature.com/peer-to-peer,
our blog about peer-review.
This email has been sent through the NPG Manuscript Tracking
System NY-610A-NPG&MTS

David W. J. Thompson
[9]www.atmos.colostate.edu/~davet
Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

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

David W. J. Thompson
[11]www.atmos.colostate.edu/~davet
Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

Phil,
I'm really sorry for the delay; my family and I have been in
transit from the US to the UK this past week, and it's taken a bit
for us to get settled.
I've attached the ENSO index I've been using. The first month is
Jan 1850; the last is Dec 2006. The time series has a silly number
of sig figures - that's just how Matlab wanted to save it.
The data are in K and are scaled as per the fit to the global-mean
(as in the paper).
I've got some new results regarding the land data... I'll think
you'll find them interesting. I'll pass them along in the next day
or so... the main point is that I suspect the land data might also
have some spurious cooling in the middle part of the century. More
to come....

-Dave

On Feb 14, 2008, at 12:35 PM, Phil Jones wrote:

David,
For a presentation I'm due to make in a few months, can you
send me the ENSO and the COWL series that are in Figure 1 in the
paper.

I'm not sure what I will do with COWL, but I want to compare

mail.2008

your ENSO

with some of the ENSO-type indices I have.

These seem monthly from about the 1860s or maybe earlier.

Cheers

Phil

At 16:49 07/02/2008, you wrote:

So it made it past the first hurdle, which is good. My hunch is that the paper will fare OK in review, but you never know with Nature. And it's possible a reviewer will insist on our providing a correction... anyway, we'll see...

-Dave

Begin forwarded message:

From: [12]j.thorpe@nature.com
Date: February 7, 2008 3:44:07 AM PST
To: [13]davet@atmos.colostate.edu
Subject: Nature 2008-01-00939 out to review

Dear Professor Thompson,

Thank you for submitting your manuscript entitled "A discontinuity in the time series of global-mean surface temperature" to Nature. I am pleased to tell you that we are sending your paper out for review.

We will be in touch again as soon as we have received comments from our reviewers.

Yours sincerely

Nichola O'Brien

Staff

Nature

For Dr. Joanna Thorpe

Associate Editor, Nature

Nature Publishing Group -- [14]<http://www.nature.com/nature>

The Macmillan Building, 4 Crinan Street, London N1 9XW, UK

Tel +44 20 7833 4000; Fax +44 20 7843 4596; [15]nature@nature.com

968 National Press Building, Washington DC 20045-1938, USA

Tel +1 202 737 2355; Fax +1 202 628 1609; [16]nature@nature.com

* Please see NPG's author and referees' website

([17]www.nature.com/authors) for information about and links to

policies, services and author benefits. See also [18][\[blogs.nature.com/nautilus\]\(http://blogs.nature.com/nautilus\),](http://</p></div><div data-bbox=)

our blog for authors, and [19][http:// blogs.nature.com/peer-to-peer](http://blogs.nature.com/peer-to-peer), our blog

about

peer-review.

This email has been sent through the NPG Manuscript Tracking System NY-610A-NPG&MTS

David W. J. Thompson

[20]www.atmos.colostate.edu/~davet

Dept of Atmospheric Science

Colorado State University

Fort Collins, CO 80523

USA

Phone: 970-491-3338

Fax: 970-491-8449

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

NR4 7TJ

Email [21]p.jones@uea.ac.uk

Page 109

UK

 David W. J. Thompson
 [22]www.atmos.colostate.edu/~davet
 Dept of Atmospheric Science
 Colorado State University
 Fort Collins, CO 80523
 USA
 Phone: 970-491-3338
 Fax: 970-491-8449

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

 David W. J. Thompson
 [24]www.atmos.colostate.edu/~davet
 Dept of Atmospheric Science
 Colorado State University
 Fort Collins, CO 80523
 USA
 Phone: 970-491-3338
 Fax: 970-491-8449

Hi Phil,
 I'd enjoy visiting.... how does the first or last week of April look to you?
 As for some new results:

I've attached two figures. Both focus on the land data.
 The first figure includes 4 time series. From top to bottom: the global-mean
 land data
 (CRUTEM 3); the ENSO fit; the COWL fit; the residual global-mean time series.

There is nothing here you haven't seen before - the residual land time series is
 identical to the one in the Nature paper.

As we've discussed, the residual land time series highlights the signature of
 the volcanos. And as far as low frequency variability goes: the residual land time
 series

supports the IPCC contention that the global warmed from ~1900-1940; did not
 warm from ~1940-1980; and warmed substantially from 1980 to present.

OK.... so now I'm going to play with removing the volcanic signal. There are a
 lot of ways to do this, and I haven't settled on the best method. For now, I am
 driving the simple climate model I've been using for ENSO with the Ammann et al. volcanic
 forcing time series. I get identical results using Crowley's estimate and Sato's
 estimate.

The figure on page 2 shows the effect of removing the volcanic signal. From top
 to

mail.2008

bottom: the the global-mean residual land time series (repeated from the previous figure); the volcanic fit; the 'ENSO/COWL/Volcano' residual land time series. Some key points:

1. the volcanic fit isn't perfect, but captures most of the volcanic signal.
2. the residual time series (bottom of Fig 2) is interesting. If you look closely, it suggests the globe has warmed continuously since 1900 with two exceptions: a 'bite' in the 1970s, and a downwards 'step' in 1945. The step in 1945 is not as dramatic as the step in the ocean data. But it's there. (I'm guessing the corresponding change in variance is due to a sudden increase in data coverage).
3. the volcanic fit highlights the fact that the lack of warming in the middle part of the century comes from only two features: the step in 45 and Agung. When Agung is removed, land temperatures march upwards from 1945-1970 (Fig 2 bottom).
4. the bite in the 1970s could be due to an underestimate of the impact of Fuego (the bite is also evident in the SST data).

what do you think? The step in 1945 is not as dramatic as the step in the SST data. But it's certainly there. It's evident in the COWL/ENSO residual time series (top of Fig 2): removing Agung simply clarifies that without the step temperatures marched steadily upwards from 1900-1970.

-Dave

On Feb 19, 2008, at 1:28 PM, Phil Jones wrote:

Dave,
Thanks.
Before seeing what you send, I think I'll find it harder to believe something is wrong with the land data. I can be convinced though....
So you're in Reading now. Do you still want to come up to distant Norwich at some point and also give a talk?
Cheers
Phil

At 16:55 18/02/2008, you wrote:

Phil,
I'm really sorry for the delay; my family and I have been in transit from the US to the UK this past week, and it's taken a bit for us to get settled.
I've attached the ENSO index I've been using. The first month is Jan 1850; the last is Dec 2006. The time series has a silly number of sig figures - that's just how Matlab wanted to save it.
The data are in K and are scaled as per the fit to the global-mean (as in the paper).
I've got some new results regarding the land data... I'll think you'll find them interesting. I'll pass them along in the next day or so... the main point is that I suspect the land data might also have some spurious cooling in the middle part of the century. More to come....
-Dave

i2/4

On Feb 14, 2008, at 12:35 PM, Phil Jones wrote:

David,
For a presentation I'm due to make in a few months, can you

mail.2008

send me the ENSO and the COWL series that are in Figure 1 in the paper.

I'm not sure what I will do with COWL, but I want to compare your ENSO

with some of the ENSO-type indices I have.

These seem monthly from about the 1860s or maybe earlier.

Cheers

Phil

At 16:49 07/02/2008, you wrote:

So it made it past the first hurdle, which is good. My hunch is that the paper will fare OK in review, but you never know with Nature. And it's possible a reviewer will insist on our providing a correction... anyway, we'll see...

-Dave

Begin forwarded message:

From: [25]j.thorpe@nature.com
Date: February 7, 2008 3:44:07 AM PST
To: [26]davet@atmos.colostate.edu
Subject: Nature 2008-01-00939 out to review

Dear Professor Thompson,

Thank you for submitting your manuscript entitled "A discontinuity in the time series of global-mean surface temperature" to Nature. I am pleased to tell you that we are sending your paper out for review.

We will be in touch again as soon as we have received comments from our reviewers.

Yours sincerely

Nichola O'Brien

Staff

Nature

For Dr. Joanna Thorpe

Associate Editor, Nature

Nature Publishing Group -- [27]<http://www.nature.com/nature>

The Macmillan Building, 4 Crinan Street, London N1 9XW, UK

Tel +44 20 7833 4000; Fax +44 20 7843 4596; [28]nature@nature.com

968 National Press Building, Washington DC 20045-1938, USA

Tel +1 202 737 2355; Fax +1 202 628 1609; [29]nature@nature.com

* Please see NPG's author and referees' website ([30]www.nature.com/authors)

for

information about and links to policies, services

and author benefits. See also [31]<http://blogs.nature.com/nautilus>,

our blog for authors, and [32]<http://blogs.nature.com/peer-to-peer>,

our blog about peer-review.

This email has been sent through the NPG Manuscript Tracking System NY-610A-NPG&MTS

David W. J. Thompson

[33]www.atmos.colostate.edu/~davet

Dept of Atmospheric Science

Colorado State University

Fort Collins, CO 80523

USA

Phone: 970-491-3338

Fax: 970-491-8449

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
NR4 7TJ
UK

mail.2008
Email [34]p.jones@uea.ac.uk

David W. J. Thompson
[35]www.atmos.colostate.edu/~davet
Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449
Phil,

I'm really sorry for the delay; my family and I have been in transit from the US to the UK this past week, and it's taken a bit for us to get settled. I've attached the ENSO index I've been using. The first month is Jan 1850; the last is Dec 2006. The time series has a silly number of sig figures - that's just how Matlab wanted to save it. The data are in K and are scaled as per the fit to the global-mean (as in the paper). I've got some new results regarding the land data... I'll think you'll find them interesting. I'll pass them along in the next day or so... the main point is that I suspect the land data might also have some spurious cooling in the middle part of the century. More to come....

-Dave

On Feb 14, 2008, at 12:35 PM, Phil Jones wrote:

David,

For a presentation I'm due to make in a few months, can you send me the ENSO and the COWL series that are in Figure 1 in the paper. I'm not sure what I will do with COWL, but I want to compare your ENSO with some of the ENSO-type indices I have. These seem monthly from about the 1860s or maybe earlier.
Cheers
Phil

At 16:49 07/02/2008, you wrote:

So it made it past the first hurdle, which is good. My hunch is that the paper will fare OK in review, but you never know with Nature. And it's possible a reviewer will insist on our providing a correction... anyway, we'll see...

-Dave

Begin forwarded message:

From: [36]j.thorpe@nature.com
Date: February 7, 2008 3:44:07 AM PST
To: [37]davet@atmos.colostate.edu
Subject: Nature 2008-01-00939 out to review

Dear Professor Thompson,

Thank you for submitting your manuscript entitled "A discontinuity in the time series of

mail.2008

global-mean surface temperature" to Nature. I am pleased to tell you that we
are sending your paper out for review.
We will be in touch again as soon as we have received comments from our
reviewers.

Yours sincerely
Nichola O'Brien
Staff

Nature

For Dr. Joanna Thorpe

Associate Editor, Nature

Nature Publishing Group -- [38]<http://www.nature.com/nature>

The Macmillan Building, 4 Crinan Street, London N1 9XW, UK

Tel +44 20 7833 4000; Fax +44 20 7843 4596; [39]nature@nature.com

968 National Press Building, Washington DC 20045-1938, USA

Tel +1 202 737 2355; Fax +1 202 628 1609; [40]nature@nature.com

* Please see NPG's author and referees' website ([41]www.nature.com/authors)

for

information about and links to policies, services and author benefits. See also

[42]<http://blogs.nature.com/nautilus>, our blog for authors, and

[43]<http://blogs.nature.com/peer-to-peer>, our blog about peer-review.

This email has been sent through the NPG Manuscript Tracking System

NY-610A-NPG&MTS

David W. J. Thompson
[44]www.atmos.colostate.edu/~davet
Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

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

David W. J. Thompson
[46]www.atmos.colostate.edu/~davet
Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

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

David W. J. Thompson
[48]www.atmos.colostate.edu/~davet
Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

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

David W. J. Thompson
www.atmos.colostate.edu/~davet
Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

References

1. <mailto:j.thorpe@nature.com>
2. <mailto:davet@atmos.colostate.edu>
3. <http://www.nature.com/nature>
4. <mailto:nature@nature.com>
5. <mailto:nature@naturedc.com>
6. <http://www.nature.com/>
7. <http://blogs.nature.com/nautilus>
8. <http://blogs.nature.com/peer-to-peer>
9. <http://www.atmos.colostate.edu/~davet>
10. <mailto:p.jones@uea.ac.uk>
11. <http://www.atmos.colostate.edu/~davet>
12. <mailto:j.thorpe@nature.com>
13. <mailto:davet@atmos.colostate.edu>
14. <http://www.nature.com/nature>
15. <mailto:nature@nature.com>
16. <mailto:nature@naturedc.com>
17. <http://www.nature.com/authors>
18. http://
19. http://
20. <http://www.atmos.colostate.edu/~davet>
21. <mailto:p.jones@uea.ac.uk>
22. <http://www.atmos.colostate.edu/~davet>
23. <mailto:p.jones@uea.ac.uk>
24. <http://www.atmos.colostate.edu/~davet>
25. <mailto:j.thorpe@nature.com>
26. <mailto:davet@atmos.colostate.edu>
27. <http://www.nature.com/nature>

mail.2008

- 28. <mailto:nature@nature.com>
- 29. <mailto:nature@naturedc.com>
- 30. <http://www.nature.com/>
- 31. <http://blogs.nature.com/nautilus>
- 32. <http://blogs.nature.com/peer-to-peer>
- 33. <http://www.atmos.colostate.edu/~davet>
- 34. <mailto:p.jones@uea.ac.uk>
- 35. <http://www.atmos.colostate.edu/~davet>
- 36. <mailto:j.thorpe@nature.com>
- 37. <mailto:davet@atmos.colostate.edu>
- 38. <http://www.nature.com/nature>
- 39. <mailto:nature@nature.com>
- 40. <mailto:nature@naturedc.com>
- 41. <http://www.nature.com/authors>
- 42. <http://blogs.nature.com/nautilus>
- 43. <http://blogs.nature.com/peer-to-peer>
- 44. <http://www.atmos.colostate.edu/~davet>
- 45. <mailto:p.jones@uea.ac.uk>
- 46. <http://www.atmos.colostate.edu/~davet>
- 47. <mailto:p.jones@uea.ac.uk>
- 48. <http://www.atmos.colostate.edu/~davet>
- 49. <mailto:p.jones@uea.ac.uk>

863. 1203631942.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Coverage
Date: Thu, 21 Feb 2008 17:12:22 -0800
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Phil,

A quick question: Do you happen to have a "percentage land coverage mask" for the HadCRUT3v data? And if so, does this exist as a netCDF file?

with best regards,

Ben
Phil Jones wrote:

- >
- > Ben,
- > Email to Dick reminded me ! Had another phone call and I'd forgotten.
- > First file is the coverage.
- >
- > Second is a program that reads this file - Channel 1.
- >
- > File is 36 by 72. 5 by 5 degs.
- >
- > It will start at 85-90N for the 36 subscript.
- >
- > for 72 it is either dateline or Greenwich.
- >
- > Cheers
- > Phil
- >
- >
- > At 16:53 15/02/2008, you wrote:
- >> Dear Dick,

mail.2008

>>
>> I'm forwarding an email that I sent out several days ago. For the last
>> month, I've been working hard to respond to a recent paper by David
>> Douglass, John Christy, Benjamin Pearson, and Fred Singer. The paper
>> claims that the conclusions of our CCSP Report were incorrect, and
>> that there is a fundamental discrepancy between simulated and observed
>> temperature changes in the tropical troposphere. Douglass et al. also
>> assert that models cannot represent the "observed" differential
>> warming of the surface and troposphere. To address these claims, I've
>> been updating some of the comparisons of models and observations that
>> we did for the CCSP Report, now using newer observational datasets
>> (among them NOAA ERSST-v2 and v3). As you can see from the forwarded
>> email, the warming rates of tropical SSTs are somewhat different for
>> ERSST-v2 and v3 - ERSST-v3 warms by less than v2. Do you understand
>> why this is?

>>
>> with best regards, and hope you are well!

>>
>> Ben

>> -----
>>
>> Benjamin D. Santer
>> Program for Climate Model Diagnosis and Intercomparison
>> Lawrence Livermore National Laboratory
>> P.O. Box 808, Mail Stop L-103
>> Livermore, CA 94550, U.S.A.
>> Tel: (925) 422-2486
>> FAX: (925) 422-7675
>> email: santer1@llnl.gov

>> -----
>>
>> X-Account-Key: account1
>> Return-Path: <santer1@llnl.gov>
>> Received: from mail-2.llnl.gov ([unix socket])
>> by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
>> wed, 13 Feb 2008 18:34:52 -0800
>> Received: from smtp.llnl.gov (nspiron-3.llnl.gov [128.115.41.83])
>> by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with
>> ESMTMP id m1E2YMTv008791;
>> wed, 13 Feb 2008 18:34:52 -0800
>> X-Attachments: LAST_IJC_figure04.pdf
>> X-IronPort-AV: E=McAfee;i="5200,2160,5229"; a="26979778"
>> X-IronPort-AV: E=Sophos;i="4.25,349,1199692800";
>> d="pdf'?scan'208";a="26979778"
>> Received: from dione.llnl.gov (HELO [128.115.57.29]) ([128.115.57.29])
>> by smtp.llnl.gov with ESMTMP; 13 Feb 2008 18:34:51 -0800
>> Message-ID: <47B3A8CB.90605@llnl.gov>
>> Date: wed, 13 Feb 2008 18:34:51 -0800
>> From: Ben Santer <santer1@llnl.gov>
>> Reply-To: santer1@llnl.gov
>> Organization: LLNL
>> User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
>> MIME-Version: 1.0
>> To: santer1@llnl.gov, Peter Thorne <peter.thorne@metoffice.gov.uk>,
>> Stephen Klein <klein21@llnl.gov>,
>> Susan Solomon <Susan.Solomon@noaa.gov>,
>> John Lanzante <John.Lanzante@noaa.gov>,
>> Melissa Free <melissa.free@noaa.gov>,
>> Dian Seidel <dian.seidel@noaa.gov>, Tom Wigley
>> <wigley@cgd.ucar.edu>,
>> Karl Taylor <taylor13@llnl.gov>,
>>

mail.2008

>> Thomas R Karl <Thomas.R.Karl@noaa.gov>, Carl Mears
>> <mears@remss.com>,
>> "David C. Bader" <bader2@llnl.gov>,
>> "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>,
>> Frank Wentz <frank.wentz@remss.com>,
>> Leopold Haimberger <leopold.haimberger@univie.ac.at>,
>> "Michael C. MacCracken" <mmaccrac@comcast.net>,
>> Phil Jones <p.jones@uea.ac.uk>,
>> Steve Sherwood <Steven.Sherwood@yale.edu>,
>> Tim Osborn <t.osborn@uea.ac.uk>,
>> Gavin Schmidt <gschmidt@giss.nasa.gov>,
>> "Hack, James J." <jhack@ornl.gov>, peter gleckler
>> <gleckler1@llnl.gov>
>> Subject: Additional calculations
>> References: <200801121320.26705.John.Lanzante@noaa.gov>
>> <478C528C.8010606@llnl.gov> <p06230904c3b2e6b2c92f@[172.17.135.52]>
>> <478EC287.8030008@llnl.gov>
>> <1200567390.8038.35.camel@eld443.desktop.frd.metoffice.com>
>> <7.0.1.0.2.20080117140720.022259c0@llnl.gov>
>> <1200995209.23799.95.camel@eld443.desktop.frd.metoffice.com>
>> <47962FD1.1020303@llnl.gov>
>> In-Reply-To: <47962FD1.1020303@llnl.gov>
>> Content-Type: multipart/mixed;
>> boundary="-----060600010907080200090109"

>> Dear folks,

>> Sorry about the delay in sending you the next version of our
>> manuscript. I decided that I needed to perform some additional
>> calculations. I was concerned that we had not addressed the issue of
>> "differential warming" of the surface and troposphere - an issue which
>> Douglass et al. HAD considered.

>> Our work thus far shows that there are no fundamental inconsistencies
>> between simulated and observed temperature trends in individual
>> tropospheric layers (T2 and T2LT). But we had not performed our
>> "paired trends" test for trends in the surface-minus-T2LT difference
>> time series. This is a much tougher test to pass: differencing
>> strongly damps the correlated variability in each "pair" of surface
>> and T2LT time series. Because of this noise reduction, the standard
>> error of the linear trend in the difference series is typically
>> substantially smaller than the size of the standard error in an
>> individual surface or T2LT time series. This makes it easier to reject
>> the null hypothesis of "no significant difference between simulated
>> and observed trends".

>> In the CCSP Report, the behavior of the trends in the
>> surface-minus-T2LT difference series led us to note that:

>> "Comparing trend differences between the surface and the troposphere
>> exposes potential discrepancies between models and observations in the
>> tropics".

>> So it seemed wise to re-examine this "differential warming" issue. I
>> felt that if we ignored it, Douglass et al. would have grounds for
>> criticizing our response.

>> I've now done the "paired trends" test with the trends in the
>> surface-minus-T2LT difference series. The results are quite
>> interesting. They are at variance with the above-quoted finding of the
>> CCSP Report. The new results I will describe show that the "potential
>> discrepancies" in the tropics have largely been resolved.

mail.2008

>> Here's what I did. I used three different observational estimates of
>> tropical SST changes. These were from NOAA-ERSST-v2, NOAA-ERSST-v3,
>> and HadISST1. It's my understanding that NOAA-ERSST-v3 and HadISST1
>> are the most recent SST products of NCDC and the Hadley Centre. I'm
>> also using T2LT data from RSS v3.0 and UAH v5.2. Here are the tropical
>> (20N-20S) trends in these five datasets over the 252-month period from
>> January 1979 to December 1999, together with their 1-sigma adjusted
>> standard errors (in brackets):

>>
>> UAH v5.2 0.060 (+/-0.137)
>> RSS v3.0 0.166 (+/-0.130)
>> HADISST1 0.108 (+/-0.133)
>> NOAA-ERSST-v2 0.100 (+/-0.131)
>> NOAA-ERSST-v3 0.077 (+/-0.121)

>> (all trends in degrees C/decade).

>>
>> The trends in the three SST datasets are (by definition) calculated
>> from anomaly data that have been spatially-averaged over tropical
>> oceans. The trends in T2LT are calculated from anomaly data that have
>> been spatially averaged over land and ocean. It is physically
>> reasonable to do the differencing over different domains, since the
>> temperature field throughout the tropical troposphere is more or less
>> on the moist adiabatic lapse rate set by convection over the warmest
>> waters.

>>
>> These observational trend estimates are somewhat different from those
>> available to us at the time of the CCSP Report. This holds for both
>> T2LT and SST. For T2LT, the RSS trend used in the CCSP Report and in
>> the Santer et al. (2005) Science paper was roughly 0.13 degrees
>> C/decade. As you can see from the Table given above, it is now ca.
>> 0.17 degrees C/decade. Carl tells me that this change is largely due
>> to a change in how he and Frank adjust for inter-satellite biases.
>> This adjustment now has a latitudinal dependence, which it did not
>> have previously.

>>
>> The tropical SST trends used in the CCSP Report were estimated from
>> earlier versions of the Hadley Centre and NOAA SST data, and were of
>> order 0.12 degrees C/decade. The values estimated from more recent
>> datasets are lower - and markedly lower in the case of NOAA-ERSST-v3
>> (0.077 degrees C/decade). The reasons for this downward shift in the
>> estimated warming of tropical SSTs are unclear. As Carl pointed out in
>> an email that he sent me earlier today:

>>
>> "One important difference is that post 1985, NOAA-ERSST-v3 directly
>> ingests "bias adjusted" SST data from AVHRR, a big change from v2,
>> which didn't use any satellite data (directly). AVHRR is strongly
>> affected in the tropics by the Pinatubo eruption in 1991. If the
>> "bias adjustment" doesn't completely account for this, the trends
>> could be changed".

>>
>> Another possibility is treatment of biases in the buoy data. It would
>> be nice if Dick Reynolds could advise us as to the most likely
>> explanation for the different warming rates inferred from
>> NOAA-ERSST-v2 and v3.

>>
>> Bottom line: The most recent estimates of tropical SST changes over
>> 1979 to 1999 are smaller than we reported in the CCSP Report, while
>> the T2LT trend (at least in RSS) is larger. The trend in the observed
>> difference series, NOAA-ERSST-v3 Ts minus RSS T2LT, is now -0.089
>> degrees C/decade, which is very good agreement with the multi-model
>> ensemble trend in the Ts minus T2LT difference series (-0.085 degrees
>> C/decade). Ironically, if Douglass et al. had applied their flawed

mail.2008

>> "consistency test" to the multi-model ensemble mean trend and the
>> trend in the NOAA-ERSST-v3 Ts minus RSS T2LT difference series, they
>> would not have been able to conclude that models and observations are
>> inconsistent!

>>
>> Here are the observed trends in the tropical Ts minus T2LT difference
>> series in the six different pairs of Ts and T2LT datasets, together
>> with the number of "Hits" (rejections of the null hypothesis of no
>> significant difference in trends) and the percentage rejection rate
>> (based on 49 tests in each case)

>>
>> "Pair" Trend 1-sigma C.I. Hits Rej.Rate
>> HadISST1 Ts minus RSS T2LT -0.0577 (+/-0.0347) 1 (2.04%)
>> NOAA-ERSST-v2 Ts minus RSS T2LT -0.0660 (+/-0.0382) 1 (2.04%)
>> NOAA-ERSST-v3 Ts minus RSS T2LT -0.0890 (+/-0.0350) 0 (0.00%)
>> HadISST1 Ts minus UAH T2LT +0.0488 (+/-0.0371) 28 (57.14%)
>> NOAA-ERSST-v2 Ts minus UAH T2LT +0.0405 (+/-0.0403) 25 (51.02%)
>> NOAA-ERSST-v3 Ts minus UAH T2LT +0.0175 (+/-0.0370) 15 (30.60%)
>> Multi-model ensemble mean -0.0846

>>
>> Things to note:

>>
>> 1) For all "pairs" involving RSS T2LT data, the multi-model ensemble
>> mean trend is well within even the 1-sigma statistical uncertainty of
>> the observed trend.

>>
>> 2) For all "pairs" involving RSS T2LT data, there are very few
>> statistically-significant differences between the observed and
>> model-simulated "differential warming" of the tropical surface and
>> lower troposphere.

>>
>> 3) For all "pairs" involving UAH T2LT data, there are
>> statistically-significant differences between the observed and
>> model-simulated "differential warming" of the tropical surface and
>> lower troposphere. Even in these cases, however, rejection of the null
>> hypothesis is not universal: rejection rates range from 30% to 57%.
>> Clearly, not all models are inconsistent with the observational
>> estimate of "differential warming" inferred from UAH data.

>>
>> These results contradict the "model inconsistent with data" claims of
>> Douglass et al.

>>
>> The attached Figure is analogous to the Figure we currently show in
>> the paper for T2LT trends. Now, however, results are for trends in the
>> surface-minus-T2LT difference series. Rather than showing all six
>> "pairs" of observational results in the top panel, I've chosen to show
>> two pairs only in order to avoid unnecessarily complicating the
>> Figure. I propose, however, that we provide results from all six pairs
>> in a Table.

>>
>> As is visually obvious from the Figure, trends in 46 of the 49
>> simulated surface-minus-T2LT difference series pairs are within the
>> 2-sigma confidence intervals of the NOAA-ERSST-v3 Ts minus RSS T2LT
>> trend (the light grey bar). And as is obvious from Panel B, even the
>> Douglass et al. " σ_{SE} " encompasses the difference series trend
>> from the NOAA-ERSST-v3 Ts/RSS T2LT pair.

>>
>> I think we should show these results in our paper.

>>
>> The bottom line: Use of newer T2LT datasets (RSS) and Ts datasets
>> (NOAA-ERSST-v3, HADISST1) largely removes the discrepancy between
>> tropical surface and tropospheric warming rates. We need to explain
>> why the observational estimates of tropical SST changes are now

mail.2008

>> smaller than they were at the time of the CCSP Report. We will need
>> some help from Dick Reynolds with this.

>> With best regards,

>> Ben

>> Benjamin D. Santer
>> Program for Climate Model Diagnosis and Intercomparison
>> Lawrence Livermore National Laboratory
>> P.O. Box 808, Mail Stop L-103
>> Livermore, CA 94550, U.S.A.
>> Tel: (925) 422-2486
>> FAX: (925) 422-7675
>> email: santer1@llnl.gov

>>
>>
>>
> 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

>
>
>

>
> program growlandmergeetc
> dimension lnd(72,36),nlnd(72,36),ivsst(72,36),jcov(72,36)
> dimension icmb(72,36),alcov(72,36),ascov(72,36),iysst(72,36)
> dimension isdvar(72,36,12),neigsd(72,36,12)
> dimension iorigt(72,36),icount(72,36)
> dimension ash(12),anh(12),ashp(12),anhp(12)
> dimension np(12),npch(12),npinf(12),npchan(12),npsst(12)
> rad=57.2958
> ir=13
> c calculate maximum % coverage of hemisphere in cos units
> xnh=0.0
> do 20 j=1,18
> w=cos((92.5-j*5)/rad)
> do 19 i=1,72
> 19 xnh=xnh+w
> 20 continue
> c read in land fraction in %
> read(1,21)i1,i2
> 21 format(2i6)
> do 22 j=1,36
> 22 read(1,23)(jcov(i,j),i=1,72)
> 23 format(72i6)
> c set coverage of land to % of at least 25% and less than 75%
> c ocean percent is then simply the rest
> do 24 j=1,36
> do 24 i=1,72
> alcov(i,j)=0.01*jcov(i,j)
> if(alcov(i,j).le.24.9)alcov(i,j)=25.0

```

mail.2008
> if(alcov(i,j).ge.75.1)alcov(i,j)=75.0
> ascov(i,j)=100.0 - alcov(i,j)
> 24 continue
> c read in the sd of the land only dataset (var corected) to assess
> c whether the neighbour check can legitimately correct values
> do 901 k=1,12
> read(4,27)ii
> do 902 j=1,36
> 902 read(4,29)(isdvar(i,j,k),i=37,72),(isdvar(ii,j,k),ii=1,36)
> 901 continue
> c read in neighbouring sd calculated from at least 4 of the
> c neighbouring 8 5 degree squares around each grid box
> do 903 k=1,12
> read(18,27)ii
> do 904 j=1,36
> 904 read(18,29)(neigsd(i,j,k),i=37,72),(neigsd(ii,j,k),ii=1,36)
> 903 continue
> c skip the first 19 years of the variance corrected land data
> c as the variance corrected SST data only starts in
> c also skip the first 19 years of the original gridded temps
> c so later can check the number of stations available per gridbox
> c per month
> do 25 k=1851,1869
> do 26 kk=1,12
> read(2,27)i1,i2
> 27 format(2i5)
> read(ir,27)i1,i2
> do 28 j=1,36
> 28 read(2,29)(lnd(i,j),i=37,72),(lnd(ii,j),ii=1,36)
> 29 format(12i5)
> do 128 j=1,36
> 128 read(ir,29)(iorigt(i,j),i=37,72),(iorigt(ii,j),ii=1,36)
> do 129 j=1,36
> 129 read(ir,29)(icount(i,j),i=37,72),(icount(ii,j),ii=1,36)
> 26 continue
> 25 continue
> c read in the land and sst data (both variance corrected)
> c reading in the land allow for the greenwich start of the land
> c and the dateline start for the SST. Output is from the dateline
> do 31 k=1870,1999
> ashy=0.0
> anhy=0.0
> if(k.ge.1901)ir=14
> if(k.ge.1951)ir=15
> if(k.ge.1991)ir=16
> if(k.ge.1994)ir=17
> do 32 kk=1,12
> npch(kk)=0
> npchan(kk)=0
> np(kk)=0
> npinf(kk)=0
> npsst(kk)=0
> c read in the original gridded land to get the station count
> c per grid box
> read(ir,27)i1,i2
> do 131 j=1,36
> 131 read(ir,29)(iorigt(i,j),i=37,72),(iorigt(ii,j),ii=1,36)
> do 132 j=1,36
> 132 read(ir,29)(icount(i,j),i=37,72),(icount(ii,j),ii=1,36)
> c read in the variance corrected land
> read(2,27)i1,i2
> write(7,27)kk,k
> do 33 j=1,36

```

```

> 33 read(2,29)(lnd(i,j),i=37,72),(lnd(ii,j),ii=1,36)
> c copy lnd array to nlnd so that the growing doesn't use already
> c infilled values
> do 34 j=1,36
> do 34 i=1,72
> 34 nlnd(i,j)=lnd(i,j)
> c read in sst data
> read(3,21)i1,i2
> do 35 j=1,36
> 35 read(3,23)(ivsst(i,j),i=1,72)
> c check land for extremes and fill in gaps (only one grid box away
> c provided there are at least 4 of the 8 surrounding boxes)
> do 41 j=1,36
> j1=j-1
> j2=j+1
> if(j1.eq.0)j1=1
> if(j2.eq.37)j2=36
> do 42 i=1,72
> sum=0.0
> nsum=0
> i1=i-1
> i2=i+1
> do 43 jj=j1,j2
> do 44 ii=i1,i2
> iii=i
> if(iii.eq.73)iii=1
> if(iii.eq.0)iii=72
> if(jj.eq.j.and.iii.eq.i)go to 44
> if(lnd(iii,jj).eq.-9999)go to 44
> sum=sum+lnd(iii,jj)
> nsum=nsum+1
> 44 continue
> 43 continue
> if(lnd(i,j).ne.-9999)np(kk)=np(kk)+1
> if(nsum.le.3)go to 47
> sum=sum/nsum
> ndep=sum+0.5
> if(sum.lt.0.0)ndep=ndep-1
> nval=ndep
> if(lnd(i,j).eq.-9999)go to 46
> npch(kk)=npch(kk)+1
> ndep=lnd(i,j)-nval
> if(neigsd(i,j,kk).eq.-9999)go to 47
> if(iabs(ndep).le.225)go to 47
> if(iabs(ndep).lt.neigsd(i,j,kk)*2.0)go to 47
> if(icount(i,j).ge.2)go to 47
> nlnd(i,j)=nval
> npchan(kk)=npchan(kk)+1
> 48 write(6,202)k,kk,j,i,nval,lnd(i,j),ndep,isdvar(i,j,kk),
> neigsd(i,j,kk),nlnd(i,j),nsum,icount(i,j),iorigt(i,j)
> 202 format(4i4,9i6)
> go to 47
> 46 nlnd(i,j)=nval
> npinf(kk)=npinf(kk)+1
> 47 continue
> 42 continue
> 41 continue
> c merge with marine using the weighting factors
> do 51 j=1,36
> do 52 i=1,72
> wx=0.0
> xx=0.0
> if(nlnd(i,j).eq.-9999)go to 55

```

```

> wx=wx+alcov(i,j)
> xx=xx+alcov(i,j)*nlnd(i,j)
> 55 if(ivsst(i,j).eq.-32768)go to 56
> wx=wx+ascov(i,j)
> xx=xx+ascov(i,j)*ivsst(i,j)
> 56 if(wx.ge.0.001)go to 59
> icmb(i,j)=-9999
> go to 57
> 59 aa=xx/wx
> ia=aa+0.5
> if(xx.lt.0.0)ia=ia-1
> icmb(i,j)=ia
> c writing out the land/sst merging checking when both are present
> c if(wx.ge.99.9)write(6,203)kk,j,i,ia,nlnd(i,j),ivsst(i,j),
> c >wx,alcov(i,j),ascov(i,j)
> c 203 format(6i6,3f7.1)
> 57 continue
> 52 continue
> 51 continue
> c write out the new merged file
> do 53 j=1,36
> 53 write(7,54)(icmb(i,j),i=1,72)
> 54 format(12i5)
> c calculate the hemispheric averages
> anh(kk)=0.0
> ash(kk)=0.0
> ashp(kk)=0.0
> anhp(kk)=0.0
> wx=0.0
> xx=0.0
> do 61 j=1,18
> w=cos((92.5-j*5.0)/rad)
> do 62 i=1,72
> if(icmb(i,j).eq.-9999)go to 62
> wx=wx+w
> xx=xx+w*icmb(i,j)
> 62 continue
> 61 continue
> anh(kk)=xx*0.01/wx
> anhp(kk)=wx*100.0/xnh
> wx=0.0
> xx=0.0
> do 63 j=19,36
> w=cos((j*5.0-92.5)/rad)
> do 64 i=1,72
> if(icmb(i,j).eq.-9999)go to 64
> wx=wx+w
> xx=xx+w*icmb(i,j)
> 64 continue
> 63 continue
> ash(kk)=xx*0.01/wx
> ashp(kk)=wx*100.0/xnh
> anhy=anhy+anh(kk)
> ashy=ashy+ash(kk)
> 32 continue
> anhy=anhy/12.0
> ashy=ashy/12.0
> write(8,89)k,anh,anhy
> 89 format(i4,12f6.2,f7.2)
> write(8,90)k,anhp
> 90 format(i4,12f6.0)
> write(9,89)k,ash,ashy
> write(9,90)k,ashp

```

mail.2008

```
> write(10,91)k,np
> write(10,91)k,npch
> write(10,91)k,npchan
> write(10,91)k,npinf
> write(10,92)
> 92 format(/)
> 91 format(i4,12i6)
> 31 continue
> stop
> end
>
>
```

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

864. 1203693276.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Yan Zhongwei" <yzw@mail.tea.ac.cn>
Subject: Re: Adjusting Beijing temperature series
Date: Fri Feb 22 10:14:36 2008

Zhongwei,
will read soon !
Attached is what I finally submitted to JGR.
Don't pass on to anyone else.
I have also received a paper from Li, Q, but have yet to
read that. He only sent it yesterday.
Cheers
Phil

At 09:55 22/02/2008, you wrote:

Hi, Phil,
Attached please find a draft paper about site-changes and urbanization at
Beijing. It
may be regarded as an extension of our early work (Yan et al 2001 AAS) and
therefore I
would be happy to ask you to join as a co-author.
Regarding your recent paper about UHI effect in China (no doubt upon a
large-scale
warming in the region), I hope the Beijing case may serve as a helpful rather
than a
contradictory (as it may appear so) reference.
The urbanization-bias at BJ was considerable but could hardly be quantified. I
suspect
it was somehow overestimated by a recent work (Ren et al 2007). Please feel
free to
comment and revise.

mail.2008

I'll check and complete the reference list, while you may also add in new references
Cheers
Zhongwei

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

865. 1204315423.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Melissa Free <Melissa.Free@noaa.gov>
Subject: Re: IJOC paper
Date: Fri, 29 Feb 2008 15:03:43 -0800
Reply-to: santer1@llnl.gov
Cc: John Lanzante <John.Lanzante@noaa.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>

<x-flowed>
Dear Melissa,

Thanks for your comments on the IJOC paper. Here are a few quick responses.

Melissa Free wrote:

- > Hi Ben,
- > I've looked through the draft and have some comments:
- > 1. I don't feel completely comfortable with the use of SSTs rather than
- > combined land-sea surface temperatures for the lapse-rate analysis. Are
- > we sure we have thought through the implications of this approach? If
- > you show that the relationship between SSTs and tropical mean
- > tropospheric temperatures is consistent between models and observations,
- > that seems to imply that they are not so consistent for land
- > surface-troposphere lapse rates. Could this be used to support the
- > Pielke-Christy theory that (land) surface temperature trends are
- > overestimated in the existing observational datasets?

I do feel comfortable with use of SSTs (rather than combined land+ocean temperatures) to estimate changes in tropical lapse rates. As Isaac Held pointed out, the temperature of the free troposphere in the deep tropics follows a moist adiabat which is largely set by the warmest SSTs in areas experiencing convection. The temperature of the free troposphere in the deep tropics is not set by temperatures over land. So if you want to see whether observations and models show lapse-rate changes that are in accord with a moist adiabatic lapse rate theory, it makes sense to look at SSTs rather than combined land+ocean surface temperatures. Admittedly, the focus of this paper is NOT on amplification behavior. Still, it does make sense to look at tropical lower tropospheric lapse rates in terms of their primary physical driver: SSTs.

As I tried to point out in the text of the IJOC paper, models and RSS-based estimates of lapse-rate changes are consistent, even if lapse-rate changes are inferred from combined land+ocean surface temperatures. The same same does not hold for lapse rate changes estimated from HadCRUT3v and UAH data. I must admit that I don't fully understand the latter result. If you look at Table 1, you'll see that

mail.2008

the multi-model ensemble-mean temporal standard deviation of $T_{\{SST\}}$ is 0.243 degrees C, while the multi-model ensemble-mean temporal standard deviation of $T_{\{L+O\}}$ is higher (0.274 degrees C). This makes good physical sense, since noise is typically higher over land than over ocean. Yet in the HadCRUT3v data, the temporal standard deviation of $T_{\{L+O\}}$ (0.197 degrees C) is very similar to that of $T_{\{SST\}}$ for the HadISST1 and HadISST2 data (HadISST2 is the SST component of HadCRUT3v). The fact that HadCRUT3v appears to have very similar variability over land and ocean seems counter-intuitive to me. Could it indicate a potential problem in the tropical land 2m temperatures in HadCRUT3v? I don't know. I'll let Phil address that one. The point is that we've done - at least in my estimation - a thorough job of looking at the sensitivity of our significance test results to current observational uncertainties in surface temperature changes.

> 2. The conclusion seems like too much of a dissertation on past history
> of the controversy.

As I pointed out in my email of Feb. 26th, I had a specific concern about the "Summary and Conclusions" section. I think that many readers of the paper will skip all the statistical stuff, and just read the Abstract and the "Summary and Conclusions". I did want the latter section to be relatively self-contained. We could have started by saying: "Here are the errors in Douglass et al., and here is what we found". But on balance, I thought that it would be more helpful to provide some scientific context. As I mentioned this morning, the Douglass et al. paper has received attention in high places. Not everyone who reads our response will be apprised of the history and context.

> 3. Regarding the time scale invariance of model amplification and the
> effects of volcanic eruptions on the trend comparisons, I am attaching a
> draft of my paper with John Lanzante comparing volcanic signals in sonde
> datasets v. models. I'm not sure if the statements on page 45 of the
> IJOC paper are consistent with my findings. (I thought about sending you
> this paper before, but it seemed like you were probably too busy with
> the IJOC paper to look at it.)

I'll look at your paper this weekend. I'm not quite sure which statements on page 45 you are referring to.

> 4. I suspect the statement in the last sentence of the conclusion won't
> represent the view of all authors-although it's certainly Dian's view. I
> don't think it is my view quite yet.

Others have also queried this final paragraph. At present, it looks like it might be tough to accommodate the divergent views on this subject. But I'll certainly try my best!

> I'm investigating an expedited internal review process and will let you
> know how it looks.

Thanks for looking into the expedited review!

> -Melissa

with best regards,

Ben

(P.S.: I hope you don't mind that I've copied my reply to Phil. I'm hoping he can chime in on the issue of land surface temperature variability in the HadCRUT3v data.)

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

866. 1205413129.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Past Millennia Climate Variability - Review Paper
Date: Thu, 13 Mar 2008 08:58:49 -0400
Reply-to: mann@psu.edu

Hi Phil,
Sorry, one other point. In item #4 below, the point that is being made, as shown
(and discussed) elsewhere, applies both to the MBH method and the the canonical
regression method (the latter is demonstrated in experiments by wahl and Ammann not shown
but referred to elsewhere in the text). So to be accurate and fair, the sentence in question
on page 50 really has to be rephrased as follows:
Examinations of this kind are shown in Figures 3a,b (and parallel experiments
not shown) demonstrating that, at least for the truncated-EOF CFR method used by MBH98
(employing inverse regression) and the canonical regression method that has been widely used
by many other paleoclimate researchers, there is some degree of sensitivity to the
climatological information available in calibration.
I realize there are many co-authors on the paper that have used the canonical
regression method before, so perhaps there is pressure to focus the criticism on the MBH
method. But that is simply not fair, as the other analyses by wahl and Ammann not shown
clearly demonstrates this applies to canonical regression as well--we can debate the
relative sensitivity of the two methods, but it is similar.
This is an absolutely essential issue from my point of view, and I'm afraid I
cannot sign my name to this paper w/out this revision.
I'm sure you understand--thanks for your help,
mike
Michael Mann wrote:

Phil,
Looks mostly fine to me now. I'm in Belgium (w/ the Louvain crowd) and only
intermittent internet access, so will be difficult to provide much more feedback than the
below. I hope

that is ok? Here are my remaining minor comments:

1) the author list is a bit front-loaded w/ CRU folks. You should certainly be the first author, but the remaining order makes this paper look more like a "CRU" effort than a "wengen" effort, and perhaps that will have an unintended impact on the way the paper is received by the broader community. I was also wondering how I ended up so far down the list

:(

I think I was one of the first to provide a substantive contribution to the paper. Was my contribution really so minor compared to those others? The mechanism behind the author list is unclear, partially alphabetical (towards the end), but partly not. You are of course the best judge of peoples' relative contributions, and if the current author order indeed represents that according to your judgment, then I'm fine w/ that. Just thought I'd check though.

2) page 45, 2nd paragraph, should substitute "(e.g. Shindell et al, 2001; Collins et al 2002)" for "Collins et al 2002"

3) page 48, 2nd paragraph, 3rd sentence, should substitute "RegEM (implemented with TTLS as described by Mann et al 2007)" for "RegEM".

4) page 50, bottom paragraph, first sentence: I think that the use of "crucially" here is unnecessarily inflammatory and overly dramatic. This word can be removed without any detriment to the point being made, don't you think?

5) page 51, 2nd paragraph, logic does not properly follow in certain places as currently phrased (a frequent problem w/ Eugene's writing unfortunately!):

a. sentence beginning at end of line 9 of paragraph, should be rephrased as follows:

Mann et al. (2005) used pseudo-proxy experiments that apparently showed that this method did not underestimate the amplitude of the reconstructed NH temperature anomalies: however,

Smerdon and Kaplan (2007) show that this may have been a false positive result arising from differences between the implementation of the RegEM algorithm in the pseudo-proxy experiments and in the real-proxy reconstructions which leads to a sensitivity of the

pseudoproxy results to the calibration period used (also noted by Lee et al., 2008).

b. the sentence following the one above should be rephrased:

Mann et al. (2007; cf. their Figs. 3-4) demonstrate that a variant of the RegEM method that uses TTLS, rather than ridge regression produces an NH temperature reconstruction whose

amplitude fidelity does not exhibit the calibration interval dependence of the previous implementation by Mann et al 2005, and yields reconstructions that do not suffer from

amplitude loss for a wide range of signal-to-noise ratios and noise spectra (though Lee et

al., 2008, suggest that an appropriately implemented ridge regression can also produce good results).

c. the sentence following the one above should be rephrased:

mail.2008

with TTLS as implemented by Mann et al (2007), RegEM performs without amplitude loss in model-based tests (versions without trend removal), including using the high-amplitude ECHO-G model output utilized by Bürger et al. (2006), von Storch et al. (2006), and Küttel et al. (2007) to examine truncated-EOF methods.

6) page 52, 1st paragraph, 7th line, the reference of "the MBH reconstruction" is erroneous, because the tests have nothing to do w/ the MBH reconstruction per se, only--potentially--the MBH method under certain circumstances. In fact, Mann et al (2007) [and Wahl and Amman(2007)] both show that the actual amplitude loss realized in the MBH reconstruction in reality is probably quite small. This very point is made at the top of page 53! So the reference to "the MBH reconstruction" needs to be eliminated here. It is already clear by context what this is actually referring to (idealized experiments using both the MBH and canonical applied to surrogate proxy networks).

7) Re, Caspar--well he seems to be in his "non-responsible" phase right now, hasn't replied to my messages either. Will keep on trying, let me know if any of the above needs further elaboration. We're travelling for the weekend but will still have intermittent email access,

mike
Phil Jones wrote:

Dear All,
Attached is the penultimate draft of the Wengen paper. If you have time can you look through this. If you've not much time, can you look through your sections and the intro/conclusions. I hope we in CRU have got all your comments in. We have been through them all - including Gene's which came last night and Francis' the night before.

WE URGENTLY NEED CASPAR TO RESPOND. Can Gene, Mike and anyone who can get Caspar to respond to emails tell him that there are a few questions in this draft we need him to respond to. We need better versions of Figure 3, plus there are some flagged points in sections 3 and 4.

Juerg - is Figure 5 OK. If not resend separately - don't embed as this screwed up last time.

Plan A is for us to submit this to The Holocene next Wednesday. So we need by then, from each of you a quick email to say you've got this and any comments by next Monday - March 17. Submission will be March 19. There is no Plan B. With the Feb 20 email, there were no responses from Peck, Eystein and Nick. If we don't hear from you three by next week, we will remove you from the author list!

If anyone knows if any of these three are in the field please let me know?
Things to check:

1. Everybody happy with the author order. The idea here was the three of us in CRU, the main authors of the sections in section order, then others in alphabetical order.
2. If you have time also look at sections 2.5 and 2.6. Issue here is - is there enough there. Thanks to Juerg for some of these sections.
3. There are a couple of refs (Juerg) we need - Buntgen et al. and D'Arrigo et al.

mail.2008

al.

Next week, we (CRU) will be working on the alterations- using IPCC rules. These are - if you want a change justify it, and if you say this is unbalanced, or just European, or emphasizes Lee et al. (2008), then gives us the additional text to make alterations. We've left a few comments in where these sorts of comments were made last time. There will be time to make alterations while The Holocene reviews it. It will also be better to read it later when there is time after submission. I've not read this version yet, so apologies if there are any pieces of poor English. I will be reading again this weekend. Finally, Thorsten, if you think I've missed anybody off this email, forward and let me know. Juerg needs to send on to the others within Bern.

Cheers

Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email [1]p.jones@uea.ac.uk
NR4 7TJ
UK

--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [2]mann@psu.edu
University Park, PA 16802-5013

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

--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [4]mann@psu.edu
University Park, PA 16802-5013

[5]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

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

mail.2008

867. 1206549942.txt

#####

From: David Parker <david.parker@metoffice.gov.uk>
To: "Mann, Michael" <mann@virginia.edu>
Subject: Heads up
Date: wed, 26 Mar 2008 12:45:42 +0000
Cc: "Folland, Chris" <chris.folland@metoffice.gov.uk>, "Kennedy, John" <john.kennedy@metoffice.gov.uk>, "Jones, Phil" <p.jones@uea.ac.uk>, "Karl, Tom" <Thomas.R.Karl@noaa.gov>

Mike

Yes it was based on only Jan+Feb 2008 and padding with that final value but John Kennedy has changed / shortly will change this misleading plot!

Regards

David

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

From: Michael Mann [mailto:mann@meteo.psu.edu]
Sent: 26 March 2008 11:19
To: Folland, Chris
Cc: Phil Jones; Thomas R Karl
Subject: heads up

Hi Chris (and Tom and Phil),

I hope you're all doing well. Just wanted to give you a heads up on something. Have you seen this?
http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual_s21.png
apparently the contrarians are having a field day w/ this graph. My understanding that it is based on using only Jan+Feb 08 and padding w/ that final value.

Surely this can't be?? Is Fred Singer now running the UK Met Office website?

would appreciate any info you can provide,

mike

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: mann@psu.edu

--
David Parker Met Office Hadley Centre FitzRoy Road EXETER EX1 3PB UK
E-mail: david.parker@metoffice.gov.uk
Tel: +44-1392-886649 Fax: +44-1392-885681 <http://www.metoffice.gov.uk>

868. 1206628118.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: trenbert@ucar.edu, "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: Re: Fwd: ukweatherworld
Date: Thu, 27 Mar 2008 10:28:38 +0000
Cc: mann@multiproxy.evsc.virginia.edu, santer1@llnl.gov, "Susan Solomon"
<susan.solomon@noaa.gov>

<x-flowed>

Peck et al,

I recall meeting David Deeming at a meeting years ago (~10).
He worked in boreholes then. I've seen his name on several of the
skeptic websites.

Kevin's idea is a possibility. I wouldn't post on the website
'ukweatherworld'.

The person who sent you this is likely far worse. This is David Holland.
He is a UK citizen who send countless letters to his MP in the UK, writes
in Energy & Environment about the biased IPCC and has also been hassling
John Mitchell about his role as Review Editor for Ch 6. You might want to
talk to John about how he's responding. He has been making requests under
our FOI about the letters Review Editors sent when signing off. I'm
sure Susan

is aware of this. He's also made requests for similar letters re
WG2 and maybe 3.

Keith has been in contact with John about this.

I've also seen the quote about getting rid of the MWP - it would seem to go
back many years, maybe even to around the TAR. I've no idea where it came
from. I didn't say it!

I've written a piece for RMS [popular journal weather on the MWP
and LIA - from a UK
perspective. It is due out in June. I can send if you want.

I'm away all next week - with Mike. PaleoENSO meeting in Tahiti - you can't
turn those sorts of meetings down!

Cheers
Phil

At 23:15 26/03/2008, Kevin Trenberth wrote:

>Hi Jon

>There is a lot to be said for ignoring such a thing. But I understand the
>frustration. An alternative approach is to write a blog on this topic of
>the medieval warm period and post it at a neutral site and then refer
>enquiries to that link. You would have a choice of directly confronting
>the statements or making a more general statement, presumably that such a
>thing is real but was more regional and not as warm as most recent times.
>This approach would not then acknowledge that particular person, except
>indirectly.

>

>A possible neutral site might be blogs.nature.com/climatefeedback/
>I posted a number of blogs there last year but not this year. I can send
>you the contact person if you are interested and you can make the case
>that they should post the blog.

mail.2008

>
>Good luck
>Kevin
>
>
>> Hi Phil, Kevin, Mike, Susan and Ben - I'm looking
>> for some IPCC-related advice, so thanks in
>> advance. The email below recently came in and I
>> googled "We have to get rid of the warm medieval
>> period" and "Overpeck" and indeed, there is a
>> person David Deeming that attributes the quote to
>> an email from me. He apparently did mention the
>> quote (but I don't think me) in a Senate hearing.
>> His "news" (often with attribution to me) appears
>> to be getting widespread coverage on the
>> internet. It is upsetting.
>>
>> I have no memory of emailing w/ him, nor any
>> record of doing so (I need to do an exhaustive
>> search I guess), nor any memory of him period. I
>> assume it is possible that I emailed w/ him long
>> ago, and that he's taking the quote out of
>> context, since know I would never have said what
>> he's saying I would have, at least in the context
>> he is implying.
>>
>> Any idea what my reaction should be? I usually
>> ignore this kind of misinformation, but I can
>> imagine that it could take on a life of it's own
>> and that I might want to deal with it now, rather
>> than later. I could - as the person below
>> suggests - make a quick statement on a web site
>> that the attribution to me is false, but I
>> suspect that this Deeming guy could then produce
>> a fake email. I would then say it's fake. Or just
>> ignore? Or something else?
>>
>> I googled Deeming, and from the first page of
>> hits got the sense that he's not your average
>> university professor... to put it lightly.
>>
>> Again, thanks for any advice - I'd really like
>> this to not blow up into something that creates
>> grief for me, the IPCC, or the community. It is
>> bogus.
>>
>> Best, Peck
>>
>>
>>>X-Sieve: CMU sieve 2.3
>>>Reply-To: "David Holland" <d.holland@theiet.org>
>>>From: "David Holland" <d.holland@theiet.org>
>>>To: <jto@u.arizona.edu>
>>>Subject: ukweatherworld
>>>Date: Mon, 24 Mar 2008 08:39:10 -0000
>>>
>>>Dear Dr Overpeck,
>>>
>>>
>>>
>>>I recall David Deeming giving evidence to a
>>>Senate hearing to the effect that he had
>>>received an email including a remark to the

mail.2008

> >>effect "we have to get rid of the warm medieval
> >>period". I have now seen several comment web
> >>pages attribute the email to your. Some serious
> >>and well moderated pages like
> >>ukweatherworld would welcome a post from you if
> >>the attribution is untrue and would, I feel
> >>sure, remove it if you were to ask them to. I am
> >>sure that many other blogs would report your
> >>denial. Is there any reason you have not issued
> >>a denial?

> >>

> >>

> >>

> >>David Holland

> >

> >

> > --

> > Jonathan T. Overpeck

> > Director, Institute for the Study of Planet Earth

> > Professor, Department of Geosciences

> > Professor, Department of Atmospheric Sciences

> >

> > Mail and Fedex Address:

> >

> > Institute for the Study of Planet Earth

> > 715 N. Park Ave. 2nd Floor

> > University of Arizona

> > Tucson, AZ 85721

> > direct tel: +1 520 622-9065

> > fax: +1 520 792-8795

> > <http://www.geo.arizona.edu/dges1/>

> > <http://www.ispe.arizona.edu/>

> >

> >

> >

> _____

>Kevin Trenberth

>Climate Analysis Section, NCAR

>PO Box 3000

>Boulder CO 80307

>ph 303 497 1318

><http://www.cgd.ucar.edu/cas/trenbert.html>

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

</x-flowed>

869. 1207158227.txt

#####

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

To: "Folland, Chris" <chris.folland@metoffice.gov.uk>

Subject: Re: heads up

mail.2008

Date: wed, 02 Apr 2008 13:43:47 -0400

Reply-to: mann@psu.edu

Cc: Phil Jones <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Richard.W.Reynolds@noaa.gov

<x-flowed>

Hey Chris,

In Tahiti (w/ Phil), limited email. Thanks so much for the detailed response. I also heard from David about this, who had similar. sounds like you guys are on top of this. The contrarians will cry conspiracy once the spurious plot is taken down and replaced w/ a corrected one, but what you can do.

I'm sorry to hear you're retiring from the Met Office, but sounds like you're going to remain active, which is great. lets catch up on things sometime soon more generally!

talk to you later,

mike

Folland, Chris wrote:

> Dear Mike and all

>

> First, thanks very much, Mike, for noticing this and preventing greater
> problems. The error arose from a pre-existing hidden software bug that
> the person updating the data had not realised was there. The software is
> a mixture of languages which makes it less than transparent. The bug is
> now fixed on all the smoothed graphs. It was made worse because the last
> point was not an average of several preceding years as it should have
> been but was just January 2008. So many apologies for any excitement
> this may have created in the hearts of the more ardent sceptics. Some
> are much on the warpath at present over the lack of recent global
> warming, fired in some cases by visions of a new solar Dalton Minimum.

>

> I'm retiring from full time work on 17th April but I will return part
> time semi-retired taking pension on 1 June. I've managed to keep my
> present grading. My Climate Variability and Forecasting group is being
> split (it's the largest in the Hadley Centre by a margin). The biggest
> part is becoming technically from today a new Climate Monitoring and
> Attribution group under Peter Stott as Head. He will bring two existing
> attribution staff to make a group of c.22. Most of the rest (12) will
> form the bulk of a new Seasonal to Decadal Forecasting group to be set
> up most likely this summer with a new Head. Finally Craig Donlon,
> Director of the GODAE GHRSSST sea surface temperature project, will go
> back to our National Centre for Ocean Forecasting (in the next wing of
> this building), but will work closely we hope with Nick Rayner in Peter
> Stott's new group on HadISST2.

>

> I will return to a new 3 day a week position in the Seasonal to Decadal
> Forecasting Group, a mixture of research, some strategy and advice, and
> importantly, operational seasonal, annual, and probably decadal,
> forecasting. The Met Office are putting more emphasis on this area,
> especially the seasonal at present, which is becoming high profile as
> seasonal success is perceived to have improved. No staff
> responsibilities! Tom Peterson will approve! I will keep my
> co-leadership with Jim Kinter of the Clivar Climate of the Twentieth
> Century modelling project for now as well.

>

> So quite a change, as I will be doing more computing work than I have
> had time for, moving into IDL this autumn which the Hadley Centre as a
> whole are moving over to about then.

mail.2008

>
> Mike, it's a fair time since we interacted so I'd be very interested in
> your activities and plans.

>
> With best regards

> Chris

>
> Prof. Chris Folland
> Head of Climate Variability and Forecasting Research

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

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

> Tel: +44 (0)1392 886646

> 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: Michael Mann [<mailto:mann@meteo.psu.edu>]

> Sent: 26 March 2008 11:19

> To: Folland, Chris

> Cc: Phil Jones; Thomas R Karl

> Subject: heads up

>
> Hi Chris (and Tom and Phil),

>
> I hope you're all doing well. Just wanted to give you a heads up on
> something. Have you seen this?

> [http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual_s21](http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual_s21.png)
> .png

> apparently the contrarians are having a field day w/ this graph. My
> understanding that it is based on using only Jan+Feb 08 and padding w/
> that final value.

>
> Surely this can't be?? Is Fred Singer now running the UK Met Office
> website?

>
> would appreciate any info you can provide,

> mike

> --

> Michael E. Mann

> Associate Professor

> Director, Earth System Science Center (ESSC)

> Department of Meteorology

Phone: (814) 863-4075

> 503 Walker Building

FAX: (814) 865-3663

> The Pennsylvania State University

email: mann@psu.edu

> University Park, PA 16802-5013

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

>

>

--

mail.2008

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

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

</x-flowed>

870. 1208278112.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>, "Clare Goodess" <C.Goodess@uea.ac.uk>, "Anthony Footitt" <a.footitt@uea.ac.uk>, "Suraje Dessai" <s.dessai@uea.ac.uk>, "Mark New" <mark.new@ouce.ox.ac.uk>, "Jim Hall" <jim.hall@newcastle.ac.uk>, "C G Kilsby" <c.g.kilsby@newcastle.ac.uk>, <ana.lopez@ouce.ox.ac.uk>
Subject: Re: EA PQQ for review by 4pm
Date: Tue Apr 15 12:48:32 2008
Cc: "Arkell, Brian" <Brian.Arkell@atkinsglobal.com>, "Sene, Kevin" <Kevin.Sene@atkinsglobal.com>

Geoff,

Have had a look through. I hope all will read their own CVs and institution bits.

My caught one word in Suraje's paragraph. The word was 'severed'. It should be 'served' ! Also his promising suit of methods would read better as a 'suite' Finally in Mark's he's a Principal Investigator.

Cheers
Phil

At 09:38 15/04/2008, Darch, Geoff J wrote:

Dear all,

Thanks to everyone for sending text etc, in particular to Jim and Chris for the succinct

answer to ET1.

Please find attached (1) the full PQQ, minus Experience and Technical (ET) text, for information; (2) the ET text, for review.

I'd be grateful for your review of the ET text. In particular (a) please comment on my

draft table in ET2 - I have done my best to capture my knowledge of CRU and Tyndall

skills with respect to the criteria, but you are clearly better placed than me!

(b) do

you think the CVs cover the technical areas adequately? we may be a little weak on

conservation and ecology. we have a good CV we can add here, and I'm sure Tyndall has

too (e.g. Andrew) but that would mean taking another out.

we are exploring a link with the specialist communications consultancy Futerra, but

apart from a brief mention, we leaving anything else on this to the full bid stage.

I'd be grateful if you would let me have any comments by 4pm today. This will

mail.2008

give me

time to finalise the document and email it first thing tomorrow.

Best wishes,

Geoff

<<EA_PQQ_ET_Draft.doc>> <<EA-PQQ_Atkins-CRU-Tyn_Draft.DOC>>

Geoff Darch

Senior Consultant

Water and Environment

ATKINS

Broadoak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough, PE2

6YS, UK

Tel: +44 (0) 1733 366969

Fax: +44 (0) 1733 366999

Mobile: +44 (0) 7834 507590

E-mail: geoff.darch@atkinsglobal.com

web: [1]www.atkinsglobal.com/climatechange

This email and any attached files are confidential and copyright protected. If you are not the addressee, any dissemination of this communication is strictly prohibited.

Unless otherwise expressly agreed in writing, nothing stated in this communication shall be

legally binding.

The ultimate parent company of the Atkins Group is WS Atkins plc. Registered in England No.

1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey KT18 5BW.

A list of

wholly owned Atkins Group companies registered in the United Kingdom can be found at:

[2]http://www.atkinsglobal.com/terms_and_conditions/index.aspx.

Please consider the environment. Please don't print this e-mail unless you really need to.

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. file://www.atkinsglobal.com/climatechange
2. http://www.atkinsglobal.com/terms_and_conditions/index.aspx

871. 1209080077.txt

#####

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@al.noaa.gov>, 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>

mail.2008

Subject: [Fwd: JOC-08-0098 - International Journal of Climatology]
Date: Thu, 24 Apr 2008 19:34:37 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear folks,

I'm forwarding an email from Prof. Glenn McGregor, the IJoC editor who is handling our paper. The email contains the comments of Reviewer #1, and notes that comments from two additional Reviewers will be available shortly.

Reviewer #1 read the paper very thoroughly, and makes a number of useful comments. The Reviewer also makes some comments that I disagree with.

The good news is that Reviewer #1 begins his review (I use this personal pronoun because I'm pretty sure I know the Reviewer's identity!) by affirming the existence of serious statistical errors in DCPS07:

"I've read the paper under review, and also DCPS07, and I think the present authors are entirely correct in their main point. DCPS07 failed to account for the sampling variability in the individual model trends and, especially, in the observational trend. This was, as I see it, a clear-cut statistical error, and the authors deserve the opportunity to present their counter-argument in print."

Reviewer #1 has two major concerns about our statistical analysis. Here is my initial reaction to these concerns.

CONCERN #1: Assumption of an AR-1 model for regression residuals.

In calculating our "adjusted" standard errors, we assume that the persistence of the regression residuals is well-described by an AR-1 model. This assumption is not unique to our analysis, and has been made in a number of other investigations. The Reviewer would "like to see at least some sensitivity check of the standard error formula against alternative model assumptions." Effectively, the Reviewer is asking whether a more complex time series model is required to describe the persistence.

Estimating the order of a more complex AR model is a tricky business. Typically, something like the BIC (Bayesian Information Criterion) or AIC (Akaike Information Criterion) is used to do this. We could, of course, use the BIC or AIC to estimate the order of the AR model that best fits the regression residuals. This would be a non-trivial undertaking. I think we would find that, for different time series, we would obtain different estimates of the "best-fit" AR model. For example, 20c3m runs without volcanic forcing might yield a different AR model order than 20c3m runs with volcanic forcing. It's also entirely likely (based on Rick Katz's experience with such AR model-fitting exercises) that the AIC- and BIC-based estimates of the AR model order could differ in some cases.

As the Reviewer himself points out, DCPS07 "didn't make any attempt to calculate the standard error of individual trend estimates and this remains the major difference between the two paper." In other words, our paired trends test incorporates statistical uncertainties for both simulated and observed trends. In estimating these uncertainties, we account for non-independence of the regression residuals. In contrast, the DCPS07 trend "consistency test" does not incorporate ANY statistical uncertainties in either observed or simulated trends. This difference in treatment of trend uncertainties is the primary issue. The issue of whether an AR-1 model is the most appropriate model to use for the

mail.2008

purpose of calculating adjusted standard errors is really a subsidiary issue. My concern is that we could waste a lot of time looking at this issue, without really enlightening the reader about key differences between our significance testing procedure and the DCPS07 approach.

One solution is to calculate (for each model and observational time series used in our paper) the parameters of an AR(K) model, where K is the total number of time lags, and then apply equation 8.39 in Wilks (1995) to estimate the effective sample size. We could do this for several different K values (e.g., K=2, K=3, and K=4; we've already done the K=1 case). We could then very briefly mention the sensitivity of our "paired trend" test results to choice of order K of the AR model. This would involve some work, but would be easier to explain than use of the AIC and BIC to determine, for each time series, the best-estimate of the order of the AR model.

CONCERN #2: No "attempt to combine data across model runs."

The Reviewer is claiming that none of our model-vs-observed trend tests made use of data that had been combined (averaged) across model runs. This is incorrect. In fact, our two modified versions of the DCPS07 test (page 29, equation 12, and page 30, equation 13) both make use of the multi-model ensemble-mean trend.

The Reviewer argues that our paired trends test should involve the ensemble-mean trends for each model (something which we have not done) rather than the trends for each of 49 individual 20c3m realizations. I'm not sure whether the rationale for doing this is as "clear-cut" as the Reviewer contends.

Furthermore, there are at least two different ways of performing the paired trends tests with the ensemble-mean model trends. One way (which seems to be what the Reviewer is advocating) involves replacing in our equation (3) the standard error of the trend for an individual realization performed with model A with model A's intra-ensemble standard deviation of trends. I'm a little concerned about mixing an estimate of the statistical uncertainty of the observed trend with an estimate of the sampling uncertainty of model A's trend.

Alternately, one could use the average (over different realizations) of model A's adjusted standard errors, or the adjusted standard error calculated from the ensemble-mean model A time series. I'm willing to try some of these things, but I'm not sure how much they will enlighten the reader. And they will not help to make an already-lengthy manuscript any shorter.

The Reviewer seems to be arguing that the main advantage of his approach #2 (use of ensemble-mean model trends in significance testing) relative to our paired trends test (his approach #1) is that non-independence of tests is less of an issue with approach #2. I'm not sure whether I agree. Are results from tests involving GFDL CM2.0 and GFDL CM2.0 temperature data truly "independent" given that both models were forced with the same historical changes in anthropogenic and natural external forcings? The same concerns apply to the high- and low-resolution versions of the MIROC model, the GISS models, etc.

I am puzzled by some of the comments the Reviewer has made at the top of page 3 of his review. I guess the Reviewer is making these comments in the context of the pair-wise tests described on page 2. Crucially, the comment that we should use "...the standard error if testing the average model trend" (and by "standard error" he means DCPS07's $\sigma\{SE\}$) IS INCONSISTENT with the Reviewer's approach #3, which involves use of the inter-model standard deviation in testing the average model trend.

mail.2008

And I disagree with the Reviewer's comments regarding the superfluous nature of Section 6. The Reviewer states that, "when simulating from a know (statistical) model... the test statistics should by definition give the correct answer. The whole point of Section 6 is that the DCPS07 consistency test does NOT give the correct answer when applied to randomly-generated data!

In order to satisfy the Reviewer's curiosity, I'm perfectly willing to repeat the simulations described in Section 6 with a higher-order AR model. However, I don't like the idea of simulation of synthetic volcanoes, etc. This would be a huge time sink, and would not help to illustrate or clarify the statistical mistakes in DCPS07.

It's obvious that Reviewer #1 has put a substantial amount of effort into reading and commenting on our paper (and even performing some simple simulations). I'm grateful for the effort and the constructive comments, but feel that a number of comments are off-base. Am I misinterpreting the Reviewer's comments?

With best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

Attachment Converted: "c:\eudora\attach\ - santerreport.pdf"
X-Account-Key: account1
Return-Path: <g.mcgregor@auckland.ac.nz>
Received: from mail-1.llnl.gov ([unix socket])
by mail-1.llnl.gov (Cyrus v2.2.12) with LMTPA;
Thu, 24 Apr 2008 12:47:37 -0700
Received: from smtp.llnl.gov (nspiron-3.llnl.gov [128.115.41.83])
by mail-1.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with ESMTMP id
m30Jlzk7028016
for <santer1@mail.llnl.gov>; Thu, 24 Apr 2008 12:47:37 -0700
X-Attachments: - santerreport.pdf
X-IronPort-AV: E=McAfee;i="5200,2160,5281"; a="32776528"
X-IronPort-AV: E=Sophos;i="4.25,705,1199692800";
d="pdf'?scan'208";a="32776528"
Received: from nsziron-3.llnl.gov ([128.115.249.83])
by smtp.llnl.gov with ESMTMP; 24 Apr 2008 12:47:36 -0700
X-Attachments: - santerreport.pdf
X-IronPort-AV: E=McAfee;i="5200,2160,5281"; a="36298571"
X-IronPort-AV: E=Sophos;i="4.25,705,1199692800";
d="pdf'?scan'208";a="36298571"
Received: from uranus.scholarone.com ([170.107.181.135])
by nsziron-3.llnl.gov with ESMTMP; 24 Apr 2008 12:47:34 -0700
Received: from tss1be0004 (tss1be0004 [10.237.148.27])
by uranus.scholarone.com (Postfix) with SMTP id 8F0554F44D5
for <santer1@llnl.gov>; Thu, 24 Apr 2008 15:47:33 -0400 (EDT)
Message-ID: <379866627.1209066453582.JavaMail.wladmin@tss1be0004>

mail.2008

Date: Thu, 24 Apr 2008 15:47:33 -0400 (EDT)
From: g.mcgregor@auckland.ac.nz
To: santer1@llnl.gov
Subject: JOC-08-0098 - International Journal of Climatology
Errors-To: masmith@wiley.co.uk
Mime-Version: 1.0
Content-Type: multipart/mixed;
boundary="-----=_Part_678_379761858.1209066453554"
X-Errors-To: masmith@wiley.co.uk
Sender: onbehalf@scholarone.com

24-Apr-2008

JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere

Dear Dr Santer

I have received one set of comments on your paper to date. Although I would normally wait for all comments to come in before providing them to you, I thought in this case I would give you a head start in your preparation for revisions. Accordingly please find attached one set of comments. Hopefully I should have two more to follow in the near future.

Best,

Prof. Glenn McGregor

Attachment Converted: "c:\eudora\attach\ - santerreport1.pdf"

872. 1209143958.txt

#####

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@al.noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.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: [Fwd: Re: JOC-08-0098 - International Journal of Climatology]
Date: Fri, 25 Apr 2008 13:19:18 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear folks,

On April 11th, I received an email from Prof. Glenn McGregor at IJoC. I am now forwarding that email, together with my response to Prof. McGregor.

Prof. McGregor's email asks for my opinion of an "Addendum" to the original DCPS07 IJoC paper. The addendum is authored by Douglass, Christy, Pearson, and Singer. As you can see from my reply to Prof. McGregor, I do not think that the Addendum is worthy of publication. Since one part of the Addendum deals with issues related to the RAOBCORE data used by DCPS07 (and by us), Leo responded to Prof. McGregor on this point. I will forward Leo's response in a separate email.

mail.2008

The Addendum does not reference our IJoC paper. As far as I can tell, the Addendum represents a response to discussions of the original IJoC paper on RealClimate.org. Curiously, Douglass et al. do not give a specific source for the criticism of their original paper. This is rather bizarre. Crucially, the Addendum does not recognize or admit ANY ERRORS in the original DCPS07 paper.

I have not yet heard whether IJoC intends to publish the Addendum. I'll update you as soon as I have any further information from Prof. McGregor.

With best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

Attachment Converted: "c:\eudora\attach\[Fwd Re JOC-08-0098 - Interna.pdf"
X-Account-Key: account1
Return-Path: <santer1@llnl.gov>
Received: from mail-1.llnl.gov ([unix socket])
by mail-1.llnl.gov (Cyrus v2.2.12) with LMTPA;
Fri, 11 Apr 2008 11:19:24 -0700
Received: from smtp.llnl.gov (nspiron-3.llnl.gov [128.115.41.83])
by mail-1.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with ESMTF id
m3BIJN5F012995
for <santer1@mail.llnl.gov>; Fri, 11 Apr 2008 11:19:24 -0700
X-Attachments: None
X-IronPort-AV: E=MCAfee;i="5200,2160,5272"; a="31695223"
X-IronPort-AV: E=Sophos;i="4.25,642,1199692800";
d="scan'208";a="31695223"
Received: from dione.llnl.gov (HELO [128.115.57.29]) ([128.115.57.29])
by smtp.llnl.gov with ESMTF; 11 Apr 2008 11:14:37 -0700
Message-ID: <47FFAA8D.8040308@llnl.gov>
Date: Fri, 11 Apr 2008 11:14:37 -0700
From: Ben Santer <santer1@llnl.gov>
Reply-To: santer1@llnl.gov
Organization: LLNL
User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
MIME-Version: 1.0
To: g.mcgregor@auckland.ac.nz
CC: Leopold Haimberger <leopold.haimberger@univie.ac.at>,
"Thorne, Peter" <peter.thorne@metoffice.gov.uk>
Subject: Re: JOC-08-0098 - International Journal of Climatology
References: <363780847.1207875178234.JavaMail.wladmin@tss1be0004>
In-Reply-To: <363780847.1207875178234.JavaMail.wladmin@tss1be0004>
Content-Type: text/plain; charset=ISO-8859-1; format=flowed
Content-Transfer-Encoding: 7bit

<x-flowed>

Dear Prof. McGregor,

Thank you for your email, and for your efforts to ensure rapid review of our paper.

Leo Haimberger (who has led the development of the RAOBCORE* datasets) and Peter Thorne would be best placed to comment on the first issue raised by the Douglass et al. "Addendum". As we show in Figure 6 of our IJoC paper, recently-developed radiosonde datasets which do not rely on reanalysis data for correction of inhomogeneities (such as the Sherwood et al. IUK product and the Haimberger et al. "RICH" dataset) yield vertical profiles of atmospheric temperature change that are in better agreement with model results, and quite different from the profiles shown by Douglass et al.

The second issue raised in the Douglass et al. "Addendum" is completely spurious. Douglass et al. argue that their "experimental design" involves "comparing like to like", and satisfying "the critical condition that the model surface temperatures match the observations". If this was indeed their experimental design, Douglass et al. should have examined "AMIP" (Atmospheric Model Intercomparison Project) simulations, in which an atmospheric model is run with prescribed changes in observed time-varying sea-surface temperatures (SSTs) and sea-ice distributions. Use of AMIP simulations would allow an analyst to compare simulated and observed tropospheric temperature changes given the same underlying changes in SSTs.

But Douglass et al. did NOT consider results from AMIP simulations, even though AMIP data were freely available to them (AMIP data were in the same "CMIP-3" archive that Douglass et al. accessed in order to obtain the model results analyzed in their original IJoC paper). Instead, Douglass et al. examined results from coupled model simulations. As we discuss at length in Section 3 of our paper, coupled model simulations are fundamentally different from AMIP runs. A coupled model is NOT driven by observed changes in SSTs, and therefore would not have (except by chance) the same SST changes as the real world over a specific period of time.

Stratifying the coupled model results by the observed surface temperature changes is not a meaningful or useful thing to do, particularly given the small ensemble sizes available here. Again, if Douglass et al. were truly interested in imposing "the critical condition that the model surface temperatures match the observations", they should have examined AMIP runs, not coupled model results.

I also note that, although Douglass et al. stipulate their "critical condition that the model surface temperatures match the observations", they do not actually perform any stratification of the model trend results! In other words, Douglass et al. do NOT discard simulations with surface trends that differ from the observed trend. They simply note that the MODEL AVERAGE surface trend is close to the observed surface trend, and state that this agreement in surface trends allows them to evaluate whether the model average upper air trend is consistent with observed upper air trends.

The Douglass et al. "Addendum" does nothing to clarify the serious statistical flaws in their paper. Their conclusion - that modelled and observed upper air trends are inconsistent - is simply wrong. As we point out in our paper, Douglass et al. reach this incorrect conclusion by ignoring uncertainties in observed and modelled upper air trends arising from interannual variability, and by applying a completely inappropriate "consistency test". Our Figure 5 clearly shows that the Douglass et al. "consistency test" yields incorrect results. The "Addendum" does not suggest that the authors are capable of recognizing or understanding the errors inherent in either their "experimental

mail.2008

method" or their "consistency test".

The Douglass et al. IJoC paper reached a radically different conclusion from the conclusions reached by Santer et al. (2005), the 2006 CCSP report, the 2007 IPCC report, and Thorne et al. (2007). It did so on the basis of essentially the same data used in previous work. Most scientists would have asked whether the "consistency test" which yielded such startlingly different conclusions was appropriate. They would have applied this test to synthetic data, to understand its behaviour in a controlled setting. They would have applied alternative tests. They would have done everything they possibly could to examine the robustness of their findings. Douglass et al. did none of these things.

I will ask Leo Haimberger and Peter Thorne to respond to you regarding the first issue raised in the Douglass et al. "Addendum".

Best regards,

Ben Santer

(* In their addendum, Douglass et al. erroneously refer to "ROABCORE" datasets. One would hope that they would at least be able to get the name of the dataset right.)

g.mcgregor@auckland.ac.nz wrote:

> 10-Apr-2008

>

> JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere

>

> Dear Dr Santer

>

> Just to let you know that I am trying to secure reviews of your paper asap.

>

> I have attached an addendum for the Douglass et al. paper recently sent to me by David Douglass. I would be interested to learn of your views on this

>

>

> Best,

>

> Prof. Glenn McGregor

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

873. 1209474516.txt

#####

mail.2008

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [Fwd: Talk on Understanding 20th C surface temperature variability]
Date: Tue Apr 29 09:08:36 2008
Cc: Ben Santer <santer1@llnl.gov>

Tom,
Here's what I sent Kevin yesterday. Still don't have the proofs with Figures in. It is most odd how this Cambridge seminar has been so widely publicised. Michael McIntyre seems to be sending it everywhere. Dave Thompson is on a sabbatical in the UK for 6 months (at Reading). Should be here soon for a visit to CRU.
The press release is very much work in progress. Appended the latest version at the end. This version still need some work. Maybe I'll get a chance later today.
cc'd Ben as if and when (hopefully) the 'where Douglass et al went wrong' paper comes out a press release then would be useful. In both cases, there is a need to say things in plain English and not the usual way we write.
For some reason the skeptics (CA) are revisiting the Douglass et al paper. A very quick look shows that a number think the paper is wrong!
There is also a head of steam being built up (thanks to a would be Australian astronaut who knows nothing about climate) about the drop in temperature due to La Nina. If you've time look at the HadCRUT3 plot for March08. It was the warmest ever for NH land. The snow cover plots at Rutgers are interesting also. Jan08 for Eurasia had the most coverage ever, but March08 had the least (for their respective months).
It seems we just need the La Nina to finally wind down and the oceans to warm up a little. The press release could be an issue, as it looks as though we are underestimating SST with the buoys - by about 0.1 deg C.
Cheers
Phil

Using a novel technique to remove the effects of temporary fluctuations in global temperature due to El Niño and transient weather patterns, researchers at Colorado State University, the University of Washington, the UK Met Office and the University of East Anglia have highlighted a number of sudden drops in global temperature.

Most of these drops coincide with the eruptions of large tropical volcanoes and are also evident in air temperatures measured over the worlds land areas, but the largest, occurring towards the end of 1945, is unrelated to any known volcanic eruption and is not apparent over land. It appears to arise from an artificial and temporary cooling caused by an abrupt change in the mix of US and UK ships reporting temperatures at the end of the second world war.

The majority of sea temperature measurements available in international data bases between 1941 and 1945 are from US ships. Far fewer data are available in this period than in the 1930s and the 1950s. The crews of US ships measured the temperature of the water

before it

was used to cool the ships engine. Because of warmth coming from the ship, the water was often a little warmer than the true sea temperature. At the end of 1945 the number of US observations in the data base dropped rapidly. At the same time the number of UK observations increased. UK ships measured the temperature of water samples collected using special buckets. Wind blowing past the buckets as they were hauled onto the deck often caused these measurements to be cooler than the actual sea temperature. The sudden change from US (engine room) to UK (bucket) measurements from warmer to cooler is what caused the abruptness of the drop.

Although the drop in 1945 was large in climate-change terms about 0.3°C its full effect is likely to be limited to the period immediately after the Second World War, because by the 1960s better-insulated buckets were coming into use and there was a more varied mix of measurements from different national merchant shipping fleets. Because it occurs in the middle of the century it will have little effect on 20th Century warming trends, which are corroborated by independent records of air temperatures taken over both land and sea.

Climate researchers at the Met Office Hadley Centre are working to reduce the biases in the temperature datasets. In the past two years, many hundreds of thousands of observations have been keyed in from hand-written log books that were kept aboard ships in the UK navy, particularly for the periods of sparse marine coverage, such as the two world war periods.

Although fixing the drop is unlikely to radically alter our understanding of climate change, having a more accurate record of the real temperature change during the mid-20th century could provide insight into the more subtle mechanisms that caused the early rise in temperatures to the 1920s and the subsequent flattening of the temperature curve that lasted into the early 1970s.

Marine temperatures are much more prone to systematic biases arising from changes in the way the measurements are taken and the platforms used, than are land air temperatures. For example, since the 1970s, sea surface temperatures have been estimated from satellites, but these need considerable adjustment (sometimes in excess of 2 deg C) to be comparable with ship and buoy measurements. The satellite sees only the top millimetre of the ocean surface, while traditional ship-based sampling sees the top few metres. A change is gradually taking place across the world's oceans in the way sea surface temperature measurements are made during the last ten years: the number of ship-based measurements has

mail.2008

reduced slightly, but there is a dramatic increase in the number of measurements coming from automatic measurements taken on fixed and drifting buoys. Work is underway to determine the size of the difference between the ships and buoys, as the bias between the two could be of the same order as that in the 1940s.

Kevin,

Odd how far and wide Cambridge seminars are advertised!

Dave Thompson has given this talk at Reading and will be here tomorrow for a similar talk. Here's an email I sent earlier to someone in London.

I'm on the Nature paper - due out end of May/early June.

Attached the draft press release as well.

Any thoughts welcome. I hope you'll see how all this could be misinterpreted!

Cheers

Phil

Chris,

David Thompson is giving a talk here tomorrow on this.

The essence of his talk will be in Nature in a few weeks time.

The skeptics will make a meal of this when it

comes out, but if they did their job properly (I know this is impossible!) they would

have found it. It relates to a problem with SST data in the late 1940s. The problem will get corrected for at some point. SSTs need adjusting as there must

be

from buckets for the period from Aug45 by about 0.3 gradually reducing to a zero adjustment by about the mid-1960s. The assumption was that after WW2

they were

all intake measurements and didn't need adjusting.

This will reduce the 1940-1970 cooling in NH temps. Explaining the cooling with sulphates won't be quite as necessary. It won't change century-scale

trends.

There is much more of an interesting thing going on now. With all the drifters now deployed measuring SST, the % of ships making measurements is now only about 40% of the total - whereas it was all in the late 1990s. In

comparisons

over the last 10 years it seems that ships measure SSTs about 0.1-0.2 higher than the drifters/buoys. As the 61-90 base period is ship based, it means recent anomalies are colder than they should be (by about 0.1 for global mean T in the last 2 years).

Working on a press release with MOHC about the Nature paper.

We've been through page proofs with Nature, but these don't yet include figs.

I can send these when we get them.

Cheers

Phil

At 15:02 28/04/2008, you wrote:

Phil

Any idea what this is about?

Kevin

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

Subject: Talk on Understanding 20th C surface temperature variability

Date: Mon, 28 Apr 2008 12:00:36 +0100 (BST)

From: Leverhulme Climate Symposium [1]<climate@esc.cam.ac.uk>

Reply-To: [2]climate@esc.cam.ac.uk

To: [3]climate@esc.cam.ac.uk

Dear Colleagues,

David Thompson of Colorado State University will be speaking in Cambridge on 22 May on 'Understanding 20th century surface temperature variability'. His talk will 'highlight a glaring but previously overlooked error in the time series of global-mean temperatures', see full abstract below. (For those too far from Cambridge to attend, this is for information and

mail.2008

interest).

The prevailing view of 20th century temperature variability is that the Earth warmed from ~1910 to 1940, cooled slightly from ~1940 to 1970, and warmed markedly from ~1970 onward. In this talk I will exploit a physically-based filtering methodology which provides an alternative interpretation of 20th century global-mean temperature variability. The results clarify the consistency between the century-long monotonic rise in greenhouse gases and global-mean temperatures, provide new insights into the climatic impact of volcanic eruptions, and highlight a glaring but previously overlooked error in the time series of global-mean temperatures.

Thursday 22 May, 2.15 pm in Meeting Room 2, Centre for Mathematical Sciences (between Clarkson and Madingley Roads)

--

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

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:climate@esc.cam.ac.uk>
2. <mailto:climate@esc.cam.ac.uk>
3. <mailto:climate@esc.cam.ac.uk>
4. <mailto:trenbert@ucar.edu>
5. <http://www.cgd.ucar.edu/cas/trenbert.html>

874. 1210030332.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: g.mcgregor@auckland.ac.nz
Subject: Re: JOC-08-0098 - International Journal of Climatology
Date: Mon, 05 May 2008 19:32:12 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Glenn,

This is a little disappointing. We decided to submit our paper to IJoC in order to correct serious scientific errors in the Douglass et al. IJoC paper. We believe that there is some urgency here. Extraordinary

mail.2008

claims are being made regarding the scientific value of the Douglass et al. paper, in part by co-authors of that paper. One co-author (S. Fred Singer) has used the findings of Douglass et al. to buttress his argument that "Nature not CO2, rules the climate". The longer such erroneous claims are made without any form of scientific rebuttal, the more harm is caused.

In our communications with Dr. Osborn, we were informed that the review process would be handled as expeditiously as possible. Had I known that it would take nearly two months until we received a complete set of review comments, I would not have submitted our paper to IJOc.

with best regards,

Ben Santer

g.mcgregor@auckland.ac.nz wrote:

> 05-May-2008

>

> JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere

>

> Dear Dr Santer

>

> I am hoping to have the remaining set of comments with 2 weeks of so. As soon as I have these in hand I will pass them onto to you.

>

> Best,

>

> Prof. Glenn McGregor

>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

875. 1210079946.txt

#####

From: Tim Osborn <t.osborn@uea.ac.uk>
To: g.mcgregor@auckland.ac.nz
Subject: Re: JOC-08-0098 - International Journal of Climatology
Date: Tue May 6 09:19:06 2008

Hi Glenn -- I hope the slow reviewer is not one that I suggested! Sorry if it is. I'm not sure what Ben Santer expects you to do about it at this stage; I guess you didn't expect such a lengthy article... I've not seen it, but Phil Jones told me it ran to around 90

mail.2008

pages! Hope all's well in NZ. Tim
At 03:32 06/05/2008, Ben Santer wrote:

Dear Glenn,
This is a little disappointing. We decided to submit our paper to IJoC in order to correct serious scientific errors in the Douglass et al. IJoC paper. We believe that there is some urgency here. Extraordinary claims are being made regarding the scientific value of the Douglass et al. paper, in part by co-authors of that paper. One co-author (S. Fred Singer) has used the findings of Douglass et al. to buttress his argument that "Nature not CO2, rules the climate". The longer such erroneous claims are made without any form of scientific rebuttal, the more harm is caused. In our communications with Dr. Osborn, we were informed that the review process would be handled as expeditiously as possible. Had I known that it would take nearly two months until we received a complete set of review comments, I would not have submitted our paper to IJoC.
With best regards,
Ben Santer
g.mcgregor@auckland.ac.nz wrote:

05-May-2008
JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere
Dear Dr Santer
I am hoping to have the remaining set of comments with 2 weeks of so. As soon as I have these in hand I will pass them onto to you.
Best,
Prof. Glenn McGregor

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

876. 1210178552.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Cater Sandra Mrs \ (FIN\)" <S.Cater@uea.ac.uk>, "Meardon Fiona Miss \ (RBS\)" <F.Meardon@uea.ac.uk>, "Meldrum Alicia Dr \ (RBS\)" <A.Meldrum@uea.ac.uk>
Subject: RE: Request for Cost date for DOE Grant
Date: Wed May 7 12:42:32 2008

Sandra,

mail.2008

These will be fine. Keep a note of these in the file to check against when the later claims are made.

Cheers
Phil

At 12:08 07/05/2008, Cater Sandra Mrs \ (FIN\) wrote:

Dear Phil,

I have reconciled the account to date and propose to send the following figures
all in US\$

Received to date	1,589,632.00
2007/08	
Staff buyout Jones	71,708.00
Cons actual to date	9,650.00
Travel actual to date	6,940.00
Indirect costs on above	66,200.00
Total to 30/04/08	1,744,130.00

April to June 08	
Staff Jones	19,290.00
Cons	10,550.00 includes some of the previous year
under spend	
Travel	3,840.00 as above
Indirect costs	25,200.00
Total	58,880.00

July to Sep 08	
Staff Jones	19,290.00
Cons	3,200.00 includes some previous under spend
Travel	4,500.00 as above
Indirect costs	20,200.00
Total	47,190.00

These figures keep within the allocated budget. Please let me know if you agree
this I will e-mail Catherine.

Regards
Sandra

Sandra M Cater
Office Supervisor
Finance Research
Registry Building
University of East Anglia
Norwich
NR 4 7TJ
Tel : 0044-1603-593216
Fax : 0044-1603-593860
e-mail: s.cater@uea.ac.uk

From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]
Sent: Thursday, May 01, 2008 9:44 AM
To: Meardon Fiona Miss (RBS); Meldrum Alicia Dr (RBS); Cater Sandra Mrs (FIN)
Page 153

mail.2008

Subject: Fwd: Request for Cost date for DOE Grant

Alicia, Fiona, Sandra,
Hope this doesn't take too long to work out and send to Catherine.
If you need any help let me know.
Cheers
Phil

X-Server-Uid: F0E03B37-707C-4DCF-A928-7EECE47830F0

Subject: Request for Cost date for DOE Grant

Date: Wed, 30 Apr 2008 13:44:38 -0500

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: Request for Cost date for DOE Grant

Thread-Index: Aciq8j7EoosKEL4QQ9OUgErATV9ppA==

From: "Richardson, Catherine" <Catherine.Richardson@ch.doe.gov>

To: p.jones@uea.ac.uk

X-OriginalArrivalTime: 30 Apr 2008 18:44:39.0681 (UTC)

FILETIME=[3F0EEF10:01C8AAF2]

X-WSS-ID: 640661D233S4167282-01-01

X-Canit-CHI2: 0.00

X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)

X-Spam-Score: 0.00 () [Tag at 5.00] HTML_MESSAGE

X-CanItPRO-Stream: UEA:f028 (inherits from

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

X-Canit-Stats-ID: 2299780 - 2e3481b4882c (trained as not-spam)

X-Antispam-Training-Forget:

[2]<https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=f>

X-Antispam-Training-Nonspam:

[3]<https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=n>

X-Antispam-Training-Spam:

[4]<https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=s>

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

X-UEA-Spam-Score: 0.0

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Fiona Meardon

East Anglia University

Dear Grantee:

SUBJECT: REQUEST FOR COST INFORMATION

In accordance with the Presidents Management Agenda, there has been and continues to be a Government-wide movement to ensure that the American people receive better results for their money. Thus, all government entities are striving to improve the quality, accuracy, and timeliness of financial information regarding the results of operations and overall performance. As we seek to accomplish this goal, we are requesting cost data from our Grant recipients that have received significant financial assistance monies from the Department of Energy Office of Science - Chicago Office. The requested information, summarized below, will assist in our continuing efforts to ensure that we produce accurate and timely financial information. We need your assistance in the following areas:

A. Providing Cumulative Cost Data:

For most of the awards administered by the Office of Science - Chicago Office, there is a financial reporting requirement to submit cost data on the Financial Status Report (SF-269) at the end of the project period. Currently, there is no requirement for you to submit cost data on a more frequent basis. However, in order to achieve our goal of improving the quality, accuracy, and timeliness of our financial information, the Departments external independent auditors have insisted that we confirm cumulative cost balances with Grantees that have received significant financial assistance monies at least annually. For each grant award listed, we request that you provide the following:

DOE Grant Award(s) No.

1.

Cumulative actual Cost through March 31, 2008 (from inception of the award):

2.

Your best estimate for costs to be incurred for April through June 30, 2008:

3.

Your best estimate for costs to be incurred for July through September 30, 2008:

We are not requiring a specific or formal format for the requested information. Instead, please e-mail your cost data as requested above for each identified grant award to Catherine Richardson at [5]catherine.richardson@ch.doe.gov. Please direct your comments and/or questions to Ms. Richardson at 630/252-6276.

B. Requesting Advances and Reimbursements:

Consistent with our efforts to improve the Departments financial information, we are reviewing significant unpaid balances on our financial assistance awards as well as any credit balances on the Quarterly Federal Cash Transactions Reports (SF-272) which would

mail.2008

indicate a delay between the performance of the work and the requests for reimbursements submitted to us from your organization. The Departments external auditors and other users of financial information are concluding that these unpaid balances may not be used and possibly should be withdrawn. Therefore, we request that you:

- Review your existing procedures for requesting advances and reimbursements from DOE; and
- Ensure that the delay between the performance of work and subsequent reimbursements is as minimal as administratively possible.

If this situation does not apply to your organization, no action is required on your part.

We appreciate your support in this important initiative. If you have any questions, please call Cornell Williams at 630/252-2394 or e-mail him at [6]cornell.williams@ch.doe.gov.

Catherine Richardson
Staff Accountant
US Department of Energy
Office of Science - Chicago Office
(630)252-6276

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

Prof. Phil Jones
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:p.jones@uea.ac.uk
2. https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=f
3. https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=n
4. https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=s
5. mailto:catherine.richardson@ch.doe.gov
6. mailto:cornell.williams@ch.doe.gov

877. 1210341221.txt

#####

mail.2008

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@meteo.psu.edu>, "raymond s. bradley" <rbradley@geo.umass.edu>
Subject: A couple of things
Date: Fri May 9 09:53:41 2008
Cc: "Caspar Ammann" <ammann@ucar.edu>

Mike, Ray, Caspar,

A couple of things - don't pass on either.

1. Have seen you're RC bet. Not entirely sure this is the right way to go, but it will drum up some discussion.

Anyway Mike and Caspar have seen me present possible problems with the SST data (in the 1940s/50s and since about 2000). The first of these will appear in Nature on May 29. There should be a News and Views item with this article by Dick Reynolds. The paper concludes by pointing out that SSTs now (or since about 2000, when the effect gets larger) are likely too low. This likely won't get corrected quickly as it really needs more overlap to increase confidence. Bottom line for me is that it appears SSTs now are about 0.1 deg C too cool globally. Issue is that the preponderance of drifters now (which measure SST better but between 0.1 and 0.2 lower than ships) mean anomalies are low relative to the ship-based 1961-90 base.

This also means that the SST base the German modellers used in their runs was likely too warm by a similar amount. This applies to all modellers, reanalyses etc.

There will be a lot of discussion of the global T series with people saying we can't

even measure it properly now.

The 1940s/50s problem with SSTs (the May 29 paper) also means there will be warmer SSTs for about 10 years. This will move the post-40s cooling to a little later - more in line with higher sulphate aerosol loading in the late 50s and 1960s/70s.

The paper doesn't provide a correction. This will come, but will include the addition of loads more British SSTs for ww2, which may very slightly cool the ww2 years. More British SST data have also been digitized for the late 1940s. Budget constraints mean that only about half the RN log books have been digitized.

Emphasis has been given to the South Atlantic and Indian Ocean log books.

As an aside, it is unfortunate that there are few in the Pacific. They have digitized

all the logbooks of the ships journeys from the Indian Ocean south of Australia and NZ

to Seattle for refits. Nice bit of history here - it turns out that most of the ships are

US ones the UK got under the Churchill/Roosevelt deal in early 1940. All the RN bases

in South Africa, India and Australia didn't have parts for these ships for a few years.

So the German group would be stupid to take your bet. There is a likely ongoing negative volcanic event in the offing!

2. You can delete this attachment if you want. Keep this quiet also, but this is the person who is putting in FOI requests for all emails Keith and Tim have written and received re Ch 6 of AR4. We think we've found a way around this.

I can't wait for the wengen review to come out with the Appendix showing what that 1990 IPCC Figure was really based on.

The Garnaut review appears to be an Australian version of the Stern Report.

This message will self destruct in 10 seconds!

Cheers

Phil

Prof. Phil Jones

mail.2008
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

878. 1210367056.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "raymond s. bradley" <rbradley@geo.umass.edu>
Subject: Re: A couple of things
Date: Fri May 9 17:04:16 2008

Hi Ray,
Press release has been being written!
I can't seem to find a meeting to go to when the paper comes out!
Moorea was good - hope you'll be able to get to Athens!
Cheers
Phil

At 16:56 09/05/2008, you wrote:

Hi Phil:
I think you should issue your own carefully-worded press release, stating
explicitly what
your results DO NOT mean, as well as what they do...otherwise you will spend
the next
few weeks trying to undo a lot of unwanted press coverage.
Hope all is well with you....we need to get together at some place...sorry I
missed
Tahiti!

ray
At 04:53 AM 5/9/2008, you wrote:

Mike, Ray, Caspar,

A couple of things - don't pass on either.
1. Have seen you're RC bet. Not entirely sure this is the right way to go,
but it will drum up some discussion.
Anyway Mike and Caspar have seen me present possible problems with the
SST data (in the 1940s/50s and since about 2000). The first of these will
appear
in Nature on May 29. There should be a News and Views item with this article
by Dick Reynolds. The paper concludes by pointing out that SSTs now (or since
about 2000, when the effect gets larger) are likely too low. This likely won't
get corrected quickly as it really needs more overlap to increase confidence.
Bottom line for me is that it appears SSTs now are about 0.1 deg C too cool
globally. Issue is that the preponderance of drifters now (which measure SST
better but between 0.1 and 0.2 lower than ships) mean anomalies are low
relative to the ship-based 1961-90 base.
This also means that the SST base the German modellers used in their runs
was likely too warm by a similar amount. This applies to all modellers,
reanalyses etc.
There will be a lot of discussion of the global T series with people saying we
can't
even measure it properly now.
The 1940s/50s problem with SSTs (the May 29 paper) also means there will be
warmer SSTs for about 10 years. This will move the post-40s cooling to a
little

mail.2008

later - more in line with higher sulphate aerosol loading in the late 50s and 1960s/70s.

The paper doesn't provide a correction. This will come, but will include the addition

of loads more British SSTs for WW2, which may very slightly cool the WW2 years.

More British SST data have also been digitized for the late 1940s. Budget constraints mean that only about half the RN log books have been digitized.

Emphasis has been given to the South Atlantic and Indian Ocean log books.

As an aside, it is unfortunate that there are few in the Pacific. They have digitized

all the logbooks of the ships journeys from the Indian Ocean south of Australia and NZ

to Seattle for refits. Nice bit of history here - it turns out that most of the ships

are

US ones the UK got under the Churchill/Roosevelt deal in early 1940. All the RN bases

in South Africa, India and Australia didn't have parts for these ships for a few years.

So the German group would be stupid to take your bet. There is a likely ongoing negative volcanic event in the offing!

2. You can delete this attachment if you want. Keep this quiet also, but this is the person who is putting in FOI requests for all emails Keith and Tim have written and received re Ch 6 of AR4. We think we've found a way around this.

I can't wait for the Wengen review to come out with the Appendix showing what that 1990 IPCC Figure was really based on.

The Garnaut review appears to be an Australian version of the Stern Report.

This message will self destruct in 10 seconds!

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

Raymond S. Bradley

Director, Climate System Research Center*

Department of Geosciences, University of Massachusetts

Morrill Science Center

611 North Pleasant Street

AMHERST, MA 01003-9297

Tel: 413-545-2120

Fax: 413-545-1200

*Climate System Research Center: 413-545-0659

< [1]<http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [2]<http://www.geo.umass.edu/climate/paleo/html>

Publications (download .pdf files):

[3]<http://www.geo.umass.edu/faculty/bradley/bradleypub.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.paleoclimate.org/>
2. <http://www.geo.umass.edu/climate/paleo/html>
3. <http://www.geo.umass.edu/faculty/bradley/bradleypub.html>

879. 1210695733.txt

#####

From: David Helms <David.Helms@noaa.gov>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: Second review of IJoC paper
Date: Tue, 13 May 2008 12:22:13 -0400
Cc: santer1@llnl.gov, "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>, ssolomon@frii.com, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, 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>, Bruce Baker <Bruce.Baker@noaa.gov>, David Helms <David.Helms@noaa.gov>, William R Moninger <William.R.Moninger@noaa.gov>, Bradley Ballish <Bradley.Ballish@noaa.gov>, Ralph Petersen <ralph.petersen@ssec.wisc.edu>, "Grooters, Frank" <Frank.Grooters@knmi.nl>, Carl Weiss <Carl.Weiss@noaa.gov>, Michael Berechree <M.Berechree@bom.gov.au>

<x-flowed>
Hi Tom,

I believe NCEP has found that, generally speaking, the AMDAR/MDCRS and radiosonde temperatures are treated in a similar fashion in assimilation. Like radiosonde which has varying performance from vendor to vendor, there are differences in performance between aircraft/series and temperature probes. Brad Ballish just had a paper approved for publication (in BAMS?) that identifies the performance differences between air carriers, aircraft type, and aircraft series. Unfortunately, we only know how the data compare with the model guess, but not necessarily absolute "truth". Hopefully Brad can share his paper with this distribution. Bill Moninger and Ralph Petersen may also have published recent papers on this issue they can share. Ralph has published papers that compare near simultaneously launched of Vaisala RS-92 sondes with ascending/descending B-757 aircraft, showing good data agreement.

One should be mindful of the potential advantages of including AMDAR data as a climate resource in addition to radiosonde.

1. Data has been available in quantity since 1992
2. Data does not have the radiation issue as the TAT probe is shielded
3. Data are available at all local times, nearly 24*7*365, at hundreds of major airports internationally, thereby supporting the climate diurnal temperature problem
4. All NMCs keep databases of individual aircraft bias, based on recent performance of the each aircraft's data verses the model guess. These information would be very useful in considering candidate aircraft for a "climate quality" long term database for AMDAR temperature data

I suspect that the reason why AMDAR data have not been used to track atmospheric change is because no-one in the climate community has ever made an effort to use these data. Availability of radiosonde data in the

mail.2008

tropics (e.g. South America and Africa) is problematic. In response, EUCOS/E-AMDAR has been adding data collection over Africa using Air France, British Airways, and Lufthansa aircraft. I have proposed expanding the U.S. data collection to include the Caribbean and South America regions from United, Delta, Continental, etc, aircraft, but have not received support for this expansion. WMO AMDAR Panel is moving to add additional regional AMDAR Programs in the developing countries, similar to the successful expansion in eastern Asia.

AMDAR data are not a replacement for radiosonde, but these data certainly can add to the climate record if the data are properly processed/QC'd.

Regards,

Dave Helms

Thomas.R.Karl wrote:

> Ben,
>
> Regarding the last comment by Francis -- Commercial aircraft data have
> not been demonstrated to be very reliable w/r to tracking changes in
> temperatures in the US. A paper by Baker a few years ago focused on US
> data showed errors in the 1C range. Not sure about the tropics and how
> many flights you could get. I have copied Bruce Baker for a copy of
> that article.
>
> Recently David Helms has been leading an effort to improve this. He
> may have more info related to global aircraft data. I will ask Bruce
> to see what data we have, just for your info.
>
> Tom
>
> P.S. Nice review by Francis, especially like his idea w/r to stat tests.

> Ben Santer said the following on 5/12/2008 9:52 PM:

>> Dear folks,

>>
>> I just received the second review of our IJoC paper (see appended PDF
>> file). This was sent to me directly by the Reviewer (Francis Zwiers).
>> Francis's comments are very thorough and constructive. They are also
>> quite positive. I don't see any show stoppers. I'll work on a
>> response this week.

>>
>> The third review is still outstanding. I queried Glenn McGregor about
>> this, and was told that we can expect the final review within the
>> next 1-2 weeks.

>> With best regards,

>>
>> Ben

>> -----

>> Benjamin D. Santer
>> Program for Climate Model Diagnosis and Intercomparison
>> Lawrence Livermore National Laboratory
>> P.O. Box 808, Mail Stop L-103
>> Livermore, CA 94550, U.S.A.
>> Tel: (925) 422-2486
>> FAX: (925) 422-7675

mail.2008

>> email: santer1@llnl.gov

>> -----
>
>
> --
>
> *Dr. Thomas R. Karl, L.H.D.*
>
> */Director/*//
>
> NOAA's National Climatic Data Center
>
> Veach-Baley Federal Building
>
> 151 Patton Avenue
>
> Asheville, NC 28801-5001
>
> Tel: (828) 271-4476
>
> Fax: (828) 271-4246
>
> Thomas.R.Karl@noaa.gov <mailto:Thomas.R.Karl@noaa.gov>
>
</x-flowed>

880. 1211040378.txt

#####

From: C.Goodess@uea.ac.uk
To: p.jones@uea.ac.uk, t.osborn@uea.ac.uk
Subject: [Fwd: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01]
Date: Sat, 17 May 2008 12:06:18 +0100 (BST)

----- Original Message -----
Subject: [Fwd: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01]
From: f034@uea.ac.uk
Date: Sat, May 17, 2008 12:04 pm
To: p.jones@uea.ac.u
t.osborn@uea.ac.uk

Can we meet on Monday to discuss this and hear from Phil what was decided at the London meeting? I'll be in late Monday (waiting for someone to look at my leaking roof) - so maybe early afternoon. I'm going down to London early evening and will be at Chelsea on tuesday. Good to see Saffron is getting some publicity!

Clare

----- Original Message -----
Subject: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01
From: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>
Date: Fri, May 16, 2008 9:06 am
To: "Jim Hall" <jim.hall@newcastle.ac.uk>
"C G Kilsby" <c.g.kilsby@newcastle.ac.uk>
"Mark New" <mark.new@ouce.ox.ac.uk>
ana.lopez@ouce.ox.ac.uk

mail.2008

"Anthony Footitt" <a.footitt@uea.ac.uk>
"Suraje Dessai" <s.dessai@uea.ac.uk>
"Phil Jones" <p.jones@uea.ac.uk>
"Clare Goodess" <C.Goodess@uea.ac.uk>
t.osborn@uea.ac.uk
Cc: "McSweeney, Robert" <Rob.Mcsweeney@atkinsglobal.com>
"Arkell, Brian" <Brian.Arkell@atkinsglobal.com>
"Sene, Kevin" <Kevin.Sene@atkinsglobal.com>

Dear all,

Please find attached the final tender pack for the Environment Agency bid. The tasks have been re-jigged, with the main change being a broadening of flood risk management to flood and coastal erosion risk management (FCERM). This means a wider audience to include all operating authorities, and the best practice guidance required (new Task 11) is now substantial element, to include evaluation of FCERM climate change adaptation, case studies and provision of evidence to help upgrade the FCDPAG3 Supplementary Note.

We have just one week to finish this tender, as it must be posted on Friday 23rd. We are putting together the bid document, which we'll circulate on Monday 19th, but in the meantime, and by the end of Tuesday 20th, I need everyone to send information (as indicated in brackets) to support the following structure:

- + Understanding of the tender
- + Methodology and programme (methodology for tasks / sub-tasks - see below - and timing)
- + Project team, including individual and corporate experience (who you are putting forward, pen portraits, corporate case studies)
- + Financial and commercial (day rates and number of days; please also highlight potential issues with the T&Cs e.g. IPR)
- + Health & Safety, Quality and Environmental Management
- + Appendices (full CVs, limited to 6 pages)

Please send to me and Rob McSweeney. The information I have already e.g. on day rates, core pen portraits etc will go straight into the version we're working on, so no need to re-send.

In terms of tasks (new nos.), the following organisation is suggested based on what has been noted to date:

- Task 1 (Inception meeting and reporting) Atkins, supported by lead representatives of partners
- Task 2 (Project board meetings) Atkins, supported by lead representatives of partners
- Task 3 (Analysis of user needs) Atkins with Tyn@UEA and OUCE, plus Futerra depending on style
- Task 4 (Phase 2 programme) Atkins, supported by all
- Task 5 (Interpret messages from UKCIP08 projections) CRU, OUCE and Newcastle, with Atkins advice on sectors
- Task 6 (Development of business specific projections) Newcastle and CRU, with Atkins advice on policy and ops
- Task 7 (Putting UKCIP08 in context) CRU, Newcastle and OUCE
- Task 8 (User guidance) Atkins, Tyn@UEA, Futerra
- Task 9 (Pilot studies) Atkins, Newcastle, OUCE, Tyn@UEA
- Task 10 (Phase 3 programme) Atkins, supported by all
- Task 11 (Best Practice Guidance for FCERM) Newcastle and Atkins, with CRU
- Task 12 (Awareness raising events) Atkins, key experts, Futerra (perhaps as an option as EA are quite specific here)

mail.2008

Task 13 (Training events) Atkins and Futerra

Note that Futerra is a communications consultancy, specialising in sustainability, who will input on workshops and on the guidance documents.

I'll be in touch again early next week.

Best wishes,

Geoff

Geoff Darch

Senior Consultant
Water and Environment
ATKINS

Broadoak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough,
PE2 6YS, UK
Tel: +44 (0) 1733 366969
Fax: +44 (0) 1733 366999
Mobile: +44 (0) 7834 507590
E-mail: geoff.darch@atkinsglobal.com
web: www.atkinsglobal.com/climate_change

This email and any attached files are confidential and copyright protected. If you are not the addressee, any dissemination of this communication is strictly prohibited. Unless otherwise expressly agreed in writing, nothing stated in this communication shall be legally binding.

The ultimate parent company of the Atkins Group is WS Atkins plc. Registered in England No. 1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey KT18 5BW. A list of wholly owned Atkins Group companies registered in the United Kingdom can be found at http://www.atkinsglobal.com/terms_and_conditions/index.aspx

Consider the environment. Please don't print this e-mail unless you really need to.

881. 1211215007.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Clare Goodess <C.Goodess@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: [Fwd: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01]
Date: Mon May 19 12:36:47 2008

OK
Phil
At 11:59 19/05/2008, Clare Goodess wrote:

OK . 2 pm - my office?

mail.2008

Clare

At 08:59 19/05/2008, Phil Jones wrote:

OK for me too.

At 08:27 19/05/2008, Tim Osborn wrote:

Hi,
yes this PM is fine with me,
Tim

----- Original Message -----
Subject: [Fwd: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01]
From: f034@uea.ac.uk
Date: Sat, May 17, 2008 12:04 pm
To: p.jones@uea.ac.u
t.osborn@uea.ac.uk

Can we meet on Monday to discuss this and hear from Phil what was decided at the London meeting? I'll be in late Monday (waiting for someone to look at my leaking roof) - so maybe early afternoon. I'm going down to London early evening and will be at Chelsea on tuesday. Good to see Saffron is getting some publicity!
Clare

----- Original Message -----
Subject: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01
From: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>
Date: Fri, May 16, 2008 9:06 am
To: "Jim Hall" <jim.hall@newcastle.ac.uk>
"C G Kilsby" <c.g.kilsby@newcastle.ac.uk>
"Mark New" <mark.new@ouce.ox.ac.uk>
ana.lopez@ouce.ox.ac.uk
"Anthony Footitt" <a.footitt@uea.ac.uk>
"Suraje Dessai" <s.dessai@uea.ac.uk>
"Phil Jones" <p.jones@uea.ac.uk>
"Clare Goodess" <C.Goodess@uea.ac.uk>
t.osborn@uea.ac.uk
Cc: "McSweeney, Robert" <Rob.Mcsweeney@atkinsglobal.com>
"Arkell, Brian" <Brian.Arkell@atkinsglobal.com>
"Sene, Kevin" <Kevin.Sene@atkinsglobal.com>

Dear all,
Please find attached the final tender pack for the Environment Agency bid. The tasks have been re-jigged, with the main change being a broadening of flood risk management to flood and coastal erosion risk management (FCERM). This means a wider audience to include all operating authorities, and the best practice guidance required (new Task 11) is now substantial element, to include evaluation of FCERM climate change adaptation, case studies and provision of evidence to help upgrade the FCDPAG3 Supplementary Note.

We have just one week to finish this tender, as it must be posted on Friday 23rd. We are putting together the bid document, which we'll circulate on Monday 19th, but in the meantime, and by the end of Tuesday 20th, I need everyone to send information (as indicated in brackets) to support the following structure:

- + Understanding of the tender
- + Methodology and programme (methodology for tasks / sub-tasks - see below - and timing)
- + Project team, including individual and corporate experience (who you are putting forward, pen portraits, corporate case studies)
- + Financial and commercial (day rates and number of days; please also highlight potential issues with the T&Cs e.g. IPR)

mail.2008

+ Health & Safety, Quality and Environmental Management
+ Appendices (full CVs, limited to 6 pages)

Please send to me and Rob McSweeney. The information I have already e.g. on day rates, core pen portraits etc will go straight into the version we're working on, so no need to re-send.

In terms of tasks (new nos.), the following organisation is suggested based on what has been noted to date:

Task 1 (Inception meeting and reporting) Atkins, supported by lead representatives of partners

Task 2 (Project board meetings) Atkins, supported by lead representatives of partners

Task 3 (Analysis of user needs) Atkins with Tyn@UEA and OUCE, plus Futerra depending on style

Task 4 (Phase 2 programme) Atkins, supported by all

Task 5 (Interpret messages from UKCIP08 projections) CRU, OUCE and Newcastle, with Atkins advice on sectors

Task 6 (Development of business specific projections) Newcastle and CRU, with Atkins advice on policy and ops

Task 7 (Putting UKCIP08 in context) CRU, Newcastle and OUCE

Task 8 (User guidance) Atkins, Tyn@UEA, Futerra

Task 9 (Pilot studies) Atkins, Newcastle, OUCE, Tyn@UEA

Task 10 (Phase 3 programme) Atkins, supported by all

Task 11 (Best Practice Guidance for FCERM) Newcastle and Atkins, with CRU

Task 12 (Awareness raising events) Atkins, key experts, Futerra (perhaps as an option as EA are quite specific here)

Task 13 (Training events) Atkins and Futerra

Note that Futerra is a communications consultancy, specialising in sustainability, who will input on workshops and on the guidance documents.

I'll be in touch again early next week.

Best wishes,

Geoff

Geoff Darch

Senior Consultant

Water and Environment

ATKINS

Broad Oak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough, PE2 6YS, UK

Tel: +44 (0) 1733 366969

Fax: +44 (0) 1733 366999

Mobile: +44 (0) 7834 507590

E-mail: geoff.darch@atkinsglobal.com

Web: [1]www.atkinsglobal.com/climate_change

This email and any attached files are confidential and copyright protected. If you are not the addressee, any dissemination of this communication is strictly prohibited. Unless otherwise expressly agreed in writing, nothing stated in this communication shall be legally binding.

The ultimate parent company of the Atkins Group is WS Atkins plc.

Registered in England No. 1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey KT18 5BW. A list of wholly owned Atkins Group companies registered in the United Kingdom can be found at

[2]http://www.atkinsglobal.com/terms_and_conditions/index.aspx

Consider the environment. Please don't print this e-mail unless you really need to.

Dr Timothy J Osborn, Academic Fellow

Climatic Research Unit

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089

mail.2008

fax: +44 1603 507784
web: [3]<http://www.cru.uea.ac.uk/~timo/>
sunclock: [4]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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

Dr Clare Goodess
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
UK
Tel: +44 -1603 592875
Fax: +44 -1603 507784
web: [5]<http://www.cru.uea.ac.uk/>
[6]<http://www.cru.uea.ac.uk/~clareg/clare.htm>

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

References

1. http://www.atkinsglobal.com/climate_change
2. http://www.atkinsglobal.com/terms_and_conditions/index.aspx
3. <http://www.cru.uea.ac.uk/~timo/>
4. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
5. <http://www.cru.uea.ac.uk/>
6. <http://www.cru.uea.ac.uk/~clareg/clare.htm>

882. 1211225754.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>, "Jim Hall" <jim.hall@newcastle.ac.uk>, "C G Kilsby" <c.g.kilsby@newcastle.ac.uk>, "Mark New" <mark.new@ouce.ox.ac.uk>, <ana.lopez@ouce.ox.ac.uk>, "Anthony Footitt" <a.footitt@uea.ac.uk>, "Suraje Dessai" <s.dessai@uea.ac.uk>, "Clare Goodess" <C.Goodess@uea.ac.uk>, <t.osborn@uea.ac.uk>
Subject: Re: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01
Date: Mon May 19 15:35:54 2008
Cc: "McSweeney, Robert" <Rob.Mcsweeney@atkinsglobal.com>, "Arkell, Brian" <Brian.Arkell@atkinsglobal.com>, "Sene, Kevin" <Kevin.Sene@atkinsglobal.com>

Geoff,

Clare is off to Chelsea - back late tomorrow. We (Clare, Tim and me) have had a brief meeting. Here are some thoughts and questions we had.

1. Were we going to do two sets of costings?

mail.2008

2. Those involved in UKCIP08 (both doing the work and involved in the SG) have signed confidentiality texts with DEFRA. Not sure how these affect access to the headline messages in the drafts we're going to be looking at over the next

few

months. Also not sure how these will affect the UKCIP workshops that are coming up before the launch.

3. We then thought about costs for the CRU work. We decided on 25K for all CRU work. At £500 per day this comes to 50 days. We then split this into the tasks: 5 - 5 days, 6 - 5 days, 7 - 30 days, 10/11 - 5 days, which leaves 5 more days for meetings. Assumed the 25K was without travel to the meetings.

4. On CVs and pen portraits. Clare will send one before she leaves. Are what you have for Tim and me OK?

5. Some thoughts on Tasks 6 and 7

Task 6 - assumed this was mostly Newcastle. Tim's work on rainfall extremes could be

fed in, and we can do something on non-rainfall variables. Assume also you expect us to

waves? do waves, but not sure what we can do. It seems as though sea level has become

Task 7 - assumed here Newcastle (Chris/Hayley) would be doing something on blocking (large-scale variability). Oxford would do the final bit on conceptual representation

of emissions and climate system and sensitivities, so based on GCMs.

This leaves CRU for the other three, which we base mainly on the 11 RCM runs, which we can access through LINK. We could also use ENSEMBLES runs for the

others,

but these would be RCMs. They seem more relevant for the sorts of scales UKCOP08 is working at.

All just a few thoughts at this time.

Can you send the UKWIR bid that went off, so we have a copy?

Cheers

Phil

At 09:06 16/05/2008, Darch, Geoff J wrote:

Dear all,

Please find attached the final tender pack for the Environment Agency bid. The tasks have been re-jigged, with the main change being a broadening of flood risk management to flood and coastal erosion risk management (FCERM). This means a wider audience to include all operating authorities, and the best practice guidance required (new Task 11) is now substantial element, to include evaluation of FCERM climate change adaptation, case studies and provision of evidence to help upgrade the FCDPAG3 Supplementary Note.

We have just one week to finish this tender, as it must be posted on Friday 23rd. We are putting together the bid document, which we'll circulate on Monday 19th, but in the meantime, and by the end of Tuesday 20th, I need everyone to send information (as indicated in brackets) to support the following structure:

+ Understanding of the tender

+ Methodology and programme (methodology for tasks / sub-tasks - see below - and timing)

+ Project team, including individual and corporate experience (who you are putting forward, pen portraits, corporate case studies)

+ Financial and commercial (day rates and number of days; please also highlight potential issues with the T&Cs e.g. IPR)

+ Health & Safety, Quality and Environmental Management

+ Appendices (full CVs, limited to 6 pages)

Please send to me and Rob McSweeney. The information I have already e.g. on day rates, core pen portraits etc will go straight into the version we're working on, so no need to re-send.

In terms of tasks (new nos.), the following organisation is suggested based on what has been noted to date:

Task 1 (Inception meeting and reporting) Atkins, supported by lead

mail.2008

representatives of partners
Task 2 (Project board meetings) Atkins, supported by lead representatives of partners
Task 3 (Analysis of user needs) Atkins with Tyn@UEA and OUCE, plus Futerra depending on style
Task 4 (Phase 2 programme) Atkins, supported by all
Task 5 (Interpret messages from UKCIP08 projections) CRU, OUCE and Newcastle, with Atkins advice on sectors
Task 6 (Development of business specific projections) Newcastle and CRU, with Atkins advice on policy and ops
Task 7 (Putting UKCIP08 in context) CRU, Newcastle and OUCE
Task 8 (User guidance) Atkins, Tyn@UEA, Futerra
Task 9 (Pilot studies) Atkins, Newcastle, OUCE, Tyn@UEA
Task 10 (Phase 3 programme) Atkins, supported by all
Task 11 (Best Practice Guidance for FCERM) Newcastle and Atkins, with CRU
Task 12 (Awareness raising events) Atkins, key experts, Futerra (perhaps as an option as EA are quite specific here)
Task 13 (Training events) Atkins and Futerra
Note that Futerra is a communications consultancy, specialising in sustainability, who will input on workshops and on the guidance documents.

I'll be in touch again early next week.

Best wishes,

Geoff

Geoff Darch

Senior Consultant

Water and Environment

ATKINS

Broadoak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough, PE2 6YS, UK

Tel: +44 (0) 1733 366969

Fax: +44 (0) 1733 366999

Mobile: +44 (0) 7834 507590

E-mail: geoff.darch@atkinsglobal.com

web: [1]www.atkinsglobal.com/climate_change

This email and any attached files are confidential and copyright protected. If you are not the addressee, any dissemination of this communication is strictly prohibited.

Unless otherwise expressly agreed in writing, nothing stated in this communication shall be legally binding.

The ultimate parent company of the Atkins Group is WS Atkins plc. Registered in England No. 1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey KT18 5BW. A

List of wholly owned Atkins Group companies registered in the United Kingdom can be

found at [2]http://www.atkinsglobal.com/terms_and_conditions/index.aspx

Consider the environment. Please don't print this e-mail unless you really need to.

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

mail.2008

1. http://www.atkinsglobal.com/climate_change
2. http://www.atkinsglobal.com/terms_and_conditions/index.aspx

883. 1211462932.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@psu.edu
Subject: Re: Thompson et al paper
Date: Thu May 22 09:28:52 2008
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Mike, Gavin,
OK - as long as you're not critical and remember the embargo. I'll expect Nature will be sending the paper around later today to the press embargoed till the middle of next week. Attached is the pdf. This is the final one bar page and volume numbers. Also attached is our latest draft press release. This is likely OK except for the last paragraph which we're still working on. There will also be a News and Views item from Dick Reynolds and a Nature news piece from Quirin Schiermeier. I don't have either of these. I did speak to Quirin on Tuesday and he's also spoke to Dave and John. It took me a while to explain the significance of the paper. I hope to get these later two items before I might have to do any interviews early next week. We have a bank holiday on Monday in the UK. The press release will go out jointly from the Met Office and UEA - not sure exactly when. Potentially the key issue is the final Nature sentence which alludes to the probable underestimation of SSTs in the last few years. Drifters now measuring SSTs dominate by over 2 to 1 cf ships. Drifters likely measure SSTs about 0.1 to 0.2 deg C cooler than ships, so we could be underestimating SSTs and hence global T. I hope Dick will discuss this more. It also means that the 1961-90 average SST that people use to force/couple with models is slightly too warm. Ship-based SSTs are in decline - lots of issues related to the shipping companies wanting the locations of the ships kept secret, also some minor issues of piracy as well. You might want to talk to Scott Woodruff more about this. A bit of background. Loads more UK ww2 logs have been digitized and these will be going or have gone into ICOADS. These logs cover the ww2 years as well as the late 1940s up to about 1950. It seems that all of these require bucket corrections. My guess will be that the period from 1945-49 will get raised by up to 0.3 deg C for the SSTs, so about 0.2 for the combined. In digitizing they have concentrated on the South Atlantic/Indian Ocean log books. [1]http://brohan.org/hadobs/digitised_obs/docs/ and click on SST to see some comparisons. The periods mentioned here don't seem quite right as more later 1940s logs have also been

mail.2008

digitized. There are more log books to digitize for ww2 - they have done about half of those not already done.

If anyone wonders where all the RN ships came from, many of those in the S. Atlantic/Indian oceans were originally US ships. The UK got these through the Churchill/Roosevelt deal in 1939/40.

Occasionally some ships needed repairs and the UK didn't have the major parts, so this will explain the voyages of a few south of OZ and NZ across the Pacific to Seattle and then back into the fray.

ICOADS are looking into a project to adjust/correct all their log books. Also attaching a ppt from Scott Woodruff. Scott knows who signed this! If you want me to look through anything then email me.

I have another paper just accepted in JGR coming out on Chinese temps and urbanization. This will also likely cause a stir. I'll send you a copy when I get the proofs from AGU. Some of the paper relates to the 1990 paper and the fraud allegation against Wei-Chyung Wang. Remind me on this in a few weeks if you hear nothing.

Cheers

Phil

PS CRU/Tyndall won a silver medal for our garden at the Chelsea Flower Show - the theme of the show this year was the changing climate and how it affects gardening.

Clare Goodess was at the garden on Tuesday. She said she never stopped for her 4 hour stint of talking to the public - only one skeptic. She met the environment minister.

She was talking about the high and low emissions garden. The minister (Phil Woolas)

seemed to think that the emissions related to the ability of the plants to extract

CO2 from the atmosphere! He'd also not heard of the UHI! Still lots of education needed.

PPS Our web server has found this piece of garbage - so wrong it is unbelievable that

Tim Ball wrote a decent paper in Climate Since AD 1500. I sometimes wish I'd never

said this about the land stations in an email. Referring to Alex von Storch just shows how up to date he is.

[2]<http://canadafreepress.com/index.php/article/3151>

At 20:12 21/05/2008, Michael Mann wrote:

Hi Phil,

Gavin and I have been discussing, we think it will be important for us to do something

on the Thompson et al paper as soon as it appears, since its likely that naysayers are

going to do their best to put a contrarian slant on this in the blogosphere.

Would you mind giving us an advance copy. We promise to fully respect Nature's embargo

(i.e., we wouldn't post any article until the paper goes public) and we don't expect to

in any way be critical of the paper. We simply want to do our best to help make sure

that the right message is emphasized.

thanks in advance for any help!

mike

--

mail.2008

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [3]mann@psu.edu
University Park, PA 16802-5013

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

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

References

1. http://brohan.org/hadobs/digitised_obs/docs/
2. <http://canadafreepress.com/index.php/article/3151>
3. <mailto:mann@psu.edu>
4. <http://www.met.psu.edu/dept/faculty/mann.htm>

884. 1211491089.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>
Subject: RE: Probabilistic information to inform EA decision making - Draft Bid
Date: Thu May 22 17:18:09 2008

Geoff,
Hopefully this will do. No narrative.
Off home now. I'll look through anything you send tomorrow.
Exam scripts to mark tonight.
Cheers
Phil

At 17:00 22/05/2008, you wrote:

Phil,

The only CV we have for you is a few years old. Can you send a more up to date one (6 pages max).

Thanks,
Geoff

From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]
Sent: 22 May 2008 13:07
To: Darch, Geoff J
Cc: Clare Goodess; t.osborn@uea.ac.uk; MCSweeney, Robert
Subject: RE: Probabilistic information to inform EA decision making - Draft Bid
Page 172

mail.2008

Geoff, Rob,

Will you be sending another version around at some time?

I can't recall where the idea of two sets of costings came from.

Here are some more thoughts

Related EA work

Drought work

impact of
groundwater
levels. Jones, P.D., Leadbetter, A., Osborn, T.J. and Bloomfield, J.P., 2006: The
Climate change on severe droughts: River-flow reconstructions and implied
levels. Science Report: SC040068/SR2, Environment Agency, 58pp.

severe
Environment
Agency, 86pp. Wade, S., Jones, P.D. and Osborn, T.J., 2006: The impact of climate change on
droughts: Implications for decision making. Science Report: SC040068/SR3,

the Ouse
(Anglian). These two bits of work related to historic records of drought on the Eden and

disaggregated to
daily
using months with similar monthly flows in the modern record from the 1960s to

the near
system
models present. The 200 years of daily flows were then put through water resource
in the two areas to see how often drought restrictions occurred. The historic

record
was then
aspect perturbed for the future time slices using three different GCMs. The important
of this

historic
droughts. work is that for both regions the perturbed futures were no worse than the

were earlier
On the Eden some recent droughts were the most severe and on the Ouse they

a better
handle on
the scale of natural variability within each region. in the 20th and in the 19th century. So, for all work, it is important to get

looked
at (i.e. since 1961). Task 6 should not just consider the instrumental observations that UKCIP08 has

very cold
ones), but
extremes). This period will very likely cover all temperature extremes (if we forget the
it will be inadequate for rainfall (changes in daily, monthly and seasonal

The EA
work (above) showed a framework for dealing with the issue with respect to
drought. The
longer

(see
attached
pdf). daily precipitation record has been looked at by Tim Osborn and Douglas Maraun

and they
leave
droughts up to the companies. Task emphasizes floods exclusively - maybe this is their responsibility

One aspect that we could develop within Task 6 is a simple soil moisture

accounting
model
using rainfall and PET and a measure of soil amount. The results from this
could then
be
linked with the heavy rainfall to determine different impacts depending on
antecedent
conditions and time of year.
CRU's work on Task 7
We will be able to use the 11 RCMs on which the whole of UKCIP08 are based -
available
through LINK. MOHC have used emulation of these to build up distributions. An
important
aspect
is to see for seasons and variables how the 11 span the probability domain of
all
the emulations (where do they sit in the pdfs).
Other GCMs - this should really be RCMs. In the ENSEMBLES project we are
comparing
trends in reality with trends from ERA-40-forced runs of 15 different RCMs
across
Europe.
This will be able to show that HadRM3 is within the range of the other RCMs
for
measures
of extremes in temperatures and daily and 5-day precipitation amounts. The
measures
here
are trends (seasonal and annual) over the period from 1961-2000.
This will also show their ability to represent current climate (61-00) not
just for the
means
and trends, but some extreme measures and their trends. This is also past
variability
as well, but I suspect they are meaning further back. We will be able to use a
HadCM3
simulation with historic forcing since 1500.
Back to other work. CRANIUM is the one to refer to. BETWIXT led to CRANIUM.
The
other thing to add in somewhere is that the UKCIP08 WG came from EARWIG, so
attaching that paper as well. There is nothing else yet.
Jones, PD, Harpham, C and Kilsby, CK, 2008: Perturbing a weather generator
using
factors
developed from RCM simulations. Int J. Climatol (not yet submitted).
This will get submitted. It shows that the way we are perturbing the WG for
UKCIP08
works.
We do this by fitting the WG to the model present. We then perturb by using
differences
between model future (2080s) and model control. These perturbations are
monthly. We
then
run the WG and look at the daily variability in the simulations compared to
the model
future at the daily timescale. It works in the sense that the RCM future run
is within
the
range the WG simulations.
Whether the RCM future is right is another matter but the WG does what the RCM
does.
Hope this helps.
Phil

mail.2008

At 16:56 21/05/2008, Darch, Geoff J wrote:

Phil,

Great. From CRU we need in particular project experience (case studies). At the moment we have CRANIUM, but other relevant ones would be good e.g. BETWIXT, SKCC, EA Drought work. Key is those related to probabilistic scenarios, weather generators, working with users and those with EA or Defra (or CCW) as the client.

Any further thoughts or elaboration of your input would be useful, particularly for Task 7, where it may be best to spell out what you will do.

Do you have any preference for the allocation of days between you, Clare and Tim? Also, do you want to revise your rates (for reference Jim Hall is in at £950, Chris Kilsby at £750)? They should apply until the end of the contract i.e. December 2009 and we are asked whether any discounts are available e.g. over and above a certain number of days, which could be worked in if you increased your rates. However, this is entirely up to you!

We are still waiting on input from Oxford, Newcastle and Futerra - all promised imminently. It will be a busy day tomorrow!

Many thanks,

Geoff

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: 21 May 2008 16:16
To: MCSweeney, Robert
Cc: Clare Goodess; t.osborn@uea.ac.uk; Darch, Geoff J
Subject: Re: Probabilistic information to inform EA decision making - Draft Bid
Geoff, Rob,

I can do some work tomorrow. Can you be a little more specific?
It looks as though you need a lot. Have you got anything from anyone else?
I assume this still has to be all off by the end of Friday.
Cheers
Phil

At 14:15 20/05/2008, MCSweeney, Robert wrote:

All,
Attached is an outline draft of the bid. It sets out the information we need to include, some of which is already in place.
Please could you take a look at it and forward any of the outstanding information to Geoff and me, such as
- CVs and pen portraits if you haven't already sent them (NB, CVs are in the Appendix and aren't in the attached document)
- Any relevant (corporate) project experience, case studies, etc
- Thoughts and input to the methodology section (NB, each task has been given a lead

mail.2008

group or groups)
- General comments and suggestions

Please send comments and information as soon as you have the opportunity, the deadline

is rapidly approaching!

Many thanks,

Rob

<<EA Tender_Draft.doc>>

Rob McSweeney

Assistant Scientist

MEng (Hons) MSc

Water and Environment (Water Resources Management)

ATKINS

Peterborough.
Atkins Limited, Broadoak, Southgate Park, Bakewell Road, Orton Southgate,

PE2 6YS.

Tel: +44 (0)1733 366900

Direct: +44 (0)1733 366981

Fax: +44 (0)1733 366999

Email: rob.mcsweeney@atkinsglobal.com

website: [3]www.atkinsglobal.com/climate_change

[4]Click Here to read our new Solutions Magazine sharing industry knowledge and addressing your challenges.

This email and any attached files are confidential and copyright protected. If you are not the addressee, any dissemination of this communication is strictly prohibited. Unless otherwise expressly agreed in writing, nothing stated in this communication shall be

legally binding.

The ultimate parent company of the Atkins Group is WS Atkins plc. Registered in England No.

1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey KT18 5BW.

A list of

wholly owned Atkins Group companies registered in the United Kingdom can be found at:

[5]http://www.atkinsglobal.com/terms_and_conditions/index.aspx.

Please consider the environment. Please don't print this e-mail unless you really need to.

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

This message has been scanned for viruses by [6]MailControl

This email and any attached files are confidential and copyright protected. If you are not the addressee, any dissemination of this communication is strictly prohibited. Unless otherwise expressly agreed in writing, nothing stated in this communication shall be

legally binding.

The ultimate parent company of the Atkins Group is WS Atkins plc. Registered in England No.

1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey KT18 5BW.

mail.2008

A list of wholly owned Atkins Group companies registered in the United Kingdom can be found at:

[7]http://www.atkinsglobal.com/terms_and_conditions/index.aspx.

Consider the environment. Please don't print this e-mail unless you really need to.

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

This message has been scanned for viruses by [8]MailControl

This email and any attached files are confidential and copyright protected. If you are not the addressee, any dissemination of this communication is strictly prohibited. Unless otherwise expressly agreed in writing, nothing stated in this communication shall be legally binding.

The ultimate parent company of the Atkins Group is WS Atkins plc. Registered in England No. 1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey KT18 5BW.

A list of wholly owned Atkins Group companies registered in the United Kingdom can be found at:

[9]http://www.atkinsglobal.com/terms_and_conditions/index.aspx.

Consider the environment. Please don't print this e-mail unless you really need to.

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:p.jones@uea.ac.uk>
2. <mailto:p.jones@uea.ac.uk>
3. file://www.atkinsglobal.com/climate_change
- 4.

http://www.atkinsglobal.com/aboutus/publications/watersolutions/7273742_solutions.pdf

5. http://www.atkinsglobal.com/terms_and_conditions/index.aspx
6. <http://bluepages.wsatkins.co.uk/?6875772>
7. http://www.atkinsglobal.com/terms_and_conditions/index.aspx
8. <http://bluepages.wsatkins.co.uk/?6875772>
9. http://www.atkinsglobal.com/terms_and_conditions/index.aspx

885. 1211816659.txt

#####

mail.2008

From: C.Goodess@uea.ac.uk
To: P.Jones@uea.ac.uk
Subject: Re: EA bid - final draft - for review by 8am Tues 27th
Date: Mon, 26 May 2008 11:44:19 +0100 (BST)
Cc: "Darch, Geoff J" <geoff.darch@atkinsglobal.com>, "Phil Jones" <p.jones@uea.ac.uk>, "Clare Goodess" <c.goodess@uea.ac.uk>, t.osborn@uea.ac.uk, a.footitt@uea.ac.uk, "Suraje Dessai" <s.dessai@uea.ac.uk>, "Jim Hall" <jim.hall@newcastle.ac.uk>, "C G Kilsby" <c.g.kilsby@newcastle.ac.uk>, mark.new@ouce.ox.ac.uk, ana.lopez@ouce.ox.ac.uk, "Ed Gillespie" <ed@futura.co.uk>, "Arkell, Brian" <brian.arkell@atkinsglobal.com>, "Mcsweeney, Robert" <rob.mcsweeney@atkinsglobal.com>

Hi Geoff

Like Phil, I've just given this a quick read through and there are only a very few minor comments on the attached.

My main concern is the cost - which I have to say is much higher than I was anticipating. But we are proposing a substantial amount of analysis and work....

Thanks for all your work on this and good luck getting it off tomorrow.

Best wishes, Clare

>
> Geoff,
> After a relatively quick read through of the meat of the
> proposal, I'm sending it back with a few minor changes.
> You've done a good job of getting a lot of information
> across. I did spend a little more time on the CRU tasks,
> and there is enough detail there for review purposes.
>
> ON costs do whatever you want to CRU costs to ensure
> apparent consistency. I just hope this hasn't been pitched
> too high - but if they want the job doing well, they should be
> paying the right price.
>
> I can't think of any IPR aspects, in addition to that which Chris
> has alluded to. Chris and I will likely need to be careful as
> to what is and what is not part of the UKCIP08 WG, but we
> can address that later. At some stage - way after launch, it is
> possible that the WG within UKCIP08 could be upgraded, a bit like
> we upgrade software, but nowhere near as frequently as Bill Gates
> makes us do.
>
> Cheers
> Phil
>
>> Dear all,
>>
>> Please find the draft final bid and costs attached. We are working on a
>> programme and a couple of summary tables.
>>
>> Method
>> * Please read this through to check you are ok with what is being
>> offered
>> (we'll go through to improve style etc), particularly those tasks you
>> are
>> (co-)leading.
>>
>> Costs

mail.2008

>> * Having initially put these in as desired, the project totalled
>> >>£350k,
>> so I have adjusted a few elements to get it to a perhaps more acceptable
>> £330k. Please check this meets your needs while at the same time please
>> ensure that we're not duplicating time effort on shared tasks. Note I
>> have applied the 10% discount for those days beyond 10 days of an
>> individual's time for Newcastle and Atkins in line with our cost models.
>> * I have guessed at rates for Anthony and Claire Walsh.
>> * Note that we may need to increase CRU and OUCE rates to improve
>> consistency (whilst maintaining overall costs).
>>
>> Contract
>> * The only prior right I have identified is the batch running model that
>> Newcastle have developed. Is this one, and are there others?
>>
>> Any comments (succinct and specific please!) must be back to me by 8am
>> on
>> Tuesday morning in order to make the print run and delivery by noon.
>>
>> Thanks,
>>
>> Geoff
>>
>> Geoff Darch
>>
>> Senior Consultant
>> Water and Environment
>> ATKINS
>>
>> Broadoak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough,
>> PE2 6YS, UK
>> Tel: +44 (0) 1733 366969
>> Fax: +44 (0) 1733 366999
>> Mobile: +44 (0) 7834 507590
>> E-mail: geoff.darch@atkinsglobal.com
>> Web: www.atkinsglobal.com/climate_change
>>
>>
>> <<EA_Probabilistic_Costs_v2_Ex.xls>> <<EA Tender_FinalDraft.doc>>
>>
>>
>> This email and any attached files are confidential and copyright
>> protected. If you are not the addressee, any dissemination of this
>> communication is strictly prohibited. Unless otherwise expressly agreed
>> in
>> writing, nothing stated in this communication shall be legally binding.
>>
>> The ultimate parent company of the Atkins Group is WS Atkins plc.
>> Registered in England No. 1885586. Registered Office Woodcote Grove,
>> Ashley Road, Epsom, Surrey KT18 5BW. A list of wholly owned Atkins Group
>> companies registered in the United Kingdom can be found at
>> http://www.atkinsglobal.com/terms_and_conditions/index.aspx
>>
>> Consider the environment. Please don't print this e-mail unless you
>> really
>> need to.
>>
>>
>

Attachment Converted: "c:\eudora\attach\EA Tender_FinalDraft2.doc"

886. 1211911286.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: David Douglass <douglass@pas.rochester.edu>
Subject: Re: Your manuscript with Peter Thorne
Date: Tue, 27 May 2008 14:01:26 -0700
Reply-to: santer1@llnl.gov
Cc: Christy John <christy@nsstc.uah.edu>, "Thorne, Peter"
<peter.thorne@metoffice.gov.uk>

<x-flowed>
Dr. Douglass:

I assume that you are referring to the Santer et al. paper which has been submitted to the International Journal of Climatology (IJOC). Despite your claims to the contrary, the Santer et al. IJOC paper is not essential reading material in order to understand the arguments advanced by Peter Thorne (in his "News and View" piece on the Allen and Sherwood "Nature Geosciences" article).

I note that you did not have the professional courtesy to provide me with any advance information about your 2007 IJOC paper, which was basically a commentary on previously-published work by myself and my colleagues. Neither I nor any of the authors of those previously-published works (the 2005 Santer et al. Science paper and the 2006 Karl et al. CCSP Report) had the opportunity to review your 2007 IJOC paper prior to its publication - presumably because you specifically requested that we should be excluded from consideration as possible reviewers.

I see no conceivable reason why I should now send you an advance copy of my IJOC paper. Collegiality is not a one-way street, Professor Douglass.

Sincerely,

Dr. Ben Santer
David Douglass wrote:

> Dear Dr Santer
>
> In a recent paper by Peter Thorne in Nature Geoscience he references a
> paper that you and he (and others) have written.
> I can not understand some parts of the Thorne paper without reading the
> Santer/Thorne reference.
> Would you please send me a copy?
>
> Sincerely;
> David Douglass

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

887. 1211924186.txt

#####

From: Caspar Ammann <ammann@ucar.edu>
To: t.osborn@uea.ac.uk
Subject: Re: request for your emails
Date: Tue, 27 May 2008 17:36:26 -0600
Cc: "keith Briffa" <k.briffa@uea.ac.uk>, p.jones@uea.ac.uk

Oh MAN! will this crap ever end??

well, I will have to properly answer in a couple days when I get a chance digging through emails.

I don't recall from the top of my head any specifics about IPCC.

I'm also sorry that you guys have to go through this BS. You all did an outstanding job and

the IPCC report certainly reflects that science and literature in an accurate and balanced way.

So long,
Caspar

On May 27, 2008, at 5:03 PM, Tim Osborn wrote:

Dear Caspar,

I hope everything's fine with you.

Our university has received a request, under the UK Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the IPCC assessment process.

We are not sure what our university's response will be, nor have we even checked whether you sent us emails that relate to the IPCC assessment or that we retained any that you may have sent.

However, it would be useful to know your opinion on this matter. In particular, we would like to know whether you consider any emails that you sent to us as confidential.

Sorry to bother you with this,
Tim (cc Keith & Phil)

Caspar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division - Paleoclimatology
1850 Table Mesa Drive
Boulder, CO 80307-3000
email: [1]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

References

- 1. mailto:ammann@ucar.edu

888. 1212009215.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: t.osborn@uea.ac.uk, "Palmer Dave Mr \ (LIB\)" <David.Palmer@uea.ac.uk>
Subject: Re: FW: Your Ref: FOI_08-23 - IPCC, 2007 WGI Chapter 6 Assessment Process [FOI_08-23]

mail.2008

Date: Wed, 28 May 2008 17:13:35 +0100
Cc: "Briffa Keith Prof \" <k.briffa@uea.ac.uk>, "Mcgarvie Michael Mr \" <m.mcgavie@uea.ac.uk>

Dave,

Although requests (1) and (2) are for the IPCC, so irrelevant to UEA, Keith (or you Dave) could say that for (1) Keith didn't get any additional comments in the drafts other than those supplied by IPCC. On (2) Keith should say that he didn't get any papers through the IPCC process.either. I was doing a different chapter from Keith and I didn't get any. What we did get were papers sent to us directly - so not through IPCC, asking us to refer to them in the IPCC chapters. If only Holland knew how the process really worked!! Every faculty member in ENV and all the post docs and most PhDs do, but seemingly not Holland.

So the answers to both (1) and (2) should be directed to IPCC, but Keith should say that he didn't get anything extra that wasn't in the IPCC comments.

As for (3) Tim has asked Caspar, but Caspar is one of the worse responders to emails known. I doubt either he emailed Keith or Keith emailed him related to IPCC.

I think this will be quite easy to respond to once Keith is back. From looking at these questions and the Climate Audit web site, this all relates to two papers in the journal Climatic Change. I know how Keith and Tim got access to these papers and it was nothing to do with IPCC.

Cheers

Phil

At 23:47 27/05/2008, Tim Osborn wrote:

Dear Dave,

re. David Holland's follow-up requests...

These follow-up questions appear directed more towards Keith than to me. But Keith may be unavailable for a few days due to family illness, so I'll attempt a brief response in case Keith doesn't get a chance to.

Items (1) and (2) concern requests that were made by the IPCC Technical Support Unit (hosted by UCAR in the USA) and any responses would have been sent direct to the IPCC Technical Support Unit, to the email address specified in the quote included in item (2). These requests are, therefore, irrelevant to UEA.

Item (3): we'll send the same enquiry to Ammann as we sent to our other colleagues, and let you know his response.

Item (3) also asks for emails from "the journal Climatic Change that discuss any matters in relation to the IPCC assessment process". I can confirm that I have not received any such emails or other documents. I expect that a similar answer will hold for Keith, since I cannot imagine that the editor of a journal would be contacting us about the IPCC process.

Best wishes

Tim

On Tue, May 27, 2008 6:30 pm, Palmer Dave Mr \ (LIB\) wrote:

> Gents,

> Please note the response received today from Mr. Holland. Could you
> provide input as to his additional questions 1, and 2, and check with
> Mr. Ammann in question 3 as to whether he believes his correspondence
> with us to be confidential?

>

> Although I fear/anticipate the response, I believe that I should inform
> the requester that his request will be over the appropriate limit and
> ask him to limit it - the ICO Guidance states:

>

> 12. If an authority estimates that complying with a request will exceed
> the cost limit, can advice and assistance be offered with a view to the
> applicant refocusing the request?

mail.2008

>
> In such cases the authority is not obliged to comply with the request
> and will issue a refusal notice. Included within the notice (which must
> state the reason for refusing the request, provide details of complaints
> procedure, and contain particulars of section 50 rights) could be advice
> and assistance relating to the
>
> refocusing of the request, together with an indication of the
> information that would be available within the cost limit (as required
> by the Access Code).
>
> This should not preclude other 'verbal' contact with the applicant,
> whereby the authority can ascertain the requirements of the applicant,
> and the normal customer service standards that the authority usually
> adopts.
>
>
> And... our own Code of Practice states (Annex C, point 5)
>
> 5. Where the UEA is not obliged to supply the information requested
> because the cost of doing so would exceed the "appropriate limit" (i.e.
> cost threshold), and where the UEA is not prepared to meet the
> additional costs itself, it should nevertheless provide an indication of
> what information could be provided within the cost ceiling.
>
> This is based on the Lord Chancellors Code of Practice which contains a
> virtually identical provision....
>
> In effect, we have to help the requester phrase the request in such a
> way as to bring it within the appropriate limit - if the requester
> disregards that advice, then we don't provide the information and allow
> them to proceed as they wish....
>
> I just wish to ensure that we do as much as possible 'by the book' in
> this instance as I am certain that this will end up in an appeal, with
> the statutory potential to end up with the ICO.
>
> Cheers, Dave
>
> _____
>
> From: David Holland [[1] <mailto:d.holland@theiet.org>]
> Sent: Tuesday, May 27, 2008 5:37 PM
> To: David Palmer
> Subject: Your Ref: FOI_08-23 - IPCC, 2007 WGI Chapter 6 Assessment
> Process
>
>
> Please find attached a response to your letter of 19th May 2008
>
> David Holland
>
>
>
>

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:d.holland@theiet.org

889. 1212009927.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>, Steven Sherwood <Steven.Sherwood@yale.edu>
Subject: Re: David Douglass
Date: Wed May 28 17:25:27 2008
Cc: santer1@llnl.gov, "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>, ssolomon@frie.com, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, 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>, Frank Wentz <frank.wentz@remss.com>

Ben et al,
Definitely the right response - so agree with Tom.
I have been known to disagree with him, and he's not
always right.
Submit asap !!

Cheers
Phil

At 23:48 27/05/2008, Tom Wigley wrote:

Steve et al.,
Sorry, but I agree with quick submission, but not with giving
anything to Douglass until the paper appears in print.
I guess the reason John likes 1.2 is because it agrees best
with UAH MSU -- which, as we all know, has been inspired by
and blessed by God, and so MUST be right.
Tom.

+++++

Steven Sherwood wrote:

Hi Ben,
I for one am happy with submission pronto, leaving to your discretion the
comments I
sent earlier.
I wouldn't feel too threatened by the likes of Douglass. This paper will
likely be
accepted as is upon resubmission, given the reviews, so why not just send him a
copy too
once it is ready and final.
On a related note I've heard from John Christy who stated his opposition to the
new
Allen+Sherwood article/method (who would've thought). He argues that Leo's
v1.2 dataset
is the "best" version because the later ones are contaminated by artifacts in
ERA-40 due
to Pinatubo. This argument made no sense to me on several levels (one of
which:
Pinatubo erupted almost exactly in the middle of the time period of interest,
thus
should have no impact on any linear trend). But there it is.

mail.2008

SS

On May 27, 2008, at 5:41 PM, Ben Santer wrote:

Dear folks,

I just wanted to alert you to an issue that has arisen in the last few days. As you probably know, a paper by Robert Allen and Steve Sherwood was published last week in "Nature Geoscience". Peter Thorne was asked to write a "News and Views" piece on the Allen and Sherwood paper. Peter's commentary on Allen and Sherwood briefly referenced our joint International Journal of Climatology (IJoC) paper. Peter discussed this with me about a month ago, and I saw no problem with including a reference to our IJoC paper. The reference in Peter's "News and Views" contribution is very general, and gives absolutely no information on the substance of our IJoC paper. At the time Peter I discussed this issue, I had high hopes that our IJoC manuscript would now be very close to publication. I saw no reason why publication of Peter's "News and Views" piece should cause us any concern. Now, however, it is obvious that David Douglass has read the "News and Views" piece and wants a copy of our IJoC paper in advance of its publication - in fact, before a final editorial decision on the paper has been reached. Dr. Douglass has written to me and to Peter, requesting a copy of our IJoC paper. In his letter to Peter, Dr. Douglass has claimed that failure to provide him (Douglass) with a copy of our IJoC paper would contravene the ethics policies of the journal "Nature". As you can see from my reply to Dr. Douglass, I feel strongly that we should not give him an advance copy of our paper. However, I think we should resubmit our revised manuscript to IJoC as soon as possible. The sooner we receive a final editorial decision on our paper, the less likely that it is that Dr. Douglass will be able to cause problems. With your permission, therefore, I'd like to resubmit our revised manuscript by no later than close of business tomorrow. I've incorporated most of the suggested changes I've received from you in the past few days. My personal feeling is that we've now reached the point of diminishing returns, and that it's more important to get the manuscript resubmitted than to engage in further iterations about relatively minor details. I will circulate a final version of the revised paper and the response to the reviewers later this evening. Please let me know if resubmission by C.O.B. tomorrow is not acceptable to you. With best regards,
Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison

mail.2008
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov <[1]mailto:santer1@llnl.gov>

Steven Sherwood
Steven.Sherwood@yale.edu <[2]mailto:Steven.Sherwood@yale.edu>
Yale University
P. O. Box 208109
New Haven, CT 06520-8109
[3]http://www.geology.yale.edu/~sherwood
ph: 203 432-3167
fax: 203 432-3134

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:santer1@llnl.gov>
2. <mailto:Steven.Sherwood@yale.edu>
3. <http://www.geology.yale.edu/~sherwood>

890. 1212026314.txt

#####

From: Tom Wigley <wigley@ucar.edu>
To: santer1@llnl.gov
Subject: Re: Our d3* test
Date: Wed, 28 May 2008 21:58:34 -0600
Cc: "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@al.noaa.gov>, 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>

<x-flowed>
Dear all,

Just to add a bit to Ben's notes. The conceptual problem is how to account for two different types of uncertainty in comparing a single observed trend (with temporal uncertainty) with the average of a bunch of model trends (where the uncertainty is from inter-model differences). The "old" d3 tried to do this, but failed the synthetic data test. The new d3 does this a different way (in the way that the inter-model uncertainty term is quantified). This passes the synthetic data test very well.

The new d3 test differs from DCSP07 only in that it includes in the

mail.2008

denominator of the test statistic an observed noise term. This is by far the bigger of the two denominator terms. Ignoring it is very wrong, and this is why the DCSP07 method fails the synthetic data test.

Tom.

+++++

Ben Santer wrote:

> Dear folks,
>
> Just wanted to let you know that I did not submit our paper to IJoC.
> After some discussions that I've had with Tom Wigley and Peter Thorne, I
> applied our d1*, d2*, and d3* tests to synthetic data, in much the same
> way that we applied the DCPS07 d* test and our original "paired trends"
> test (d) to synthetic data. The results are shown in the appended Figure.
>
> Relative to the DCPS07 d* test, our d1*, d2*, and d3* tests of
> hypothesis H2 yield rejection rates that are substantially
> closer to theoretical expectations (compare the appended Figure with
> Figure 5 in our manuscript). As expected, all three tests show a
> dependence on N (the number of synthetic time series), with rejection
> rates decreasing to near-asymptotic values as N increases. This is
> because the estimate of the model-average signal (which appears in the
> numerator of d1*, d2*, and d3*) has a dependence on N, as does the
> estimate of $s_{\{b_{\{m\}}\}}$, the inter-model standard deviation of trends
> (which appears in the denominator of d2* and d3*).
>
> The worrying thing about the appended Figure is the behavior of d3*.
> This is the test which we thought Reviewers 1 and 2 were advocating. As
> you can see, d3* produces rejection rates that are consistently LOWER
> (by a factor of two or more) than theoretical expectations. We do not
> wish to be accused by Douglass et al. of devising a test that makes it
> very difficult to reject hypothesis H2, even when there is a significant
> difference between the trends in the model average signal and the
> 'observational signal'.
>
> So the question is, did we misinterpret the intentions of the Reviewers?
> Were they indeed advocating a d3* test of the form which we used? I will
> try to clarify this point tomorrow with Francis Zwiers (our Reviewer 2).
>
> Recall that our current version of d3* is defined as follows:
>
> $d3^* = (b_{\{o\}} - \langle\langle b_{\{m\}} \rangle\rangle) / \sqrt{ (s_{\{b_{\{m\}}\}}^2 + (s_{\{b_{\{o\}}\}}^2)) }$
>
> where
>
> $b_{\{o\}}$ = Observed trend
> $\langle\langle b_{\{m\}} \rangle\rangle$ = Model average trend
> $s_{\{b_{\{m\}}\}}$ = Inter-model standard deviation of ensemble-mean trends
> $s_{\{b_{\{o\}}\}}$ = Standard error of the observed trend (adjusted for
> autocorrelation effects)
>
> In Francis's comments on our paper, the first term under the square root
> sign is referred to as "an estimate of the variance of that average"
> (i.e., of $\langle\langle b_{\{m\}} \rangle\rangle$). It's possible that Francis was referring to
> $\sigma_{\{SE\}}$, which IS an estimate of the variance of $\langle\langle b_{\{m\}} \rangle\rangle$. If one
> replaces $s_{\{b_{\{m\}}\}}$ with $\sigma_{\{SE\}}$ in the equation for d3*, the
> performance of the d3* test with synthetic data is (at least for large
> values of N) very close to theoretical expectations. It's actually even
> closer to theoretical expectations than the d2* test shown in the
> appended Figure (which is already pretty close). I'll produce the

mail.2008

> "revised d3*" plot tomorrow...
>
> The bottom line here is that we need to clarify with Francis the exact
> form of the test he was requesting. The "new" d3* (with sigma{SE} as the
> first term under the square root sign) would lead to a simpler
> interpretation of the problems with the DCPS07 test. It would show that
> the primary error in DCPS07 was in the neglect of the observational
> uncertainty term. It would also simplify interpretation of the results
> from Section 6.

>
> I'm sorry about the delay in submission of our manuscript, but this is
> an important point, and I'd like to understand it fully. I'm still
> hopeful that we'll be able to submit the paper in the next few days.
> Many thanks to Tom and Peter for persuading me to pay attention to this
> issue. It often took a lot of persuasion...

> with best regards,
>
> Ben

> -----
>
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-2486
> FAX: (925) 422-7675
> email: santer1@llnl.gov
> -----

</x-flowed>

891. 1212063122.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: IPCC & FOI
Date: Thu, 29 May 2008 08:12:02 -0400
Reply-to: mann@psu.edu

<x-flowed>
Hi Phil,

laughable that CA would claim to have discovered the problem. They would
have run off to the Wall Street Journal for an exclusive were that to
have been true.

I'll contact Gene about this ASAP. His new email is: generwahl@yahoo.com

talk to you later,
mike

Phil Jones wrote:

>
>> Mike,
> Can you delete any emails you may have had with Keith re AR4?

mail.2008

> Keith will do likewise. He's not in at the moment - minor family crisis.

>

> Can you also email Gene and get him to do the same? I don't
> have his new email address.

>

> We will be getting Caspar to do likewise.

>

> I see that CA claim they discovered the 1945 problem in the Nature
> paper!!

>

> Cheers

> Phil

>

>

>

>>

>

> Prof. Phil Jones

> Climatic Research Unit Telephone +44 (0) 1603 592090

> School of Environmental Sciences Fax +44 (0) 1603 507784

> University of East Anglia

> Norwich

Email p.jones@uea.ac.uk

> NR4 7TJ

> UK

>

>

--

Michael E. Mann

Associate Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology

Phone: (814) 863-4075

503 Walker Building

FAX: (814) 865-3663

The Pennsylvania State University

email: mann@psu.edu

University Park, PA 16802-5013

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

</x-flowed>

892. 1212067640.txt

#####

From: Peter Thorne <peter.thorne@metoffice.gov.uk>

To: Tom Wigley <>wigley@ucar.edu>

Subject: Re: Our d3* test

Date: Thu, 29 May 2008 09:27:20 +0100

Cc: Ben Santer <santer1@llnl.gov>, 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@al.noaa.gov>, Melissa Free <melissa.free@noaa.gov>, peter gleckler

<gleckler1@llnl.gov>, Phil 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>, Steve Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz

<frank.wentz@remss.com>

One more addendum:

We still need to be aware that this ignores two sources of uncertainty that will exist in the real world that are not included in Section 6 which is effectively 1 perfect obs and finite number of runs of a perfect model:

1. Imperfect models
2. Observational uncertainty related to dataset construction choices (parametric and structural)

Of course, with the test construct given #1 becomes moot as this is the thing we are testing for with H2. This is definitely not the case for #2 which will be important and is poorly constrained.

For Amplification factors we are either blessed or cursed by the wealth of independent estimates of the observational record. One approach, that I would advocate here because I'm lazy / because its more intuitive* (*=delete as appropriate) is that we can take the obs error term outside the explicit uncertainty calculation by making comparisons to each dataset in turn. However, the alternative approach would be to take the range of dataset estimates, make the necessary poor-mans assumption that this is the 1 sigma or 2 sigma range depending upon how far you think they span the range of possible answers and then incorporate this as an extra term in the denominator to d3. As with the other two it would be orthogonal error so still SQRT of sum of squares. Such an approach would have advantages in terms of universal applicability to other problems where we may have less independent observational estimates, but a drawback in terms of what we should then be using as our observational yardstick in testing H2 (the mean of all estimates, the median, something else?).

Anyway, just a methodological quirk that logically follows if we are worried about ensuring universal applicability of approach which with the increasingly frequent use of CMIP3 archive for these types of applications is something we maybe should be considering. I don't expect us to spend very much time, if any, on this issue as I agree that key is submitting ASAP.

Peter

On wed, 2008-05-28 at 21:58 -0600, Tom wigley wrote:

> Dear all,
 >
 > Just to add a bit to Ben's notes. The conceptual problem is how to
 > account for two different types of uncertainty in comparing a single
 > observed trend (with temporal uncertainty) with the average of a
 > bunch of model trends (where the uncertainty is from inter-model
 > differences). The "old" d3 tried to do this, but failed the synthetic
 > data test. The new d3 does this a different way (in the way that the
 > inter-model uncertainty term is quantified). This passes the synthetic
 > data test very well.
 >
 > The new d3 test differs from DCSP07 only in that it includes in the
 > denominator of the test statistic an observed noise term. This is by
 > far the bigger of the two denominator terms. Ignoring it is very
 > wrong, and this is why the DCSP07 method fails the synthetic data
 > test.
 >
 > Tom.
 >
 > ++++++

mail.2008

> Ben Santer wrote:
> > Dear folks,
> >
> > Just wanted to let you know that I did not submit our paper to IJoC.
> > After some discussions that I've had with Tom Wigley and Peter Thorne, I
> > applied our $d1^*$, $d2^*$, and $d3^*$ tests to synthetic data, in much the same
> > way that we applied the DCPS07 d^* test and our original "paired trends"
> > test (d) to synthetic data. The results are shown in the appended Figure.
> >
> > Relative to the DCPS07 d^* test, our $d1^*$, $d2^*$, and $d3^*$ tests of
> > hypothesis H2 yield rejection rates that are substantially
> > closer to theoretical expectations (compare the appended Figure with
> > Figure 5 in our manuscript). As expected, all three tests show a
> > dependence on N (the number of synthetic time series), with rejection
> > rates decreasing to near-asymptotic values as N increases. This is
> > because the estimate of the model-average signal (which appears in the
> > numerator of $d1^*$, $d2^*$, and $d3^*$) has a dependence on N, as does the
> > estimate of $s\{b_{\{m\}}\}$, the inter-model standard deviation of trends
> > (which appears in the denominator of $d2^*$ and $d3^*$).
> >
> > The worrying thing about the appended Figure is the behavior of $d3^*$.
> > This is the test which we thought Reviewers 1 and 2 were advocating. As
> > you can see, $d3^*$ produces rejection rates that are consistently LOWER
> > (by a factor of two or more) than theoretical expectations. We do not
> > wish to be accused by Douglass et al. of devising a test that makes it
> > very difficult to reject hypothesis H2, even when there is a significant
> > difference between the trends in the model average signal and the
> > 'observational signal'.
> >
> > So the question is, did we misinterpret the intentions of the Reviewers?
> > Were they indeed advocating a $d3^*$ test of the form which we used? I will
> > try to clarify this point tomorrow with Francis Zwiers (our Reviewer 2).
> >
> > Recall that our current version of $d3^*$ is defined as follows:
> >
> > $d3^* = (b_{\{o\}} - \langle\langle b_{\{m\}} \rangle\rangle) / \sqrt{ (s\{b_{\{m\}}\} ** 2) + (s\{b_{\{o\}}\} ** 2) }$
> >
> > where
> >
> > $b_{\{o\}}$ = Observed trend
> > $\langle\langle b_{\{m\}} \rangle\rangle$ = Model average trend
> > $s\{b_{\{m\}}\}$ = Inter-model standard deviation of ensemble-mean trends
> > $s\{b_{\{o\}}\}$ = Standard error of the observed trend (adjusted for
> > autocorrelation effects)
> >
> > In Francis's comments on our paper, the first term under the square root
> > sign is referred to as "an estimate of the variance of that average"
> > (i.e., of $\langle\langle b_{\{m\}} \rangle\rangle$). It's possible that Francis was referring to
> > $\sigma\{SE\}$, which IS an estimate of the variance of $\langle\langle b_{\{m\}} \rangle\rangle$. If one
> > replaces $s\{b_{\{m\}}\}$ with $\sigma\{SE\}$ in the equation for $d3^*$, the
> > performance of the $d3^*$ test with synthetic data is (at least for large
> > values of N) very close to theoretical expectations. It's actually even
> > closer to theoretical expectations than the $d2^*$ test shown in the
> > appended Figure (which is already pretty close). I'll produce the
> > "revised $d3^*$ " plot tomorrow...
> >
> > The bottom line here is that we need to clarify with Francis the exact
> > form of the test he was requesting. The "new" $d3^*$ (with $\sigma\{SE\}$ as the
> > first term under the square root sign) would lead to a simpler
> > interpretation of the problems with the DCPS07 test. It would show that
> > the primary error in DCPS07 was in the neglect of the observational
> > uncertainty term. It would also simplify interpretation of the results
> > from Section 6.

mail.2008

> >
> > I'm sorry about the delay in submission of our manuscript, but this is
> > an important point, and I'd like to understand it fully. I'm still
> > hopeful that we'll be able to submit the paper in the next few days.
> > Many thanks to Tom and Peter for persuading me to pay attention to this
> > issue. It often took a lot of persuasion...

> >
> > With best regards,

> >
> > Ben

> > -----

> >
> > Benjamin D. Santer
> > Program for Climate Model Diagnosis and Intercomparison
> > Lawrence Livermore National Laboratory
> > P.O. Box 808, Mail Stop L-103
> > Livermore, CA 94550, U.S.A.
> > Tel: (925) 422-2486
> > FAX: (925) 422-7675
> > email: santer1@llnl.gov

> > -----

> >
> >
> >

--
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

893. 1212073451.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@meteo.psu.edu>
Subject: IPCC & FOI
Date: Thu May 29 11:04:11 2008

Mike,

Can you delete any emails you may have had with Keith re AR4?
Keith will do likewise. He's not in at the moment - minor family crisis.

Can you also email Gene and get him to do the same? I don't
have his new email address.

We will be getting Caspar to do likewise.
I see that CA claim they discovered the 1945 problem in the Nature paper!!
Cheers
Phil

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

894. 1212088415.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: Our d3* test
Date: Thu May 29 15:13:35 2008

Ben,

Hopefully the email to Francis will help to resolve this quickly. It would seem from Tom's email that the new d3 approaches the expected result for largish N. A test ought to do this as Tom says.

You'll need to change the response a little as although you may have misinterpreted

Francis, you may not have Rev 1.

Hope this is out of your hair as soon as feasible.

Climate Audit are an odd crowd. McIntyre is claiming that he spotted the problem

in 1945 in the marine data - and refers to a blog page from late last year! We were already on to it by then and he didn't really know what he was talking about anyway.

Maybe this paper and the various press coverage (especially Dick Reynold's N&V as he spelt it out) will allow them to realize that what is really robust in all this is the

land record. I suspect it won't though. One day they may finally realize the concept of effective spatial degrees of freedom. John Christy doesn't understand this!

Cheers

Phil

At 04:46 29/05/2008, you wrote:

Dear folks,

Just wanted to let you know that I did not submit our paper to IJoC. After some discussions that I've had with Tom Wigley and Peter Thorne, I applied our d1*, d2*, and d3* tests to synthetic data, in much the same way that we applied the DCPS07 d* test and our original "paired trends" test (d) to synthetic data. The results are shown in the appended Figure.

Relative to the DCPS07 d* test, our d1*, d2*, and d3* tests of hypothesis H2 yield rejection rates that are substantially closer to theoretical expectations (compare the appended Figure with Figure 5 in our manuscript). As expected, all three tests show a dependence on N (the number of synthetic time series), with rejection rates decreasing to near-asymptotic values as N

increases. This is because the estimate of the model-average signal (which appears in the numerator of d1*, d2*, and d3*) has a dependence on N, as does the estimate of

$s\{<b_{m}>\}$, the inter-model standard deviation of trends (which appears in the denominator of d2* and d3*).

The worrying thing about the appended Figure is the behavior of d3*. This is the test

which we thought Reviewers 1 and 2 were advocating. As you can see, d3* produces

rejection rates that are consistently LOWER (by a factor of two or more) than theoretical expectations. We do not wish to be accused by Douglass et al. of

devising a

test that makes it very difficult to reject hypothesis H2, even when there is a significant difference between the trends in the model average signal and the 'observational signal'.

they

So the question is, did we misinterpret the intentions of the Reviewers? Were indeed advocating a $d3^*$ test of the form which we used? I will try to clarify

this point

tomorrow with Francis Zwiers (our Reviewer 2).

Recall that our current version of $d3^*$ is defined as follows:

$$d3^* = (b\{o\} - \langle\langle b\{m\}\rangle\rangle) / \sqrt{ (s\{\langle b\{m\}\rangle\} ** 2) + (s\{b\{o\}\} ** 2) }$$

where

$b\{o\}$ = Observed trend

$\langle\langle b\{m\}\rangle\rangle$ = Model average trend

$s\{\langle b\{m\}\rangle\}$ = Inter-model standard deviation of ensemble-mean trends

$s\{b\{o\}\}$ = Standard error of the observed trend (adjusted for autocorrelation effects)

is

In Francis's comments on our paper, the first term under the square root sign

referred to as "an estimate of the variance of that average" (i.e., of $\langle\langle b\{m\}\rangle\rangle$).

It's possible that Francis was referring to $\sigma\{SE\}$, which IS an estimate of the variance

of $\langle\langle b\{m\}\rangle\rangle$. If one replaces $s\{\langle b\{m\}\rangle\}$ with $\sigma\{SE\}$ in the equation for $d3^*$,

the

performance of the $d3^*$ test with synthetic data is (at least for large values

of N) very

close to theoretical expectations. It's actually even closer to theoretical

expectations

than the $d2^*$ test shown in the appended Figure (which is already pretty close).

I'll

produce the "revised $d3^*$ " plot tomorrow...

The bottom line here is that we need to clarify with Francis the exact form of the test

he was requesting. The "new" $d3^*$ (with $\sigma\{SE\}$ as the first term under the square root

sign)

would lead to a simpler interpretation of the problems with the DCPS07

test. It

would show that the primary error in DCPS07 was in the neglect of the

observational

uncertainty term. It would also simplify interpretation of the results from

Section 6.

I'm sorry about the delay in submission of our manuscript, but this is an

important point, and I'd like to understand it fully. I'm still hopeful that we'll be

able to

submit the paper in the next few days. Many thanks to Tom and Peter for

persuading me to

pay attention to this issue. It often took a lot of persuasion...

with best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

mail.2008
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

895. 1212156886.txt

#####

From: Caspar Ammann <ammann@ucar.edu>
To: t.osborn@uea.ac.uk
Subject: Re: request for your emails
Date: Fri, 30 May 2008 10:14:46 -0600
Cc: "keith Briffa" <k.briffa@uea.ac.uk>, p.jones@uea.ac.uk

Hi Tim,

in response to your inquiry about my take on the confidentiality of my email communications with you, Keith or Phil, I have to say that the intent of these emails is to reply or communicate with the individuals on the distribution list, and they are not intended for general 'publication'. If I would consider my texts to potentially get wider dissemination then I would probably have written them in a different style. Having said that, as far as I can remember (and I haven't checked in the records, if they even still exist) I have never written an explicit statement on these messages that would label them strictly confidential. Not sure if this is of any help, but it seems to me that it reflects our standard way of interaction in the scientific community.

Caspar
On May 27, 2008, at 5:03 PM, Tim Osborn wrote:

Dear Caspar,
I hope everything's fine with you.
Our university has received a request, under the UK Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the IPCC assessment process.
We are not sure what our university's response will be, nor have we even checked whether you sent us emails that relate to the IPCC assessment or that we retained any that you may have sent.
However, it would be useful to know your opinion on this matter. In particular, we would like to know whether you consider any emails that you sent to us as confidential.
Sorry to bother you with this,
Tim (cc Keith & Phil)

Caspar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division - Paleoclimatology
1850 Table Mesa Drive
Boulder, CO 80307-3000
email: [1]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

References

- 1. mailto:ammann@ucar.edu

896. 1212166714.txt

#####

From: Tim Osborn <t.osborn@uea.ac.uk>
 To: Caspar Ammann <ammann@ucar.edu>
 Subject: Re: request for your emails
 Date: Fri May 30 12:58:34 2008
 Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, p.jones@uea.ac.uk

Hi again Caspar,
 I don't think it is necessary for you to dig through any emails you may have sent us to determine your answer.
 Our question is a more general one, which is whether you generally consider emails that you sent us to have been sent in confidence. If you do, then we will use this as a reason to decline the request.

Cheers
Tim

At 00:36 28/05/2008, Caspar Ammann wrote:

Oh MAN! will this crap ever end??
 Well, I will have to properly answer in a couple days when I get a chance digging through emails. I don't recall from the top of my head any specifics about IPCC.

I'm also sorry that you guys have to go through this BS. You all did an outstanding job and the IPCC report certainly reflects that science and literature in an accurate and balanced way.

So long,
Caspar

On May 27, 2008, at 5:03 PM, Tim Osborn wrote:

Dear Caspar,
 I hope everything's fine with you.
 Our university has received a request, under the UK Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the IPCC assessment process.
 We are not sure what our university's response will be, nor have we even checked whether you sent us emails that relate to the IPCC assessment or that we retained any that you may have sent.
 However, it would be useful to know your opinion on this matter. In particular, we would like to know whether you consider any emails that you sent to us as confidential.
 Sorry to bother you with this,
 Tim (cc Keith & Phil)

Caspar M. Ammann
 National Center for Atmospheric Research
 Climate and Global Dynamics Division - Paleoclimatology
 1850 Table Mesa Drive
 Boulder, CO 80307-3000
 email: [1]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

References

- 1. mailto:ammann@ucar.edu

897. 1212276269.txt

#####

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
 Subject: RE: [Fwd: of buckets and blogs...]
 Date: Sat, 31 May 2008 19:24:29 -0400 (EDT)
 Reply-to: gschmidt@giss.nasa.gov
 Cc: Phil Jones <P.Jones@uea.ac.uk>, mann@psu.edu

<x-flowed>

Phil - here's the text minus figures and links... It's subject to a little revision, but let me know if there are any factual or emphasis issues that are perhaps misplaced.

Thanks

Gavin

=====

Of buckets and blogs

This last week has been an interesting one for observers of how climate change is covered in the media and online. On wednesday an interesting paper (Thompson et al) was published in Nature, pointing to a clear artifact in the sea surface temperatures in 1945 and associating it with the changing mix of fleets and measurement techniques at the end of world war II. The mainstream media by and large got the story right - puzzling anomaly tracked down, corrections in progress after a little scientific detective work, consequences minor - even though a few headline writers got a little carried away in equating a specific dip in 1945 ocean temperatures with the more gentle 1940s-1970s cooling that is seen in the land measurements. However, some blog commentaries have gone completely overboard on the implications of this study in ways that are very revealing of their underlying biases.

The best commentary came from John Nielsen-Gammon's new blog where he described very clearly how the uncertainties in data - both the known unknowns and unknown unknowns - get handled in practice (read this and then come back). Stoa, quite sensibly, suggested that it's a bit early to be expressing an opinion on what it all means. But patience is not one of the blogosphere's virtues and so there was no shortage of people extrapolating wildly to support their pet hobbyhorses. This in itself is not so unusual; despite much advice to the contrary, people (the media and bloggers) tend to weight individual papers that make the news far more highly than the balance of evidence that really underlies assessments like the IPCC. But in this case, the addition of a little knowledge made the usual extravagances a little more scientific-looking and has given it some extra steam.

Like almost all historical climate data, ship-board sea surface temperatures (SST) were not collected with long term climate trends in mind. Thus practices varied enormously among ships and fleets and over time. In the 19th Century, simple wooden buckets would be thrown over the

mail.2008

side to collect the water (a non-trivial exercise when a ship is moving, as many novice ocean-going researchers will painfully recall). Later on, special canvas buckets were used, and after WWII, insulated 'buckets' became more standard - though these aren't really buckets in the colloquial sense of the word as the photo shows (pay attention to this because it comes up later).

The thermodynamic properties of each of these buckets are different and so when blending data sources together to get an estimate of the true anomaly, corrections for these biases are needed. For instance, the canvas buckets give a temperature up to 1C cooler in some circumstances (that depend on season and location) than the modern insulated buckets. Insulated buckets have a slight cool bias compared to temperature measurements that are taken at the inlet for water in the engine room which is the most used method at present. Automated buoys which became more common in recent decades tend to be cooler than the engine intake measures as well. The recent IPCC report had a thorough description of these issues (section 3.B.3) fully acknowledging that these corrections were a work in progress.

And that is indeed the case. The collection and digitisation of the ship logbooks is a huge undertaking and continues to add significant amounts of 20th Century and earlier data to the records. This dataset (ICOADS) is continually growing, and the impacts of the bias adjustments are continually being assessed. The biggest transitions in measurements occurred at the beginning of WWII between 1939 and 1941 when the sources of data switched from European fleets to almost exclusively US fleets (and who tended to use engine inlet temperatures rather than canvas buckets). This offset was large and dramatic and was identified more than ten years ago from comparisons of simultaneous measurements of night-time marine air temperatures (NMAT) which did not show such a shift. The experimentally based adjustment to account for the canvas bucket cooling brought the sea surface temperatures much more into line with the NMAT series (Folland and Parker, 1995). (Note that this reduced the 20th Century trends in SST).

More recent work (for instance, at this workshop in 2005), has focussed on refining the estimates and incorporating new sources of data. For instance, the 1941 shift in the original corrections, was reduced and pushed back to 1939 with the addition of substantial and dominant amounts of US Merchant Marine data (which mostly used engine inlets temperatures).

The version of the data that is currently used in most temperature reconstructions is based on the work of Rayner and colleagues (reported in 2006). In their discussion of remaining issues they state:

Using metadata in the ICOADS it is possible to compare the contributions made by different countries to the marine component of the global temperature curve. Different countries give different advice to their observing fleets concerning how best to measure SST. Breaking the data up into separate countries' contributions shows that the assumption made in deriving the original bucket correctionsthat is, that the use of uninsulated buckets ended in January 1942is incorrect. In particular, data gathered by ships recruited by Japan and the Netherlands (not shown) are biased in a way that suggests that these nations were still using uninsulated buckets to obtain SST measurements as late as the 1960s. By contrast, it appears that the United States started the switch to using engine room intake measurements as early as 1920.

They go on to mention the modern buoy problems and the continued need to work out bias corrections for changing engine inlet data as well as minor issues related to the modern insulated buckets. For example, the differences in co-located modern bucket and inlet temperatures are around 0.1 deg C:

(from John Kennedy).

However it is one thing to suspect that biases might remain in a dataset (a sentiment shared by everyone), it is quite another to show that they are really there. The Thompson et al paper does the latter quite effectively by removing variability associated with some known climate modes (including ENSO) and seeing the 1945 anomaly pop out clearly. In doing this in fact, they show that the previous adjustments in the pre-war period were probably ok (though there is substantial additional evidence of that in any case - see the references in Rayner et al, 2006). The Thompson anomaly seems to coincide strongly with the post-war shift back to a mix of US, UK and Dutch ships, implying that post-war bias corrections are indeed required and significant. This conclusion is not much of a surprise to any of the people working on this since they have been saying it in publications and meetings for years. The issue is of course quantifying and validating the corrections, for which the Thompson analysis might prove useful. The use of canvas buckets by the Dutch, Japanese and some UK ships is most likely to blame, and given the mix of national fleets shown above, this will make a noticeable difference in 1945 up to the early 1960s maybe - the details will depend on the seasonal and areal coverage of those sources compared to the dominant US information. The schematic in the Independent is probably a good first guess at what the change will look like (remember that the ocean changes are constrained by the NMAT record shown above).

So far, so good. The fun for the blog-watchers is what happened next. What could one do to get the story all wrong? First, you could incorrectly assume that scientists working on this must somehow be unaware of the problems (that is belied by the frequent mention of post WWII issues in workshops and papers since at least 2005, but never mind). Next, you could conflate the 'buckets' used in recent decades (as seen in the graphs in Kent et al 2007's discussion of the ICOADS meta-data) with the buckets in the pre-war period (see photo above). If you do make that mistake however, you can extrapolate to get some rather dramatic (if erroneous) conclusions. For instance, that the effect of the 'corrections' would be to halve the SST trend from the 1970s. Gosh! (The mismatch this would create with the independent NMAT data series should not be mentioned). But there is more! You could take the (incorrect) prescription based on the bucket confusion, apply it to the full global temperatures (land included, hmm) and think that this merits a discussion on whether the whole IPCC edifice had been completely undermined (Answer: no). And it goes on - the bucket confusion was pointed out but the complaint switches to the scandal that it wasn't properly explained.

All this shows is wishful thinking overcoming logic. However many times there is a similar rush to judgment that is subsequently showed to be based on nothing, it still adds to the vast array of similar 'evidence' that keeps getting trotted out by the ill-informed. The excuse that these are just exploratory exercises in what-if thinking wears a little thin when the 'what if' always leads to the same (desired) conclusion. This week's play-by-play was quite revealing on that score.

Gavin Schmidt	NASA/Goddard Institute for Space Studies
Tel: (212) 678 5627	2880 Broadway New York, NY 10025

</x-flowed>

898. 1212413521.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Carl Mears <mears@sonic.net>
Subject: Re: Our d3* test
Date: Mon, 02 Jun 2008 09:32:01 -0700
Reply-to: santer1@llnl.gov
Cc: Steven Sherwood <Steven.Sherwood@yale.edu>, "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@al.noaa.gov>, 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>, Frank Wentz <frank.wentz@remss.com>

<x-flowed>

Dear Carl,

This issue is now covered in the version of the manuscript that I sent out on Friday. The d2* and d3* statistics have been removed. The new d1* statistic DOES involve the standard error of the model average trend in the denominator (together with the adjusted standard error of the observed trend; see equation 12 in revised manuscript). The slight irony here is that the new d1* statistic essentially reduces to the old d1* statistic, since the adjusted standard error of the observed trend is substantially larger than the standard error of the model average trend...

with best regards,

Ben

Carl Mears wrote:

- > Hi
- >
- > I think I agree (partly, anyway) with Steve S.
- >
- > I think that d3* partly double counts the uncertainty.
- >
- > Here is my thinking that leads me to this:
- >
- > Assume we have a "perfect model". A perfect model means in this context
- > 1. Correct sensitivities to all forcing terms
- > 2. Forcing terms are all correct
- > 3. Spatial temporal structure of internal variability is correct.
- >
- > In other words, the model output has exactly the correct "underlying"
- > trend, but
- > different realizations of internal variability and this variability has
- > the right
- > structure.
- >
- > We now run the model a bunch of times and compute the trend in each case.
- > The spread in the trends is completely due to internal variability.
- >

mail.2008

> We compare this to the "perfect" real world trend, which also has
> uncertainty due
> to internal variability (but nothing else).
>
> To me either one of the following is fair:
>
> 1. We test whether the observed trend is inside the distribution of
> model trends. The uncertainty in the
> observed trend is already taken care of by the spread in modeled trends,
> since the representation of
> internal uncertainty is accurate.
>
> 2. We test whether the observed trend is equal to the mean model trend,
> within uncertainty. Uncertainty here is
> the uncertainty in the observed trend $s\{b\{o\}\}$, combined with the
> uncertainty in the mean model trend ($SE\{b\{m\}\}$).
>
> If we use $d3^*$, I think we are doing both these at once, and thus double
> counting the internal variability
> uncertainty. Option 2 is what Steve S is advocating, and is close to
> $d1^*$, since $SE\{b\{m\}\}$ is so small.
> Option 1 is $d2^*$.
>
> Of course the problem is that our models are not perfect, and a
> substantial portion of the spread in
> model trends is probably due to differences in sensitivity and forcing,
> and the representation
> of internal variability can be wrong. I don't know how to separate the
> model trend distribution into
> a "random" and "deterministic" part. I think $d1^*$ and $d2^*$ above get at
> the problem from 2 different angles,
> while $d3^*$ double counts the internal variability part of the
> uncertainty. So it is not surprising that we
> get some funny results for synthetic data, which only have this kind of
> uncertainty.
>
> Comments?
>
> -Carl
>
>
>
>
> On May 29, 2008, at 5:36 AM, Steven Sherwood wrote:
>
>>
>> On May 28, 2008, at 11:46 PM, Ben Santer wrote:
>>>
>>> Recall that our current version of $d3^*$ is defined as follows:
>>>
>>> $d3^* = (b\{o\} - \langle\langle b\{m\}\rangle\rangle) / \sqrt{ (s\{\langle\langle b\{m\}\rangle\rangle\} ** 2) + (s\{b\{o\}\} ** 2) }$]
>>>
>>> where
>>>
>>> $b\{o\}$ = Observed trend
>>> $\langle\langle b\{m\}\rangle\rangle$ = Model average trend
>>> $s\{\langle\langle b\{m\}\rangle\rangle\}$ = Inter-model standard deviation of ensemble-mean trends
>>> $s\{b\{o\}\}$ = Standard error of the observed trend (adjusted for
>>> autocorrelation effects)
>>>
>>> Shouldn't the first term under sqrt be the standard deviation of the
>> estimate of $\langle\langle b\{m\}\rangle\rangle$ -- e.g., the standard error of $\langle\langle b\{m\}\rangle\rangle$ -- rather
>> than the standard deviation of $\langle\langle b\{m\}\rangle\rangle$? $d3^*$ would I think then be

mail.2008

>> equivalent to a z-score, relevant to the null hypothesis that models
>> on average get the trend right. As written, I think the distribution
>> of d3* will have less than unity variance under this hypothesis.

>>
>> SS

>>
>>
>> -----

>> Steven Sherwood
>> Steven.Sherwood@yale.edu <mailto:Steven.Sherwood@yale.edu>
>> Yale University ph: 203
>> 432-3167
>> P. O. Box 208109 fax: 203
>> 432-3134
>> New Haven, CT 06520-8109
>> http://www.geology.yale.edu/~sherwood

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

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

899. 1212435868.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: nomination: materials needed!
Date: Mon, 02 Jun 2008 15:44:28 -0400
Reply-to: mann@psu.edu

Hi Phil,
This is coming along nicely. I've got 5 very strong supporting letter writers lined up to support your AGU Fellowship nomination (confidentially: Ben Santer, Tom Karl, Jean Jouzel, and Lonnie Thompson have all agreed, waiting to hear back from one more individual, maximum is six letters including mine as nominator).
Meanwhile, if you can pass along the following information that is needed for the nomination package that would be very helpful. thanks in advance!
mike

selected bibliography

mail.2008

- * Must be no longer than 2 pages.
- * Begin by briefly stating the candidate's total number and types of publications and specifying the number published in AGU journals.
- * Do not just select the most recent publications; choose those that best support your argument for Fellowship.

Curriculum Vitae

- * Must be no longer than 2 pages.
- * List the candidate's name, address, history of employment, degrees, research experience, honors, memberships, and service to the community through committee work, advisory boards, etc.

--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [1]mann@psu.edu
University Park, PA 16802-5013

[2]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

1. <mailto:mann@psu.edu>
2. <http://www.met.psu.edu/dept/faculty/mann.htm>

900. 1212587222.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: A couple of things
Date: Wed, 04 Jun 2008 09:47:02 -0400
Reply-to: mann@psu.edu
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Hi Phil,
Seems to me that CRU should charge him a fee for the service. He shouldn't be under the assumption that he has the right to demand reports be scanned in for him on a whim. CRU should require reasonable monetary compensation for the labor, effort (and postage!).
If this were a colleague acting in good faith, I'd say do it at no cost. But of course, he's not. He's not interested in the truth here, he's just looking for another way to try to undermine confidence in our science.
Henry's review looks helpful and easy to deal w/. Will be interesting to see the other reviews. I guess you're going to get your moneys' worth out of your scanner,
mike
Phil Jones wrote:

mail.2008

Gavin, Mike,

1. This email came to CRU last night.

From: Steve McIntyre [[1] <mailto:stephen.mcintyre@utoronto.ca>]

Sent: Tuesday, June 03, 2008 5:09 PM

To: [2] alan.ovenden@uea.ac.uk

Subject: Farmer et al 1989

Dear Sir, Can you please send me a pdf of the Farmer et al 1989, cited in Folland and Parker 1995, which, in turn is cited in the IPCC Fourth Assessment Report.

Thanks,

Steve McIntyre

Farmer, G., Wigley, T. M. L., Jones, P. D. and Salmon, M., 1989 'Documenting and explaining recent global-mean temperature changes'. Climatic Research Unit, Norwich,

Final Report to NERC, UK, Contract GR3/6565 (unpublished)

CRU has just the one copy of this! We've just got a new scanner for a project, so

someone here

is going to try this out - and scan the ~150pp. I'm doing this as this is one of the project

reports that I wished I'd written up. It's got all the bucket equations,

assessments of

the accuracy of the various estimates for the parameters that have to be made.

It also

includes discussion of the shapes (seasonal cycles) of the residual seasonal

cycles you

get from different types of buckets prior to WW2 relative to intakes. It also

includes

a factor

they haven't considered at all yet - ship speed and its changes over time.

This turns

out

to be important. It has a lot more than Folland and Parker (1995). Doubt it will

shut them

up for

long - but it will justify your faith in those doing the SST work that we have considered everything

we could think of. We'll also put it up on our web site at the same time.

2. Reviews of the Holocene epic.

Got this today - so a journal still working by post! Here is Henry's review.

Possibly the other two might involve hand-written comments on hard copies.

Will get these scanned when they arrive and send around if necessary.

Dear Phil

I have today posted two referees' reports to you and the verdict of accepted subject to taking account of referees' comments. These two reports do not include the report of Henry Diaz which has just been sent to you directly. Please take his comments into account too.

John A Matthews

Emeritus Professor of Physical Geography
Editor, The Holocene

Department of Geography
School of the Environment and Society
University of Wales Swansea
Singleton Park
SWANSEA SA2 8PP

Prof. Phil Jones
Climatic Research Unit

Telephone +44 (0) 1603 592090
Page 204

School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
UK

mail.2008
Fax +44 (0) 1603 507784
Email [3]p.jones@uea.ac.uk

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

Department of Meteorology
503 Walker Building
The Pennsylvania State University
University Park, PA 16802-5013

Phone: (814) 863-4075
FAX: (814) 865-3663
email: [4]mann@psu.edu

[5]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

1. <mailto:stephen.mcintyre@utoronto.ca>
2. <mailto:alan.ovenden@uea.ac.uk>
3. <mailto:p.jones@uea.ac.uk>
4. <mailto:mann@psu.edu>
5. <http://www.met.psu.edu/dept/faculty/mann.htm>

901. 1212686327.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Christoph Kull <christoph.kull@scnat.ch>, <bo@gfy.ku.dk>, <thompson.4@osu.edu>, <EWWO@bas.ac.uk>, <jan.esper@wsl.ch>, Janice Lough <j.lough@aims.gov.au>, Juerg Luterbacher <juerg@giub.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Kim Cobb <kcobb@eas.gatech.edu>, Heinz Wanner <wanner@giub.unibe.ch>, Jonathan Overpeck <jto@u.arizona.edu>, Michael Schulz <mschulz@palmod.uni-bremen.de>, Eystein Jansen <Eystein.Jansen@geo.uib.no>, Nick Graham <ngraham@hrc-lab.org>, Francis Zwiers <francis.zwiers@ec.gc.ca>, Caspar Ammann <ammann@ucar.edu>, "Michael E. Mann" <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Sandy Tudhope <sandy.tudhope@ed.ac.uk>, Tas van Ommen <tas.van.ommen@utas.edu.au>, "Wahl, Eugene R" <wahl@alfred.edu>, Brendan Buckley <bmb@ldeo.columbia.edu>, Hugues Goosse <hugues.goosse@uclouvain.be>
Subject: Review Comments on the Wengen paper
Date: Thu Jun 5 13:18:47 2008
Cc: <larry.williams@targetedgrowth.com>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>, Naresh Kumar <NKumar@epri.com>

Dear All (especially Peck!),

Attached are three sets of reviews of the paper - 2 in the pdf file and one in the small doc file.

As you'll be able to see, there isn't that much to do and the reviews have been good. All three reviewers seem to be in awe of the group! I've had a brief discussion with Keith as to who should do what. You're all welcome to help but I only think most of you will need go through the revised version when we get that out - hopefully asap. John Matthews is still hopeful of a 2008 publication date, and you'll see we won't be going out for any further reviews - just John checking.

mail.2008

Many of the comments relate to the tree-ring section and Keith will deal with these. They involve some re-organization and some additional refs on dendro isotope work.

The coral and isotope sections get praised for organization - so well done! I'll need some help with the one coral comment on 'vital effects', so can Janice, Kim and Sandy work on that. I think it only needs a few sentences and maybe extra refs. I know some of you are in Trieste next week, so maybe you can work on it there.

I'll work on the documentary section a bit and liaise with Juerg. This shouldn't involve much extra work.

I'll also look at the borehole section together with what was in Ch 6 of AR4.

The major bit of new text we need is on the high-res varves and laminated lake records, so this is why I highlighted Peck. They aren't used in large-area high-freq climate

reconstructions, so emphasis there and to a few key review papers. Is this doable in the next couple of weeks, Peck? I don't think more than a page or two is required.

Related to the issue of the different proxies use or potential use in high-freq reconstructions, I'll work on trying to bring that out in the Introduction. I'll bring out the issues of the maturity of the different proxy disciplines.

Sections 3 and 4 just seem to need some minor wording changes and some clarification - possibly in a revised introduction. We're hoping that Tim here will be able to do that. Note that although the reviewer suggested dropping

the forcing section, John Matthews would like that kept.

In conclusion, we are nearly there. CRU will be able to find the colour costs envisaged.

To those in Trieste - enjoy the week and I hope it will as fruitful as wengen was.

If anyone is going to be out of contact during the second half of June and early July

can you let me know.

I've reattached the submission as a word file.

Cheers
Phil

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

902. 1212924720.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: request for some additional info.
Date: Sun, 08 Jun 2008 07:32:00 -0400
Reply-to: mann@psu.edu

Hi Phil,
I'm continuing to work on your nomination package (here in my hotel room in
Page 206

Trieste--the weather isn't any good!). If its possible for a case to be too strong, we may have that here! Lonnie is also confirmed as supporting letter writer, along w/ Kevin, Ben, Tom K, and Jean J. (4 of the 5 are already AGU fellows, which I'm told is important! Surprisingly, Ben is not yet, nor am I. But David Thompson is (quite young for one of these). I'm guessing Mike wallace and Susan Solomon might have had something to do w/ that ;)
Anyway, I wanted to check w/ you on two things:
1. One thing that people sometimes like to know is the maximum value of "N" where "N" is the number of papers an individual authored/co-authored that have more than N citations. N=40 (i.e., an individual has published at least 40 papers that have each been cited at least 40 times) is supposedly an important threshold for admission in the U.S. National Academy of Sciences. I'm guessing your N is significantly greater than that, and it would be nice to cite that if possible. would you mind figuring out that number and sending--I think it would be useful is really sealing the case.
2. Would you mind considering a minor revision of your 2 page bibliography. In my nomination letter, I'm trying to underscore the diverse areas where you've made major contributions, and I think its well known and obvious to many that two of these are instrumental data and paleoclimate reconstructions. But it occurs to me that it is equally important to stress your work in detection of anthropogenic impacts on climate w/ both models and observations. For example, your early Nature papers w/ wigley. in '80 and '81 seem to be among the earliest efforts to try to do this (though I don't have copies of the papers, so can't read them!), and that seems very much worth highlighting to me. My suggestion is that you add a category on "Anthropogenic Climate Signal" detection and include this work (say, 8 or so of the key papers in this area including the two early Nature one's w/ wigley) as well as some of your later work w/ Santer/Tett/Thorne/Hegerl/Barnett. I realize that most of your work in this area isn't as primary author, but I do think it would be helpful to show this side of your research, and I'd like to incorporate that into my nomination letter (i.e. how critical your efforts have been to developments in areas such as D&A). You could still fit this onto 2 pages by making the font smaller for the references (10pt rather than 11 pt) while keeping the headings at 11 pt, and if necessary you could probably sacrifice a few of the surface temperature record references to make space for the additional references. Also, if you happen to have pdfs of the two early wigley papers, or even just the text for the abstracts, it would be great to have a little more detail about those papers so I can appropriately work them into the narrative of my letter.
thanks for any help,

mail.2008

mike
p.s. please tell Keith I was very sorry he was unable to make it here to Trieste, I was really looking forward to seeing him (as were Ed and many others here). I hope all is well w/ his daughter.

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [1]mann@psu.edu
University Park, PA 16802-5013

[2]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

- 1. <mailto:mann@psu.edu>
- 2. <http://www.met.psu.edu/dept/faculty/mann.htm>

903. 1213201481.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: P.Jones@uea.ac.uk
Subject: Re: request for some additional info.
Date: wed, 11 Jun 2008 12:24:41 -0400
Reply-to: mann@psu.edu

thanks Phil--yes, that's perfect. I just wanted to have some idea of the paper, that's more than enough info. I wouldn't bother worrying about scanning in, etc. I should have a draft letter for you to comment on within a few days or so, after I return from Trieste, talk to you later, mike
[1]P.Jones@uea.ac.uk wrote:

Mike,
Thanks.

The 1980/1981 papers. I don't have the pdfs.

1980: This paper looked (spatially) at temperatures and precipitation for the 5 warmest years during the 20th century and the 5 coldest. We then differenced these to produce what might happen. We expanded this in a DOE Tech Report to look at the warmest/coldest 20-year periods. This latter effort didn't make much difference.

1981: This looked at statistics of annual/winter/summer Temperatures for the NH and zones of the NH to see what signals might you be able to detect. SNR problem really. Showed that best place to detect was NH annual and also Tropics in summer. Last place to look was the Arctic because variability was so high.

mail.2008

I did look a while ago to see if Nature had back scanned these papers, but they hadn't.

Is the above enough? I have hard copies of these two papers - in Norwich

Cheers
Phil

Hi Phil,

thanks---yes, revised bibliography looks great.

I'll can send you a copy of my nominating letter for comment/suggestions when done.

also--can you provide one or two sentences about the '80 and '81 Nature articles w/ wigley so that I might be able to work this briefly into the narrative of my letter?

thanks,

mike

[2]P.Jones@uea.ac.uk wrote:

Mike.

Will this do? Have added in a section on D&A.
You didn't send the narrative. Will I have to alter that?

Hope to get out of AVL at 5pm tonight - thunderstorms permitting.

Cheers
Phil

```
<!DOCTYPE html PUBLIC "-//W3C//DTD HTML 4.01 Transitional//EN">
```

```
<html>
```

```
<head>
```

```
<meta content="text/html; charset=ISO-8859-1"
```

```
http-equiv="Content-Type">
```

```
</head>
```

```
<body bgcolor="#ffffff" text="#000000">
```

```
Hi Phil,<br>
```

```
<br>
```

```
OK--thanks, I'll just go w/ the H=62. That is an impressive number and almost certainly higher than the vast majority of AGU Fellows.<br>
```

```
<br>
```

```
I've attached the 2 page bibliography. I think it would be good to add some some of the more prominent D&A type papers, especially those early ones because they seem to be ahead of their time, and it is a high profile topic (more so than hydrology!). but its your call.<br>
```

```
<br>
```

```
Enjoy Asheville--say hi to Tom for me.<br>
```

```
<br>
```

```
talk to you later,<br>
```

```
<br>
```

```
mike<br>
```

```
<br>
```

mail.2008

P.Jones@uea.ac.uk wrote:
Mike,
Off to the US tomorrow for 1.5 days in Asheville.

On 1, this is what people call the H index. I've tried working this out and there is software for it on the web of science.

Problem is my surname. I get a number of 62 if I just use the software, but I have too many papers. I then waded through and deleted those in journals I'd never heard of and got 52. I think this got rid of some biologist from the 1970s/1980s, so go with 52.

I don't have pdfs of the early papers. I won't be able to do anything for a few days either. When do you want this in, by the way? Can you email me the piece I wrote for you, as I don't have this on my lap top. I can then pick it up tomorrow at some airport.

The D&A work has always been with others. There is another area on hydrology that I omitted as well.

Keith's daughter is OK. She had the operation last Tuesday. He should be over in Birmingham this weekend.

Cheers
Phil

Hi Phil,

I'm continuing to work on your nomination package (here in my hotel room in Trieste--the weather isn't any good!). If its possible for a case to be too strong, we may have that here! Lonnie is also confirmed as supporting letter writer, along w/ Kevin, Ben, Tom K, and Jean J. (4 of the 5 are already AGU fellows, which I'm told is important! Surprisingly, Ben is not yet, nor am I. But David Thompson is (quite young for one of these). I'm guessing Mike wallace and Susan Solomon might have had something to do w/ that ;)

Anyway, I wanted to check w/ you on two things:

1. One thing that people sometimes like to know is the maximum value of "N" where "N" is the number of papers an individual authored/co-authored that have more than N citations. N=40 (i.e., an individual has published at least 40 papers that have each been cited at least 40 times) is supposedly an important threshold for admission in the U.S. National Academy of Sciences. I'm guessing your N is significantly greater than that, and it would be nice to cite that if possible. Would you mind figuring out that number and sending--I think it would be useful is

mail.2008

really sealing the case.

2. Would you mind considering a minor revision of your 2 page bibliography. In my nomination letter, I'm trying to underscore the diverse areas where you've made major contributions, and I think its well known and obvious to many that two of these are instrumental data and paleoclimate reconstructions. But it occurs to me that it is equally important to stress your work in detection of anthropogenic impacts on climate w/ both models and observations. For example, your early Nature papers w/ wigley. in '80 and '81 seem to be among the earliest efforts to try to do this (though I don't have copies of the papers, so can't read them!), and that seems very much worth highlighting to me. My suggestion is that you add a category on "Anthropogenic Climate Signal" detection and include this work (say, 8 or so of the key papers in this area including the two early Nature one's w/ wigley) as well as some of your later work w/ Santer/Tett/Thorne/Hegerl/Barnett. I realize that most of your work in this area isn't as primary author, but I do think it would be helpful to show this side of your research, and I'd like to incorporate that into my nomination letter (i.e. how critical your efforts have been to developments in areas such as D&A). You could still fit this onto 2 pages by making the font smaller for the references (10pt rather than 11 pt) while keeping the headings at 11 pt, and if necessary you could probably sacrifice a few of the surface temperature record references to make space for the additional references.

Also, if you happen to have pdfs of the two early wigley papers, or even just the text for the abstracts, it would be great to have a little more detail about those papers so I can appropriately work them into the narrative of my letter.

thanks for any help,

mike

p.s. please tell Keith I was very sorry he was unable to make it here to Trieste, I was really looking forward to seeing him (as were Ed and many others here). I hope all is well w/ his daughter.

-- Michael E. Mann Associate Professor Director, Earth System Science Center (ESSC) Department of Meteorology Phone: (814) 863-4075 503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [7]mann@psu.edu University Park, PA 16802-5013 [9]http://www.met.psu.edu/dept/faculty/mann.htm

```
</pre>
</blockquote>
<pre wrap=""><!-->
</pre>
```

mail.2008

</blockquote>

<pre class="moz-signature" cols="72">--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: <a
class="moz-txt-link-abbreviated"
href=[10]"mailto:mann@psu.edu">[11]mann@psu.edu
University Park, PA 16802-5013

<a class="moz-txt-link-freetext"
href=[12]"http://www.met.psu.edu/dept/faculty/mann.htm">[13]http://www.met.psu.edu/d
ept/faculty/mann
.htm

</pre>

</body>

</html>

--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [14]mann@psu.edu
University Park, PA 16802-5013

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

--

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

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [16]mann@psu.edu
University Park, PA 16802-5013

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

References

1. mailto:P.Jones@uea.ac.uk
2. mailto:P.Jones@uea.ac.uk
3. mailto:P.Jones@uea.ac.uk
4. mailto:P.Jones@uea.ac.uk
5. mailto:mid:1079.87.113.67.115.1212941466.squirrel@webmail.uea.ac.uk
6. mailto:mann@psu.edu

mail.2008

- 7. mailto:mann@psu.edu
- 8. http://www.met.psu.edu/dept/faculty/mann.htm
- 9. http://www.met.psu.edu/dept/faculty/mann.htm
- 10. mailto:mann@psu.edu
- 11. mailto:mann@psu.edu
- 12. http://www.met.psu.edu/dept/faculty/mann.htm
- 13. http://www.met.psu.edu/dept/faculty/mann.htm
- 14. mailto:mann@psu.edu
- 15. http://www.met.psu.edu/dept/faculty/mann.htm
- 16. mailto:mann@psu.edu
- 17. http://www.met.psu.edu/dept/faculty/mann.htm

904. 1213387146.txt

#####

From: Ben Santer <santer1@llnl.gov>
 To: amlibpub@gmail.com
 Subject: Your website
 Date: Fri, 13 Jun 2008 15:59:06 -0700
 Reply-to: santer1@llnl.gov

<x-flowed>
 To the Editor
 American Liberty Publishers
 Minneapolis, MN 55418

Dear Sir,

Your website (http://www.amlibpub.com/top/contact_us.html) was recently brought to my attention. On this site, you make the following claims:

"In the Second Assessment Report, Benjamin Santer, lead author of a crucial study, falsified a chart to make it appear to support global warming—a conclusion not supported at all by the original data. But two climatologists, Knappenberger and Michaels, looked up the data and exposed the fraud. Santer said he adjusted the data to make it agree with political policy."

These claims have no factual basis whatsoever, and are demonstrably libelous. I did not falsify data. I did not commit fraud. I did not - nor have I ever - "adjusted" scientific data "to make it agree with political policy." Nor did I ever state that I had made data adjustments in order to conform to political policy.

I request that you retract these claims immediately. They are completely fictitious, and are harmful to my scientific reputation. If you do not retract these claims immediately, I will transfer this matter to the attention of legal staff at Lawrence Livermore National Laboratory.

Sincerely,

Dr. Benjamin Santer
 U.S. Dept. of Energy Distinguished Scientist (2006)
 Ernest Orlando Lawrence Award (2002)
 John D. and Catherine T. MacArthur Fellow (1998)

 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.

mail.2008

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

</x-flowed>

905. 1213882741.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: nomination letter
Date: Thu, 19 Jun 2008 09:39:01 -0400
Reply-to: mann@psu.edu

<x-flowed>
thanks Phil--fixed!

waiting on two more letters, then I'll send in the package to AGU.
Should be a no-brainer!

talk to you later,

mike

Phil Jones wrote:

>
> Mike,
> There is one type in your nomination letter. I missed it first
> time I read it.
>
> In the second paragraph, second line remove the first 'surface'. You
> have
> two one before and one after (CRU). Just the one after needed.
>
> Cheers
> Phil
>
>

> At 16:59 18/06/2008, you wrote:

>> hey Phil, at Dulles waiting for flight to Orlando Florida.

>>
>> IUGG is the first time I ever met you. but I believe I had already
>> corresponded w/ you about some of the work I was doing w/ Ray w/
>> proxy records. But the thing we talked about was the quality of the
>> early Trenberth and Paolino SLP gridbox data. you alerted me to some
>> of the early problems w/ that dataset. It was very helpful. I was
>> young and naive!
>> anyway, it made a very positive impression on me that you were so
>> approachable. im' sure many others agree.

>> got to run to my flight now. talk later,

>>
>> mike

>> Phil Jones wrote:

>>>
>>> Mike,
>>> This is fine. I don't remember talking to you at IUGG in Boulder !
>>> I am approachable though and have talked to lots of people. I get

mail.2008

>>> people
>>> coming up to me now saying we met in 199? and have no recall
>>> of our meeting - sometime no recall of even going to the meeting
>>> where I was supposed to have met them!
>>>
>>> Another thanks for putting this all together.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>> At 22:04 14/06/2008, you wrote:
>>>> Hi Phil,
>>>>
>>>> I've attached a copy of my nomination letter. I just want to make
>>>> sure I've got all my facts right--please let me know if there is
>>>> anything I've gotten wrong or should be changed. I would be shocked
>>>> if this doesn't go through--you're a no-brainer, and long overdue
>>>> for this.
>>>>
>>>> I've got letters from 3 of the 5 other letter writers now, waiting
>>>> on the 2 last ones, then will submit the package.
>>>>
>>>> talk to you alter,
>>>>
>>>> mike
>>>>
>>>> --
>>>> Michael E. Mann
>>>> Associate Professor
>>>> Director, Earth System Science Center (ESSC)
>>>>
>>>> Department of Meteorology Phone: (814) 863-4075
>>>> 503 Walker Building FAX: (814) 865-3663
>>>> The Pennsylvania State University email: mann@psu.edu
>>>> University Park, PA 16802-5013
>>>>
>>>> <http://www.met.psu.edu/dept/faculty/mann.htm>
>>>>
>>>>
>>>>
>>>> Prof. Phil Jones
>>>> Climatic Research Unit Telephone +44 (0) 1603 592090
>>>> School of Environmental Sciences Fax +44 (0) 1603 507784
>>>> University of East Anglia
>>>> Norwich Email p.jones@uea.ac.uk
>>>> NR4 7TJ
>>>> UK
>>>> -----
>>>>
>>>>
>>>>
>>>> --
>>>> Michael E. Mann
>>>> Associate Professor
>>>> Director, Earth System Science Center (ESSC)
>>>>
>>>> Department of Meteorology Phone: (814) 863-4075
>>>> 503 Walker Building FAX: (814) 865-3663
>>>> The Pennsylvania State University email: mann@psu.edu
>>>> University Park, PA 16802-5013
>>>>

mail.2008

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

>>

>

> Prof. Phil Jones

> Climatic Research Unit Telephone +44 (0) 1603 592090

> School of Environmental Sciences Fax +44 (0) 1603 507784

> University of East Anglia

> Norwich

Email p.jones@uea.ac.uk

> NR4 7TJ

> UK

> -----

>

--

Michael E. Mann

Associate Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology

503 Walker Building

The Pennsylvania State University

University Park, PA 16802-5013

Phone: (814) 863-4075

FAX: (814) 865-3663

email: mann@psu.edu

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

</x-flowed>

906. 1214228874.txt

#####

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

To: Tim Osborn <t.osborn@uea.ac.uk>, P.Jones@uea.ac.uk, "Caspar Ammann"

<ammann@ucar.edu>

Subject: Re: Fwd: IPCC FOIA Request

Date: Mon Jun 23 09:47:54 2008

Caspar

I have been of the opinion right from the start of these FOI requests, that our private ,

inter-collegial discussion is just that - PRIVATE . Your communication with individual

colleagues was on the same basis as that for any other person and it discredits the IPCC

process not one iota not to reveal the details. On the contrary, submitting to these

"demands" undermines the wider scientific expectation of personal confidentiality . It is

for this reason , and not because we have or have not got anything to hide, that I believe

none of us should submit to these "requests". Best wishes

Keith

At 09:01 23/06/2008, Tim Osborn wrote:

Hi Caspar,

I've just had a quick look at CA. They seem to think that somehow it is an advantage to

send material outside the formal review process. But *anybody* could have emailed us

mail.2008

then we directly. It is in fact a disadvantage! If it is outside the formal process
could simply ignore it, whereas formal comments had to be formally considered.
Strange that they don't realise this and instead argue for some secret conspiracy that
they are excluded from!
I'm not even sure if you sent me or Keith anything, despite McIntyre's
conviction! But I'd ignore this guy's request anyway. If we aren't consistent in keeping our
discussions out of the public domain, then it might be argued that none of them
can be kept private. Apparently, consistency of our actions is important.

Best wishes

Tim

At 07:37 23/06/2008, P.Jones@uea.ac.uk wrote:

Caspar,

In Zurich at MeteoSwiss for a meeting this week.

It doesn't discredit IPCC!

Cheers

Phil

> FYI, more later.

> Caspar

>

>

> Begin forwarded message:

>

>> From: Brian Lynch <killballyowen2003@yahoo.co.uk>

>> Date: June 21, 2008 3:30:28 PM MDT

>> To: ammann@ucar.edu

>> Subject: IPCC FOIA Request

>> Reply-To: killballyowen2003@yahoo.co.uk

>>

>> Dear Sir,

>>

>> I have read correspondence on web about your letter to the in
>> relation to expert comments on IPCC chapter 6 sent directly by you
>> to Keith Briffa, sent outside the formal review process.

>>

>> The refusal to give these documents tends to discredit you and the
>> IPCC in the eyes of the public,

>>

>> Could I suggest that you make your letter and documents public. I
>> would be very glad if you gave me a copy and oblige,

>>

>> Yours faithfully,

>>

>> Brian Lynch

>> Galway

>>

>> Sent from Yahoo! Mail.

>> A Smarter Email.

>

> Caspar M. Ammann

> National Center for Atmospheric Research

> Climate and Global Dynamics Division - Paleoclimatology

> 1850 Table Mesa Drive

> Boulder, CO 80307-3000

> email: ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

>

>

>

mail.2008

>

Dr Timothy J Osborn, Academic Fellow
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ, UK
e-mail: t.osborn@uea.ac.uk
phone: +44 1603 592089
fax: +44 1603 507784
web: [1]http://www.cru.uea.ac.uk/~timo/
sunclock: [2]http://www.cru.uea.ac.uk/~timo/sunclock.htm

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

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

References

1. http://www.cru.uea.ac.uk/~timo/
2. http://www.cru.uea.ac.uk/~timo/sunclock.htm
3. http://www.cru.uea.ac.uk/cru/people/briffa/

907. 1214229243.txt

#####

From: Tim Osborn <t.osborn@uea.ac.uk>
To: P.Jones@uea.ac.uk, k.briffa@uea.ac.uk, ammann@ucar.edu
Subject: Re: CA
Date: Mon Jun 23 09:54:03 2008

Hi Phil, Keith and "Confidential Agent Ammann",
At 17:00 21/06/2008, P.Jones@uea.ac.uk wrote:

This is a confidential email

So is this.

Have a look at Climate Audit. Holland has put all the responses and letters up. There are three threads - two beginning with Fortress and a third later one. Worth saving the comments on a Jim Edwards - can you do this Tim?

I've saved all three threads as they now stand. No time to read all the comments, but I did note in "Fortress Met Office" that someone has provided a link to a website that helps you to submit FOI requests to UK public institutions, and subsequently someone has made a further FOI request to Met Office and someone else made one to DEFRA. If it turns into an organised campaign designed more to inconvenience us than to obtain useful information, then we may be able to decline all related requests without spending ages on

mail.2008

considering
them. Worth looking out for evidence of such an organised campaign.
Tim

908. 1215477224.txt

#####

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Andrew Revkin" <anrevk@nytimes.com>
Subject: Re: clearing up climate trends sans ENSO and perhaps PDO?
Date: Mon, 7 Jul 2008 20:33:44 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: gschmidt@giss.nasa.gov, mann@psu.edu, davet@atmos.colostate.edu,
p.jones@uea.ac.uk, david.parker@metoffice.gov.uk, wpatzert@jpl.nasa.gov,
ackerman@atmos.washington.edu, wallace@atmos.washington.edu, tbarnett-ul@ucsd.edu,
sarachik@atmos.washington.edu, peter.thorne@metoffice.gov.uk,
john.kennedy@metoffice.gof.uk, cwunsch@mit.edu

Andy
Here's some further results, based on the time series for 1900 to 2007

Results:

- (0) correlation between ENSO and PDO: for the smoothed IPCC decadal filter: 0.490662
- (0) correlation between ENSO and PDO: for the annual means: 0.527169
- (0) regression coef for PDO with global T : 0.0473447
- (0) regression coef for N34 with global T : 0.0664886

Data sources:

```

;-----
; PDO: http://www.jisao.washington.edu/pdo/
;      http://jisao.washington.edu/pdo/PDO.latest
;-----
; N34: http://www.cgd.ucar.edu/cas/catalog/climind/Nino_3_3.4_indices.html
;      http://www.cgd.ucar.edu/cas/catalog/climind/TNI_N34/index.html#Sec5
;-----
; CRU: http://www.cru.uea.ac.uk/cru/data/temperature/
; Hadcrut: http://www.cru.uea.ac.uk/cru/data/temperature/hadcrut3vgl.txt
;=====
; Files were manually stripped for 1900 to 2007
;=====

```

These numbers mean that for a one standard deviation in the ENSO index there is 0.066C change in global T, or from PDO: 0.047C, but that much of the latter comes from the ENSO index. Very roughly, since the correlation is 0.5 between PDO and ENSO, half of the 0.066 or 0.033C of the 0.047 is from ENSO. Strictly one should do this properly using screening regression.

Kevin

> dear all,
> re-sending because of a glitch.
>
> finally got round to posting on an earlier inquiry I made to some of
> you about whether there was a 'clean' graph of multi-decades
> temperature trends with ENSO wiggles removed -- thanks to gavin (and

mail.2008

> david thompson) posting on realclimate.
> here's Dot Earth piece with link to Realclimate etc..
>
> <http://dotearth.blogs.nytimes.com/2008/07/07/climate-trends-with-some-noise-removed/?ex=1216094400&en=a57177d93165cba3&ei=5070>
>
> next step is PDO. has anyone characterized how much impact (if any)
> PDO has on hemispheric or global temp trends, and if so is there a
> graph showing what happens when that's accounted for?
>
> as you are doubtless aware, this is another bone of contention with a
> lot of the anti-greenhouse-limits folks and some scientists (the post
> 1970s change is a PDO thing, etc etc). hoping to show a bit of how
> that works.
>
> thanks for any insights.
> and i encourage you to comment and provide links etc with the current
> post to add context etc.
>
> --
> Andrew C. Revkin
> The New York Times / Science
> 620 Eighth Ave., NY, NY 10018
> Tel: 212-556-7326 Mob: 914-441-5556
> Fax: 509-357-0965
> www.nytimes.com/revkin

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

909. 1215712600.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]
Date: Thu, 10 Jul 2008 13:56:40 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Phil,

The wedding was really very moving and beautiful. I had a great time. I'm sending along a picture of Tom and Helen which was taken at Granite Island (near Victor Harbor). I don't know whether I've ever seen Tom as happy as he is now...

Myles (if it is Myles) was a bit pedantic in his second review. Karl (who is a very-mild-mannered guy) described the tone of the review as "whining". It seems like the Reviewer was saying, "I'm a lot smarter than you, and I could do all of this stuff much better than you've done". I was very unhappy about the "wilfully ignoring" bit. That was completely uncalled for.

Have a great time at Lake Constance, Phil. It's a beautiful part of the
Page 220

world.

Best regards, and best wishes to Ruth,

Ben

P.Jones@uea.ac.uk wrote:

> Ben,
> Will read the comments in detail tomorrow, when at CRU.
> I presume the wedding went well and a good time was had
> by all.
>
> I'm in CRU tomorrow, but away next week. I'm off to one
> your old hunting grounds - Friedrichshafen. I am going to
> a summer school on the other side of the Lake near Konstanz.
> Can't recall the village name - something like Treffpunkt.
>
> Only gone a week, back Friday week.
>
> From a quick scan below Myles does seem to be a pain!
> As we both know he can be.
>
> Cheers
> Phil

>> Dear folks,

>> I just returned from my trip to Australia - I had a great time there.
>> Now (sadly) it's back to the reality of Douglass et al. I'm forwarding
>> the second set of comments from the two Reviewers. As you'll see,
>> Reviewer 1 was very happy with the revisions we've made to the paper.
>> Reviewer 2 was somewhat crankier. The good news is that the editor
>> (Glenn McGregor) will not send the paper back to Reviewer 2, and is
>> requesting only minor changes in response to the Reviewer's comments.
>>
>> Once again, Reviewer 2 gets hung up on the issue of fitting higher-order
>> autoregressive models to the temperature time series used in our paper.
>> As noted in our response to the Reviewer, this is a relatively minor
>> technical point. The main point is that we include an estimate of the
>> standard error of the observed trend. DCPS07 do not, which is the main
>> error in their analysis.
>>
>> In calculating modeled and observed standard errors, we assume an AR-1
>> model of the regression residuals. This assumption is not unreasonable
>> for many meteorological time series. We and others have made it in a
>> number of previous studies.
>>
>> Reviewer 2 would have liked us to fit higher-order autoregressive models
>> to the T2, T2LT, and TS-T2LT time series. This is a difficult business,
>> particularly given the relatively short length of the time series
>> available here. There is no easy way to reliably estimate the parameters
>> of higher-order AR models from 20 to 30 years of data. The same applies
>> to reliable estimation of the spectral density at frequency zero (since
>> we have only 2-3 independent samples for estimating the spectral density
>> at frequency zero). Reviewer 2's comments are not particularly relevant
>> to the specific problem we are dealing with here.
>>
>> It's also worth mentioning that use of higher-order AR models for
>> estimating trend standard errors would likely lead to SMALLER effective
>> sample sizes and LARGER standard errors, thus making it even more
>> difficult to find significant differences between modelled and observed
>> trends! Our use of an AR-1 model makes it easier for us to obtain

mail.2008

>> "DCPS07-like" results, and to find significant differences between
>> modelled and observed trends. DCPS cannot claim, therefore, that our
>> test somehow stacks the deck in favor of obtaining a non-significance
>> trend difference - which they might claim if we used a
>> (poorly-constrained) higher-order AR model for estimating standard
>> errors.

>>
>> The Reviewer does not want to "see the method proposed in this paper
>> become established as the default method of estimating standard errors
>> in climatological time series". We do not claim universal applicability
>> of our approach. There may well be circumstances in which it is more
>> appropriate to use higher-order AR models in estimating standard errors.
>> I'd be happy to make a statement to this effect in the revised paper.

>>
>> I have to confess that I was a little ticked off by Reviewer 2's
>> comments. The bit about "wilfully ignoring" time series literature was
>> uncalled for. Together with my former MPI colleague wolfgang
>> Brueggemann, I've fooled around with a lot of different methods of
>> estimating standard errors, in both the time domain and frequency
>> domain. One could write a whole paper on this subject alone. Such a
>> paper would not help us to expose the statistical deficiencies in
>> DCPS07. Nor would in-depth exploration of this issue lead to the shorter
>> paper requested by the Reviewer.

>>
>> It should take me a few days to revise the paper and draft a response to
>> Reviewer 2's comments. I'll send you the revised paper and draft
>> response early next week. Slowly but surely, we are getting there!

>>
>> 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
>> -----

>>
>>
>
>
>

--

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>

mail.2008

Attachment Converted: "c:\eudora\attach\DSCN2786.JPG"

910. 1215713915.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>
Subject: [Fwd: Re: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]]
Date: Thu, 10 Jul 2008 14:18:35 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Glenn,

I thought you might be interested in this email exchange with Francis
Zwiers. It's directly relevant to the third criticism raised by Reviewer 2.

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

</x-flowed>
X-Account-Key: account1
Return-Path: <francis.zwiers@ec.gc.ca>
Received: from mail-1.llnl.gov ([unix socket])
by mail-1.llnl.gov (Cyrus v2.2.12) with LMTPA;
Thu, 10 Jul 2008 13:08:08 -0700
Received: from nspiron-2.llnl.gov (nspiron-2.llnl.gov [128.115.41.82])
by mail-1.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.7 \$) with ESMTMP id
m6AK864P023034
for <santer1@mail.llnl.gov>; Thu, 10 Jul 2008 13:08:07 -0700
X-Attachments: None
X-IronPort-AV: E=McAfee;i="5200,2160,5336"; a="21284881"
X-IronPort-AV: E=Sophos;i="4.30,340,1212390000";
d="scan'208";a="21284881"
Received: from nsziron-2.llnl.gov ([128.115.249.82])
by nspiron-2.llnl.gov with ESMTMP; 10 Jul 2008 13:08:06 -0700
X-Attachments: None
X-IronPort-Anti-Spam-Filtered: true
X-IronPort-Anti-Spam-Result: Ao4AAHkJdkjH1BOCmmdsb2JhbACSJgEBAQEBCAUIBxGfMgE
X-IronPort-AV: E=McAfee;i="5200,2160,5336"; a="42743336"
X-IronPort-AV: E=Sophos;i="4.30,340,1212390000";
d="scan'208";a="42743336"
Received: from ecdown130.tor.ec.gc.ca (HELO OntExch1.ontario.int.ec.gc.ca)
([199.212.19.130])
by nsziron-2.llnl.gov with ESMTMP; 10 Jul 2008 13:07:46 -0700
Received: from OntExch3.ontario.int.ec.gc.ca ([142.97.202.217]) by
OntExch1.ontario.int.ec.gc.ca with Microsoft SMTPSVC(6.0.3790.3959);
Thu, 10 Jul 2008 16:07:45 -0400
Content-class: urn:content-classes:message

mail.2008

MIME-Version: 1.0
Content-Type: text/plain;
charset="us-ascii"
X-MimeOLE: Produced By Microsoft Exchange V6.5
Subject: RE: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]
Date: Thu, 10 Jul 2008 16:07:45 -0400
Message-ID: <33F9E32CDB0917428758DD583E747CC804095CEA@OntExch3.ontario.int.ec.gc.ca>
In-Reply-To: <487663E3.1040309@l1n1.gov>
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]
Thread-Index: ACjiw9lJw91pKfupQQOFebAg5s2/SgAAHtnA
References: <48764B2C.5050004@l1n1.gov>
<33F9E32CDB0917428758DD583E747CC804095CB7@OntExch3.ontario.int.ec.gc.ca>
<487663E3.1040309@l1n1.gov>
From: "Zwiers,Francis [Ontario]" <francis.zwiers@ec.gc.ca>
To: <santer1@l1n1.gov>
X-OriginalArrivalTime: 10 Jul 2008 20:07:45.0611 (UTC) FILETIME=[9E3BB9B0:01C8E2C8]

Hi Ben, sure, that would be fine.

Cheers, Francis

Francis Zwiers
Director, Climate Research Division, Environment Canada
4905 Dufferin St., Toronto, Ont. M3H 5T4
Phone: 416 739 4767, Fax 416 739 5700

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

From: Ben Santer [mailto:santer1@l1n1.gov]
Sent: July 10, 2008 3:33 PM
To: Zwiers,Francis [Ontario]
Subject: Re: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]

Dear Francis,

Thanks - this information will be extremely helpful in responding to Reviewer 2. I really do feel that the Reviewer is getting overly exercised about a relatively minor technical point. As you note, the key issue is that, in terms of the statistical significance testing, we are making it easier to get a "Douglass-like" result by using an AR-1 model for calculating the adjusted standard errors.

I'm concerned that going down the road proposed by Reviewer 2 could leave us open to unjustified criticism. It would be a shame if Douglass et al. argued (erroneously) that our failure to find significant differences between modelled and observed trends was spurious, and arose primarily from use of higher-order autoregressive models for calculating the adjusted standard errors.

would it be o.k. to share your email with Glenn McGregor and with my other coauthors on the paper? Since you've looked at these issues in detail in your previous papers with Thiebaut and with Hans, your comments would be very useful background information for Glenn.

with best regards,

Ben

Zwiers,Francis [Ontario] wrote:
> Hi Ben,
>

mail.2008

> Sorry the 2nd reviewer is being a pain. As you say, there is already
> quite a bit of literature on dealing with dependence in tests of the
> mean (and this referee would have been critical if this paper had
> gone over that ground again :)).

>
> Regardless, you might be interested in the attached papers. Both
> contain relevant information and might help to formulate a response to
> the editor.

>
> Thiebaut and Zwiers show that the equivalent sample size is hard to
> estimate well, particularly from small samples. The approach proposed
> by the reviewer is what we termed the "ARMA" method, and it produces
> equivalent sample size estimates that have unacceptably large RMSE's
> when the sample is small, even when the time series in question is not

> very persistent (see Table 6).

>
> Zwiers and von Storch show the performance of an estimator of
> equivalent sample size using the approach you use (i.e., assume the
> data are AR(1)). They show that the equivalent sample size tends to be

> over-estimated (Table 1) particularly when samples are small, and that
> the corresponding t-test tends to operate at significance levels above

> the nominal level (i.e., rejects too frequently - Table 2). So using
> such a test in effect gives those who would like to reject the null
> hypothesis a small leg up.

>
> Directly comparable results are not shown in the two papers, but you
> can infer, from the comparison between equivalent sample size results
> (Table
> 6 in TZ, Table 2 in ZVS) that the "ARMA" approach for estimating
> equivalent sample size would be much less reliable than the approach
> that you are using (and thus, the sampled series would have to be very

> far from being AR(1) for the ARMA approach to be beneficial). The
> absolute key is to keep things as parsimonious as possible - there is
> simply not enough data to entertain complex models of the
> auto-covariance structure.

>
> Cheers, Francis

>
> Francis Zwiers
> Director, Climate Research Division, Environment Canada
> 4905 Dufferin St., Toronto, Ont. M3H 5T4
> Phone: 416 739 4767, Fax 416 739 5700

>
> -----Original Message-----
> From: Ben Santer [mailto:santer1@llnl.gov]
> Sent: July 10, 2008 1:47 PM
> To: Thorne, Peter; Leopold Haimberger; Karl Taylor; Tom Wigley; John
> Lanzante; ssolomon@frii.com; Melissa Free; peter gleckler; 'Philip D.
> Jones'; Thomas R Karl; Steve Klein; carl mears; Doug Nychka; Gavin
> Schmidt; Steven Sherwood; Frank Wentz
> Subject: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]

>
> Dear folks,

>
> I just returned from my trip to Australia - I had a great time there.
> Now (sadly) it's back to the reality of Douglass et al. I'm forwarding

mail.2008

> the second set of comments from the two Reviewers. As you'll see,
> Reviewer 1 was very happy with the revisions we've made to the paper.
> Reviewer 2 was somewhat crankier. The good news is that the editor
> (Glenn McGregor) will not send the paper back to Reviewer 2, and is
> requesting only minor changes in response to the Reviewer's comments.
>
> Once again, Reviewer 2 gets hung up on the issue of fitting
> higher-order autoregressive models to the temperature time series used
> in our paper.
> As noted in our response to the Reviewer, this is a relatively minor
> technical point. The main point is that we include an estimate of the
> standard error of the observed trend. DCPS07 do not, which is the main
> error in their analysis.
>
> In calculating modeled and observed standard errors, we assume an AR-1
> model of the regression residuals. This assumption is not unreasonable
> for many meteorological time series. We and others have made it in a
> number of previous studies.
>
> Reviewer 2 would have liked us to fit higher-order autoregressive
> models to the T2, T2LT, and TS-T2LT time series. This is a difficult
> business, particularly given the relatively short length of the time
> series available here. There is no easy way to reliably estimate the
> parameters of higher-order AR models from 20 to 30 years of data. The
> same applies to reliable estimation of the spectral density at
> frequency zero (since we have only 2-3 independent samples for
> estimating the spectral density at frequency zero). Reviewer 2's
> comments are not particularly relevant to the specific problem we are
> dealing with here.
>
> It's also worth mentioning that use of higher-order AR models for
> estimating trend standard errors would likely lead to SMALLER
> effective sample sizes and LARGER standard errors, thus making it even
> more difficult to find significant differences between modelled and
> observed trends! Our use of an AR-1 model makes it easier for us to
> obtain "DCPS07-like" results, and to find significant differences
> between modelled and observed trends. DCPS cannot claim, therefore,
> that our test somehow stacks the deck in favor of obtaining a
> non-significance trend difference - which they might claim if we used
> a
> (poorly-constrained) higher-order AR model for estimating standard
> errors.
>
> The Reviewer does not want to "see the method proposed in this paper
> become established as the default method of estimating standard errors
> in climatological time series". We do not claim universal
> applicability of our approach. There may well be circumstances in
> which it is more appropriate to use higher-order AR models in
> estimating standard errors.
>
> I'd be happy to make a statement to this effect in the revised paper.
>
> I have to confess that I was a little ticked off by Reviewer 2's
> comments. The bit about "wilfully ignoring" time series literature was
> uncalled for. Together with my former MPI colleague Wolfgang
> Brueggemann, I've fooled around with a lot of different methods of

mail.2008

> estimating standard errors, in both the time domain and frequency
> domain. One could write a whole paper on this subject alone. Such a
> paper would not help us to expose the statistical deficiencies in
> DCPS07. Nor would in-depth exploration of this issue lead to the
> shorter paper requested by the Reviewer.
>
> It should take me a few days to revise the paper and draft a response
> to Reviewer 2's comments. I'll send you the revised paper and draft
> response early next week. Slowly but surely, we are getting there!
>
> 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
> -----
> --
> ----
>

--

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

911. 1216753979.txt

#####

From: Tim Osborn <t.osborn@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: A long and rocky road...
Date: Tue Jul 22 15:12:59 2008

Dear Ben,
well, thanks for your thanks. I'm not sure that I did all that much, but glad
that the
small amount is appreciated. It's a shame that the process couldn't have been
quicker
still, but hopefully the final production stage will pass smoothly.
Thanks for the copy of the paper, which I've skim read already -- looks very

mail.2008

Carefully done

and therefore convincing (I'm sure you already heard that from others).

I note that you also provide some supporting online material (SOM). Provision of SOM is a

relatively new facility for IJoC to offer and it may be suffering from teething problems.

A paper of mine (Maraun et al.) that appeared online in IJoC back in February still has its

SOM missing! Hopefully this is a one-off omission, but I'll now email Glenn to remind him

of this in relation to my paper and also point out that your paper has SOM. I think this

is a problem on the publisher's side of things rather than an editorial problem.

Because of our absent SOM, we've temporarily posted a copy of the SOM on our personal

website. If your SOM was delayed, and if you think that critics might complain if the

paper appears without the SOM, you might want to post a copy of the SOM on your own website

when the paper appears online. But hopefully there'll be no problem with it!

I heard you had a recent trip to Australia for Tom's wedding -- hope that was fun!

Best regards

Tim

At 22:28 21/07/2008, you wrote:

Dear Tim,

Our response to the Douglass et al. IJoC paper has now been formally accepted, and is

"in press" at IJoC. I've appended a copy of the final version of the manuscript. It's

been a long and rocky road, and I'll be quite glad if I never have to write another MSU

paper again - ever!

I'd be grateful if you handled the paper in confidence at present. Since IJoC now has

online publication, we're hoping that the paper will appear in the next 4-6 weeks.

Hope you are well, Tim. Thanks for all your help with the tricky job of brokering the

submission of the paper to IJoC.

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

912. 1217431501.txt

#####

From: Mike MacCracken <mmaccrac@comcast.net>
To: Jason Lowe <jason.lowe@metoffice.gov.uk>, Jerry Meehl <meehl@ucar.edu>
Subject: Re: Proposed experiment design for CMIP5
Date: Wed, 30 Jul 2008 11:25:01 -0400

mail.2008

Cc: "Cox, Peter" <P.M.Cox@exeter.ac.uk>, Karl Taylor <taylor13@llnl.gov>, <bryant.mcavaney@lmd.jussieu.fr>, Curtis Covey <covey1@llnl.gov>, "Mitchell, John FB (Chief Scientist)" <john.f.mitchell@metoffice.gov.uk>, <mlatif@ifm-geomar.de>, <Tom.Delworth@noaa.gov>, Andreas Hense <ahense@uni-bonn.de>, Asgeir Sorteberg <asgeir.sorteberg@bjerknes.uib.no>, Erich Roeckner <roeckner@dkrz.de>, Evgeny Volodin <volodin@inm.ras.ru>, "Gary L. Russell" <Gary.L.Russell@nasa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, <GFDL.Climate.Model.Info@noaa.gov>, Greg Flato <gflato@ec.gc.ca>, Helge Drange <helge.drange@nersc.no>, Jean-Francois Royer <jean-francois.royer@meteo.fr>, Jean-Louis Dufresne <Jean-Louis.Dufresne@lmd.jussieu.fr>, Jozef Syktus <jozef.syktus@qld.gov.au>, Julia Slingo <J.M.Slingo@reading.ac.uk>, Kimoto Masahide <kimoto@ccsr.u-tokyo.ac.jp>, Peter Gent <gent@ucar.edu>, Qingquan Li <liqq@cma.gov.cn>, Seita Emori <emori@nies.go.jp>, Seung-Ki Min <seung-ki.min@ec.gc.ca>, Shan Sun <ssun@giss.nasa.gov>, Shoji Kusunoki <skusunok@mri-jma.go.jp>, Shuting Yang <shuting@dmi.dk>, Silvio Gualdi <gualdi@bo.ingv.it>, Stephanie Legutke <legutke@dkrz.de>, Tongwen Wu <twwu@cma.gov.cn>, Tony Hirst <Tony.Hirst@csiro.au>, Toru Nozawa <nozawa@nies.go.jp>, Wilhelm May <wm@dmi.dk>, Won-Tae Kwon <wontk@metri.re.kr>, Ying Xu <xuying@cma.gov.cn>, Yong Luo <yluo@cma.gov.cn>, Yongqiang Yu <yyq@lasg.iap.ac.cn>, Kamal Puri <K.Puri@bom.gov.au>, Tim Stockdale <Tim.Stockdale@ecmwf.int>, Gabi Hegerl <hegerl@duke.edu>, James Murphy <james.murphy@metoffice.gov.uk>, Marco Giorgetta <marco.giorgetta@zmaw.de>, George Boer <George.Boer@ec.gc.ca>, Myles Allen <m.allen1@physics.ox.ac.uk>, claudia tebaldi <claudia.tebaldi@gmail.com>, Ben Santer <santer1@llnl.gov>, Tim Barnett <tbarnett-ul@ucsd.edu>, Nathan Gillett <n.gillett@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, Dáithí Stone <stoned@atm.ox.ac.uk>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, Francis Zwiers <Francis.Zwiers@ec.gc.ca>, Ken Sperber <sperber1@llnl.gov>, Dave Bader <bader2@llnl.gov>, <boyle5@llnl.gov>, Stephen Klein <klein21@llnl.gov>, "A. Pier Siebesma" <siebesma@knmi.nl>, William Rossow <wbrossow@gmail.com>, Chris Bretherton <breth@atmos.washington.edu>, George Tselioudis <gtselioudis@giss.nasa.gov>, Mark Webb <mark.webb@metoffice.gov.uk>, Sandrine Bony <Sandrine.Bony@lmd.jussieu.fr>, James Hack <jhack@cgd.ucar.edu>, Martin Miller <Martin.Miller@ecmwf.int>, Ken Kunkel <kkunkel@uiuc.edu>, Christian Jakob <c.jakob@bom.gov.au>, Kathy Hibbard <kathyh@cgd.ucar.edu>, "Eyring, Veronika" <veronika.eyring@dlr.de>, <pasb@dsm-mail.saclay.cea.fr>, <giorgi@ictp.trieste.it>, <c.lequere@uea.ac.uk>, <naki@eeg.tuwien.ac.at>, <stephen.griffies@noaa.gov>, Pierre Friedlingstein <pierre.friedlingstein@cea.fr>, Olivier Boucher <olivier.boucher@metoffice.gov.uk>, Bala Govindasamy <bala1@llnl.gov>, Jonathan Gregory <j.m.gregory@reading.ac.uk>, Chris Jones <chris.d.jones@metoffice.gov.uk>, "Jones, Gareth S" <gareth.s.jones@metoffice.gov.uk>, David Lobell <dlobell@stanford.edu>, peter gleckler <gleckler1@llnl.gov>, Cath Senior <cath.senior@metoffice.gov.uk>, Keith Williams <keith.williams@metoffice.gov.uk>, "stephen e. schwartz" <ses@bnl.gov>, David Easterling <David.Easterling@noaa.gov>, Inez Fung <ifung@berkeley.edu>, Duane Waliser <duanewaliser@mac.com>, William Collins <wcollins@ucar.edu>, Ken Caldeira <kcaldeira@stanford.edu>, Dave Randall <randall@atmos.colostate.edu>, Joyce Penner <Penner@umich.edu>, Anna Pirani <anna.pirani@noc.soton.ac.uk>, Bjorn Stevens <bstevens@atmos.ucla.edu>, Ronald Stouffer <Ronald.Stouffer@noaa.gov>

Dear Jason and Jerry (and Karl and Ron)--One of my suggestions on an earlier round was such a simulation--to determine how models might do and compare with a declining concentration (optimistic as such a scenario might be). The one you are doing would seem to have an overshoot on the forcing, but probably not (or not much) on the global average temperature due to lag effects in the system. It seems to me it would be worthwhile figuring out such a run that also got the temperature decreasing, so maybe returned to below the equivalent concentration we have now (so below something like 375 ppm when counting aerosol effects). In that such scenarios would likely lead to sharp cuts in CO2 emissions, they would also presumably lead to sharp reductions in the SO2/SO4 offset, we are really already at about 450 ppm CO2 equivalent for GHGs alone--and so to really get cooling started, the run would likely have to go back to 350 ppm or below--so basically to the level Jim Hansen has been arguing is required to get back near 1990s climatic conditions.

mail.2008

I would also note that the CO2 equivalence calculations are being done using the 100-year GWPs. While there is not much difference for N2O and most halocarbons, the 20-year GWP for methane is about 3 times the 100-year value and so over the near-term methane changes (from stringent methane control, or additional release from thawing tundra) could have a very large effect on the short-term forcing and so on temperature change over the next several decades, so when the peak occurs and how one comes back thereafter. While CO2 control may well take time, methane control is very cost effective and should be being pushed very hard as a strategy (along with soot and air pollutants contributing to tropospheric ozone--a point made several years ago by Jim Hansen). In any case, it seems to me it is not implausible to imagine that we could get to conditions where radiative forcing is coming down, and that type of run needs to be explored--so having some sort of standard run that groups could try if they have resources would make good sense.

Mike MacCracken

On 7/29/08 4:48 PM, "Jason Lowe" <jason.lowe@metoffice.gov.uk> wrote:

> Hi Peter,
> I seem to be the only person not in Snowmass!
>
> In addition to the Japanese proof of concept the EU Ensembles project
> is also running a model intercomparison with a low end scenario that
> peaks at a little over 500ppm CO2eq before declining to an eventual
> 450ppm. Emissions will be diagnosed and, hopefully, many of
> the groups with C-C cycle feedback will also diagnose the feedback!
> It will be interesting to see the spread.
>
> Regards,
>
> Jason
>
> On Tue, 2008-07-29 at 11:48 -0600, Jerry Meehl wrote:
>> Hi Peter,
>>
>> How long will you be in Snowmass? I get there tomorrow late afternoon
>> and will be there for the sessions Thursday and Friday. Ron and I were
>> planning on re-visiting the experimental design more then, and if you
>> could join in that would be great.
>>
>> Regarding your point in favor of using the RCPs for carbon cycle
>> feedback, I think Ron and I arrived at this conclusion independently
>> while we both attended a US-Japan workshop in Colorado a few weeks ago.
>> The Japanese have performed a proof-of-concept experiment using two
>> idealized mitigation scenarios and basically computed numbers for the
>> Aspen experiments you originally proposed in 2006. There were two key
>> additional points that we noted--one was that they started from a
>> pre-industrial control run so they had 20th and 21st century in the
>> "climate-carbon feedback" contrasted to "no-climate carbon feedback"
>> allowable emissions plots. Second, they had some kind of 20th century
>> "observations" of carbon emissions they plotted on their allowable
>> emissions graphs to show that their model with carbon-climate feedback
>> actually tracked those observations for 20th century. Since there are
>> so few observations to compare carbon cycle feedback to, this seemed
>> like a fairly compelling reason to use RCPs, which is what you also note
>> below.
>>
>> I think Karl and Ron had lumped the carbon cycle feedback experiments in
>> the 1% runs both because this had come up as a possibility in the

mail.2008

>> post-Aspen WGCM meeting in Victoria in 2006, and because it could
>> possibly present a more pleasing context to evaluate all feedbacks,
>> carbon cycle and all others. However, on further review, in addition to
>> the points you raised, deriving allowable emissions from RCPs allows a
>> check to what the IAMs used for emissions in the first place (and used
>> to derive concentrations used in the ESMS). Also, it seems to me that
>> carbon cycle feedback falls into a new category of feedback that we in
>> the AOGCM world are not used to evaluating. We must depend on the
>> advice from you and others in that community. Though it's tempting to
>> think that everything can be boiled out of 1% runs, I think those are
>> most useful for feedbacks basically "managed" by the atmosphere (like
>> clouds, water vapor, etc.). The original Aspen concept for carbon cycle
>> feedback always depended on using actual mitigation scenarios, and I
>> think we're coming around again to agreeing on that.

>>
>> Another point is that the cloud feedback community will make a proposal
>> to WGCM to enlarge the idealized 1% feedback experiment list, so that
>> makes separating out the carbon cycle feedback experiments in a separate
>> category using RCPs more compelling.

>> Hopefully we can discuss this more Thursday.

>> Jerry

>> Cox, Peter wrote:
>>> Dear Karl and Ron

>>> Thanks for this very thorough document.

>>> Generally speaking I think we should be focusing much more on realistic
>>> policy relevant scenarios rather than 1% per year type experiments. There
>>> are two reasons for this:

>>> 1) Most now consider a ("business as usual") 1% per year scenario not to
>>> represent a viable future. So detailed information on these scenarios is
>>> less and less relevant to people outside of the GCM modeling community.
>>> 2) More realistic scenarios allow us to utilize observations to validate
>>> models/reduce uncertainties in a way that idealized scenarios do not.

>>> So I am in favour of diagnosing feedbacks in the more policy-relevant RCP
>>> scenarios wherever possible. I say this even though Ron, who is sitting
>>> beside me here now in Snowmass, has told me that this makes identifying
>>> model differences more difficult. Ron also tells me that this is a fight not
>>> worth fighting, but I can't resist commenting anyway...:-)

>>> More usefully I would like to respond to your PS. regarding the diagnosis of
>>> carbon cycle feedbacks. I strongly believe these should be diagnosed
>>> relative to the RCP scenarios. Carbon cycle feedbacks cannot easily be
>>> reduced to an equilibrium response plus a timescale. Carbon uptake
>>> essentially relies on disequilibrium and is therefore dependent on scenario,
>>> so I don't think it is very helpful to define c cycle feedback relative to
>>> idealised 1% per year runs. There are also the potential for significant
>>> "cold-start" problems with the carbon cycle (as land and ocean uptake are
>>> both highly dependent on history). So I vote for diagnosing carbon cycle
>>> feedbacks (at least) relative to the RCP scenarios.

>>> All the best

>>> Peter

>>> PLEASE NOTE NEW MOBILE NUMBER

>>> Prof Peter Cox,
>>> Met Office Chair in Climate System Dynamics,
>>> Room 336, Harrison Building,

mail.2008

>>> School of Engineering, Computing and Mathematics,
>>> University of Exeter,
>>> Exeter,
>>> EX4 4QF,
>>>
>>> Email: P.M.Cox@exeter.ac.uk,
>>> Tel (univ): 01392 269220,
>>> Tel (mob) : 07827 412572
>>>

>>>
>>>

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

>>> From: Karl Taylor [mailto:taylor13@llnl.gov]

>>> Sent: Tue 22-Jul-08 09:25 AM

>>> To: bryant.mcavaney@lmd.jussieu.fr; Curtis Covey; Jerry Meehl; Mitchell,
>>> John FB (Chief Scientist); mlatif@ifm-geomar.de; Tom.Delworth@noaa.gov;
>>> Andreas Hense; Asgeir Sorteberg; Erich Roeckner; Evgeny Volodin; Gary L.
>>> Russell; Gavin Schmidt; GFDL.Climate.Model.Info@noaa.gov; Greg Flato; Helge
>>> Drange; Jason Lowe; Jean-Francois Royer; Jean-Louis Dufresne; Jozef Syktus;
>>> Julia Slingo; Kimoto Masahide; Peter Gent; Qingquan Li; Seita Emori;
>>> Seung-Ki Min; Shan Sun; Shoji Kusunoki; Shuting Yang; Silvio Gualdi;
>>> Stephanie Legutke; Tongwen Wu; Tony Hirst; Toru Nozawa; Wilhelm May; Won-Tae
>>> Kwon; Ying Xu; Yong Luo; Yongqiang Yu; Kamal Puri; Tim Stockdale; Gabi
>>> Hegerl; James Murphy; Marco Giorgetta; George Boer; Myles Allen; claudia
>>> tebaldi; Ben Santer; Tim Barnett; Nathan Gillett; Phil Jones; David Karoly;
>>> Dáithí Stone; Stott, Peter; Francis Zwiers; Toru Nozawa; Ken Sperber; Dave
>>> Bader; Mike MacCracken; boyle5@llnl.gov; Stephen Klein; A. Pier Siebesma;
>>> William Rossow; Chris Bretherton;

>> George Tselioudis; Mark Webb; Sandrine Bony; James Hack; Martin Miller; Ken

>> Kunkel; Christian Jakob; Kathy Hibbard; Eyring, Veronika;

>> pasb@lsce.saclay.cea.fr; giorgi@ictp.trieste.it; c.lequere@uea.ac.uk;

>> naki@eeg.tuwien.ac.at; stephen.griffies@noaa.gov; Cox, Peter; Pierre

>> Friedlingstein; Olivier Boucher; Bala Govindasamy; Jonathan Gregory; Chris

>> Jones; Jones, Gareth S; David Lobell; peter gleckler; Cath Senior; Keith

>> Williams; stephen e. schwartz; David Easterling; Inez Fung; Duane Waliser;

>> William Collins; Ken Caldeira; Dave Randall; Joyce Penner; Anna Pirani; Bjorn

>> Stevens

>>> Cc: Ronald Stouffer

>>> Subject: Proposed experiment design for CMIP5

>>>

>>> Dear all,

>>>

>>> As most of you know, plans are well underway for a coordinated set of
>>> climate model experiments, which will constitute the Fifth phase of
>>> CMIP. Attached is a description of the proposed experiments. As
>>> members of the CMIP panel, which was established by the WCRP's Working
>>> Group on Coupled Modelling (WGCM) to help coordinate this activity, we
>>> are seeking your comments. Considerable thought and input from a wide
>>> community of scientists have already contributed to the CMIP5 design,
>>> and therefore major changes are not envisioned. Competing interests and
>>> various tradeoffs have been carefully considered before coming up with
>>> the proposed suite of experiments. Please keep in mind that modeling
>>> groups have limited resources and the experiment must represent a
>>> compromise among various priorities. We will not be able to please everyone.

>>>

>>> The CMIP panel must present a final design plan for CMIP5 to the WGCM at
>>> its annual meeting in September, just two months from now. Given this
>>> tight deadline (which cannot slip if the CMIP5 results are to be
>>> available in time for the IPCC's Fifth Assessment Report). For this
>>> reason, we ask that you send us (taylor13@llnl.gov and
>>> Ronald.Stouffer@noaa.gov) any comments and suggestions you have by
>>> September 1, 2008.

>>>

mail.2008

>>> Feel free to pass this document on to anyone you think will have an
>>> interest in it. We invite comments from scientists associated with all
>>> aspects of the climate change issue, spanning the three IPCC working groups.

>>> With best regards,
>>> Karl Taylor (PCMDI) and Ron Stouffer (Chair, CMIP panel).

>>> P.S. Please note that there are remaining details yet to be worked out.
>>> In particular it has been suggested that experiments 4.2 a&b described
>>> in the document should be performed in conjunction with the so-called
>>> RCP-driven experiments given in Table 2 rather than with the idealized
>>> (1% CO2 increase per year) experiments of Table 4. Experiments 4.2
>>> allow us to separate out the climate-carbon cycle feedback. The original
>>> proposal was in fact to do this separation for the RCP runs, but several
>>> scientists offered compelling arguments for switching this diagnostic
>>> analysis to the 1% runs. Some of the reasons for making this change
>>> from the original proposal can be found in section 9. Still, there are
>>> some scientists who continue to express a preference for the original
>>> design. Please let us know what you think about this.

913. 1219078495.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>
Subject: RE: EA 21389 - Probabilistic information to inform EA decision making on
climate change impacts - PCC(08)01
Date: Mon Aug 18 12:54:55 2008

At 13:35 20/05/2008, you wrote:

Phil,
Thanks for this.

In response:

1. I can't remember the thinking behind this - can you?
2. I don't think we'll be doing anything with UKCIP08 material, or briefing

people;

initially at least it will be about user needs without people thinking about
how they

might use UKCIP08, if that makes sense!

3. This is fine, although we may want some consistency between us e.g.
Newcastle rates

have been revised and are substantially larger than yours.

4. We need a pen portrait for Tim.

5. Thanks - we'll use this in with the other text.

Best wishes,

Geoff

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

From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]

mail.2008

Sent: 19 May 2008 15:36

To: Darch, Geoff J; Jim Hall; C G Kilsby; Mark New; ana.lopez@ouce.ox.ac.uk;
Anthony Footitt; Suraje Dessai; Clare Goodess; t.osborn@uea.ac.uk
Cc: MCSweeney, Robert; Arkell, Brian; Sene, Kevin
Subject: Re: EA 21389 - Probabilistic information to inform EA decision making

on climate change impacts - PCC(08)01

Geoff,

Clare is off to Chelsea - back late tomorrow. We (Clare, Tim and me) have had a brief meeting. Here are some thoughts and questions we had.

1. Were we going to do two sets of costings?
2. Those involved in UKCIP08 (both doing the work and involved in the SG)

have signed confidentiality texts with DEFRA. Not sure how these affect access to the headline messages in the drafts we're going to be looking at over the

next few

months. Also not sure how these will affect the UKCIP workshops that are

coming

up before the launch.

3. We then thought about costs for the CRU work. We decided on 25K for all CRU work. At £500 per day this comes to 50 days. We then split this into the tasks: 5 - 5 days, 6 - 5 days, 7 - 30 days, 10/11 - 5 days, which leaves

5

more days for meetings. Assumed the 25K was without travel to the meetings.

you

4. On CVs and pen portraits. Clare will send one before she leaves. Are what

have for Tim and me OK?

5. Some thoughts on Tasks 6 and 7

Task 6 - assumed this was mostly Newcastle.

Tim's work on rainfall extremes could be

fed in, and we can do something on non-rainfall variables. Assume also you expect us

to

do waves, but not sure what we can do. It seems as though sea level has become waves?

Task 7 - assumed here Newcastle (Chris/Hayley) would be doing something on blocking (large-scale variability). Oxford would do the final bit on

conceptual

representation

of emissions and climate system and sensitivities, so based on GCMs.

This leaves CRU for the other three, which we base mainly on the 11 RCM runs, which we can access through LINK. We could also use ENSEMBLES runs for the

others,

but these would be RCMs. They seem more relevant for the sorts of scales

UKCOP08

is working at.

All just a few thoughts at this time.

Can you send the UKWIR bid that went off, so we have a copy?

Cheers

Phil

At 09:06 16/05/2008, Darch, Geoff J wrote:

>Dear all,

>

>Please find attached the final tender pack for the Environment Agency

>bid. The tasks have been re-jigged, with the main change being a

>broadening of flood risk management to flood and coastal erosion risk

>management (FCERM). This means a wider audience to include all

>operating authorities, and the best practice guidance required (new

>Task

>11) is now substantial element, to include evaluation of FCERM climate

>change adaptation, case studies and provision of evidence to help

>upgrade the FCDPAG3 Supplementary Note.

mail.2008

>
>We have just one week to finish this tender, as it must be posted on
>Friday 23rd. We are putting together the bid document, which we'll
>circulate on Monday 19th, but in the meantime, and by the end of
>Tuesday 20th, I need everyone to send information (as indicated in
>brackets) to support the following structure:
>
>+ Understanding of the tender
>+ Methodology and programme (methodology for tasks / sub-tasks - see
>below - and timing)
>+ Project team, including individual and corporate experience (who you
>are putting forward, pen portraits, corporate case studies)
>+ Financial and commercial (day rates and number of days; please also
>highlight potential issues with the T&Cs e.g. IPR)
>+ Health & Safety, Quality and Environmental Management Appendices
>+ (full CVs, limited to 6 pages)
>
>Please send to me and Rob McSweeney. The information I have already
>e.g. on day rates, core pen portraits etc will go straight into the
>version we're working on, so no need to re-send.
>
>In terms of tasks (new nos.), the following organisation is suggested
>based on what has been noted to date:
>
>Task 1 (Inception meeting and reporting) Atkins, supported by lead
>representatives of partners Task 2 (Project board meetings) Atkins,
>supported by lead representatives of partners Task 3 (Analysis of user
>needs) Atkins with Tyn@UEA and OUCE, plus Futerra depending on style
>Task 4 (Phase 2 programme) Atkins, supported by all Task 5 (Interpret
>messages from UKCIP08 projections) CRU, OUCE and Newcastle, with Atkins
>advice on sectors Task 6 (Development of business specific projections)
>Newcastle and CRU, with Atkins advice on policy and ops Task 7 (Putting
>UKCIP08 in context) CRU, Newcastle and OUCE Task 8 (User guidance)
>Atkins, Tyn@UEA, Futerra Task 9 (Pilot studies) Atkins, Newcastle,
>OUCE, Tyn@UEA Task 10 (Phase 3 programme) Atkins, supported by all Task
>11 (Best Practice Guidance for FCERM) Newcastle and Atkins, with CRU
>Task 12 (Awareness raising events) Atkins, key experts, Futerra
>(perhaps as an option as EA are quite specific here) Task 13 (Training
>events) Atkins and Futerra
>
>Note that Futerra is a communications consultancy, specialising in
>sustainability, who will input on workshops and on the guidance
>documents.
>
>I'll be in touch again early next week.
>
>Best wishes,
>
>Geoff
>
>Geoff Darch
>
>Senior Consultant
>Water and Environment
>ATKINS
>
>Broadoak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough,
>PE2 6YS, UK
>Tel: +44 (0) 1733 366969
>Fax: +44 (0) 1733 366999
>Mobile: +44 (0) 7834 507590
>E-mail: geoff.darch@atkinsglobal.com
>Web: [2]www.atkinsglobal.com/climate_change

mail.2008

>
>
>
>
>

>This email and any attached files are
>confidential and copyright protected. If you are not the addressee, any
>dissemination of this communication is strictly prohibited. Unless
>otherwise expressly agreed in writing, nothing stated in this
>communication shall be legally binding.

>
>The ultimate parent company of the Atkins Group is WS Atkins plc.
>Registered in England No.
>1885586. Registered Office Woodcote Grove, Ashley Road, Epsom, Surrey
>KT18 5BW. A list of wholly owned Atkins Group companies registered in
>the United Kingdom can be found at
>[3]http://www.atkinsglobal.com/terms_and_conditions/index.aspx

>
>Consider the environment. Please don't print this e-mail unless you
>really need to.

>
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

This message has been scanned for viruses by MailControl - (see
<http://bluepages.wsatkins.co.uk/?6875772>)

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:p.jones@uea.ac.uk>
2. http://www.atkinsglobal.com/climate_change
3. http://www.atkinsglobal.com/terms_and_conditions/index.aspx

914. 1219239172.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: Revised version the wengen paper
Date: Wed Aug 20 09:32:52 2008
Cc: Michael Mann <mann@meteo.psu.edu>

Gavin,
Almost all have gone in. Have sent an email to Janice re the regional
freshening.

On the boreholes I've used mostly Mike's revised text, with bits of
Page 236

mail.2008

yours making it read a little better.

Thinking about the final bit for the Appendix. Keith should be in later, so I'll check with him - and look at that vineyard book. I did rephrase the bit about the 'evidence' as Lamb refers to it. I wanted to use his phrasing - he used this word several times in these various papers. What he means is his mind and its inherent bias(es).

Your final sentence though about improvements in reviewing and traceability is a bit of a hostage to fortune. The skeptics will try to hang on to something, but I don't want to give them something clearly tangible.

Keith/Tim still getting FOI requests as well as MOHC and Reading. All our FOI officers have been in discussions and are now using the same exceptions not to respond - advice they got from the Information Commissioner. As an aside and just between us, it seems that Brian Hoskins has withdrawn himself from the WG1 Lead nominations. It seems he doesn't want to have to deal with this hassle.

The FOI line we're all using is this. IPCC is exempt from any countries FOI - the skeptics

have been told this. Even though we (MOHC, CRU/UEA) possibly hold relevant info the IPCC is not part our remit (mission statement, aims etc) therefore we don't have an obligation to pass it on.

Cheers

Phil

At 18:07 19/08/2008, you wrote:

Phil, here are some edits - mostly language, a couple of bits of logic, an attempt to soothe Mike on the borehole bit, and a paragraph for consideration in the Appendix. Two questions require a little thinking - the reference to 'regional freshening' on the coral section needs to be more specific - I doubt it is a global phenomena, second there is an 'in prep' reference to some new work by van Ommen - I don't think this is appropriate and should either be removed and put as a personal communication.

Having looked over the tropical trees section, I think that's fine.

The fig A1 does need labelling though.

Gavin

On Tue, 2008-08-19 at 09:11, Phil Jones wrote:

> Mike,

> Peck didn't do the speleothem bit either.

> Cheers

> Phil

>

> Mike,

> Have your text in - just need to read the borehole section again.

> Noted your comment re the final Appendix figure. Will look at more when Tim back.

> Peck's bit is 2.5 and the terrestrial part of 2.6 - except for the borehole text.

>

> Next time I co-ordinate anything I'll get the GB cycling coach

> involved. We've just one our 7th gold medal on two wheels. Only

> one short of Phelps.

>

> Cheers

> Phil

>

>

> At 13:52 19/08/2008, Michael Mann wrote:

> > thanks Phil--which part is Peck's? I'd like to read it over

> > carefully,

> >

> > mike

mail.2008

> >
> > Phil Jones wrote:
> > > Mike, Gavin,
> > > On the final Appendix plot, the first and last 12 years of
> > > the annual CET record
> > > were omitted from the smoothed plot. Tim's away, but when he did
> > > this with
> > > them in the light blue line goes off the plot at the end. The
> > > purpose of the piece
> > > was to show that the red/black lines were essentially the same.
> > > It wasn't
> > > to show the current light blue smoothed line was above the
> > > red/blue lines,
> > > as they are crap anyway.
> > > The y-axis scale of the plot is constrained by what was in
> > > the IPCC
> > > diagram from the first report. What we'll try is adding it fully
> > > back in or
> > > dashing the first/last 12 years. The 50-year smoother includes
> > > quite
> > > a bit of padding - we're using your technique Mike. The issue is
> > > that CET
> > > has been so warm the last 20 years or so.
> > > Normal people in the UK think the weather is cold and the
> > > summer is
> > > lousy, but the CET is on course for another very warm year.
> > > warmth
> > > in winter/spring doesn't seem to count in most people's minds
> > > when it comes to warming.
> > >
> > > will mod the borehole section now. Because this had been
> > > written
> > > by Juerg initially, I added in a paraphrased section from AR4. I
> > > will
> > > mod this accordingly. Hope you noticed Peck's stuff.
> > >
> > > Cheers
> > > Phil

> > > At 17:28 18/08/2008, Michael Mann wrote:
> > > > Hi Phil,
> > > >
> > > > traveling, and only had brief opportunity to look this over.
> > > > only 2 substantial comments:
> > > >
> > > > 1. I don't know who wrote the first paragraph of section 3.3
> > > > (bottom of page 52/page 53), but the lack of acknowledgement
> > > > here in this key summary that we actually introduced the idea of
> > > > 'pseudoproxies' into the climate literature is very troubling.
> > > > the end of the first sentence:
> > > > e.g., Zorita and González-Rouco, 2002, Küttel et al., 2007),
> > > > should be changed to:
> > > > e.g., Mann and Rutherford, 2002; Zorita and González-Rouco,
> > > > 2002, Rutherford et al, 2003; Küttel et al., 2007),
> > > >
> > > > 2. I'm also a bit confused and very concerned about the
> > > > description of smoothing in Appendix A Figure 1. It sounds like
> > > > the last 12 years were removed from the end of the series? If
> > > > so, that's not a fair comparison because it's really the past
> > > > decade that takes us into 'unprecedented' territory. I would
> > > > suggest one of two alternative approaches:
> > > > a. show the full smoothed curve without removing end data (I
> > > > don't see any objective justification for doing that) or

mail.2008

> > > > b. show the raw annual data through 2006 so readers can see how
> > > > the most recent values compare w/ the MWP peak.
> > > >
> > > > By the way, I have a revised version of Mann [2004] now in press
> > > > in GRL, I've attached. Please don't distribute or cite prior to
> > > > publication (which should be one or two weeks from now).
> > > >
> > > > thanks,
> > > >
> > > > mike
> > > >
> > > >
> > > > Phil Jones wrote:
> > > > Dear All,
> > > > Here's the revised version of the paper, together with
> > > > the responses to the reviewers.
> > > > We have told John Matthews, that we will get this back to him
> > > > by the beginning
> > > > of next week. To us in the UK this means Aug 26/27 as next
> > > > Monday is a national
> > > > holiday. So, to those not away at the moment, can you look
> > > > through your
> > > > parts and get any comments back to us by the end of this week
> > > > or over the
> > > > weekend?
> > > > Can you also look at the references - those in yellow and
> > > > let me know of
> > > > any that have come out, or are able to correct those that I
> > > > think just look
> > > > wrong?
> > > > I hope you'll think of this as an improvement.
> > > >
> > > > Cheers
> > > > Phil
> > > >
> > > >
> > > > Prof. Phil Jones
> > > > Climatic Research Unit Telephone +44 (0) 1603 592090
> > > > School of Environmental Sciences Fax +44 (0) 1603 507784
> > > > University of East Anglia
> > > > Norwich Email p.jones@uea.ac.uk
> > > > NR4 7TJ
> > > > UK
> > > >

> > > >
> > > >
> > > >
> > > >
> > > >
> > > >
> > > >
> > > > --
> > > > Michael E. Mann
> > > > Associate Professor
> > > > Director, Earth System Science Center (ESSC)
> > > >
> > > > Department of
> > > > Meteorology
> > > > Phone: (814) 863-4075
> > > > 503 Walker
> > > > Building
> > > > FAX: (814) 865-3663

mail.2008

> > > > The Pennsylvania State University
> > > > email: mann@psu.edu
> > > > University Park, PA 16802-5013
> > > >
> > > > website:
> > > >
> > > > [1]<http://www.met.psu.edu/dept/faculty/mann.htm>
> > > > "Dire Predictions" book site:
> > > >
> > > >
> > > >

[2]<http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.htm>

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

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

> > --
> > Michael E. Mann
> > Associate Professor
> > Director, Earth System Science Center (ESSC)
> >
> > Department of
> > Meteorology
> > Phone: (814) 863-4075
> > 503 Walker
> > Building
> > FAX: (814) 865-3663
> > The Pennsylvania State University
> > email: mann@psu.edu
> > University Park, PA 16802-5013
> >
> > website:
> >
> > [3]<http://www.met.psu.edu/dept/faculty/mann.htm>
> > "Dire Predictions" book site:
> >
> >
> >

[4]<http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.htm>

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

Prof. Phil Jones

mail.2008
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.met.psu.edu/dept/faculty/mann.htm>
2. <http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>
3. <http://www.met.psu.edu/dept/faculty/mann.htm>
4. <http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>

915. 1219844013.txt

#####

From: Gabi Hegerl <gabi.hegerl@ed.ac.uk>
To: tbarnett-ul@ucsd.edu
Subject: Re: comments on AR5 experimental design - reply by Aug 28 (thursday)
Date: Wed, 27 Aug 2008 09:33:33 +0100
Cc: dpierce@ucsd.edu, JKenyon <kenyon@duke.edu>, Myles Allen <m.allen1@physics.ox.ac.uk>, Nathan <n.gillett@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, Knutti Reto <reto.knutti@env.ethz.ch>, Toru Nozawa <nozawa@nies.go.jp>, Tom Knutson <tom.knutson@noaa.gov>, Doug Nychka <nychka@ucar.edu>, Claudia Tebaldi <tebaldi@ucar.edu>, Ben Santer <santer1@llnl.gov>, Richard Smith <rls@email.unc.edu>, Daithi Stone <stoned@atm.ox.ac.uk>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, Michael Wehner <mfwehner@lbl.gov>, Francis Zwiers <francis.zwiers@ec.gc.ca>, Hans von Storch <hvonstorch@web.de>

<x-flowed>

Thanks Tim! we'll have another round later, confirmed by Tim, when we discuss storage and documentation - probably should try before WGCN meeting so that David can present results.

the 'near term prediction' is a mip all by itself, so there will be some guidance coming up hopefully!
In terms of ensemble size: for the stuff I was involved in, even one run from a model was good since it increased the overall ensemble size for multi model means and estimates of variance - did you analyze models individually? I would be keen to hear from the group:

is say a single 20th c run, single natural only run, single ghg run
a) useless
b) much better than nothing?

I vouch for b) for things I was involved in but it would be good to know for which applications its a!
Gabi

Tim Barnett wrote:

- > hi gabi..in real haste.....people will use the AR5 data set for impact studies no doubt about it. so what will they find when they jump
- > in....same as we did trying to do the western D&A work with AR4....a very disparate set of numbers.
- > 1.some models don't give the data one would like.
- > 2.some models have only 1 realization...which makes them useless. we

mail.2008

> found that with multiple realizations one can do statistics with ensemble
> techniques which give a lot more statistical power. suggesting 10 member
> ensembles. with less the S/N can be small...e.g. we could not use the
> GFDL runs very well as they were so noisy and had few (5) realizations)
> 3. daily data is required. storage is cheap these days so at least daily
> data for order 100 years is desired. otherwise it is finaged a la the
> current downscaling methods (save one).
> 4. the 20th century runs need to go to 2015 as suggested by IDAG. we had
> to stop at 1999 and lost 8 years we would well like to have studies.
> 5. some of the variables we needed to compare with satellite obs were
> largely missing, e.g. clouds information.
> 6. to Mike's point...just what data is going to be saved?
> 7. i hope potential users of the data aside from the modeling groups get
> a say in what is archived. we are to the point now where policy makers
> want our best guesses as to what will happen in the next 20 years. the
> people who will make those 'guesses' are most likely not in the major
> model centers.

>
> I invite David Pierce to chip in here as he spend alot of time in the
> details of the data sets and associated problems.

> sorry to be so hasty but such is life at the moment. best, tim

>> Hi IDAG'ies,

>> As you probably know, a proposal for the AR5 experiments is being
>> circulated in the moment, with comments due by September 1. This will
>> then be presented at the working group for coupled modelling (WGCM)
>> meeting in Paris, which David Karoly will attend.
>> Peter Stott and I discussed the draft when I visited last week, and we
>> drafted a response and suggestions from IDAG (attached) Please let me
>> know if you are ok with this (if I dont hear back I assume you are),
>> if you suggest changes and if you want us to add another topic/concern.

>> I would need this by next thursday to add it to a comment 'from IDAG'
>> to be sent in time, and then hopefully David can present this also in
>> Paris at the WGCM meeting.

>> hope you all had a nice summer, and still remember our next meeting in
>> planning, and your IDAG tasks :))

>> Gabi

>> p.s. we were wondering also about forcing, and if the forcing issue
>> (how stored, synchronized?) should be added. However, given even some
>> 'rich' modelling groups worry about getting the mandatory experiments
>> through we should however not hope that groups will run more than 1
>> single forcing set for the 20th century, and arguments against
>> synchronizing are that its not feasible for many forcings (eg
>> aerosols) and that we loose quite a bit of information if only a
>> single, for example, set of solar forcings were used and with this
>> open the AR5 up for criticism. Ideally, of course, one center would
>> systematically explore all the forcings - but I am not sure somebody
>> is planning to do this - in that case, a common set of 20th century
>> forcings may be an advantage. Based on some EU project, forcings are
>> synchronized for some European modeling centers - we could draw
>> attention to that if you feel strongly about this...anyway, I hesitate
>> to start a discussion about this...

mail.2008

>>
>> --
>> Gabriele Hegerl
>> School of GeoSciences
>> University of Edinburgh
>> <http://www.geos.ed.ac.uk/people/person.html?indv=1613>
>>
>> --
>> The University of Edinburgh is a charitable body, registered in
>> Scotland, with registration number SC005336.
>>
>>
>>
>
>
>

--
Dr Gabriele Hegerl
School of GeoSciences
The University of Edinburgh
Grant Institute, The King's Buildings
West Mains Road
EDINBURGH EH9 3JW
Phone: +44 (0) 131 6519092, FAX: +44 (0) 131 668 3184
Email: Gabi.Hegerl@ed.ac.uk

The University of Edinburgh is a charitable body, registered in
Scotland, with registration number SC005336.

</x-flowed>

916. 1219861908.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: New Wengen Draft -- including changes to accommodate new Figure 3
Date: Wed Aug 27 14:31:48 2008
Cc: Eugene Wahl <Eugene.R.Wahl@noaa.gov>, t.osborn@uea.ac.uk

Caspar,
Thanks.

Phil

At 14:16 27/08/2008, Caspar Ammann wrote:

Phil,

I worked on the figures yesterday and sent them off to Gene for double check.
will be

one panel each (6), much improved legibility and significantly reduced
"footprint" in

the appearance of the text. You should have them before the end of your day.

Thanks for all your work on this paper! (Tim too!)

Cheers,

Caspar

On Aug 27, 2008, at 2:42 AM, Phil Jones wrote:

Caspar, Gene,

we're going to send the manuscript back tomorrow. If we get a

mail.2008

revised diagram we'll include - otherwise we won't.

Have had a few more comments, but nothing substantial. All yours Gene are in, as are those from Gavin, Mike, Juerg and the coral people. There is a completely revised tropical dendro section and Peck finally came through with a section on less-resolved proxies and varves.

All in all it reads very well and the recommendations should prove very useful for PAGES.

Cheers

Phil

At 04:52 26/08/2008, Caspar Ammann wrote:

Hey Gene,

I'll see how I can adjust the figures to fit.

Caspar

On Aug 25, 2008, at 8:30 PM, Eugene Wahl wrote:

Hi Phil and Tim, and Caspar:

Here are my full set of comments on the entirety of section 3, the figures relevant to

section 3, the authors' address, and abstract (none there). I made slight changes in

the portion of the text already sent last night, sorry that I could not avoid that!

Caspar, please note that I've operated here on the assumption that Figure 3 is simplified to one panel for each section, according to the suggestions we have talked

about, but does contain all 6 portions, A-F.

There are two versions: one with just the relevant portions of the text, and the full

amended text document. The changes noted should be identical in each version.

Peace, Gene

Dr. Eugene R. Wahl

Physical Scientist

NOAA/NESDIS/NCDC/Paleoclimate Branch

325 Broadway Street

Boulder, CO 80305

303-497-6297

[1]<http://www.ncdc.noaa.gov/paleo/paleo.html>

[2]P.Jones@uea.ac.uk wrote:

Gene,

Thanks. Today is a holiday here. We'll all be back in CRU tomorrow. So, we'll begin revising Section 3 then.

Have had quite a few comments so far, and all are in.

New Figure 3 most appreciated. We must send this off on Thursday or Friday.

Hope you're settling in to Boulder life. At least you should be able to contact Caspar more easily!

Cheers

Phil

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

Subject: New Wengen Draft

From: [3]Eugene.R.Wahl@noaa.gov

Date: Mon, August 25, 2008 2:45 am

To: [4]p.jones@uea.ac.uk

Hi Phil:

I've had to wait to the weekend to get to this, due to several other matters that had to be attended to here at NOAA this week and in relation to a report required by a funder that was due Friday.

I've looked over about half of section 3 (up to the start of section 3.4.2), and also the abstract and the authors' address section.

Attached are my comments on those sections. I will be getting to the

mail.2008

rest of section 3 tonight and tomorrow and will send anything else to you. Everything is done in WORD with "Track Changes" turned on.

HIGHLIGHTS

1) My address information has been updated to include my NOAA information, which is now appropriate. The original Alfred information is kept, as also appropriate. I've condensed it all to not change the overall page spacing of the address citations.

2) The addition to the results description of the Riedwyl et al. (2008) paper across pp 10-11 here (near the top of p 56 in the text you sent this week). It is NECESSARY to keep this addition, as the text as it was "overemphasized" the differential quality of the RegEM results in this study. Their graphs 4 and 6 clearly show the results I added, in which RegEM for winter adds quite problematic artifacts at the highest levels of noise added. The white-noise SNR at which this happens (0.25), while low, is not outside of what reality might bring. [NB: I have talked with Juerg about this situation, and he is clearly aware of my sense that RegEM is given too high marks in this context.]

3) I added very brief descriptions how the CFRs actually come up with a reconstruction to the descriptions of them in section 3.2. If you feel these three sentences cannot be included I understand, but I think they are useful for the readers to know HOW the covariance information we are talking about there is actually used.

TO COME: Caspar and I are working out a much simplified version of Figure 3 (one panel per each section A-F), which I think will be much better than what is there now. We communicated on that Friday and yesterday, and are now close to having a new graphic. I will adapt the references to Figure 3 in section 3.4.2 and in the figure caption in my next message accordingly, which I plan will come either tonight or tomorrow.

Peace, and again thanks!

Gene

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

From: From Phil Jones New Wengen Draft

Dear All,

Here's the revised version of the paper, together with the responses to the reviewers. We have told John Matthews, that we will get this back to him by the beginning of next week. To us in the UK this means Aug 26/27 as next Monday is a national holiday. So, to those not away at the moment, can you look through your parts and get any comments back to us by the end of this week or over the weekend?

Can you also look at the references - those in yellow and let me know of any that have come out, or are able to correct those that I think just look wrong?

I hope you'll think of this as an improvement.

Cheers

Phil

Prof. Phil Jones

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

Norwich
NR4 7TJ
UK

mail.2008
Email [5]p.jones@uea.ac.uk

>

<wengendraft_version_18Aug_wahl_review_SHORT_b.doc><wengendraft_version_18Aug_wahl_r
evie
w.doc>

Caspar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division - Paleoclimatology
1850 Table Mesa Drive
Boulder, CO 80307-3000
email: [6]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

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

Caspar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division - Paleoclimatology
1850 Table Mesa Drive
Boulder, CO 80307-3000
email: [8]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

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.ncdc.noaa.gov/paleo/paleo.html>
2. <mailto:P.Jones@uea.ac.uk>
3. <mailto:Eugene.R.Wahl@noaa.gov>
4. <mailto:p.jones@uea.ac.uk>
5. <mailto:p.jones@uea.ac.uk>
6. <mailto:ammann@ucar.edu>
7. <mailto:p.jones@uea.ac.uk>
8. <mailto:ammann@ucar.edu>

917. 1220039621.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>

mail.2008

Subject: Re: paper on smoothing

Date: Fri, 29 Aug 2008 15:53:41 -0400

Reply-to: mann@psu.edu

Cc: Kevin Trenberth <trenbert@ucar.edu>, Curtis Covey <covey1@llnl.gov>, mann@psu.edu, "Folland, Chris" <chris.folland@metoffice.gov.uk>, Ben Santer <santer1@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>

<x-flowed>

yeah, its statistically real, but an artifact almost certainly of natural variability. As Josh willis nicely pointed out in a recent interview, anyone citing this as a reason to doubt the reality of anthropogenic climate change is like a vegas roller thinking he can beat the system because he's on a momentary winning streak...

m

Thomas.R.Karl wrote:

> Curt,

>

> At this point the leveling off is more of a Blog myth than any change
> point scientific analysis

>

> Tom

> Kevin Trenberth said the following on 8/29/2008 3:47 PM:

>> No

>> Kevin

>>

>> Curtis Covey wrote:

>>> Very interesting. Does it mean that the apparent leveling-off of
>>> global mean surface temperature since the turn of the century is due
>>> to "artificial suppression of trends near the time series boundaries" ?
>>>

>>> - Curt

>>>

>>> Michael Mann wrote:

>>>> dear all,

>>>>

>>>> attached is a paper of mine (GRL) on time series smoothing that
>>>> might be of interest.

>>>>

>>>> best regards,

>>>>

>>>> mike

>>>>

>>

>

--

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

Department of Meteorology
503 Walker Building
The Pennsylvania State University
University Park, PA 16802-5013

Phone: (814) 863-4075
FAX: (814) 865-3663
email: mann@psu.edu

website: <http://www.met.psu.edu/dept/faculty/mann.htm>
"Dire Predictions" book site:

http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html

</x-flowed>

918. 1221683947.txt

#####

From: P.Jones@uea.ac.uk
To: trenbert@ucar.edu
Subject: Re: Climate
Date: wed, 17 Sep 2008 16:39:07 +0100 (BST)
Cc: wibjörn Karlén <wibjorn.karlen@kultgeog.uu.se>, "Phil Jones" <p.jones@uea.ac.uk>

wibjorn,

I'm in Athens at the moment. Unless you're referring specifically to the Arctic the temperature curves in IPCC Ch 3 all include the oceans.

Fennoscandia is just a small part of the NH. When I'm back next week, I'll be able to calculate the boxes that encompass Fennoscandia, so you can compare with this region. As you're aware Anders did lots of the update work in 2001-2002 and he included all the NORDKLIM data. I can send you a list of the Fennoscandian data if you want - either the sites used or their data as well.

I guess you're attachments are in your direct email, which I come to later.

One final thing - we are getting SST data in from some of the new sea-ice free parts of the Arctic. We are not using these as we've yet to figure out how to as we don't have normals for these 'mostly covered by sea ice in the 1961-90' areas.

Cheers
Phil

> Hi wibjorn
> It appears that your concern is mainly with the surface temperature record,
> and my co lead author in IPCC, Phil Jones, is best able to address those
> questions. However the IPCC only uses published data plus their
> extensions and in our Chapter the sources of the data are well documented,
> along with their characteristics. I offer a few more comments below (my
> comments are limited as I am on vacation and away from my office).

>
>
>>
>> Uppsala 17 September 2008,
>>

>> Dear Kevin,
>>

>> In short, the problem is that I cannot find data supporting the
>> temperature
>> curves in IPCC and also published in e.g. Forster, P. et al. 2007:
> Assessing uncertainty in climate simulation. Nature 4: 63-64.

>>
>>
>>
>> In attempts to reconstruct the temperature I find an increase from the
> early
>> 1900s to ca 1935, a trend down until the mid 1970s and so another
> increase
>> to about the same temperature level as in the late 1930s.
>>
>>
>>
>> A distinct warming to a temperature about 0.5 deg C above the level 1940
>> is
>> reported in the IPCC diagrams. I have been searching for this recent
> increase, which is very important for the discussion about a possible
> human
>> influence on climate, but I have basically failed to find an increase
>> above
>> the late 1930s.
>>
>
> This region, as I am sure you know, suffers from missing data and large
> gaps spatially. How one covered both can greatly influence the outcome.
> In IPCC we produce an Arctic curve and describe its problems and
> character. In IPCC the result is very conservative owing to lack of
> inclusion of the Arctic where dramatic decreases in sea ice in recent
> years have taken place: 2005 was lowest at the time we did our assessment
> but 2007 is now the record closely followed by 2008. Anomalies of over 5C
> are evident in some areas in SSTs but the SSTs are not established if
> there was ice there previously. These and other indicators show that
> there is no doubt about recent warming; see also chapter 4 of IPCC.
>
>>
>>
>> In my letter to "Klass V" I included diagram showing the mean annual
> temperature of the Nordic countries (1890-ca 2001) presented on the net
> by
>> the database NORDKLIM, a joint project between the meteorological
> institutes
>> in the Nordic countries. Except for Denmark, the data sets show an
>> increase
>> after the 1970s to the same level as in the late 1930s or lower. None
> demonstrates the distinct increase IPCC indicates. The trends of these 6
> areas are very similar except for a few interesting details.
>>
>>
>
> Results will also depend on the exact region.
>
>>
>> I have in my studies of temperatures also checked a number of areas
> using
>> data from NASA. One, in my mind interesting study, includes all the 13
> stations with long and decent continuously records north of 65 deg N.
> The
>> pattern is the same as for the Nordic countries. This diagram only shows
> 11-yr means of individual stations. A few stations such as Verhojansk
> and
>> Svalbard indicate a recent mean 11-year temperature increase up to 0.5
> deg
>> C
>> above the late 1930s. Verhojansk, shows this increase but the
> temperature

mail.2008

>> has after the peak temperature decreased with about 0.3 deg C during the
> last few years. The majority of the stations show that the recent
> temperatures are similar to the one in the late 1930s.

>>

>>

>>

>> In preparation of some talks I have been invited to give, I have
> expanded

>> the Nordic area both west and east. The area of similar change in
> climate

>> is

>> vast. Only a few stations near Bering Strait deviates (e.g. St Paul,
> Kodiak,

>> Nome, located south of 65 deg. N).

>>

>>

>>

>> My studies include Africa, a study which took me most of a summer
> because

>> there are a large number of stations in the NASA records. I found 11
> stations including data from 1898-1975 and 16 stations including

> 1950-2003.

>> The data sets could in a convincing way be spliced. However, I noticed
>> that

>> some persons were not familiar with "splicing" technique so I have
>> accepted

>> to reduce the study to the 7 stations including data from the whole
> period

>> between 1898-2003. The results are similar as to the spiced data set and
> also, surprisingly similar to the variability of the Nordic data.

> Regression

>> indicates a minor (if any) decrease in temperature (I have used all
> stations

>> independent of location, city location or not).

>>

> Africa is notorious for missing and inaccurate data and needs careful
> assessment.

>>

>>

>> Another example is Australia. NASA only presents 3 stations covering the
> period 1897-1992. what kind of data is the IPCC Australia diagram based

> on?

>> If any trend it is a slight cooling. However, if a shorter period
> (1949-2005) is used, the temperature has increased substantially.

>>

>>

>

> The Australians have many stations and have published more detailed maps
> of changes and trends.

>

>>

>>

>> There are more examples, but I think this is much enough for my present
> point:

>>

>>

>>

>> How has the laboratories feeding IPCC with temperature records selected
> stations?

>>

> See our chapter and the appendices.

>>

>>

mail.2008

>> I have noticed that major cities often demonstrate a major urban effect
> (Buenos Aires, Osaka, New York Central Park, etc). Have data from major
> cities been used by the laboratories sending data to IPCC? Lennart
> Bengtsson and other claims that the urban effect is accounted for but
> from
>> what I read, it seems like the technique used has been a simplistic
>>
>
> Major inner cities are excluded: their climate change is real but very
> local.
>
>>
>>
>> Next step has been to compare my results with temperature records in the
> literature. One interesting figures is published by you in:
>>
>>
>>
>> Trenberth, K., 2005: Uncertainty in Hurricanes and Global warming.
>> Science
>> 308: 1753-1754.
>>
>>
>> As you obviously know, the recent increase in temperature above the
> 1940s
>> is
>> minor between 10 deg N and 20 deg N and only slightly larger above the
> temperature maximum in the early 1950s. Booth the increases in
> temperature
>> in the 1930s and in the 1980s to 1990s is of similar amplitude and
> similar
>> steepness, if any difference possibly slightly less steep in the
> northern
>> area than in the southern (the eddies slow down the warm water
>> transport?).
>> Your diagram describes a limited area of the North Atlantic because you
>> are
>> primarily interested in hurricanes. The complexity of sea surface
> temperature increases and decreases is seen in e.g. Cabanes, C, et al.
> 2001
>> (Science 294: 840-842).
>>
>
> As we discuss, there is a lot of natural variability in the North Atlantic
> but there is also a common component that relates to global changes. See
> my GRL article with Shea for more details.
> Trenberth, K. E., and D. J. Shea, 2006: Atlantic hurricanes and natural
> variability in 2005. Geophys. Res. Lett., 33, L12704,
> doi:10.1029/2006GL026894.
>
>>
>>
>> One example of sea surface temperature is published by:
>>
>>
>>
>> Goldenberg, S.B., Landsea, C.W., Mestas-Nuñez, A.M. and Gray, W.M.,
> 2001:
>> The recent increases in Atlantic hurricane activity: causes and
>> implications. Science 293: 474-479.
>>
>>

mail.2008

>>
>> Again, there is a marked increase in temperature in the 1930s and 1950s
> (about 1 deg C), a decrease to approximately the level in the 1910s and
> thereafter a new increase to a temperature slightly below the level in
> the1940s.

>>
>>
>> One example of published data not supporting a major temperature
> increase
>> during recent time is:

>>
>>
>> Polyakov, I.V., Bekryaev, R.V., Alekseev, G.H., Bhatt,U.S., Colony,
> R.L.,
>> Johnson, M.A., Maskstas, A.P. and Walsh, D., 2003: Variability and
> Trends
>> of Air Temperature and Pressure in the Maritime Arctic, 1875-2000.
> Journal
>> of Climate: vol. 16 (12): 2067-2077.

>>
>>
>>
>>
>> He included many more stations than I did in my calculation of
>> temperatures
>> N 65 N, but the result is similar. It is hard to find evidence of a
>> drastic
>> warming of the Arctic.

>>
>>
>>
>>
>> It is also difficult to find evidence of a drastic warming outside urban
> areas in a large part of the world outside Europe. However the increase
> in
>> temperature in Central Europe may be because the whole are is urbanised
> (see
>> e.g. Bidwell, T., 2004: Scotobiology - the biology of darkness. Global
> change News Letter No. 58 June, 2004).

>>
>>
>>
>>
>>
>> So, I find it necessary to object to the talk about a scaring
> temperature
>> increase because of increased human release of CO2. In fact, the warming
> seems to be limited to densely populated areas. The often mentioned
> correlation between temperature and CO2 is not convincing. If there is a
> factor explaining a major part of changes in the temperature, it is
> solar
>> irradiation. There are numerous studies demonstrating this correlation
> but
>> papers are not accepted by IPCC. Most likely, any reduction of CO2
> release
>> will have no effect whatsoever on the temperature (independent of how
> expensive).

>>
>
> You can object all you like but you are not looking at the evidence and
> you need to have a basis, which you have not established. You seem to
> doubt that CO2 has increased and that it is a greenhouse gas and you are
> very wrong. But of course there is a lot of variability and looking at
> one spot narrowly is not the way to see the big picture.

mail.2008

>
>
>>
>>
>> In my mind, we have to accept that it is great if we can reduce the
>> release
>> of CO2 because we are using up a resource the earth will be short of in
>> the
>> future, but we are in error if we claims a global warming caused by CO2.
>>
> I disagree.
>>
>>
>> I also think we had to protest when erroneous data like the claim that
> winter temperature in Abisko increased by 5.5 deg C during the last 100
> years. The real increase is 0.4 deg C. The 5.5 deg C figure has been
> repeated a number of times in TV-programs. This kind of exaggerations is
> not
>> supporting attempts to save fossil fuel.
>>
>>
>>
>> I have numerous diagrams illustrating the discussion above. I don't
>> include
>> these in an e-mail because my computer can only handle a few at a time.
> If
>> you would like to see some, I can send them by air mail.
>>
>>
>>
>> I am often asked about why I don't publish about my views. I have. Just
>> one
>> example of among 100 other I could select is: Karlén, w., 2001: Global
> temperature forces by solar irradiation and greenhouse gases? Ambio
> 30(6):
>> 349-350.
>>
>>
>>
>> Yours sincerely
>>
>>
>>
>> wibjörn
>>
>>
>>
>> Geografiska Annaler
>> Professor em wibjörn Karlén
>>
>> Department of Social and Economic Geography
>>
>>
>> Geografiska Annaler Ser. A
>>
>> Box 513
>> SE-751 20 Uppsala
>>
>> SWEDEN
>>
>>

mail.2008

>>
>> Wibjorn.Karlen@kultgeog.uu.se
>>
>
> I trust that Phil Jones may also respond
> Regards
> Kevin Trenberth
>
>
> _____
> Kevin Trenberth
> Climate Analysis Section, NCAR
> PO Box 3000
> Boulder CO 80307
> ph 303 497 1318
> <http://www.cgd.ucar.edu/cas/trenbert.html>
>
>
>
>

919. 1221742524.txt

#####

From: Clare Goodess <C.Goodess@uea.ac.uk>
To: R.L.Wilby@lboro.ac.uk,c.harpham@uea.ac.uk,M.agnew@uea.ac.uk, s.busby@uea.ac.uk
Subject: Fwd: RE: AXA Research Fund: launch of a new call for projects
Date: Thu, 18 Sep 2008 08:55:24 +0100
Cc: P.Jones@uea.ac.uk,k.briffa@uea.ac.uk

Dear all
Jacquie had sounded very positive about this back in August, but it sounds like CSERGE are as stretched as much as people in CRU. I'm afraid it's looking like we're not going to be able to get anything together on this unless Rob is able to take a lead. But I think that we would still be lacking the interdisciplinary research team that AXA are stressing.
Clare
PS Rob - sorry not to have been in touch with you sooner about this, but I didn't know until Tuesday that you were interested/had been approached.

Subject: RE: AXA Research Fund: launch of a new call for projects
Date: Thu, 18 Sep 2008 08:32:25 +0100
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: AXA Research Fund: launch of a new call for projects
Thread-Index: AckXVyDtvdpNCFYaR+WQSE/hzBjNYgCCW77g
From: "Burgess Jacquelin Prof \ (ENV\)" <Jacquie.Burgess@uea.ac.uk>
To: "Goodess Clare Dr \ (ENV\)" <C.Goodess@uea.ac.uk>
Hi Clare I dont think weve got the capacity to take this on at this stage.
Never mind
there will always be other opportunities.
Best wishes
Jacquie

mail.2008

From: Clare Goodess [[1] mailto:C.Goodess@uea.ac.uk]
Sent: 15 September 2008 18:19
To: Burgess Jacquelin Prof (ENV)
Cc: Alexander Jan Dr (ENV); Agnew Maureen Dr (ENV); Harpham Colin Dr (ENV);
Busby Simon
Mr (ENV)
Subject: RE: AXA Research Fund: launch of a new call for projects

Dear Jacquie
I'm afraid that I've not had time to do anything about this call since
returning from
holiday. The deadline is rapidly approaching - 3 October and after this week,
I'm away
at meetings until after the deadline. I also have two ARCC proposals and a DCMS
tender
to get sorted out this week.
So, I am not going to be able to take any kind of a lead on this even if we
think its
worth trying to get a last minute proposal together. No-one else from CRU has
time to
take a leading role, but Colin and Maureen are interested. Colin has been
working on the
CRU weather generator which will be an integral part of the UKCIP08 user
interface and
Maureen has a broader impacts perspective and is lead author on the climate
chapter in
the forthcoming CII report. Simon Busby might also be interested - and has
good
experience of working with climate model outputs (although for a rather
different
purpose). One task for CRU would be to extend some of the validation work of
the
ENSEMBLES RCM runs. I should also be able to read and comment on material and
provide
some short draft sections of text (e.g., on ENSEMBLES, PRUDENCE, MICE and
STARDEX) - I
will have at least sporadic email access while away I hope.
But I think this is only going to be viable if somebody from CSERGE or the
decision-making group is able to co-ordinate things. And we don't have the
capacity for
hydrological modelling in CRU - so again, this would need input from others.
Though
there is also the requirement in the call to assess the quality of flood
modelling tools
currently licensed by insurers - about which I know nothing. If it would be
helpful to
have a quick meeting this week, Iet me know.
Best wishes, Clare
At 16:30 12/08/2008, you wrote:
Dear Clare,
Many thanks for this I think it would be an excellent opportunity for a CRU +
other
parts of the School response. I know Jan Alexander has already got a European
bid
through to second stage on floods. We could certainly put something together
with the
environmental decision-making components too. Lets discuss when you get back
from
holiday.
Best wishes
Jacquie

mail.2008

From: Clare Goodess [[2]mailto:C.Goodess@uea.ac.uk]
Sent: 12 August 2008 14:58
To: Burgess Jacquelin Prof (ENV)
Cc: Jones Philip Prof (ENV); Osborn Timothy Dr (ENV); Agnew Maureen Dr (ENV);

Harpham

Colin Dr (ENV)
Subject: Fwd: AXA Research Fund: launch of a new call for projects

Dear Jacquie

CRU is interested in putting in a proposal under this call. As you can see, as well as the climate science aspects, there is also a need to work on economic issues - so this could be a good opportunity for putting in a joint proposal with people in CSERGE or other parts of ENV. There are also additional collaborators on the climate and flooding

aspects that we could involve both in the UK and Germany.

I'm away from tomorrow for a couple of weeks, but the CRU people copied in on this email are also all interested in a potential proposal. Though currently we're not sure which

if any of us has time to lead on this at least immediately.

Best wishes, Clare

Subject: AXA Research Fund: launch of a new call for projects

Date: Tue, 22 Jul 2008 19:18:02 +0200

X-MS-Has-Attach: yes

X-MS-TNEF-Correlator:

Thread-Topic: AXA Research Fund: launch of a new call for projects

Thread-Index: AcjsHuVgYlR8ndbHSHiv/kwz02+NeQ==

From: "CHOUX Mathieu" <mathieu.choux@axa.com>

To: <C.Goodess@uea.ac.uk>

Cc: "appelaprojets" <appelaprojets@axa.com>

X-Canit-CHI2: 0.00

X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f034)

X-Spam-Score: 4.10 (****) [Tag at 5.00]

DEAR_SOMETHING,HTML_MESSAGE,MIME_QP_LONG_LINE

X-CanItPRO-Stream: UEA:f034 (inherits from

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

X-Canit-Stats-ID: 6808857 - c6a2c2ad9106

X-Antispam-Training-Forget:

[3]<https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=f>

X-Antispam-Training-Nonspam:

[4]<https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=n>

X-Antispam-Training-Spam:

[5]<https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=s>

X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.185

Hello Clare,

AXA recently launched a call for projects to academic institutions focused on the flooding risk and the impacts of climate change. The Climatic Research Unit may have been approached with the email reproduced below, and I just wanted to make sure you received the information.

Sincerely Yours,

Mathieu Choux

Dear Madam/Sir,

The AXA Research Fund has been created in order to encourage research in a number of disciplines that touch on the risks, challenges and major transformations that affect our rapidly changing world. The Fund will award 100 million Euros over five years to finance innovative research.

The AXA Research Fund team is delighted to announce the launch of a new call for projects on climate change impacts on the risk of flooding in Europe (see attached document).

All the information needed to apply can be found on our internet site:
[6]<http://researchfund.axa.com/en/research-funding/calls-projects/>

Please make sure this information is communicated within your institution. The results of the selection process will be communicated to them as of January 15, 2009 .

Sincerely,

The AXA Research Fund Team
[7]appelaprojets@axa.com

Mathieu CHOUX
Risk Analyst - Catastrophe Modeling Department
AXA Group
GIE AXA - 9 av. de Messine - Paris, France
[8]mathieu.choux@axa.com
Tel. : +33 1 40 75 55 68 - Fax : +33 1 40 75 58 27
AXA redefining / standards
Please consider the environment before printing this message

Ce message
est

confidentiel; Son contenu
ne represente en
aucun

cas

un engagement de la part
de

AXA sous reserve de
tout accord

conclu

par ecrit entre

mail.2008

vous

et AXA. Toute
publication,

utilisation
ou

diffusion,
meme

partielle, doit
etre

autorisee

prealablement.

Si

vous n'etes pas

destinataire de ce
message,

merci d'en
avertir

immEDIATEMENT

l'expediteur.

This message is

confidential;
its contents

do

not
constitute

a

commitment by AXA

except where provided for
in a written
agreement

between you and AXA.

Any unauthorised
disclosure,
use

or

dissemi-

nation, either whole
or

partial, is
prohibited. If you are
not

the

intended recipient of
the

message, please
notify
the

sender

imme-

diately.

Dr Clare Goodess
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
UK
Tel: +44 -1603 592875
Fax: +44 -1603 507784
web: [9]<http://www.cru.uea.ac.uk/>
[10]<http://www.cru.uea.ac.uk/~clareg/clare.htm>

Dr Clare Goodess
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
UK
Tel: +44 -1603 592875
Fax: +44 -1603 507784
web: [11]<http://www.cru.uea.ac.uk/>
[12]<http://www.cru.uea.ac.uk/~clareg/clare.htm>

Dr Clare Goodess

mail.2008

Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
UK
Tel: +44 -1603 592875
Fax: +44 -1603 507784
web: [13]<http://www.cru.uea.ac.uk/>
[14]<http://www.cru.uea.ac.uk/~clareg/clare.htm>

References

1. <mailto:C.Goodess@uea.ac.uk>
2. <mailto:C.Goodess@uea.ac.uk>
3. <https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=f>
4. <https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=n>
5. <https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=s>
6. blocked::<http://researchfund.axa.com/en/research-funding/calls-projects/>
7. <mailto:appelaprojets@axa.com>
8. <mailto:mathieu.choux@axa.com>
9. <http://www.cru.uea.ac.uk/>
10. <http://www.cru.uea.ac.uk/~clareg/clare.htm>
11. <http://www.cru.uea.ac.uk/>
12. <http://www.cru.uea.ac.uk/~clareg/clare.htm>
13. <http://www.cru.uea.ac.uk/>
14. <http://www.cru.uea.ac.uk/~clareg/clare.htm>

920. 1221851501.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: Status of IJoC manuscript
Date: Fri Sep 19 15:11:41 2008

Ben,
Good news. Endnote types is a much better option than in the text - not as good as footnotes.

Yes the paper you attached does look crap. I will read it though even if the journal is even worse.

This paper has come out. The plot of London and Vienna temps, although an aside, is something I need to follow up more. London has a UHI, but it doesn't mean any more warming in the 20th century!

Hope all is well with you.

Cheers
Phil

PS Attached another paper - has some nice photos!
At 17:12 18/09/2008, you wrote:

Dear folks,

I just wanted to give you a brief update on the status of our IJoC manuscript.

I received the page proofs about three weeks ago. Unfortunately, IJoC did not allow us to employ footnotes. You may recall that we made liberal use of footnotes in order to present technical information that would have interfered with the "flow" of the main text. The IJoC copy editors simply folded all footnotes into the main text.

mail.2008

This was done without any regard for context. It made the main text very difficult to read. After lengthy negotiations with IJoC editors, we decided on a compromise solution. while IJoC was unwilling to accept footnotes (for reasons that are still unclear to me), they did agree to accept endnotes. The footnotes have now been transferred to an Appendix 2 entitled "Technical Notes". while this is not an optimal solution, it's a heck of a lot better than IJoC's original "assimilate in main text" solution. Now that the footnote issue has been resolved, I'm hoping that online publication of our paper will happen within the next several weeks. I'll let you know as soon as I receive a publication date from IJoC. LLNL (and probably NOAA, too) will be working on press releases for the paper. I'll also be drafting a one-page, plain English "fact sheet", which will address why we initiated this study, what we learned, why I'll never do this again, etc. I'll circulate this fact sheet for your comments early next week. With best regards,

Ben

(P.S.: David Douglass and John Christy continue to publish crappy papers. For their

latest science fiction, please see:
[1]<http://arxiv.org/ftp/arxiv/papers/0809/0809.0581.pdf>)

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

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://arxiv.org/ftp/arxiv/papers/0809/0809.0581.pdf>

921. 1222285054.txt

#####

From: "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: London UHI
Date: wed, 24 Sep 2008 15:37:34 +0100
Cc: "Wilby, Robert" <r.wilby@lancaster.ac.uk>

mail.2008

Hi Phil

Thanks for the comments on the Briefing report. You say "There is no evidence with London of any change in the amount of the UHI over the last 40 years. The UHI is clear, but it's not getting any worse" and sent a paper to show this. By coincidence I also got recently a paper from Rob which says "London's UHI has indeed become more intense since the 1960s esp during spring and summer". Its not something I need to sort out for UKCIP08, but I thought you both might like to be aware of each others findings. I didn't keep a copy of Rob's PDF after I printed it off but I am sure you can swap papers. I don't need to be copied in to any discussion.

Cheers
Geoff

922. 1222901025.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, 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@noaa.gov, 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: Next version of press release
Date: wed, 01 Oct 2008 18:43:45 -0700
Reply-to: santer1@llnl.gov
Cc: Anne Stark <stark8@llnl.gov>, "Parker, David (Met Office)" <david.parker@metoffice.gov.uk>, "David C. Bader" <bader2@llnl.gov>, "Bamzai, Anjali" <Anjali.Bamzai@science.doe.gov>

<x-flowed>
Dear folks,

Here is the next version of the press release for our IJoC paper. I received a number of comments from you (many thanks!), and have tried hard to incorporate them without increasing the length of the release.

Peter Thorne suggested that it might be useful to delete the explicit reference to the UR/UAH group, and instead refer to the Douglass et al. IJoC paper in a footnote. After some internal debate, I have not done that. Anne Stark advised me that footnotes are not often used in press releases (they tend to get ignored by reporters). Furthermore, I couldn't see an easy way of getting rid of the "UR/UAH" acronym, yet still making a clear distinction between their results and our results, their test and our test, etc., etc.

I've tried to capture the spirit if not the letter of your suggested edits. Unfortunately, I don't think we have the time to iterate for days on the press release - we really need to finalize this tomorrow. We will have a little more time to finalize the "fact sheet".

mail.2008

So please let me know as soon as possible if there's anything you can't live with in the press release.

One final point. Peter also asked whether it might be useful to include the telephone numbers of co-authors in the final paragraph of the press release. Anne and I would prefer not to do that. If you are agreeable to fielding press inquiries about the paper, please let me know, and send me a telephone number under which you can be reached in the next few days. We'll then compile a list (with contact information) of co-authors willing to discuss the paper with interested reporters.

I hope to send you a revised version of the fact sheet later tomorrow.

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

</x-flowed>

Attachment Converted: "c:\eudora\attach\Santer_IJC_Sept_2008_v7.doc"

923. 1223915581.txt

#####

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>, Clare Goodess <C.Goodess@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Douglas Maraun" <d.maraun@uea.ac.uk>, "Janice Darch" <J.Darch@uea.ac.uk>
Subject: Re: potential DfID funding for climate centre
Date: Mon, 13 Oct 2008 12:33:01 +0100

<x-flowed>

have not been approached - but I think it really does sound like the sort of initiative CRU/ENV are looking for. I get the feeling this is the sort of potential contact ENV would wish to take over.
Keith

At 11:31 13/10/2008, Tim Osborn wrote:

>Hi CRU Board,

>

>I just had an interesting chat with Jack Newnham
>from the International Development Team at Price
>Waterhouse Cooper. They get lots of DfID
>(Douglas: DfID is the UK Government Department
>for International Development) funding.

mail.2008

>
>They've heard that DfID are likely to call for
>expressions of interest for a new centre
>focussing on international climate
>change. Their idea is to fund a centre that
>would be the first point of call for advice and
>for commissioning research related to climate
>change and development or to climate change in countries where DfID operate.

>
>He was talking about £15 million per year for 5
>years! Not sure how much would be from DfID and
>how much raised from other donors (and hence
>uncertain), nor how much would be given up-front
>versus how much spent later on specific research
>projects organised via this centre.

>
>Nevertheless, sounds big enough to be worth getting involved in.

>
>He was clearly just testing the water with us,
>so not sure that they definitely wish to involve
>us. He may want to meet to talk through things,
>if they decide to ask us to join their
>proposal. He said he'd email me later -- I'll
>forward this when it arrives. They're also
>contacting the Tyndall Centre, and no doubt a number of other institutes.

>
>Has anyone else in CRU been approached?

>
>Presumably, if this call for tenders is actually
>issued, this is likely to interest Tyndall
>greatly. But CRU can offer a significant
>contribution -- especially data and scenarios
>developed for specific (developing) countries --
>and this should be seen as independent from
>Tyndall rather than part of Tyndall
>contribution. There's also Declan/DEV, so UEA as a whole has much to offer.

>
>Any thoughts on this?

>
>Tim

>
>
>
>
>Dr Timothy J Osborn, Academic Fellow
>Climatic Research Unit
>School of Environmental Sciences
>University of East Anglia
>Norwich NR4 7TJ, UK

>
>e-mail: t.osborn@uea.ac.uk
>phone: +44 1603 592089
>fax: +44 1603 507784
>web: <http://www.cru.uea.ac.uk/~timo/>
>sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

mail.2008

Phone: +44-1603-593909
Fax: +44-1603-507784

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

</x-flowed>

924. 1224005421.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: David Douglass <douglass@pas.rochester.edu>
Subject: Response
Date: Tue, 14 Oct 2008 13:30:21 -0700
Reply-to: santer1@llnl.gov
Cc: "Peter W. Thorne" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <>wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, 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>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, "David C. Bader" <bader2@llnl.gov>

<x-flowed>

Prof. Douglass,

You have access to EXACTLY THE SAME radiosonde data that we used in our recently-published paper in the International Journal of Climatology (IJoC). You are perfectly within your rights to verify the calculations we performed with those radiosonde data. You are welcome to do so.

We used the IUK radiosonde data (the data mentioned in your email) to calculate zonal-mean temperature changes at different atmospheric levels. You should have no problem in replicating our calculation of zonal means. You can compare your results directly with those displayed in Figure 6 of our paper. You do not need our "numerical quantities" in order to determine whether we have correctly calculated zonal-mean trends, and whether the IUK data show tropospheric amplification of surface temperature changes.

Similarly, you should have no problem in replicating our calculation of "synthetic" MSU temperatures from radiosonde data. Algorithms for calculating synthetic MSU temperatures have been published by ourselves and others in the peer-reviewed literature. You have already demonstrated (in your own IJoC paper of 2007) that you are capable of computing synthetic MSU temperatures from climate model output. Furthermore, I note that in your 2007 IJoC paper, you have already successfully replicated our "model average" synthetic MSU temperature trends (which were published in the Karl et al., 2006 CCSP Report).

In summary, you have access to the same model and observational data that we used in our 2008 IJoC paper. You have all the information that you require in order to determine whether the conclusions reached in our IJoC paper are sound or unsound.

You are quick to threaten your intent to file formal complaints against me "with the journal and other scientific bodies". If I were you, Dr. Douglass, I would instead focus my energies on rectifying the serious

mail.2008

error in the "robust statistical test" that you applied to compare modeled and observed temperature trends.

I am copying this email to all co-authors of the 2008 Santer et al. IJoC paper, as well as to Professor Glenn McGregor at IJoC. They deserve to be fully apprised of your threat to file formal complaints.

Please do not communicate with me in the future.

Ben Santer

David Douglass wrote:

- > My request is not unreasonable. It is normal scientific discourse and should not be a personal matter.
- > This is a scientific issue. You have published a paper with conclusions based upon certain specific numerical quantities. As another scientist, I challenge the value of those quantities. These values can not be authenticated by my calculating them because I have nothing to compare them to.
- >
- > If you will not give me the values of the IUK data in figure 6 then I will consider filing a formal complaint with the journal and other scientific bodies.
- >
- > David Douglass

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>

925. 1224035484.txt

#####

From: Gabi Hegerl <Gabi.Hegerl@ed.ac.uk>
 To: "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>
 Subject: RE: Meeting Jan 21-23
 Date: Tue, 14 Oct 2008 21:51:24 +0100
 Cc: Myles Allen <allen@atm.ox.ac.uk>, claudia tebaldi <claudia.tebaldi@gmail.com>, Knutti Reto <reto.knutti@env.ethz.ch>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, "Zwiers, Francis [Ontario]" <francis.zwiers@ec.gc.ca>, Tim Barnett <tbarnett-ul@ucsd.edu>, Hans von Storch <hvonstorch@web.de>, Claudia Tebaldi <tebaldi@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, Toru Nozawa <nozawa@nies.go.jp>, Ben Santer <santer1@llnl.gov>, 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>, Chris Miller <christopher.d.miller@noaa.gov>, Tom Knutson <Tom.Knutson@noaa.gov>, Tim Delsole <delsole@cola.iges.org>, Susan Solomon <Susan.Solomon@noaa.gov>, "Jones, Gareth S" <gareth.s.jones@metoffice.gov.uk>, Tara Torres <tara@ucar.edu>

<x-flowed>

Hi all, I assume this is general interest, not IDAG meeting - I think the

mail.2008

meeting would be a bit too big and complicated if we would try to resolve IPCC type issues - on the other hand, involving Chris Field and maybe Tom Stocker may be an interesting way to vent the scientific issues in a relaxed setting. But I would suggest to avoid agency type things - can be convinced otherwise if you feel strongly. we do have a limited budget, too!

Gabi

Quoting "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>:

> Myles,
>
> The Dept of State is the U.S. lead on IPCC, Conference of Party
> discussions, etc. USAID does the bulk of adaptation assistance at the
> international level. At the national level, there are various CCSP
> agencies, e.g. Dept of Agriculture, Dept of Interior, EPA, who are more
> on the 'application' side of the CCSP.
>
> I'd need to ask someone in those agencies on how they are approaching
> the issues you raise. Perhaps Chris Miller knows someone there...?
>
> Programs such as NOAA Climate Change Data Detection (CCDD), and DOE
> Climate Change Prediction Program (CCPP) focus almost exclusively on
> IPCC WG I type of questions.
>
> Anjuli
>
>
> -----Original Message-----
> From: Myles Allen [mailto:allen@atm.ox.ac.uk]
> Sent: Tuesday, October 14, 2008 5:00 AM
> To: claudia tebaldi; Gabi Hegerl
> Cc: Knutti Reto; Stott, Peter; Zwiers, Francis [Ontario]; Tim Barnett;
> Hans von Storch; Claudia Tebaldi; Phil Jones; David Karoly; Toru Nozawa;
> Ben Santer; Daithi Stone; Richard Smith; Nathan Gillett; Michael Wehner;
> Doug Nychka; Xuebin Zhang; Bamzai, Anjuli; Chris Miller; Tom Knutson;
> Tim Delsole; Susan Solomon; Jones, Gareth S; Tara Torres
> Subject: RE: Meeting Jan 21-23
>
> Hi All,
>
> That is a very good idea indeed. I was talking to Tom Stocker last week,
> arguing that resolving the differences in the definition of attribution
> between WG1 and WG2 was going to be one of the key challenges for AR5,
> particularly as attribution of impacts becomes a live topic as countries
> start to make the case for adaptation assistance. How about we invite
> the co-Chair of WG1 along as well?
>
> If we are going to invite Chris Field, we should definitely also invite
> someone from the "double attribution" community, or it will seem a bit
> like WG1 lecturing to the co-Chair of WG2. Any suggestions, David?
>
> Anjuli, has anyone in the US State Department (or whichever department
> will handle this) started addressing the question of how the US
> government will distinguish "impacts of climate change" from
> "vulnerability to natural climate variability" in allocating resources
> for adaptation assistance? If anyone has even started thinking about
> this problem, it would be very interesting to hear from them to know
> what questions they are likely to need answering. we could also try and
> find out if anyone in the European Commission is worrying about this.
>
> Regards,

mail.2008

>
> Myles
>
> -----Original Message-----
> From: claudia tebaldi [mailto:claudia.tebaldi@gmail.com]
> Sent: 13 October 2008 20:46
> To: Gabi Hegerl
> Cc: Myles Allen; Knutti Reto; Stott, Peter; Zwiers, Francis [Ontario];
> Tim Barnett; Hans von Storch; Claudia Tebaldi; Phil Jones; David Karoly;
> Toru Nozawa; Ben Santer; stoned@csag.uct.ac.za; Richard Smith; Nathan
> Gillett; Michael Wehner; Doug Nychka; Xuebin Zhang; Bamzai, Anjali;
> Chris Miller; Tom Knutson; Tim Delsole; Susan Solomon; Jones, Gareth S;
> Tara Torres
> Subject: Re: Meeting Jan 21-23
>
> Hi Gabi et al.
>
> I wonder if we could try to get Chris Field, who is going to be the
> chair of working group 2 for AR5...I don't know how likely it is to get
> him but it may be interesting to get his perspective on what was done in
> AR4 WG2 and what he would like to see in AR5 WG2.
>
> c
>
> On Mon, Oct 13, 2008 at 10:51 AM, Gabi Hegerl <gabi.hegerl@ed.ac.uk>
> wrote:
>> Hi IDAG people,
>>
>> Its time to start planning our next IDAG meeting in detail. A
> provisional
>> coarse agenda is attached. Please feel free to email me suggestions
>> to improve/update this, and if there is a topic you would
> love
>> to see covered but that isn;t please get in touch as well.
>> Also, we should have one topic related to the impacts review paper
> that is
>> to be written in year 2 of the grant. Therefore, if you have a
>> suggestion of a guest that would help us elucidate the
> challenges in
>> impact attribution but also to move forward on this, please let me
>> know!
>> Tara Torres from UCAR (tara@ucar.edu) will help us to plan the
> meeting.
>> Also, I hope to hire a student helper at Duke to get our meeting
> webpage
>> going, keep track of agenda items etc, but please bear with me and
>> tolerate a bit of chaos before we have succeeded with this!
>>
>> What I need from you is to please
>> - let me know if you can make it, and what you would vaguely like to
> speak
>> about (you can do the first now and postpone the second)
>> - get in touch with Tara to book your travel - ideally, towards the
> end of
>> October / or in early November (she is a bit buried right now)
>> - get in touch with me when you have suggestions, or want to bring
> somebody
>>
>> Gabi
>>
>> --
>> Dr Gabriele Hegerl School of GeoSciences The University of Edinburgh
>> Grant Institute, The King's Buildings West Mains Road EDINBURGH EH9

mail.2008

>> 3JW Phone: +44 (0) 131 6519092, FAX: +44 (0) 131 668
> 3184

>> Email: Gabi.Hegerl@ed.ac.uk

>>
>> The University of Edinburgh is a charitable body, registered in
>> Scotland, with registration number SC005336.

>>
>>
>
>
>
> --

> Claudia Tebaldi
> Research Scientist, Climate Central
> <http://www.climatecentral.org>
> currently visiting IMAGE/NCAR
> PO Box 3000
> Boulder, CO 80305
> tel. 303.497.2487

>
>
>
>

--
Gabriele Hegerl
School of GeoSciences
University of Edinburgh
<http://www.geos.ed.ac.uk/people/person.html?indv=1613>

--
The University of Edinburgh is a charitable body, registered in
Scotland, with registration number SC005336.

</x-flowed>

926. 1224176459.txt

#####

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: why are the temperature data from Hadley different from NASA?
Date: Thu, 16 Oct 2008 13:00:59 -0400
Cc: Judith Lean <jlean@ssd5.nrl.navy.mil>, Yousif K Kharaka <ykharaka@usgs.gov>

thanks Phil--this all makes sense. I'll be intrigued to hear more about how the
melting sea
ice issue is going to be dealt with. no question there is a lot of warming going
on up
there.

hope to see you one of these days,
mike

On Oct 16, 2008, at 6:52 AM, Phil Jones wrote:

Hi Mike, Judith and Yousif,

mail.2008

Mike has basically answered the question. The GISS group average surface T data into 80 equal area boxes across the world. The UK group (CRU/MOHC) grid the data into 5 by 5 degree lat/long boxes, as does NCDC. These griddings don't allow so much extrapolation of data - no extrapolation beyond the small grid box. The US groups also calculate the globe as one domain, whereas we in the UK use (NH+SH)/2. This also makes some difference as most of the missing areas are in the SH, and currently the NH is warmer than the SH with respect to 1961-90. Our rationale for doing what we do is that it is better to estimate the missing areas of the SH (which we do by tacitly assuming they are the average of the rest of the SH) from the rest of the SH as opposed to the rest of the world.

The Arctic is a problem now. With less sea ice, we are getting SST data in for regions for which we have no 1961-90 averages - because it used to be sea ice (so had no measurements).

We are not using any of the SST from the central Arctic in summer. So we are probably underestimating temperatures in the recent few years. We're working on what we can do about this. There are also more general SST issues in recent years.

In 1990, for example, almost all SST values came from ships. By 2000 there were about 20% from buoys and drifters, but by 2008 this percentage is about 85%. We're also doing comparisons of the drifters with the ships where both are plentiful, as it is likely that drifters measure a tenth of one degree C cooler than ships, and the 1961-90 period is ship-based average.

New version of the dataset coming in summer 2009.

All the skeptics look at the land data to explain differences between datasets and say urbanization is responsible for some or all of the warming. The real problem is the marine data at the moment.

Attaching a recent paper on urbanization and effects in China.

Cheers

Phil

At 22:08 15/10/2008, Michael Mann wrote:

Hi Judith,

It's nice to hear from you, been too long (several years??). My understanding is that the differences arise largely from how missing data are dealt with. For example, in Jim

et al's record the sparse available arctic data are interpolated over large regions, whereas Phil and co. either use the available samples or in other versions (e.g. Brohan

et al) use optimal interpolation techniques. The bottom line is that Hansen et al 'j05 I

believe weights the high-latitude warming quite a bit more, which is why he gets a warmer '05, while Phil and co find '98 to be warmer.

But Phil can certainly provide a more informed and complete answer!

mike

p.s. see you at AGU this year??

mail.2008

On Oct 15, 2008, at 5:03 PM, Judith Lean wrote:

Hi Yousif,
Many apologies for not replying sooner to your email - but I've only just returned from travel and am still catching up with email.
Unfortunately, I am simply a "user" of the surface temperature data record and not an expert at all, so cannot help you understand the specific issues of the analysis of the various stations that produce the differences that you identify. I too would like to know the reason for the differences.
Fortunately, there are experts who can tell us, and I am copying this email to Mike Mann and Phil Jones who are such experts.
Mike and Phil (hi! hope you are both well!), can you please, please help us to understand these differences that Yousif points out in the GISS and Hadley Center surface temperature records (see two attached articles).
Many thanks, for even a brief answer, or some reference.
Judith
On Oct 8, 2008, at 1:50 PM, Yousif K Kharaka wrote:

Judith:
I hope you are doing well (these days OK would be good!) at work and personally.
Can you help me to understand the huge discrepancy (see below) between the temperature data from the Hadley Center and GISS? Any simple explanations, or references that I can read on this topic? I certainly would appreciate your help on this.
Best regards. Yousif Kharaka
Yousif Kharaka, Research Geochemist Phone: (650) 329-4535
U. S. Geological Survey, MS 427 Fax: (650) 329-4538
345, Middlefield Road Mail:
[1]ykharaka@usgs.gov
Menlo Park, California 94025, USA
----- Forwarded by Yousif K Kharaka/WRD/USGS/DOI on 10/08/2008 10:42 AM -----
Yousif K Kharaka/WRD/USGS/DOI
10/06/2008 02:07 PM

To

"Dr David Jenkins" <[2]jenkins@chartwood.com >

cc

[3]allyson_anderson@energy.senate.gov, [4]drahovzal@uky.edu,
[5]dvance@arcadis-us.com,
[6]ebarron@jsg.utexas.edu, "'Gene Shinn'" <[7]eshinn@marine.usf.edu>,
[8]jarmenrock@gmail.com, [9]jblank@aapg.org, [10]Jeffrey@LevineOnLine.com,
[11]jjones@vanoperating.com, [12]julie.kupecz@shell.com,
[13]pgrew@unlnotes.unl.edu,
[14]rick-bsr@tyler.net, [15]scott.tinker@beg.utexas.edu, [16]tpaexpl@aol.com,
[17]w.a.morgan@conocophillips.com

Subject

why are the temperature data from Hadley different from NASA? [18]Link
Page 271

mail.2008

David and all:

One advantage (or great disadvantage if you are very busy!) of membership in GCCC is that you are forced to investigate topics outside your areas of expertise. For some time now, I have been puzzled as to why global temperature data from the British Hadley Centre are different from those reported by NASA GISS, especially in the last 10 years. GISS reports that 2005 was the warmest year (see first attachment) on record, and that 2007 tied 1998 for the second place. The Hadley group continues reporting 1998 (a strong El Nino year) as having the highest global temperature, and then showing temperature decreases thereafter. The two groups report their temperatures relative to different time intervals (1951-1980 for GISS; 1961-1990 for Hadley), but much more important is the fact that GISS data include temperatures from the heating Arctic that are excluded by others (see second attachment). If you are interested in the topic of sun spots, the 11-year irradiance cycle, and solar forcing versus AGHGs, see the first attachment for what NASA has to say.

We may need help on this complex topic from a "true climate scientists", such as Judith

Lean!

Cheers. Yousif Kharaka

Yousif Kharaka, Research Geochemist
U. S. Geological Survey, MS 427
345, Middlefield Road

Phone: (650) 329-4535

Fax: (650) 329-4538

Mail:

[19]y^kharaka@usgs.gov

Menlo Park, California 94025, USA

<GCC-Data @ NASA GISS_ GISS Surface Temperature Analysis_ 2007.pdf>

<GCC-2005 Warmest Year In A Century.pdf>

<GCC-Data @ NASA GISS_ GISS Surface Temperature Analysis_ 2007.pdf><GCC-2005

Warmest

Year In A Century.pdf>

--

Michael E. Mann

Associate Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology

Phone: (814) 863-4075

503 Walker Building

FAX: (814) 865-3663

The Pennsylvania State University

email: [20]mann@psu.edu

University Park, PA 16802-5013

website: [21]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[22]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 [23]p.jones@uea.ac.uk

NR4 7TJ

UK

mail.2008

<jonesetal2008_china.pdf>

--

Michael E. Mann
 Associate Professor
 Director, Earth System Science Center (ESSC)
 Department of Meteorology Phone: (814) 863-4075
 503 Walker Building FAX: (814) 865-3663
 The Pennsylvania State University email: [24]mann@psu.edu
 University Park, PA 16802-5013
 website: [25]http://www.meteo.psu.edu/~mann/Mann/index.html
 "Dire Predictions" book site:
 [26]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. mailto:ykharaka@usgs.gov
2. mailto:jenkins@chartwood.com
3. mailto:allyson_anderson@energy.senate.gov
4. mailto:drahovzal@uky.edu
5. mailto:dvance@arcadis-us.com
6. mailto:ebarron@jsg.utexas.edu
7. mailto:eshinn@marine.usf.edu
8. mailto:jarmenrock@gmail.com
9. mailto:jblank@aapg.org
10. mailto:Jeffrey@LevineOnLine.com
11. mailto:jjones@vanoperating.com
12. mailto:julie.kupecz@shell.com
13. mailto:pgrew@unlnotes.unl.edu
14. mailto:rick-bsr@tyler.net
15. mailto:scott.tinker@beg.utexas.edu
16. mailto:tpaexpl@aol.com
17. mailto:w.a.morgan@conocophillips.com
- 18.

Notes:///8825668F00670ABE/DABA975B9FB113EB852564B5001283EA/A93F684FF508B452872574D90044850F

19. mailto:ykharaka@usgs.gov
20. mailto:mann@psu.edu
21. http://www.meteo.psu.edu/~mann/Mann/index.html
22. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
23. mailto:p.jones@uea.ac.uk
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

Hidden links:

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

927. 1225026120.txt

#####

From: Mick Kelly <mick.tiempo@googlemail.com>
 To: <P.Jones@uea.ac.uk>
 Subject: RE: Global temperature
 Date: Sun, 26 Oct 2008 09:02:00 +1300

Yeah, it wasn't so much 1998 and all that that I was concerned about, used to dealing with that, but the possibility that we might be going through a longer - 10 year - period of relatively stable temperatures beyond what you

mail.2008

might expect from La Nina etc.

Speculation, but if I see this as a possibility then others might also. Anyway, I'll maybe cut the last few points off the filtered curve before I give the talk again as that's trending down as a result of the end effects and the recent cold-ish years.

Enjoy Iceland and pass on my best wishes to Astrid.

Mick

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

> From: P.Jones@uea.ac.uk [mailto:P.Jones@uea.ac.uk]

> Sent: 24 October 2008 20:39

> To: Mick Kelly

> Subject: Re: Global temperature

>

>

> Mick,

> They have noticed for years - mostly wrt

> the warm year of 1998. The recent coolish years

> down to La Nina. When I get this question I

> have 1991-2000 and 2001-2007/8 averages to hand.

> Last time I did this they were about 0.2 different,

> which is what you'd expect.

> In Iceland at a meeting that Astrid invited me to.

> Cold with snow on the ground, but things cheap as the

> currency has gone down 30-40% wrt even the pound.

>

>

> Cheers

> Phil

>

> > Hi Phil

> >

> > Just updated my global temperature trend graphic for a

> > public talk and

> > noted

> > that the level has really been quite stable since 2000 or

> > so and 2008

> > doesn't look too hot.

> >

> > Anticipating the sceptics latching on to this soon, if they

> > haven't done

> > already, has anyone had a good look at the large-scale circulation

> > anomalies

> > over this period? I haven't noticed anything consistent

> > coming up in the

> > annual climate reviews but then I wasn't really looking.

> >

> > Be awkward if we went through a early 1940s type swing!

> >

> > Hope all's well with you

> >

> > Mick

> >

> >

> >

> > _____

> > Mick Kelly

> > PO Box 4260

> > Whangarei 0141

> > email: mick.tiempo@gmail.com

> > web: www.tiempocyberclimate.org

Kamo

New Zealand

> >
> >
> >
>
>

928. 1225140121.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: End of the road...
Date: Mon Oct 27 16:42:01 2008

Ben,

It seems that Climate Audit has been discussing the paper. I ad
a look whilst I was in Iceland as I had nothing better to do a few times.
It was cold and snowy outside, there was internet.....

Seems as though they are making some poor assumptions; someone
is trying to defend us, but gets rounded upon and one of the co-authors
on the paper is in touch with McIntyre.

As it isn't me, and I can rule out a number of the others, my list of who
it might be isn't that long....

Looking forward to next week !!

Cheers
Phil

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

929. 1225412081.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: "'Philip D. Jones'" <p.jones@uea.ac.uk>
Subject: [Fwd: Re: [Fwd: Typo in equation 12 Santer.]]
Date: Thu, 30 Oct 2008 20:14:41 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear Phil,

I thought you'd be interested in my reply to Gavin (see forwarded email).

Cheers,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory

mail.2008

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
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, 30 Oct 2008 20:10:53 -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
m9V3Arh7024023;
Thu, 30 Oct 2008 20:10:53 -0700
X-Attachments: None
X-IronPort-AV: E=McAfee;i="5300,2777,5419"; a="30418306"
X-IronPort-AV: E=Sophos;i="4.33,519,1220252400";
d="scan'208";a="30418306"
Received: from dione.llnl.gov (HELO [128.115.57.29]) ([128.115.57.29])
by nspiron-1.llnl.gov with ESMTTP; 30 Oct 2008 20:10:53 -0700
Message-ID: <490A773D.20807@llnl.gov>
Date: Thu, 30 Oct 2008 20:10:53 -0700
From: Ben Santer <santer1@llnl.gov>
Reply-To: santer1@llnl.gov
Organization: LLNL
User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
MIME-Version: 1.0
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
CC: Karl Taylor <taylor13@llnl.gov>
Subject: Re: [Fwd: Typo in equation 12 Santer.]
References: <1224543811.19301.2452.camel@isotope.giss.nasa.gov>
In-Reply-To: <1224543811.19301.2452.camel@isotope.giss.nasa.gov>
Content-Type: text/plain; charset=ISO-8859-1; format=flowed
Content-Transfer-Encoding: 7bit

<x-flowed>
Dear Gavin,

There is no typo in equation 12. The first term under the square root in equation 12 is a standard estimate of the variance of a sample mean (see, e.g., "Statistical Analysis in Climate Research", Zwiers and Storch, their equation 5.24, page 86). The second term under the square root sign is a very different beast - an estimate of the variance of the observed trend. As we point out, our $d1^*$ test is very similar to a standard Student's t-test of differences in means (which involves, in its denominator, the square root of two pooled sample variances).

In testing the statistical significance of differences between the model average trend and a single observed trend, Douglass et al. were wrong to use σ_{SE} as the sole measure of trend uncertainty in their statistical test. Their test assumes that the model trend is uncertain, but that the observed trend is perfectly-known. The observed trend is not a "mean" quantity; it is NOT perfectly-known. Douglass et al. made a demonstrably false assumption.

Bottom line: σ_{SE} is a standard estimate of the uncertainty in a sample mean - which is why we use it to characterize uncertainty in the estimate of the model average trend in equation 12. It is NOT appropriate to use σ_{SE} as the basis for a statistical test between

mail.2008

two uncertain quantities (see our comments in our point #3, immediately before equation 12). The uncertainty in the estimates of both modeled AND observed trend needs to be explicitly incorporated in the design of any statistical test comparing modeled and observed trends. Douglass et al. incorrectly ignored uncertainties in observed trends.

Our Figure 6A is not a statistical test. It does not show the standard errors in the observed trends at discrete pressure levels (which would have made for a very messy Figure, given that we show results from 7 different observational datasets). Had we attempted to show the observed standard errors in Figure 6A, I suspect that standard errors from the RICH, IUK, RAOBCORE-v1.3, and RAOBCORE 1.4 datasets would have overlapped with the multi-model average trend at most pressure levels. I can easily produce such a Figure if necessary.

with best regards,

Ben

Gavin Schmidt wrote:

> Ben, Just thought I'd check with you first. I don't think there is a
> problem - but I think the question is really alluding to is our comment
> about Douglass et al 'being wrong' in using sigma_SE - since if we use
> it in the denominator in the d1* test, it can't be wrong, see?

>
> My response would be that we are testing a number of different things
> here: d1* tests whether the ensemble mean is consistent with the obs
> (given their uncertainty). Whereas our figure 6 and the error bars shown
> there are testing whether the real world obs are consistent with a
> distribution defined from the model ensemble members.

>
> gavin

> -----Forwarded Message-----

>
>> From: lucia liljegren <lucia@rankexploits.com>
>> To: gschmidt@giss.nasa.gov
>> Subject: Typo in equation 12 Santer.
>> Date: 20 Oct 2008 15:46:51 -0500

>> Hi Gavin,

>>
>> Someone commenting at ClimateAudit is suggesting that equation 12
>> contains a typo. They are under the impression the 1/nm does not
>> belong in the circled term. Rather than going back and forth with "is
>> not a typo", "is so a typo", I figured I'd just ask you. Is there a
>> typo in equation 12 below.

>> ----

>>

>>

>>

>>

>> BTW: I think Santer is pretty good paper.

>> Thanks, Lucia

>>

>>

>>

>>

>>

>>

--

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>

930. 1225462391.txt

 #####

From: Ben Santer <santer1@llnl.gov>
 To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, 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@noaa.gov, 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: [Fwd: Santer et al 2008]
 Date: Fri, 31 Oct 2008 10:13:11 -0700
 Reply-to: santer1@llnl.gov
 Cc: "David C. Bader" <bader2@llnl.gov>

Dear folks, while on travel in Hawaii, I received a request from Steven McIntyre for all of the model data used in our IJoC paper (see forwarded email). After some conversation with my PCMDI colleagues, I have decided not to respond to McIntyre's request. If McIntyre repeats his request, I will provide him with the same answer that I gave to David Douglass - all model and observational data used in our IJoC paper are freely available to scientific researchers (as are algorithms for calculating synthetic MSU temperatures from climate model and radiosonde data). If Mr. McIntyre wishes to "audit" our analysis and findings, he has access to exactly the same raw data that we employed. He can compute synthetic MSU temperatures exactly the same way that we did. And he has full details of the statistical tests we applied to compare modeled and observed temperature trends. Recall that McIntyre is the guy who "audited" the temperature reconstructions of Mike Mann and colleagues. Now it appears as if McIntyre wants to audit us. McIntyre should have "audited" the methods and findings of Douglass et al. 2007 - not the methods and findings

mail.2008

of Santer

et al. 2008. I thought you should know about this development. 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

X-Account-Key:

account1 Return-Path: Received: from mail-2.llnl.gov ([unix socket]) by
mail-2.llnl.gov

(Cyrus v2.2.12) with LMTPA; Mon, 20 Oct 2008 10:29:15 -0700 Received: from
mail-2.llnl.gov

(localhost.localdomain [127.0.0.1]) by mail-2.llnl.gov (8.13.1/8.12.3/LLNL
evision: 1.7 \$)

with ESMTMP id m9KHTFlg029183 for <[vacation]santer1@mail.llnl.gov>; Mon, 20 Oct
2008

10:29:15 -0700 Received: (from vacmgr@localhost) by mail-2.llnl.gov
(8.13.1/8.13.1/Submit)

id m9KHTFgz029180 for [vacation]santer1@mail.llnl.gov; Mon, 20 Oct 2008 10:29:15
-0700

X-Authentication-warning: mail-2.llnl.gov: vacmgr set sender to
stephen.mcintyre@utoronto.ca using -f Received: from nspiron-2.llnl.gov

(nspiron-2.llnl.gov

[128.115.41.82]) by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.7 \$) with
ESMTMP id

m9KHSuoB029014 for ; Mon, 20 Oct 2008 10:29:14 -0700 X-Attachments: None

X-IronPort-AV:

E=McAfee;i="5300,2777,5408"; a="29194653" X-IronPort-AV:

E=Sophos;i="4.33,453,1220252400";

d="scan'208,217";a="29194653" Received: from nsziron-1.llnl.gov

([128.115.249.81]) by

nspiron-2.llnl.gov with ESMTMP; 20 Oct 2008 10:29:13 -0700 X-Attachments: None

X-IronPort-Anti-Spam-Filtered: true X-IronPort-Anti-Spam-Result:

AosBADJd/EiAZISXgwdsb2JhbACCRyyHF4l1AQELBwQKBxGVE4Ns X-IronPort-AV:

E=McAfee;i="5300,2777,5408"; a="65324012" X-IronPort-AV:

E=Sophos;i="4.33,453,1220252400";

d="scan'208,217";a="65324012" Received: from bureau61.ns.utoronto.ca

([128.100.132.151]) by

nsziron-1.llnl.gov with ESMTMP; 20 Oct 2008 10:29:13 -0700 Received: from

acerd3c08b49af

(CPE0050bfe94416-CM00195efb6eb0.cpe.net.cable.rogers.com [99.231.2.44])

(authenticated

bits=0) by bureau61.ns.utoronto.ca (8.13.8/8.13.8) with ESMTMP id m9KHT9Ds024194

(version=TLSv1/SSLv3 cipher=RC4-MD5 bits=128 verify=NOT) for ; Mon, 20 Oct 2008

13:29:11

-0400 From: "Steve McIntyre" To: Subject: Santer et al 2008 Date: Mon, 20 Oct

2008 13:29:11

-0400 Message-ID: <000001c932d9\$5e5831a0\$6602a8c0@acerd3c08b49af> MIME-Version:

1.0

Content-Type: multipart/alternative;

boundary="-----_NextPart_000_0001_01c932B7.D74691A0"

X-Priority: 3 (Normal) X-MSMail-Priority: Normal X-Mailer: Microsoft Outlook,

Build

10.0.2627 Importance: Normal X-MimeOLE: Produced By Microsoft MimeOLE

v6.00.2900.3350

Dear Dr Santer,

mail.2008

Could you please provide me either with the monthly model data (49 series) used for statistical analysis in Santer et al 2008 or a link to a URL. I understand that your version has been collated from PCMDI ; my interest is in a file of the data as you used it (I presume that the monthly data used for statistics is about 1-2 MB) .

Thank you for your attention,

Steve McIntyre

931. 1225465306.txt

#####

From: "Cawley Gavin Dr \ (CMP\)" <G.Cawley@uea.ac.uk>
To: <santer1@llnl.gov>
Subject: RE: Possible error in recent IJC paper
Date: Fri, 31 Oct 2008 11:01:46 -0000
Cc: "Jones Philip Prof \ (ENV\)" <P.Jones@uea.ac.uk>, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "Tom Wigley" <wigley@cgd.ucar.edu>

Dear Ben,
many thanks for the full response to my query. I think my confusion arose from the discussion on RealClimate (which prompted our earlier communication on this topic), which clearly suggested that the observed trend should be expected to lie within the spread of the models, rather than necessarily being close to the mean as the models are stochastic simulations (which seemed reasonable). I've just re-read that post, the key paragraph from

[1]<http://www.realclimate.org/index.php/archives/2007/12/tropical-troposphere-trends/> is as

follows:
"The interpretation of this is a little unclear (what exactly does the sigma refer to?), but the most likely interpretation, and the one borne out by looking at their Table IIa, is that sigma is calculated as the standard deviation of the model trends. In that case, the formula given defines the uncertainty on the estimate of the mean - i.e. how well we know what the average trend really is. But it only takes a moment to realise why that is irrelevant. Imagine there were 1000's of simulations drawn from the same distribution, then our estimate of the mean trend would get sharper and sharper as N increased. However, the chances that any one realisation would be within those error bars, would become smaller and smaller. Instead, the key standard deviation is simply sigma itself. That defines the

mail.2008

likelihood that one realisation (i.e. the real world) is conceivably drawn from the distribution defined by the models."

I had therefore expected the test to use the standard deviations of both the models and the observations (which would give a flat plot in 5B and there would be an obvious overlap of the uncertainties in 6a at say 500hPa).

best regards
Gavin

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

From: Ben Santer [[2]mailto:santer1@llnl.gov]

Sent: Fri 10/31/2008 4:06 AM

To: Cawley Gavin Dr (CMP)

Cc: Jones Philip Prof (ENV); Gavin Schmidt; Thorne, Peter; Tom Wigley

Subject: Re: Possible error in recent IJC paper

Dear Gavin,

Thanks very much for your email, and for your interest in our recent paper in the International Journal of Climatology (IJoC). There is no error in equation (12) in our IJoC paper. Let me try to answer the questions that you posed.

The first term under the square root in our equation (12) is a standard estimate of the variance of a sample mean - see, e.g., "Statistical Analysis in Climate Research", by Francis Zwiers and Hans von Storch, Cambridge University Press, 1999 (their equation 5.24, page 86). The second term under the square root sign is a very different beast - an estimate of the variance of the observed trend. As we point out, our $d1^*$ test is very similar to a standard Student's t-test of differences in means (which involves, in its denominator, the square root of two pooled sample variances).

In testing the statistical significance of differences between the model average trend and a single observed trend, Douglass et al. were wrong to use σ_{SE} as the sole measure of trend uncertainty in their statistical test. Their test assumes that the model trend is uncertain, but that the observed trend is perfectly-known. The observed trend is not a "mean" quantity; it is NOT perfectly-known. Douglass et al. made a demonstrably false assumption.

Bottom line: σ_{SE} is a standard estimate of the uncertainty in a sample mean - which is why we use it to characterize uncertainty in the estimate of the model average trend in equation (12). It is NOT appropriate to use σ_{SE} as the basis for a statistical test between two uncertain quantities. The uncertainty in the estimates of both modeled AND observed trend needs to be explicitly incorporated in the design of any statistical test seeking to compare modeled and observed trends. Douglass et al. incorrectly ignored uncertainties in observed trends.

I hope this answers your first question, and explains why there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the comments that we made in point #3 [immediately before equation (12)]. As we note in point #3, "while σ_{SE} is an appropriate measure of how well the multi-model mean trend can be estimated from a finite sample of model results, it is not an appropriate measure for deciding whether this trend is consistent with a single observed trend."

We could perhaps have made point #3 a little clearer by inserting "imperfectly-known" before "observed trend". I thought, however, that the uncertainty in the estimate of the observed trend was already made very clear in our point #1 (on page 7, bottom of column 2).

To answer your second question, $d1^*$ gives a reasonably flat line in Figure 5B because the first term under the square root sign in equation (12) (the variance of the model average trend, which has a dependence on N, the number of models used in the test) is roughly a factor of 20 smaller than the second term under the square root sign (the variance of the observed trend, which has no dependence on N). The behaviour of $d1^*$

mail.2008

with synthetic data is therefore dominated by the second term under the square root sign - which is why the black lines in Figure 5B are flat. In answer to your third question, our Figure 6A provides only one of the components from the denominator of our $d1^*$ test (σ_{SE}). Figure 6A does not show the standard errors in the observed trends at discrete pressure levels. Had we attempted to show the observed standard errors at individual pressure levels, we would have produced a very messy Figure, since Figure 6A shows results from 7 different observational datasets.

We could of course have performed our $d1^*$ test at each discrete pressure level. This would have added another bulky Table to an already lengthy paper. We judged that it was sufficient to perform our $d1^*$ test with the synthetic MSU T2 and T2LT temperature trends calculated from the seven radiosonde datasets and the climate model data. The results of such tests are reported in the final paragraph of Section 7. As we point out, the $d1^*$ test "indicates that the model-average signal trend (for T2LT) is not significantly different (at the 5% level) from the observed signal trends in three of the more recent radiosonde products (RICH, IUK, and RAOBCORE v1.4)." So there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the results displayed in Figure 6.

Thanks again for your interest in our paper, and my apologies for the delay in replying to your email - I have been on travel (and out of email contact) for the past 10 days.

With best regards,

Ben

Cawley Gavin Dr (CMP) wrote:

>

>

> Dear Prof. Santer,

>

> I think there may be a minor problem with equation (12) in your paper
> "Consistency of modelled and observed temperature trends in the tropical
> troposphere", namely that it includes the standard error of the models
> $1/n_m s_{\langle b_m \rangle}^2$ instead of the standard deviation $s_{\langle b_m \rangle}^2$. Firstly
> the current formulation of (12) seems at odds with objection 3 raised at
> the start of the first column of page 8. Secondly, I can't see how the
> modified test d_{1^*} gives a flat line in Figure 5B as the test statistic
> is explicitly dependent on the size of the model ensemble n_m . Thirdly,
> the equation seems at odds with the results depicted graphically in
> Figure 6 which would suggest the models are clearly inconsistent at
> higher levels (400-850 hPa) using the confidence interval based on the
> standard error. Lastly, (12) seems at odds with the very lucid
> treatment at RealClimate written by Dr Schmidt.

>

> I congratulate all 17 authors for an excellent contribution that I have
> found most instructive!

>

> I do hope I haven't missed something - sorry to have bothered you if
> this is the case.

>

> best regards

>

> Gavin

>

--

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

References

1. <http://www.realclimate.org/index.php/archives/2007/12/tropical-troposphere-trends/>
2. <mailto:santer1@llnl.gov>

932. 1225579812.txt

#####

From: Tom Wigley <wigley@ucar.edu>
To: Ben Santer <santer1@llnl.gov>, Phil Jones <p.jones@uea.ac.uk>
Subject: [Fwd: Re: Possible error in recent IJC paper]
Date: Sat, 01 Nov 2008 18:50:12 -0600

Hi Ben & Phil, No need to push this further, and you probably realize this anyhow, but the

RealClimate criticism of Doug et al. is simply wrong. Ho hum. Tom. Return-Path: Received:

from nscan2.ucar.edu (nscan2.ucar.edu [128.117.64.192]) by upham.cgd.ucar.edu (8.13.1/8.13.1) with ESMTMP id m9VB1nbA017855 for ; Fri, 31 Oct 2008 05:01:49 -0600

Received: from localhost (localhost.localdomain [127.0.0.1]) by nscan2.ucar.edu (Postfix) with ESMTMP id 215F8309C01C for ; Fri, 31 Oct 2008 05:01:49 -0600 (MDT) Received: from

nscan2.ucar.edu ([127.0.0.1]) by localhost (nscan2.ucar.edu [127.0.0.1]) (amavisd-new, port

10024) with ESMTMP id 24343-06 for ; Fri, 31 Oct 2008 05:01:48 -0600 (MDT)

X-SMTP-Auth: no

Received: from mailgate5.uea.ac.uk (mailgate5.uea.ac.uk [139.222.130.185]) by nscan2.ucar.edu (Postfix) with ESMTMP id 7B9B2309C018 for ; Fri, 31 Oct 2008 05:01:47 -0600

(MDT) Received: from [139.222.130.203] (helo=UEAEXCHCLUS01.UEA.AC.UK) by mailgate5.uea.ac.uk with esmtp (Exim 4.50) id 1Kvr1C-00006x-Sp for

wigley@cgd.ucar.edu;

Fri, 31 Oct 2008 11:01:46 +0000 X-MimeOLE: Produced By Microsoft Exchange V6.5

Content-class: urn:content-classes:message MIME-Version: 1.0 Content-Type:

multipart/alternative; boundary="-----_=_NextPart_001_01C93B48.10CD099C" Subject:

RE:

Possible error in recent IJC paper Date: Fri, 31 Oct 2008 11:01:46 -0000

Message-ID:

<63675957ADD2DF4D9E246871174BEF1EC901E1@UEAEXCHCLUS01.UEA.AC.UK> X-MS-Has-Attach:

X-MS-TNEF-Correlator: Thread-Topic: Possible error in recent IJC paper

Thread-Index:

Ack7DrU3+LlgMjttS5+lB1r2EiTAKAANYJtF References:

<63675957ADD2DF4D9E246871174BEF1EC901CE@UEAEXCHCLUS01.UEA.AC.UK>

<490A8447.1010603@llnl.gov> From: "Cawley Gavin Dr \ (CMP\)" To: Cc: "Jones Philip

Prof

\ (ENV\)" , "Gavin Schmidt" , "Thorne, Peter" , "Tom Wigley" X-Virus-Scanned:

amavisd-new at

ucar.edu

Dear Ben,

many thanks for the full response to my query. I think my confusion arose from the

discussion on RealClimate (which prompted our earlier communication on this topic), which

mail.2008

clearly suggested that the observed trend should be expected to lie within the spread of the models, rather than necessarily being close to the mean as the models are stochastic simulations (which seemed reasonable). I've just re-read that post, the key paragraph from

[1]<http://www.realclimate.org/index.php/archives/2007/12/tropical-troposphere-trends/> is as

follows:

"The interpretation of this is a little unclear (what exactly does the sigma refer to?), but the most likely interpretation, and the one borne out by looking at their Table IIa, is that sigma is calculated as the standard deviation of the model trends. In that case, the formula given defines the uncertainty on the estimate of the mean - i.e. how well we know what the average trend really is. But it only takes a moment to realise why that is irrelevant. Imagine there were 1000's of simulations drawn from the same distribution, then our estimate of the mean trend would get sharper and sharper as N increased. However, the chances that any one realisation would be within those error bars, would become smaller and smaller. Instead, the key standard deviation is simply sigma itself. That defines the likelihood that one realisation (i.e. the real world) is conceivably drawn from the distribution defined by the models."

I had therefore expected the test to use the standard deviations of both the models and the observations (which would give a flat plot in 5B and there would be an obvious overlap of the uncertainties in 6a at say 500hPa).

best regards

Gavin

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

From: Ben Santer [[2]mailto:santer1@llnl.gov]

Sent: Fri 10/31/2008 4:06 AM

To: Cawley Gavin Dr (CMP)

Cc: Jones Philip Prof (ENV); Gavin Schmidt; Thorne, Peter; Tom Wigley

Subject: Re: Possible error in recent IJC paper

Dear Gavin,

Thanks very much for your email, and for your interest in our recent paper in the International Journal of Climatology (IJoC). There is no error in equation (12) in our IJoC paper. Let me try to answer the questions that you posed.

The first term under the square root in our equation (12) is a standard estimate of the variance of a sample mean - see, e.g., "Statistical Analysis in Climate Research", by Francis Zwiers and Hans von Storch, Cambridge University Press, 1999 (their equation 5.24, page 86). The second term under the square root sign is a very different beast - an estimate of the variance of the observed trend. As we point out, our $d1^*$ test is very similar to a standard Student's t-test of differences in means (which involves, in its denominator, the square root of two pooled sample variances).

In testing the statistical significance of differences between the model average trend and a single observed trend, Douglass et al. were wrong to use σ_{SE} as the sole measure of trend uncertainty in their statistical test. Their test assumes that the model trend is uncertain, but that the observed trend is perfectly-known. The observed trend is

mail.2008

not a "mean" quantity; it is NOT perfectly-known. Douglass et al. made a demonstrably false assumption.

Bottom line: σ_{SE} is a standard estimate of the uncertainty in a sample mean - which is why we use it to characterize uncertainty in the estimate of the model average trend in equation (12). It is NOT appropriate to use σ_{SE} as the basis for a statistical test between two uncertain quantities. The uncertainty in the estimates of both modeled AND observed trend needs to be explicitly incorporated in the design of any statistical test seeking to compare modeled and observed trends. Douglass et al. incorrectly ignored uncertainties in observed trends.

I hope this answers your first question, and explains why there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the comments that we made in point #3 [immediately before equation (12)]. As we note in point #3, "while σ_{SE} is an appropriate measure of how well the multi-model mean trend can be estimated from a finite sample of model results, it is not an appropriate measure for deciding whether this trend is consistent with a single observed trend."

We could perhaps have made point #3 a little clearer by inserting "imperfectly-known" before "observed trend". I thought, however, that the uncertainty in the estimate of the observed trend was already made very clear in our point #1 (on page 7, bottom of column 2).

To answer your second question, $d1^*$ gives a reasonably flat line in Figure 5B because the first term under the square root sign in equation (12) (the variance of the model average trend, which has a dependence on N , the number of models used in the test) is roughly a factor of 20 smaller than the second term under the square root sign (the variance of the observed trend, which has no dependence on N). The behaviour of $d1^*$ with synthetic data is therefore dominated by the second term under the square root sign - which is why the black lines in Figure 5B are flat. In answer to your third question, our Figure 6A provides only one of the components from the denominator of our $d1^*$ test (σ_{SE}). Figure 6A does not show the standard errors in the observed trends at discrete pressure levels. Had we attempted to show the observed standard errors at individual pressure levels, we would have produced a very messy Figure, since Figure 6A shows results from 7 different observational datasets.

We could of course have performed our $d1^*$ test at each discrete pressure level. This would have added another bulky Table to an already lengthy paper. We judged that it was sufficient to perform our $d1^*$ test with the synthetic MSU T2 and T2LT temperature trends calculated from the seven radiosonde datasets and the climate model data. The results of such tests are reported in the final paragraph of Section 7. As we point out, the $d1^*$ test "indicates that the model-average signal trend (for T2LT) is not significantly different (at the 5% level) from the observed signal trends in three of the more recent radiosonde products (RICH, IUK, and RAOBCORE v1.4)." So there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the results displayed in Figure 6.

Thanks again for your interest in our paper, and my apologies for the delay in replying to your email - I have been on travel (and out of email contact) for the past 10 days.

With best regards,

Ben

Cawley Gavin Dr (CMP) wrote:

>

>

> Dear Prof. Santer,

>

> I think there may be a minor problem with equation (12) in your paper
> "Consistency of modelled and observed temperature trends in the tropical
> troposphere", namely that it includes the standard error of the models
> $1/n_m s\{b_m\}^2$ instead of the standard deviation $s\{b_m\}^2$. Firstly

mail.2008

> the current formulation of (12) seems at odds with objection 3 raised at
> the start of the first column of page 8. Secondly, I can't see how the
> modified test $d_{1\wedge}$ gives a flat line in Figure 5B as the test statistic
> is explicitly dependent on the size of the model ensemble n_m . Thirdly,
> the equation seems at odds with the results depicted graphically in
> Figure 6 which would suggest the models are clearly inconsistent at
> higher levels (400-850 hPa) using the confidence interval based on the
> standard error. Lastly, (12) seems at odds with the very lucid
> treatment at RealClimate written by Dr Schmidt.

> I congratulate all 17 authors for an excellent contribution that I have
> found most instructive!

> I do hope I haven't missed something - sorry to have bothered you if
> this is the case.

> best regards

> Gavin

--

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

References

1. <http://www.realclimate.org/index.php/archives/2007/12/tropical-troposphere-trends/>
2. <mailto:santer1@llnl.gov>

933. 1226337052.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: Steve McIntyre <stephen.mcintyre@utoronto.ca>
Subject: Re: FW: Santer et al 2008
Date: Mon, 10 Nov 2008 12:10:52 -0800
Reply-to: santer1@llnl.gov
Cc: "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>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>

<x-flowed>
Dear Mr. McIntyre,

I gather that your intent is to "audit" the findings of our recently-published paper in the International Journal of Climatology

mail.2008

(IJoC). You are of course free to do so. I note that both the gridded model and observational datasets used in our IJoC paper are freely available to researchers. You should have no problem in accessing exactly the same model and observational datasets that we employed. You will need to do a little work in order to calculate synthetic Microwave Sounding Unit (MSU) temperatures from climate model atmospheric temperature information. This should not pose any difficulties for you. Algorithms for calculating synthetic MSU temperatures have been published by ourselves and others in the peer-reviewed literature. You will also need to calculate spatially-averaged temperature changes from the gridded model and observational data. Again, that should not be too taxing.

In summary, you have access to all the raw information that you require in order to determine whether the conclusions reached in our IJoC paper are sound or unsound. I see no reason why I should do your work for you, and provide you with derived quantities (zonal means, synthetic MSU temperatures, etc.) which you can easily compute yourself.

I am copying this email to all co-authors of the 2008 Santer et al. IJoC paper, as well as to Professor Glenn McGregor at IJoC.

I gather that you have appointed yourself as an independent arbiter of the appropriate use of statistical tools in climate research. Rather than "auditing" our paper, you should be directing your attention to the 2007 IJoC paper published by David Douglass et al., which contains an egregious statistical error.

Please do not communicate with me in the future.

Ben Santer

Steve McIntyre wrote:

> Could you please reply to the request below, Regards, Steve McIntyre
>
> -----Original Message-----
> *From:* Steve McIntyre [mailto:stephen.mcintyre@utoronto.ca]
> *Sent:* Monday, October 20, 2008 1:29 PM
> *To:* ' (santer1@llnl.gov)'
> *Subject:* Santer et al 2008
>
> Dear Dr Santer,
>
> Could you please provide me either with the monthly model data (49
> series) used for statistical analysis in Santer et al 2008 or a link to
> a URL. I understand that your version has been collated from PCMDI ; my
> interest is in a file of the data as you used it (I presume that the
> monthly data used for statistics is about 1-2 MB) .
>
> Thank you for your attention,
>
> Steve McIntyre
>
>

--

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>

934. 1226451442.txt

#####

From: Ben Santer <santer1@llnl.gov>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: FOI Request]
Date: Tue, 11 Nov 2008 19:57:22 -0800
Reply-to: santer1@llnl.gov
Cc: Karen Owen <Karen.Owen@noaa.gov>, Sharon Leduc <Sharon.Leduc@noaa.gov>, "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>, "David C. Bader" <bader2@llnl.gov>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, "Bamzai, Anjali" <Anjali.Bamzai@science.doe.gov>

<x-flowed>
Dear Tom,

Thanks for your email regarding Steven McIntyre's twin requests under the Freedom of Information (FOI) Act. Regarding McIntyre's request (1), no "monthly time series of output from any of the 47 climate models" was "sent by Santer and/or other coauthors of Santer et al 2008 to NOAA employees between 2006 and October 2008".

As I pointed out to Mr. McIntyre in the email I transmitted to him yesterday, all of the raw (gridded) model and observational data used in the 2008 Santer et al. International Journal of Climatology (IJOC) paper are freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit us, and determine whether the conclusions reached in our paper are sound, he has all the information necessary to conduct such an audit. Providing Mr. McIntyre with the quantities that I derived from the raw model data (spatially-averaged time series of surface temperatures and synthetic Microwave Sounding Unit [MSU] temperatures) would defeat the very purpose of an audit.

I note that David Douglass and colleagues have already audited our calculation of synthetic MSU temperatures from climate model data. Douglass et al. obtained "model average" trends in synthetic MSU temperatures (published in their 2007 IJOC paper) that are virtually identical to our own.

McIntyre's request (2) demands "any correspondence concerning these monthly time series between Santer and/or other coauthors of Santer et al 2008 and NOAA employees between 2006 and October 2008". I do not know how you intend to respond this second request. You and three other NOAA co-authors on our paper (Susan Solomon, Melissa Free, and John Lanzante) probably received hundreds of emails that I sent to you in the course of our work on the IJOC paper. I note that this work began in December 2007, following online publication of Douglass et al. in the IJOC. I

mail.2008

have no idea why McIntyre's request for email correspondence has a "start date" of 2006, and thus predates publication of Douglass et al.

My personal opinion is that both FOI requests (1) and (2) are intrusive and unreasonable. Steven McIntyre provides absolutely no scientific justification or explanation for such requests. I believe that McIntyre is pursuing a calculated strategy to divert my attention and focus away from research. As the recent experiences of Mike Mann and Phil Jones have shown, this request is the thin edge of wedge. It will be followed by further requests for computer programs, additional material and explanations, etc., etc.

Quite frankly, Tom, having spent nearly 10 months of my life addressing the serious scientific flaws in the Douglass et al. IJoC paper, I am unwilling to waste more of my time fulfilling the intrusive and frivolous requests of Steven McIntyre. The supreme irony is that Mr. McIntyre has focused his attention on our IJoC paper rather than the Douglass et al. IJoC paper which we criticized. As you know, Douglass et al. relied on a seriously flawed statistical test, and reached incorrect conclusions on the basis of that flawed test.

I believe that our community should no longer tolerate the behavior of Mr. McIntyre and his cronies. McIntyre has no interest in improving our scientific understanding of the nature and causes of climate change. He has no interest in rational scientific discourse. He deals in the currency of threats and intimidation. We should be able to conduct our scientific research without constant fear of an "audit" by Steven McIntyre; without having to weigh every word we write in every email we send to our scientific colleagues.

In my opinion, Steven McIntyre is the self-appointed Joe McCarthy of climate science. I am unwilling to submit to this McCarthy-style investigation of my scientific research. As you know, I have refused to send McIntyre the "derived" model data he requests, since all of the primary model data necessary to replicate our results are freely available to him. I will continue to refuse such data requests in the future. Nor will I provide McIntyre with computer programs, email correspondence, etc. I feel very strongly about these issues. We should not be coerced by the scientific equivalent of a playground bully.

I will be consulting LLNL's Legal Affairs Office in order to determine how the DOE and LLNL should respond to any FOI requests that we receive from McIntyre. I assume that such requests will be forthcoming.

I am copying this email to all co-authors of our 2008 IJoC paper, to my immediate superior at PCMDI (Dave Bader), to Anjali Bamzai at DOE headquarters, and to Professor Glenn McGregor (the editor who was in charge of our paper at IJoC).

I'd be very happy to discuss these issues with you tomorrow. I'm sorry that the tone of this letter is so formal, Tom. Unfortunately, after today's events, I must assume that any email I write to you may be subject to FOI requests, and could ultimately appear on McIntyre's "ClimateAudit" website.

with best personal wishes,

Ben

Thomas.R.Karl wrote:

> FYI --- Jolene can you set up a conference call with all the parties
> listed below including Ben.
>

mail.2008

> Thanks

>

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

> Subject: FOI Request

> Date: Mon, 10 Nov 2008 10:02:00 -0500

> From: Steve McIntyre <stephen.mcintyre@utoronto.ca>

> To: FOIA@noaa.gov

> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

>

>

>

> Nov. 10, 2008

>

>

>

> National Oceanic and Atmospheric Administration

>

> Public Reference Facility (OFA56)

>

> Attn: NOAA FOIA Officer

>

> 1315 East West Highway (SSMC3)

>

> Room 10730

>

> Silver Spring, Maryland 20910

>

>

>

> Re: Freedom of Information Act Request

>

>

>

> Dear NOAA FOIA Officer:

>

>

>

> This is a request under the Freedom of Information Act.

>

>

>

> Santer et al, Consistency of modelled and observed temperature trends in

>

> the tropical troposphere, (Int J Climatology, 2008), of which NOAA

> employees J. R. Lanzante, S. Solomon, M. Free and T. R. Karl were

> co-authors, reported on a statistical analysis of the output of 47 runs

> of climate models that had been collated into monthly time series by

> Benjamin Santer and associates.

>

>

>

> I request that a copy of the following NOAA records be provided to me:

> (1) any monthly time series of output from any of the 47 climate models

> sent by Santer and/or other coauthors of Santer et al 2008 to NOAA

> employees between 2006 and October 2008; (2) any correspondence

> concerning these monthly time series between Santer and/or other

> coauthors of Santer et al 2008 and NOAA employees between 2006 and

> October 2008.

>

>

>

> The primary sources for NOAA records are J. R. Lanzante, S. Solomon, M.

> Free and T. R. Karl.

mail.2008

>
>
>
> In order to help to determine my status for purposes of determining the
> applicability of any fees, you should know that I have 5 peer-reviewed
> publications on paleoclimate; that I was a reviewer for WG1; that I made
> a invited presentations in 2006 to the National Research Council Panel
> on Surface Temperature Reconstructions and two presentations to the
> Oversight and Investigations Subcommittee of the House Energy and
> Commerce Committee.
>
>
>
> In addition, a previous FOI request was discussed by the NOAA Science
> Advisory Board's Data Archiving and Access Requirements Working Group
> (DAARWG). [http:// www.
> joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf](http://www.joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf).
>
>
>
> I believe a fee waiver is appropriate since the purpose of the request
> is academic research, the information exists in digital format and the
> information should be easily located by the primary sources.
>
>
>
> I also include a telephone number (416-469-3034) at which I can be
> contacted between 9 and 7 pm Eastern Daylight Time, if necessary, to
> discuss any aspect of my request.
>
>
>
> Thank you for your consideration of this request.
>
>
>
> I ask that the FOI request be processed promptly as NOAA failed to send
> me a response to the FOI request referred to above, for which Dr Karl
> apologized as follows:
>
>
>
> due to a miscommunication between our office and our headquarters, the
> response was not submitted to you. I deeply apologize for this
> oversight, and we have taken measures to ensure this does not happen in
> the future.
>
>
>
>
> Stephen McIntyre
> 25 Playter Blvd
> Toronto, Ont M4K 2W1
>
>
>

--

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>

935. 1226456830.txt

#####

From: Tom Wigley <wigley@ucar.edu>
 To: santer1@llnl.gov
 Subject: Re: [Fwd: FOI Request]
 Date: Tue, 11 Nov 2008 21:27:10 -0700
 Cc: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>, Karen Owen <Karen.Owen@noaa.gov>, Sharon Leduc <Sharon.Leduc@noaa.gov>, "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>, 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>, "David C. Bader" <bader2@llnl.gov>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>

<x-flowed>

Hmmm. I note the following ,,,

"at which I can be contacted between 9 and 7 pm Eastern Daylight Time"

Is this a 22 hour, or, for people with time machine, a negative 2 hour window?

Joking aside, it seems as a matter of principle (albeit a principle yet to be set by the courts) that provision of primary data sources that are sufficient to reproduce the results of a scientific analysis is all that is necessary under FOI.

It also seems that judgment of what correspondence is central to the analysis can only be made by the persons involved. As a participant in many of these inter-author communications, I do not recall any that would give information not already contained in the published paper.

Tom.

+++++

Ben Santer wrote:

> Dear Tom,

>

> Thanks for your email regarding Steven McIntyre's twin requests under

mail.2008

> the Freedom of Information (FOI) Act. Regarding McIntyre's request (1),
> no "monthly time series of output from any of the 47 climate models" was
> "sent by Santer and/or other coauthors of Santer et al 2008 to NOAA
> employees between 2006 and October 2008".

>
> As I pointed out to Mr. McIntyre in the email I transmitted to him
> yesterday, all of the raw (gridded) model and observational data used in
> the 2008 Santer et al. International Journal of Climatology (IJoC) paper
> are freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit
> us, and determine whether the conclusions reached in our paper are
> sound, he has all the information necessary to conduct such an audit.
> Providing Mr. McIntyre with the quantities that I derived from the raw
> model data (spatially-averaged time series of surface temperatures and
> synthetic Microwave Sounding Unit [MSU] temperatures) would defeat the
> very purpose of an audit.

>
> I note that David Douglass and colleagues have already audited our
> calculation of synthetic MSU temperatures from climate model data.
> Douglass et al. obtained "model average" trends in synthetic MSU
> temperatures (published in their 2007 IJoC paper) that are virtually
> identical to our own.

>
> McIntyre's request (2) demands "any correspondence concerning these
> monthly time series between Santer and/or other coauthors of Santer et
> al 2008 and NOAA employees between 2006 and October 2008". I do not know
> how you intend to respond to this second request. You and three other NOAA
> co-authors on our paper (Susan Solomon, Melissa Free, and John Lanzante)
> probably received hundreds of emails that I sent to you in the course of
> our work on the IJoC paper. I note that this work began in December
> 2007, following online publication of Douglass et al. in the IJoC. I
> have no idea why McIntyre's request for email correspondence has a
> "start date" of 2006, and thus predates publication of Douglass et al.

>
> My personal opinion is that both FOI requests (1) and (2) are intrusive
> and unreasonable. Steven McIntyre provides absolutely no scientific
> justification or explanation for such requests. I believe that McIntyre
> is pursuing a calculated strategy to divert my attention and focus away
> from research. As the recent experiences of Mike Mann and Phil Jones
> have shown, this request is the thin edge of wedge. It will be followed
> by further requests for computer programs, additional material and
> explanations, etc., etc.

>
> Quite frankly, Tom, having spent nearly 10 months of my life addressing
> the serious scientific flaws in the Douglass et al. IJoC paper, I am
> unwilling to waste more of my time fulfilling the intrusive and
> frivolous requests of Steven McIntyre. The supreme irony is that Mr.
> McIntyre has focused his attention on our IJoC paper rather than the
> Douglass et al. IJoC paper which we criticized. As you know, Douglass et
> al. relied on a seriously flawed statistical test, and reached incorrect
> conclusions on the basis of that flawed test.

>
> I believe that our community should no longer tolerate the behavior of
> Mr. McIntyre and his cronies. McIntyre has no interest in improving our
> scientific understanding of the nature and causes of climate change. He
> has no interest in rational scientific discourse. He deals in the
> currency of threats and intimidation. We should be able to conduct our
> scientific research without constant fear of an "audit" by Steven
> McIntyre; without having to weigh every word we write in every email we
> send to our scientific colleagues.

>
> In my opinion, Steven McIntyre is the self-appointed Joe McCarthy of
> climate science. I am unwilling to submit to this McCarthy-style
> investigation of my scientific research. As you know, I have refused to

mail.2008

> send McIntyre the "derived" model data he requests, since all of the
> primary model data necessary to replicate our results are freely
> available to him. I will continue to refuse such data requests in the
> future. Nor will I provide McIntyre with computer programs, email
> correspondence, etc. I feel very strongly about these issues. We should
> not be coerced by the scientific equivalent of a playground bully.

>
> I will be consulting LLNL's Legal Affairs Office in order to determine
> how the DOE and LLNL should respond to any FOI requests that we receive
> from McIntyre. I assume that such requests will be forthcoming.

>
> I am copying this email to all co-authors of our 2008 IJoC paper, to my
> immediate superior at PCMDI (Dave Bader), to Anjali Bamzai at DOE
> headquarters, and to Professor Glenn McGregor (the editor who was in
> charge of our paper at IJoC).

>
> I'd be very happy to discuss these issues with you tomorrow. I'm sorry
> that the tone of this letter is so formal, Tom. Unfortunately, after
> today's events, I must assume that any email I write to you may be
> subject to FOI requests, and could ultimately appear on McIntyre's
> "ClimateAudit" website.

>
> With best personal wishes,

>
> Ben

>
> Thomas.R.Karl wrote:

>> FYI --- Jolene can you set up a conference call with all the parties
>> listed below including Ben.

>>
>> Thanks

>>
>> ----- Original Message -----
>> Subject: FOI Request
>> Date: Mon, 10 Nov 2008 10:02:00 -0500
>> From: Steve McIntyre <stephen.mcintyre@utoronto.ca>
>> To: FOIA@noaa.gov
>> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

>>
>>
>>
>> Nov. 10, 2008

>>
>>
>> National Oceanic and Atmospheric Administration
>> Public Reference Facility (OFA56)
>> Attn: NOAA FOIA Officer
>>
>> 1315 East West Highway (SSMC3)
>>
>> Room 10730
>>
>> Silver Spring, Maryland 20910

>>
>>
>> Re: Freedom of Information Act Request

>>
>>
>>

>> Dear NOAA FOIA Officer:

>>
>>
>>

>> This is a request under the Freedom of Information Act.

>>
>>

>> Santer et al, Consistency of modelled and observed temperature trends in
>> the tropical troposphere, (Int J Climatology, 2008), of which NOAA
>> employees J. R. Lanzante, S. Solomon, M. Free and T. R. Karl were
>> co-authors, reported on a statistical analysis of the output of 47
>> runs of climate models that had been collated into monthly time series
>> by Benjamin Santer and associates.

>>
>>

>> I request that a copy of the following NOAA records be provided to me:
>> (1) any monthly time series of output from any of the 47 climate
>> models sent by Santer and/or other coauthors of Santer et al 2008 to
>> NOAA employees between 2006 and October 2008; (2) any correspondence
>> concerning these monthly time series between Santer and/or other
>> coauthors of Santer et al 2008 and NOAA employees between 2006 and
>> October 2008.

>>
>>

>> The primary sources for NOAA records are J. R. Lanzante, S. Solomon,
>> M. Free and T. R. Karl.

>>
>>

>> In order to help to determine my status for purposes of determining
>> the applicability of any fees, you should know that I have 5
>> peer-reviewed publications on paleoclimate; that I was a reviewer for
>> WG1; that I made a invited presentations in 2006 to the National
>> Research Council Panel on Surface Temperature Reconstructions and two
>> presentations to the Oversight and Investigations Subcommittee of the
>> House Energy and Commerce Committee.

>>
>>

>> In addition, a previous FOI request was discussed by the NOAA Science
>> Advisory Board's Data Archiving and Access Requirements Working Group
>> (DAARWG). [http:// www.
>> joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf](http://www.joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf).

>>
>>
>>
>>

>> I believe a fee waiver is appropriate since the purpose of the request
>> is academic research, the information exists in digital format and the
>> information should be easily located by the primary sources.

>>
>>

>> I also include a telephone number (416-469-3034) at which I can be
>> contacted between 9 and 7 pm Eastern Daylight Time, if necessary, to
>> discuss any aspect of my request.

>>
>>

mail.2008

>>
 >> Thank you for your consideration of this request.
 >>
 >>
 >> I ask that the FOI request be processed promptly as NOAA failed to
 >> send me a response to the FOI request referred to above, for which Dr
 >> Karl apologized as follows:
 >>
 >>
 >> due to a miscommunication between our office and our headquarters, the
 >> response was not submitted to you. I deeply apologize for this
 >> oversight, and we have taken measures to ensure this does not happen
 >> in the future.
 >>
 >>
 >>
 >> Stephen McIntyre
 >>
 >> 25 Playter Blvd
 >>
 >> Toronto, Ont M4K 2W1
 >>
 >>
 >>
 >
 >

</x-flowed>

936. 1226500291.txt

 #####

From: Phil Jones <p.jones@uea.ac.uk>
 To: santer1@llnl.gov
 Subject: Re: [Fwd: FOI Request]
 Date: Wed Nov 12 09:31:31 2008

Ben,

Another point to discuss when you have your conference call - is why don't they ask Douglass for all his data. It is essentially the same. You can also think of all this positively - they think a few of us do really important work, so they concentrate on what they think are the cutting edge pieces of work.

I have a big review on paleo coming out soon in The Holocene - with 20+ others.

won't be out till next year, but I can say for certain that it will feature strongly on

CA. Not too much they can request via FOI, but they will think of something. This

paper will explain where a Figure came from in the First IPCC Report - the infamous

one that Chris Folland put together on the last 1000 years. CA will say they found this out

- they had

a thread on it 9 months ago according to Gavin. I have the submission date of

mail.2008

the article
and more detail though - to show we found out first.
Cheers
Phil

At 03:57 12/11/2008, you wrote:

Dear Tom,
Thanks for your email regarding Steven McIntyre's twin requests under the Freedom of Information (FOI) Act. Regarding McIntyre's request (1), no "monthly time series of output from any of the 47 climate models" was "sent by Santer and/or other coauthors of Santer et al 2008 to NOAA employees between 2006 and October 2008". As I pointed out to Mr. McIntyre in the email I transmitted to him yesterday, all of the raw (gridded) model and observational data used in the 2008 Santer et al. International Journal of Climatology (IJoC) paper are freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit us, and determine whether the conclusions reached in our paper are sound, he has all the information necessary to conduct such an audit. Providing Mr. McIntyre with the quantities that I derived from the raw model data (spatially-averaged time series of surface temperatures and synthetic Microwave Sounding Unit [MSU] temperatures) would defeat the very purpose of an audit. I note that David Douglass and colleagues have already audited our calculation of synthetic MSU temperatures from climate model data. Douglass et al. obtained "model average" trends in synthetic MSU temperatures (published in their 2007 IJoC paper) that are virtually identical to our own. McIntyre's request (2) demands "any correspondence concerning these monthly time series between Santer and/or other coauthors of Santer et al 2008 and NOAA employees between 2006 and October 2008". I do not know how you intend to respond this second request. You and three other NOAA co-authors on our paper (Susan Solomon, Melissa Free, and John Lanzante) probably received hundreds of emails that I sent to you in the course of our work on the IJoC paper. I note that this work began in December 2007, following online publication of Douglass et al. in the IJoC. I have no idea why McIntyre's request for email correspondence has a "start date" of 2006, and thus predates publication of Douglass et al. My personal opinion is that both FOI requests (1) and (2) are intrusive and unreasonable. Steven McIntyre provides absolutely no scientific justification or explanation for such requests. I believe that McIntyre is pursuing a calculated strategy to divert my attention and focus away from research. As the recent experiences of Mike Mann and Phil Jones have shown, this request is the thin edge of wedge. It will be followed by further requests for computer programs, additional material and explanations, etc., etc.

mail.2008

Quite frankly, Tom, having spent nearly 10 months of my life addressing the serious scientific flaws in the Douglass et al. IJoC paper, I am unwilling to waste more of my time fulfilling the intrusive and frivolous requests of Steven McIntyre. The supreme irony is that Mr. McIntyre has focused his attention on our IJoC paper rather than the Douglass et al. IJoC paper which we criticized. As you know, Douglass et al. relied on a seriously flawed statistical test, and reached incorrect conclusions on the basis of that flawed test.

I believe that our community should no longer tolerate the behavior of Mr. McIntyre and his cronies. McIntyre has no interest in improving our scientific understanding of the nature and causes of climate change. He has no interest in rational scientific discourse. He deals in the currency of threats and intimidation. We should be able to conduct our scientific research without constant fear of an "audit" by Steven McIntyre; without having to weigh every word we write in every email we send to our scientific colleagues.

In my opinion, Steven McIntyre is the self-appointed Joe McCarthy of climate science. I am unwilling to submit to this McCarthy-style investigation of my scientific research.

As you know, I have refused to send McIntyre the "derived" model data he requests, since all of the primary model data necessary to replicate our results are freely available to him. I will continue to refuse such data requests in the future. Nor will I provide McIntyre with computer programs, email correspondence, etc. I feel very strongly about these issues. We should not be coerced by the scientific equivalent of a playground bully.

I will be consulting LLNL's Legal Affairs Office in order to determine how the DOE and LLNL should respond to any FOI requests that we receive from McIntyre. I assume that such requests will be forthcoming.

I am copying this email to all co-authors of our 2008 IJoC paper, to my immediate superior at PCMDI (Dave Bader), to Anjuli Bamzai at DOE headquarters, and to Professor Glenn McGregor (the editor who was in charge of our paper at IJoC).

I'd be very happy to discuss these issues with you tomorrow. I'm sorry that the tone of this letter is so formal, Tom. Unfortunately, after today's events, I must assume that any email I write to you may be subject to FOI requests, and could ultimately appear on McIntyre's "ClimateAudit" website.

With best personal wishes,
Ben
Thomas.R.Karl wrote:

FYI --- Jolene can you set up a conference call with all the parties listed below

mail.2008

including Ben.
Thanks

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

Subject: FOI Request
Date: Mon, 10 Nov 2008 10:02:00 -0500
From: Steve McIntyre <stephen.mcintyre@utoronto.ca>
To: FOIA@noaa.gov
CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>
Nov. 10, 2008

National Oceanic and Atmospheric Administration
Public Reference Facility (OFA56)
Attn: NOAA FOIA Officer
1315 East West Highway (SSMC3)
Room 10730
Silver Spring, Maryland 20910

Re: Freedom of Information Act Request

Dear NOAA FOIA Officer:

This is a request under the Freedom of Information Act.

R. Santer et al, Consistency of modelled and observed temperature trends in the tropical troposphere, (Int J Climatology, 2008), of which NOAA employees J. Lanzante, S. Solomon, M. Free and T. R. Karl were co-authors, reported on a statistical analysis of the output of 47 runs of climate models that had been collated into monthly time series by Benjamin Santer and associates.

I request that a copy of the following NOAA records be provided to me: (1) any monthly time series of output from any of the 47 climate models sent by Santer and/or other coauthors of Santer et al 2008 to NOAA employees between 2006 and October 2008; (2) any correspondence concerning these monthly time series between Santer and/or other coauthors of Santer et al 2008 and NOAA employees between 2006 and October 2008.

The primary sources for NOAA records are J. R. Lanzante, S. Solomon, M. Free and T. R. Karl.

In order to help to determine my status for purposes of determining the applicability of any fees, you should know that I have 5 peer-reviewed publications on paleoclimate; that I was a reviewer for WG1; that I made a invited presentations in 2006 to the National Research Council Panel on Surface Temperature Reconstructions and two presentations to the Oversight and Investigations Subcommittee of the House Energy and Commerce Committee.

In addition, a previous FOI request was discussed by the NOAA Science Advisory Boards Data Archiving and Access Requirements Working Group (DAARWG). [1][http:// www.joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf](http://www.joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf).

mail.2008

I believe a fee waiver is appropriate since the purpose of the request is academic research, the information exists in digital format and the information should be easily located by the primary sources.

I also include a telephone number (416-469-3034) at which I can be contacted between 9 and 7 pm Eastern Daylight Time, if necessary, to discuss any aspect of my request.

Thank you for your consideration of this request.

I ask that the FOI request be processed promptly as NOAA failed to send me a response to the FOI request referred to above, for which Dr Karl apologized as follows:

due to a miscommunication between our office and our headquarters, the response was not submitted to you. I deeply apologize for this oversight, and we have taken measures to ensure this does not happen in the future.

Stephen McIntyre
25 Playter Blvd
Toronto, Ont M4K 2W1

--

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

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://>

937. 1226959467.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: GHCN

Date: Mon Nov 17 17:04:27 2008

Gavin,

First the figures are just for you - don't pass on!!! I don't normally see these. I just asked my MOHC contact - and he's seen the furore on the blogs. why did the Daily Telegraph run with the story - it's all back to their readers thinking the UK is run by another country!

These 3 paras (below) are from the GHCN web site. They appear to be the only mention

I can see of the WMO CLIMAT network on a web site. The rigorous QC that is being talked about is

done in retrospect. They don't do much in real time - except an outlier check.

Anyway - the CLIMAT network is part of the GTS. The members (NMSS) send their monthly averages/total around the other NMSS on the 4th and the 18-20th of the month afterwards. Few seem to adhere to these dates much these days, but the aim is to send the data around twice in the following month. Data comes in code like everything else on the GTS, so a few centres (probably a handful,

NOAA/CPC,

MOHC, MeteoFrance, DWD, Roshydromet, CMA, JMA and the Australians)

that are doing analyses for weather forecasts have the software to pick out the CLIMAT data and put it somewhere.

At the same time these same centres are taking the synop data off the system and summing it to months - producing flags of how much was missing. At the MOHC they compare the CLIMAT message with the monthly calculated average/total. If they are close they accept the CLIMAT. Some countries don't use the mean of max and min (which the synops provide) to calculate the mean, so it is important to use the CLIMAT as this is likely to ensure continuity. If they don't agree

they

check the flags and there needs to be a bit of human intervention. The figures are examples for this October.

what often happens is that countries send out the same data for the following month.

This happens mostly in developing countries, as a few haven't yet got software to

produce the CLIMAT data in the correct format. There is WMO software to produce these from a wide variety of possible formats the countries might be using.

Some seem to do this by overwriting the files from the previous month. They add in the correct data, but then forget to save the revised file. Canada did this a few years ago - but they sent the correct data around a day later and

again

the second time, after they got told by someone at MOHC.

My guess here is that NOAA didn't screw up, but that Russia did. For all countries

except Russia, all data for that country comes out together. For Russia it comes out in regions - well it is a big place! Trying to prove this would need some

Russian

help - Pasha Groisman? - but there isn't much point. The fact that all the

affected

data were from one Russian region suggests to me it was that region.

Probably not of much use to an FAQ!

Cheers

Phil

The Global Historical Climatology Network (GHCN-Monthly) data base contains historical

temperature, precipitation, and pressure data for thousands of land stations worldwide. The

period of record varies from station to station, with several thousand extending back to

1950 and several hundred being updated monthly via CLIMAT reports. The data are available

mail.2008

without charge through NCDCs anonymous FTP service.

Both historical and near-real-time GHCN data undergo rigorous quality assurance reviews.

These reviews include preprocessing checks on source data, time series checks that identify spurious changes in the mean and variance, spatial comparisons that verify the accuracy of the climatological mean and the seasonal cycle, and neighbor checks that identify outliers

from both a serial and a spatial perspective.

GHCN-Monthly is used operationally by NCDC to monitor long-term trends in temperature and

precipitation. It has also been employed in several international climate assessments,

including the Intergovernmental Panel on Climate Change 4th Assessment Report, the Arctic

Climate Impact Assessment, and the "State of the Climate" report published annually by the

Bulletin of the American Meteorological Society.

At 12:56 17/11/2008, you wrote:

thanks.

Actually, I don't think that many people have any idea how the NWS's send out data, what data they send out, what they don't and how these things are collated. Perhaps you'd like to send me some notes on this that I could write up as a FAQ? Won't change anything much, but it would be a handy reference....

gavin

On Mon, 2008-11-17 at 07:53, Phil Jones wrote:

> > Gavin,

> I may be getting touchy but the CA thread on the HadCRUT October 08 data seems full of snidey comments. Nice to see that they have very little right. Where have they got the idea that the data each month come from GHCN? There are the daily synops and the CLIMAT messages - nothing to do with GHCN. All they have to do is read Brohan et al (2006) and they can see this - and how we merge the land and marine! They seem to have no idea about the Global Telecommunications System. Anyway - expecting the proofs of the wengen paper any day now. Have already sent back loads of updated references and sorted out almost

all

> of the other reference problems.

> When the paper comes out - not sure if The Holocene do online first - happy for you to point out the publication dates (date first received etc) when

> they scream that they sorted out that diagram from the first IPCC Report.

> Don't know how you find the time to do all this responding- keep it up!

> Cheers

> Phil

>

>

>

>

> Prof. Phil Jones

> Climatic Research Unit Telephone +44 (0) 1603 592090

> School of Environmental Sciences Fax +44 (0) 1603 507784

> University of East Anglia

> Norwich

Email p.jones@uea.ac.uk

> NR4 7TJ

> UK

>

mail.2008

>

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

938. 1228249747.txt

#####

From: wigley@ucar.edu
To: santer1@llnl.gov
Subject: Re: Further fallout from our IJoC paper
Date: Tue, 2 Dec 2008 15:29:07 -0700 (MST)
Cc: santer1@llnl.gov, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, peter.thorne@noaa.gov, "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@noaa.gov, "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>

Ben,

I support you on this. However, there is more to be said than what you give below. For instance, it would be useful to note that, in principle, an audit scheme could be a good thing if done properly. But an audit must start at square one (your point). So, one can appear to applaud McIntyre at first, but then go on to note that his modus operandi seems to be flawed.

In this case, as you have noted before, if Mc could not get the data from us, then he could have got it from Douglass. Given this, it is strange to keep hounding us. This would, of course, raise the issue of whether the Douglass data are the same as ours (and/or the same as in CCSP 1.1). I'm not sure whether Douglass et al. actually state that their data are the same as CCSP 1.1, but it would be good if they did -- because our IJoC data are the same as CCSP 1.1.

Mc could say that Douglass already effectively audited our calculations from the raw data, which is why he does not want to/need to repeat this step. But if he does say this then why not get the data from Douglass?

Have a go at writing something -- but try to pre-empt any come back from Mc or others. Also, don't just consider our case, but put it as an example of more general issues.

The issue of auditing is a tricky one. The auditors must, themselves, be able to demonstrate that they have no ulterior motives. One way to do this would be to audit papers on both sides of an issue. In other words, both us and Douglass should be audited together. In a sense, our paper is an audit of Douglass -- and we found his work to be flawed. A second opinion on this already exists, through the

mail.2008

refereeing of our paper. I suppose a third opinion from the likes of Mc might be of value in a controversial area like this. But then, is Mc the right person to do this? Is he unbiased? Does he have the right credentials (as a statistician)?

One could argue that IPCC had an auditing system in place. This is partly through the multiple levels of review -- but doesn't each chapter have another person(s) to sign off on the responses to review comments?

There are some interesting general issues here.

Tom.

+++++

I'm happy to co-author anything you write.

> Dear folks,

>

> There has been some additional fallout from the publication of our paper
> in the International Journal of Climatology. After reading Steven
> McIntyre's discussion of our paper on climateaudit.com (and reading
> about my failure to provide McIntyre with the data he requested), an
> official at DOE headquarters has written to Cherry Murray at LLNL,
> claiming that my behavior is bringing LLNL's good name into disrepute.
> Cherry is the Principal Associate Director for Science and Technology at
> LLNL, and reports to LLNL's Director (George Miller).

>

> I'm getting sick of this kind of stuff, and am tired of simply taking it
> on the chin.

>

> Accordingly, I have been trying to evaluate my options. I believe that
> one option is to write a letter to Nature, briefly outlining some of the
> events that have transpired subsequent to the publication of our IJoC
> paper. Nature would be a logical choice for such a letter, since they
> published a brief account of our findings in their "Research Highlights"
> section. The letter would provide some public record of my position
> regarding McIntyre's data request, and would note that:

>

> "all of the raw (gridded) model and observational data used in the 2008
> Santer et al. International Journal of Climatology (IJoC) paper are
> freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit us,
> and determine whether the conclusions reached in our paper are sound, he
> has all the information necessary to conduct such an audit. Providing
> Mr. McIntyre with the quantities that I derived from the raw model data
> (spatially-averaged time series of surface temperatures and synthetic
> Microwave Sounding Unit [MSU] temperatures) would defeat the very
> purpose of an audit." (email from Ben Santer to Tom Karl, Nov. 11, 2008).

>

> I think that some form of public record would be helpful, particularly
> if LLNL management continues to receive emails alleging that my behavior
> is tarnishing LLNL's scientific reputation.

>

> Since it was my decision not to provide McIntyre with derived quantities
> (synthetic MSU temperatures), I'm perfectly happy to be the sole author
> of such a letter to Nature.

>

> Your thoughts or advice in this matter would be much appreciated.

>

> with best regards,

>

> Ben

>

mail.2008

> 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
> -----
>

939. 1228258714.txt

#####

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
To: santer1@llnl.gov
Subject: Re: Further fallout from our IJoC paper
Date: 02 Dec 2008 17:58:34 -0500
Cc: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, 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@noaa.gov, 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>, Steve Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

Ben, there are two very different things going on here. One is technical and related to the actual science and the actual statistics, the second is political, and is much more concerned with how incidents like this can be portrayed. The second is the issue here.

The unfortunate fact is that the 'secret science' meme is an extremely powerful rallying call to people who have no idea about what is going on. Claiming (rightly or wrongly) that information is being hidden has a huge amount of resonance (as you know), much more so than whether Douglass et al know their statistical elbow from a hole in the ground.

Thus any increase in publicity on this - whether in the pages of Nature or elsewhere - is much more likely to bring further negative fallout despite your desire to clear the air. whatever you say, it will still be presented as you hiding data.

The contrarians have found that there is actually no limit to what you can ask people for (raw data, intermediate steps, additional calculations, residuals, sensitivity calculations, all the code, a workable version of the code on any platform etc.), and like Somali pirates they have found that once someone has paid up, they can always shake them down again.

Thus, I would not advise any public statements on this. Instead, email you immediate superiors and the director with a short statement along the lines of what you suggest below (i.e. of course you want open science, the data *are* in the public domain (with links) and calls for more intermediate steps are just harassment to prevent scientists doing what they are actually paid too). I wouldn't put in anything specifically related to McIntyre.

A much more satisfying response would be to demonstrate how easy it is to replicate the analysis in the paper starting from scratch using

mail.2008

openly available data (such as through Joe Sirott's portal) and the simplest published MSU weighting function. If you can show that this can be done in a couple of hours (or whatever), it makes the other side look like incompetent amateurs. Maybe someone has a graduate student available....?

Gavin

On Tue, 2008-12-02 at 15:52, Ben Santer wrote:

> Dear folks,
>
> There has been some additional fallout from the publication of our paper
> in the International Journal of Climatology. After reading Steven
> McIntyre's discussion of our paper on climateaudit.com (and reading
> about my failure to provide McIntyre with the data he requested), an
> official at DOE headquarters has written to Cherry Murray at LLNL,
> claiming that my behavior is bringing LLNL's good name into disrepute.
> Cherry is the Principal Associate Director for Science and Technology at
> LLNL, and reports to LLNL's Director (George Miller).

> I'm getting sick of this kind of stuff, and am tired of simply taking it
> on the chin.

> Accordingly, I have been trying to evaluate my options. I believe that
> one option is to write a letter to Nature, briefly outlining some of the
> events that have transpired subsequent to the publication of our IJoC
> paper. Nature would be a logical choice for such a letter, since they
> published a brief account of our findings in their "Research Highlights"
> section. The letter would provide some public record of my position
> regarding McIntyre's data request, and would note that:

> "all of the raw (gridded) model and observational data used in the 2008
> Santer et al. International Journal of Climatology (IJoC) paper are
> freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit us,
> and determine whether the conclusions reached in our paper are sound, he
> has all the information necessary to conduct such an audit. Providing
> Mr. McIntyre with the quantities that I derived from the raw model data
> (spatially-averaged time series of surface temperatures and synthetic
> Microwave Sounding Unit [MSU] temperatures) would defeat the very
> purpose of an audit." (email from Ben Santer to Tom Karl, Nov. 11, 2008).

> I think that some form of public record would be helpful, particularly
> if LLNL management continues to receive emails alleging that my behavior
> is tarnishing LLNL's scientific reputation.

> Since it was my decision not to provide McIntyre with derived quantities
> (synthetic MSU temperatures), I'm perfectly happy to be the sole author
> of such a letter to Nature.

> Your thoughts or advice in this matter would be much appreciated.

> 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

> -----
>

940. 1228330629.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov, Tom Wigley <wigley@ucar.edu>
Subject: Re: Schles suggestion
Date: wed Dec 3 13:57:09 2008
Cc: mann <mann@psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Karl Taylor <taylor13@llnl.gov>, peter gleckler <gleckler1@llnl.gov>

Ben,
when the FOI requests began here, the FOI person said we had to abide by the requests. It took a couple of half hour sessions - one at a screen, to convince them otherwise showing them what CA was all about. Once they became aware of the types of people we were dealing with, everyone at UEA (in the registry and in the Environmental Sciences school - the head of school and a few others) became very supportive. I've got to know the FOI person quite well and the Chief Librarian - who deals with appeals. The VC is also aware of what is going on - at least for one of the requests, but probably doesn't know the number we're dealing with. We are in double figures.

One issue is that these requests aren't that widely known within the school. So I don't know who else at UEA may be getting them. CRU is moving up the ladder of requests at UEA though - we're way behind computing though. We're away of requests going to others in the UK - MOHC, Reading, DEFRA and Imperial College. So spelling out all the detail to the LLNL management should be the first thing

you do. I hope that Dave is being supportive at PCMDI. The inadvertent email I sent last month has led to a Data Protection Act request sent by a certain Canadian, saying that the email maligned his scientific credibility with his peers!

If he pays 10 pounds (which he hasn't yet) I am supposed to go through my emails and he can get anything I've written about him. About 2 months ago I deleted loads of emails, so have very little - if anything at all. This legislation is different from the

FOI - it is supposed to be used to find out why you might have a poor credit rating ! In response to FOI and EIR requests, we've put up some data - mainly paleo data.

Each request generally leads to more - to explain what we've put up. Every time, so far, that hasn't led to anything being added - instead just statements saying read what is in the papers and what is on the web site! Tim Osborn sent one such response (via the FOI person) earlier this week. We've never sent programs, any codes and manuals.

In the UK, the Research Assessment Exercise results will be out in 2 weeks

mail.2008

time.

These are expensive to produce and take too much time, so from next year we'll be moving onto a metric based system. The metrics will be # and amounts of grants, papers and citations etc. I did flippantly suggest that the # of FOI requests you get should be another.

when you look at CA, they only look papers from a handful of people. They will start on another coming out in The Holocene early next year. Gavin and Mike are on this with loads of others. I've told both exactly what will appear on

CA once they get access to it!

Cheers

Phil

At 01:17 03/12/2008, Ben Santer wrote:

Dear Tom,

I think that the idea of a Commentary in Science or Nature is a good one. Steve Sherwood

made a similar suggestion. I'd be perfectly happy NOT to be involved in such a Commentary. My involvement would look too self-serving.

One of the problems is that I'm caught in a real Catch-22 situation. At present, I'm

damned and publicly vilified because I refused to provide McIntyre with the data he

requested. But had I acceded to McIntyre's initial request for climate model data, I'm

convinced (based on the past experiences of Mike Mann, Phil, and Gavin) that I would

have spent years of my scientific career dealing with demands for further explanations,

additional data, Fortran code, etc. (Phil has been complying with FOIA requests from

McIntyre and his cronies for over two years). And if I ever denied a single request for

further information, McIntyre would have rubbed his hands gleefully and written: "You

see - he's guilty as charged!" on his website.

You and I have spent over a decade of our scientific careers on the MSU issue, Tom.

During much of that time, we've had to do science in "reactive mode", responding to the

latest outrageous claims and inept science by John Christy, David Douglass, or S. Fred

Singer. For the remainder of my scientific career, I'd like to dictate my own research

agenda. I don't want that agenda driven by the constant need to respond to Christy,

Douglass, and Singer. And I certainly don't want to spend years of my life interacting

with the likes of Steven McIntyre.

I hope LLNL management will provide me with their full support. If they do not, I'm

fully prepared to seek employment elsewhere.

with best regards,

Ben

Tom Wigley wrote:

Ben,

Re the idea Michael sent around (to Revkin et al.)

this is something that Nature or Science might like

as a Commentary. It might even be possible to include

mail.2008

some indirect reference to the Mc audit issue. The notes I sent could be a starting point. One problem is that you could not be first author as this would look like garnering publicity for your own work (as the 2 key papers are both Santer et al.) Even having me as the first author may not work. An ideal person would be Tom Karl, who sent me a response saying "nice summary".

What do you think?

Tom.

--

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

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

941. 1228412429.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: wigley@ucar.edu
Subject: Re: Schles suggestion
Date: Thu Dec 4 12:40:29 2008

Tom,

Obviously don't pass on! These proofs have gone back with about 60 changes to be made. Should be out first issue of 2009.

The bet is that CA will say they found that the IPCC Figure from 1990 was a Lamb diagram 6 months ago. They did, but they didn't get the right source, and our paper was submitted in early 2008. CA will also comment on the section on pp21-31. The summary of where we are with the individual proxies is useful for most of them - but we didn't get anyone working with speleothems involved. I remain unconvinced they get the resolution claimed. Yet to see a speleothem paper which doesn't compare their (individual site) reconstruction

with

either the MBH series or a solar proxy.

I hope Ben gets the support from PCMDI and LLNL.

Cheers
Phil
Cheers
Phil

At 22:33 03/12/2008, you wrote:

Phil,

mail.2008

Thanks for all the information on the GISS etc. data.
Re below -- can you send me a preprint of the Holocene
paper.

Tom.

+++++

>
> Ben,
> When the FOI requests began here, the FOI person said we had to abide
> by the requests. It took a couple of half hour sessions - one at a
> screen, to convince them otherwise
> showing them what CA was all about. Once they became aware of the
> types of people we were
> dealing with, everyone at UEA (in the registry and in the
> Environmental Sciences school
> - the head of school and a few others) became very supportive. I've
> got to know the FOI
> person quite well and the Chief Librarian - who deals with appeals.
> The VC is also
> aware of what is going on - at least for one of the requests, but
> probably doesn't know
> the number we're dealing with. We are in double figures.

>
> One issue is that these requests aren't that widely known within
> the School. So
> I don't know who else at UEA may be getting them. CRU is moving up
> the ladder of
> requests at UEA though - we're way behind computing though. We're away
> of
> requests going to others in the UK - MOHC, Reading, DEFRA and
> Imperial College.

>
> So spelling out all the detail to the LLNL management should be
> the first thing
> you do. I hope that Dave is being supportive at PCMDI.

>
> The inadvertent email I sent last month has led to a Data
> Protection Act request sent by
> a certain Canadian, saying that the email maligned his scientific
> credibility with his peers!
> If he pays 10 pounds (which he hasn't yet) I am supposed to go
> through my emails
> and he can get anything I've written about him. About 2 months ago
> I deleted loads of
> emails, so have very little - if anything at all. This legislation
> is different from the FOI -
> it is supposed to be used to find out why you might have a poor
> credit rating !

>
> In response to FOI and EIR requests, we've put up some data -
> mainly paleo data.
> Each request generally leads to more - to explain what we've put
> up. Every time, so
> far, that hasn't led to anything being added - instead just
> statements saying read
> what is in the papers and what is on the web site! Tim Osborn sent one
> such
> response (via the FOI person) earlier this week. We've never sent
> programs, any codes
> and manuals.

>
> In the UK, the Research Assessment Exercise results will be out
> in 2 weeks time.
> These are expensive to produce and take too much time, so from next

mail.2008

> year we'll
> be moving onto a metric based system. The metrics will be # and
> amounts of grants,
> papers and citations etc. I did flippantly suggest that the # of
> FOI requests you get
> should be another.
>
> when you look at CA, they only look papers from a handful of
> people. They will start on another coming out in The Holocene early
> next year. Gavin
> and Mike are on this with loads of others. I've told both exactly
> what will appear on
> CA once they get access to it!
>
> Cheers
> Phil
>
>

> At 01:17 03/12/2008, Ben Santer wrote:

>>Dear Tom,

>>

>>I think that the idea of a Commentary in Science or Nature is a good
>>one. Steve Sherwood made a similar suggestion. I'd be perfectly
>>happy NOT to be involved in such a Commentary. My involvement would
>>look too self-serving.

>>

>>One of the problems is that I'm caught in a real Catch-22 situation.
>>At present, I'm damned and publicly vilified because I refused to
>>provide McIntyre with the data he requested. But had I acceded to
>>McIntyre's initial request for climate model data, I'm convinced
>>(based on the past experiences of Mike Mann, Phil, and Gavin) that I
>>would have spent years of my scientific career dealing with demands
>>for further explanations, additional data, Fortran code, etc. (Phil
>>has been complying with FOIA requests from McIntyre and his cronies
>>for over two years). And if I ever denied a single request for
>>further information, McIntyre would have rubbed his hands gleefully
>>and written: "You see - he's guilty as charged!" on his website.

>>

>>You and I have spent over a decade of our scientific careers on the
>>MSU issue, Tom. During much of that time, we've had to do science in
>>"reactive mode", responding to the latest outrageous claims and
>>inept science by John Christy, David Douglass, or S. Fred Singer.
>>For the remainder of my scientific career, I'd like to dictate my
>>own research agenda. I don't want that agenda driven by the constant
>>need to respond to Christy, Douglass, and Singer. And I certainly
>>don't want to spend years of my life interacting with the likes of
>>Steven McIntyre.

>>

>>I hope LLNL management will provide me with their full support. If
>>they do not, I'm fully prepared to seek employment elsewhere.

>>

>>With best regards,

>>

>>Ben

>>

>>Tom Wigley wrote:

>>>Ben,

>>>Re the idea Michael sent around (to Revkin et al.)
>>>this is something that Nature or Science might like
>>>as a Commentary. It might even be possible to include
>>>some indirect reference to the Mc audit issue. The
>>>notes I sent could be a starting point. One problem
>>>is that you could not be first author as this would

mail.2008

>>>look like garnering publicity for your own work (as
>>>the 2 key papers are both Santer et al.) Even having
>>>me as the first author may not work. An ideal person
>>>would be Tom Karl, who sent me a response saying "nice
>>>summary".
>>>what do you think?
>>>Tom.

>>
>>
>>--
>>

>>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
>>

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

Prof. Phil Jones
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

942. 1228841349.txt

#####

From: David Thompson <davet@atmos.colostate.edu>
To: Phil Jones <p.jones@uea.ac.uk>, John Kennedy <john.kennedy@metoffice.gov.uk>, Mike Wallace <wallace@atmos.washington.edu>
Subject: the paper and a can of worms
Date: Tue, 9 Dec 2008 11:49:09 -0700

hi all, I plan on sending the 'penultimate' draft of the full paper later today, but thought I'd comment on the NH/SH comparison in a separate email. Anyway, I've been debating adding a comparison of the NH and SH, as per your suggestions. But I think I'm going to delay that discussion to a different paper. The current paper is already long. And I think looking at the differences between the hemispheres is going to open a can of worms. Here is an example that influenced my thinking: The time series in the attached figure show the

mail.2008

differences between the NH and SH mean (0-90N minus 0-90S) for the raw data (top) and ENSO/COWL residual data (bottom). (COWL is removed only from the NH). Among many things, the difference time series show that the cooling in the 70s is largest in the NH, which we know from previous work. Maybe it's just my eye, but the differences between the time series in the 70s look almost discrete. It's as if the NH ratcheted downwards relative to the SH in a very short period ~1968, then crept upwards through the present. My thinking is that we will get a lot of mileage out of comparing the hemispheres, but that to do it right, it's going to take a fair bit more analysis. And at 27 pages I think we're pushing the attention span of the average reader. So I'm going to delay the analysis to our next paper. It gives us something to do in future! Paper will follow later... -Dave

----- David W. J.

Thompson

www.atmos.colostate.edu/~davet Dept of Atmospheric Science Colorado State University
Fort Collins, CO 80523 USA Phone: 970-491-3338 Fax: 970-491-8449 hi all,

I plan on sending the 'penultimate' draft of the full paper later today, but thought I'd comment on the NH/SH comparison in a separate email.

Anyway, I've been debating adding a comparison of the NH and SH, as per your suggestions.

But I think I'm going to delay that discussion to a different paper. The current paper is already long. And I think looking at the differences between the hemispheres is going to open a can of worms. Here is an example that influenced my thinking:

The time series in the attached figure show the differences between the NH and SH mean (0-90N minus 0-90S) for the raw data (top) and ENSO/COWL residual data (bottom). (COWL is removed only from the NH).

Among many things, the difference time series show that the cooling in the 70s is largest in the NH, which we know from previous work. Maybe it's just my eye, but the differences between the time series in the 70s look almost discrete. It's as if the NH ratcheted downwards relative to the SH in a very short period ~1968, then crept upwards through the present.

My thinking is that we will get a lot of mileage out of comparing the hemispheres, but that to do it right, it's going to take a fair bit more analysis. And at 27 pages I think we're pushing the attention span of the average reader. So I'm going to delay the analysis to our next paper. It gives us something to do in future!

Paper will follow later...

-Dave

David W. J. Thompson
www.atmos.colostate.edu/~davet

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

Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

943. 1228922050.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: A quick question
Date: Wed Dec 10 10:14:10 2008

Ben,

Haven't got a reply from the FOI person here at UEA. So I'm not entirely confident the numbers are correct. One way of checking would be to look on CA, but I'm not doing that. I did get an email from the FOI person here early yesterday to tell me I shouldn't be deleting

emails -

unless

this was 'normal' deleting to keep emails manageable! McIntyre hasn't paid his £10, so nothing looks likely to happen re his Data Protection Act email.

Anyway requests have been of three types - observational data, paleo data and who made IPCC changes and why. Keith has got all the latter - and there have been at least 4. We made Susan aware of these - all came from David Holland. According to the FOI Commissioner's Office, IPCC is an international organization, so is above any national FOI. Even if UEA holds anything about IPCC, we are not obliged to pass it on, unless it has anything to do with our core business - and it doesn't! I'm sounding like Sir Humphrey here!

McIntyre often gets others to do the requesting, but requests and responses all get posted up on CA regardless of who sends them.

On observational data, there have been at least 5 including a couple from McIntyre. Others here came from Eschenbach and also Douglas Keenan. The latter relate to Wei-Chyung Wang, and despite his being exonerated by SUNY, Keenan has not changed his web site since being told the result by SUNY!
[1]<http://www.informath.org/>

The paleo data requests have all been to Keith, and here Tim and Keith reply. The recent couple have come from McIntyre but there have been at least two others from Holland.

So since Feb 2007, CRU is in double figures. We never get any thanks for putting things up - only abuse and threats. The latest lot is up in the last 3-4 threads on

CA.

I got this email over the weekend - see end of this email. This relates to what Tim sent back late last week. There was another one as well - a chatty one saying why didn't I respond to keep these people on CA quiet. I've ignored both.

Finally, I know that DEFRA receive Parliamentary Questions from MPs to

mail.2008

answer. One of these 2 months ago was from a Tory MP asking how much money DEFRA has given to CRU over the last 5 years. DEFRA replied that they don't give money - they award grants based on open competition. DEFRA's system also told them there were no awards to CRU, as when we do get something it is down as UEA!

I've occasionally checked DEFRA responses to FOI requests - all from Holland.

Cheers
Phil

Dear Mr Jones

what are you frightened of?

Is it that suddenly mugs like me who pay our taxes suddenly realise we are paying your wages.

Please respond to Climate Audit's valid queries otherwise I will contact my MP. Please see below.

Quote From CA

As it happens, I have experience in mining exploration programs and I can assure Phil Jones that, contrary to this experience enabling me to "understand why some samples are excluded", it gives me exactly the opposite perspective. It makes it virtually impossible for me to think up valid explanations for "excluding" some samples. It's illegal in the businesses that I know.

Anyhow, CRU answered as follows:

we have checked our files and no manuals, computer code, documents or correspondence are available. we can confirm, however, that we did not use a different data set and therefore there is no further data to provide.

Your behaviour is absolutely outrageous.

Best regards

Stuart Harmon

At 01:48 09/12/2008, you wrote:

Dear Phil,
I had a quick question for you: what is the total number of FOIA requests that you've received from Steven McIntyre?
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

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.informath.org/>

944. 1229468467.txt

#####

From: Tom Wigley <wigley@ucar.edu>
To: santer1@llnl.gov
Subject: Re: FOIA request
Date: Tue, 16 Dec 2008 18:01:07 -0700
Cc: "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>, "David C. Bader" <bader2@llnl.gov>, Bill Goldstein <goldstein3@llnl.gov>, Tomas Diaz De La Rubia <delarubia@llnl.gov>, Hal Graboske <graboske1@llnl.gov>, Cherry Murray <murray38@llnl.gov>, mann <mann@psu.edu>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Bill Fulkerson <wfulk@utk.edu>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, Luca Delle Monache <ldm@llnl.gov>, "Hack, James J." <jhack@ornl.gov>, Thomas C Peterson <Thomas.C.Peterson@noaa.gov>, vladeckd@law.georgetown.edu, miller21@llnl.gov, Michael Wehner <mfwehner@lbl.gov>, "Bamzai, Anjali" <Anjali.Bamzai@science.doe.gov>

<x-flowed>
Dear Ben,

This is a good idea. However, will you give only tropical (20N-20S) results? I urge you to give data for other zones as well, viz, SH, NH, GL, 0-20N, 20-60N, 60-90N, 0-20S, 20-60S, 60-90S (plus 20N-20S). To have these numbers on line would be of great benefit to the community. In other words, although prompted by McIntyre's request, you will actually be giving something to everyone.

Also, if you can give N3.4 SSTs and SOI data, this would be an additional huge boon to the community.

For the data, what period will you cover. Although for our paper we only use data from 1979 onwards, to give data for the full 20th century runs would be of great benefit to all. This, of course, raises the issue of drift. Even over 1979 to 1999 some models show appreciable drift. From memory we did not account for this in our paper -- but it is an important issue.

mail.2008

This is a lot of work -- but the benefits to the community would be truly immense.

Finally, I think you need to formally get McIntyre to list the 47 models that he wants the data for. The current request is ambiguous -- or, at least, ill defined. I think it is crucial for McIntyre to state specifically what he wants. Even if we think we know what he wants, this is not good enough -- FOIA requests must be clear, complete and unambiguous. This, after all, is a legal issue, and no court of law would accept anything less.

Tom.

+++++

Ben Santer wrote:

> Dear co-authors,
>
> I just wanted to alert you to the fact that Steven McIntyre has now made
> a request to U.S. DOE Headquarters under the Freedom of Information Act
> (FOIA). McIntyre asked for "Monthly average T2LT values for the 47
> climate models (sic) as used to test the H1 hypothesis in Santer et al.,
> Consistency of modelled and observed temperature trends in the tropical
> troposphere". I was made aware of the FOIA request earlier this morning.
>
> McIntyre's request eventually reached the U.S. DOE National Nuclear
> Security Administration (NNSA), Livermore Site Office. The requested
> records are to be provided to the "FOIA Point of Contact" (presumably at
> NNSA) by Dec. 22, 2008.
>
> McIntyre's request is poorly-formulated and misleading. As noted in the
> Santer et al. paper cited by McIntyre, we examined "a set of 49
> simulations of twentieth century climate change performed with 19
> different models". McIntyre confuses the number of 20th century
> realizations analyzed in our paper (49, not 47!) with the number of
> climate models used to generate those realizations (19). This very basic
> mistake does not inspire one with confidence about McIntyre's
> understanding of climate models, or his ability to undertake meaningful
> analysis of climate model results.
>
> Over the past several weeks, I've had a number of discussions about the
> "FOIA issue" with PCMDI's Director (Dave Bader), with other LLNL
> colleagues, and with colleagues outside of the Lab. Based on these
> discussions, I have decided to "publish" all of the climate model
> surface temperature time series and synthetic MSU time series (for the
> tropical lower troposphere [T2LT] and the tropical mid- to
> upper-troposphere [T2]) that we used in our International Journal of
> Climatology (IJoC) paper. This will involve putting these datasets
> through an internal "Review and Release" procedure, and then placing the
> datasets on PCMDI's publicly-accessible website. The website will also
> provide information on how synthetic Microwave Sounding Unit (MSU)
> temperatures were calculated, anomaly definition, analysis periods, etc.
>
> After publication of the model data, we will inform the "FOIA Point of
> Contact" that the information requested by McIntyre is publicly
> available for bona fide scientific research.
>
> Unfortunately, we cannot guard against intentional or unintentional
> misuse of these datasets by McIntyre or others.
>
> By publishing the T2, T2LT, and surface temperature data, we will be
> providing far more than the "Monthly average T2LT values" mentioned in

mail.2008

> McIntyre's FOIA request to DOE. This will make it difficult for McIntyre
> to continue making the bogus claim that he is being denied access to the
> climate model data necessary to evaluate the validity of our findings.
> All of the raw model output used in our IJoC paper are already available
> to Mr. McIntyre (as I informed him several months ago), as are the
> algorithms required to calculate synthetic MSU temperatures from raw
> model temperature data.
>
> I hope that "publication" of the synthetic MSU temperatures resolves
> this matter to the satisfaction of NNSA, DOE Headquarters, and LLNL.
>
> 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

>

</x-flowed>

945. 1229712795.txt

#####

From: Phil Jones <p.jones@uea.ac.uk>
To: "Allan Astrup Jensen" <aaj@force.dk>, "Stefan Reimann" <Stefan.Reimann@empa.ch>
Subject: RE: WP8 added text and additional person from CMA
Date: Fri Dec 19 13:53:15 2008
Cc: "lu xiaoxia" <luxx@urban.pku.edu.cn> "Brian Reid" <b.reid@uea.ac.uk>,
<p.burton@uea.ac.uk>

Allan,
I was leaving that for Brian Reid or Paul Burton here.
Cheers
Phil

At 13:32 19/12/2008, Allan Astrup Jensen wrote:

Fine, do you know how status is with WP14?
Allan Astrup Jensen
Technical Vice President
Secretariat for Quality Management and Metrology
FORCE Technology, Brøndby
Park Allé 345
2605 Brøndby
Denmark
Phone: +45 43 26 70 00
Direct: +45 43 26 70 81
Mobile: +45 40 94 10 22
Fax: +45 43 26 70 11
e-mail: aaj@force.dk <[1]mailto:aaj@force.dk>
www: [2]www.forcetechnology.com <[3]http://www.forcetechnology.com/>

mail.2008

This email and any files transmitted with it may contain confidential information intended for the addressee(s) only. The information is not to be surrendered or copied to unauthorised persons. If you have received this communication in error, please notify us immediately by email at: info@forcetechnology.com

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

From: Phil Jones [[4]mailto:p.jones@uea.ac.uk]
Sent: 19. december 2008 14:29
To: Allan Astrup Jensen; Stefan Reimann
Cc: lu xiaoxia
Subject: RE: WP8 added text and additional person from CMA

Stefan,
Can you contact your person, as they are more senior to mine?
I'll make modifications to WP8 and get it back to Allan.
Cheers
Phil

At 13:12 19/12/2008, Allan Astrup Jensen wrote:
>First you should contact them and hear if they
>would be interested, they may be occupied by
>another proposal. If they are ready, they should
>send me urgently their ½ pages descriptions of
>each and CMA, their PIC no., email and salary.
>May be Peking University know them. We add them then as partner no. 21.

>
>Yours truly,
>
>Allan Astrup Jensen
>
>Technical Vice President
>Secretariat for Quality Management and Metrology
>
>FORCE Technology, Brøndby
>Park Allé 345
>2605 Brøndby
>Denmark
>
>Phone: +45 43 26 70 00
>Direct: +45 43 26 70 81
>Mobile: +45 40 94 10 22
>Fax: +45 43 26 70 11
>e-mail: aaj@force.dk <[5]mailto:aaj@force.dk>
>www: [6]www.forcetechnology.com <[7]http://www.forcetechnology.com/>
>

>*****

>This email and any files transmitted with it may contain confidential information intended for the addressee(s) only. The information is not to be surrendered or copied to unauthorised persons. If you have received this communication in error, please notify us immediately by email at: info@forcetechnology.com

>*****

>
>-----Original Message-----
>From: Stefan Reimann [[8]mailto:Stefan.Reimann@empa.ch]
>Sent: 19. december 2008 13:51
>To: Allan Astrup Jensen; Phil Jones
>Subject: WP8 added text and additional person from CMA
>
>Dear Allan, Phil and Bill,
>I have added some text concerning greenhouse gas and air pollution monitoring.

mail.2008

>
>I hope that this is precise enough.
>
>I also have an extremely good contact in CMA. Prof. Lingxi Zhou,
>CMA, CAWAS (Center for Atmosphere Watch and Services)
>Further,
>she has been newly elected into the bureau of
>the task force in National greenhouse gas inventories of IPCC
>
>[9]<http://www.ipcc-nggip.iges.or.jp/org/overview.html>
>
>I suggest that we have Phils and our contact
>from CMA included (Zhongwei Yan and Lingxi
>Zhou). Can you please tell me if this is ok?
>
>Stefan
>
>Stefan Reimann
>Empa - Materials Science & Technology
>Ueberlandstr.129
>8600 Duebendorf
>Switzerland
>Tel:0041 (0)44823 46 38
>Fax:0041 (0)44821 62 44
>e-mail stefan.reimann@empa.ch
>[10]http://www.empa.ch/climate_gases
Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

Prof. Phil Jones
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:aaj@force.dk>
2. <http://www.forcetechnology.com/>
3. <http://www.forcetechnology.com/>
4. <mailto:p.jones@uea.ac.uk>
5. <mailto:aaj@force.dk>
6. <http://www.forcetechnology.com/>
7. <http://www.forcetechnology.com/>
8. <mailto:Stefan.Reimann@empa.ch>
9. <http://www.ipcc-nggip.iges.or.jp/org/overview.html>
10. http://www.empa.ch/climate_gases

946. 1230052094.txt

#####

mail.2008

From: Ben Santer <santer1@llnl.gov>
To: lbutler@ucar.edu
Subject: Re: averaging
Date: Tue, 23 Dec 2008 12:08:14 -0800
Reply-to: santer1@llnl.gov
Cc: Tom Wigley <wigley@cgd.ucar.edu>, kevin trenberth <trenbert@ucar.edu>

<x-flowed>

Dear Lisa,

That's great news! I've confirmed with DOE that I can use up to \$10,000 of my DOE Fellowship to provide financial support for Tom's Symposium. I will check with Anjali Bamzai at DOE to determine whether there are any strings attached to this money. I'm hopeful that we'll be able to use the DOE money for the Symposium dinner, and to defray some of the travel expenses of international participants who can't come up with their own travel money. I'll try to resolve this question in the next few days.

Best wishes to you and your family for a very Merry Christmas, and a happy, healthy, and peaceful 2009!

Ben

Lisa Butler wrote:

> Hi Ben,
> Sorry for the slow reply -- I had to check on a few things, but yes, now
> I can agree that June 19th seems like a good bet for our Wigley
> Symposium. CCSM in Breckenridge will adjourn sometime on Thursday
> afternoon, 6/18.

>
> For June 19 I reserved the Main Seminar Room at the Mesa from 8:00 AM -
> 5:30 PM and the Damon Room (for a reception) from 5:30 PM to 8:00 PM. Of
> course we can tweak these times as we get closer if need be.

>
> After the holidays I work up a rough draft budget for the catering and
> see what, if any, financial help we might be able to get from CGD
> and/or NCAR Directorate.

>
> Best wishes for a Merry Christmas and Happy New Year!
> Lisa

> Ben Santer wrote:

>> Dear Tom,

>>
>> I think we agreed that your symposium would be after the 2009 CCSM
>> Workshop in Breckenridge, which will take place during the week of
>> June 15th. I do not yet have the exact dates of the CCSM meeting - I
>> don't know whether it ends on Thursday, June 18th. I suspect it will.
>> In the past, CCSM workshops have generally started on a Tuesday and
>> ended on a Thursday. So my guess is that Friday, June 19th would
>> probably be our best bet for your symposium. CCSM workshops are
>> usually preceded by a Monday meeting of the CCSM Scientific Steering
>> Committee, CCSM Working Group Co-Chairs, and CCSM Advisory Board. As a
>> Co-Chair of the Climate Change Working Group, I would be involved in
>> this Monday meeting.

>>
>> I'm copying Lisa on this email, in order to check whether Friday, June
>> 19th is a good date for the symposium.

>> Cheers,

>>
>> Ben

>> Tom Wigley wrote:

mail.2008

>>> Ben,
>>>
>>> Did you get my email about papers on averaging of
>>> model results? Do you want me to email the papers?
>>>
>>> Is there a date for my symposium? Have you invited
>>> anyone? Shall I make a priority list? This would/could
>>> be based on ...
>>>
>>> (1) A balance of sub-disciplines so as to have the
>>> potential to produce a useful book
>>>
>>> (2) Importance of topics, perhaps determined via
>>> citations of related papers by the invited participants
>>>
>>> (3) Closeness to me personally
>>>
>>> (4) Numbers of jointly authored papers
>>>
>>> -----
>>>
>>> So, e.g., there would have to be presentations by you
>>> and Phil. Also (as a close friend) Tim -- on paleoclimate
>>> in general I guess rather than just isotopes in speleothems.
>>> He could easily slot in some cool caving stuff.
>>>
>>> Jerry Meehl on AOGCMs. Malte and/or Sarah on UD EBMs.
>>> (But how to get some SCENGEN in? ... as this is almost
>>> totally my work.)
>>>
>>> Rob Wilby on downscaling.
>>>
>>> Niel Plummer would be nice to invite, but I'm not sure
>>> how he would fit in subject wise.
>>>
>>> Peter Foukal (or Claus Frohlich) on the Sun -- altho I've not
>>> worked much with them, this is an important subject area.
>>>
>>> Caspar on volcanoes.
>>>
>>> Also, Jean Palutikof on impacts and adaptation (her new Oz
>>> job is focussed on adaaptation).
>>>
>>> I'm just thinking out loud here. Might be good to talk about
>>> this soon.
>>>
>>> -----
>>>
>>> But in the meantime -- what is the proposed date?
>>>
>>>
>>
>>
>

--

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.

mail.2008

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

</x-flowed>