

December 01, 2009

Climate Conspiracy Appendix B

By [David H. Douglass](#) and [John R. Christy](#)



Appendix B. Email chronology.

[Appendix A [here](#)]

31 May 2007

Douglass, Christy, [Pearson](#), Singer paper (DCPS) submitted to IJC

11 Oct 2007

DCPS paper accepted.

1 Nov 2007

corrected proofs of DCPS paper returned to IJC

30 Nov 2007

In the Dec 4 2009 email below there is this embedded email by Revkin

Date: Fri, 30 Nov 2007 11:38:52 -0500

To: santer1@xxxxxxxxxxx.xxx, broccoli@xxxxxxxxxxx.xxx, mears@xxxxxxxxxxx.xxx

From: Andrew Revkin anrevk@xxxxxxxxxxx.xxx

Subject: sorry to take your time up, but really do need a scrub of singer/christy/etc effort

>Mime-Version: 1.0

>Content-Type: multipart/mixed;

>X-NYTOriinatingHost: [10.149.144.50]

hi,

for moment please do not distribute or discuss. trying to get a sense of whether singer / christy can get any traction with this at all.

* _ ANDREW C. REVKIN

<<http://www.nytimes.com/revkin>>_*The **New York Times**  / Environment / Dot Earth
<[http://](http://dotearth.blogs.nytimes.com/)

dotearth.blogs.nytimes.com/>Blog<<http://dotearth.blogs.nytimes.com/>

>620

Eighth Ave., NY, NY 10018-1405

phone: 212-556-7326 fax: 509/-357-0965 mobile: 914-441-5556

Attachments: DCPS-proofs_IJC07.pdf

[Note: Revkin of the NYT is asking Santer, Mears and Broccoli not to distribute or discuss what? What does scrubbing mean? (Revkin indicated later it means "critiquing"). If they are the proofs, how did Revkin get a copy? From one of DCPS?]

Dec 4 2009 [1196795844]

1. At 14:17:24 Mears sends the email from Revkin to Wigley

copies: Jones, Santer, Thorne, Sherwood, Lanzante, Taylor, Seidel, Free and Wentz

Hi Ben, Phil and others

"To me, the fundamental error is 2.3.1 **[Note a]**

Does IJC publish comments?"

2. At 17:53 Santer responds

Dear folks

I'm forwarding this to you in confidence. We all knew that some journal, somewhere, would eventually publish this stuff. Turns out that it was the International Journal of Climatology.

copies to Mears and Jones

[note a. from DCPS manuscript]

[note b. Santer knows about the coming publication of DCPS. He rejected this paper when it was previously submitted to another journal, so is very familiar with it]

3. Jones responds to Santer

Ben,

It sure does! Have read briefly - the surface arguments are wrong. I know editors have difficulty finding reviewers, but letting this one pass is awful - and IJC was improving

Cheers

Phil

5 Dec 2007 [1196877845]

From: Ben Santer

To: Peter Thorne

cc: Carl Mears , Leopold Haimberger, Karl Taylor, Tom Wigley, Phil Jones, Tom Wigley, Steve Sherwood, John Lanzante, Dian Seidel, Melissa Free, Frank Wentz , Steve Klein.

Dear folks

Peter, I think you've done a nice job in capturing some of my concerns about the Douglass et al. paper... . I don't think it's a good strategy to submit a response to the Douglass et al. paper to

the International Journal of Climatology (IJC). As Phil pointed out, IJC has a large backlog, so it might take some time to get a response published. Furthermore, Douglass et al. probably would be given the final word.[**Note b**]

[Note a. Santer has receive a communication from Peter Thorne. He is alerting the team which includes Phil Jones concerning the DCPS paper.]

[Note b. In typical procedures a "Response" to a published paper, such as Santer is suggesting regarding DCPS, includes a "Reply" (i.e. "final word") from the authors of the original paper side by side so readers can judge arguments conveniently. As shown below, the team, with cooperation of IJC editors, orchestrated a different outcome which was to exclude DCPS from offering a "Reply".]

5 Dec 2007 [1196882357]

From: Ben Santer <santer1@xxxxxxxxxxx.xxx>

To: Phil Jones p.jones@xxxxxxxxxxx.xxx

Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/ etc effort]

Cc: Carl Mears, Leopold Haimberger, Karl Taylor, Tom Wigley, Phil Jones, Tom Wigley, Steve Sherwood, John Lanzante, Dian Seidel, Melissa Free, Frank Wentz, Steve Klein

Embedded email from Jones to Santer

Phil Jones wrote:

All,

IJC do have comments but only very rarely. I see little point in doing this as there is likely to be a word limit, and if the system works properly Douglass et al would get the final say. There is also a large backlog in papers awaiting to appear, so even if the comment were accepted it would be some time after Douglass et al that it would appear.

...

Cheers Phil

[Note a. The group does not want the procedure to work "properly" and is aware a normal publication schedule would have DCPS appear before their response could. Phil Jones is pointing out problems with IJC]

5 Dec 2007

Douglass et al. paper (DCPS) published on line

6 Dec 2007 [1196956362]

From: Ben Santer <santer1@xxxxxxxxxxx.xxx>

To: Melissa Free <Melissa.Free@xxxxxxxxxxx.xxx>

Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/
etc effort]

Cc: Phil Jones, carl mears, Karl Taylor, Tom Wigley, Tom Wigley, "Thorne, Peter", Steven
Sherwood, John Lanzante "Dian J. Seidel", Frank Wentz, Steve Klein, Leopold Haimberger,
peter gleckler

Dear Melissa,

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

With best regards,

Ben

[Note a. Melissa Free is a little cautious. About this time she presented evidence at an AMS conference that the radiosonde and model trends were indeed significantly different.]

Melissa Free wrote:

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

Melissa

Benjamin D. Santer

6 Dec 2007 [1196964260]

From: Dian Seidel <dian.seidel@xxxxxxxxxx.xxx>

To: santer1@xxxxxxxxxx.xxx

Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/
etc effort]

Cc: Phil Jones, carl mears, Karl Taylor, Tom Wigley, Tom Wigley, "Thorne, Peter", Steven Sherwood, John Lanzante, Melissa Free, Frank Wentz, Steve Klein, Leopold Haimberger, peter gleckler

[Note a. Dian Siedel is a little cautious.]

Hello Ben and Colleagues,

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

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

[Note b. By using the average, DCPS gave each model one vote.]

With regards,
Dian

Ben Santer wrote:

Dear Phil,

-----skip beginning-----

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

With best regards,
Ben

Phil Jones wrote:

All,

IJC do have comments but only very rarely. I see little point in doing this as there is likely to be a word limit, and if the system works properly Douglass et al would get the final say. There is also a large backlog in papers awaiting to appear, so even if the comment were accepted it would be some time after Douglass et al that it would appear. Better would be a submission to another journal (JGR?) which would be quicker. This could go in before Douglass et al appeared in print - it should be in the IJC early online view fairly soon based on recent experiences. **[Note a]** A paper pointing out the issues of trying to weight models in some way would be very beneficial to the community. AR5 will have to go down this route at some point. How models simulate the recent trends at the surface and in the [troposphere](#) /stratosphere and how they might be ranked is a possibility. This could bring in the new work Peter alludes to with the sondes. There are also some aspects of recent surface T changes that could be discussed as well. These relate to the growing dominance of buoy SSTs (now 70% of the total) vs conventional ships. There is a paper in J. Climate accepted from Smith/Reynolds et al at NCDC, which show that buoys could conceivably be cooler than ship-based SST by about 0.1C - meaning that the last 5-10 years are being gradually underestimated over the oceans. Overlap is still too short to be confident about this, but it highlights a major systematic change occurring in surface ocean measurements. As the buoys are presumably better for absolute SSTs, this means models driven with fixed SSTs should be using fields that are marginally cooler. And then there is the continual reference to Kalnay and Cai, when Simmons et al (2004) have shown the problems with NCEP. It is possible to add in the ERA-Interim analyses and operational analyses to being results from ERA-40 up to date.

Cheers

Phil

[Note a. Phil Jones need not worry. In a few weeks team member and fellow CRU employee Tim Osborn (and on the Editorial Board of International Journal of Climatology) will talk to editor Glenn McGregor and get assurances that will please Phil Jones. See below.]

10 Dec 2008 [1197325034]

From: Tom Wigley <wigley@xxxxxxxxxxx.xxx>
To: santer1@xxxxxxxxxxx.xxx
Subject: Re: [Fwd: [Fwd: FW: Press Release from The Science & Environmental Policy Project]]

Cc: carl mears, Frank Wentz, Tom Wigley, Steven Sherwood, John Lanzante, "Dian J. Seidel", Melissa Free, Karl Taylor, Steve Klein, Leopold Haimberger, "Thorne, Peter", "Philip D. Jones"

Dear all,

I think the scientific fraud committed by Douglass needs to be exposed. His co-authors may be innocent bystanders, but I doubt it. In normal circumstances, what Douglass has done would cause him to lose his job -- a parallel is the South Korean cloning fraud case. I have suggested that someone like Chris Mooney should be told about this.

Tom.

[Note a. The DCPS paper was published through the traditional anonymous-reviewer process. The arithmetic was confirmed by S08. DCPS did not attempt to communicate outside of proper channels with the editor and reviewers. We don't know why Tom Wigley believes this is fraud. The implication here is that Mooney of the *Washington Post* is a useful tool for the climate establishment]

12 Dec 2008 [1197507092]

From: Ben Santer <santer1@xxxxxxxxxxx.xxx>
To: Tim Osborn <t.osborn@xxxxxxxxxxx.xxx>
Subject: Re: Douglass paper
Cc: Phil Jones, Keith Briffa, Tom Wigley

Dear Tim,

Thanks for the "heads up". As Phil mentioned, I was already aware of this. The Douglass et al. paper was rejected twice before it was finally accepted by IJC **[Note a]**. I think this paper is a real embarrassment for the IJC. It has serious scientific flaws. I'm already working on a response.

Phil can tell you about some of the other sordid details of Douglass et al. These guys ignored

information from radiosonde datasets that did not support their "models are wrong" argument (even though they had these datasets in their possession)[**Note b**]. Pretty deplorable behaviour...Douglass is the guy who famously concluded (after examining the temperature response to Pinatubo) that the climate system has negative sensitivity. Amazingly, he managed to publish that crap in GRL. Christy sure does manage to pick some brilliant scientific collaborators...

With best regards,
Ben

[Note a. Santer was the reviewer who rejected the first submission to GRL]

[Note b. DCPS ignored radiosonde datasets that were demonstrably faulty, but were not allowed the chance to explain that in this process controlled by McGregor as will be demonstrated below.]

Tim Osborn [**Note a**] wrote:
Hi Ben,

I guess it's likely that you're aware of the Douglass paper that's just come out in IJC, but in case you aren't then a reprint is attached. They are somewhat critical of your 2005, paper, though I recall that some (most?) of Douglass' previous papers -- and papers that he's tried to get through the review process -- appear to have serious problems.

cc Phil & Keith for your interest too!

Cheers
Tim
Dr Timothy J Osborn, Academic Fellow
Climatic Research Unit

[Note a. Tim Osborn, a colleague of Phil Jones at CRU, is on the Editorial Board of IJC and inserts himself into the process, indicating a clear bias regarding DCPS. Why does he not inform DCSP of this activity or his concerns?]

12 Dec 2007.

An unsigned report attacking the DCPS paper appears on the RealClimate blog. The issue of DCPS not using RAOBCORE 1.4 data is raised. It is noted that team member and coauthor Gavin Schmidt is one of the founders of RealClimate. The RAOBCORE issue is discussed below.

3 Jan 2008

Addendum to DCPS paper was submitted to IJC explaining the omission of RAOBCORE prompted by the RealClimate blog discussion. The addendum explains why RAOBCORE was not used. IJC never published the Addendum. See more below in regard to editor McGregor and Santer in orchestrating this issue. (A copy of the addendum may be found at [http://www.pas.rochester.edu/~douglass/.](http://www.pas.rochester.edu/~douglass/))

10 Jan 2008 13:00 [1199988028]

From: Ben Santer
To: Tim Osborn
Subject: Re: Update on response to Douglass et al.
Cc: "Philip D. Jones"

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

[Note a. Osborn works with Jones at CRU and is on the editorial board of IJC:

1. recognizes "downside" of allowing DCPS to reply. Why is there a downside associated with open debate?]

[Note b. Osborn contacts McGregor editor of IJC who:

1. promises to do everything he can

2. quick turn-around time

3. is keen to correct the scientific record

4. will hold back the hard copy appearance of DCPS so that the Santer paper could appear along side it.

5. asks Osborn to identifying in advance reviewers who are both suitable and available.]

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...

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. 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. ...

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.

[Note c. Ben Santer accepts offer of IJC Editorial Board member Osborn to be the conduit to McGregor and feels that McGregor would be receptive to receiving Santer's emails to Osborn.]

[Note d. The evidence indicates Santer is orchestrating the process with the aid of the IJC management. He does not want DCPS to have the opportunity to reply (i.e. have the last word.) In essence he is stating he wants done for his submission what should have been granted DCPS under normal conditions. How could his paper not possibility be viewed as a comment on DCPS? This is not how the peer-review process works in general, nor how it worked for DCPS at IJC.]

With best regards,
Ben

10 Jan 2008 16:14 [1199999668] "part of a secret"

From: Phil Jones
To: santer1

copies to: Tom Wigley, Karl Taylor, John Lanzante, carl mears, "David C. Bader", "Francis W. Zwiers", Frank Wentz, Leopold Haimberger, Melissa Free, "Michael C. MacCracken", "Philip D. Jones", Steven Sherwood, Steve Klein, 'Susan Solomon', "Thorne, Peter", Tim Osborn, Gavin Schmidt, "Hack, James J."

Subject: An issue/problem with Tim's idea !!!!!!!

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 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

[Note a. Osborn has told Phil Jones "part of secret" from Glenn McGregor that the DCPS paper had two reviews - one positive, the other said it wasn't great but not flawed, but would leave it up to the editor's discretion.]

[Note b. Coauthor Phil Jones says to Santer "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?". Questions: Does Santer scan his email list wondering who is reviewer #2 on DCPS? Does team member Osborn reveal this part of McGregor's secret?

[Note c. Phil Jones has revealed the idea of a "secret" to 17 other members of the team]

[Note d. On the basis of Osborn/McGregor discussion of quick review Phil Jones suggests Kevin Trenberth as a reviewer. Did this happen? In some journals, the editors who are unfamiliar with the area under examination solicit potential reviewer recommendations who may be called on at their discretion.]

[Note e. Jones feels "we can lean on Glenn" in regard to charges for colour. This was not a possibility for DCPS]

11 Jan 2008 [1200076878]

From: Tim Osborn
To: santer
Subject: Re: Update on response to Douglass et al.
Cc: "Philip D. Jones"

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

Dr Timothy J Osborn, Academic Fellow
Climatic Research Unit

[Note a. McGregor reconfirms the plan of complete cooperation with team member Osborn, essentially promising a "quick turn around" and acceptance as a "new submission" to disallow DCSP the opportunity to reply to direct criticisms in the normal manner.]

[Note b. Osborn reiterates to Santer and Jones that McGregor will delay the print publication of DCPS, though he feels compelled to note that this is a restricted piece of information. Question: Will Santer and Jones reveal this secret to their coauthors?]

11 Jan 2008 23:33 [1200112408]

From: Leopold Haimberger

To: santer1

Subject: Re: IJoC and Figure 4

Cc: Peter Thorne, Dian Seidel, Tom Wigley, Karl Taylor, Thomas R Karl, John Lanzante, Carl Mears, "David C. Bader", "Francis W. Zwiers", Frank Wentz, Melissa Free, "Michael C. MacCracken", Phil Jones, Steve Sherwood, Steve Klein, 'Susan Solomon', Tim Osborn, Gavin Schmidt, "Hack, James J."

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. **[Note a]** From my perspective, IJoC should be given the opportunity to set the record straight. **[Note b]** 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. **[Note c]** 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 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.

[Note d] 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. 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. **[Note e]** 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. 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. **[Note f]**

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.

[Note g] 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

[Note a. Santer notifies 17 team members that the plan has been agreed to by Glenn McGregor]

[Note b] The IJC editor and editorial board members never contacted DCPS for a rebuttal or explanation. They accepted Santer's story without investigation.]

[Note c. Having the "last word" is the critical goal rather than open debate with the normal back-and-forth arguments of scientists.]

[Note d. DCSP used published values of radiosonde errors - which include structural uncertainty.]

[Note e. It shows DCSP were aware of the problems with RAOBCORE. On April 10 McGregor will send Santer a copy of the DCSP Addendum which explains the problems. Briefly Sakamoto and Christy (2009 - SC09) looked closely at the ERA-40 Reanalyses on which RAOBCORE 1.3,1.4 were based. SC09 demonstrated that a spurious warming shift occurred in 1991 which was then absorbed into RAOBCORE temperatures, producing spurious positive trends in the upper troposphere and lower stratosphere. SC09 had been working on this since 2006, and so were aware of the problems at the time of DCPS. Even though Santer had seen the Addendum with the explanation of the RAOBCORE problems on that date (10 Apr 2008) their published paper contains the statement " Although DCPS07 had access to all three RAOBCORE

versions, they presented results from v1.2 only."]

[Note f. These extended results that they do "NOT" want to show (HadAT2 and Sherwood's IUK) actually agree with UAH data and the results of DCPS. These extended results were not shown in Santer et al.

[Note g. DCSP did not use unpublished datasets about which problems were already apparent and were soon to be published.]

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

[Note h. Leo is the author of the RAOBCORE datasets, so has a vested interest in seeing they are featured and likely not ready to accept the problems of their reference base in ERA-40 (see above.)]

1 April, 2008 (from Douglass' files)

To: glenn.mcgregor

from david douglass

Dr Glen McGregor
Director of School
Geography, Geology and Environmental Science
The University of Auckland, New Zealand

Dear Dr McGregor;

Congratulations and good wishes for success in your new position. I understand that you are still the Editor of the International Journal of Climatology (IJC). So this communication is in that regard. On Jan 3, 2008 I submitted an addendum to the published paper

A comparison of tropical temperature trends with model Predictions. David H. Douglass, John R. Christy, Benjamin D. Pearson, and S. Fred Singer . *Int. J. Climatol.* (2007) DOI: 10.1002/joc.1651 using the IJC webpage submission process. I received an acknowledgment of receipt via your computerized system a PDF copy of the submitted addendum manuscript which is attached.

I am inquiring as to the status of this submission.

Sincerely;

David Douglass

10 April 2008 [8:45PM] (from Douglass' files)

from:Glenn McGregor

To: David Douglass

dear david

thanks for your kind message

I am having great difficulty locating your addendum on the "system"

Were you allocated with a submission number eg. joc-08-0??

best

glenn

10 April 2008 [10:31PM](from Douglass' files)

from glen mcgregor

to: david douglass

thanks and will investigate further [**Note a**]

David Douglass wrote:

Glenn;

I did not record the submission number. The label on the PDF file that was sent back to me is s1-ln377204795844769-1939656818Hwf-88582685IdV9487614093772047PDF_HI0001.pdf.. Does this help? If not, can you proceed by just entering the file I sent you into the system?

Regards;
David Douglass

best
glenn

[Note a. this must have been successful because he sent it immediately to Santer. See next entry]

[Note b. This was never published. Nor was Douglass ever sent any notice]

10 April 2008 (from 25 april [1209143958])

g.mcgregor@xxxxxxxxxxx.xxx 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[**Note a**] for the Douglass et al. paper recently sent to me by

David Douglass. [Note b] I would be interested to learn of your views on this [Note c]
Best,

Prof. Glenn McGregor

[Note a. this addendum was submitted to IJC on Jan3. 2008]

[Note b. McGregor sends the DCPS Addendum Santer on 10 April, the day he apparently receives it for the first time. This is an extremely close relationship between the author and editor, and indicates Santer has some say in the issue.]

[Note c. Santer rejects see below]

24 april 2008 [1209080077]

From: Ben Santer

To: "Thorne, Peter", Leopold Haimberger, Karl Taylor, Tom Wigley, John Lanzante, "Susan Solomon", 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 - International Journal of Climatology]

Date: Thu, 24 Apr 2008 15:47:33 -0400 (EDT)

From: g.mcgregor@xxxxxxxxxxx.xxx

To: santer1@xxxxxxxxxxx.xxx

Subject: JOC-08-0098 - International Journal of Climatology

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

[Note a. Again, the evidence indicates a very close relationship. In other words, the editor is telling Santer that the paper is in essence accepted since he allows this early

response without seeing the other reviews.]

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."

---rest of email omitted---

[Note b. Santer informs 17 team members that McGregor has given them a head start. They only have to deal with reviewer #1, whom Santer apparently knows. (Reviewer 1 misses the specific question DCPS addressed and is thinking like S08 about comparing observations with a universe of model results but which do NOT have the same surface trend as observations - the key condition in the DCPS paper - see Appendix A.)].

With best regards,

Ben

5 May 2008 [1210030332]

From: Ben Santer <santer1@xxxxxxxxxxx.xxx>

To: g.mcgregor@xxxxxxxxxxx.xxx

Subject: Re: JOC-08-0098 - International Journal of Climatology

g.mcgregor@xxxxxxxxxxx.xxx 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

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

[Note a. Even though Santer should be extremely grateful to McGregor, he complains.]

6 May 2008 [1210079946]

From: Tim Osborn <t.osborn@xxxxxxxxxxx.xxx>

To: g.mcgregor@xxxxxxxxxxx.xxx

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 pages! Hope all's well in NZ.

Tim

[Note a. Osborn explains to McGregor that Santer is at fault.]

[Note b. In CRU email # 1226451442 Santer laments to Thomas Karl "Quite frankly, Tom, having spent nearly 10 months of my life addressing the serious scientific flaws in the Douglass et al. IJoC paper... ". Question: Why does it take 90 pages and 10 months to show "serious flaw" in any paper? Why, if the DCPS error was so flagrant, would Santer et al. need the protection of IJC to eliminate the possibility of a simultaneous comment from DCPS?]

26 May 2008 (Douglass' email)

Douglass to Thorne

Dr. Peter Thorne
Climate Research Scientist
Hadley Centre for Climate Prediction and Research
Met Office

Dear Dr. Thorne;

I have read your article in the most recent Nature Geoscience: "*The answer is blowing in the wind*". I am trying to understand the difference between your figure 1 and a similar figure by Douglass et al. [Nov. 2007 Int. J. Climatol. doi:10.1002/joc.1651]. For example, the plots IUK(?) and MALR in the fig are unfamiliar to me. The caption states that this figure is based in part from Santer et al. (submitted). Would you send me a copy of the Santer manuscript?

Thank you;
David Douglass

27 May 2008_A 4:04 AM (Douglass' email)

Thorne to Douglass

Dear David,

As I am not first author on the Santer et al manuscript I am not in a position to send it on to you.

You would have to ask Dr. Santer directly.

The Figure contains several new datasets.

RAOBCORE 1.4 and RICH are from Leo Haimberger and described in his recently accepted J. Clim piece that I believe is available online. IUK is from Steve Sherwood and is described in a paper also accepted by J. Clim. Again, I believe this is now available online. The dataset is available from this site:

<http://earth.geology.yale.edu/~sherwood/radproj/>

MALR is not a dataset but rather what the Moist Adiabatic Lapse Rate would imply.

The other major difference is in how the models are treated. In my figure the range of model amplification behaviour found in the Santer et al. Science paper has been used to determine an expectation by scaling the 2 sigma range of model amplification behaviour at each pressure level by the observed surface warming. This is possible as the behaviour is found to be so strongly tied across models regardless of the modelled absolute trends. It is this behaviour that I strongly believe we should be testing against and hence I feel this to be the most logical and appropriate test approach.

Yours

Peter

27 May 2008_B (9:06 AM)

Douglass to Thorne

Peter;

I believe that you are required to make reference 5 [your paper with Santer] available to me (see Nature's policy on ethics below). I suggest that you send your paper with Santer to me and inform Santer that Nature's ethics policy requires you to do so.

Regards;

David Douglass

-----NATURE JOURNALS' POLICIES ON PUBLICATION ETHICS-----

Availability of data and materials

"An inherent principle of publication is that others should be able to replicate and build upon

the authors' published claims. Therefore, a condition of publication in a Nature journal is that authors are required to make materials, data and associated protocols available .."

27 May 2008_C [1211911286]

From: Ben Santer <santer1@xxxxxxxxxxx.xxx>
To: David Douglass <douglass@xxxxxxxxxxx.xxx>
Subject: Re: Your manuscript with Peter Thorne
Date: Tue, 27 May 2008 14:01:26 -0700
Reply-to: santer1@xxxxxxxxxxx.xxx
Cc: Christy John , "Thorne, Peter"

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

[Note a. Douglass asks for a copy of the new Santer et al. paper (recall at this point Santer has already seen the DCPS Addendum)].

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

[Note b Santer, as the reviewer of the earlier DCSP paper, had DCSP his possession from 2007]

27 May 2008_C 11:19PM

Peter

I felt after I sent the e-mail that I was too hasty to assume that Santer would not send a copy of the manuscript. So I did ask him. His very quick and long reply ended with *"I see no conceivable reason why I should now send you an advance copy of my IJoC paper."*

It is, of course, your paper too.

David Douglass

28 May 2008 [1212009927]

From: Phil Jones

To: Tom Wigley, Steven Sherwood

Subject: Re: David Douglass

Date: Wed May 28 17:25:27 2008

Cc: santer1"Thorne, Peter", Leopold Haimberger, Karl Taylor, Tom Wigley, John Lanzante, ssolomon Melissa Free, peter gleckler, Thomas R Karl, Steve Klein, carl mears, Doug Nychka, Gavin Schmidt, Frank

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.

[Note a. Santer knows of the DCPS Addendum. Did he ever correct the Wigley statement? Wigley apparently is unaware of the problems with RAOBCORE 1.3, 1.4.]

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. **[Note b]**

[Note b. Sherwood misunderstand the problem. RAOBCORE 1.4 had a *shift* to warmer temperatures in the middle of the time series, not a spike, which means the

problem was located at a point in the time series which causes the greatest error in the trend calculation.]

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". [Note c] 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 [Note d].

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

[Note c. Douglass had pointed out to Thorne that he believes that it is the Nature's ethics policy that authors should provide upon request copies of references. (See email

of 27 May 2008_B above.) We find out now that they could not because it was not completed.]

[Note d. Santer seems worried that Douglass has a point]

29 May 2008 5:16AM (Douglass' email)

Thorne to Douglass

Dear David,

after discussion with relevant Met Office colleagues it has been decided that we are not in a position to release a paper to anyone when the lead author has expressly forbidden it. Furthermore, it is our position that in such cases the lead author should also be asked first so your direct request to Dr. Santer would have made no material difference to whether this was released or not. I cannot therefore provide you with this manuscript which I note is currently under revision anyway.

I will continue to provide any further advice that you need to be able to understand the figure and how it was constructed. Please let me know if you need further assistance in this regard and I will be happy to help you with any reasonable requests.

I note that the quoted ethics relates to submitted and not invited works. As the editors at no point raised that I would be required either to lodge a copy of this paper with them or make it available upon request I do not foresee any conflict with Nature Geoscience ethics and practises and this Met Office agreed position

Yours

Peter

30 May 2008 (Douglass' email)

Dear Peter;

I am surprised that invited papers are exempt from Nature's policy on ethics. However, I accept that because Santer has expressly forbidden it that you feel that you can not send me the Santer/Thorne paper. I want to be able to continue our scientific relationship so I will not pursue this issue with you.

I have two questions about your Nature Geophysics paper.

1. You state

"The uncertainty with respect to upper air temperature estimates in the tropics is so substantial that we can draw no meaningful conclusions as to whether or not there is a

discrepancy between long-term trends in the real world and our expectations from climate models."

Does this statement include the results in the IJC paper that I published with John Christy? In particular, are the UAH and the RSS satellite MSU temperature trends also included?

2. Your paper contains the statement

"... evidence for a strong warming in the tropical upper troposphere, providing long-awaited experimental verification of model predictions."

Will you expand on this statement? Are you among those "long-awaiting" the experimental verification of the model predictions?

Regards;

David

30 May 2008 7:07 AM (Douglass' email)

Thorne to Douglass

Dear David,
with respect to your two questions.

- > 1. You state
- > "The uncertainty with respect to upper air temperature estimates
- > in the tropics is so substantial that we can draw no meaningful
- > conclusions as to whether or not there is a discrepancy between long-
- > term trends in the real world and our expectations from climate
- > models."
- > Does this statement include the results in the IJC paper that I
- > published with John Christy? In particular, are the UAH and the RSS
- > satellite MSU temperature trends also included?

The statement refers to the spread of all datasets now available and so includes all the satellite datasets including those from Maryland and NESDIS that show more warming than RSS and all radiosonde datasets including the newer ones contained in the Figure that you referred to in your original request as well as the work with our HadAT automated system which was not included but to which McCarthy et al. J. Clim 08 refers. These newer raobs datasets are RAOBCORE 1.4 and RICH from the Vienna team of Leo

Haimberger and IUK which is a temperature analysis from Steve Sherwood's group. So, it was trying to be fairly animal farm-esque and treat all datasets as equal. I know that this does not suit all tastes, but issues have been found or undoubtedly exist with all datasets and I don't think any group kids themselves that they have the answer (we don't). I can't see a better way that is truly unambiguous (that's not to dismiss work in this regard - I just can't say that I see it as yet being 100% scientifically definitive), but I'd love there to be one as reducing the ambiguity would mean being able to do useful science rather than being "angels dancing on the head of a pin".

>

> 2. Your paper contains the statement

> "... evidence for a strong warming in the tropical upper

> troposphere, providing long-awaited experimental verification of model

> predictions."

> Will you expand on this statement? Are you among those "long-awaiting"

> the experimental verification of the model predictions?

This statement was in fact an editorial change inserted by the editors compared to what I originally wrote. My original statement was along the lines (I can't remember the exact wording ...) "that the vertical structure of the warming which was a common feature in the models had not been replicated before". Yes, some satellite datasets had hinted at it if you screwed your eyeballs hard enough and wished for interpretation that wasn't really valid a la multi-channel splicing (see Nature Corr arising to Fu et al.), but the structure had never been shown.

In terms of long-awaited I would say by the community as a whole this would probably be true which is why I accepted the editorial change. This was, after all, meant to reflect in part a broader community view context. News and views have a very different remit given than a classical paper and tend to be more black and white than a scientist would like as a result.

Myself? Harder to say. I'd like to think that I follow the evidence rather than pre-conceived dogma but like everyone else I can't be sure I do this. That said I'd not doubted that such a solution was possible by any definition of reasonable dataset construction given the fairly massive non-climatic influences inherent in the data - at least the raobs data. I believe that winds may help us get the answer. I believe multi-variate analyses are the only way to reduce this ambiguity unambiguously. Whether that answer is as reported in A&S certainly requires further work as I hopefully make clear in my final paragraphs.

I hope this answers both questions adequately
Peter

10 July 2008 13:56 [1215712600]

From: Ben Santer
To: P.Jones
Subject: Re: [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 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.

With best regards,
Ben

[Note a. McGregor does not allow the "cranky" reviewer a chance to look at the final manuscript again, making it very easy on S08 to get through the system. For DCPS on the other hand, there were a number of iterations with the one neutral reviewer who kept wanting more details on the statistical aspects of DCPS. DCPS was not accepted until these were satisfied.]

22 July 2008 [1216753979]

From: Tim Osborn <t.osborn@xxxxxxxxxxx.xxx>
To: santer1@xxxxxxxxxxx.xxx
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 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

[Note a. This email from Santer to Osborn speaks for itself.]

10 Oct 2008

Santer et al published on line

15 November 2008

The print version DCPS and the Santer & team papers are publish together in the same issue

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Published Online: Dec 5 2007 8:29AM

DOI: 10.1002/joc.1651

Consistency of modelled and observed temperature trends in the tropical troposphere (p 1703-1722)

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Published Online: Oct 10 2008 3:21AM

DOI: 10.1002/joc.1756

[Note a. There were over eleven months between the on-line publication of DCPS and its appearance in print. There were 36 days between the on-line publication of Santer et al. and appearance in print which happened to appear immediately following the Douglass et al. paper. This is just what Santer had planned and Osborn and McGregor had hoped to achieve. Though Santer et al is clearly a comment on DCPS, the procedure had been orchestrated to eliminate the possibility of a DCPS reply.]
on "*Climate Conspiracy Appendix B*"