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# A Climatology Conspiracy?

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The CRU e-mails have revealed how the normal conventions of the peer review process appear to have been compromised by a team\* of [global warming](#) scientists, with the willing cooperation of the editor of the International Journal of Climatology (IJC), Glenn McGregor. The team spent nearly a year preparing and publishing a paper that attempted to rebut a previously published paper in IJC by Douglass, Christy, Pearson, and Singer (DCPS). The DCPS paper, reviewed and accepted in the traditional manner, had shown that the IPCC models that predicted significant "global warming" in fact largely disagreed with the observational data.

We will let the reader judge whether this team effort, revealed in dozens of e-mails and taking nearly a year, involves inappropriate behavior, including (a) unusual cooperation between authors and editor, (b) misstatement of known facts, (c) character assassination, (d) avoidance of traditional scientific give-and-take, (e) using confidential information, (f) misrepresentation (or misunderstanding) of the scientific question posed by DCPS, (g) withholding data, and more.

\*The team is a group of climate scientists who frequently collaborate and publish papers which often support the hypothesis of human-caused global warming. For this essay, the leading team members include Ben Santer, Phil Jones, Timothy Osborn, and Tom Wigley, with lesser roles for several others.

## **Introduction**

This story involves the publication of:  
Douglass et al. 2007 (DCPS)

31 May 2007 submitted to IJC

11 Oct        accepted

1 Nov        page proofs accepted

5 Dec 2007   published online

15 Nov 2008   print version (11+ months after on-line publication)

[reference in appendix B]

and the subsequent publication of

Santer and 17 team members  
25 Mar 2008 submitted to IJC

18 July revised

20 July accepted

10 Oct published on-line

15 Nov print version (36 days after on-line publication)

[reference in appendix B]

This story uses various "CRU e-mails" and our own personal knowledge of events and issues. References will be made to items in an appendix that are arranged chronologically. The e-mails have an index number which comes from a compilation at <http://www.eastangliaemails.com/index.php>.

## **2. The story**

Our record of this story begins when Andrew Revkin, a reporter for the *New York Times*, sends three team members an e-mail [30 Nov 2007] with the page proofs of the DCPS paper. This is a week before the online publication. The subject of Revkin's email --

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

-- implies prior correspondence.

Team member Mears quickly responds with an e-mail to fellow team members Jones, Santer, Thorne, Sherwood, Lanzante, Taylor, Seidel, Free, and Wentz [4 Dec 2007]. Santer replies to all:

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*. Santer knows this because he reviewed and rejected the DCPS paper when it was previously submitted to another journal. Phil Jones responds to Santer:

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.

This exchange provides the first reference to the *International Journal of Climatology*.

The next day (5 Dec 2007 -- the day the DCPS paper appeared on-line), Santer sends an e-mail to Peter Thorne with copies to Carl Mears, Leopold Haimberger, Karl Taylor, Tom Wigley, Phil Jones, Steve Sherwood, John Lanzante, Dian Seidel, Melissa Free, Frank Wentz, and Steve Klein. Santer says,

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 [Jones] 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. [TL1]

The most critical point throughout these emails is the goal of preventing DCPS from providing what is considered normal in the peer-reviewed literature: an opportunity to respond to their critique, or as they put it, "be given the final word." One wonders if there is ever a "final word" in science, as the authors here seem to imply.

The next day (6 Dec 2007), Melissa Free responds with a cautious note, evidently because she had presented a paper with Lanzante and Seidel at an American Meteorological Society conference (18<sup>th</sup> Conference on Climate Variability and Change) acknowledging the existence of a discrepancy between observations and models -- the basic conclusion of the DCPS paper.

What about the implications of a real model-observation difference for upper-air trends? Is this really so dire?

Santer responds (6 Dec 2007) with the key reason for attacking DCPS:

What is dire is Douglass et al.'s willful neglect of any observational datasets that do not support their arguments.

This "willful neglect" of "observational datasets" refers to the absence of two balloon datasets, RAOBCORE v1.3 and v1.4. (DCPS explain in an addendum that these data sets are faulty. See below.)

A further e-mail from Jones (6 Dec 2007) discusses options to beat DCPS into print. Wigley enters (10 Dec 2007) to accuse DCPS of "fraud" and that under "normal circumstances," this would "cause him [Douglass] to lose his job." We remind the reader that DCPS went through traditional, anonymous peer review with iterations to satisfy the reviewers and without communicating outside proper channels with the editor and reviewers.

Tim Osborn, a colleague of Jones at CRU and a member of the editorial board of IJC, inserts himself into the process, declaring a bias on the issue and stating that Douglass's previous papers "appear to have serious problems." Santer responds with gratitude for the "heads up," again making the claim that DCPS ignored certain balloon datasets. As noted below, DCPS did not use these datasets because they were known to be faulty.

On this day (12 Dec 2007), an unsigned report appeared on RealClimate.org attacking DCPS, especially about not using RAOBCORE 1.4. This prompted the DCPS authors to submit an Addendum to IJC on 3 Jan 2008 to explain in one page two issues: (1) the reason for not using RAOBCORE 1.4 and (2) explaining the experimental design to show why using the full spread of model results to compare with observations (as Santer et al. would do) would lead to wrong conclusions about the relationship between trends in the upper air temperature versus the surface -- see Appendix A. (A copy of the addendum may be found at <http://www.pas.rochester.edu/~douglass.>)

Osborn (10 Jan 2008), writing to Santer and Jones, then discusses the "downside" of the normal comment-reply process, in which DCPS would be given an "opportunity to have a response." He explains that he has contacted the editor, Glenn McGregor, to "see what he can do." According to Osborn, McGregor "promises to do everything he can to achieve a quick turn-around." He also says:

(and please treat this in confidence, which is why I emailed to you and Phil only) that he [McGregor] 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.

He [McGregor] also intends to "correct the scientific record" and to identify in "advance reviewers who are both suitable and available," perhaps including "someone on the email list you've been using." Given the bias of Osborn and McGregor as expressed in the emails, one could wonder what it means to be a "suitable" reviewer of the Santer paper.

Santer responds with his conditions, highlighting the intent that he must have the "last word".

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.

This Osborn e-mail and response by Santer essentially lays out the publication strategy apparently agreed to by Santer, Jones, Osborn, and editor McGregor. Santer accepts Osborn as a conduit and defines the conditions (having the "last word"). This is exactly what he seeks to deny for DCPS, who published the original paper in this sequence in IJC.

DCPS are never informed of this process, which specifically addresses our paper, nor are we contacted for an explanation on any point raised in these negotiations. Santer's allegations regarding DCPS and his conditions for publication are simply accepted. One wonders that if the results of DCPS were so obviously and demonstrably in error, why would anyone fear a response by DCPS? See Appendix A.

A new development occurs on this day (10 Jan 2008) as Jones tells the team (Wigley, K. Taylor, Lanzante, Mears, Bader, Zwiers, Wentz, Haimberger, Free, MacCracken, Jones, Sherwood, Klein, Solomon, Thorne, Osborn, Schmidt, and Hack) a "secret" he learns from Osborn: that one of the recipients on the Santer e-mail list is one of the original reviewers of DCPS who did not reject the article.

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!

Does Santer start wondering who the original reviewer is? Does Osborn reveal this part of McGregor's secret?

Then, on the matter of paying for expensive color plots, Jones adds, "I'm sure I can lean on Glenn [McGregor]" to evidently deal with the costs. Obviously, this was not offered to DCPS.

The final approval of the strategy (Santer's conditions) to deny DCPS an opportunity to respond in the normal way is acknowledged by Osborn to Santer and Jones (11 Jan 2008) in that Osborn writes that McGregor, as editor, is "prepared to treat it as a new submission rather than a comment on Douglass et al." and "my [McGregor's] offer of a quick turn around time etc. still stands." Osborn also reminds Santer and Jones of the potential impropriety of this situation:

... 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 ...

Santer now informs the team that the strategy has been agreed to (11 Jan 2008). DCPS were never notified of these machinations, and it is clear that Santer's story of the situation was never investigated independently. In this long e-mail, the issues of radiosonde errors is discussed and the fact one dataset, RAOBCORE v1.4, is missing from DCPS. To explain briefly, Sakamoto and Christy (SC09, accepted in 2008 and appearing in 2009) looked closely at the ERA-40 Reanalyses on which RAOBCORE v1.3 and v1.4 were based. SC09 demonstrated that a spurious warming shift occurred in 1991 (a problem with a satellite channel: HIRS 11), which was then assimilated into RAOBCORE, producing spurious positive trends in the upper [troposphere](#) and lower stratosphere. SC09 had been working on this since 2006 when they first met, so they were aware of the problems at that time. Sherwood later comments (27 May 2008) on this evidence during the deliberations of Santer's publication, so the team was aware of the problem, too. Even though Santer had seen the DCPS addendum (sent to him by McGregor) with the explanation of the RAOBCORE problems as early as 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 ...

Another interesting comment here is that Santer does "NOT" want to "show the most recent radiosonde [balloon] results" from Hadley Center and Sherwood's IUK (i.e., withholding data that does not support his view). The reason is likely that these two datasets, extended out in time, provide even stronger evidence in favor of DCPS. The final paper cuts off these datasets in 1999.

Douglass becomes concerned that he has not heard a response from McGregor on the Addendum sent on 3 Jan 2008. He writes on 1 Apr 2008 as to the status of the Addendum. On 10 April 2008, McGregor responds that he has "great difficulty locating [the] addendum." Douglass responds with the file number sent back to Douglass from IJC defining the event on 3 Jan but attaches the Addendum again. This was obviously successful because that very day (10 Apr 2008), McGregor sends the Addendum to Santer to "learn your views." Santer is afforded the opportunity to comment on the DCPS Addendum. DCPS never hear from McGregor again concerning the Addendum.

McGregor informs Santer that he has received one set of comments (24 Apr 2008) and though he

... 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 of revisions.

-- Santer informs the team of the situation (24 Apr 2008). One wonders if there was any possibility that Santer's paper could have been rejected, given the many favors already extended to this submission. McGregor now knows in the Addendum what the core response to Santer et al. might be, yet he evidently drops it from consideration. At this point, DCPS are unaware of a response by Santer, as they were dealing with the RealClimate.org blog with this matter.

Santer is worried about the lack of "urgency" in receiving the remaining reviews and complains to McGregor (5 May 2008). He reminds McGregor that Osborn had agreed to the strategy that the "process would be handled as expeditiously as possible." McGregor hopes that the further comments will come within "2 weeks" or so. Osborn writes to McGregor on the next day (6 May 2008) that Santer's ninety-page article was much more than anticipated, implying that Santer is being rather demanding, considering how much has been done to aid him. One wonders why it should take ten months and ninety pages to show that any paper contained a "serious flaw" and why Santer et al. needed to be protected from a response.

A paper by Thorne now appears in Nature Geosciences which referenced the as-yet-unpublished paper by Santer et al. (including Thorne). Douglass writes to Thorne (26 May 2008) asking for a copy and is told (27 May 2008\_A) that he cannot do so because Santer is the first author. Douglass (27 May 2008\_B) points out to Thorne Nature's ethics policy --

### ***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 .."*

-- and asks again for a copy of the paper. At the same time, Douglass asks Santer for a copy (27 May 2008\_C). Santer responds by saying, "I see no conceivable reason why I should now send you an advance copy of my IJoC paper." From the e-mails, we now know that the Santer et al. manuscript had not been accepted at this point, though it was cited in a *Nature Geosciences* article. What is very curious is that in the e-mail, Santer claims that Douglass did

... not have the professional courtesy to provide me with any advance information about your 2007 IJoC paper ...

In fact, Santer had been a reviewer of this paper when it had been submitted earlier, so he was in possession of the material (only slightly changed) for at least a year. Additionally, Santer received a copy of the DCPS page proofs about a week before it even appeared online.

In further e-mail exchanges the next day (28 May 2008) the author team discusses this uncomfortable situation of having a citation in *Nature Geosciences* and being unable to provide the paper to the public before "a final decision on the paper has been reached." Santer states they should "resubmit our revised manuscript to IJoC as soon as possible," which implies that Douglass's point about the **ethics policies** of *Nature*, which likely requires the availability of cited literature, may put them in jeopardy.

Santer writes to Jones (10 July 2008) that the two subsequent reviews are in, but reviewer number two was "somewhat crankier." Santer indicates that McGregor has told him that he will not resend the coming revised manuscript to the "crankier" reviewer in another apparent effort by McGregor to accommodate Santer.

## **Conclusion**

On 21 July 2008, Santer hears that his paper is formally accepted and expresses his sincere gratitude to Osborn for "all your help with the tricky job of brokering the submission of the paper to IJoC." Osborn responds that "I'm not sure that I did all that much."

On 10 Oct 2008, the Santer et al. paper is published online. Thirty-six *days* later Santer et al. appears in print, immediately following DCPS, who have waited now over eleven months for their paper to appear in print. The strategy of delaying DCPS and not allowing DCPS to have a simultaneous response to Santer et al. has been achieved.

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