<u>Comments on the Climategate 2.0 Emails</u>

On February 11, 2010, Peabody Energy Corporation ("Peabody") filed an extensive Petition for Reconsideration of U.S. EPA's final action entitled, "Endangerment and Cause or Contribute Findings for Greenhouse Gases under Section 202(a) of the Clean Air Act," 74 Fed. Reg. 66496 (Dec. 15, 2009) (the "Endangerment Finding"). Peabody's reconsideration petition ("Petition") was largely based on information and materials that became available as a result of Climategate—the release on the Internet of a trove of email communications and documents from the University of East Anglia's ("UEA") Climatic Research Unit ("CRU") in early November of 2009.[1] The CRU emails garnered significant attention among the media, the blogosphere, professional and lay scientists, and government agencies because they revealed, for the first time, disturbing and highly questionable practices and actions by leading climate scientists and researchers employed at UEA-CRU and their colleagues at other leading academic facilities.[2] Indeed, certain government agencies held hearings on, or conducted inquiries into the circumstances of the release.

In its reconsideration petition, Peabody brought these questionable practices and activities to the attention of EPA. Peabody argued that the CRU material significantly weakened EPA's reliance on the work of the United Nations Intergovernmental Panel on Climate Change ("IPCC") as support for EPA's Endangerment Finding. The very scientists implicated in the Climategate scandal were heavily involved in all stages of the production of the IPCC's Fourth Assessment Report ("AR4") and otherwise highly influential in the workings of the IPCC and

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[1] *See* "Scientist steps down during e-mail probe; Hacked messages about global warming caused controversy," *Washington Post* (Dec. 4, 2009).

[2] *See* "Investigator Assigned To Review Climate Research" *New York Times* (Dec. 4, 2009).

climate science. The analyses, conclusions, and recommendations contained in AR4 concerning purported greenhouse gas emissions, climate change, and related effects of global warming served as the cornerstone for EPA's Endangerment Finding.

Citing numerous CRU emails as first-hand evidence, Peabody showed that leading CRU researchers and their colleagues engaged in dubious academic and professional behavior, including: (1) stonewalling requests for data and information under freedom of information laws and advocating the destruction of information; (2) presenting scientific conclusions in order to advance specific objectives, namely demonstration of a late 20th Century warming trend—despite evidence to the contrary; (3) stifling legitimate debate about scientific conclusions and blackballing scientists who did not fully subscribe to the purported "consensus" view of recent climate change; (4) manipulating the peer-review process of academic publishing to ensure preferable treatment of favored colleagues and/or favored papers; and, (5) manipulating the IPCC process during the preparation of AR4 to incorporate academic papers that supported pre-conceived conclusions about climate change. Peabody urged EPA to reconsider the Endangerment Finding in light of the CRU revelations because the revelations seriously undermined the credibility of leading climate scientists and the IPCC, thereby casting substantial doubt on the validity of the Endangerment Finding.

On July 29, 2010, EPA denied Peabody's petition.[3]

Approximately two years after the original Climategate emails appeared on the Internet, a second, even larger release of material from CRU became available online in late October 2011.[4]

[3] *See EPA's Denial of the Petitions To Reconsider the Endangerment and Cause or Contribute Findings for Greenhouse Gases Under Section 202(a) of the Clean Air Act,* 75 Fed. Reg. 49556 (Aug. 13, 2010).

Although this installment contained nearly five times the number of emails – approximately 5,000 in total[5] – the release generated less coverage in the mainstream media. Peabody has reviewed the contents of Climategate 2.0 and the emails are every bit as damaging to the credibility of leading climate scientists, if not more so. In these comments, Peabody focuses on discussing five themes that are readily apparent in the Climategate 2.0 emails: (1) the Divergence Problem; (2) evasion of freedom of information act requests and possible destruction of responsive material; (3) internal expressions of doubt among the leading climate scientists as to the validity and accuracy of their work (which are not shared with the public); (4) a concerted effort to marginalize and discount the Medieval Warm Period; and, (5) disdain and contempt for lesser-known scientists and their work, especially work that reached different conclusions about purported recent climate change. In short, the second installment of Climategate emails provides a strong basis for renewed scrutiny of the Endangerment Finding.[6]

1. The Divergence Problem.

In emails going back to the late 1990s, leading climate scientists raise and discuss the "divergence problem" in paleo-climatology. The divergence problem refers to historical temperature reconstructions based on tree ring analysis that fail to correspond to instrument-

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[4] *See* "Hacker releases new batch of climate scientists' e-mails," *Washington Post* (Nov. 23, 2011); "New Trove of Stolen E-Mails From Climate Scientists," *New York Times* (Nov. 23, 2011).

[5] The Climategate 2.0 emails are available at the following Internet addresses: http://climategate2011.blogspot.com/ and http://foia2011.org/index.php?id=4 (last accessed June 20, 2012).

[6] Peabody provides two appendices with this report that organize and further summarize the Climategate 2.0 emails. Appendix A consists of two parts, which cover, respectively, emails preceding the IPCC's Third Assessment Report ("TAR") and Fourth Assessment Report ("AR4"). Appendix B covers emails from 2008 and 2009.

based temperature records of warming in recent decades. The tree ring records *diverge* in this respect from temperature measurements, and generally show a marked cooling trend after 1950 that is contrary to instrument-based observations. This phenomenon presents a significant problem for the reliability of tree ring reconstructions because it means that similar behavior may have existed in the past, such that tree ring reconstructions may *understate* temperature increases in the pre-instrumental time period. *See* Petition at IV.C.2. This problem was glossed over in AR4, which concluded that it was "likely" that temperatures during the last 50 years of the 20th century in the Northern Hemisphere were the highest during the last 1,300 years.

The Climategate 2.0 emails undermine the "consensus" view and show that leading climate scientists were aware of the divergence problem prior to, and during the preparation of AR4. Indeed, divergence created significant controversy and doubt among the leading scientists, and eventually led Keith Briffa, a leading expert on tree ring-based paleo-climatology to propose, nearly ten years after the fact, a special initiative with the United Kingdom's National Environmental Research Council ("NERC") to study the problem. What is most disturbing, however, is that gate-keepers of the "consensus" view allowed the divergence problem to fester for more than ten years, finally owning up to the fact that in 2009, they still had no convincing explanation. The initial email discussed below, and the final emails are compelling "bookends" for this disturbing chapter of climate science.

An early email sets the stage for the controversy that would eventually erupt from the divergence problem. In this note from Sept. 1999 (no. 3357), Tim Osborn responded to an email from Julie Jones, at the time a CRU colleague, about tree ring data that he, Phil Jones and Keith Briffa were working with. Osborn was apparently optimistic about the data, but then disclosed that the divergence problem required a fundamental leap of faith:

> ***
> I should tell you that there is a fairly strong temperature signal in the tree-ring density series, ***but that a non-temperature trend is also apparent post-1950 that gets bigger and deteriorates the temperature relationship.*** This makes calibration somewhat harder (hence I've been working on them for 2 years...!), ***but you also have to make the assumption that this non-temperature signal is something g anthropogenic and didn't occur in the past.***

Osborn's leap of faith, of course, proved to be a giant one, which was marginalized and ignored for considerable time.

Another early email from the Climategate 2.0 database suggests awareness and concern about tree ring proxies. Yet, these concerns were never fully explored before tree ring proxies were offered as conclusive evidence of a decidedly cooler past climate. In this email, Fritz Schweingruber wrote to Keith Briffa on October 5, 1999 (no. 4453):

> It was a good idea to sensilibise some Americans for boreal dendroclimatic studies. I think that also the densitometric labs from Marseill (Tessier) and Quebec (Payette) should be included. It is a fascinating idea to bring all dendroclimatologists und on umbrella with two topics:
> − ***what about the last 40 years?***
> − millenial growth/climate fluctuations
> ***One problem is still not solved or even not discussed. The European foresters clearly say that forests are growing more and more − no downward trend since 1960. We should bring this problem on the table.*** Recently several foresters at the WSL addressed the proplem. [sic] I initiated now a PhD. thesis in this respect. I gatherd hundrets [sic] of modern (mostly ring width) chronologies from France, Switzerland and Germany from lowland and subalpine reginos, [sic] from dry and normal sites. Burkhard Neuwirth from Bonn has ***to address this problem*** besid [sic] the spatial pointer year analysis. Perhaps we should include few laboratories with have large data bases.
> Yours Fritz
> ***What do you think, should we address this discrepancy***

An early exchange between Jeff Severinghaus of the University of California-San Diego and Michael Mann and Phil Jones, dated Feb. 3, 2003 (no. 0019), captures what were apparently obvious problems with tree ring proxy reconstructions in studies dating well-before AR4. Mann and Jones, however, dismissed and deflected troubling inquiries. After seeing a presentation by Thomas Karl, Mr. Severinghaus asked the following:

> I enjoyed your presentation yesterday at the MIT Global Change forum. *You may recall that I asked about the failure of tree rings to record the 20th century warming. Now that I look at my records, I realize that I remembered this wrongly:* ***it is the LATE 20th century warming that the Tree rings fail to record,*** and indeed, they do record the early 20th century warming. *If you look at the figure in the attached article in Science by Briffa and Osborn,* ***you will note that tree-ring temperature reconstructions are flat from 1950 onward. I asked Mike Mann about this discrepancy at a meeting recently, and he said he didn't have an explanation. It sounded like it is an embarrassment to the tree ring community*** *that their indicator does not seem to be responding to the pronounced warming of the past 50 years.* Ed Cook of the Lamont Tree-Ring Lab tells me that there is some speculation that stratospheric ozone depletion may have affected the trees, in which case the pre-1950 record is OK.
>
> * * *
>
> ***Personally, I think that the tree ring records should be able to reproduce the instrumental record, as a first test of the validity of this proxy. To me it casts doubt on the integrity of this proxy that it fails this test.***

Alarmed, Karl wrote back, suggesting that the problem was a lack of tree-ring *samples,* after 1980, not that the more recent samples were, in fact, diverging from the instrument record—which they were:

> *Correct me if I am wrong, but I always thought the failure was a lack of tree cores subsequent to the 1980s. Please correct me if I am wrong, and* ***if Jeff is correct, then indeed we have a significant implication.***

At this point, Michael Mann intervened to snuff out the controversy, sending a few passing insults to Mr. Severinghaus:

> Have no fear, Jeff [Severinghaus] has still got his facts wrong, even after going back and checking once...
>
> First off, I never made any such comment to Jeff–he clearly misunderstood comments that I made at EGS a year ago in response to a question he asked. Of course, it is well know [sic] that ***there are a number of competing explanations*** [this is what I said––to quote this as offering "no Explanation" is a bit unfair Jeff, don't you think? As I recall, I even invited Tim Osborn in the audience to add his own comments––but he had little to say] for ***the fact that *high latitude*, primarily *summer responsive*, tree-ring *density* data have exhibited a noteable decline in the past few decades in the amplitude of their response to temperature variability.*** We have discussed this issue time and again in our own work, and Keith Briffa, Malcolm Hughes, and many others have published on this, *w/ competing possible explanations (stratospheric ozone changes, incidentally, is the least plausible to me of multiple competing, more plausible explanations that have been published).*
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> * * *
>
> There are some good reasons that some of the other purely tree-ring based reconstructions differ in their details, in addition to the greater influence of the recent high-latitude density decline issue, and these are discussed in IPCC and the Science piece.
>
> * * *
>
> I know that Jeff has seen me talk on this many times, and probably has read our work (I would hope), so I'm frankly a bit disappointed at the comments. ***I would have liked to think that he would have approached us first, before broadcasting a message full of factual errors.***

There are several disturbing aspects of Mann's email, but perhaps the most is Mann's insistence that a number of *competing explanations*—some of which he dismisses as being implausible—provide an *answer* to the divergence problem, as opposed to simply creating more doubt. Somewhat more politely, Phil Jones chimes into the discussion to blame poor explanations provided in the past:

> Mike's answer is a fair response. Jeff has mixed some facts up and this is maybe because we've never explained them clearly enough. There are two facts:
>
> 1. There are few tree-core series that extend beyond the early 1980s. This is because many of the sites we're using were cored before the early 1980s. So most tree-ring records just don't exist post 1980.
>
> 2. The majority of the recent warming is post-1980, so no proxy would pick this up. This warming has been large and ***it would be good to go back and see if the trees have picked it up. It would give more faith in tree-ring reconstructions,*** but any reconstruction method is being pushed to the limit by the rate of temperature rise over the late 20th century.
>
> Applies to other proxies but you have to note the following: It is important to remember that ***locally few regions exhibit statistically significant warming.*** *Highly significant at the hemispheric level, but not great at the local level due to high level's of variability.* The spatial scales are important and this is difficult to get across.

Undaunted by Mann's condescension and apparently still confused by Jones' reply,

Severinghaus responds:

> Please accept my apologies if I have gotten the story wrong. I am not a specialist in the tree-ring field, and ***was simply reporting what I saw in the Briffa and Osborne paper, several other papers, and what several tree-ring people have told me in conversations.*** I agree, we need to keep the level of misinformation out there down to a minimum! I regret adding to it.
>
> ***I am still confused, however, about Mike's explanation for the Briffa and Osborne paper's curve appearing flat after 1950 AD.*** Can you try explaining this again, Mike, please? I don't understand how aligning could change the slope of a curve. *The curves appear to continue to 1990 AD or so, and the Esper et al. curve continues to 1993. So the explanation that the records only go up to 1980 doesn't seem to hold in this case. The dashed black line is the instrumental record for warm−season >20 N latitudes and it does indeed diverge from the tree-ring records in the 1980s. Can you help me out here?*

The persistent problem of divergence continued to trouble the leading paleo-climatologists in the period of the preparation of AR4, and the Climategate 2.0 emails capture parts of the discussion. For example, in an email from the pre-AR4 period, dated July 18, 2005 (no. 0316), Keith Briffa wrote to Tom Wigley about certain temperature proxies. He observed,

> ***
> The scaling of the data we used to produce the Crowley curve that formed one of the lines in our spaghetti diagram (that we put on the web site under my name and made available to NGDC), was based on taking the unscaled composite he sent and re-calibrating against April − Sept. average for land North of 20 degrees Lat., ***and repeating his somewhat bazaar [sic] calibration procedure (which deliberately omitted the data between 1900−1920 that did not fit with the instrumental data*** (remember his data are also decadal smoothed values).

In March 2006 (no. 0237), Keith Briffa and Tim Osborn had an exchange with Diane Gustafson of National Academy of Sciences/National Research Council, in which they responded to a question from Michael Wallace, a colleague of Gustafson. The question concerned proxy data selection in a paper offered by Briffa and Osborn. Wallace wrote:

> Our National Research Council Committee on Surface Temperature Reconstructions has been considering your paper with Keith Briffa published in a recent issue of Science. Could you please elaborate on your criterion for selecting the proxy time series included in the analysis. We are interested in how you computed the correlation between the proxy time series and local temperature time series. Is the correlation based on filtered or detrended time series? ***How would you counter the potential criticism that your selection method tends to favor proxy time series that show a strong 20th century warming?***

Briffa and Osborn emailed a lengthy joint response. Key excerpts are provided below, including Briffa's admission that he made a conscious decision not to "overplay" divergence in the preparation of AR4, suggesting that error ranges for the proxy records sufficiently addressed the divergence problem. The joint response also indicates that Briffa and Osborn excluded proxy-series that did not track the temperature record:

> We decided, therefore, to make use of as many of the individual records used in almost all the previously published NH temperature reconstructions, ***excluding any records for which an indication of at least partial temperature sensitivity was lacking.*** So, very low resolution records *for which comparison with instrumental temperatures is problematic* were excluded.
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> * * *
>
> ***We excluded records that did not show a *positive* correlation with their local temperatures.*** The remaining set includes most of the long, high resolution records used by others, such as Moberg et al., Crowley and Lowery, Hegerl et al., Mann, Bradley and Hughes, etc. as well as by Mann and Jones and Esper et al.
>
> *The final question, regarding the selection method favouring records that show a strong 20th century warming trend,* **is a more philosophical issue.** As stated above, we did not actually use strongly selective criteria, ***preferring to use those records that others had previously used and only eliminating those that were clearly lacking in temperature sensitivity.*** To some extent, therefore, the question is then directed towards the studies whose selection of data we used. *Certainly we did not look through a whole host of possibilities and just pick those with a strong upward trend in the last century! And we don't think the scientists whose work we selected from would have done this either. There are very few series to choose from that are >500 years long and are from proxy types/locations where temperature sensitivity might be expected.* It would be entirely the wrong impression to think that there are 140 such a priori suitable possible series, and that we picked (either explicitly or implicitly) just those 10% that happened by chance to exhibit upward 20th century trends.
>
> The correlation with local temperature is an entirely appropriate factor to consider when selecting data; these could be computed using detrended data, though for those that we calculated, our use of unfiltered data means that the trend is unlikely to dominate the correlation. One would need to inspect the trend in the temperature data at each location to evaluate how much influence it would have on the results; *but in locations where a strong upward trend is present, it would be right to exclude proxy records that did not reproduce it, though also correct that a proxy shouldn't be included solely on the basis of it having the trend,*

> especially where the proxy resolution is sufficient to test its ability to capture shorter term fluctuations.
>
> * * *
> I would also like to take the opportunity, if you will allow, to comment briefly on some reports that have reached me concerning the contribution made by Rosanne D'Arrigo to your Committee. Apparently, this is being interpreted by some as reflecting adversely on the validity of numerous temperature reconstructions that involve significant dependence on tree-ring data. *This is related to Rosanne's focus in her presentation on the apparent difference between measured temperatures and tree growth in recent decades – a so-called "divergence" problem.*
>
> * * *
> ***It was my call not to "overplay" the importance of the divergence issue,*** knowing the subtlety of the issues, in the fortcoming IPCC Chapter 6 draft. We did always intend to have a brief section about the assumption of uniformitarianism in proxy interpretation , including mention of the possible direct carbon dioxide fertilization effect on tree growth (equally controversial), but it is likely to conclude that here as well , there is no strong evidence of any major real−world effect. ***This and the divergence problem are not well defined, sufficiently studied, or quantified to be worthy of too much concern at this point.*** The uncertainty estimates we calibrate when interpreting many tree-ring series will likely incorporate the possibility of some bias in our estimates of past warmth, but these are wide anyway. This does not mean that temperatures were necessarily at the upper extreme of the reconstruction uncertainty range 1000 years ago, any more than they may have been at the bottom.

In February of 2006 (no. 1341), Tim Osborn had the following exchange with Peter Stott of the U.K.'s Met Office, and he copied Keith Briffa:

> Hi Peter − thanks for your interesting question [see below]. I don't think we can rule out systematic bias in the proxies in the most recent decades, but random noise in the proxies is also capable of producing such a deviation, given that the noise could be autocorrelated and anyway we are working with 20-year smoothed results and *the number of proxy records drops from 14 to 5 over the final few decades through to 1995 (the instrumental data are also included up to 2004, covering more of the warmest period). There's still more work to be done, but we really need more long proxies and more brought up to date.* Cheers, Tim

> Hi Tim,
>
> I enjoyed reading your paper in Science today. ***One issue I was interested in was the separation in fig 3D between the instrumental data and the proxy data.*** You comment in the paper that this could be expected consequence of noise in the proxy records *but naively it looks like there might be something more systematic in the last few decades.* Are you able to rule out systematic non temperature effects on the proxies in recent decades then ?
>
> Thanks !
> Peter

In a later email, Tim Osborn wrote to his colleague, Tom Kleinan on Dec. 20, 2006 (no. 4005):

> ***
> Because every grid box contains a tree-ring chronology, there is less extrapolation/interpolation and therefore it's more appropriate for comparison with models. Unfortunately we haven't yet published the details of how the gridding and calibration were done. ***Also we have applied a completely artificial adjustment to the data after 1960, so they look closer to observed temperatures than the tree-ring data actually were–don't rely on the match after 1960 to tell you how skilfull [sic] they really are!***

The shared uncertainty over the divergence problem culminated in an initiative to undertake a comprehensive review of dendro-climatology, proposed in late 2009. The proposal reveals a significant degree of uncertainty as to the real validity of tree ring proxy reconstructions, amounting to a soup-to-nuts review of the state of the science. Several emails illustrate the massive problem this posed for "consensus" scientists. For example, Tim Osborne wrote to a colleague in an email dated July 29, 2009 (no. 2836):

> ***
> In some northern areas of the world, recent ***observations of tree growth and measured temperature trends appear to have diverged in recent decades, the so called "divergence" phenomenon.*** There has been much speculation, and numerous theories proposed, to explain why the previous temperature sensitivity of tree growth in these areas is apparently breaking

> down. ***The existence of divergence casts doubt on the uniformitarian assumption that underpins a number of important tree-ring based (dendroclimatic) reconstructions. It suggests that the degree of warmth in certain periods in the past, particularly in medieval times, may be underestimated or at least subject to greater uncertainty than is currently accepted.*** The lack of a clear overview of this phenomenon and the lack of a generally accepted cause had led some to challenge the current scientific consensus, represented in the 2007 report of the IPCC on the likely unprecedented nature of late 20th century average hemispheric warmth when viewed in the context of proxy evidence (mostly from trees) for the last 1300 years.
>
> *This project will seek to systematically reassess and quantify the evidence for divergence in many tree-ring data sets around the Northern Hemisphere. It will establish a much clearer understanding of the nature of the divergence phenomenon, characterising the spatial patterns and temporal evolution.* Based on recent published and unpublished work by the proposers, it has become apparent that foremost amongst the possible explanations is the need to account for systematic bias potentially inherent in the methods used to build many tree-ring chronologies including many that are believed to exhibit this phenomenon.

In an email dated Sept. 17, 2009 (no. 0232), Keith Briffa and Tom Melvin announced acceptance of the project to a number of their colleagues:

> We are writing now to inform you that our application to the UK NERC for support to investigate the so-called "Divergence" phenonomen in temperature-sensitive trees over a range of geographical and ecological situations has formally been approved. This message is addressed to those of you who generously offered support and indicated willingness to collaborate with us in this work. We were and are grateful and excited by the prospect of our collaboration.

A later email from October 14, 2009 (no. 2881) from Tom Melvin at UEA to Keith Briffa captures no less than ***nine*** issues related to divergence that were still troubling the field, shortly before the initial Climategate scandal broke:

> ***CHALLENGES POSED BY DIVERGENCE***
> 1. Problem with curve-fitting e.g. Hugershoff (Briffa 1998) and trend distortion − part solution Signal free.

2. Problem with mixing sloping and horizontal curve fitting in Arstan (e.g. D'Arrigo 2004) − part solution RCS.
3. End effect problems with RCS (Briffa − Hughes book) − e.g. sample bias
4. Problem with updating chronologies (TTHH and Grudd 2008, Tornetrask)
5. Potential problem with Crown dieback (e.g. responders / non responders)
6. Potential MXD in sapwood problem ????
7. Potential competition problem − tree density changes RCS shape (Helama 2006)
8. Problem with non−linear response / skewed index distribution (Barber, Wilmking etc)
9. Remove all these and residual is real divergence − problem with identifying cause:

CO2 change / Nitrogen fertilisation / Global dimming / UV light / Drought stress/

Conclusion − Lots of work to do to clarify situation.

2. Stonewalling of FOIA Requests.

In the wake of the now-notorious "hockey stick" illustration and related publicity following former Vice-President Al Gore's movie "An Inconvenient Truth," amateur and trained scientists, academics, and other interested persons began to request data and information directly from leading climate scientists in order to better understand the underpinnings of the hockey stick and "consensus" conclusions about unprecedented warming in the late 20th century. Two early investigators were Steve McIntyre, a trained mathematician and economist, and Ross McKitrick, an economics professor at the University of Guelph. McIntyre and McKitrick were not the only persons seeking information, and many of these requests are captured in the Climategate 2.0 emails.

Peabody's reconsideration petition showed that leading climate scientists, threatened by enhanced scrutiny and ensuing criticism of their work, eventually engaged in a concerted practice of stonewalling requests for data and information, including those submitted under freedom of information laws of the U.S. and the U.K. Petition at VI.H. The Climategate 2.0 emails further demonstrate this troubling practice and substantiate the likely destruction of emails or other data.

For example, in an email dated Oct. 19, 2003 (no. 1566), Michael Mann warned Phil Jones about inquiries from McIntyre related to the hockey stick paper and underlying data. He stated that he regretted providing data to McIntyre and characterized him as a "shill for industry." Mann also revealed that he installed an automatic filter in his email to screen out inquiries that he believed were coming from so-called skeptics:

> FYI--thought you guys should have this (below). *This guy "McIntyre" appears to be yet another shill for industry−he appears to be the one who forwarded the the [sic] scurrilous "climateskeptic" criticisms of the recent Bradley et al Science paper.* Here is an email I sent him a few weeks ago in response to an inquiry. It appears, by the way, that he has been trying to break into our machine ("multiproxy"). Obviously, this character is looking for any little thing he can get ahold of.
>
> * * *
> *The best that can be done is to ignore their desperate emails and, if they manage to slip something into the peer-reviewed literature, as in the case of Soon & Baliunas, deal w/ it as we did in that case*—i.e., the Eos response to Soon et al−−they were stung badly by that, and the bad press that followed.
>
> * * *
> p.s. *I'm setting up my email server so that it automatically rejects emails from the "usual suspects".* You might want to do the same. As they increasingly get automatic reject messages from the scientists, they'll start to get the picture...

Jones responded to Mann's email the next day (no. 1566), stating that he has simply stopped responding to McIntyre's requests. At this point, Jones did not appear to adopt the draconian recommendation of his colleague:

> I've had several emails from Steve McIntyre. He comes across in these as friendly, but then asks for more and more. I have sent him

> some station temperature data in the past, but eventually had to stop replying to me. Last time he emailed me directly was in Relation to the Mann/Jones GRL paper. That time he wanted the series he used. I suspect that he is the person who sent the email around about only 7 of the 23 series used by Ray et al. being in WDC-Paleo. I told him then that he needs to get in contact with the relevant paleo people.

Reflecting on this period – in the context of providing information for a story to be reported by Olive Heffernan of *Nature* – Jones wrote the following in an email to Heffernan dated Aug. 9, 2009 (no. 3497):

> I did send some of the data to a person working with Peter Webster at Georgia Tech. The email wasn't to PW, but he was in the CC list. *I don't know how McIntyre found out, but I thought this was a personal email.* This was one of the first times I'd sent some data to a fellow scientist who wasn't at the Hadley Centre. As I said I have taken pity on African and Asian PhD students who wanted some temperature and precipitation data for their country. *The email has only gotten me grief, so this is another reason for being much less helpful to people emailing CRU.* ***This goes against my nature, but I've been driven to it. You'd better not say this, otherwise McIntyre will request the emails where to prove I've been unhelpful!***
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> * * *
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> ***I also don't see why I should help people, I don't want to work with and who spend most of their time critisising [sic] me. Years ago I did send much paleo data to McIntyre but have also had nothing but criticism on his blog ever since.*** *As I said, this criticism on blog sites is not the way to do science.* If they want to engage, they have to converse in civil tones, and if people don't want to work with them, they have to respect that and live with it.

After the leading climate scientists stopped communicating with trained and amateur scientists and other interested persons who requested data informally, these individuals turned to freedom of information laws to support their requests. The Climategate 2.0 emails document the response of the leading scientists, such as Mann, Briffa, and Jones. In an email dated June 20, 2007 (no. 1506), Eugene Wahl wrote to Phil Jones about inquiries from McIntyre, critical postings on McIntyre's "ClimateAudit.com" website, and FOIA requests:

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

An email exchange between Phil Jones and David Jones, an Australian scientist, dated Sept. 7, 2007 (no. 0601) illustrates the attitude toward sunshine laws among consensus scientists. Phil Jones wrote:

> Hi David, Shoni tells me you're having to respond to some skeptics. I commiserate with you!
>
> * * *
> ***All this stems from a number of Freedom of Information requests we've had in the UK. I've stuck to my principles and said I won't be releasing the station data ,*** but instead will be putting this list up. The requester agreed to these fields. They didn't ask for the years of record for each site, nor would I have provided this. ***So, in a way, it is useless.***
>
> * * *
> As another aside, I did respond to another FOI request. This related to this paper Jones, P.D., Groisman, P.Ya., Coughlan, M., Plummer, N., Wang, W–C. and Karl, T.R., 1990: Assessment of urbanization effects in time series of surface air temperature over land. Nature 347, 169–172. This has resulted in a fraud allegation

> by one of the skeptics, which is being dealt with! Don't mention this to anyone at the moment − except Neil Plummer and Mile Coughlan.

Perhaps somewhat taken aback by the tone of this email, David Jones replied:

> Thanks Phil for the input and paper. I will get back to you with comments next week. Fortunately in Australia our sceptics are rather scientifically incompetent. ***It is also easier for us in that we have a policy of providing any complainer with every single station observation when they question our data (this usually snows them)*** and the Australian data is in pretty good order anyway.

In July 2008 (no. 2094), there was a discussion among UEA staff as to freedom of information requests. Briffa argued against any release, and apparently moved his IPCC-related emails to a private storage location, as a protective measure:

> ***
> While I believe UEA should not be in any way responsible for our academic opinions, it should take responsibility for our right to academic freedom. This is why I am arguing that we (UEA and authors) should not release our emails − regardless of whether they are held at UEA, in principal or in substance. ***Incidentally . [sic] UEA does not hold the very vast majority of mine anyway which I copied onto private storage after the completion of the IPCC task.***

This view apparently gained currency, as revealed in an email from Phil Jones to Gavin Schmidt, dated Aug. 20, 2008 (no. 1492) (which was captured in the initial Climategate release):

> ***
> 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.***

By December of 2008, the leading UEA scientists had made up their minds on one approach to FOIA requests – deletion of emails. In this message from Tim Osborn to Phil Jones, dated Dec. 8, 2008 (no. 3791), Osborn joked with Jones about Jones' deletion of emails in order to avoid FOIA, referring tongue-in-cheek to "a spring clean of various other emails that hadn't been requested, as part of your regular routine of deleting old emails." The full text of the email strongly suggests that leading scientists were, in fact, deleting emails to evade FOIA:

> Hi Phil!
> re. your email to Dave Palmer [which he copied in his response to you and cc'd to me, Keith & Michael McGarvie, and which has hence already been multiply copied within the UEA system, and therefore will probably exist for a number of months and possibly years, and could be released under FOI if a request is made for it during that time!]... ***I assume that you didn't delete any emails that David Holland has requested (because that would be illegal) but that instead his request merely prompted you to do a spring clean of various other emails that hadn't been requested, as part of your***
> ***regular routine of deleting old emails.*** *If that is what you meant, then it might be a good idea to clarify your previous email to Dave Palmer, to avoid it being misunderstood.* :–)
>
> The way things seem to be going, I think it best if we discuss all FOI, EIR, Data Protection requests in person wherever possible, rather than via email. It's such a shame that the skeptics' vexatious use of this legislation may prevent us from using such an efficient modern technology as email, but it seems that if we want to have confidential discussions then we may need to avoid it. ***I shall delete this email and those related to it as part of my regular routine of deleting old emails!***
> Cheers
> Tim

By mid-2009, with preparations for the Fifth Assessment Report (AR5) underway, the leading scientists were sufficiently aware and alarmed by potential FOIA requests that Phil Jones actively advocated destruction of emails relevant to AR5. Jones wrote to Thomas Stocker in

May of 2009 (no. 2440) at the University of Bern (IPCC Working Group I co-Chair), giving him a brief primer and advice on FOIA:

> ***
> Below there is a link to Climate Audit and their new thread with another attempt to gain access to the CRU station temperature data. I wouldn't normally bother about this – but will deal with the FOI requests when they come. Despite WMO Resolution 40, I've signed agreements not to pass on some parts of the CRU land station data to third parties. If you click on the link below and then on comments, look at # 17. This refers to a number of appeals a Brit has made to the Information Commissioner in the UK. You can see various UK Universities and MOHC listed. For UEA these relate to who changed what and why in Ch 6 of AR4. *We are dealing with these, but I wanted to alert you to few sentences about Switzerland, your University and AR5.*
>
> * * *
> You might want to check with the IPCC Bureau. *I've been told that IPCC is above national FOI Acts.* ***One way to cover yourself and all those working in AR5 would be to delete all emails at the end of the process.*** *Hard to do, as not everybody will remember to do it.*

A few months later, on July 28, 2009 (no. 1577), Jones again wrote to several UEA colleagues about FOIA requests. He appears to be annoyed that the UK Met Office released a temperature data set in response to a request from Steve McIntyre:

> Dear All,
> Here are a few other thoughts. *From looking at Climate Audit every few days, these people are not doing what I would call academic research.* Also from looking they will not stop with the data, but will continue to ask for the original unadjusted data (which we don't have) and then move onto the software used to produce the gridded datasets (the ones we do release). CRU is considered by the climate community as a data centre, but we don't have any resources to undertake this work. ***Any work we have done in the past is done on the back of the research grants we get – and has to be well hidden. I've discussed this with the main funder (US Dept of Energy) in the past and they are happy about not releasing the original station data.*** We are currently trying to do some more work with other datasets, which will get released (as gridded datasets) through the British Atmospheric Data Centre

> (BADC). This will involve more than just station temperature data. Perhaps we should consider setting up something like this agreement below
> [1]http://badc.nerc.ac.uk/data/surface/met−nerc_agreement.html
> I just want these orchestrated requests to stop. I also don't want to give away years of hard effort within CRU. Many of the agreements were made in the late 1980s and early 1990s and I don't have copies to hand. I also don't want to waste my time looking for them. Even if I were to find them all, it is likely that the people we dealt with are no longer in the same positions. These requests over the last 2.5 years have wasted much time for me, others in CRU and for Dave and Michael. *Some of you may not know, but the dataset has been sent by someone at the Met Office to McIntyre. The Met Office are trying to find out who did this. I've ascertained it most likely came from there, as I'm the only one who knows where the files are here.*

Another email, dating from the same time period, contains another reference to deletion or destruction of emails in order to hide them from interested persons. Apparently, Phil Jones intentionally deleted data in order to prevent "skeptics" from having access to it. In this email, dated Aug. 24, 2009 (no. 1899), Harold Ambler replies to a communication from Jones, which is not captured in the Climategate 2.0 database. Mr. Ambler writes:

> I do not share the view that the days when amateurs contributed meaningfully to the development of science have come to an end. If you have studied the history of science, particularly that of your own great country, then you already know that non−academic, Frequently self-taught individuals have changed the scientific debate permanently in a given field because of their own (frequently scoffed-at) work. Your somewhat condescending position toward "non-scientists" is in keeping with Royal Society snobbery of the 19th century[.]
>
> You write, *"Our ftp site has had some data deleted from it. It is a site we use when working with other scientists around the world. The datasets were not explained. It seemed easier to stop people wasting their time trying to determine what it was."*
>
> I admit that this does not seem as straightforward as, again, one might expect from a public servant. ***The decision to delete data was made during a white-hot dispute with a little-liked and extremely dogged and intelligent statistician by the name of Steve***

> ***McIntyre.*** Whether or not you view Mr. McIntyre as the kind of figure whom the Royal Society fought to keep on the margins of scientific inquiry (or farther out than that), he is exactly such a figure. ***If you wanted to "defeat" him in intellectual battle, as you naturally would, the best way to do so is not to hide data and maintain that you are not hiding data. The data should be restored to the website, ASAP.*** Mr. McIntyre should be allowed to "audit" your methodology. If your intellectual position is truly superior to his, then the "schooling" that you give to him in response will be of note to many.

Jones promptly replied on Aug. 24, 2009 (no. 1338):

> Dear Harold,
> I realise [sic] that I have again wasted my time trying to respond to people who do not want to understand some simple arguments. You have put completely wrong motives to statements in my last email. ***I work in a University. In the UK I am not considered a public servant.*** Attached is another paper about the costs of climate data in Europe. It is the Met services that you should be lambasting. I have been for years, but have not gotten very far. It is better to work with them according to their rules.

Earlier in the same month, on August 5, 2009 (no. 856), Phil Jones wrote to his colleagues at UEA concerning changing the British freedom of information law so as to exclude universities:

> * * *
> FOI is causing us a lot of problems in CRU and even more for Dave, as he has to respond to them all. *It would be good if UEA went along with any other Universities who might be lobbying to remove academic research activities from FOI.* FOI is having an impact on my research productivity. I also write references for people leaving CRU, students and others. If I have to write a poor one, I make sure I get the truth to the recipient in a phone call. *I'm also much less helpful responding to members of the public who email CRU regularly than I was 2-3 years ago.* I've seen some of what I considered private and frank emails appear on websites. Issue here is blogsites have allowed these climate change deniers to find one another around the world.

3. Leading Climate Scientists' Internal Expressions of Doubt as to the Competence and Work Product of Other Leading Climate Scientists.

A prominent theme running through the Climategate 2.0 emails is the doubt that many leading climate scientists harbor as to the competence and work product of their equally prominent colleagues. These expressions of skepticism and misgiving about colleagues and their research take many forms, but frequently manifest as disdain for published papers and knowledge of uncorrected errors despite the peer review process. While these doubts are pervasive, they have clearly been kept below the surface for the sake of projecting and protecting the "consensus" view of warming in the 20th century and anthropogenic climate change. This section of Peabody's comments provides numerous examples of internal doubt, around specific topics.

a. Michael Mann's Work and the Hockey Stick.

There is recurrent doubt concerning the Hockey Sticky and the two papers providing the foundation: Michael Mann et al., *Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations,* 26 Geophys. Res. Lett. 759 (1999) ("MBH99"); and Michael Mann et al., *Global Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries,* 392 Nature 779 (1998) ("MBH98"). There is also recurrent criticism of Michael Mann's published work.

For example, Simon Tett of the U.K. Met Office wrote to Matt Collins, and copied Tim Osborn. The email, dated Aug. 25, 2001 (no. 0562), apparently relates to review of a paper submitted by Collins and editorial comments:

> Mat,
> The papers [sic] looks very good. Hope these comments aren't too late.... I don't think I need to see it again.
> Simon
>
> * * *
>
> Reviewer B.

> 1) Didn't see a justification for use of tree-rings and not using ice cores - ***the obvious one is that ice cores are no good*** - see Jones et al, 1998.
> 2) No justification for regional reconstructions rather than what Mann et al did **(*I don't think we can say we didn't do Mann et al because we think it is crap!*)**
> 3) No justification in the paper for the 9 regions. I think there is justification in the JGR Briffa paper.
> 4) That is a good point –– I would strongly suspect that the control has a lot less variance than the observations over the last century –– not the ALL run though!
> 5) No response to this in the paper. ***I suspect we are doing better stats than all the rest though!***

In late 2001, Ed Cook was moving forward with a paper that differed from MBH in its approach to tree-ring proxies. The Climategate 2.0 emails show that Keith Briffa, Michael Mann, and Malcolm Hughes became aware of his work and began to ask questions. Responding to Keith Briffa on Sept. 10, 2001 (no. 0639), he wrote:

> * * *
> Sorry for sounding a bit testy here. I've been fielding a whole raft of questions, comments, and criticisms from Mike Mann, Tom Crowley, and Malcolm Hughes. Some of them useful, many of them tiresome or besides the point. *I never wanted to get involved in this quixotic game of producing the next great NH temperature reconstruction because of the professional politics and sensitivities involved.*
>
> * * *
>
> ***(… I should also say that the amount of ignorance about tree rings in the global change/paleo/modeling community is staggering given what has been published. Like it or not, they simply don't read our papers.).***
>
> * * *
> This all reinforces my determination to leave this NH/global temperature reconstruction junk behind me once I get this paper submitted. It's not worth the aggravation. However, the paper is something that I need to do for Jan. And I still think it is a good paper.

In April of 2002, Keith Briffa became involved in a testy exchange with Michael Mann, concerning Briffa's comment on the work of another scientist. Mann's rebuke – "Sadly, your piece on the Esper et al paper is more flawed than even the paper itself." – was captured in the original Climategate files, but Briffa's response (no. 1272) was not. Briffa suggested that there were indeed flaws in Mann's work:

> ***I certainly do not consider that scaling any single limited-coverage (possibly seasonally biased) averaged record is an appropriate way of reconstructing Hemispheric temperature. This is just what several of the records do, though, certainly the original Bradley and Jones series, the Jones et al.series , and that of Crowley.*** *However, even your own series, prior to 1400, could be taken to represent a major western N. American bias as regards evidence of Hemispheric changes.* Finally, I have to say that I, for one, do not feel constrained in what I say to the media or write in the scientific or popular press, by what the skeptics will say or do with our results. We can only strive to do our best and address the issues honestly. Some "skeptics" have their own dishonest agenda − I have no doubt of that. *If you believe that I, or Tim, have any other objective but to be open and honest about the uncertainties in the climate change debate, then I am disappointed in you also.*

In another email from April 2002 (no. 4369), Tim Osborn and Ed Cook engage in a discussion of Mann's past work in connection with their own new work. Portions of the exchange are set forth below, including Cook's conclusion, which appears to be a reference to the hockey stick:

> *I will be sure not to bring this up to Mike. As you know, he thinks that CRU is out to get him in some sense.* So, a very carefully worded and described bit by you and Keith will be important. ***I am afraid that Mike is defending something that increasingly can not be defended. He is investing too much personal stuff in this and not letting the science move ahead.*** I am afraid that he is losing out in the process. That is too bad.

In an email dated June 17, 2002 (no. 5055), three of the leading climate scientists, Keith Briffa, Tim Osborn and Ed Cook trade emails related to a letter published by Michael Mann in *Science.* They express doubts as to the validity of Mann's past work, and his defensiveness:

> *I have just read this lettter − **and I think it is crap.** I am sick to death of Mann stating his reconstruction represents the tropical area just because it contains a few (poorly temperature representative) tropical series.* He is just as capable of regressing these data again any other "target" series , such as the increasing trend of self-opinionated verbage [sic] he has produced over the last few years , and ... ***(better say no more)***
> Keith
>
> Hi Tim,
> There was indeed a letter from Mike and Malcolm (the Prat; in Medieval times, that would have been his surname instead of Hughes) published in Science, with a reply from me. See below. ***In all honesty, I haven't even read what was published. I am tired of the whole thing. At every meeting I go to where Mike gives a talk, he always presents more on why his series is correct. Honestly, most people I talk to think that he is being way too defensive (as we all know too well).*** In any case, he is coming out with a new NH reconstruction. It will be interesting to see what it looks like. One problem is that he will be using the RegEM method, which provides no better diagnostics (e.g. betas) than his original method. So we will still not know where his estimates are coming from.
> Cheers,
> Ed

On April 29, 2003 (no. 1238), Keith Briffa wrote to Ed Cook:

> Thanks Ed
> Can I just say that I am not in the MBH camp − if that be characterized by an unshakable "belief" one way or the other , regarding the absolute magnitude of the global MWP. I certainly believe the " medieval" period was warmer than the 18th century − the equivalence of the warmth in the post 1900 period, and the post 1980s ,compared to the circa Medieval times is very much still an area for much better resolution. I think that the geographic / seasonal biases and dating/response time issues still cloud the picture of when and how warm the Medieval period was.

In an email dated July 15, 2003 (no. 0774), an exchange between Ed Cook and Keith Briffa shows that they were aware of flaws in the MBH work:

> Hi Keith,
>
> Thanks for the paper and help in toning down Mike's efforts to put a stake in the Esper heart. I quickly read the paragraph you mention. *Undoubtedly part of what is said is true, but it doesn't explain it all of the differences between the original MBH reconstruction and any of the other NH recons.* Now that Mike has moved on to a totally new NH recon, I suppose all of this is a mute [sic] point. ***However, your Blowing Hot and Cold piece clearly showed that the MBH estimates were undoubtedly deficient in low-frequency variability compared to ANY other recon.*** Enough said. I need to enjoy myself.
>
> Cheers,
> Ed

In yet another example, in an email to Ed Cook at Columbia University, dated Sept. 3, 2003 (no. 5036), Keith Briffa sought to actively exclude Michael Mann and Phil Jones from a project. The subject line is "Forgot" and the main body says:

> to say would prefer *no involvement of Mann and Phil − and can you tell me what reconstruction Bradley did ever ?* unless you mean the Bradley and Jones early decadal series?

In a later exchange, in December of 2003 (no. 3373), Ray Bradley expressed his doubts to Keith Briffa about Mann's work and his objectivity:

> Furthermore, the model output is very much determined by the time series of forcing that is selected, and the model sensitivity which essentially scales the range. ***Mike only likes these because they seem to match his idea of what went on in the last millennium, whereas he would savage them if they did not.*** *Also--& I'm sure you agree--the Mann/Jones GRL paper was* ***truly pathetic and should never have been published.*** *I don't want to be associated with that 2000 year "reconstruction".*

The Climategate 2.0 emails capture a later exchange between Simon Tett of the Met Office and several colleagues, concerning a new report on the McIntyre and McKitrick paper. Responding to a question about whether Mann "got it wrong", he wrote on October 14, 2004 (no. 0518):

> ***I think there are issues in Mann et al's approach*** –– recall the Esper et al paper which produced a reconstruction with lots more low frequency variability than others. From the comment on the paper by Keith Briffa and Tim Osborn (attached) you can see that Mann's reconstruction had the least variability of any of the reconstructions.
>
> * * *
>
> ***a) Did Mann et al get it wrong? Yes Mann et al got it wrong. How wrong is still under debate*** and the ECHO−G/HadCM3 results may be over-exaggerating the variance loss for some model-specific reasons.

In February of 2005, David Ritson, a physicist at Stanford University, wrote to Keith Briffa and Tim Osborn (no. 1667), commenting in particular on MBH98. In the middle of a sophisticated exchange, he offered some unvarnished criticism:

> ***My context is a belief that the climate field is losing and has lost a great deal of credibility over the years as to whether it is serious science.*** Practically any of my colleagues in the physics department would say that things ***are so politicized that they wouldn't know what to believe,*** but that, at some point, if you keep adding greenhouse gasl s [sic] you are going to have a problem. *The handling of millenium temperature records certainly lends support to this cynicism. In the MBH instance* ***virtually all the simple internal consistency checks. [sic] one should expect to find, are missing.***
>
> * * *
>
> *I failed to find a coherent description in the literature as to where and how MBH calibrated their data on an absolute scale.* Maybe they finally regressed their results relative to the observational data? *In that case yours and Von Storch et al work would be misleading. I had expected that you and/or Von Storch et al, could provide the answer to this most basic question, and*

> *e−mailed both of you, however to no avail (not that you both didn't try.).*
>
> * * *
>
> The above is not to say that thre isn't a lot of good work, Crowley, Esper etc. *I give M&M lots of credit for stirring things up but poor marks for their basic understanding and objectivity on many of the issues,* ***and the same goes for MBH.*** What is so damaging about the current debate as to whether current temperatures exceed anything in the past millenium ***is the poverty of the work and, by inference, the refereeing of it.*** *Final scientific answers seem out of current reach.*

In an email dated June 14, 2005 (no. 3353), Tom Wigley wrote to Keith Briffa, posing fairly direct questions about problematic differences in key temperature proxies used in the leading papers. He asked Briffa why there are such discrepancies and suggested that the differences create a serious problem for the literature:

> No doubt you have thought through this, *but what particular choice of input proxies makes the Esper curve in 1600-50 different from others (see attached)?* What is interesting is that Keith's curve is the only other one to show this. Briffa and Esper also are similar for dips around 1350, 1470, 1820 – so I presume they have data in common that is not expressed in the other curves. *I note, however, that Briffa and Esper are opposite in the second half of the 17th century. Any idea why there is this contrast with the early 17th century?* I realize that Esper is made up of different bits - but it does have some very odd behavior. For example, if I lowpass his annual data, then the amplitude of the low-frequ fluctuations that I get is noticeably less that what he has (i.e., as in the second attached plot). I guess there is some scaling done somewhere - ***which of course is statistically bogus.*** Since you have compared all these things before, I'm sure you have some answers. ***It seems to me that the radical differences between different data sets (notwithstanding the multiple reasons for differences) do not engender confidence in any of them. Comparisons with model results do not make things much better. These points seem to be glossed over in the literature*** - please tell me if this is a false impression on my part (since I would not want to propogate bad press in our review paper).

In March 2006, another UEA scientist, Rob Wilson wrote to Ed Cook and several colleagues of a hockey-stick inducing bias in Mann's work – referring to an issue identified by Steve McIntyre. Wilson discovered it while "playing around" with the data. His email, dated March 7, 2006 (no. 4241), and Cook's peculiar response are excerpted, below:

> Hi Rob,
> You are a masochist. Maybe Tom Melvin has it right:
> "Controversy about which bull caused mess not relevant [sic]**.** *The possibility that **the results in all cases were heap of dung** has been missed by commentators."*
> Cheers,
> Ed
>
> Greetings All,
> I thought you might be interested in these results. The wonderful thing about being paid properly (i.e. not by the hour) is that I have time to play. *The whole Macintyre issue got me thinking about over-fitting and the potential bias of screening against the target climate parameter. Therefore, I thought I'd play around with some randomly generated time-series and see if I could 'reconstruct' northern hemisphere temperatures.*
>
> * * *
> The results are attached. Interestingly, the averaging method produced the best results, although for each method there is a linear trend in the model residuals − perhaps an end−effect problem of over−fitting. ***The reconstructions clearly show a 'hockey−stick' trend. I guess this is precisely the phenomenon that Macintyre has been going on about.*** It is certainly worrying, but I do not think that it is a problem so long as one screens against LOCAL temperature data and not large scale temperature where trend dominates the correlation.

A later email suggests serious reconsideration and efforts by a wider circle of scientists to understand differences and problems among proxy series, including MBH. On June 23, 2006 (no. 1911), Tim Obsorn wrote to several colleagues about a meeting:

> The meeting included fairly intensive discussions about many issues, and this included some discussion of von Storch et al. (2004, 2006), Wahl et al. (2006), Mann et al. (2005), Burger and Cubasch (2005) and Burger et al. (2006). Generally the discussion

> was quite open, with only a few disdainful remarks made about the work of people not there - certainly not enough to distract from useful discussions. *In general, most people accepted that the MBH method could, in some situations, result in biased reconstructions with too little low-frequency.* I'm not sure how much Mike Mann accepted this, but it was reinforced by findings shown by Eugene Wahl that indicated some bias in their CSM pseudo-proxy studies, and particularly by Francis Zwiers who looked to have almost completely replicated the von Storch et al. results with respect to the MBH method (though he emphasised the preliminary nature of his work and he may not have implemented the MBH method correctly... we'll have to wait and see). Mike showed many detailed pseudo-proxy tests of the RegEM method and these seemed quite convincing in showing little problem with that method... it does assume equal error in both instrumental and proxies, so it should show less bias than other methods that wrongly put all the error in the instrumental record (i.e., "typical" regression). So... there was some confusion about how the MBH method can be biased but the RegEM not be biased (in pseudo-proxy tests) yet they give the same results for the real proxies. Mike thought it might be the ECHO−G vs CSM differences, but I argued against this and was supported by Caspar Ammann and Eugene Wahl who did not think that the character of the model runs was a big factor in explaining different results.

b. Models are not Sufficiently Sophisticated and Subject to Manipulation.

The Climategate 2.0 emails also demonstrate concerns of leading scientists – expressed internally – that their temperature and climate computer models are not sufficiently robust, and that output, meaning conclusions, can be too easily manipulated by the inputs. If this is indeed true, then the modeling-based predictions of future warming (and climate change) become subject to doubt, especially when it is apparent that many of the leading scientists have a "warmist" agenda. The emails also reveal reservations about the strength of models in predicting past climate.

For example, on March 11, 2004 (no. 4443), Phil Jones wrote to Tim Carter (now of the Finnish Environmental Institute) about a number of models and apparent discrepancies in their results. He copied Mike Hulme of UEA:

> I've sent an email to Tim Mitchell for his thoughts (and asked him what the new job is like). ***I'm not surprised by what you've found – i.e. the large inter-model differences.*** In the EU-project SWURVE, we've gone back to calculating PET (assuming this is why you want a humidity type variable) with Thornthwaite and Blaney/Criddle as they only depend on temperature. This is being written into project final report and the special issue of HESS (Hyd. and Earth System Science). Project run by Chris Kilsby and he's arranged this issue. Even with HadCM3 with small changes in vapour pressure (well in HadAM3P/HadRM3P – same there also), the increasing temperature means that vapour pressure deficit becomes very large, so PET calculated with Penman formula is ridiculous. If this is why you want vapour pressure I would suggest you go down this route also. Happy for you to forward this to Nigel as he'll understand what I'm on about. Hydrologists know that Penman should be best, but not with models. ***Even for 1961–90 the problem can be seen in the warmer summers. Basic problem is that all models are wrong – not got enough middle and low level clouds. Problem will be with us for years, according to Richard Jones.*** Chris has talked to him About it at length. It looks as though CSIRO2 may be the best one. CGCM2 looks most odd.

On January 11, 2005 (no. 5156), David Rind at NASA wrote to Stefan Rahmstorf, a lead IPCC author, and to Jonathan Overpeck, expressing caveats as to the strength of modeling the paleo-climate. His qualification and the proposed text of AR4 are set forth below:

> *Sorry, but I don't think the first comment is either necessarily true or helpful to the chapter.* ***The "encouraging successes" I believe are not based on first principle models – here Stefan and I disagree most strongly, but I would say my point of view does represent that of GCM modelers in general.*** Concerning the disagreement in the other sections *I've agreed to mute my comments in this regard,* ***but saying that such models have effectively solved problems (encouraging successes) is too much.***
>
> * * *
>
> Paleoclimatic modeling has become a well–established branch of climate research in recent years. The full spectrum of models that is used for simulating present climate and future scenarios is now being tested on many different problems of past climate, with encouraging successes. [Could specify some examples if desired.]

On January 25, 2005 (no. 0501) Stefan Rahmstorf wrote to David Rind at NASA, perhaps as part of an ongoing discussion. The underlying email from Rind is not captured; however, Rahmstof's response suggests manipulation of modeling results:

> How well any attempts to parameterise these effects work has to be judged by the results, e.g., the comparison of a parameterisation with data and with results from more comprehensive models. How well, for example, a model like CLIMBER2 performs in this respect is documented in the published literature. The results for Large-scale features (e.g., zonal averages and the like), which we are aiming for, lie generally within the range of GCM simulations. ***And this is not the result (as is a common prejudice amongst the GCM friends) of some illegitimate "tuning" practice[.]***

On July 20, 2005 (no. 0400), Stefan Rahmstorf wrote to colleagues for the sake of preserving decorum when addressing other work. He apparently took exception to a model that had been described as "simplistic":

> I have a request on procedure. In the interest of a good and constructive working atmosphere, I would suggest that all of us focus on sober scientific arguments and refrain from unneccessarily [sic] derogatory comments about the work of colleagues. I'm referring in this case to David's comment
>
> − this reference is overused, especially for such a simplistic model
>
> The reference concerned is our theory of DO events which appeared in Nature in 2001 and has since been cited 133 times according to the Web of Science (a sign of overuse?) The model concerned is the CLIMBER−2 model, featured in over 50 peer-reviewed publications since 1998, including 7 in Nature and Science. This model is different from David's model, because it has been constructed for a differenet [sic] purpose, but it is not "simplistic". ***It would never occur to me to call David's model "simplistic" because it does not include an interactive continental ice sheet model, vegetation model, carbon cycle model, sediment model and isotope model.***

In another example suggesting "tuning" to achieve desired results, Tim Barnett of the University of California San Diego wrote to Gabi Hegerl at Duke University on May 18, 2007 (no. 0850) apparently about preparation of AR5. With regard to models, he noted:

> the actual forcing data is a must. right now we have some famous models that all agree surprisely well with 20th obs, *but whose forcing is really different.* ***clearly, some tuning or very good luck involved. I doubt the modeling world will be able to get away with this much longer****....*so let's preempt any potential problems.

Later, as part of the same discussion (no. 5066), Karl Taylor of the Lawrence Livermore National Laboratory wrote:

> * * *
> Likewise, suppose two candidate models were identical in most respects, but one could accurately simulate the climate of the 20[th] century (when all forcings were included), whereas the second had a very low global sensitivity *and produced too little warming.* The developer would again want to choose the model that reproduced the observed trends. In fact this model would probably produce a better estimate when forced by future emissions scenarios too (because, presumably, its sensitivity is closer to the truth).
>
> ***It would be hard to argue that information about 20th century trends shouldn't be used in model development.***

Taylor apparently endorsed the position that a warming trend should be built into the models—which leads to a prediction of future warming. To which Hegerl replied:

> ***So using the 20th c for tuning is just doing what some people have long suspected us of doing***...and what the nonpublished diagram from NCAR showing correlation between aerosol forcing and sensitivity also suggested. ***Slippery slope...*** I suspect Karl is right and our clout is not enough to prevent the modellers from doing this if they can. We do loose [sic] the ability, though, to use the tuning variable for attribution studies.

On February 13, 2008 (no. 5131), Jagadish Shukla, an IPCC lead author, commented broadly to IPCC contributors on the use of models, their strengths and weaknesses, and their relevance:

> ***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 cannot 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.***

4. Making the Medieval Warm Period Disappear.

Peabody's Petition for Reconsideration of the Endangerment Finding demonstrated the conscious bias shared by the leading climate scientists towards work—including their own—which showed dramatic, unprecedented warming in the late 20th Century. *See* Petition at IV.C. In particular, the leading climatologists sought to bolster and reinforce a "consensus" view that warming in the late 20th Century surpassed by far any natural variation in the Earth's climate over the last 2,000 years. However, in their attempt to establish this view, they were hamstrung by past climate studies, including predecessors of AR4, which supported the existence of a

"Medieval Warm Period"—where temperatures equaled those of the late 20th Century—and a "Little Ice Age" that followed the warming period.

The existence of the Medieval Warm Period undermined the leading scientists' preferred narrative of unprecedented recent warmth. It also called into serious doubt the conclusion that recent warmth should be attributed to the build-up anthropomorphic greenhouse gases since the Industrial Revolution. If the Earth warmed naturally during the Medieval Period, then recent warming could also be attributed to a natural process—not anthropomorphic greenhouse gases. Given this problem, the leading scientists needed to make the Medieval Warm Period (and the Little Ice Age) go away, and the Climategate 2.0 emails capture their concerted effort to do so because it conflicted with their preconceived theory of a global warming climate crisis.

Several early emails indicate that Phil Jones and Michael Mann were advocating a revisionist approach to historical temperature studies, challenging previously held views of the MWP. On September 19, 1999 (no. 0646), in preparing the Third Assessment Report, Mann wrote to Briffa and Jones, suggesting deletion of a Briffa reconstruction in favor of ones by Mann and Jones:

> We would like to show just the Mann et al (1999) and Jones et al (1998) reconstructions, along w/ the instrumental record, in the "multiproxy" section of the report, leaving discussions of reconstructions based on specific proxy types to the earlier proxy-specific sections (e.g., the dendro section) and to the general section *"Was there a little Ice Age and a Medieval Warm Period"* which seeks to bring all of the different pieces of evidence together.

In another email, from Sept. 2000 (no. 0550), Jones wrote to Mikami Takehio about the topic for a discussion on historical climate. The Medieval Warm Period is plainly in the crosshairs. He observed:

> The talk will discuss many of the issues and show the results of the latest compilations of proxy evidence for the last millennium, ***work that is begining to rewrite our understanding of the period and challenging the accepted view (a Medieval Warm Period from AD 900−1200 and a Little Ice Age from AD 1450−1850).*** As we gain more evidence from different proxies and diverse regions the 'North Atlantic/European' evidence appears less appropriate from a global-scale viewpoint. ***The new studies show that the 20th century was both the warmest of the millennium and the warming during it unprecedented over the last 1000 years.***

The investment of the Hockey Stick proponents in relegating the MWP is illustrated in an exchange concerning a response by Tom Crowley and Michael Mann to an essay in *Science* by Wallace S. Broecker, which suggested that the MWP was both global and equal to the late 20th Century warmth. Crowley and Mann circulated a draft response to their colleagues, which asserted, "It cannot be reasonably argued that the Middle Ages were as warm as the 20th century at global or hemispheric scales." Thomas Delworth of NOAA took exception to this extreme pronouncement and cautioned his colleagues as follows (March 1, 2001, no. 1369):

> I agree with the overall message you are conveying, but might choose somewhat differing wording in a place or two. *The statement is made "(1) It cannot reasonably be argued that the Middle Ages were as warm as the 20th century at global or hemispheric scales."* **This might be a bit strong ... I would think one can have a reasoned discussion on this topic.** Perhaps something like "We strongly disagree with the assertion that the Middle Ages were as warm as the 20th century at global or hemispheric scales."

Later in 2001, another email exchange illustrates the peculiar interest that the leading scientists had in downplaying the MWP. Tom Crowley wrote to Ed Cook on May 2, 2001 (no. 0466), apparently having heard that Cook was preparing a northern hemisphere temperature reconstruction. Crowley's hyper-sensitivity to the MWP is obvious:

> Ed,

> heard some rumor that you are involved in a non−hockey stick reconstruction of northern hemisphere temperatures. I am very intrigued to learn about this − ***are these results suggesting the so called Medieval Warm Period may be warmer than the early/mid 20th century?***
>
> *any enlightenment on this would be most appreciated,* Tom

Perhaps sensing a brewing controversy, Cook attempted to ameliorate the concern. He provided a lengthy explanation of his focus:

> As rumors often are, the one you heard is not entirely accurate. So, I will take some time here to explain for you, Mike, and others exactly what was done and what the motivation was, in an effort to hopefully avoid any misunderstanding. I especially want to avoid any suggestion that this work was being done to specifically counter or refute the "hockey stick". However, it does suggest (as do other results from your EBM, Peck's work, the borehole data, and Briffa and Jones large-scale proxy estimates) that there are unresolved (I think) inconsistencies in the low-frequency aspects of the hockey stick series compared to other results.
>
> * * *
> ***I do think that the Medieval Warm Period was a far more significant event than has been recognized previously, as much because the high-resolution data to evaluate it had not been available before.*** That is much less so the case now. It is even showing up strongly now in long SH tree-ring series. However, there is still the question of how strong this event was in the tropics. I maintain that we do not have the proxies to tell us that now.
>
> * * *
> ***So, at this stage I would argue that the Medieval Warm Period was probably a global extra-tropical event, at the very least, with warmth that was persistent and probably comparable to much of what we have experienced in the 20th century.*** However, I would not claim (and nor would Jan) that it exceeded the warmth of the late 20th century. We simply do not have the precision or the proxy replication to say that yet. This being said, I do find the dismissal of the Medieval Warm Period as a meaningful global event to be grossly premature and probably wrong. Kind of like Mark Twain's comment [sic] that accounts of his death were greatly exaggerated.

The bias among the certain "consensus" scientists becomes apparent in an email exchange between Keith Briffa and Ed Cook, dated April 19, 2003 (no. 1238), in which they discussed various proxy series, apparently for a forthcoming paper. Cook wrote to Briffa:

> Bradley still regards the MWP as "mysterious" and "very incoherent" (his latest pronouncement to me) based on the available data. ***Of course he and other members of the MBH camp have a fundamental dislike for the very concept of the MWP, so I tend to view their evaluations as starting out from a somewhat biased perspective,*** i.e. the cup is not only "half-empty"; it is demonstrably "broken". ***I come more from the "cup half-full" camp when it comes to the MWP, maybe yes, maybe no, but it is too early to say what it is.*** Being a natural skeptic, I guess you might lean more towards the MBH camp, which is fine ***as long as one is honest and open about evaluating the evidence (I have my doubts about the MBH camp).*** We can always politely(?) disagree given the same admittedly equivocal evidence.

Briffa's response is very intriguing because it not only affirms the bias of the "MBH" camp—the proponents of the hockey stick—but also indicates that the MWP may have equaled late 20th Century warming. Briffa wrote:

> ***Can I just say that I am not in the MBH camp − if that be characterized by an unshakable "belief" one way or the other ,*** regarding the absolute magnitude of the global MWP. I certainly believe the " medieval" period was warmer than the 18th century − *the equivalence of the warmth in the post 1900 period, and the post 1980s ,compared to the circa Medieval times* ***is very much still an area for much better resolution.*** I think that the geographic / seasonal biases and dating/response time issues still cloud the picture of when and how warm the Medieval period was . On present evidence , even with such uncertainties I would still come out favouring the "likely unprecedented recent warmth" opinion − but our motivation is to further explore the degree of certainty in this belief − based on the realistic interpretation of available data.

Later, Keith Briffa wrote a message to Jan Esper about a paper by Willie Soon and Sallie Baliunas[7], which contradicted the hockey stick and concluded that late 20th Century warming was not unprecedented and affirmed the existence of the Medieval Warm Period. In his note from April 7, 2003 (no. 0556), Briffa commented,

> it is a paper by Soon and Baliunas published 2003 but I can't remember where. ***It is concerned with MWP particularly and has engendered a lot of annoyance among palaeo types*** **.** I mentioned it because the issue of scaling is relevant to their poor conclusions. I think it may have been in JGR. I think you can track it pretty easily via the web

Concerted efforts to marginalize the Medieval Warm Period can be seen in emails relating to AR4. For example, on Jan. 13, 2005 (no. 2019), Jonathan Overpeck wrote to Tim Osborn and Keith Briffa, copying several other colleagues. His enthusiasm for dismissing the MWP is illustrated in the last sentence:

> Commonly cited warm periods, including the Medieval Warm Period, Holocene Climate Optimum, Altithermal, Hypsithermal and others appear to have been distinct only regionally, and in a time-transgressive manner. They should not be cited as globally warm intervals comparable to the late 20th century, and are usually too poorly defined to be of use in the literature. There will soon be a box on the Medieval Warm Period that makes this case for the MWP. Tim and Keith − when drafting, perhaps you should change the box's emphasis slightly to include these other periods. Title it "Box 6.1: The Medieval Warm Period and other Poorly Defined Periods of Regional Warmth." Use the MWP as the in-depth example, and then we can mention the other terms only in the intro. After the ZOD, we can make sure we got it all perfect.
>
> ***Lets put an end to some myths that have been around longer than we have!***

---

[7] Soon, Willie; Sallie Baliunas, "Proxy climatic and environmental changes of the past 1000 years". Climate Research (January 31, 2003).

Overpeck's zeal to "nail" the Medieval Warm Period alarmed other colleagues, as captured in the following exchange dated July 19, 2005 (no. 0262). Overpeck's all caps response related to a cautionary email from Keith Briffa and Tim Osborn:

> First, we have no objection to a Figure . Our only concerns have been that we should 1/... be clear what we wish this Figure to illustrate (in the specific context of the MWP box) − note that this is very different from trying to produce a Figure in such a way as to bias what it says (I am not suggesting that we are, but we have to guard against any later charge that we did this). ***We say this because there are intonations in some of Peck's previous messages that he wishes to "nail" the MWP − i.e. this could be interpreted as trying to say there was no such thing,*** and
>
> SORRY TO SCARE YOU. I **ABSOLUTELY** AGREE THAT WE MUST AVOID ANY BIAS OR PERCEPTION OF BIAS. MY COMMENT ON "NAILING" WAS MADE TO MEAN THAT ININFORMED [sic] PEOPLE KEEPING COMING BACK TO THE MWP, AND DESCRIBING IT FOR WHAT I BELIEVE IT WASN'T. OUR JOB IS TO MAKE IT CLEAR WHAT IT WAS WITHIN THE LIMITS OF THE DATA. IF THE DATA ARE NOT CLEAR, THEN WE HAVE TO BE NOT CLEAR. THAT SAID, I THINK TOM'S FIGURE CAPTURED WHAT I HAVE SENSED IS THE MWP

Yet, clearly Overpeck wanted to "nail" the MWP, despite his contrary representations, as revealed in an email from approximately a month earlier. With regard to the Medieval Warm Period discussion box to be included in AR4, Overpeck wrote on June 27, 2005 (no. 0479):

> ***it would be cool to have another figure that made the point about no single synchronous period warmer than late 20th century.*** This is where I get soft with respect to Tom's plot. If it is published to the extent we need it, and if the composite or large-area average recon is the same as you are showing in your great new Fig 1, then it seems that it would be reasonable to show Tom's fig as part of the Box − just to show the same thing in a different way, ***and to hammer in one more nail.***

In another email, Overpeck returns to his "hammer" and "nail" analogy. Concerning development of the same MWP figure, he wrote on July 18, 2005 (no. 3715) to Tim Osborn of his plans with Keith Briffa and Eystein Jansen:

> * * *
> Keith, Eystein and I talked *and have agreed that it would be good* ***to hammer home that available data do not support the concept of a single (or multiple) globally synchronous (e.g., to the degree that the late 20th century is) warm events during anyone's definition of Medieval times.*** We also agreed that this fig would focus on that issue only, and not Medieval warmth vs 20th century. This amplitude issue is dealt with in the main "temps of the last 2K" figs that Tim and Keith produced. But, given all the misunderstanding and misrepresenting that is going on wrt to the Medieval Warm Period, we concluded that it's worth the extra space to address the issue in more than one way

An email exchange among Keith Briffa, Tom Crowley, Tim Osborn, Eystein Jansen and Jonathan Overpeck, dated June 15, 2005 (no. 0346), also reinforces the fact of conscious efforts to shrink the significance of the MWP. Tom Crowley wrote to several collegues:

> ***I have been fiddling with the best way to illustrate the stable nature of the medieval warm period*** − the attached plot has eight sites that go from 946−1960 in decadal std. dev. units − although small in number there is a good geographic spread −− four are from the w. hemisphere, four from the east. I also plot the raw composite of the eight sites and scale it to the 30−90N decadal temp. record. ***this record illustrates how the individual sites are related to the composite and also why the composite has no dramatically warm MWP*** −− there is no dramatically warm clustering of the individual sites.

An email dated October 16, 2007 (no. 771) captures an exchange between Peter Stott and Geoff Jenkins, both of the UK Met Office. Phil Jones of CRU is copied on Jenkins' reply to Stott:

> I think I will say: "Anecdotal evidence, for example the growing of grapes in the medieval period, has been used to imply that current warm temperatures in England have not been influenced by human activities. *However, the popularity of grape growing is*

related to many other factors apart from temperature, and the longest temperature record in existence (that for the Low Countries (van Engelen, refernce??)) indicates a medieval warm period that was cooler than current temperatures". ***OK? I am not very convinced by it myself, but it's the best I can think of.*** Realclimate points out that "attribution doesn't depend on previous climates changes", which I have used myself, but doesnt seem to apply here, does it, because you use the lack of any natural warming from obs/model as the way to rule out natural causes for the last 50 years. ***van Engelen (Fig 6 in UKCIP02) seems to show sustained warmings as big as 1970−2000 in the 1300s.***

5. Problems with Peer Review: Papers by Other Scientists are Disparaged and What this Means for the Field.

I WOULD THINK IT OBVIOUS THAT PEER-REVIEW ALONE IS *NOT* [] SUFFICIENT TO ESTABLISH WHAT IS "GOOD SCIENCE"

Michael Mann – July 6, 2004 (no. 0384)

I HOPE WE CAN RESOLVE THE SCIENTIFIC ISSUES OBJECTIVELY, AND W/OUT INJECTING OR [SIC] ANY PERSONAL FEELINGS INTO ANY OF THIS. THERE ARE SOME SUBSTANTIAL SCIENTIFIC DIFFERENCES HERE, LETS LET THEM PLAY OUT THE WAY THEY ARE SUPPOSED TO, OBJECTIVELY, AND IN THE PEER REVIEWED LITERATURE.

Michael Mann – April 2002 (no. 1705)

The Climategate 2.0 emails demonstrate that the leading scientists hold disdain for the work of their lesser-known colleagues, especially when those scientists publish papers that question the "consensus" view or take a different approach to the historical climate and recent purported climate change. The climate elites dismiss these papers as being the work of unskilled buffoons and charlatans. Their favorite description by far for these papers is "crap" which appears again and again in the Climategate 2.0 emails.

While the arrogance and boorishness is alarming, the collective derision reflects badly on the peer review process because it indicates (i) that much of the work in the climate field is highly suspect—that is, if the elites are to be believed; (ii) that the peer-review process fails to keep "crap" from being published; and (iii) a clubby attitude among the elite that protects their work from genuine scientific scrutiny. There is a certain degree of irony in the elite's derision: it indicates that the peer-review process is fundamentally flawed, which, in turn, undermines the validity of their own work.

For example, on June 22, 2003 (no. 4207), Raymond Bradley wrote to Phil Jones about a Chinese temperature series that was included in a paper published in *Geophysical Research Letters:*

> You commented that the Chinese series of Yang et al (GRL 2002) looked weird. ***Well, that's because it's crap—no further comment on what stuff gets into GRL!*** You appear to have used their so-called "complete" China record. *You really should consider what went into this -- 2 ice core delta 18O records of dubious relationship to temperature (one is cited as correlating with NW China temperatures at r=0.2−0.4), 3 tree ring series, one of which is a delta C-13 record of questionable climatic significance (to be generous).* The other series include two records from a Taiwan Lake--a carbon/nitrogen isotope and a total organic carbon series (interpreted as high="warm, wet") and an oxygen isotope series from cellulose in peat!!! (& don't ask about the C−14 based chronology, interpolated to decadal averages!) ***I loved this sentence:*** "Although a quantitative relationship between the proxy records of the Jinchuan peat, the Japan tree-ring series and the Taiwanese sediment records with modern climate data are not given in the original works, the qualitative connectivity with temperature as the dominant controlling factor has undoubtedly been verified"
>
> ***Oh, undoubtedly!!***

On October 26, 2003 (no. 2527), Michael Mann wrote to Ray Bradley, Malcolm Hughes, Keith Briffa, Tom Osborn, Ben Santer and others, warning them of a forthcoming paper to be published in the journal *Energy and Environment:*

> Dear All,

> This has been passed along to me by someone whose identity will remain in confidence. ***Who knows what trickery has been pulled or selective use of data made.*** *Its [sic] clear that "Energy and Environment" is being run by the baddies-only a shill for industry would have republished the original Soon and Baliunas paper as submitted to "Climate Research" without even editing it.* Now apparently they're at it again... My suggested response is:
> 1) to dismiss this as stunt, appearing in a so-called "journal" which is already known to have defied standard practices of peer-review. *It is clear, for example, that nobody we know has been asked to "review" this so-called paper*
> 2) to point out the claim is nonsense since the same basic result has been obtained by numerous other researchers, using different data, elementary compositing techniques, etc. Who knows what sleight of hand the authors of this thing have pulled. ***Of course, the usual suspects are going to try to peddle this crap.*** The important thing is to deny that this has any intellectual credibility whatsoever and, if contacted by any media, to dismiss this for the stunt that it is.

Michael Mann's self-serving view of the peer review process is illustrated by an exchange with Roger Pielke (July 8, 2004; no. 0384). Mann responded to an email from Pielke who took the position that peer-reviewed papers merit consideration in synthesis reports, even if their results are subsequently disputed by other peer-reviewed papers. Mann took exception, asserting that only so-called "experts" can serve as final arbiters of "good science":

> With regard to your argument, ***I would think it obvious that peer-review alone is *not* a [sic] sufficient to establish what is "good science".*** *That is why we do *assessment*, i.e., use our own expert judgement [sic] to assess what is and is not appropriate or relevant for our report within the peer-reviewed literature.* ***I think this is obvious.***

On December 13, 2004 (no. 0380), Michael Mann wrote to Keith Briffa, expressing his view on a paper critiquing the work of McIntyre and McKitrick:

> Keith,
> This paper is in review, and can be referred to (just clear w/ Caspar or Gene first) for IPCC draft purposes. *They basically show that the McIntyre and McKitrick paper is total crap,* and they provide an online version of the Mann et al method (and the proxy data), so individuals can confirm for themselves...

Mike

On January 12, 2005 (no. 3232), Mike Mann wrote to Tim Osborn and copied Raymond Bradley, Phil Jones and Malcolm Hughes:

> ***There is clearly a problem at GRL now.*** *I don't know which editor is allowing these papers in (Soon et al, now this one), but its clearly beyond our control.*
>
> ***The paper is all crap.*** I don't think I'll respond. "RealClimate" already discredits their PCA centering convention claims, and our papers in review and in press make this all moot...
>
> They can continue trying to go after MBH98−−It's now chasing a ghost, since we've all moved on to other methods, and other results, which support the same contention.
>
> ***I can't allow myself to be sidelined with this sort of crap any further,***

On January 21, 2005 (no. 2696), Michael Mann wrote to Phil Jones and copied a number of the "elite" scientists, including Keith Briffa, Tim Osborn, and Raymond Bradley:

> Yeah—this [paper] looks bad too. ***IJC has published some decent stuff, but lots of crap.*** Didn't R.G. Currie publish nearly all of his *notorious* ***solar-looney*** *papers* there? "New Hope Environmental Sciences" is, it may not surprise anyone, a front organization. It's entirely funded by Pat Michaels, through sources of funding whose dubiousness you can only begin to imagine. As for U.Va/Michales/Cato, lets just say, something *should* have been done a long time ago.

In April 2005, several of the leading scientists discuss forthcoming and recent papers by other researchers, characterizing these efforts—not surprisingly—as "crap." On April 4th and 5th, Phil Jones, Tom Wigley and Myles Allen exchanged emails under the subject "Douglass and Knox, GRL, March 2005."

> Thanx Phil. I will see Myles at the end of the month before coming up to Norwich. By the way, my paper says one CAN get sensitivity from volcanoes, but cannot narrow the uncertainty range.

Tom.

* * *

Tom,
I gave Myles the crap paper last week when we met at Duke for an IDAG meeting. He has a paper coming out soon in GRL saying much the same as you − volcanoes can't be used to estimate the climate sensitivity. He was unaware of Douglass and Knox. I think Myles paper has someone else as the first author. Myles is aware of your paper. He refers to it and made a comment to getting the same sort of answer in his presentation. I am saying all this as Myles went onto Sydney, Australia and is there for much of this week. I think we've all signed off on the NRC review. You should get something in the next 2−3 weeks so I'm told**. *I couldn't seem to stop Lindzen referring to the crap paper nor his own in response to some comments in Chapter 5.*** With your paper coming up and the one Myles is involved with you'll have enough to not bother answering.
Cheers
Phil

I am writing a comment on this, with Ben and Caspar Ammann. It is total crap. *It is a pain to do, but important to have a response on the record.* A number of us suspect that one of the editors of GRL is deliberately choosing 'sympathetic' referees for papers like this (another e.g. is the recent M&M paper criticizing the hockey stick).
Myles −− did I send you my volcano paper (soon in JGR)?
Tom.

On Dec. 5, 2007 (no. 0174), Michael Mann wrote to Phil Jones, attacking the *Journal of Geophysical Research Atmospheres:*

> well put Phil,
> I think you've put your finger right on it. *JGR−Atmospheres has been publishing some truly awful papers lately;* we responded (Gavin, me, James Annan) to the awful Schwartz sensitivity estimate paper, but ***there are so many other bad papers that are appearing there (Chylak, etc.) that its [sic] just impossible to respond to them all.***

On Dec. 12, 2007 (no. 0661), Ben Santer expressed his opinion to Tim Osborn about a recent paper in the *International Journal of Climatology,* copying Phil Jones, Keith Briffa and Tom Wigley. He dismissed it as "crap," and questioned the author's abilities:

> Dear Tim,
> Thanks for the "heads up". As Phil mentioned, I was already aware of this. The Douglass et al. paper was rejected twice before it was finally accepted by IJC. ***I think this paper is a real embarrassment for the IJC. It has serious scientific flaws.*** I'm already working on a response. Phil can tell you about some of the other sordid details of Douglass et al. These guys ignored information from radiosonde datasets that did not support their "models are wrong" argument (even though they had these datasets in their possession). Pretty deplorable behaviour... Douglass is the guy who famously concluded (after examining the temperature response to Pinatubo) that the climate system has negative sensitivity. ***Amazingly, he managed to publish that crap in GRL. Christy sure does manage to pick some brilliant scientific collaborators...***
> With best regards,
> Ben
>
> 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

On Nov. 9, 2009 (no. 2869), Tom Wigley wrote to Phil Jones about a forthcoming paper in *Geophysical Research Letters:*

> Thanks, Phil.
> ***A bunch of us are putting something together on the latest Lindzen and Choi crap (GRL).*** Not a comment, but a separate paper to avoid giving Lindzen the last word.

Plainly, if the leading, peer-reviewed journals routinely allow purported “crap” to be published, then the peer review process in the field of climate science is fundamentally flawed. What the leading scientists fail to realize, however, is that their scorn and derision has significant blow-back. It undermines their authority and expertise, which is based on their own “peer reviewed” body of work. Moreover, their disdain for peer-reviewed work fundamentally calls into question the validity of climate science and the so-called “consensus” view of climate change that the leading scientists attempt to project in public.

Appendix A

Part One

Review of Climategate 2.0 emails: 1996-2001 pre-TAR Period

Survey of Main Themes

1. Hide the decline: the full context in IPCC

The central figure is Keith Briffa. He found that MXD (maximum density) tree ring data was showing a breakdown in the correlation with temperature in the post-1960 era. If true, this situation would undermine the scientific basis of using tree rings as historical climate proxies.

At first Briffa considered the problem a scientific issue and raised it in publications. He began to hear from colleagues in other countries noticing the same thing, in some cases in ring width data as well. There were no explanations for the phenomenon. At the time of the TAR, Mann had entered the field and was promoting an aggressive "unified front." Briffa's work did not fit this agenda and Mann proposed deleting it from the IPCC graph. Jones and Briffa resisted this and Mann backed down. But the IPCC was pressing for a paleoclimate graph and there was pressure for a "tidy" consensus. Then Osborne suggested deleting the post-1960 portion of Briffa's data when he sent it to Mann for inclusion in the IPCC report. Shortly after that Jones did the same thing for a WMO report at Osborne's suggestion. The authors began claiming that the post-1960 data was contaminated by non-climatic effects that had never occurred before. But, they knew privately that they had no proof of this.

(Note: "…" denotes irrelevant text deleted)

1.1 Early recognition of reality of issue.

3909

```
date: Wed, 14 Aug 1996 14:59:37 -0500 (EST)
from: LUCKMAN@SSCL.UWO.CA
subject: Icefields paper
to: K.BRIFFA@UEA.AC.UK


Dear Keith/ Phil,

                 I have been working on the paper but am now in
need of some help and an outside perspective. THis is
particualrly true for the introductory and concluding sections
where some broader vision is needed to put this study in
perspective. I have attached a revised text with a list of
questions and figures. I have faxed new figures to you that you
have not seen: several of the figures you already have in the
earlier report.

Comments, suggestions and corrections are needed and welcome. I
look forward to hearing from you in the near future.

Cheers

Brian

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

…


                          ABIES PROBLEM

Until I plotted up Figure 2 I did not realise the proportion of
the chronology that is Abies in the 14-1600 period. Although the
picea density chronology does (as stated) correlate very well
with the combined chronology throughout this interval, the
correlation between the abies and picea chronologies themselves
```

is much lower (ca. 0.5-0.7 see draft diagram). **Examination of the statistics for the Abies MXD series indicates that their mean density is about 0.1 greater than picea (which should not be a problem because the series were indexed before averaging in the chronology-right?) and their mean sensitivity value is about half that of picea. i.e. there appears to be less interannual variability.**

It seems to me that this could be a serious problem and/or be picked up as such by a referee. The obvious question would be if we substituted the picea chronology and did the reconstruction over the results be any different over this interval? The justification for including the abies was to increase replication in the snag record but I assumed the climate signal was similar from the two species. We have no data from the Icefields to test this- Colenutt and Luckman 1991 did develop abies and picea chronologies from the same site at Larch Valley which show a similar pattern of response (see Figure 4, xerox sent) but they are clearly not identical. The question is basically do we address this issue head on, citing these data and indicate that reconstruction from picea alone is very similar to that produced in the paper- and present (or have available) data to back it up? Although I could probably find picea snags (and Fritz has cores) to address this issue it would take a year or more to process this material and is not a solution at this stage.

so- can you try a reconstruction using picea alone and see if they are different. I have assumed that because the two chronologies are very highly correlated then reconstructions from these two chronologies (using the same transfer function) would be equally similar. Is that a valid assumption? …

3973

date: Tue Oct 15 17:01:05 1996
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: New Scientist article
to: Fred Pearce <100713.1311@CompuServe.COM>

Dear Fred

I have done a redraft of the article. I know you said not to rewrite it (preferably) but rather to correct, make notes suggestins etc. I thought about this for some time and realized that it woulld be far more difficult to indicate the precise places,the precise problems and the suggested corrections at all of the places I considered were subtle misinterpretations of what I said, or meant, or feel. …

There remain a couple of points for your consideration. Is it possible, somehow, to get the ADVANCE-10K name in and explained(i.e. the project title)? This is important to us as publicity in the context of our funding. **Also, I spoke to you about the problem of anthropogenic influences (i.e. increased CO2, nitrate fallout , increased UV radiation) possibly having an influence on recent tree growth and so complicating our efforts to use these recent data to define how we interpret past tree growth. Is it possible to put in some reference to me worrying about this?**

Finally, can you suggest to the editor that we put a footnote in to flag our home page which details all the objectives and participants ? (perhaps with the reference to the ADVANCE-10K acronym,title and grant number)

I look forward to hearing from you and can send the text as ASCII, WORD or WORDPERFECT files - for now should I fax it and if so to where?

cheers
Keith

3477

date: Fri, 08 Nov 1996 17:38:50 +0100
from: Theodor Forster <theodor.forster@wsl.ch>

subject: Sharpness quality test
to: K.Briffa@uea.ac.uk

Dear Keith,
By order of Fritz Schweingruber I send you 10 chronologies from the sibirian region 65°21'N 66°58'E to 69°06'N 84°32'E.
You found a drastical decrease in the max. latewooddensity compared to the measured summertemperatures from 1950 to 1990.
To check the influence of technical inaccuracies in that timeframe, I choosed from each site all cores with top sharpness quality and calculaded a second chronology with them.
Therefore you receive the following files splited out in larix sibirica and picea obovata. The second divide up is rowdates and indexed dates.
LASIALL.HCR larix / all cores / from 7 sites / row
LASITOP.HCR larix / 50 cores top quality / from same 7 sites / row
LASI ALL.HCI larix / all cores / from same 7 sites / index
LASITOP.HCI larix / 50 cores top quality / from same 7 sites / index
PCOBALL.HCR spruce / all cores / from 3 sites / row
PCOBTOP.HCR spruce / 26 cores top quality / from same 3 sites / row
PCOBALL.HCI spruce / all cores / from same 3 sites / index
PCOBTOP.HCI spruce / 26 cores top quality / from same 3 sites / index
Please compare the complete chronologies (LASIALL,PCOBALL) and the chronologies only with cores of top quality (LASITOP,PCOBTOP) with the summertemperatures in the same region.
The results will show us the influence of technical inaccuracies, which we can not eliminate totaly.
Sincerily yours
Theo Forster

Attachment Converted: c:\eudora\attach\SHARTEST.zip

690

date: Tue, 21 Jan 97 06:53:51 EST
from: drdendro@lamont.ldgo.columbia.edu (edward cook)
subject: Que pasa?
to: k.briffa@uea.ac.uk

Hi Keith,

I was just wondering how you are making out with that Kalman filter mess I sent you. I am only going to be around for about 2 more weeks before I go downunder. So, if you have anything you want to pass by me, it ought to be before then. **In my conversations with Brendan, it has occurred to me that something analogous to what you find in your data (a systematic departure between tree rings and temperature over the past few decades) also is apparent in some of the Huon pine data. Specifically, the BCH site of Brendan's, which is the second highest site compared to Lake Johnston, shows the same effect as you see, at least in the ring widths anyway. We don't yet have density data for that site. The high-pass variations in ring width lock in beautifully with temperature, better in fact than does Lake Johnston. However, the low-pass side goes down over the past 30 years years as temperatures have increased. Brendan and I have speculated about this a lot. My pet theory is** that temperatures have risen sufficiently to cause net photosynthesis to go into deficit occasionally (i.e. respiration exceeds primary photosynthesis). Of course, this theory is pretty bad as is because it doesn't explain why the slightly higher (say 50m) Lake Johnston site maintains its temperature response at all frequencies. I suppose it is arguable that what we are seeing is a very sharply defined threshold response and the LJH site is just cold enough to escape this effect. There also appears to be an inversion layer over western Tasmania that kicks in at around 900m. **I don't know. Maybe it is totally coincidental.**

Cheers,

Ed

1554

date: Wed, 5 Mar 97 16:42:10 EST
from: drdendro@lamont.ldgo.columbia.edu (edward cook)
subject: The devil ...
to: k.briffa@uea.ac.uk

Hi Keith,

The devil made me do it. I have nominated you for a LDEO Climate Center visiting Climate Scholar. If it comes about, you can of course tell them (me) to get stuffed! I really think that there would be keen interest here on your work.

Cheers,

Ed

P.S.

Here is my message to Broecker's secretary:

Hi Moanna,

Sorry for not responding on that. Bob Dickson would be fine. My nomination for a CC visitor in the future is Keith Briffa from the Climatic Research Unit, University of East Anglia. **He is doing some very interesting work with multi-millennial tree-ring records covering much of the Holocene and is working on understanding the cause(s) of a very large-scale change in the response of trees to climate (e.g. over most of Siberia) that has resulted in an anomalous divergence between temperature and tree rings since ca. 1950.**

Cheers,

Ed

PP.SS.

Any more luck with the Kalman files?

1483

date: Mon Nov 3 18:28:04 1997
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re:
to: Tom Wigley <wigley@meeker.ucar.edu>

Tom
thanks for the info. Actually this is a chance for me to to mention that we have for the last few months at least, been reworking the idea of looking in the Schweingruber network data for evidence of increasing tree growth and hence ,potentially at least, evidence of changing tree(read biomass) uptake of carbon.
The results are dramatic - not to say earth shattering because they demonstrate major time-dependent changes - but changes that are consistent in different areas of the network. We have regionalised over 350 site collections , each with ring width and density data , age-banded the data so that we look only at relative growth in similar ages of trees through time and recombined the standardisd curves to produce growth changes in each region. **Basically growth is roughly constant (except for relatively small climate variablity forcing) from 1700 to about 1850. It then increases linearly by about up until about 1950 after which time young (up to 50 year old) basal area explodes but older trees remain constant .** The implication is a major increase in carbon uptake before the mid 20th century - temperatue no doubt partly to blame but much more likely to be nitrate/Co2 . **Equally important though is the levelling off of carbon uptake in the later 20th century. This levelling is coincident with the start of a density decline - we have a paper coming out in Nature documenting the decline . …**
I have been agonising for months that these results are not some statistical artifact of the analysis method but we can't see how. For just two species (spruce in the western U.S. Great Basin area and larch in eastern Siberia) we can push the method far enough to

get an indication of much longer term growth changes (from about 1400) and the results confirm a late 20th century apparent fertilization! The method requires standardizing (localized mean subtraction and standard deviation division) by species/age band so we reconstruct relative (e.g. per cent change) only .

…

There are problems with explaining and interpreting these data but they are by far the best produced for assessing large scale carbon-cycle-relevant vegetation changes - at least as regards well-dated continous trends. I will send you a couple of Figures (a tiny sample of the literally hundreds we have) which illustrate some of this. I would appreciate your reaction. Obviously this stuff is very hush hush till I get a couple of papers written up on this. We are looking at a moisture sensive network of data at the moment to see if any similar results are produced when non-temperature-sensitive data are used. You would expect perhaps a greater effect in such data if Co2 acts on the water use efficiency .

4688

cc: fritz.schweingruber@wsl.ch
date: Thu Oct 1 18:05:02 1998
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: increasing tree biomass results
to: joos@climate.unibe.ch

Dear Fortunat

I don't know if you remember , but some time ago you and Tom visited us at the Climatic Research Unit and you and I discussed some work I was doing with Fritz Schweingruber's tree-ring densitometric data base . It contains annual measurements of spring and summer radial tree growth and corresponding mean density values. **The data represent nearly 400 sites, mainly around the northern boreal forest. I now have sufficient output to write up what I believe will be a very significant paper showing that growth has increased greatly during the 19th century - but perhaps more significantly it levelled off after the middle of the 20th century.** To complicate issues it seems that this is not so for very young trees (under 50) . The replication of these young tree data is very low but it seems that basal area increment and maximum latewood density increase remarkably in the last few decades up to the 1980s when the data run out. **The maximum density data decline steadily from 1950 onwards . So the picture is not simple** … Incidentally, though I will only send illustrations of overall geographically-averaged data, remember that different large sub-regions (e.g. central Siberia, Eastern North America etc.)all seem to show the general dramatic increase in radial growth in the last century. In one or two very restricted areas where we have long data (e.g. eastern Siberia) the increase is unprecedented since 1200 or 1400 A.D.

I will post the Figures tomorrow. Best wishes

Keith

1.2 Recognition that issue undermines dendro-climatology

4904

At 16:45 29/04/97 -0500, you wrote:
>Keith:
>
>I am not sure where we are in our conversation. I thought you had a few
>more comments about my comments.
>
….
>
>On a more productive science note: **I have begun some analyses of the data**
>from our Taymyr chronologies and there is a noticeable change in response
>to climate in recent decades. It is not as clear as the Alaska spruce
>situation exactly what is happening but one cannot make simple models
>assuming a constant relationship. I would assume you are aware of this and
>wonder what your thoughts are.
>
>Gordon

4904

date: Wed Apr 30 15:57:27 1997
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: your note and CAPE
to: druid@ldgo.columbia.edu (Gordon Jacoby)

Gordon
I too am not sure where we left our conversation last. Yes I couldn't agree more about PAGES and the million spawning devils that seem to be the product…

But to go to science - **surely you have talked to Ed about the work I've been doing trying to document and understand the change in sensitivity of northern density (and ring widths) to temperature. I have a manuscript but the problem is a major one and is complicated by the issues of CO2 , temperature thresholds,standardisation regional coverage(i.e. spatial scale),appropriate climate signal etc. etc. Simply we see a loss of decadal scale sensitivity in much of our large spatial average data - and in our recent Russian calibrations. I know you talked about this in a couple of papers and have suggested a recent appearance of moisture sensitivity in your northern American trees. I do not know why this is happening but it seems clearer in the density data. We (at least Fritz) has explored the technological possibilities - i.e. that density is biased by an inability to record maximums correctly in very narrow rings but we do not believe this to be the case. I think some threshold may have been crossed that means the densities are limited in their ability to record high temperatures and of course it could be a drought type response in warm periods. These suggestions do not seem to be the answer - or at least all of the answer. Similar warmth before and less of an underprediction of temperatue then, plus the widespread (though not perfectly synchronous) manifestation of the phenomenon lead me to suspect synergistic influences. I really think that nitrates,CO2.tropospheric ozone - and ,certainly not least, increased uv could each or all be playing some part. As for calibrating transfer functions , I think we have to somehow adjust recent tree growth records or not use recent data in the calibrations!**

…

as always best wishes to you
Keith

1731

cc: m.hulme@uea.ac.uk
date: Fri, 01 Oct 1999 10:18:41 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Paper 980G by Luterbacher et al
to: b.d.mcgregor@bham.ac.uk

Dear Glenn,
I've tried to ring you to talk about this paper that IJC has rejected on 21/9/99. …
His two points are basically wrong !

1) 'Patterns during the 20th century are applicable to earlier epochs'. This assumption applies to all paleo reconstruction papers ever written. OK, it is an assumption called the 'Principal of Uniformitarianism' and we could have stated it clearer, but it is one that has been made by countless thousands before us. If it is not valid we might as well give up.
The method used in the paper is not the same as infilling SST fields to get complete fields, a technique that I would question (this technique is usually used to get complete fields to drive GCMs). Our paper uses real data for the past and attempts through a calibration/verification exercise to derive circulation patterns for earlier periods. ….
The problems raised by the reviewer are no problems and we can easily address them. They don't invalidate the results.

Cheers
Phil

3357

date: Tue Nov 2 16:10:25 1999
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: dendro data
to: Julie Jones <jones@gkss.de>

At 02:51 PM 9/22/99 +0200, you wrote:
…

Hi Julie

…

What we've got are:

(1) the raw chronologies (dimensionless time series between 100 and 600 years long from about 390 locations).
(2) a list of those chronologies that correlate significantly with growing season temperature.
(3) nine regional averages of the chronologies, that have been calibrated to produce reconstructions of regional April-September mean temperatures.
(4) one hemispheric wide calibrated reconstruction.

and what we've almost got are:

(5) gridded, calibrated reconstructions of growing season (April-September) temperature on the Jones (that's Phil not Julie!) 5 by 5 grid.

I should tell you that there is a fairly strong temperature signal in the tree-ring density series, but that a non-temperature trend is also apparent post-1950 that gets bigger and deteriorates the temperature relationship. This makes calibration somewhat harder (hence I've been working on them for 2 years...!), but **you also have to make the assumption that this non-temperature signal is something anthropogenic and didn't occur in the past.**

…

Cheers

Tim

1.3 Confirmation from other scholars

1469 Jacoby, USA

date: Fri, 16 May 1997 17:28:32 -0500
from: druid@ldgo.columbia.edu (Gordon Jacoby)
subject: Your Paper
to: k.briffa@uea.ac.uk

Keith:

Your paper is interesting and I would agree that it s a large-scale problem. I have also found the problem in the Taymyr trees. At one site there is a definite change and increase in moisture stress; at others the explanation is not obvious. For full understanding each site may have to be examined in detail. I have found individual sites/trees where the response to temperature is still continuing.

The second sentence raises a point that I have mentioned to you before. A substantial number of the sites across Canada are in the boreal forest but nowhere near latitudinal or elevational treeline. The boreal forest is complex and should not be catagorized by a blanket "temperature sensitive" description regarding ring widths. I suggest a qualifying phrase to indicate something about the site variations.and not the use of the word "all". …

I do not believe the problem will be solved by lumping grand arrays of data and regionalizing some varied gross impacts. It can be used to point out a serious problem but will not lead to real understanding of causes. Maybe just calling attention to the problem is your intent with this paper.
…

Cheers, Gordon

577 Woodhouse, USA

date: Tue, 03 Mar 1998 10:00:15 -0700
from: Connie Woodhouse <woodhous@ngdc.noaa.gov>
subject: Nature paper
to: Keith Briffa <k.briffa@uea.ac.uk>

Keith, I found your recent Nature paper on the decreasing sensitivity of tree-growth to temperature in the second half of the 20th century quite interesting. **I've been working with a collection of tree-ring chronologies for the Colorado Front Range (you've probably used some of these chronologies in your analysis) and have noted something similar.** Although I'm looking at ring widths, and these are more high-elevation than high latitude, **I found that most of the higher elevation species (limber pine, lodgepole pine, bristlecone pine) have an inconsistent response to climate.** I've been trying to put together a decent regional precipitation reconstruction, **so I just deleted those chronologies from my analysis**, and have been working with ponderosa pine and Douglas fir, the lower elevation species, which seem to have a stable response to climate. I know Don Graybill noted this inconsistent response with high elevation species when he worked in this area 10 years ago, and he seemed to think the change took place in the 1930s. I haven't looked very closely at these chronologies yet, but this change in response/sensitivity is something I'd like to look at, especially in light of you and your co-authors' findings which suggests a possible hemispheric-scale forcing.

Thanks for a thought-provoking paper!

Connie

Connie Woodhouse
NOAA Paleoclimatology Program
National Geophysical Data Center
325 Broadway St.
Boulder, CO 80303
(303)497-6297

woodhous@ngdc.noaa.gov

4799 Itoh-Japan

date: Wed, 19 May 1999 16:30:06 -0700
from: Kiminori Itoh <itoh@kan.ynu.ac.jp>
subject: Question:Dr.Briffa
to: k.briffa@uea.ac.uk

Dear Dr. Briffa;
I have read your very interesting article "Seeing the Wood from the Trees" (Science, 7 May, p.926). Would you please comment on my questions below if possible?
1) Do you have any estimation on the effect of the growth promotion due to CO2? I think that many people will feel curious about the lower temperature of "the medieval optimum" in the reconstruction.
2) **What do you think about the discrepancy between the reconstruction and the instrumental record in this 20 years? I think this is important because the recent high global temperatures reported so far are based on the instrumental data. It is striking to see that the recent temperature increased only moderately. I saw a similar tendency in the borehole data (Dahl-Jensen et al., Science, 9 Oct. 1998, p. 268), but thought that this was a local effect.**
I appreciate in advance your kindest considerations.

Sincerely Yours,
Kiminori Itoh, Professor,
Institute of Environmental Science and Technology,
Yokohama National University,
79-7 Tokiwadai, Hodogaya-ku, Yokohama 240-8501
Tel. +81-45-339-4354
Fax. +81-45-339-4373
E-mail: itoh@kan.ynu.ac.jp

1.4 No apparent explanation

1851
At 08:54 05/06/97 -0400, you wrote:
>Hi Keith,
>
>I got your draft paper from Gordon, with his comments, and read it. …
>
>Obviously, there are numerous ways to proceed with the analyses that would
>answer many of the questions and criticisms that Gordon raises. It should
>be possible to more objectively regionalize the data using rotated EOF
>analysis. That would, perhaps, blunt that criticism. Also, doing the high
>and low-pass filtering should enable one to indirectly determine the degree
>to which a change in climate response (say temperature to moisture) is
>involved. **I am sure that Gordon is right about some sites have increasing
>moisture stress, but I doubt that that is the main cause, or even one that
>is of any significance in a large-scale sense. The main hypotheses I favor
>are (not necessarily in order of importance): Arctic haze, excessively high
>temperatures, and UV-B. However, I do favor the first two above UV-B.
>"Maybe I don't know" (a Nepali saying).**
>
>Cheers,
>
>Ed
>

1851
date: Thu Jun 5 14:06:36 1997
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Your paper
to: drdendro@ldgo.columbia.edu (Edward R. Cook)

Ed,
many thanks indeed for your comments….We are ahead of you on the rotated PCA of the effect and in factoring out the decline in the density data by identifying it in a analysis of the rotated PCs. This stuff is for a more detailed (some would say scientific) paper - but is too detailed for a first (wider appeal!) description of the phenomenon. **I too agree that soil moisture stress must play some part in this but I do not believe we have unprecidented conditions (summer precip. or temp) that explain this without the need to invoke a new synergistic effect - nitrate/CO2/UVb or whatever. This effect - and particularly in the density , is I believe real and important.**
I appreciate Gordon's comments too - but the effect is a valid signal even when identified on these spatial scales. We are justified in drawing attention to it and following up with a detailed analysis. For once I think this deserves the audience of a Nature paper which is why it will probably be rejected!
We need you to do some work with us on this and I still want to get the means of doing this sorted. Please let us make it happen.
Keith

1404

date: Tue Mar 23 17:08:43 1999
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: tree growth analysis
to: David Peterson <wild@u.washington.edu>

David

I am really tied up at the moment…. We've also done elevation breakdown of the analyses in the U.S. data but find little effect with elevation. **All of the data show a consistent continuing fertilzation of young trees but a levelling off post 1950 in all old ones - I have theories to bore you with.** Get back to me. Great to hear from you and sorry about the rush

best wishes

Keith

5287

cc: <k.briffa@uea.ac.uk>
date: Fri, 22 Sep 2000 06:22:50 -0700
from: mhughes@ltrr.arizona.edu
subject: Re: old stuff
to: tom crowley <tom@ocean.tamu.edu>

Dear Tom,
…If you examine my Fig 1 closely you will see that the Campito record and Keith's reconstruction from wood density are extraordinarily similar until 1850. After that they differ not only in the lack of long-term trend in Keith's record, but in every other respect - the decadal-scale correlation breaks down. I tried to imply in my e-mail, but will now say it directly, that **although a direct carbon dioxide effect is still the best candidate to explain this effect, it is far from proven.** In any case, the relevant point is that there is no meaningful correlation with local temperature. Not all high-elevation tree-ring records from the West that might reflect temperature show this upward trend. **It is only clear in the driest parts (western) of the region (the Great Basin), above about 3150 meters elevation, in trees old enough (>~800 years) to have lost most of their bark - 'stripbark' trees. As luck would have it, these are precisely the trees that give the chance to build temperature records for most of the Holocene. I am confident that, before AD1850, they do contain a record of decadal-scale growth season temperature variability. I am equally confident that, after that date, they are recording something else.**
I'm split between Harvard Forest and UMASS these days, and my copy of your paper is not with me today. I'd be interested to know what the name of the site for the LaMarche central Colorado record was.
Cheers, Malcolm

[“strip bark” trees in Great Basin are the bristlecones. Their post-1850 behaviour, which Hughes claims is showing something other than temperature, was the basis for Mann’s hockey stick calibration work. Removal of the bristlecones destroys the hockey stick. Hughes apparently did not realize this, though Mann did.]

1.5 Briffa tells his colleagues: proxy methods weak

1836

cc: D Parker D Parker <deparker@meto.gov.uk>, John Mitchell John Mitchell <jfbmitchell@meto.gov.uk>, k.briffa@uea.ac.uk
date: Tue, 23 Jun 1998 22:49:49 +0100
from: sfbtett@meto.gov.uk
subject: Re: Draft Report on Detection Workshop (March 1998, Bracknell)
to: Phil Jones Phil Jones <p.jones@uea.ac.uk>

Hi Phil,
thanks for your comments -- almost missed including them (my fault) but here they are. Thanks for taking the time to read and comment on the report. Below is my response to your and Keith's comments.

Phil Jones wrote:
>
> Simon,
> Here are some comments from Keith and me.
>
> Background 1.1
> Should the 3rd paragraph include that the program also needs

```
>  longer climatic datasets to estimate longer timescale variability ?
```

Yes -- added a sentence saying "Homogeneous observations of climate
greater than 50 years would allow better comparison of model simulated
variability with observed variability." -- sound OK?
>   Page 6 #4
>
>   Rewritten as :
>
>   **The reasons for the poor comparison between many proxy data**
> sources and temperature measurements should be explored with a
> view to improving the proxy data. The development and expansion
> of existing datasets of tree-ring density and tree-ring width
> should be continued and these data made easily available to
> the scientific community in order to allow validation of model
> variability.

OK -- included the 2nd sentence (1st sentence is what we have already)
I note the addition of the word "expansion" -- John you happy with this?

>   Summaries of talks
>
> Mine : David sent some - here are some more
>
…
>
> Keith's talk
>
>   Tree-ring width and tree-ring density measurements are a good
> proxy for surface temperatures on timescales of 1 to 100 years.
> On timescales beyond 100 years the results are often affected by
> the removal of biological factors related to tree aging. Research   is
> currently being undertaken to extend the climate information
> recoverable on longer timescales. Dating is very good for these
> data sources. **However, post-1950 they may contain significant**
> anthropogenic effects, which need to be removed. Other proxy data
> sources such as corals and ice cores have great potential for
> representing tropical and polar/high elevation regions but are
> presently less certain in terms of dating and further work in
> formal calibration with climatic data is desirable.

**Um, I thought that a fair summary of Keith's talk was that the Proxy
data was really bad -- I think that
does need to be said.** How about, for the last sentence:

"Other proxy data sources such as corals and ice cores have great
potential for representing
tropical and polar/high elevation regions **but are currently unreliable
as measures of
surface temperature**. This may be because the dating of them is less
certain than tree ring proxies,
and further work in formal calibration of these proxies with climatic
data is needed."

Simon
Attachment Converted: "c:\eudora\attach\vcard1.vcf"

1.6 Mann: need a united front for IPCC

717

cc: coleje@spot.colorado.edu, jto@ngdc.noaa.gov, k.briffa@uea.ac.uk,
luckman@sscl.uwo.ca, mann@geo.umass.edu, mhughes@ltrr.arizona.edu,
rbradley@geo.umass.edu
date: Thu, 17 Sep 1998 10:35:12 -0400 (EDT)
from: mann@snow.geo.umass.edu
to: p.jones@uea.ac.uk

Dear Phil,

Thanks for your message. I've chosen to "expand" the distribution list to include a few other individuals who can better address some of the key points you raise.

…

As for your general comments, they get to some essential points. The modeling community leaders are probably about as skeptical about our paleo-reconstructions as we are of their sulphate aerosol parameterizations, flux corrections (or more worrying, supposed lack thereof in some cases!), and handling of the oh-so-important tropical Pacific ocean-atmosphere interface...
…

More to the point, though, I strongly believe the paleo community needs to present an honest but unified front regarding what we all agree we can definitely, probably, and simply not yet say about the climate of the past several centuries, and plan strategies that will allow us all to work towards improved reconstructions without stepping on each others toes. …

I share Phil's concern about getting things "straightened out" before the IPCC report. As one of the lead authors on the "observed climate variation and change" chapter for the 3rd assessment report, a key goal of mine will
be to present fairly and accurately all of our different efforts, and the common denominator amongst them...

2490

date: Tue Oct 6 13:38:33 1998
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: climate of the last millennia...
to: "Jonathan T. Overpeck" <jto@ngdc.noaa.gov>, p.jones@uea.ac.uk, mann@snow.geo.umass.edu, rbradley@climate1.geo.umass.edu,   drdendro@ldgo.columbia.edu, coleje@spot.colorado.edu,   Brian Luckman <luckman@sscl.uwo.ca>

Hi Peck et al.
A little late but I'd like to put in my twopence worth regarding your original message and Phil's reply. I have been tied up with a load of stuff so don't interpret my lack of speedy response as a lack of interest in these matters.
My first comment is that I agree with all of your general remarks and with your implied rebuke to Phil that we should be very wary of seeming to dam certain proxies and over hype others when we all know that there are real strengths and weaknesses associted with them all. The truth is that all of this group are well aware of this and of the associated fact that even within each of these sub-disciplines e.g. Dendro, coral etc. there is a large range of value , or concern with the external usage of our data. However, **my own and Phil's concerns are motivated ,like yourself, by the outside world's inability to appreciate these points and the danger that we will all be seen as uncritical or niave about the real value of proxy data.** The rationale for the recent Jones et al paper, and some things that I have written in the past is to inform would be users , particularly the modellers, that there are critical questions to be addressed about how the palaeo-data are best used in a 'detection' or 'model validation' context.
Many in the palaeo-community understand these issues , but perhaps there has been some reluctance to air them in sufficient depth or in the right situations where they will be heard/seen by those people who now seek to use the data . I believe that many of the modellers , having been blissfully unaware for years of the need to work with the palaeo-community, are now expecting too much . This carries the danger of a backlash as they undertake simple assessments of the palaeo-series and conclude that they are all of very little use. The problem is that as we try to inform them we may get the balance between valueable self criticism and scientific flagellation wrong. The more so when the whip is seemingly aimed at others!
There is no doubt though, that many palaeo- types are not concerned with the 'bigger issues' of climate change , so it is up to those who do ,such as this group, to try to sort out some sensible approach to how we do explore the good and bad ,fairly, in our collective data and how we present this to the outside world. The meeting you propose is a good way forward.If he is already not included, I also urge you to invite Ed Cook.
…

I agree that we must be careful not to appear to be knocking other proxies- even if this is not intended . We must also be explicit about where problems lie and in suggesting the ways to overcome them. I for one do not think the world revolves only around trees. The only sensible way forward is through interpretation of multiple proxies and we need much more work comparing and reconciling the different evidence they hold. Let's have more balance in the literature and more constructive dialogue /debate between ourselves.

Keith

4123

date: Tue, 6 Oct 1998 11:06:20 -0400 (EDT)
from: mann@snow.geo.umass.edu
subject: Re: climate of the last millennia...
to: coleje@spot.colorado.edu, drdendro@ldgo.columbia.edu, jto@ngdc.noaa.gov, k.briffa@uea.ac.uk, luckman@sscl.uwo.ca, p.jones@uea.ac.uk, rbradley@climate1.geo.umass.edu

Dear all,

I just wanted to thank Keith for his comments. They are right on target. **There is indeed, as many of us are aware, at least one key player in the modeling community that has made overly dismissive statements about the value of proxy data as late**, because of what might be argued as his/her own naive assessment/analysis of these data. This presents the danger of just the sort of backlash that Keith warns of, and **makes all the more pressing the need for more of a community-wide strategizing on our part.** I think the workshop in Jan that Peck is hosting will go far in this regard, and I personally am really looking forward to it!

cheers,

mike.

1.7 Mann seems prepared to incorporate Briffa's evidence, then proposes dropping his data; Jones and Briffa fight back

1574

date: Sat, 23 Jan 1999 22:19:12 -0500 (EST)
from: mann@snow.geo.umass.edu
subject: IPCC
to: k.briffa@uea.ac.uk

Dear Keith,

Let me start out by apologizing. Meant to get in touch w/ you several weeks ago but forgot. I was hoping you would be willing to act as a contributor on the IPCC report for the climate variability and change chapter which I'm a co-lead author on.

We've already solicited from Phil a discussion of the various hemispheric temperatures reconstructiosn, etc., but **I was hoping you could provide some discussions about two specific topics** related to your recent work (and any other related work that is relevant).

1) the reduced sensitivity of high-lat dendro indicators in recent decades

and

2) the detection of volcanic events (based on dendro and otherwise).

Just a few paragraphs, and perhaps a figure if especially helpful, would be great. We have a Feb 20th deadline for preparation of the rough draft, so if you could do this within the next 1-2 weeks that would be very helpful.

SOrry again for the lateness of this message. I'm hoping it isn't too late to get this valuable contribution from you!

thanks in advance for any help you can provide.

Hoping to see you one of these days!

cheers,

mike.

1967

date: Mon, 1 Mar 1999 09:15:26 -0500 (EST)
from: mann@snow.geo.umass.edu
subject: ipcc
to: f023@cpca11.uea.ac.uk

Dear Keith,

Thanks for your message. **I think there will be opportunity for inclusion of more material such as you mention** at the next stage of revision, and will be in contact wityh you about that sometime during the next couple months. **In Figure 9, we do show the the low-frequency trend (provided by Phil) of your *newest?) average density series, but there could be more discussion of the decline issue**, etc. as you mention, and the possibility of including at leas one more figure, and certainly discussion of some others (**although my section, undoubtedly, is way too long as it its, and their will yet be substantial shortening**). So I'll be in touch soon. Thanks again for getting back to me. I hope things improve for you soon!

talk to you soon,

mike

646

cc: ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov
date: Sat, 11 Sep 1999 19:35:35 -0400
from: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
subject: IPCC revisions
to: p.jones@uea.ac.uk, k.briffa@uea.ac.uk

Dear Phil and Keith,

I wanted to get your feedback on an important suggested revision of the original (0th draft) version of TAR chapter 2.

We received some criticism in the initial review of the mixing of the truly "multiproxy" reconstructions (e.g., Jones et al and Mann et al) with e.g. dendro-only reconstructions (e.g. Briffa et al) in the current figure which compares Northern Hemisphere trends that we show in the chapter.

We would like to show just the Mann et al (1999) and Jones et al (1998) reconstructions, along w/ the instrumental record, in the "multiproxy" section of the report, leaving discussions of reconstructions based on specific proxy types to the earlier proxy-specific sections (e.g., the dendro section) and to the general section "Was there a little Ice Age and a Medieval Warm Period" which seeks to bring all of the different pieces of evidence together.

I should report that there was fairly strong support among all of the lead authors present for taking this action. But given the key role you both have in this area, I was hoping to confirm with both of you that there would be no objection to this.

Please let me know what you think as soon as you have the chance.
See you both in Venice?

best,
mike

1893

cc: ckfolland@meto.gov.uk,tkarl@ncdc.noaa.gov
date: Wed, 22 Sep 1999 12:58:14 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: IPCC revisions
to: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>,k.briffa@uea.ac.uk

Mike,
Been away in Japan the last week or so. Malcolm was there in a wheelchair because of his ruptured achilles. We both mentioned the lack of evidence for global scale change related to the MWE and LIA, but all the later Japanese speakers kept saying the same old things.

As for the TAR Chap 2 it seems somewhat arbitrary divison to exclude the tree-ring only reconstructions. Keith's reconstruction is of a different character to other tree-ring work as it is as 'hemispheric in scale' as possible so is unlike any other tree-ring related work that is reported upon.
If we go as is suggested then there would be two diagrams - one simpler one with just Mann et al and Jones et al and in another section Briffa et al. This might make it somewhat awkward for the reader trying to put them into context.
…

Another issue I would like to raise is availability of all the series you use in your reconstructions. That old chestnut again !

…

Cheers
Phil

3272

\>> >At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:
\>> >>Dear All
\>> >>
\>> >>**A proxy diagram of temperature change is a clear favourite for the**
>> Policy
>> >>Makers summary. But the current diagram with the tree ring only data
>> >>somewhat contradicts the multiproxy curve and dilutes the message rather
>> >>significantly. We want the truth. Mike thinks it lies nearer his result
\>> >>(which seems in accord with what we know about worldwide mountain
\>> glaciers
\>> >>and, less clearly, suspect about solar variations). The tree ring
\>> results
\>> >>may still suffer from lack of multicentury time scale variance. This is
\>> >>probably the most important issue to resolve in Chapter 2 at present.
\>> >>
\>> >>Chris

3272

\>> At 04:19 PM 9/22/99 +0100, Keith Briffa wrote:
\>> >
\>> >Hi everyone
\>> >   Let me say that I don't mind what you put in the policy makers

>> >summary if there is a general concensus. However some general discussion
>> >would be valuable . First **, like Phil , I think that the supposed**
>> >separation of the tree-ring reconstruction from the others on the grounds
>> >that it is not a true "multi-proxy" series is hard to justify. What is
>> true
>> >is that these particular tree-ring data best represent SUMMER
>> temperatures
>> >mostly at the northern boreal forest regions. By virtue of this **, they**
>> also
>> >definately share significant variance with Northern Hemisphere land and
>> >land and marine ANNUAL temperatures - but at decadal and multidecadal
>> >timescales - simply by virtue of the fact that these series correlated
>> with
>> >the former at these timescales. The multi proxy series (Mann et al .
>> Jones
>> >et al) supposedly represent annual and summer seasons respectively, and
>> >both contain large proportions of tree-ring input. The latest tree-ring
>> >density curve (i.e. our data that have been processed to retain low
>> >frequency information) shows more similarity to the other two series- as
>> do
>> >a number of other lower resolution data (Bradley et al, Peck et al .,
>> and
>> >new Crowley series - see our recent Science piece) **whether this**
>> represents
>> >'TRUTH' however is a difficult problem. I know Mike thinks his series is
>> >the 'best' and he might be right - but he may also be too dismissive of
>> >other data and possibly over confident in his (or should I say his use of
>> >other's). After all, the early (pre-instrumental) data are much less
>> >reliable as indicators of global temperature than is apparent in modern
>> >calibrations that include them and when we don't know the precise role of
>> >particular proxies in the earlier portions of reconstruction it remains
>> >problematic to assign genuine confidence limits at multidecadal and
>> longer
>> >timescales. **I still contend that multiple regression against the recent**
>> >very trendy global mean series is potentially dangerous. You could
>> >calibrate the proxies to any number of seasons , regardless of their true
>> >optimum response . Not for a moment am I saying that the tree-ring , or
>> any
>> >other proxy data, are better than Mike's series - indeed I am saying that
>> >the various reconstructions are not independent but that they likely
>> >contribute more information about reality together than they do alone. I
>> do
>> >believe , that **it should not be taken as read that Mike's series (or**
>> >Jone's et al. for that matter) is THE CORRECT ONE. I prefer a Figure
>> that
>> >shows a multitude of reconstructions (e.g similar to that in my Science
>> >piece). Incidently, arguing that any particular series is probably better
>> >on the basis of what we now about glaciers or solar output is flaky
>> indeed.
>> >Glacier mass balance is driven by the difference mainly in winter
>> >accumulation and summer ablation , filtered in a complex non-linear way
>> to
>> >give variously lagged tongue advance/retreat .Simple inference on the
>> >precidence of modern day snout positions does not translate easily into
>> >absolute (or relative) temperature levels now or in the past. Similarly,
>> I
>> >don't see that we are able to substantiate the veracity of different
>> >temperature reconstructions through reference to Solar forcing theories
>> >without making assumptions on the effectiveness of (seasonally specific)
>> >long-term insolation changes in different parts of the globe and the
>> >contribution of solar forcing to the observed 20th century warming .
>> > **There is still a potential problem with non-linear responses in the**
>> >very recent period of some biological proxies (or perhaps a
>> fertilisation
>> >through high CO2 or nitrate input) . I know there is pressure to present
>> a
>> >nice tidy story as regards 'apparent unprecedented warming in a thousand
>> >years or more in the proxy data' but in reality the situation is not

>> quite
>> >so simple. We don't have a lot of proxies that come right up to date and
>> >those that do (at least a significant number of tree proxies) some
>> >unexpected changes in response that do not match the recent warming. I do
>> >not think it wise that this issue be ignored in the chapter.
>> > For the record, I do believe that the proxy data do show unusually
>> >warm conditions in recent decades. I am not sure that this unusual
>> warming
>> >is so clear in the summer responsive data. **I believe that the recent**
>> warmth
>> >was probably matched about 1000 years ago. I do not believe that global
>> >mean annual temperatures have simply cooled progressively over thousands
>> of
>> >years as Mike appears to and I contend that that there is strong evidence
>> >for major changes in climate over the Holocene (not Milankovich) that
>> >require explanation and that could represent part of the current or
>> future
>> >background variability of our climate. I think the Venice meeting will
>> be
>> >a good place to air these isssues.
>> > Finally I appologise for this rather self-indulgent ramble, but I
>> >thought I may as well voice these points to you . I too would be happy to
>> >go through the recent draft of the chapter when it becomes available.
>> >
>> > cheers to all
>> > Keith

3272

>> From: Michael E. Mann [SMTP:mann@multiproxy.evsc.virginia.edu]
>> Sent: 22 September 1999 17:35
>> To: Keith Briffa; Folland, Chris; 'Phil Jones'
>> Cc: tkarl@ncdc.noaa.gov; mann@virginia.edu
>> Subject: RE: IPCC revisions
>>
>> Thanks for your response Keith,
>>
>> For all:
>>
>> Walked into this hornet's nest this morning! Keith and Phil have both
>> raised some very good points. And I should point out that Chris, through
>> no
>> fault of his own, but probably through ME not conveying my thoughts very
>> clearly to the
>> others, definitely overstates any singular confidence I have in my own
>> (Mann et al) series… I
>> certainly don't want to abuse my lead authorship by advocating my own
>> work.
>>
>> I am perfectly amenable to keeping Keith's series in the plot, and can ask
>> Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody
>> liked my own color/plotting conventions so I've given up doing this
>> myself).
>> The key thing is making sure the series are vertically aligned in a
>> reasonable
>> way. I had been using the entire 20th century, but in the case of Keith's,
>> **we need to align the first half of the 20th century w/ the corresponding**
>> mean
>> values of the other series, due to the late 20th century decline.
>>
>> So if Chris and Tom (?) are ok with this, **I would be happy to add Keith's**
>> series. That having been said, it does raise a conundrum: We demonstrate
>> (through comparining an exatropical averaging of our nothern hemisphere
>> patterns with Phil's more extratropical series) that the major
>> discrepancies between Phil's and our series can be explained in terms of
>> spatial sampling/latitudinal emphasis (seasonality seems to be secondary
>> here, but probably explains much of the residual differences). But **that**

>> explanation certainly can't rectify why Keith's series, which has similar
>> seasonality
>> *and* latitudinal emphasis to Phil's series, differs in large part in
>> exactly the opposite direction that Phil's does from ours. This is the
>> problem we
>> all picked up on (everyone in the room at IPCC was in agreement that this
>> was a problem and a potential distraction/detraction from the reasonably
>> concensus viewpoint we'd like to show w/ the Jones et al and Mann et al
>> series.
>>
>> **So, if we show Keith's series in this plot, we have to comment that**
>> **"something else" is responsible for the discrepancies in this case.**
>> **Perhaps**
>> **Keith can**
>> **help us out a bit by explaining the processing that went into the series**
>> **and the potential factors that might lead to it being "warmer" than the**
>> **Jones**
>> et al and Mann et al series?? We would need to put in a few words in this
>> regard. **Otherwise, the skeptics have an field day casting**
>> **doubt on our ability to understand the factors that influence these**
>> **estimates**
>> **and, thus, can undermine faith in the paleoestimates.** I don't think that
>> doubt is scientifically justified, and I'd hate to be the one to have
>> to give it fodder!
>>
>>
>> The recent Crowley and Lowery multiproxy estimate is an important
>> additional piece of information which I have indeed incorporated into the
>> revised draft.
…
>>
>> Looking forward to hearing back w/ comments,
>>
>> mike
>>

1.8 Briffa is isolated and under pressure to fall on his sword

2700

cc: tkarl@ncdc.noaa.gov, mann@virginia.edu
date: Thu Sep 23 18:29:05 1999
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: RE: IPCC revisions
to: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>

Dear Mike (and all)

Some remarks in response to your recent message

>I believe strongly that the strength in our discussion
>will be the fact that certain key features of past climate estimates are
>robust among a number of quasi-independent and truly independent estimates,
>each
>of which is not without its own limitations and potential biases

Mike , I agree very much with the above sentiment. **My concern was motivated by the possibility of expressing an impression of more concensus than might actually exist . I suppose the earlier talk implying that we should not 'muddy the waters' by including**

contradictory evidence worried me . IPCC is supposed to represent concensus but also areas of uncertainty in the evidence. Of course where there are good reasons for the differences in series (such as different seasonal responses or geographic bias) it is equally important not to overstress the discrepancies or suggest contradiction where it does not exist.

……

>So, if we show Keith's series in this plot, we have to comment that
>"something else" is responsible for the discrepancies in this case. Perhaps
>Keith can
>help us out a bit by explaining the processing that went into the series
>and the potential factors that might lead to it being "warmer" than the Jones
>et al and Mann et al series?? We would need to put in a few words in this
>regard. Otherwise, the skeptics have an field day casting
>doubt on our ability to understand the factors that influence these estimates
>and, thus, can undermine faith in the paleoestimates.

The best approach here is for us to circulate a paper addressing all the above points. I'll do this as soon as possible.

> I don't think that
>doubt is scientifically justified, and I'd hate to be the one to have
>to give it fodder!
>
>
>The recent Crowley and Lowery multiproxy estimate is an important
>additional piece of information which I have indeed incorporated into the
>revised draft.
>Tom actually estimates the same mean warming since the 17th century in his
>reconstruction, that we estimate in ours, so it is an added piece of
>information that Phil and I are probably in the ballpark (Tom has used
>a somewhat independent set of high and low-resolution proxy data and a very
>basic compositing methodology, similar to Bradley and Jones, so there is
>some independent new information in this estimate.
>

fair enough - but I repeat that the magnitude of the observed warming in the 20th century is different in summer and annual data

>One other key result with respect to our own work is from a paper in the
>press in "Earth Interactions". An unofficial version is available here:
>
>http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html
>
>THe key point we emphasize in this paper is that the low-frequency
>variability in our hemispheric temperature reconstruction is basically the
>same if we don't use any dendroclimatic indicators at all (though we
>certainly resolve less variance, can't get a skillful reconstruction as far
>back, and there are notable discrepancies at the decadal and interannual
>timescales). A believe I need to add a sentence to the current discussion
>on this point,
>since there is an unsubstantiated knee-jerk belief that our low-frequency
>variability is suppressed by the use of tree ring data.
>
>We have shown that this is not the case: (see here:
>http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html
>and specifically, the plot and discussion here:
>http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html
>Ironically, you'll note that there is more low-frequency variability when
>the tree ring data *are* used, then when only other proxy and
>historical/instrumental data are used!
>

This is certainly relevant and sounds really interesting. I need to look at this in detail. The effect of the including tree-ring data or not, is moderated by the importance of the particular series in the various reconstructions (relative coefficient magnitudes). **There is certainly some prospect of affecting (reducing) the apparent**

magnitude of the 20th century warming by loading on high-pass filtered chronologies , but equally a danger of exagerating it if the series used or emphasised in th calibration have been fertilized by CO2 or something else. As you know we (Tim, Phil and I) would love to collaborate with you on exploring this issue (and the role of instrumental predictors) in the various approaches.
The key here is knowing much more about the role of specific predictors through time and their associated strengths and weaknesses.

>SO I think we're in the position to say/resolve somewhat more than, frankly,
>than Keith does, about the temperature history of the past millennium.
>And the issues I've spelled out all have to be dealt with in the chapter.
>

I certainly do not disagree with you - the scale of your input data undoubtedly must contain more information than our set . I have never implied anything to the contrary. I do not believe that our data are likely to tell us more than summer variability at northern latitudes . The discussion is only about how close our and your data likely represent what they are calibrated against , back in time. Let's not imagine a disagreement where there is none.

… Tommorrow I'll send some very minor comments on typos and the like if you want them - or have you picked many of them up? Anyway , keep up the good work .

best wishes
Keith

[With respect to the above, bear in mind that Mann was, throughout this period, aware of the contents of his CENSORED folder, showing that without the handful of CO_2 –fertilized bristlecone data series his hockey stick shape disappears. Briffa had nailed the point exactly: a danger of exagerating it [20th century warming] if the series used or emphasised in th calibration have been fertilized by CO2]

1.9 Tim Osborne suggests solution to Mann: delete post-1960 data

4105

cc: k.briffa@uea,p.jones@uea
date: Tue, 05 Oct 1999 16:18:29 +0100
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Briffa et al. series for IPCC figure
to: mann@virginia.edu,imacadam@meto.gov.uk

Dear Mike and Ian

Keith has asked me to send you a timeseries for the IPCC multi-proxy reconstruction figure, to replace the one you currently have. The data are attached to this e-mail. They go from 1402 to 1995, **although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use.** I haven't put a 40-yr smoothing through them - I thought it best if you were to do this to ensure the same filter was used for all curves.

The raw data are the same as used in Briffa et al. (1998), the Nature paper that I think you have the reference for already. They are analysed in a different way, to retain the low-frequency variations. In this sense, it is one-step removed from Briffa et al. (1998). It is not two-steps removed from Briffa et al. (1998), since the new series is simply a *replacement* for the one that you have been using, rather than being one-step further.

…

With regard to the baseline, the data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep tempratures averaged over all land grid boxes with observed data that are north of 20N. As such, the

```
mean of our reconstruction over 1881-1960 matches the mean of the observed
target series over the same period.  Since the observed series consists of
degrees C anomalies wrt to 1961-90, we say that the reconstructed series
also represents degrees C anomalies wrt to 1961-90.  One could, of course,
shift the mean of our reconstruction so that it matched the observed series
over a different period - say 1931-60 - but I don't see that this improves
things.  Indeed, if the non-temperature signal that causes the decline in
tree-ring density begins before 1960, then a short 1931-60 period might
yield a more biased result than using a longer 1881-1960 period.

If you have any queries regarding this replacement data, then please e-mail
me and/or Keith.

Best regards

Tim
```

1.10 Further pressure for consensus: rewrite peoples' perceptions

914

[Shaopeng Huang, Henry Pollack and Po Yu Shen had published a paper in GRL based on borehole proxy data showing a large Medieval Warm Period. Their graph would be mentioned, but not shown, in the TAR. Instead what was shown was a separate, shorter graph only going back to the Little Ice Age.]

On Fri, 3 Mar 2000, Phil Jones wrote:

\>
\> Dear Shaopeng and Henry,
\>   First, congratulations on the Nature paper. Can you send me some
\> reprints when you get them ?
\>
\>   **I was at a meeting this week with Tom Crowley and we were discussing**
\> ways to reconcile the high-freq proxies with your borehole data. Here
\> are a couple of our thoughts. Involving Mike Mann and others here in CRU, as
\> they all have an input.
\>
\> …I realise you've taken great care with the selection, but this is
\> a nagging doubt and will be picked up by the few skeptics trying to divide
\> us all about the course of change over the last millennium. Is it possible
\> to subdivide the North American borehole data into regions where we can
\> be confident of no land-use changes (possibly and thinking aloud say Canada
\> and the western US and Alaska) ? **The aim of this (possibly joint work) is**
\> to try and reconcile the low- and high-freq proxies. Tom Crowley has a
\> series for the NH where he's combined about 20 series (a few of which are
\> in Mike's and the series we've produced here but he has over half the series
\> from less-well resolved proxies - shallow marine and lake sediments) and
\> he gets something very similar to Mike and CRU.
\>
\> 2. As all our (Mike, Tom and CRU) all show that the first few centuries of
\> the millennium were cooler than the 20th century, **we will come in for some**
\> flak from the skeptics saying we're wrong because everyone knows it was
\> warmer in the Medieval period. We can show why we believe we are correct
\> with independent data from glacial advances and even slower responding
\> proxies, however, **what are the chances of putting together a group of**
\> a very few borhole series that are deep enough to get the last 1000 years.
\> Basically trying to head off criticisms of the IPCC chapter, but good
\> science in that we will be rewriting people's perceived wisdom about
\> the course of temperature change over the past millennium. It is important
\> as studies of the millennium will help to show that the levels of natural
\> variability from models are reasonable. Tom has run his EBM with current
\> best estimates of past forcing (Be-10 as a proxy for solar output and Alan
\> Robock's ice core volcanic index) and this produces a series similar
\> to all series of the last 1000 years.
\>
\> The above is just ideas of how we, as a group, could/should try and reduce
\> criticisms etc over the next year or so. Nothing is sacred. Your North

```
>  American borehole series could be correct as it is annual and most of the
>  high-freq proxy series respond mainly to summer variations. Is yours really
>  annual when there is a marked seasonal snow cover season ?
>
>  Cheers
>  Phil
>
>
```

2. Hide the decline in WMO

WMO wanted a high profile report for their 50th anniversary. Jones oversold paleo work and it was selected for the cover. The report will be prominent; Osborne suggested hiding the decline.

191

```
cc: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk
date: Tue, 16 Nov 1999 09:20:35 +0000
from: Phil Jones <p.jones@uea.ac.uk>
subject: WMO Climate Statement for 1999 - IMPORTANT !
to: ray bradley <rbradley@geo.umass.edu>,mann@virginia.edu, mhughes@ltrr.arizona.edu
```

Dear Ray, Mike and Malcolm,

…

The pertinent item from Geneva concerns the WMO statement on the Climate of 1999. WMO has been issuing these for the past 6 years. There are 10,000 printed each time. **There were two possibilities for the front cover (1998's showed the instrumental record from 1856) - the millennial long temperature series or the contrasting storm tracks for 1998 and 1999. I was the only one voting for the latter - partly personal as I knew I would have to organise the former. I was outvoted 12-1, maybe because in a brief presentation I oversold the advances made in paleoclimate studies over the last few years !**

That's the background. WMO want to go with the millennial record on the cover and I said I would produce something and some text. The figure will be the 3 curves (Mike's, mine amd Keith/Tim's). Tim is producing this curve (all wrt 61-90 and 50 year smoothed). Each will be extended to 1999 by instrumental data for the zones/seasons they represent. The attached text briefly discusses the differences and what is shown. The text is attached
as a word file….

There will be a press release in Geneva on Dec16 - they need two weeks to approve the text internally. The full text of the report is then printed during Feb 2000 - last year's was 12 pages long. It will be released on March 15 in Geneva to coincide with WM (World Met) day and the 50th anniversary celebrations of WMO as well. **WMO are planning to print at least twice as many copies as usual and were talking about 25,000 !**
Copies go to all WMO members and are distributed at countless meetings and sent to loads of address lists available.

…

Cheers
Phil

1645

```
date: Tue Nov 16 08:57:47 1999
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: time series for WMO diagram
to: p.jones@uea
```

```
The age-banded density Briffa et al. series can be got from:
/cru/u2/f055/tree6/NHtemp_agebandbriffa.dat
It is ready calibrated in deg C wrt. 1961-90, against the average Apr-Sep land
temperature north of 20N.  It goes from 1402 to 1994 - but you really ought to replace
the values from 1961 onwards with observed temperatures due to the decline.

Rather than give you a new file of your reconstruction (Jones et al.) that is re-
calibrated, I thought it was easier to just give you the coefficients.  Your original
normalised file should be multiplied by 0.3856, and then subtract 0.1112 to give the
calibrated time series, i.e.:
CAL = (X*0.3856) - 0.1112

Cheers

Tim
```

3451

```
cc: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk
date: Tue, 16 Nov 1999 13:31:15 +0000
from: Phil Jones <p.jones@uea.ac.uk>
subject: Diagram for WMO Statement
to: ray bradley <rbradley@geo.umass.edu>,mann@virginia.edu, mhughes@ltrr.arizona.edu


 Dear Ray, Mike and Malcolm,
   Once Tim's got a diagram here we'll send that either later today or
 first thing tomorrow.
   I've just completed Mike's Nature trick of adding in the real temps
 to each series for the last 20 years (ie from 1981 onwards) amd from
 1961 for Keith's to hide the decline. Mike's series got the annual
 land and marine values while the other two got April-Sept for NH land
 N of 20N. The latter two are real for 1999, while the estimate for 1999
 for NH combined is +0.44C wrt 61-90. The Global estimate for 1999 with
 data through Oct is +0.35C cf. 0.57 for 1998.
   Thanks for the comments, Ray.

 Cheers
 Phil
```

1161

```
date: Tue Nov 16 15:34:46 1999
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: wmo cover ps and data
to: p.jones@uea

Phil - both the PS file and the data for WMO are attached - Tim
```

3. Cherry Picking

By dropping the post-1960 data that doesn't fit the theory we are cherry picking. So we can hardly criticise others for doing it too.

2753

```
cc: k.briffa@uea,t.osborn@uea
date: Mon, 28 Feb 2000 13:50:17 +0000
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: newest reconstruction
to: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
```

At 11:56 25/02/00 -0500, you wrote:
>I need your newest northern hemisphere density-based tree-ring reconstruction
>and appropriate reference for updating IPCC. Please send in ASCII format as
>soon as possible so we can incorporate. I hope all is well. Thanks,

Hi Mike

Keith asked me to get back to you on this. The reconstruction is the same as the one I sent on the 5th October 1999, but I'm sending it again in case that e-mail isn't handy. The reconstruction has now been published, in the following paper:

Briffa K.R. (2000) Annual climate variability in the Holocene: interpreting the message of ancient trees. Quaternary Science Reviews 19, 87-105.

This paper does not, however, give full details about how the reconstruction was obtained. The details are not yet published, but will soon be submitted:

Briffa KR, Osborn TJ, Schweingruber FH, Harris IC, Jones PD, Shiyatov SG and Vaganov EA (2000) Low-frequency temperature variations from a northern tree-ring density network. In preparation (to be submitted to Journal of Geophysical Research).

Details about the file I'm sending you (repeated from 5th Oct 99):

The data are attached to this e-mail. They go from 1402 to 1994, although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use. I haven't put a 40-yr smoothing through them - I thought it best if you were to do this to ensure the same filter was used for all curves. The data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep tempratures averaged over all land grid boxes (that have observed data) that are north of 20N. As such, the mean of our reconstruction over 1881-1960 matches the mean of the observed target series over the same period. Since the observed series consists of degrees C anomalies wrt to 1961-90, we say that the reconstructed series also represents degrees C anomalies wrt to 1961-90.

(I've already truncated the series at 1960 because of the problems with the recent period.)

Best regards

Tim

4758

cc: p.jones@uea.ac.uk
date: Thu, 12 Oct 2000 21:47:38 +0100
from: Tim Osborn <T.Osborn@uea.ac.uk>
subject: RE: seasonaliy
to: Keith Briffa <k.briffa@uea.ac.uk>

Hi Phil & Keith,

…

> and we wonder why the extra
>20 years will make such a difference ?

**Because how can we be critical of Crowley for throwing out 40-years in the middle of
his calibration, when we're throwing out all post-1960 data 'cos the MXD has a non-temperature signal in it, and also all pre-1881 or pre-1871 data 'cos the temperature data may have a non-temperature signal in it! If we write the Holocene forum article then we'll have to be critical or our paper as well as Crowley's!**

…

Cheers

Tim

4. Pressure to overstate things in IPCC: Extremes and Thermohaline circulation

4.1 Jones, Feb 1999: No evidence of increase in extremes

3089

```
cc: m.hulme@uea.ac.uk
date: Fri, 26 Feb 1999 10:16:06 +0000
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Dear Professor Jone,
to: Duncan Barker <D-Barker@dfid.gov.uk>
```

Dear Duncan,
The questions you are going to be addressing are not easy ones. We have done some work in them and in related areas.
…

A couple of points I can reply to :

1) From the climate scenarios we develop in CRU **there is no evidence that there will be any increase in tropical storms. In the area with the best data - the tropical Atlantic, there has been a reduction in both the numbers and the severity of Atlantic Hurricanes over the last 50 years.** There has been a lot of US work on this subject. Although only applying to the US area, the work shows that **damage (when normalized to a common point in $'s) and lives lost have both reduced.** Claims are much higher because of greater insured areas and the much greater population living in affected areas (particularly in Florida).

2) A recent paper in Climatic Change by S. Ungar, 1999 called 'Is strange weather in the air ? A study of US National Network News coverage of extreme weather events' shows that there hasn't been an increase in extreme events reporting (global areas) since the 1960s.

Most lay people beleive there has been an increase because the pictures make news stories. In the past there were reports but no pictures. The media also always like an explaination for an extreme, so the greenhouse effect or ENSO often gets the blame. There have, however, been few studies which have attempted to look at extreme events on a continental scale to se whether they have been increasing or decreasing in frequency.

I hope we can manage to sort out a convenient date.

Best Regards

Phil Jones

4.2: Houghton, July 1999: can we have more emphasis on extremes

1474

```
date: Wed, 14 Jul 1999 13:54:30 +0000
from: Sir John Houghton <jthoughton@ipccwg1.demon.co.uk>
subject: tar ch 13 draft 0 comments
to: tar13@meto.gov.uk
```

TO LINDA MEARNS AND MIKE HULME
FROM JOHN HOUGHTON

PLEASE SEE COMMENTS ON ZERO DRAFT OF CH 13

o The chapter presents useful and helpful background information and assessment for those employing or wishing to employ climate scenarios.

o The chapter would be more readable if it were shorter and punchier -many of the sentences are more complicated -or convoluted - than they need be.

o **Can there be more emphasis on climate extremes and how appropriate scenarios for these can be provided? There is quite a bit on climate variability but it is the extremes –how their intensity and frequency might change - that provide the greatest impact.**

o There is significant overlap with chapter 10 esp for instance in 13.4 which can probably be significantly shortened by reference to ch 10.

o The chapter contains a few examples. But it would be much more interesting, readable and useful if there were more examples (say 2 well chosen ones) of climate scenarios and their use.

4.3 Von Storch, March 2000: Houghton's an activist, "encouraged" regional info even though we don't have any

3419

> -----Original Message-----
> From: Hans von Storch [SMTP:h.vonstorch@phys.uu.nl]
> Sent: 13 March 2000 12:39
> To: tar10@egs.uct.ac.za
> Subject: Re: chapter 10
>
> Dear Tar10ers,
>
> I have had other obligations so far and have only now an oportunity to
> deal
> with the IPCC revision.
> I don't feel particularly bad about this as the IPCC business is
> voluntary,
> unpaid work meant to review the state of the art. That is, I am not
> willing
> to accept a dead line, given by Filippo ("so please set your mind to this
> in the next 3 weeks") or by the IPCC secretariat, which does not take
> account my other obligations. I hope to have a revised draft of 10.6 eary
> next week; updating the list of studies may need a bit longer as it means
> checking a substantial amount of additional material.
>
> Anyway, I would like to comment on some of Filippo's statements.
>
> **First, I don't think that John Houghton is particularly qualified in**
> saying
> anything about regional assessments. So far as I know he has no relevant
> official capacity in the process,and he has not been particulaly helpful
> inSAR. Actually, I consider him a politially intersted activitst and not
> as a scientist. This would be very different with somebody like Mike
> Wallace, Hans Oerlemanns or Neville Nicholls, just to give an example.
> **Thus, if Sir John thinks that something is useful or not, does not bother**
> me in any more sense as if Karlchen Mueller is making a statement, as long
> as Karlchen Mueller is a respectable scientist.
>
> **Seond: "The chapter lacks discussion of extremes and variability." I am**
> happy to include in 10.6 all statements in this respect if somebody is
> telling me where such things are published. Please come forward with the
> material.
>
> **Third: "Under the "encouragement" of Sit John, we also decided to add a**
> text box on what we can say about regional climate change over different
> continents. This will probably be the most-read part of the chapter, so we

> need to be very careful with it. I and Peter will produce a draft to
> circulate. **I know that originally we did not want to do this, but this is**
> **what they are asking us to do and it is now very clear that it is the main**
> **purpose of the chapter, so we have to do it. " I do not agree. What were**
> **the arguments we originally did not want to do this? What are the new**
> **arguments overriding our previous concerns? I am sure that people would**
> **love to read this statement in New York Times. We don't feel confident to**
> **make a statement, and then, suddenly, under the encouragement of Sir John,**
> **we cinclude it? This is truely embarassing. If the purpose of the Chapter**
> **is to produce statements on regions, and we found we can not do that, what**
> **should the assessment be? Simply: "We can not do it at this time, but we**
> **have a veriety of tehcniques to derive scenarios. However, for various**
> **reasons, we can not say that they are consistent, even if there is soem**
> **convergence."**
>
> …
>
> With regrads
>
> Hans
>
> PS: Maria, I understand that this e-mail will be filed with the IPCC
> secretariat, right?

4.4 WG2 people pressured WG1 on collapse of Thermohaline Circulation (THC)

5196

cc: zkundze@man.poznan.pl
date: Wed Oct 18 18:44:53 2000
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: Re: THC Europe
to: PARRYML@aol.com, tim.carter@vyh.fi

My view is that we cannot assess non-existent material.
It is not so much the IPCC position as the fact that to date scientists have not explored
the consequences - it is a failing (if we call it that) of science, not of the IPCC.
I doubt whether the TSU would help much here. **The people who have been pushing the TAR re.**
the THC collapse are people in WGII like Steve Schneider and Barrie Pittock. I agree with
Tim, that WGI have ducked the issue of saying anything very loud about it.
In all this we should remember, and this is a partial reply to Qs on the launch day, that
most/all of the scenarios considered by the work assessed in the TAR already *have* a
weakening of the THC (since most coupled GCMs show this). It is not a 'collapse' and it
does not take us beyond
2100, when things under some scenarios may be different.
And we certainly are not yet in a position to say how likely such behaviour is.
Worth noting that a new NERC Thematic Programme on the THC is likely to be funded as from
2001, one objective being to 'provide scenarios for risk assessment of the impacts of THC
changes on climate'. Check out [1]http://www.nerc.ac.uk/ms/THC/index.[2]htm .
Mike

4038

cc: t.mitchell
date: Fri Dec 22 16:44:54 2000
from: Mike Hulme <m.hulme@uea.ac.uk>
subject: query
to: rlniels@mail.winshop.com.au

Ron,

Tim forwarded your questions to me ....
…

[RN] 2. What effect, if any, could a change in sea currents have on the
estimated temperature increases discussed in your report?
[MH] Our T estimates allow for changes in ocean circulation, as simulated by the
current
set of GCM simulations. What you may be getting at is if there are much more dramatic
changes in ocean circulation than have been simulated in a coupled AOGCM. **Well of
course
in this case some of our country T changes may alter a lot, but this is getting into
the
realm of hand-waving - the model simulations that show a much more dramatic slow-down
of
THC circulation are either low complexity models or else are high complexity models
forced
with arbitrary forcing scenarios (e.g. 4 or 8 xCO2). The latest IPCC TAR comments on
some
of these results, but they do represent a significiant outlier compared to where we
think
climate is heading in the next 100 years.**
Best regards,
Mike

1580

date: Mon, 07 Feb 2000 11:23:33 +0000
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Draft of RP4 (Extremes etc)
to: John Shepherd <John.G.Shepherd@soc.soton.ac.uk>, Neil Adger <n.adger@uea.ac.uk>,Peter Allen <p.m.allen@cranfield.ac.uk>, Julian Andrews <j.andrews@uea.ac.uk>, Nigel Arnell <N.W.Arnell@soton.ac.uk>, Terry Barker <tsb1@econ.cam.ac.uk>, Frans Berkhout <f.berkhout@sussex.ac.uk>, Abigail Bristow <abristow@its.leeds.ac.uk>, Kate Brown <k.brown@uea.ac.uk>,Melvin Cannell <m.cannell@ite.ac.uk>, Tom Choularton <t.w.choularton@umist.ac.uk>, Trevor Davies <t.d.davies@uea.ac.uk>,Paul Dennis <p.dennis@uea.ac.uk>, Jim Halliday <j.a.halliday@rl.ac.uk>,Mike Hulme <m.hulme@uea.ac.uk>, Nick Jenkins <jenkins@fs5.ee.umist.ac.uk>, Andy Jones <a.p.jones@uea.ac.uk>, Jonathan Kohler <j.kohler@econ.cam.ac.uk>, Brian Launder <mcjtsbl@fs1.me.umist.ac.uk>, Peter Liss <p.liss@uea.ac.uk>, Gordon MacKerron <gmackerron@mistral.co.uk>, Tom Markvart <t.markvart@soton.ac.uk>, Michael McIntyre <M.E.McIntyre@damtp.cam.ac.uk>, Chris Nash <cnash@its.leeds.ac.uk>, "Tim O'Riordan" <t.oriordan@uea.ac.uk>, Jean Palutikof <j.palutikof@uea.ac.uk>,Martin Parry <parryml@aol.com>, Sarah Raper <s.raper@uea.ac.uk>, Nick Reynard <nsr@unixa.nerc-wallingford.ac.uk>, Darren Robinson <dr203@cam.ac.uk>, Simon Shackley <simon.shackley@umist.ac.uk>, John Shepherd <j.g.shepherd@soton.ac.uk>, Steve Sorrell <S.R.Sorrell@sussex.ac.uk>, Koen Steemers <kas11@cam.ac.uk>,Kerry Turner <r.k.turner@uea.ac.uk>, Andy Watkinson <a.watkinson@uea.ac.uk>,Andy Watson <a.watson@uea.ac.uk>, Ian Woodward <f.i.woodward@sheffield.ac.uk>

John,
Here are a few comments from a reading over the weekend.

At times the flow of the text isn't good. I've a few suggestions later.

1) LIA 1450-1850
2) **There is only some concern rather than serious concern about the THC.
Neither HadCM2 nor HadCM3 turn it off, only reduces it a little.
If it turns off there is nothing we can do about it, except try and
survive. I would play this down a bit, even say it is scaremongering by
Rahmstorf.**

…
Cheers
Phil

4.5 A few people decide everything

1611

date: Wed, 18 Oct 2000 20:06:18 +0300

from: Timothy Carter <tim.carter@vyh.fi>
subject: Re: THC Europe
to: PARRYML@aol.com, m.hulme@uea.ac.uk, zkundze@man.poznan.pl

Martin et al.,

Some responses [TC],

At 12:38 18/10/2000 -0400, PARRYML@aol.com wrote:
>
>Tim, Mike, Zbyszek:
>Following Mike's and Tim's comments, I think I am now back where I started
>which was to be able to state that **it is the IPCC position (since you two are**
>the scenario 'people' for WGII) that a) there are no scenarios for impacts of
>possible THC change, b)no assessment has been done AND c) THEREFORE THE IPPC
>HAS CONCLUDED NO ASSESSMENT CAN BE MADE OF IT AT THIS POINT.

[TC] IPCC doesn't make conclusions of this kind - we, the assessors, in our chapters draw the conclusion. You are the IPCC Martin, didn't you realise that?

>
>I am very happy if that is the position (which I think was\where we started 3
>years ago). I may have misunderstood Tim'spoint that the Polar ch and ch 19
>deal with THC change (but if it is to say no more than the para above, then
>we are all agreed).
>
[TC] I didn't intend to mislead. I only pointed out where the issue has been raised. **Chapter 19 engages in some detailed speculation on the issue (did you read their sections?), but without any examples of impact studies to draw on.**

>Regarding the phrase 'IPCC position'? Would it be wise to check that
>McCarthy /Watson have the same understanding as we do.

[TC] You could try, but **it has been tricky getting anyone to make statements about anything. It seems that a few people have a very strong say, and no matter how much talking goes on beforehand, the big decisions are made at the eleventh hour by a select core group.**

For example, currently the WG I extremes Table has been completely (radically!) revised by WG I for their SPM, and I will now have to do the same to our WG II Table for consistency. The THC entries in the Table are anyway unique to WG II, because WG I did not tabulate this extreme.

Indeed, only a small part of the discussion in WG I has been about the THC, and the only prominent statement that is made in the SPM (as I understand it) is from Ch 9 - that the THC is expected to weaken. THC collapse and the cooling implications for Europe is not considered explicitly - it is buried in the chapter 7 text.

So maybe that answers your question - it is not considered likely enough and/or there isn't enough scientific evidence to merit a headline statement on this.

You might like to find out the Chapter 19 view on this. That is the chapter in which this issue is pursued most vigorously. John Schellnhuber and Barrie Pittock have taken the lead on this.

>
>The reason I would like to clarify this is the following: a) It is certain
>that readers of TAR will ask: What will be would be the effect of possible
>change in THC on Europe? Our answer would be that IPCC has not assessed this
>(because scenarios have not been developed nor impact assessments done). The
>riposte may be: Then why not, but that is a riposte to the IPCC not us ; b)
>on the other hand, Mike, Jorgen and I will be presenting the ACACIA results

>to a press \briefing on 1st November; and the same Q may well arise and we
>would then give the same response (since ACACIA is a an IPCC precursor).
>Correct?

[TC] I suppose so. The IMAGE exercise (hardly a study) is the only one I know of, and that used an arbitrary scenario and was almost 10 years ago!

I think we would have to say that no AOGCMs show an abrupt collapse, and so scenarios have not be constructed or impact studies conducted to consider this unlikely phenomenon.

>
>I think I am now clear about this, but I would like to be clearer about how
>far this is an IPCC position.

That's all,

Regards,

Tim

****

Dr. Timothy Carter
Finnish Environment Institute
Box 140, Kesäkatu 6, FIN-00251 Helsinki, FINLAND

Tel: +358-9-40300-315; GSM +358-40-740-5403
Fax: +358-9-40300-390
Email: tim.carter@vyh.fi

****

4.6 WG2 Struggles to sound certain over uncertain matters

4429

cc: "Pittock,Barrie" <barrie.pittock@dar.csiro.au>, nleary@earth.usgcrp.gov, shs@stanford.edu, "Pittock,Barrie" <barrie.pittock@dar.csiro.au>, lindam@ucar.edu, m.hulme@uea.ac.uk, djgriggs@meto.gov.uk, meehl@ncar.ucar.edu, "Whetton, Peter" <peter.whetton@dar.csiro.au>, tkarl@ncdc.noaa.gov, m.manning@niwa.cri.nz, shs@stanford.edu
date: Mon, 23 Oct 2000 22:46:31 +1100
from: "Jones, Roger" <roger.jones@dar.csiro.au>
subject: RE: Table 3-10: a third version and some other considerations
to: 'Timothy Carter' <tim.carter@vyh.fi>

Dear Tim,

Well done to get all this down in the rush!

And all -

The 3rd version sits best with my point of view and I agree with the star changes suggested so far. **WGII really has to stress the importance of climate extremes in impact**, V and A analysis and cannot be seen to endorse by default a structure that cannot pass on confidences in phenomena (or create scenarios!) until they have been represented in GCMs. One can still take information from Table 3-10, regional means for P and E from climate models, and artificial or historical variability and create valid scenarios in a better form than a GCM can represent directly.
…
Is a
complete shutdown of the THC >95% not likely to happen this century? Or it could happen but we have very little confidence in the knowledge surrounding that possibility (ditto for the collapse of the WAIS). This is the reason why I think this scale is best used with the supplemental qualitative uncertainty terms in Moss and Schneider. However, further work has to be

done on how to best represent confidences in terms of probabilities, particular where several competing alternatives may be present.

…

cheers

Roger

Dr. Roger Jones
Climate Risk and Integrated Assessment Project
Climate Impact Group
CSIRO Atmospheric Research
Private Bag No.1, Aspendale
Victoria 3195 Australia

2183

cc: "Pittock,Barrie" <barrie.pittock@dar.csiro.au>, nleary@earth.usgcrp.gov, "Pittock,Barrie" <barrie.pittock@dar.csiro.au>, lindam@ucar.edu, "Jones, Roger" <roger.jones@dar.csiro.au>, m.hulme@uea.ac.uk, djgriggs@meto.gov.uk, meehl@ncar.ucar.edu, "Whetton, Peter" <peter.whetton@dar.csiro.au>, tkarl@ncdc.noaa.gov, m.manning@niwa.cri.nz
date: Mon, 23 Oct 2000 10:37:12 -0700 (PDT)
from: Stephen H Schneider <shs@stanford.edu>
subject: RE: Table 3-10: a third version and some other considerations
to: Timothy Carter <tim.carter@vyh.fi>

Hello all--…I do think two stars would be wrong because that implies we have a great deal of information--thus confidence--that the event is pretty unlikely. **Tim, you are indeed right that medium confidence means indifference to more or less since 50% is the random event. THis is why both in umpteen e-mail reviews of SPMs and TS (as well as in the guidance paper) I have tried--mostly in vain--to get people to make positive assertions without qualifiers like could.** Then medium confidence has much more meaning. For instance, your table goes at least half way--you do specify the year and rough climate scenario. **The best thing would be to make a real estimate of what might happen then--like the 10% ncrease in hurricane intensity--or give a range, say, temperature will increase by 2-4 deg C. Then a medium confidence is a pretty affirmative statement of what we think we know.** Medium confidence is true, virtually by definition, when we restrict ourselves to predicting just dierction of change and haven't much extra info to push it up or down. Nevertheless, it does make sense to keep it here, since **the WG 2 assignment is for consequences, and if it is consequential to have an event that we deem equally likely to happen and it matters, then so be it--this is represented by your last table with the impacted sectors explicit.** Of course, it would be more controvrsial and take a sub group months to craft a real range of projections for 2100 for a given scenario, but then the confidence scale would be more meaningful. But at this stage just stay with what most of us seem to be able to live with--directions of change--and thus we'll have medium confidence almost by definition for those categories where the state of the science doesn't push our confidence in the projection much higher or lower--and that is, as I said, non-trivial information for policymakers who otherwise would be clueless whether such events were expected.
Cheers, Steve

4.7 Hiding aerosol uncertainty

Joyce Penner <penner@umich.edu>
Sent by: owner-tar_ts@meto.gov.uk

To: tar_ts@meto.gov.uk
cc: tar_cla@meto.gov.uk
Subject: Issue for TS and SPM

09/25/00 03:18
PM

Dear all:

I had hoped to see a revision of the Chapt 12 "integrating" issue with respect to detection in the face of uncertainties associated with aerosols, but it never appeared. In going through my email, I see a still not-final version of Ch. 12 executive summary that remains extremely positive:

There is now stronger evidence for a human influence on global climate than at the time of the SAR.

This, I think, was the basis for making a stronger claim than that in the SAR for anthropogenic influences.

I was extremely skeptical that we could make a stronger conclusion, since, in my view, both data that was in favor of a stronger conclusion has been added (e.g. the longer record of temperature increases) and data that was in favor of a less strong conclusion (e.g. the high estimates of indirect forcing from models coupled with the clear observational evidence of influences).

In my view, adding evidence in favor and adding evidence that would shed doubt on the interpretation of the evidence in favor means that the correct formulation of the state of our knowledge is still that in the SAR:

"The balance of evidence ... for an anthropogenic influence".

I welcome your discussion (though I'm still not through with my chapter!!)

Joyce
****
Joyce Penner, Professor Office: 2516 Space Research Building
Dept. of Atmospheric, Oceanic, Phone: 734-936-0519
and Space Sciences Fax: 734-764-4585
University of Michigan E-mail: Penner@umich.edu
2455 Hayward
Ann Arbor, MI http://aoss.engin.umich.edu/Penner/
48109-2143
****

3006

cc: owner-tar_ts@meto.gov.uk, tar_cla@meto.gov.uk, tar_ts@meto.gov.uk
date: Tue, 26 Sep 2000 09:54:47 -0400
from: Michael_Oppenheimer@environmentaldefense.org
subject: Re: Issue for TS and SPM
to: Joyce Penner <penner@umich.edu>

Joyce:

The implication of the current wording is that the "balance" has shifted, whether the word "balance" is used or not. But I assume your criticism of the wording is substantive rather than semantic. **That is, in your opinion, roughly equal weight has been added on both sides since the SAR, leaving the "balance" unchanged. I disagree.** Many factors contributed to the overall judgment that "there is now stronger evidence...", and they are summarized in the four bullets at the beginning of section E. **From my point of view, the key factors were the much-improved paleoclimatic data analysis (e.g., Mann and Bradley)** that help constrain low frequency variability and demonstrate the unique behavior of inferred NH temperature beginning fairly recently; and the introduction of multi-signal, time-dependent detection and attribution methods which begin to constrain the role of solar and volcanic forcing. Balancing these developments against the broadening of the range of uncertainty for the indirect aerosol effect that has occurred since SAR (and considering other developments on both sides of the equation), leads me to the judgment that the "balance" is now struck more firmly in the direction of detection of climate change and its attribution to anthropogenic forcing.

Michael

3580

cc: tar_cla@meto.gov.uk, tar_ts@meto.gov.uk
date: Fri, 06 Oct 2000 13:20:58 -0700
from: Michael Prather <mprather@uci.edu>
subject: Re: 'balance' Issue for TS and SPM
to: Michael_Oppenheimer@environmentaldefense.org, Joyce Penner <penner@umich.edu>, John Stone <John.Stone@EC.GC.CA>, griggs <djgriggs@meto.gov.uk>

Dear David, John, Joyce, and Michael

My apologies, I have been unable to contribute to this very important debate until I cleared my chapter.

The wording in the SPM draft we were discussing (15 Apr draft given below) is far too strong a statement: it removes the fundamental issue that this finding is basically still a balance of the evidence. Admittedly what is new since the SAR is that more weight has accumulated on the "have-detected-human-influence" side of the balance (as Michael O notes). **Nevertheless, there are still some large and open problems (e.g., indirect aerosol effects) that prevent this from being a closed case.**

Today a new SPM draft appeared (6 Oct, below) that chooses more measured words (I only wish that 'balance' could somehow be worked in).

BUT the final bullet in the new section stands out in that it avoids the major new uncertainties that have been identified - merely by doing a GHGas+Sulfate vs. GHGas alone model does not address the uncertainties in "other" forcings, such as other aerosols or the history of the increase in tropospheric ozone - **which cannot be explained well and is certainly not documente**d. I doubt that these studies considered the range of uncertainty in tropospheric ozone growth or in OC/BC aerosols and indirect effects. **This last bullet cannot be supported from what I found in Chapters 4 and 5.**

I leave these issues for discussion in NY,

Michael

0869

cc: 'Joyce Penner' <penner@umich.edu>, 'Michael Prather' <mprather@uci.edu>, Michael_Oppenheimer@environmentaldefense.org, John Stone <John.Stone@ec.gc.ca>, griggs <djgriggs@meto.gov.uk>, tar_cla@meto.gov.uk, tar_ts@meto.gov.uk
date: Tue, 17 Oct 2000 08:30:09 -0600 (MDT)
from: Kevin Trenberth <trenbert@cgd.ucar.edu>
subject: RE: 'balance' Issue for TS and SPM
to: "Mitchell, John FB" <jfbmitchell@meto.gov.uk>

Hi all:

I have the impression that John is under attack here and I decided to come to his aid (I think). It is unfortunate that a number of issues have been left hanging in IPCC and have not been adequately confronted up front. It is not the fault of John's chapter either, in my view, **the problem should be shared. It relates to the large uncertainties in the aerosol forcing and the failure of chapter 6 to add up the forcings and address the issues that arise in doing so.** In chapters 9 and 12 these things have to be confronted and assumptions are made. **At some point there is some circular reasoning because the main constraint is the observed record of warming, and that places**

constraints on the magnitude of the negative radiative forcing from aerosols and couples any other changes in aerosol radiative forcing to the sensitivity response to GHGs, as John says.

This also hinges on other aspects and they are the magnitude of natural variability and thus the prevailing view that the warming observed is now well outside the realm of natural variability and thus it is forced and predictable and can be linked to the forcings.

You can argue that this is a house of cards but the building is getting stronger.

I now worry that this has not helped.
Kevin

Appendix A

Part Two

Review of Climategate 2.0 emails: 2004-2007 pre-AR4 Period

Survey of Main Themes

1. Jones' refusal to acknowledge possibility of error in his work

Summary: Jones provides surface data record to IPCC, acts as reviewer of his own work in Ch3 of the AR4. His lack of objectivity is shown by his 100% belief in his own work, dismissal of contrary evidence, immediate uncritical embrace of any new evidence supporting his position, and efforts to conceal his work from outside scrutiny.

1.1 100% Certainty

#1336

```
cc: Keith Briffa <k.briffa@uea.ac.uk>
date: Thu, 09 Mar 2006 13:48:31 +0000
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Climate Audit
to: Eystein Jansen <Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>

<x-flowed>

  Dear All,
     A lot of good points raised by the horizontal Eystein. Keith is
  hoping to do something on the recent tree growth issue.

     What this sad crowd (nice words - I'll use the phrase again) don't
  realise is that the satellite data now agree with the surface. This is
  said in Ch 3 and will come home more forcefully once the CCSP
  report on vertical temperature trends comes out. This should be
  April or May according to Tom Karl who is overseeing it all. I say
  should as it apparently has to be approved by the White House!
  Peck will know why this is and the expertise of the people doing
  the approval!

     I can say for certain (100% - not any probable word that IPCC
  would use) is that the surface temperature data are correct.

     McIntyre is determined and the blog does influence people, unfortuately
  the media.  As you say as issues are partially closed, they will move on
  to others.

  Cheers
  Phil
```

#3101 (Referring to SPM presentation – categorically claims temperature data problems are minuscule based on Jones 1990. That paper made up 0.005 figure in conclusion without basis. Jones also claims no other studies besides Parker's and his looked at large scale patterns, despite his knowing by then about the McKitrick and McIntyre and the de Laat and Maurellis studies.)

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

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

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

Affected site are identified and excluded from the records used to create the
continental and global values (as not all sites are tested, part of the error range
assumes an urban component of 0.0055 deg C/decade)
Cheers
Phil

#170 (Adjustments solve the problems. Peterson shows this in US; it's harder outside US but we have homogeneity adjustments.)

date: Fri Jul 8 11:43:30 2005
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Your hurricane article
to: Kevin Trenberth <trenbert@ucar.edu>

Kevin,
…
I got the attached from Tom Peterson yesterday. Shows that if you adjust
adequately you get the same trends as good stations that haven't changed
any routines. I presume some of these poorer sites are those that Pielke Sr. has
pictures of. He'll no doubt respond at some time to say that people use the
raw data - but clearly one needs to be in possession of all the facts, and not
just throw up ones arms and say all is wrong. The US does have good metadata,
he will likely say many other countries don't. **That is why homogeneity assessments
are done. They take a long time, they aren't sexy science and don't get reported in
detail.**
If we wanted a figure for one of the Appendix on this subject (which we don't)
this would be a good one to use. When I'm reading there later I'll see if a ref
could go in. Problem is that citing one example, opens us to others showing
more plots of raw temperatures. Best probably to talk in general terms.
Cheers
Phil

#2976 (But in March of 2004 he had told his colleagues they don't make homogeneity adjustments after mid-80s; urbanization adjustments for paper with Brohan will involve "best guess" estimates)

cc: Chris Folland <chris.folland@metoffice.com>, Simon Tett <simon.tett@metoffice.com>
date: Tue Mar 9 16:12:58 2004
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: 6 month contract - HadCRUT3
to: "Brohan, Philip" <philip.brohan@metoffice.com>

Philip,
Here are some comments on the current HadCRUT3 work plan. I've done this as
comments/suggestions and where CRU's work would fit in.

1. Land Uncertainties.
Agree we can neglect measurement error per se, but there is an additional error due to transcription/coding mistakes.We could assess this by comparing a subset of stations with similar data from GHCN. Suspect differences would be small and random. QC work has been done on the input CLIMAT data from the mid-1980s and I've spent loads of time in the past checking outliers. Values in excess of 5SDs are currently not used.

CRU work in this section would be on digitising the corrections applied to land data (from work in the mid-1980s) and process these to error fields. Agree that 5 weeks is a good estimate, but the error fields need to combine the corrections with the other data for each grid box (i.e. a grid box might be made up of 2 stations, yet only one had a correction, so the effect of the correction is halved). The influence of the corrections would be zero after about 1980 as none of this sort of work has been done since the mid-1980s.

On the thatched sheds and urbanization, I would suggest coming up with best guess values for these with some sort of ranges. We could then apply these to the data as ranges.

For example, for urbanization we have some numbers from a paper in 1991. We could apply these, then in a second run double them. It would be good to give readers an idea of what effect worst case scenarios (e.g. urban effect of 0.5 over all populated areas) might have.

So for CRU here 6 weeks.

2. Land -sea Blending. Would like to be involved here, so suggesting CRU 2 weeks. I suspect that the exact method isn't that important to large-scale averages, so need to consider a method of testing effects - against what etc.

Aside - I've been working with Adrian Simmons of ECMWF on comparisons of ERA-40 surface temps with CRUTEM2v. This has spotted lots of problems with the ERA-40 in some regions - lack of synops, but it found the Turkish problem in CRUTEM2v in Nov 1981. I suspect it is good enough to find others (obviously post-1958), but in many parts of the world ERA-40 would appear good enough to test the 'growing land'/infilling currently undertaken when the land/sea data get blended.

3. Land Gridder

The above aside may be the way to assess the effect of growing land/infilling, so CRU could do some work here - 1 week. Not sure what reverse engineer means here?

For data to be used we need normals - see later.

4/5 Optimal Interpolation/Averaging

I'll leave this up to you. People want fields not just the one final series, so if the final OA can be got from simple averaging of OI all the better. It would be good here to test the OI with the simple blending from 3).

6. Variance Correction

Key issue here is that this must be done before OI/OA. It doesn't make sense to me to do the OI/OA on non-variance corrected fields (at least when this can be done). This section is the one that could expand and get out of control. There are a number of options/routes to take, so suggest that comparison of the three or combinations of them is the way to go. I've always been surprised how relatively small this really is - at least since the 1950s. Suggest that the various methods need to be compared, then a decision on which to use is made. Intuitively, I would like to believe the one that reduces the variance the most ought to be the best.

Users want variance reduction because we introduced it. It is vital if you want to look at changes in extremes - well extremes at the monthly timescale.

Here CRU 9 weeks.

7. Generalise to other variables.

Suspect that this will only work for MSLP and maybe Humidity. It is not going to work for precipitation. For precip, GPCC will have a better product than we can ever produce from the 1950s only because they have much more data than we can ever get hold of.

8. Rationalise Normals

Given that the SST component won't have this problem, some work on the CRU normals might be useful. Work here could expand out of all proportion as we don't have the time, nor the resources to check all normals. I suspect though that some work would be beneficial. The problem the normals creates in the land component is that the average of the 1961-90 period, both for the NH/SH and for individual grid boxes doesn't average to zero. You are currently applying an adjustment to account for this. With some weeks work, we could adjust the normals to ensure this. We could us HadCRUT2v to achieve this - only changing those stations where the current normals are not based on complete

```
 1961-90 normals. I know how to do this and suggest it would take about 4 weeks.
     It would solve the need to rezero both at the large scale and at the grid-box scale.
This
 would omit one later step and reduce the number of versions.
 7. Write-up
      This is the one section that I think is very optimistic. It always takes longer.
Section 3
 of the detail on new capabilities needs careful thought.  CRU input here 4 weeks, as this
 is the most important part of the work, particularly wrt IPCC AR4.
   So, made the total come to 26, only had to increase the first and sixth by 2 weeks each.
   Here's a budget I've had produced.  As you may know payscales at all of the older
 Universities are still under negotiation, so this may increase a little (3-5%).  I have to
 budget at a level of a typical CRU employee, here one with 3-5 years experience post-PhD.
   Staff  £14478
   O/H (46%) £6660
   Travel/subsistence (3 trips to Exeter at £200 each)  £600
   Total  £21738
   I will be fully paid by UEA during this time and will be afterwards, so the costs are
 just for a 6 month Post-Doc, who will likely be on something else afterwards. So, they
 will be around for discussion etc.
   If this takes off, I need to pass these costs onto UEA, with the outlien of work. There
is
 no point my doing this until you make a decision at your end.
 Cheers
 Phil
```

1.2 Recognized of attitude by colleagues

#1435

```
date: Thu, 26 Feb 2004 11:56:46 -0000
from: "Rob Wilson" <rjwilson_dendro@blueyonder.co.uk>
subject: Fw: When Jones Meets MSU
to: <K.briffa@uea.ac.uk>

   Hi Keith,

   I am on this climate sceptics e-mail group that you might have heard of. Please don't
   groan. I am only signed on so that I can sit on the fence and see both sides of the
   argument.



   The e-mail below just came through and it is very interesting. I have already read about
   the difference between ground-based temperature data and the MSU satellite lower
   troposphere temperature data in the literature, but never really thought about it. However,
   having read this e-mail, I can't help think that IF there is a 'significant' problem with
   the ground-based data-set, that they suggest, it would partly explain the divergence
   problems between TR data and temperature data.



   Any comments?

   I did not send this e-mail to Phil as I do not know how testy he is on the subject.



   All the best

   Rob
```

1.3 Uncritical embrace of and reliance on any study apparently supporting his position

(Immediate embrace of Fu's satellite reanalysis showing strong tropospheric warming)
#770

date: Thu May 6 16:43:10 2004
from: Phil Jones <p.jones@uea.ac.uk>
subject: [Fwd: Nature Fu et al New Try against UAH MSU Satellite data]
to: Ben Santer <santer1@llnl.gov>

Ben,
You've probably seen all this - the pdf may be useful. This paper may be why Nature didn't send your paper out for review - well maybe. Saw Heike Langenburg in Nice at the EGU, but purposely didn't talk to her.
Christy and Spencer's recollection of history is from a distorted view. They barely mentioned the stratospheric contamination when the first paper came out in 1990. They must spend all their time defending their series with RSS, Vinnikov/Grody and now this new one saying their wrong ! Steve Warren and Diane Siedel know what their doing.

Hope all is well with you ! I've agreed earlier in the week to be the CLA (with Kevin) on the Atmos. Obs. chapter for 4AR. Kevin and I will have to review all these papers ! The surface temp record, precip, extremes and even the odd downward evaporation trends should all be easy.
Cheers
Phil

(Later blasted for this by his own former student Thorne for over-relying on Fu in IPCC draft) #4417

At 10:57 04/02/2005, Thorne, Peter wrote:
Kevin, Phil et al.,
my substantive comments on the upper-air portion only. Before I give specific comments below I have some over-arching comments:
This draft and the CCSP report seem at best tangential - is this desirable or sensible?
There is little effective communication in the main text of the uncertainty that is inherent in these measures due to the poor quality of the underlying data and metadata and to the choices made - "structural uncertainty". It seems that a decision has been made that RSS and the Fu et al. method are "right" or at least "most right" and this is what we will put forward as gospel truth almost. Other datasets are given a cursory once over almost. This completely ignores legitimate concerns that "structural uncertainty" is large aloft - seemingly reasonable choices made as to how you homogenise and then analyse the data can have very large effects. This is not at all clearly communicated in the current draft.
The essential distilled message that I think the analysis of UA temperatures has left us since the TAR, and what this chapter should say, is:
"Independent efforts to create climate records from satellite and radiosonde records since the TAR have served to illuminate previously unrecognised uncertainties in temperature evolution aloft (Seidel et al., 2004, Thorne et al., 2005). Further, choices in post-processing (e.g. Fu et al., 2004) may help to clarify satellite retrievals, but legitimate concerns remain (Thorne and Tett, 2004, Spencer et al., 2005) and other equally plausible approaches should be actively considered. Our increased understanding of trend uncertainty aloft means that we can no longer dismiss warming aloft of similar or greater magnitude than at the surface over the satellite record. Nor can we discount a relative cooling aloft. Uncertainties are largest in the tropics and Southern Hemisphere high latitudes where radiosonde coverage is poorest. Obviously, the climate has only evolved along a single pathway. Therefore a major challenge to the climate community is to refine our range of estimates."
This is what CCSP effectively says.
What, rightly or wrongly, I get out of the current draft on an initial read is:
"We don't like UAH. We don't believe radiosondes over the satellite period, but do over the longer period (paradox). We believe Fu et al. is correct. There is no longer any problem whatsoever."
I don't think this simple message is actually remotely supported by the science. Therefore at the very least efforts are required to balance the

text so that this is not the message communicated. I don't think we should be scared of admitting that we just don't know, if indeed we just don't know (which I believe is a fair reflection of the state of the science).
Specific comments:
p.23 lines 13-14 and 53-57 and p.24 lines 1-6. I disagree strongly with these as written. I do not believe that Fu et al. weightings is some panacea nor that the "cancellation" works on all space and timescales (the statement needs to be *proved* it cannot be accepted as an article of faith - that is not the way science works). I'd be amazed if it did. The reservations raised in the peer reviewed literature need to be better articulated here for the document to be fair and balanced. I guess this whole area will evolve significantly over the next 12 months or so though.
General concern: In the TAR we used 20N to 20S to define the tropics - here (Table 3.4.1.b) you use 30N to 30S. I'd suggest 20-20 is physically more logical and has backward compatablity and should be used. This is a recommendation of the Exeter workshop report queued for review in BAMS. Regardless, you need to alight on a single definition of these regions here and elsewhere in the report and stick to it. If you look at zonal mean profiles from any UA dataset then 20-20 shows marked trend changes N and S of it (greater warming) so using 30-30 gives a chance of a fools gold scenario arising.
In Table 3.4.1.b TLT is the acronym used in Christy et al. 2003 for T2LT - this may very well cause confusion. Admittedly I was only scanning the tables but I thought that this claimed there was a RSS 2LT channel equivalent!
Page 26 lines 28-37. This is at significant odds with the CCSP report conclusions as currently written. Much of this relates to the relative weighting being given to the Fu et al. approach by the different author teams. It will seem very odd to a policy maker to read two such disparate threads. **I particularly dislike the use on line 30 of "when the stratospheric influence is properly taken into account (Fu et al., 2004a)". How can we say it is properly taken into account that way? There are a very large population of plausible approaches that could be taken and to date we have two - a "physical" 2LT and a statistical T850-300. That is grossly insufficient to make bold statements regarding one of them properly taking the effect into account. Again, this needs balance and caveats on the Fu et al. technique until we resolve unanswered questions.** Likewise, T2LT has not been proven to be untenable in the peer reviewed press - so you cannot make this statement. My feeling is that we are missing a significant opportunity here to outline the considerable uncertainty in evolution aloft in favour of deciding one subset of approaches is right and presenting this as gospel truth. I am very uncomfortable with this. As I said it is at significant odds with CCSP.
page 26. para starting on line 46. Seems almost an afterthought. For HadAT (at least, but as they are so highly correlated, also highly likely LKS) the long-term trend in the tropics is entirely an artifact of the regime shift - if you split time periods then pre- and post-1979 have negative trends and the whole period has a strong positive trend. So to state boldly that trends agree and therefore all is well is again our living in a fools paradise. It is true, but it just shows that trend metrics are very dangerous beasties and should be handled with care. The Seidel and lanzante paper should also be quoted here.
page 68. Bullets on line 15, line 19, and line 31. Again, my concern here is that these are far too narrow and you are effectively claiming that one approach is right. Really refers back to my earlier points. This is painting a light fuzzy grey as black when I don't believe the science to date supports such an interpretation.
Page 110, line 55. Containing 676 stations (not CDRs).
Page 111, line 31 The Thorne et al. referenced is a paper under review at BAMS that you don't have in your current reference list. Reference is: Causes of differences in observed climate trends Peter W. Thorne, David E. Parker, John R. Christy, Carl A. Mears

Common question 3.2. You'll be unsurprised to hear that I think this paints too rosy a picture of our understanding the vertical structure of temperature changes. Observations do not show rising temperatures throughout the tropical troposphere unless you accept one single study and approach and discount a wealth of others. This is just downright

dangerous. We need to communicate the uncertainty and be honest.
Phil, hopefully we can find time to discuss these further if necessary
either in Chicago or when I visit in March (has a date been decided
yet?).
I'll be away from three weeks from today and unable to access this email
account. If we need to iterate further I can be reached (intermittently)
on peterwthorne@btinternet.com but will be fairly busy and then on
holiday in the middle week.
Peter
--
Peter Thorne Climate Research Scientist
Hadley Centre for climate prediction and research
Met Office, FitzRoy Road, Exeter, EX1 3PB
Tel:+44 1392 886552 Fax:+44 1392 885681 [1]http://www.hadobs.org

(Heavy dependence on wind study by Parker of the Met Office to answer all concerns about surface data)
#5325 – starting portion is from Trenberth

>> > Do you have any more or are missing any of these?
>> >
>> > The attached paper makes the same mistake as Kalnay and Cai. It
>> > believes
>> > NCEP ! **Judith Curry on the NRC panel came up with a tirade of**
>> things
>> > that she said
>> > were wrong with the surface temp record. She said she would send me
>> more
>> > references but I've only managed to locate one of those she
>> mentioned.
>> >
>> > Also if you're interested you can get Chet's paper. Saw him last
>> week.
>> >
date: Wed Mar 16 13:38:30 2005
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: ZOD IPCC
to: trenbert@ucar.edu

Kevin,
We have a paper by Parker (2004) in Nature saying it isn't important. This got used
in some Royal Society (UK) release (which has had little impact) - I'll forward this, for
interest when you're back.
It'll be difficult to find out which stations they've used in China, but I would expect
the affect to be small. Their conclusions are based on NCEP being right ! HadCRUT2v
doesn't use a place called Shenzhen, nor any place with a similar sounding name
in SE China - at least in the anglicized Chinese names we have ! Time Atlas has
Shenzhen just across the old border from Hong Kong. We use the Royal Obs. in
HK, but this stopped reporting in 1992.
Cheers
Phil

#715 (in 2007, to coauthors on 1990 paper, regarding how their results have held up. Note Peterson is study of US only.)

Phil Jones said the following on 2/20/2007 4:01 AM:

Dear All,
Remember this paper !
Jones, P.D., Groisman, P.Ya., Coughlan, M., Plummer, N., Wang, W-C. and Karl, T.R.,
1990: Assessment of urbanization effects in time series of surface air temperature over
land. Nature 347, 169-172.
Well on this web site, the work is being hotly debated!
[1]http://www.climateaudit.org/
Their renewed interest seems to stem from modifications NCDC are

```
making to USHCN and as I hear from Tom Peterson to their global and
hemispheric averages.  Ridiculous statements are being made about the
NCDC work modifying data to make recent warming greater - and more
like the CRU data!  On the Russian part of our study, the old chestnut of
temperature data being modified in Soviet days to make the data cooler
during the 1930s and 1940s!  Also the Russian network failing apart when
the Soviet Union came to an end.
```

No doubt this will surface somewhere when the Chapter from AR4 comes out. We still refer to this paper, but there are more recent studies by Tom Peterson and David Parker. These studies and some earlier ones by Tom Karl are still the only ones to look at the issue over large scales.

```
   Anyway, I'd just thought I'd warn you all in case they ever get their act
together (and stop their diatribes).
   I'd thought I'd also welcome you to the Hockey Team (but you're all
reserves) - to get onto the ice, you have to do some paleo work!
Wei-Chung therefore has a good chance of playing some day.
   It's also good that we're all still working hard in the field, most of us
writing less unfortunately as we're higher up the ladder!
   1990 seems a long time ago !  By the way, I do have the data
from the study on disk!  I was wise even when Steve McIntyre first
requested the data many years ago. I think I could replicate the
study if I had that rare commodity - time.
   The penultimate paragraph of the 1990 paper was mainly written by
Tom - thanks. It even has pre-IPCC definitions of likelihood!
 Neil - can you pass this on with my best wishes to Mike.
Cheers
Phil
```

1.4 Immediate dismissal of any work that challenges his views

#818 (complaining about having to "deal with" McKitrick and Michaels analysis in IPCC review; would end up simply leaving it out of IPCC drafts.)

```
cc: jto@u.arizona.edu,rbradley@geo.umass.edu,k.briffa@uea.ac.uk
date: Wed, 14 Jul 2004 17:08:23 +0100
from: Phil Jones <p.jones@uea.ac.uk>
subject: Fwd: Re: The broken "Hockey Stick"
to: mann@virginia.edu

 Dear All,
     I have just wasted an hour responding to this. Already had 2 calls - one from the BBC
 about this new paper by Legates !
     Whilst doing this Hameranta sent round a paper saying that Ice Ages are caused by
 cosmic rays and not related at all to Milankovitch forcing. What is the world coming to !
     Susan Solomon was here yesterday getting an honorary degree. Had a brief chat
 with her and she went out of her way to tell me that AR4 will have to deal with all this
sort
 of rubbish - so Peck (and Keith) you can deal with McIntyre and McKittrick and all the
 other paleoloonies out there.
```

I don't get away scot free, I'll have to deal with the MSU record, the Mckittrick and Michaels work on the instrumental record and more that Legates hasn't thought of yet. Latter will be easy as Legates doesn't seem to think.

```
    I'm really looking forward to the first IPCC meeting in Trieste.
    Off home now to our new house and a bottle of wine - the attached is just to show
 Ray we've moved. The front part is 17th century - might think about asking Keith to date
 some timbers, but I want the house not to fall down. It looks old and that's good enough
 for us !
 Cheers
 Phil
```

#1706 (Chylek paper in *Climatic Change* showing cooling of Greenland)

```
Phil,
What do you think of this?
Tom.
```

=================
-------- Original Message --------
Subject: Interesting Abtstract
Date: Mon, 22 Mar 2004 08:34:57 -0700
From: Joel Smith [2]<JSmith@stratusconsulting.com>
To: Tom Wigley (E-mail) [3]<wigley@ucar.edu>
CC: Jane Leggett (E-mail) [4]<leggett.jane@epa.gov>
Tom,
Relevant to our discussion about rapid melting of the Greeland Ice Sheet.
Joel
Climatic Change
63 (1-2): 201-221, March 2004
Global Warming and the Greenland Ice Sheet
Petr Chylek
Space and Remote Sensing Sciences, Los Alamos National Laboratory, Mail Stop D436, Los Alamos, NM 87545, and Department of Physics, New Mexico State University, Las Cruces, NM, U.S.A. E-mail: [5]chylek@lanl.gov <[6]mailto:chylek@lanl.gov>; Department of Physics and Atmospheric Science, Dalhousie University, Halifax, NS, Canada B3H 3J5
Jason E. Box
Byrd Polar Research Center, Ohio State University, Columbus, OH 43210, U.S.A.
Glen Lesins
Department of Physics and Atmospheric Science, Dalhousie University, Halifax, NS, Canada B3H 3J5
Abstract
The Greenland coastal temperatures have followed the early 20th century global warming trend. Since 1940, however, the Greenland coastal stations data have undergone predominantly a cooling trend. At the summit of the Greenland ice sheet the summer average temperature has decreased at the rate of 2.2 °C per decade since the beginning of the measurements in 1987. This suggests that the Greenland ice sheet and coastal regions are not following the current global warming trend. A considerable and rapid warming over all of coastal Greenland occurred in the 1920s when the average annual surface air temperature rose between 2 and 4 °C in less than ten years (at some stations the increase in winter temperature was as high as 6 °C). This rapid warming, at a time when the change in anthropogenic production of greenhouse gases was well below the current level, suggests a high natural variability in the regional climate. High anticorrelations (r = -0.84 to -0.93) between the NAO (North Atlantic Oscillation) index and Greenland temperature time series suggest a physical connection between these processes. Therefore, the future changes in the NAO and Northern Annular Mode may be of critical consequence to the future temperature forcing of the Greenland ice sheet melt rates.

Phil Jones wrote:

Tom,
I got the whole paper.
…
It has the words, this is wrong, this is crap in red on several pages. There was a lot of unwarranted speculation, many of the correlations (those in the abstract) come after filtering both series with a 5-year running mean !! Abstract didn't say that - seems
important when using the NAO.
Also, it said that a most promising NAO reconstructions was Appenzeller et al. !
This has no correlation with real world NAO that I could find, nor any relationship with
Ed Cook's or Jurg Luterbacher's.
Cheers
Phil

At 12:50 25/03/2004 -0700, you wrote:

Thanks Phil -- in a word, you seem to think the paper is crap.
So, what can one do? Seems like it is worth putting such an
opinion in print -- or doing it right.
Tom.

date: Fri Mar 26 07:59:55 2004
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: [Fwd: Interesting Abtstract]
to: Tom Wigley <wigley@cgd.ucar.edu>

Tom,
   Possibly will one day - not soon, but later this year. For Keith's Rapid project we've got all
the early Greenland data, which enables a couple of the sites on the western side to be
taken back to the early 19th century. When this gets written up, I'll add in proper correlations
with the NAO.
   For Easter, I'll give Sarah a call and see what a good time might be. I'll bring the Greenland
file.
Cheers
Phil

1.5 Deception about data availability behind 1990 paper on which he relies to defend surface record

#2655 (McIntyre's Feb 22, 2007 request to Jones for the Jones et al. 1990 data. Palmer identifies it as a legitimate request. Jones replies to McGarvie, February 26 that he cannot find or reassemble the data.)

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: Monday, February 26, 2007 10:25 AM
To: Mcgarvie Michael Mr (ACAD) k364; david.palmer@uea.ac.uk
Subject: Re: FW: Jones et al 1990

MIchael, David,
   I don't really see this as an FOI request. **I am really loathed to send them the data even if I could find it. The paper was published in 1990 and the work done in 1989. The work was done years before there was the FOI. The data used were from the Soviet Union, Australia and China.**
   One of the reason's for not helping them is this link.
[3]http://www.climateaudit.org/
and then click on the story called 'Phil Jones and the Dutiful Comrades'
The story (for want of a better way of describing it) was written by the person who has asked me for the data.
I would ask you to skim the story and read the tone of it and some of the comments on the site. No matter what I do or say will make one bit of difference to their attitudes. It will just waste my time. If you want me to go through this pointless exercise then it is only me who can do this and with a number of trips away, I don't have the time before the last week of March.
As an aside - the data we have for Malye Karamkuly is almost complete from about 1920 until 1988. This one happens to be the first one in the list. The 1990 paper had co-authors from Russia, China, Australia and the US. The Russian/Soviet data were received from the Russian. He is now working in the USA.
Best Regards
Phil

(But see email #715. Jones, one week earlier, Feb 20, 2007, to Groisman et al., his colleagues on the 1990 paper, that he still has the data.)

Dear All,
   Remember this paper !
Jones, P.D., Groisman, P.Ya., Coughlan, M., Plummer, N., Wang, W-C. and Karl, T.R., 1990: Assessment of urbanization effects in time series of surface air temperature over land. Nature 347, 169-172.
   Well on this web site, the work is being hotly debated!
[1]http://www.climateaudit.org/
   Their renewed interest seems to stem from modifications NCDC are making to USHCN and as I hear from Tom Peterson to their global and hemispheric averages. Ridiculous statements are being made about the NCDC work modifying data to make recent warming greater - and more

like the CRU data! On the Russian part of our study, the old chestnut of temperature data being modified in Soviet days to make the data cooler during the 1930s and 1940s! Also the Russian network failing apart when the Soviet Union came to an end.
No doubt this will surface somewhere when the Chapter from AR4 comes out. We still refer to this paper, but there are more recent studies by Tom Peterson and David Parker. These studies and some earlier ones by Tom Karl are still the only ones to look at the issue over large scales.
Anyway, I'd just thought I'd warn you all in case they ever get their act together (and stop their diatribes).
I'd thought I'd also welcome you to the Hockey Team (but you're all reserves) - to get onto the ice, you have to do some paleo work! Wei-Chung therefore has a good chance of playing some day.
It's also good that we're all still working hard in the field, most of us writing less unfortunately as we're higher up the ladder!
1990 seems a long time ago ! **By the way, I do have the data from the study on disk! I was wise even when Steve McIntyre first requested the data many years ago. I think I could replicate the study if I had that rare commodity - time.**
The penultimate paragraph of the 1990 paper was mainly written by Tom - thanks. It even has pre-IPCC definitions of likelihood!
Neil - can you pass this on with my best wishes to Mike.
Cheers
Phil

2. Private expressions of uncertainty or weakness of findings

2.1 Data overlap among supposedly independent paleoclimate studies

#1244
cc: Scott Rutherford <srutherford@rwu.edu>, mann@virginia.edu
date: Mon, 19 Jan 2004 15:59:36 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: Re: J. Climate paper - in confidence
to: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Briffa Keith <k.briffa@uea.ac.uk>

Malcolm,
series (5) is 'trd.dat', a Bradley & Jones (93) series. BJ93 was of course the nucleus of the MBH98 network, which was constructed by adding other indicators to that initial dataset. Of course, that does imply some redundancy, since many of the BJ93 series were composites of other data, etc. I might have gotten the reference from BJ93 for trd.dat wrong (Fritts and Shao is for correct for trw.dat, but perhaps not trd.dat, right?). I don't have BJ93 w/ me? What reference does it give for trd.dat? Scott should fix this in the revised MBH98 data list:
[1]ftp://holocene.evsc.virginia.edu/pub/sdr/temp/nature/MANNETAL98/PROXY/mbh98datasummary.txt
In any case, this hardly constitutes "considerably more overlap". This represents 1 series/indicator out of 415 series/112 indicators used.
So, in total, there are 24 density series used out of a total of 415 proxy indicators, in the MBH98 network. Its fair to say this comprises a "very small fraction" of the network, **but of course we must be careful to point out that the two networks are therefore not entirely independent. I** will modify the wording in the paper accordingly.
One final question, was each of the 24 density series in question actually used in the Briffa et al MXD network (Tim/Keith?).
Thanks all for the feedback,
mike
At 01:42 PM 1/19/2004 -0700, Malcolm Hughes wrote:

Mike - there are the following density data in that set:
1) 20 Schweingruber/Frttss series from the ITRDB (those that met the criteria described in the Mann et al 2000 EI paper)
2) Northern Fennoscandia reconstruction (from Keith)
3) Northern Urals reconstruction (from Keith)

4) 1 density series for China (Hughes data) and one from India
(also Hughes data) - neither included in Keith's data set, I think.
5) **To my great surprise I find that you used the Briffa gridded temperature reconstruction from W. N. America (mis-attributed to Fritts and Shao) - of course I should have picked up on this 6 years ago when reading the proofs of the Nature sup mat. It was my understanding that we had decided not to use these reconstructions, as the data on which they were based were in the ITRDB, and had been subject to that screening process. So depending on whether you used the long or the shorter versions of these, there will have been a considerable number of density series included , some of them twice. It means that there is considerably more overlap between the two data sets, in North America, than I have been telling people. I stand corrected.**
Cheers, Malcolm
.
.Malcolm Hughes
Professor of Dendrochronology
Laboratory of Tree-Ring Research
University of Arizona
Tucson, AZ 85721
520-621-6470
fax 520-621-8229

(#1602)

cc: Scott Rutherford <srutherford@rwu.edu>
date: Sat, 17 Jan 2004 16:32:20 -0500
from: "Michael E. Mann" <mann@virginia.edu>
subject: J. Climate paper
to: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Briffa Keith <k.briffa@uea.ac.uk>

Malcolm/Tim/Keith,
As I go over the comments of the reviewers on our manuscript, I realize that one thing we clearly have to establish is the degree of potential overlap between the data used by MBH98 and the Briffa et al MXD network.
At some level the data are clearly different--the gridding and standardization method applied to the MXD data underlying the Briffa et al network, if nothing else, is completely distinct from anything used by MBH98.
On the other hand, MBH98 did use a certain number of density series. SEe the attached MBH98 data list (I believe the ITRDB data with an "x" in the title are density data).
Can we clarify which data may be in common between the two datasets?
Thanks in advance for the help,
Mike

(#1922)

Third
I suggest this should be
Taken together , the sparse evidence of Southern Hemisphere temperatures prior to the period of instrumental records indicates that overall warming has occurred during the last 350 years, but the even fewer longer regional records indicate earlier periods that are as warm, or warmer than, 20th century means.
Fourth
fine , though perhaps "warmth" instead of "warming"?
and need to see EMIC text
Fifth
suggest delete
Sixth
suggest delete
Peck, you have to consider that since the TAR , there has been a lot of argument re "hockey stick" and the real independence of the inputs to most subsequent analyses is minimal. True, there have been many different techniques used to aggregate and scale data - but the efficacy of these is still far from established. We should be careful not to push the conclusions beyond what we can securely justify - and this is not much other than a confirmation of the general conclusions of the TAR . We must resist being pushed to present the results such that we will be accused of bias - hence no need to attack Moberg . Just need to show the "most likely"course of temperatures over the last 1300

years - which we do well I think. Strong confirmation of TAR is a good result, given that we discuss uncertainty and base it on more data. Let us not try to over egg the pudding.
For what it worth , the above comments are my (honestly long considered) views - and I would not be happy to go further . Of course this discussion now needs to go to the wider Chapter authorship, but do not let Susan (or Mike) push you (us) beyond where we know is right.
--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
http://www.cru.uea.ac.uk/cru/people/briffa/

2.2 Weakness of models

(#1498)
At 15:26 11/03/04, you [JONES] wrote:

> Tim and Mike,
> I've sent an email to Tim Mitchell for his thoughts (and asked him
> what the new job is like).
> I'm not surprised by what you've found - i.e. the large inter-model
> differences. In the EU-project
> SWURVE, we've gone back to calculating PET (assuming this is why you
> want a humidity
> type variable) with Thornthwaite and Blaney/Criddle as they only depend
> on temperature.
> This is being written into project final report and the special
> issue of HESS (Hyd. and Earth
> System Science). Project run by Chris Kilsby and he's arranged this
> issue. Even with HadCM3
> with small changes in vapour pressure (well in HadAM3P/HadRM3P - same
> there also), the
> increasing temperature means that vapour pressure deficit becomes very
> large, so PET
> calculated with Penman formula is ridiculous.
> If this is why you want vapour pressure I would suggest you go down
> this route also.
> Happy for you to forward this to Nigel as he'll understand what I'm on
> about. Hydrologists
> know that Penman should be best, but not with models. Even for 1961-90
> the problem can
> be seen in the warmer summers.
> **Basic problem is that all models are wrong - not got enough middle**
> **and low level clouds.**
> **Problem will be with us for years, according to Richard Jones. Chris has**
> **talked to him about**
> **it at length. It looks as though CSIRO2 may be the best one. CGCM2 looks**
> **most odd.**
> The HC think their variable tile parameterization may help. This can
> keep some small
> portion of open water in each box, so the whole thing doesn't dry out.
>
> There was a paper in Science a year or so ago, that showed PET (from
> evaporimeters)
> going down recently in many regions !
>
> I'll let you know what Tim thinks. Omitted the two pdfs as they were
> large. The ppt plot
> gives the essence of the message.
>
> I'm assuming here that Tim hasn't made a mistake - the HadCM3 plots
> look like the
> ones Declan produced for SWURVE a while ago and similar to ones Marie
> has produced

> for RM3P and AM3P.
>
> Cheers
> Phil

</x-flowed>

cc: m.hulme@uea.ac.uk, d.w.wilson@soton.ac.uk, Dagmar.Schroeter@pik-potsdam.de, markus.erhard@imk.fzk.de
date: Thu, 11 Mar 2004 17:00:30 +0200
from: Timothy Carter <tim.carter@ymparisto.fi>
subject: Re: Vapour pressure scenarios
to: Phil Jones <p.jones@uea.ac.uk>

<x-flowed>
Dear Phil,

Thanks for the prompt reply. I am copying this to Dave Wilson and the ATEAM co-ordinators. I suspect there may be some others in ATEAM who are using Penman and haven't looked into the humidity changes in this detail. It's probably too late to expect them to do new runs. One recommendation might be simply to assume no change in VP, but this would introduce internal inconsistency in the scenarios (though that's been the practice in many applications before humidity scenarios were being provided to impact analysts).

Apart from the weaknesses in the GCMs themselves, I wonder if the empirical conversion methods used to derive VP from temperature/SLP have contributed to some of the rather odd results. Let's see what Tim has to say.

Regards,

Tim

2.3 Weakness of data

#943 (Thread concerning CRU-TS data set, that it has serious quality failings that are not explained to users)

At 18:35 30/03/2004 +0300, Timothy Carter wrote:

Hello Tim,
Thanks for the clarifications. **I recognise the enormous amount of thought and effort that has gone in to developing these data sets and agree that this is probably the best available climatological data set at this resolution. However, therein lies the problem encountered in ATEAM, and it really is a problem. This is a climatological data set; it is not a data set that is immediately applicable in impact assessment. If the data set is to be used, it is essential, at the minimum, to understand where data are present and where they are absent. This information is not provided here. I also think that there is a difference between information on presence/absence and information on unreliability (e.g. due to interpolation). You seem to be arguing that there is a continuum between complete absence of data (relax to zero anomaly) and fully reliable data. I would at least distinguish first between some data and no data.**
The researchers applying these data are not climatologists, and I think there is a perception among most that the data sets are comprehensive in time and space (that word is even used in the title of the submitted paper!). Yes, they are comprehensive in that they offer values for each grid box and month for all variables. However, in cases where there are missing or sparse data, these "values" are simply equivalent to 1961-1990 means. This makes them unusable in most impact assessments where inter-annual variability is of importance.
So I wonder why we decided to provide the data in this format (I was part of that decision process, of course), especially since no detailed information is provided to describe those grid boxes/years in which data are missing. I don't think it is sufficient to refer to New et al. (2000) for more (and by no means complete) information. Nor is it fair to the impact analysts to expect them to "allow for this feature in their experimental design". The "feature" is hardly made clear in the documentation, and is extremely difficult to avoid, considering that the climate data were provided to partners as full 200-year pre-processed data sets. The problem is

confounded by repeating the historical inter-annual variability into the future. This procedure is fine if there is historical variability to repeat, but this wasn't the case here for at least half of the 20th century for cloud, VP and DTR.
I wonder if there is something that can be done to assist those partners who need realistic inter-annual data? One method would be to attempt to predict cloudiness and DTR from temperature and or precipitation using regression relationships developed for periods with more reliable data. The correlations are not always very high, but at least this would provide annually varying surrogate series that are related in some way to the variables (T and P) for which we do (I assume) have full coverage.
My colleague has been looking at this possibility with the detrended anomalies. The idea would be to create a surrogate series for e.g. 1901-1950, and then to repeat this series in 2001-2050, superimposed on the GCM-based trend that is already included in the data set.
Do you have any comments on this approach? It is quick and dirty, and would require some documentation. But it would then offer at least the possibility for partners requiring these data to run their models for time series that are comparable across the project for 1901-2100. This would not be the case if, for example, the 1951-2000 data were used twice historically and twice in the future. Nor would it make much sense to apply 1951-2000 inter-annual variability in cloud alongside 1901-1950 temperature and precipitation.
Sorry for prolonging the agony of this debate. I don't think this in any way invalidates the data sets, or the paper describing them. But it does require us to highlight when they can be applied and when they cannot.
Best regards,
Tim
At 13:27 30/03/04, Tim Mitchell wrote:

Tim,
I'll deal with the issues you raise below, but I think it is important to emphasise that:
(a) these data-sets, warts and all, are already in the public domain and cannot be withdrawn, but can be improved and updated;
(b) the J Clim paper submitted last July should be published ASAP, and certainly without undue delay on the part of the authors.
The data-sets have been publicly available for over a year and the proper documentation (in the peer-reviewed literature) ought to be available. Also, we have a revised version of the observed 0.5deg grids, based on a complete overhaul of the underlying databases, which extends the period covered to 2002. Thus far we have not felt able to release either these data or the accompanying paper (Mitchell and Jones, 2004) into the public domain until the previous version of the data-set has been accepted for publication.
I would recommend that you inspect not just the J Clim paper submitted last July, but also Mark New's 2000 paper on the 0.5deg gridded time-series. This gives more helpful background on the methods used in the gridding, and might clear up some misunderstandings.
You appear to be have the impression that a time-series is calculated independently for each grid-box. That is not the case. A smooth surface of anomalies is calculated for each time-step, and the grid of values is derived from the smooth surface. See the New et al 2000 paper. We - including Mark New - have always presented these spatially complete time-series as best-estimates, with data quality varying in space and time. We will never have complete records of inter-annual variability, so if we were to wait until we had, you would never have a valuable - but imperfect - data-set to use.
Regarding your numbered questions:
1. Yes it has. See New et al, 2000. There is also some discussion of this in Mitchell and Jones, 2004.
2. I would dispute your labelling of this as a 'problem'. It is actually a feature that was specifically allowed and controlled in the design of the data-sets. It only becomes a 'problem' in experiments that use these data-sets and that have not - for whatever reason - allowed for this feature in their experimental design. In some senses this feature is present for all boxes and at all periods of time, because the interpolated surface is always based on an imperfect representation of the true climate variability. DTR, and hence vapour pressure and cloud cover, are likely to be less well represented than temperature and precipitation.
3. If we had such information then it would already be included! The 0.5deg grids are based on exactly the same underlying databases, so will offer no improvement. There is no substitute for the long-term painstaking improvement of the underlying databases. There are no quick fixes.
Best wishes

Tim
On 24/3/04 6:47 pm, "Timothy Carter" <tim.carter@ymparisto.fi> wrote:
> Dear Tim,
>
> I have just talked with some ATEAM colleagues who are applying the climate
> scenarios in long-term simulations of forest growth over Europe. These
> simulations have exposed some important problems with the gridded data.
> This concerns the representation of inter-annual variability in the
> historical and scenario time series. As I understand it, in some data
> sparse regions for some periods (early in the historical record) the annual
> anomalies have been "relaxed to zero". Checking the J. Climate paper, I see
> that this is indeed reported, but the implications of this procedure have
> not been apparent to me until now. The text on page 21 of the paper is as
> follows ....
>
> "....If reflected in the time series of c, an abrupt transition in variability
> would be introduced from one century to the next. This problem is
> relatively small in Europe, so for TYN SC 1.0 advantage was taken of the
> larger sample of interannual variability available from the entire 20th
> century. ....."
>
> I'm in France at present, so can't check the data sets. However, it seems
> that for some (all?) regions of Europe, the 10' cloudiness and DTR time
> series at individual grid boxes is "flat" for the first half-century, and
> inter-annual variability only begins in the second half of the century.
> Moreover, this sequence then repeats into the 21st century, giving a sharp
> discontinuity at 2001 from variable to flat and then variable again in the
> second half of the 21st century.
>
> This procedure may well be justifiable from the climatological point of
> view (lack of stations to interpolate between), but perhaps we should have
> supplied only those parts of the time series for which inter-annual
> variability could be defined. As it is, people are applying the full series
> and noticing major effects when alternating between zero variability and
> realistic variability.
>
> I also wonder about the advisability of making these data available and
> reporting them in the paper until the time series of inter-annual
> variability are complete for ALL grid boxes.
>
> Moreover, in the submitted paper, there is mention of a different procedure
> that was used for the 0.5 degree global data set involving repeating the
> 1951-2000 series in 1901-1951.
>
> I wonder if you could clarify:
>
> 1. For what regions is inter-annual variability information lacking? Has
> this been mapped/summarised somewhere?
> 2. For which climatic variables is this a problem? Note that even 1 grid
> box could be a problem if people happen to be working in that area!
> 3. Do you have suggestions for providing inter-annual variability
> information in the periods currently lacking such data? Could we substitute
> information from the 0.5 degree grid?
>
> Sorry to bring this up at this late stage, but some people are having real
> problems with these data and I need to understand what has been done and
> how to advise the ATEAM groups.
>
> Best regards,
>
> Tim
>
>

____
Dr. T. D. Mitchell --- 07906 922 489
tim.mitchell@surrey.ac.uk

#1991 (Jones to Vose, simply delete stations that are missing data, ERA-40 Australian early figures are too warm so they'll have to be adjusted).

```
Russ,
   I'll send the update file with station data for 1991-2003 when I can find someone to
tell
 me why my disk can't be attached. Hopefully later in the day I can send it. It would be
good
 to update the series to include 2003. The file will be the same as the one you have, so
 it should just be a matter of rerunning some programs.
   I will over the next few months add some stations, but haven't done this yet. There
seem
 to be about 40 stations that now send CLIMATs as a result of being GSN stations that
I'm
 not using. I have back data for them but my files have nothing for them between 1991
 and 2000 so I omitted them from the update file.
   As for the ERA-40 work, mins do warm a little more than maxs, but they've not
calculated
 averages yet. I'm just looking at maps and can see more reds on one compared to the
 other. There are problems with a few countries though as they didn't get synops before
1967.
 Australia is blue on all maps for 1958-2001 as all surface data is too warm before 1967
 by 1 deg C at least.
   I am hoping to compare your new GHCN gridded max and min fields with ERA-40. There
 might be too much in the paper with just the means though. With NCEP as well, the ECMWF
 people have sent me loads of plots and loads of text - all of which I've still to take
in.
   Good luck with the thesis defence in April. I'm sure it will be fine.
 Cheers
 Phil
```

#1609 (Sparseness of sea level network)

```
date: Fri, 5 Jan 2007 14:20:44 +0000
from: Jonathan Gregory <j.m.gregory@reading.ac.uk>
subject: New study: Current sea level rise 'not particularly unusual'
to: Phil Jones <p.jones@uea.ac.uk>

Dear Phil

Yes, we are going to have fun with sea level in Paris. Actually I don't think
I've read that paper yet but we have a diagram already that shows the very
large variability in decadal trends from tide-gauges. Personally, I suspect
that the network is too sparse, so that the variability is to some extent
measurement noise. It is so large we have no physical explanation for it.

Thanks. Best wishes

Jonathan
```

2.4 Impacts analysis

#933

```
cc: Mike Hulme <m.hulme@uea.ac.uk>
date: Wed Mar 31 10:18:16 2004
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: ATEAM climate data
to: Timothy Carter <tim.carter@ymparisto.fi>, Tim Mitchell <tim.mitchell@surrey.ac.uk>

  Tim C.
     Quickly reading your response to Tim. M. I think you're defending impacts analysts
  far too much. Whenever I meet some of these people, I have to bite my lip to avoid
  saying something I'll regret. Impacts people need to be made aware of the limitations
  of observed data and even more of model data. What Tim has done is likely the best that
  can be done given the limitations of what we can get hold off, yet still trying to
maintain
  the weak correlations between variables.
```

At many meetings impacts people ask for model futures for variables and time intervals
we just don't have in the real world. How then do they test their models? Chris Kilsby is working to derive 5 minute rainfall scenarios for an EPSRC project, because the hydrologists on one project want this. There is one raingauge in the UK with 5 minute rainfall for 20 years. They want it for urban catchments in northern England, the long record is from Farnborough. **When pushed on this they gave us one year's data for site near Bradford. They said they had techniques for making 1000 years of records from one year of data.** Despite this being a climate change project they just thought that high-frequency rainfall variations will change according to the mean.
To show them at our next meeting, we're going through HadAM3P/H and HadRM3P/H looking at convective/total precip and large-scale/total precip ratios and A2 scenario changes. I've never seen these sorts of plots before. The results are frightening. In winter
over the Mediterranean, 90% of the rainfall over the sea is convective, but on land less than 10% is convective. I've never seen a variable delineate the coastline so well. How does
large-scale rainfall which falls on the land not fall into the sea.

Tim may not have said, but we already have one review of the J. Climate paper (from Tom WIgley) which is by Tom's standards good. I'm dreading getting the reviews back as I think it will be me who has to respond to them. I know I'm not going to have much time to respond, so the first thing I'll do will be to ask for an extension of the likely 1 month
that we'll be given - if the other reviews are as favourable as Tom's.
Cheers
Phil

#1015

cc: Mike Hulme <m.hulme@uea.ac.uk>
date: Wed, 31 Mar 2004 14:52:24 +0300
from: Timothy Carter <tim.carter@ymparisto.fi>
subject: Re: ATEAM climate data
to: Phil Jones <p.jones@uea.ac.uk>, Tim Mitchell <tim.mitchell@surrey.ac.uk>

<x-flowed>
Dear Phil,

I agree with the thrust of your argument here. Yes indeed, impacts people rarely have much of a conception about the limitations of climate data, but I would say that this is particularly true where they wish to apply scenario information. They quite often expect to be provided with scenario information at the same spatial and temporal scale as the climate inputs for their models.

I usually suggest to impact modellers who ask that they should first get their observational data in order before worrying about the scenarios. That is a obvious prerequisite for effective impact studies. The impacts observed in the recent past should be reproducable based on the climate observed during the same period. Some impact studies fall short even of this basic validation step.

If they have reasonable quality high resolution observed data (which may be the case for individual sites or even limited regions) then how they perturb this for developing scenarios of the future climate is as much an art as a science. It is also worth noting that analysts sometimes use weather generators to represent present and future climate. Often, close inspection reveals a poor representation by WGs of the observed climate, in which case baseline impacts are estimated erroneously, even before considering future climate. So, again, the baseline climate is key.

I recognise that one of the ATEAM ambitions was to have European coverage in the climate time series. This is fairly straightforward for future scenarios from GCM outputs. Unfortunately, it is not straightforward for representing the historical climate, as you at CRU can attest after 30 years of working with such data.

I am not criticising the climatological data that Tim et al. have prepared

- it is the best one could have hoped for. The problem has been that the drawbacks of the data were not effectively communicated (I take equal responsibility for this). The data set was presented as a package - very convenient to download and apply, but not very easy at all to modify by non-climatologists according to user needs. The ATEAM impacts people knew that their models are sensitive to climate variability, so they were delighted to be offered this feature in the data set. What they didn't realise, was that the data sets are actually incomplete, although they appear not to be, by having values allocated at all grid boxes.

So how are they to adjust their analysis to cover only the more reliable regions and time periods in the record? Some of the models being used in ATEAM are transient, so the effects of climate variability are cumulative over time - trees grow; species succeed one another according to tree mortality and ambient climatic variability. It isn't possible to run these models for 20 years in the 20th century and compare with the same period in the 21st century as it might be for e.g. hydrological models. Other methods are required to create a realistic time series over periods of hundreds of years.

It is these issues that we have regrettably overlooked in providing these data. In this case, I do not blame the impacts people. In fact, I am grateful to them for highlighting some obvious difficulties in providing climate data for application. Yes, the problems are documented somewhere (as Tim points out), but how many ecologists have time or expertise to find the relevant climate journals and to interpret the subtleties of the many methods used to generate these observed data?

I think the lesson to be learnt is that these data sets need to have up front (at the site of downloading) documentation that provides basic information on applications for which the data are or are not appropriate. This requires second guessing some of the potential applications, and though we already tried to do that in ATEAM we only partly succeeded. With only T and P, I doubt if anyone would have noticed any weaknesses in the data sets (they are reasonably complete). It was only because we were ambitious in introducing other variables, that the problems emerged.

I suppose this dialogue process takes time, and we learn from our mistakes.

I have seen Tom's (amazingly conciliatory) review. You should frame it in the Unit!

Best regards,

Tim C.

2.5 Climate sensitivity

#5254 (thread debating implications of knowing magnitude of MWP)

cc: wg1-ar4-ch06@joss.ucar.edu
date: Mon, 10 Jan 2005 11:19:55 -0500
from: David Rind <drind@giss.nasa.gov>
subject: Re: [Wg1-ar4-ch06] comments to 6.3.2.1 (mainly for Keith)
to: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>

Hi Stefan,

Thanks for your comments. **As to what I believe, I think that both the forcing and the response are too poorly known to make any definitive comment about climate sensitivity from this time period, although there have been plenty of people who have tried. That's basically the conclusion I drew in the climate sensitivity section, 5.8. (which includes a listing of the various references that have interpreted climate sensitivity by choosing to believe that they knew either the forcing or the response).**

With respect to the question at hand, your comment that the uncertainty for the LIA does not bear on the question of the "Medieval Warm Period": if it is the response which is at issue for the LIA, then it is equally at issue for the "MWP". As suggested above, I'm equally skeptical of the response as of the forcing, for it suggests a very low tropical sensitivity relative to that in the extratropics. There is now ample evidence in paleoclimate that what has started out as a view of small tropical response (LGM, Tertiary climates in general) is now being seen more and more as an underestimate of the tropical response. Granted, these are equilibrium climates, and it is possible the extratropical responses seen over the last 1000 years have more to do with atmospheric wave propagation changes (although what drove them?) than radiative forcing. Nevertheless, the scarcity of tropical data, and the questions associated with attempts to reconstruct them from extratropical variability, leave room for a lot of doubt on this score.

David

2.6 Weakness of paleo work

#2009
date: Thu Jan 20 10:04:49 2005
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Re:
to: Edward Cook <drdendro@ldeo.columbia.edu>

Ed
will be discussing all this early next week with Gerrard. He is doing the US stuff at least . We wish to do some longer (based on station records) stuff for some European locations and try some reconstructions against oak data also.
I am trying to track down the NAO MSc thesis but it might be that the guy only looked at post 1950 data - will let you know.
I am attaching the short 2000 year section from the ZOD of the IPCC report and the text of a "box" on the MWP (both confidential for now)
but if we can get more space , it needs expanding to cover SH and more hydro . They also want an appendix on standardisation - so you will be involved in this also.
Really happy to get critical comment here . There is no doubt that this section will attract all the
venom from the sceptics. **I find myself in the strange position of being very skeptical of the quality of all present reconstructions, yet sounding like a pro greenhouse zealot here!**
Told Peck that you (and Jan) will be CLAS
BEST WISHES
Keith

#262 (Overpeck in CAPITALS, Crowley in regular case)

date: Tue Jul 19 09:31:05 2005
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Fwd: Re: thoughts and Figure for MWP box
to: Tim Osborn <t.osborn@uea.ac.uk>

Date: Mon, 18 Jul 2005 11:50:46 -0600
To: Tom Crowley <tcrowley@duke.edu>
From: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: thoughts and Figure for MWP box
Cc: Keith Briffa <k.briffa@uea.ac.uk>,
Eystein Jansen <eystein.jansen@geo.uib.no>
X-Virus-Scanned: amavisd-new at email.arizona.edu
X-UEA-Spam-Score: 0.2
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO
Tom - thanks. Let's see what Keith says too. My comments below (BOLD)

a few comments -
1) are you trying to choose between my way of presenting things and your way - ie, w w/out composite?

IF YOU USE A COMPOSITE IN THE BOX FIGURE, THEN IT SHOULD MATCH ONE OF THE COMPOSITE SERIES IN THE TEXT FIGS. THAT WOULD BE OK?
2) with your data, do they all go through from beginning to end?
KEITH HAS TO ANSWER
3) why include chesapeake, which is likely a salinity record?
KEITH HAS TO ANSWER, BUT DON'T NEED A SALINITY RECORD.
4) some of your data are from virtually the same site - Mangazeja and yamal are both w.

siberia - I composited data available from multiple sites to produce one time series, which is equally counted against the other regions, which might (greenland, w.U.S., e. Asia) or might not have multiple records in them
KEITH HAS TO ANSWER

5) I am not sure whether it is wise to add me to the CA list, just because the reviewer is supposed to be impartial and a CA loses that appearance of impartiality if he has now been included as a CA - may want to check with Susan S. on this one to be sure - still happy to provide advice

WE CAN CHECK W/ SUSAN (WE HAVE A FEW THINGS TO DISCUSS W/ HER). LETS SEE WHAT THE FIG DISCUSSION LEADS TO FIRST. FRANKLY, I'D RATHER HAVE YOUR COMMENTS ON OUR NEW DRAFT BEFORE WE COMPLETE THE FOD, BUT I SEE YOUR POINT. IT HAS BEEN NICE HAVING YOUR INDEPENDENT COMMENTS.

6) I am happy to go in either direction - include or not include my figure - all I need are specific directions as to what to do, as CLAs you people need to decide, and then just tell me what or what not to do

THANKS - LETS SEE WHAT KEITH SAYS ABOUT ALL THIS.

7) I am a little unhappy with the emphasis on hemispheric warmth - lets face it, almost all of the long records are from 30-90N - the question is: how representative is 30-90N to the rest of the world? for the 20th c. one can do correlations with the instrumental record, but co2 has almost certainly increased the correlation scale beyond what it was preanthropogenic. you could correlate with quelcaya - not sure how many other records there are that are annual resolution - in the tropics I have produced a tropical composite (corals + Quelc.) but it only goes back to ~1780 - corals just don't live v long - in that interval at least the agreement is satisfactory with the mid latitude reconstruction but there is only 100 years extra of independent information beyond the instrumental record...THIS MAY NEED TO BE ADDRESSEDAS A GENERAL ISSUE SOMEWHERE (SHORTLY) IN YOUR DOC

I AGREE THAT WE NEED TO BE CLEAR ON THIS. KEITH?

tom
THANKS AGAIN, PECK

#2359 (responses by Briffa (in blue) to question from colleague at Hull.)

date: Wed May 4 14:28:54 2005
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: A quick question if i may.
to: R.Platt@geo.hull.ac.uk

At 23:55 03/05/2005, you wrote:

Dear Dr Briffa

Hi Rob

I know its marking season, but i wonder if you might answer me a few quick questions.

will have to be brief , 'cause got to go to China at weekend and need to do loads of stuff before

Having conducted some reading into the climate change debate, i became rather unstuck as i found myself reading in energy and environment of the rejection of mann's climate curve by mckintyre and mckittrick last year. This led me to look into more of the proxy data records, yours being one them. As i read the various discussion i suddenly had a thought, and i'm not sure where to get an answer so i hope you don't mind me asking you.

Of course not

I may be rather over simplifying dendrochronolgy, but am i correct to believe that the signal for temperature is based simply on the size of the tree ring, or is it more complex than this.

It is often mean width of rings from many trees at a site , averaged for each year AFTER measurements have been processed to remove geometric bias due to rings getting thinner as they
are laid down round an increasing circumference ie young (inner trunk) rings are thicker and older (outer trunk) rings are progressively thinner - even in constant climate. Maximum ring density (hardness of wood - related to how densely packed the cells are and how wide their cells walls are) is also used , and also has a geometric bias that needs to be accounted for.

If its not, surely the size of the tree ring, which represents growth during a certain season, can be affected by many, if not all environmental parameters.

This is a much discussed , and potentially true , issue. In fact, many theoretical models of tree growth (such as the vegetation schemes used in some large climate models) assume that tree productivity (and hence carbon sequestration) increases as CO2 increases. There is conflicting literature arguing that we can , and can not, observe such changes (over and above the influence of climate) on the growth rates of some trees in the late 20th century. Any "fertilizing" effect , such as the increased transport of nitrogen compounds to higher latitudes (that might be expected to be nitrogen poor) from increasing industrialisation might be expected to result in increased tree growth , possibly exaggerating (or obscuring) the apparent role of warming in causing modern ring widths in these areas to increase. However , while direct fertilization in trees (by N,P,K) undoubtedly causes increased ring widths (in the absence of other limitation such as by water shortage) **, it is still hotly debated as to whether the controlled greenhouse experiments , or open top chamber experiments using increased CO2 levels, actually indicate any real evidence of fertilization (except perhaps for very brief periods).** It is interesting to note , that stomatal density changes have been used to infer past atmospheric CO2 levels , during the last 10000 years, suggesting that trees adapt to the ambient CO2 , and so may not simply increase in growth proportionately.

Could an increase in carbon in the atmosphere therefore give the same result as an increase in temperature? How can one distinguish the two? and what would this mean for our understanding of proxy based climate change?

We can not give a definitive answer as of yet , but the general idea is to attempt to separate them using statistical techniques . The short answer is that we should not rule out the possibility that the apparent increase in 20thcentury tree growth around the world , might be partly due to higher CO2 levels.

Any thoughts would be gratefully received. Cheers.

In fact the issues are much more complex , due to the confounding effects of the need to manipulate tree-ring measurements before environmental interpretation , and because various aspects of the environment have shown (partly parallel) trends over the 20th century but I have to do other stuff now

Rob

cheers
Keith

#2600

cc: "Keith R. Briffa" <k.briffa@uea.ac.uk>, <eystein.jansen@geo.uib.no>
date: Tue, 19 Jul 2005 15:35:39 -0300
from: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
subject: Re: the regional section and MWP Figure
to: "Jonathan Overpeck" <jto@u.arizona.edu>, "Edward R. Cook" <drdendro@ldeo.columbia.edu>

Dear Keith and Ed,

Please, find attached the new version of the SH figure for the IPCC. I have now included the New Zealand record. All the records have been scaled to 4 °C amplitude. Variability in the Tas record is reduced compared to New Zealand and Patagonian records. The reference lines is the mean used for the calibration period in each record, 15 C for New Zealand, 14.95 C for Tasmania and 0 C for the Patagonian records (they show departures). Please, let me know if you want to introduce some changes in the figure. **The opposite phase in the Patagonia-New Zealand records is so clear before 1850,**

which is consistent with our previous TPI. For instance, in the instrumental record the 1971 and 1976 are the coolest summer in northern Patagonian during the past 70 years, but the warmest in New Zealand reconstruction!! This out of phase relationship between regions in the Southern Hemisphere points out to the difficulty of using few records to get a hemispheric average. Cheers,

Ricardo

#3409

At 12:24 14/12/2006, David Frank wrote:

Dear Kurt (and all others).
Thanks for the nice figures. I can only agree with your demonstration and point that a combination of all suitable data should produce a more robust estimate for past temperature trends.
It is more and more apparent that any record which we consider a temperature proxy underestimates the early instrumental warm season warmth. The general tendencies displayed by the newer datasets that you show, seem to be consistent with some comparisons between the early instrumental records and other previously described tree-ring recons. However, in response to Reinhard's question to the tree-ringers, I could easily say there could be a whole variety of reasons why the tree-ring data contain more low-frequency variability than they should. The troubling part is that we can, and have, put out lots of hypotheses why these records all tend to "undershoot" the early instrumental data.
From your graphs (and other quicker comparisons that i have done), it appears that Ulf's LADE-MXD record slightly underestimates the recent warming trend in the last 20 or so years in comparison to most other records (and also the instrumental data). During the earlier periods it seems to generally fall in the middle of the crowd and also captures the higher-frequency variability in the inst. records very well over a 240 year period. It seems like an advantage to be able to see how as many independent records as possible lie on the spaghetti plate.
Perhaps, Keith or Tom have some helpful insights... Any thoughts on biological autocorrelation(esp. for MXD data) and detrending issues?
best wishes,
David

cc: 'Michael Grabner' <michael.grabner@boku.ac.at>, t.m.melvin@uea.ac.uk, Reinhard Boehm <Reinhard.Boehm@zamg.ac.at>, 'Phil Jones' <p.jones@uea.ac.uk>, 'Maurizio Maugeri' <maurizio.maugeri@unimi.it>, 'Michele Brunetti' <m.brunetti@isac.cnr.it>, jan.esper@wsl.ch, 'Ulf Buentgen' <buentgen@wsl.ch>
date: Thu Dec 14 14:37:45 2006
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: AW: A plot
to: David Frank <david.frank@wsl.ch>, Kurt Nicolussi <kurt.nicolussi@uibk.ac.at>

Hi David and others
The resilience of the tree-ring information , I agree , seems only to be enhanced by the multiple data set comparison. The issue of the specific "band limited" calibration is an important one here , in as much as the different data sets will require different optimal scaling (calibrations) , and the reconstructions should be considered along with their appropriate uncertainty bands. Your remarks on the density , support our ideas regarding the possibility (or even desirability) of using "band specific calibrations" , as we discussed in the paper by Tim and myself (resurrecting the original idea by Joel). It is desirable to show the separate band reconstructions (and verification performance and regression coefficients) **. Having said all this , it remains likely that difference between temperature and tree indices is pervasive .**
I was interested also to see that in a previous message (as copied by Kurt) that your group is working on putting all the long Alpine temperature sensitive tree-ring data together - we (Tom and I with Kurt and Michael) were also working towards this (hopefully with the benefit of the data your group has published) as originally outlined in the ALP-IMP plans, and I wonder what the precise plans you have ? We would not like to work at cross purposes. Cheers
Keith

2.7 Weakness of Mann's work

#3119 (Myles Allen couldn't replicate Mann's error bars, even after talking to Mann)

cc: "Martin Juckes" <M.N.Juckes@rl.ac.uk>, <t.osborn@uea.ac.uk>
date: Fri, 25 Feb 2005 09:42:41 -0000
from: "Myles Allen" <m.allen1@physics.ox.ac.uk>
subject: RE: Millenial Temperature Reconstruction Intercomparison and
to: "Keith Briffa" <k.briffa@uea.ac.uk>, "Martin Juckes" <M.N.Juckes@rl.ac.uk>, <hegerl@duke.edu>

I floated the idea of bringing Anders Moberg and Jan Esper in on the proposal (offering them both travel money), because **one of the things I would want to do would be to get a better grasp of their error analysis, and it's always a lot easier to do this by talking friendlily to people than by reverse-engineering their papers. I tried and failed to understand Mann's error analysis using both approaches about 5 years ago, so I don't think it is worth trying again, particularly given his current level of sensitivity.** I don't think anyone was particularly against the idea, but we haven't done anything about it. Would people like me to?

Can we make a deliverable of this project a piece of public-domain IDL code (or matlab, if people prefer and someone else volunteers to write it) that takes temperature and proxy inputs and generates reconstructions using at least two methods (Moberg and Crowley et al, for example, plus ideally MBH, Juckes et al, Osborne & Briffa etc etc), providing a framework for comparison. This kind of exercise was hugely valuable in teasing out the origins of differences between different approaches to optimal fingerprinting prior to the TAR. If people provide me with inputs and exact specification of algorithms, I would be happy to produce this (if it's in IDL) as my contribution.

Myles

#4908

date: Fri, 9 Dec 2005 15:07:35 +0000 (GMT)
from: Martin Juckes <M.N.Juckes@rl.ac.uk>
subject: First draft
to: mitrie -- Anders Moberg <anders@misu.su.se>, Eduardo Zorita <Eduardo.Zorita@gkss.de>, hegerl@duke.edu, Jan Esper <esper@wsl.ch>, Keith Briffa <k.briffa@uea.ac.uk>, Myles Allen <m.allen1@physics.ox.ac.uk>, Nanne Weber <weber@knmi.nl>, t.osborn@uea.ac.uk

<x-flowed>

Hello,

here, at last, is a draft review. It is still rough, but I would appreciate any comments on the from and content. There are probably plenty of key papers I have forgotten to mention.

I've organised the discussion of the various reconstructions into thematic sections.

I've also added a couple of plots of my own, going through the steps of the Mann et al. reconstruction. I am now sceptical about the ability of his network to reconstruct temperatures back to 1000AD, but back to 1400AD appears to be robust.
The quality of these figures is currently very bad:
I'll deal with that soon.

I don't want to over emphasise the McIntyre and McKitrick claims, but I thought it was important to go through the major issues.

I've used the Briffa et al. (2001) reconstruction back to 1400AD rather than the Briffa (2000) reconstruction back to 0, because the former publication says more about the segment length curse.

cheers,

Martin

#1828

cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>,
drdendro@ldgo.columbia.edu
date: Sun, 17 Jul 2005 20:40:15 -0600
from: Jonathan Overpeck <jto@u.arizona.edu>
subject: Re: the regional section and MWP Figure
to: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>
Thanks Ricardo and Ed! **I personally am not a big fan of the Jones and Mann SH recon. It is based on so little.** On the other hand, it is in the literature. So, I leave it up to you and Keith to decide - perhaps Eystein can weigh in too.

I do, however, think it would be really helpful to included the borehole data (see prev. emails) - either as a single SH curve, or (probably better) two regional curves (Australia and S. Africa). Is there a reason this is not a good idea? Can't complain about snow bias down there...

Thanks again - I look forward to seeing the next draft and figure - complete w/ borehole I hope.

thx, Peck

#4185

> At 19:42 06/09/2006, [Hegerl] wrote:
>> ps Keith, even Mike agrees they are uncetain, so I just leave that
>> caution in
>>
>> Gabi Hegerl wrote:
>>
>>> Keith, do you say that SH temperature reconstructions are
>>> substantially more uncertain, and what
>>> section should I cite? (refrencing Andrononova et al showing that
>>> EBM runs with volcanism dont well
>>> agree with Mann 2003 SH recon, **but do we believe that recon?**)
>>>
>>> Gabi

Quoting Keith Briffa <k.briffa@uea.ac.uk>:

> Gabi
> I was away yesterday - a cold and lack of enthusiasm! **The answer to**
> your question is that NO - "we" do not believe the 2003
> reconstruction - or the earlier (Jones et al.) one either. These
> rely heavily on two long tree-based reconstructions by Ricardo
> Villalba and Antonio Lara and colleagues, in Argentina and Chile ,
> both based on a tree called Fitzroya . Now , I doubt that these
> authors would sanction either reconstruction , or the processing
> methods used to produce the chronologies. I am copying this to
> Ricardo in case he would like to disagree or expand. In Chapter 6 we
> now say that there are not sufficient data to produce a mean Southern
> Hemisphere curve , but rather we are best to consider the present

cc: ricardo@lab.cricyt.edu.ar
date: Thu, 12 Oct 2006 10:03:53 -0400
from: hegerl@duke.edu
subject: Re: quick question IPCC
to: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
Good thanks Keith - we have that caution in, still, too, as far as I remember. We got one reviewer worrying about it, but even Mike agreed that he didn't necessarily believe that recon, so I left the caution in (Andronova et al find poor agreement between their SH forced run and the recon, and I figured it was the recon since NH worked just fine).
…

Gabi

2.8 Paleo calibration/divergence problems

#4005 (Kleinen wrote to Osborne saying he has Guiot's proxy data, and since Guiot claimed to be extending grid-cell temperatures he asks if these can be used for that purpose. Osborne responds by warning about interpreting proxy graph as equal to temperatures)

At 10:38 11/12/2006, Thomas Kleinen wrote:
>Hi Tim and Keith.
>
>I have had a quick look at the Guiot and the Schweingruber data on the SOAP
>website.
>
>In his paper **Guiot writes that his aim was to extend the CRU 5° gridded**
>temperature series, but on the SOAP page only the European mean timeseries is
>available. Does the gridded timeseries exist, and do we have access somehow?
>
>The Schweingruber data is more like what I had in mind, but density / ring
>width doesn't really help, ideally I'd still need to translate that into
>temperature changes, and I had hoped I wouldn't need to go into the theory on
>that... So do you have that database as temperature changes as well, or
>should I rather use that as "qualitative" data (little growth = rather cool,
>much growth = rather warm)?
>
>Thanks.
>Thomas

cc: Keith Briffa <k.briffa@uea.ac.uk>
date: Wed, 20 Dec 2006 17:07:31 +0000
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: Guiot, Schweingruber data
to: t.kleinen@uea.ac.uk

<x-flowed>
Hi Thomas,

the gridded Guiot data do exist. I don't have a copy, though I could ask Joel for them and I'm fairly sure he would send them. **However I don't think that they are appropriate to use, since values are computed even where no proxy values are available, and grid boxes with proxy data in them also include information from other proxies, if I recall his method correctly.** For the Guiot reconstruction, therefore, I suggest just using the area-mean time series.

For the Schweingruber data, calibrated regional-mean time series from Briffa et al. (2001) are available under plates 2 and 3 here:
http://www.cru.uea.ac.uk/cru/people/briffa/jgr2001/
and are useful because the averaging enhances the signal to noise ratio and because we have estimated the error ranges (note that the error ranges are not available on Keith's webpage, so I'll email those to you separately).

We have also gridded the Schweingruber data and then calibrated it to represent April-September temperatures. These data are available in the attached text-format file. Because every grid box contains a tree-ring chronology, there is less extrapolation/interpolation and

therefore it's more appropriate for comparison with
models. **Unfortunately we haven't yet published the details of how the gridding and calibration were done. Also we have applied a completely artificial adjustment to the data after 1960, so they look closer to observed temperatures than the tree-ring data actually were -- don't rely on the match after 1960 to tell you how skilfull they really are!** Finally, note that the files gives the latitudes and
longitudes of the centre of each box above each column (which is the
time series for that box). +ve longitude is East of Greenwich
meridian, -ve is west. The time series run from 1400 to 1994.

Finally, finally, we (Keith and I) were wondering what more you
wanted to discuss about proxy data/reconstructions on Thursday
morning, and whether we could instead cover things via
emails? Please let us know what you had in mind to cover at the meeting.

Cheers

Tim

#2948 (Briffa responding to student's query about post-1980 proxies)

date: Mon Oct 24 18:23:55 2005
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Proxy data question
to: Nicola.Williams@uea.ac.uk

Nicola
it is , as you imply, only "useful" to look at proxies before the availability of climate data - as they are meant to give some information on climate when we have no other - ie they are used as palaeoclimatic substitutes. Yes , one needs to compare them with the "target" climate to be able to judge how well or poorly this substitution is . The formal regression procedures involved in "calibrating" and "verifying the calibration results" provide insight into how well the inferences on past climate are likely to stand up (as shown by quantified uncertainty bounds on our reconstructions) - provided of course that the "uniformitarian principle is maintained. The reason Mann only used the proxies up until about 1980 , is because they did not extend through to the present day at many sites , and 1980 was a convenient cut off to use in his calibration . So we can not say how well the proxies would mimic the recent (post 1980) warming. This is the point I stressed in my lecture - that **we need to update many crucial proxies , and then test the relationships we have derived for retrodicting climate - by using them to estimate the warming in different areas (and the globe) in the last 20 years and comparing the estimates with measured reality.This is likely to be a harsh test and will likely show that we may underestimate the true magnitude of the warming** - but whether to an extent that exceeds the calculated uncertainty in the past regression estimates is a moot point.
At 17:37 24/10/2005, you wrote:

Hi Keith,
I hvae a question related to your lecture and the M525 coursework which I
wondered if you could help me with.
How recently are proxy indicators (specifically tree rings) used as a
record of climate? What I mean is, can proxy records be used for the last
20 years? I realise there is instrumental data for this period but is it
possible to use proxy data to look at temperature in this most recent
period as well? Does it match with the instrumental record for this
period? I think I read in Mann et al 1998 that it was only used up to 1980
- what is the reason for this, is the time lag between change and response
this long?
Hope that makes sense!
Thanks for your help,
Nikki Williams

#316 (referring to Crowley reconstruction that appears in spaghetti graphs. Reconstruction "works" because 20 years of ill-fitting data are dropped in the early 20^{th} century)

cc: t.osborn@uea.ac.uk,p.jones@uea.ac.uk

date: Mon Jul 18 12:22:19 2005
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: crowley
to: Tom Wigley <wigley@cgd.ucar.edu>

as a first quick response - the Crowley numbers came from his paper with Lowery. I seem to remember that there were 2 versions of the composite that he produced - certainly we used the data that did not include Sargasso and Michigan site data. I presume the other (from the CRU web site) were the data used by Phil and Mike Mann that they got from him (where exactly did you pick then up from?)and could be the other data set (with those sites included). It seems odd that the values are so high in the recent period of this series and could conceivably be instrumental data , but would have to check. **The scaling of the data we used to produce the Crowley curve that formed one of the lines in our spaghetti diagram (that we put on the web site under my name and made available to NGDC), was based on taking the unscaled composite he sent and re-calibrating against April - Sept. average for land North of 20 degrees Lat., and repeating his somewhat bazaar calibration procedure (which deliberately omitted the data between 1900-1920 that did not fit with the instrumental data (remember his data are also decadal smoothed values).** In fact , as we were using summer data we calibrated over 1881-1900 (avoiding the high early decades that I still believe are biased in summer) and 1920 - 1960 , whereas he used 1856-1880 and 1920-1965. Of the precise details might differ - but the crux of the matter is that I suspect one of the Figures you show may have instrumental data in the recent period - but not ours. If you say exactly where these series came from I can ask Tim (who will have done the calibrations) to check.

…

Keith

#604 (allusion to cherry picking leading to artificial boost in statistical power, and "uniformitarian" assumption, the idea that there is a constant relationship between proxies and temperature, which is the bedrock of paleoclimate reconstructions, and which is threatened by the divergence problem)

date: Mon Nov 28 15:32:03 2005
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Fw: 2005JC003188 Decision Letter
to: Sandy Tudhope <sandy.tudhope@ed.ac.uk>

Hi Sandy
I look forward to more drink and discussion - but in the meantime hope everything is well with you. My feeling as you know , is that the pre-selection of "useful" coral records does negate some of the power in the verification - and we kow of course about the uniformatarianism assumption . However this is a first attempt , and still suggests unprecedented warming (in the context of these quite short data) and I suppose we could refer to "isotopically warmer" and put the caveats in - but the work still deserves publication. Let's see what Rob comes up with as a revision and response - but I am confident we can get this (and it deserves to be) published.
Cheers
Keith

#1341

cc: Keith Briffa <k.briffa@uea.ac.uk>
date: Tue, 14 Feb 2006 11:54:44 +0000
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: your paper today
to: peter.stott@metoffice.gov.uk

<x-flowed>
Hi Peter - thanks for your interesting question. **I don't think we can rule out systematic bias in the proxies in the most recent decades,** but random noise in the proxies is also capable of producing such a deviation, given that the noise could be autocorrelated and anyway we are working with 20-year smoothed results and the number of proxy records drops from 14 to 5 over the final few decades through to 1995 (the instrumental data are also included up to 2004, covering more of the warmest period). **There's still more work to be done, but we really need more long proxies and more brought up to date.** Cheers, Tim

At 18:40 10/02/2006, you wrote:
>Hi Tim,
>
>I enjoyed reading your paper in Science today. **One issue I was**
>interested in was the separation in fig 3D between the instrumental data
>and the proxy data. You comment in the paper that this could be expected
>consequence of noise in the proxy records but naively it looks like
>there might be something more systematic in the last few decades. Are
>you able to rule out systematic non temperature effects on the proxies
>in recent decades then ?
>
>Thanks !
>Peter
>

#4990 (Mann responding to Alley during discussion of NAS tree ring panel)

date: Sun, 19 Mar 2006 16:53:00 -0500
from: "Michael E. Mann" <mann@meteo.psu.edu>
subject: Re: Trees
to: Richard Alley <ralley@geosc.psu.edu>

Hi Richard,
Thanks for your email, and for your earnest views. There was indeed considerable discussion of thes issues on friday, the day after your talk. Both Malcolm Hughes and I discussed these issues in some detail with the committee. Please feel free to take a look at the presentation I gave to the committee:
[1]http://www.meteo.psu.edu/~mann/Mann/lectures/lectures.html
There is no doubt that there are issues with the potential non-stationarity of tree responses to climate, and this introduces caveats. As I pointed out to the committee, these issues were actually stressed in our '99 article which produced the millennial temperature reconstruction, the title of which was (emphasis added) "[2]Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations". The underlying assumption of our own work has always been that each of the proxies have their own potential problems, and "multiproxy" approaches are probably the most robust. **I don't have a particular axe to grind about any particular proxy, and recognize that there are some pretty serious potential problems with all proxies, including ice core delta o18 (as you're aware, these are not clean paleotemperature proxies at all), and Sr/Ca or o18 from corals.** There is a good discussion of the strengths and weaknesses in all of the proxies in Jones and Mann (2004): Jones, P.D., Mann, M.E., [3]Climate Over Past Millennia, Reviews of Geophysics, 42, RG2002, doi: 10.1029/2003RG000143, 2004.
I won't try to defend Rosanne D'Arrigo's analysis, because frankly many in the tree-ring community feel it was not very good work.You should be aware that her selection criteria were not as rigorous as those used by other researchers, and the conclusions she comes to reflect only the data and standardization methods she used--they don't speak for many other, in my mind, more careful studies. If you want the views of the leading experts in this community, I would refer you to my colleagues Malcolm Hughes and Keith Briffa, who have been carefully researching these issues for decades. With your permission, I'd like to forward your email to them for a more informed response--would that be ok?
>From the questions asked by the community, I really only sensed from one individual the sort of extreme tree-ring skepticism that you describe. And I frankly think the individual proved himself to be not especially informed. The committee appeared to be convinced by the responses I provided to that individual. …

thanks,
mike

2.9 Paleo reliance on bristlecone pines 06-1484 06-339 06-1906

#1484 (Note this email and the next one from Keith Briffa indicate deep skepticism about bristlecone-based reconstructions, yet he based his conclusions in the IPCC AR4 on reconstructions that rely on bristlecone-based reconstructions)

date: Mon Nov 13 09:29:49 2006

from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Mitrie
to: "Nanne Weber" <weber@knmi.nl>

Between you and I , I believe there may be problems with the analysis of the Bristlecone data. We can talk by phone about this
Keith
At 08:56 13/11/2006, you wrote:

Martin Juckes wrote:

I'm going to send an email to Prof. North of the NAS panel to ask if he really meant "don't use bristlecones", as he is quoted by McIntyre.

I talked with Bette Otto-Bliesner a few weeks ago. She was a Panel member and said that they
had asked a tree-ring specialist and he had adviced not to use BCpines. Not a very deep argument.
The report is available I think, but it is not final yet.
Keith, what do you think about this?

#339

cc: anders@misu.su.se, Eduardo.Zorita@gkss.de, hegerl@duke.edu, esper@wsl.ch, weber@knmi.nl, t.osborn@uea.ac.uk
date: Thu Nov 16 11:57:09 2006
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: Mitrie: Bristlecones
to: Martin Juckes <m.n.juckes@rl.ac.uk>, "Myles Allen" <allen@atm.ox.ac.uk>

Martin and all,
I know Franco very well - but he has not worked extensively with the Bristlecones. I still believe that it would be wise to involve Malcolm Hughes in this discussion - though I recognise the point of view that says we might like to appear (and be) independent of the original Mann, Bradley and Hughes team to avoid the appearance of collusion. **In my opinion (as someone how has worked with the Bristlecone data hardly at all!) there are undoubtedly problems in their use that go beyond the strip bark problem** (that I will come back to later).
The main one is an ambiguity in the nature and consistency of their sensitivity to temperature variations. It was widely believed some 2-3 decades ago, that high-elevation trees were PREDOMINANTLY responding to temperature and low elevation ones to available water supply (not always related in a simple way to measured precipitation) . However, **response functions (ie sets of regression coefficients on monthly mean temperature and precipitation data derived using principal components regression applied to the tree-ring data) have always shown quite weak and temporally unstable associations between chronology and climate variations** (for the high-elevations trees at least). **The trouble is that these results are dominated by inter-annual (ie high-frequency) variations and apparent instability in the relationships is exacerbated by the shortness of the instrumental records that restrict analyses to short periods, and the large separation of the climate station records from the sites of the trees.** Limited comparisons between tree-ring density data (which seem to display less ambiguos responses) imply that there is a reasonable decadal time scale association and so indicate a real temperature signal , on this time scale .The bottom line though is that these trees likely represent a mixed temperature and moisture-supply response that might vary on longer timescales.
The discussion is further complicated by the fact that the first PC of "Western US" trees used in the Mann et al. analyses is derived from a mixture of species (not just Bristlecones) and they are quite varied in their characteristics , time span, and effective variance spectra . Many show low interannual variance and a long-term declining trend , up until about 1850 , when the Bristlecones (and others) show the remarkable increasing trend up until the end of the record. The earlier negative trend could be (partly or more significantly) a consequence of the LACK of detrending to allow for age effects in the measurements (ie standardisation) - the very early sections of relative high growth were removed in their analysis, but no explicit standardistion of the data was made to account for remaining slow width changes resulting from tree aging. This is also related to the "strip bark" problem , as these types of trees will have unpredictable trends as a consequence of aging and depending on the precise nature of each tree's structure .
Another serious issue to be considered relates to the fact that the PC1 time series in the Mann et al. analysis was adjusted to reduce the positive slope in the last 150 years (on the assumption - following an earlier paper by Lamarche et al. - that this incressing growth was evidence of carbon dioxide fertilization) , by differencing the data from

another record produced by other workers in northern Alaska and Canada (which incidentally was standardised in a totally different way). This last adjustment obviously will have a large influence on the quantification of the link between these Western US trees and N.Hemisphere temperatures. **At this point , it is fair to say that this adjustment was arbitrary and the link between Bristlecone pine growth and CO2 is , at the very least, arguable.** Note that at least one author (Lisa Gaumlich) has stated that the recent growth of these trees could be temperature driven and not evidence of CO2 fertilisation.
The point of this message is to show that that this issue is complex , and I still believe the "Western US" series and its interpretation in terms of Hemispheric mean temperature is perhaps a "Pandora's box" that we might open at our peril!
What does Jan say about this - he is very acquainted with these issues?
cheers
Keith

#1906

cc: <anders@misu.su.se>, <Eduardo.Zorita@gkss.de>, <hegerl@duke.edu>, <weber@knmi.nl>, <t.osborn@uea.ac.uk>
date: Thu, 16 Nov 2006 14:51:38 -0000
from: "Rob Wilson" <rob.wilson@ed.ac.uk>
subject: Re: Mitrie: Bristlecones
to: "Keith Briffa" <k.briffa@uea.ac.uk>, "Martin Juckes" <m.n.juckes@rl.ac.uk>, "Myles Allen" <allen@atm.ox.ac.uk>, "Jan Esper" <esper@wsl.ch>

Dear All,

For the D'Arrigo et al. 2006 paper, **I did indeed consider using the Bristlecone pine data.**

However, due to the issues raised by Macintyre and others, we felt that it would be unwise to use these data, especially as our data-set was biased more to higher latitudes.

However, I did look at the data. I do not like ignoring potential data-sets.

Of the BP data that I managed to get my hands on, I identified a significant, but relatively weak, correlation with local gridded mean summer temperatures for three sites. These three sites are: Hermit Hill (N = 38; 1048-1983) and Windy Ridge (N = 29; 1050-1985) from Colorado and Sheep Mountain (N = 71; 0 1990) from California.

The attached figure compares the RCS chronology using these data (very similar to the STD version in actual fact) with the North American RCS composite series used in D'Arrigo et al. (2006). Both series have been normalised to the 1200-1750 period to highlight any potential differences in the 20th century.

There is generally fairly good coherence between the two series between 1100 and the 1900. I personally do not think we have enough sites prior to 1400, so the lack of coherence prior to 1100 might just reflect regional differences and not enough series to derive a meaningful mean function. Although correlation with gridded temperatures are relatively low (~0.40), the coherence with the NA composite would seem to suggest that temperature is the dominant signal over the last 900 years or so.

In the 20th century, the BP index values are clearly UNDER the NA mean. I would interpret this as suggesting that there does not appear to be any CO2 influence in the BP data. This of course assumes that there is no fertilisation effect in the rest of the NA data.

There is also the Salzer BP based temperature reconstruction:

[1]http://www.ncdc.noaa.gov/paleo/pubs/salzer2005/salzer2005.html

again this does not correlate particular well with gridded temperatures - in fact it is driven more by trends, but there are some similarities with my BP chronology and NA series.

I hope this helps the discussion

best regards

Rob

2.10 Existence and magnitude of MWP

#1683 (Keith Briffa, despite being IPCC Lead Author whose job was to assess evidence about MWP, agrees to play a role in a BBC documentary "proving" exceptional 20th century warmth. Unfortunately, we do not have Briffa's response to this email.)

date: Wed, 7 Sep 2005 13:56:57 +0100
from: "Jonathan Renouf" <jonathan.renouf@bbc.co.uk>
subject: Final thoughts
to: "Keith Briffa" <k.briffa@uea.ac.uk>

Hi Keith,
Good to talk to you this morning. Just a few thoughts to reiterate what we're hoping to get out of filming tomorrow.
1) Your interview appears at a crucial point in the film. Up until now our presenter (Paul Rose, he'll be there tomorrow) has followed two conflicting thoughts. On the one hand he's understood that the world is currently getting warmer. But on the other he's discovered lots of historical stories (the Bronze Age, the MWP, the LIA) which tell him that climate changes naturally all the time. In trying to resolve this paradox he's come across this thing called the hockey stick curve, and he's come to you to explain it to him.

2) **Your essential job is to "prove" to Paul that what we're experiencing now is NOT just another of those natural fluctuations we've seen in the past. The hockey stick curve is a crucial piece of evidence because it shows how abnormal the present period is** - the present warming is unprecedented in speed and amplitude, something like that. This is a very big moment in the film when Paul is finally convinced of the reality of man made global warming.

3) The hockey stick curve shows that what Paul thought were big climate events (the Bronze Age maximum, the MWP, the LIA) actually when looked at in a global context weren't quite as dramatic as he thought. They're there, but they are nothing like as sudden or big.

4) Paul can question you on things like: How reliable is the hockey stick curve? How do you work out past climate (cue for you to talk about proxies)? What drives all the "natural" fluctations in climate (this can be answered in very broad terms eg it's down to changes in the sun's output, volcanoes etc)

5) In terms of filming my first choice is to do it as a projection in Zicer, where you show the Mann curve, then flick up as many other ones as you think are important (within reason!) and elaborate the point that what's happening now is unprecedented compared to these historic records. In my ideal world, you walk right up to the projector image and point things out on the screen, with parts of the projected image falling on your heads and shoulders. Stills of tree rings or anything else climate related eg ice cores, corals, would also work as powerpoints, because you could talk about them as egs of proxies.

Hopefully this makes it clear what I'm trying to achieve.
Look forward to tomorrow.
All best
Jonathan

#262 (Overpeck (in CAPS), regarding early draft of AR4 ch 6, was criticized by Crowley regarding apparent desire to downplay MWP; backs off yet still wants to make sure text doesn't "dilute the message")

```
Date: Mon, 18 Jul 2005 11:50:46 -0600
     To: Tom Crowley <tcrowley@duke.edu>
     From: Jonathan Overpeck <jto@u.arizona.edu>
     Subject: Re: thoughts and Figure for MWP box
     Cc: Keith Briffa <k.briffa@uea.ac.uk>,
             Eystein Jansen <eystein.jansen@geo.uib.no>
     X-Virus-Scanned: amavisd-new at email.arizona.edu
     X-UEA-Spam-Score: 0.2
     X-UEA-Spam-Level: /
     X-UEA-Spam-Flag: NO
     Tom - thanks. Let's see what Keith says too. My comments below (BOLD)
```

…

Dear Peck, Eystein and Tom
At this point we thought it was important to review where we think we are with the MWP Figure.
First, we have no objection to a Figure . Our only concerns have been that we should 1/... be clear what we wish this Figure to illustrate (in the specific context of the MWP box) - note that this is very different from trying to produce a Figure in such a way as to bias what it says (I am not suggesting that we are, but we have to guard against any later charge that we did this). **We say this because there are intonations in some of Peck's previous messages that he wishes to "nail" the MWP - i.e. this could be interpreted as trying to say there was no such thing**, and

SORRY TO SCARE YOU. I **ABSOLUTELY** AGREE THAT WE MUST AVOID ANY BIAS OR PERCEPTION OF BIAS. MY COMMENT ON "NAILING" WAS MADE TO MEAN THAT ININFORMED PEOPLE KEEPING COMING BACK TO THE MWP, AND DESCRIBING IT FOR WHAT I BELIEVE IT WASN'T. OUR JOB IS TO MAKE IT CLEAR WHAT IT WAS WITHIN THE LIMITS OF THE DATA. IF THE DATA ARE NOT CLEAR, THEN WE HAVE TO BE NOT CLEAR. THAT SAID, I THINK TOM'S FIGURE CAPTURED WHAT I HAVE SENSED IS THE MWP FOR A LONG TIME, AND BASED ON OTHER SOURCES OF INFO - INCLUDING KEITH'S PROSE. THE IDEA OF A FIGURE, IS THAT FIGURES CAN BE MORE COMPELLING AND CONNECT BETTER THAN TEXT. ALSO, THERE ARE MANY WAYS TO LOOK AT THE MWP, AND AS LONG AS WE DON'T INTRODUCE BIAS OR ANYTHING ELSE THAT WILL DILUTE THE MESSAGE IN THE END, THE IDEA IS TO SHOW THE MWP IN MORE WAYS THAN TWO (THAT IS, THE EXISTING FIGS IN THE TEXT THAT KEITH AND TIM MADE).

2.11 Likely Roman Warm Period, Mid-Holocene Optimum (~4000 BC)

#262 (Crowley emphasizes likelihood of Roman Warm Period)

```
Date: Mon, 18 Jul 2005 11:50:46 -0600
     To: Tom Crowley <tcrowley@duke.edu>
     From: Jonathan Overpeck <jto@u.arizona.edu>
     Subject: Re: thoughts and Figure for MWP box
     Cc: Keith Briffa <k.briffa@uea.ac.uk>,
             Eystein Jansen <eystein.jansen@geo.uib.no>
     X-Virus-Scanned: amavisd-new at email.arizona.edu
     X-UEA-Spam-Score: 0.2
     X-UEA-Spam-Level: /
     X-UEA-Spam-Flag: NO
     Tom - thanks. Let's see what Keith says too. My comments below (BOLD)
```

…

a few comments -
Jonathan Overpeck wrote:

Hi Keith, Eystein and Tom: See below (BOLD) for my comments. Thanks for moving this forward and making sure we do it right (i.e., without any bias, or perception of bias).

Dear Peck, Eystein and Tom
At this point we thought it was important to review where we think we are with the MWP Figure.

```
First, we have no objection to a Figure . Our only concerns have been that we should
  …
 2/ ...agree that we have done this in the best way.
```
The truth is that there IS a period of relative warmth around the end of the 1st and start of the 2nd millennium C.E. , but that there are much fewer data to base this conclusion on (and hence the uncertainty around even our multiple calibrated multi-proxy reconstructions are wide). The geographical spread of data also impart a northern (and land) bias in our early proxy data.

NEED TO BE CLEAR ABOUT THIS BIAS IN THE CAPTION AND BOX TEXT

#2067 (Jones replying to inquiry from colleague)

```
cc: john.sefton@tiscali.co.uk, k.briffa@uea.ac.uk
date: Wed, 18 Oct 2006 11:53:53 +0100 (BST)
from: P.Jones@uea.ac.uk
subject: Re: FW: Medieval Warm Period
to: "Sheppard Sylv Miss \(SCI-LS\) ks918" <Sylvia.Sheppard@uea.ac.uk>

 John,
   The simple answer to your question is that the average
 temperatures (global) for the last 20 years are likely
 warmer than they were during the MWP. Keith Briffa
 who I've cc'd can send you a paper and a diagram. I'm
 on travel at the moment.
   Although we are warmer now than during the MWP, it
 might hve been warmer during the Roman Warm Period
 and was also likely warmer about 6000 years ago.
   So warmest only now in the last 1000 years context.

 Cheers
 Phil
>
> -----Original Message-----
> From: John Sefton [mailto:john.sefton@tiscali.co.uk]
> Sent: Thursday, May 17, 2007 3:33 PM
> To: cru@uea.ac.uk
> Subject: Medieval Warm Period
>
>
>
>         Can you help please.
>
> I have found a graph ex NOAA showing temperatures from the year 1000 to
> 2000. Accepting the uncertainties about temperature measurement and
> variability throghout the Northern hemishere could you answer this
> question.
>
> What were the temperature variations in the Medieval Warm period and do
> those noted in the last 20 years exceed them. Essentially can we say
> that currently this has been the warmest period since the Last Ice Age.
>
> Thank you- references would be fine
>
> John Sefton
>
```

3. IPCC reliance on backchannel processes outside of proper review system

For insiders, there are unwritten rules that allow them to send material directly to chapter authors, even if unpublished, which then goes straight into chapter drafts. These emails show the backroom influence of Mann, even though he was not a lead author.

#899 (ROG = Review of Geophysics)

date: Mon Mar 15 13:24:13 2004
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: ROG paper
to: "Michael E. Mann" <mann@virginia.edu>

Mike,
Done.
Phil
At 19:14 12/03/2004 -0500, you wrote:

HI Phil,
Can you forward Susan Solomon a copy of our latest verison of the ROG paper? She's very interested in this, in part I'm sure because of IPCC. She knows not to distribute. I told her we'd keep her posted on the status of this. Meanwhile, my GRL paper on smoothing should be going to press shortly--will keep you posted on that.
have a good weekend,
mike

#1593 (We are the consensus)

date: Thu, 25 May 2006 12:43:13 -0400
from: "Michael E. Mann" <mann@meteo.psu.edu>
subject: Re: expert review comments on AR4
to: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
thanks a bunch Keith,

yes, lets definitely discuss in Switzerland. **Perhaps you, Tim, Phil, and I (maybe more, but I think this would be just right) could get together over a few beers** and really have an honest open discussion of where we can sort out the real issues (of which there are many) from the specious ones (of which there are also many!). Especially, see the latest RealClimate article by David Ritson, its also relevant to the discussion even though it is of course not true peer-reviewed literature:
http://www.realclimate.org/index.php/archives/2006/05/how-red-are-my-proxies/

My guess is that anything that the 4 of us all can find consensus on, is probably a good reflection of what the consensus is within the leaders in this field, and you could certaintly use that as ammunition in your deliberations with Peck and Susan...

see you soon,

mike

Keith Briffa wrote:

> Hi Mike
> thanks for these comments and especially thanks for your remarks on
> the effort of trying to produce a balanced picture of the current
> state of things in the IPCC Chapter 6. In fact , I know that it is
> already out of date and I am going to get particularly lambasted for
> not discussing problems with recent tree responses to warming and
> potential problems wit CO2 fertilization - I may have to add even more
> text yet .You are absolutely correct that we had unreasonable trouble
> from Susan , who was not as "hands off" as she might have been. I will
> certainly study your comments carefully - as I always do . I would
> rather reserve comment on the Crowley reconstruction til I speak
> personally to you. I really hope that we can get an atmosphere of
> constructive discussion that , I believe, must include some discussion
> of the sceptics . Look forward to those drinks and some time away from
> the mad house of teaching/exam marking etc. See you soon
>
> best wishes

> Keith

#160 (The next two emails are part of an extensive conversation between Keith Briffa and Eugene Wahl in late 2006. Briffa turned to Eugene Wahl for text in the section of Chapter 6 that summarized the hockey stick, even though Wahl was not on the author list and was clearly a partisan on Mann's behalf, as well as on behalf of the House Democrats)

From: Keith Briffa [mailto:k.briffa@uea.ac.uk]
Sent: Mon 7/24/2006 3:16 PM
To: Wahl, Eugene R
Subject: RE: confidential

Gene
here is where I am up to now with my responses (still a load to do) - you can see that I have "borrowed (stolen)" from 2 of your responses in a significant degree - please assure me that this OK (and will not later be obvious) hopefully.
You will get the whole text(confidentially again) soon. You could also see that I hope to be fair to Mike - but he can be a little unbalanced in his remarks sometime - and I have had to disagree with his interpretations of some issues also.

Please do not pass these on to anyone at all.
Keith

Will pass all comments to you before they are fixed in stone- nothing from review article will be mentioned.
Really grateful to you - thanks
Keith

#2829
date: Fri Jul 21 19:00:20 2006
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: RE: confidential
to: "Wahl, Eugene R" <wahle@alfred.edu>

Gene
your comments have been really useful and reassuring that I am not doing MM a disservice. I will use some sections of your text in my comments that will be eventually archived so hope this is ok with you. I will keep the section in the chapter very brief - but will cite all the papers to avoid claims of bias. I really would like to discuss the whole issue of the reconstruction differences at a later , less stressful time. I completely accept the arguments about the limitation in the r2 and the value of capturing longer-term variance .
I think I will have to stop now as the temp and humidity are killing here.
Thanks a lot again
Keith
At 18:39 21/07/2006, you wrote:

Hi Keith:

I'm sorry that there is a bit to digest...although I know it is just a result of the nature of things.

By the way, copied below is a synopsis that I sent this morning to a person in DC who is working on all this with regard to the House of Representative hearings. Evidently, there is to be at least one more hearing next week, and Mike Mann will go. The person I sent this to is trying to understand the importance of the proxy PC issues --especially how, no matter what way the PC extraction is done, the reconstructions converge if the structures actually present in the data are not tossed out by truncating the number retained PCs at a too low level. What I've copied is this synopsis. I think it is straightforward -- maybe a bit dense, but at least brief….

4. Tribalism in IPCC procedures

4.1 Chapter 3 author selection

Trenberth and Jones, as Lead Authors, take great care to find the "right" contributing authors and writers for the SPM.

#649 (May 10 2004) (Trenberth in blue, Jones otherwise. Jones cannot think of anyone else qualified to write on the temperature record)

```
Phil Jones wrote:

>
>  Kevin,
>     Your language seemed clear enough for me. Hopefully Susan's
> generalizations won't
>  lose any clarity. I had a look at all the documentation over the
> weekend. CVs of our LAs
>  would be very useful. Although Susan went through them with me over
> the phone, I've
>  already forgotten some of their areas of expertise.
>     Looking at the chapter outline, we can go one of two ways
>
>  - assign the LAs and ourselves to the sections and subsections
>  - assign just you or me to one of sections 3.1 to 3.10 (with the 10th
> the Appendix).
>
>   Suspect the CVs will tell us that we can only rely on 2-3 of the
> LAs, so the second option
>  will likely involve less need for rewriting - but potentially more
> for us in the first instance. I
>  reckon we could only rely on Dave Easterling to do a good job with
> 3.8 on extremes - maybe
>  also the Canadian (Soden?).

I favor less for us in first instance.  We will have our hands full
filling in and bridging gaps.
Brian Soden is from GFDL and is currently at U Miami.  He is the only
person who has solid credentials on satellite data.  I expect we should
assign him to take the lead on evaluating all the satellite stuff:
clouds, radiation budget, UTH, etc etc.

We may want to create a 3D matrix of variables (T, q, u,v, cloud,
precip,  etc) and region (sfc, trop, strat, in situ, satellite,
tropics,  extratropics, polar) and phenomena (monsoons, storms,
hurricanes, extremes, teleconnections (ENSO, NAO, NAM, SAM, etc)).  I am
also attaching part of a talk I gave to a meeting of oceanographers on
the next IPCC:  note the emphasis in phenomenology.

>
>   Thinking aloud now, I could be responsible for 3.2 and 3.5 and you
> for 3.3, 3.6 and 3.7
>  with 3.4 being relatively short. Length seems unknown at the moment,
> but that would help
>  determining how large/small the different sections will be. Appendix
> will either be easy
>  or a minefield. The HC are giving me a little bit of money to work
> with them putting errors
>  on the gridded temp data - not just the hemispheric curves. We can
> refer to GCOS
>  documents to show what should happen and then say it doesn't ! This
> could be an
>  opportunity?
>
>    I'm off after May 13 for much of the rest of May, then here for
```

> most of June and July (but
> I will have 2 weeks off when I move house). It seems that the GCOS
> meeting in August
> is our best option to meet. Is this still possible for you?

I have told them I will come to the GCOS meeting in the expectation that we can get together there.

I think the appendix will be a major difficulty. Since we are supposed to deal with what is in the literature it means unevenness. I think some of this has to be invented by us. How to do this???

…

Cheers
Kevin

date: Mon May 10 17:20:05 2004
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: [Fwd: Re: Procedures for LAs]
to: Kevin Trenberth <trenbert@cgd.ucar.edu>

Kevin,
My email is OK. It is likely the UEA system which is bouncing things. There were problems last week. Try sending in a few hours when everyone has gone home here.
70pp should be fine ! Didn't think it would be this large.
Agree we need to give them all assignments, but we can't till we know what they are like as you say. I think WGI are still contacting the LAs. Keith Briffa got an email here on the paleo chapter, but only today ! We can't do much till we here from the TSU of WGI.
They are
all in Ireland this week. We also need to know who will be involved in other chapters !
Tom Peterson is with Chapter 1, for example.

Assigning all the writing is one way to go, but we will have to do a little ourselves. **I suspect**
there is little point in giving the temp stuff to anyone else or the MSU/surf area to one of our LAs. Pairing them off with a native speaker in each pair (where possible) is a possibility. All must come to Trieste, but we will need a more detailed outline of the sections
to use as a strawman then - which we should be able to come up with in Geneva and email out for all to think about a few weeks before.
I hope your email gets through later.
Cheers
Phil

#1359 (Christy – we're not saddled with him as lead author; Benestad [realclimate coauthor] is good because he's anti-Svensmark)

At 18:23 10/06/2004 -0600, you wrote:

Hi Phil
I was out today away from NCAR so this is from home. Some quick reactions to your comments and extra notes of my own problems at the end. I see we finally have contact with Brian Soden via a new email address I found.
On Thu, 10 Jun 2004, Phil Jones wrote:
>
>   Kevin,
>   Hopefully Brian Soden is just away for a 2 week vacation. If not this
> doesn't augur well.
>   Sarah Raper told me that Ambenje was at Maynooth last month for the IPCC
> meeting there.
>
>   **The additional list sent from WG1 contains a FEW useful names.** There
> are some who
>   are CLAs/LAs on other chapters (Forster, Hegerl, Hewitson, Karoly, Lean,
> Nicholls, Peterson
>   and Villalba). Of those only Neville Nicholls, Bruce Hewitson and Tom

> Peterson would likely
>   have been useful.
>
>   **As for the others, there are loads I've never heard of. Useful ones**
> might be Baldwin,
>   Benestad (written on the solar/cloud issue - on the right side, i.e
> anti-Svensmark),
>   Bohm, Brown, Christy (will be have to involve him ?), Dai (good),
> Fraedrich (circulation),
I believe John [Christy] is keen and in talking with Susan **I thought we were going to be saddled with him as a LA. He does contribute. So it may be prudent to ask him to write something.**
>   Frei (good, for extremes), Fyfe (circulation), Gallo, Groisman,
> Hanssen-Bauer (OK),
>   Hurrell (good), King (good for Antarctic), Kodera, Kunkel (has written
> stuff, but never met him),
>   McBride (OK, but will he do anything in our timeframe), Nobre (simialr to
> McBride),
Is that Carlos Nobre: he is on my paper for the CLIVAR conference and I have had great trouble getting him to respond to queries. He has not contributed usefully.
>   Power (good), Rayner (good but will mostly be in the oceans chapter),
> Salinger (knows the
>   ropes), Seidel (good), Stephenson (just about OK), Vose (probably) and
> Zhang. There are
>   a few others (e.g. Ramaswamy, but they seem more appropriate elsewhere in
> AR4).
>
>   WRT to Aiguo and Jim with you, there are three people here in CRU (Tim
> Osborn, Clare Goodess
>   and Malcolm Haylock) who could also be useful. Malcolm is putting
> together the European
>   part of the background on extremes that Dave Easterling mentioned. Tim
> Carter is
>   co-ordinating this for WGII. Should be available for end of June. Malcolm
> is collecting
>   references with a bit of text. I have the S. American part. I'll email
> this when have all
>   continents - should be useful for Trieste.
>
>   There are a number of S. Americans for Matilde to use. Will likely need
> to try to use
>   Bruce Hewitson to make sure we have decent Africans. Can discuss with
> Bruce in Trieste.
There is work going on in Africa under the CLIVAR project and that includes AMMA. The latter is not funded in the US. I know an atlas is being
> prepared. Hopefully there is some useful stuff for us.
>   The extremes workshop attached to the IMSC went well, so he should know
> whether
>   we have any good ones.
>
>   **There are a few I would want nothing to do with - Gerstengarbe,**
> Michaels, Schoenwiese.
>   Also like to avoid Grassl and Gruza and probably Ogallo.
The latter may be useful for Africa?
>
>   Just a few quick thoughts on the list. Move is still set for June 25 -
> will be away from June 23
>   for about 2 weeks. Will occasionally come in a couple of times a week for
> an hour or
>   two.
>
>   Cheers
>   Phil
>

#714
date: Wed Sep 15 16:18:03 2004

from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Chap 3
to: trenbert@ucar.edu

Kevin,
This will be my last email this week. I'll check again on Saturday, then maybe next week,
but definitely Thurs/Fri next week.
Glad to hear that Jim is recovering well. Give me my best if you see him. Also good on the jury duty !
The temperature trends would seem to be best (to me at least) as the knowns/unknowns/ unknowable will enable the others to see the thinking and how we have to get across uncertainties. Need to keep us all focussed.
I'd let Susan go through the scoping material and we/you can pick up on the CAs etc.
I haven't prepared anything by the way. I have a number of talks on the lap top, but not
as appropriate as yours.
Getting people we know and trust is vital - hence my comment about the tornadoes group.
I still favour Steve Warren on clouds, but there are a whole range of aspects to consider there.
Merging wind and waves is fine with me - should be a small section anyway.
I hope to get a few people involved within CRU (Tim Osborn, Malcolm Haylock), but some are already involved in other chapters and being courted by WGII.
Have another (!) good week in Geneva !. I should arrive in Trieste on the Sunday morning.
Staying with 2 others from CRU in Venice on the Saturday night.
Cheers
Phil
At 15:37 15/09/2004, you wrote:

Hi Phil
I was supposed to be on jury duty this week but I was excused after 5 hours, thank goodness. Next week I am in Geneva at WCRP mtg and I go from there to Trieste. So this week is all I have to prepare further.
I have several possible presentations to consider. One was the one I sent you earlier based on the scoping material, although Susan may cover a lot of that. One is a paper I gave last week where I was tasked to review temperature trends: the known, the unknown and the unknowable.
The question is whether these might help get us on the same wavelength or highlight disparate views. To the extent we can get on the same page, so much the better.
I also have some draft material, adpated from last time, on letters to CAs recruiting them to do the task required (to be sent by email). Question, should the LA send these or us? It may carry more weight if we send them.
It may also give us more control.
Some thoughts follow.
On Wed, 15 Sep 2004, Phil Jones wrote:
\>
\> Kevin,
\> Here's a few thoughts. Not added any of this to the annotated outline.
\>
\> 3.2 and 3.3. We should talk to Dave Easterling and Tom Peterson about who
\> additionally
\> might be involved from NCDC. I'm working with Russ Vose on a comparison
\> of the NCDC/CRU/GISS
\> land temperature datasets. Nor sure how many more in Asheville can get
\> involved.
Tom is on another chapter I believe. Dave is one of our LAs and so I think will bring a lot of NCDC along with him, just as you will UEA and me NCAR.
I hope.
\>
\> I'll talk to Mike Hulme here, but he's changed his research areas a lot
\> in the last few years,
\> so is much more WGII now.
\>
\> I'm at an Antarctic meeting tomorrow and Friday in Cambridge, so can ask
\> John Turner or a
\> colleague there. Might be useful re 3.6.5 or 3.9 but in all sections we
\> are trying to get the
\> large-scale picture, so bits on different continents are useful, but will
\> need a lot of integration.

> There will be a paper on the latest Antarctic temp trends, so this can be
> referred to in 3.2.
>
> I'm involved in an EU project on the greater Alpine region. This has
> extensive analyses of many
> variables from the best observed mountaineous region. There are precip
> and temperature datasets
> going back to 1800. Also the Austrian group (Boehm, Auer at the NMS) have
> a paper on
> temp changes inferred from pressures at high and low elev sites in the
> region. It confirms the
> surface warming - could be a box in 3.2?
>
> I've emailed Adrian Simmons on another issue and asked him how much he
> would like to get
> involved. Need to add Peter Thorne to 3.4.1.6. Co-ordination with the
> various US efforts essential.
> Tom Wigley tells me he's heavily involved in one of these.
>
> Clouds in 3.4.3 are a problem.
I have done a very preliminary review of literature on clouds. I can send to you if you like? Liepert might be better there. **Rossow also? But I don't trust him. Norris has done a lot but I don't trust him either.**
>
> For 3.5.3 I'm aware of an EU project which tried to look at this from
> pressure triangles. This
> could more likely go in 3.8.1. There is a lot of work on winds, to do
> with citing wind turbines,
> but hardly any of it looks at longer timescale changes (and certainly not
> on large scales).
I think we should probably merge 3.5.3 and 3.5.4 winds and waves.
>
> For 3.6 is Jim recovering? He will be very useful, if he can give us some
> time. We will need
> something in 3.6.6, even if small - Dave Gutzler. Noting a continuing
> problem - all my names
> are US, UK, OZ/NZ or western European.
>
Jim is now fine. He was knocked down for a few days. He had a seizure and is not allowed to drive for 3 months. After that he may be allowed. That is a major handicap as it takes him 1.5 hrs to commute each way by bus. In evening he often gets a ride (he lives in Denver). But he is acting CGD Director and has little free time.
Yes I am aware of the mostly western names.
> For 3.7, Neville Nicholls will be in Trieste for another chapter, He
> could suggest someone for
> Australia (Wasyl Drozdowsky springs to mind, but there will be others).
>
> For 3.8, there is the storm tracking work of Ian Simmonds. There is also
> some European
> work. Do we believe trends those based on Reanalyses? There is lots of
> much older work from
> Klein from the 1960s/1970s?
We have had a project on this at NCAR for some time. We have a lot of the data, based on band passed stats. Needs to get written up. A fellow called Edmund Chang (SUNY Stony Brook) has done some really good work on storm tracks and spurious trends in NCEP.
>
> For 3.9 add Lisa Alexander for 3.9.2.1.
> Aiguo's work on PDSI may be useful for 3.9.3.3.
> For 3.9.3.4 there is Nikolai Dotzek who said he could do something on
> tornadoes and
> severe local weather events. No idea how good he is. Their web site
> ([1]http://www.tordach.org)
> has some links to work in Germanic countries. Tordach has a US page as
> well. Ever heard
> of this? Another person here is Rudolf Brazdil (Czech Rep.)
>

#1726 (Trenberth to Jones: no one has strong enough views to let them write the SPM)

At 22:57 18/04/2005, you wrote:

> Hi Phil
> I just talked with Susan Solomon about the forthcoming IPCC meeting.
> Below somewhat confidential.
> Seems like they will go ahead and we will be up on the last day to give our views on the AR4 as a whole. This means a bit of homework to say how our chapter relates to others and whether or not we are at odds.
> Will our obs changes be related to those from models? might be one question. I have not looked at any other chapters ZOD. Any ideas?
> Seems like I will be co-chairing the inter-chapter group on obs (oceans, cryosphere, paleo). So we will need to also prepare for that: how we integrate with snow and glacier melt, sea level rise, sub-surface T changes, overlaps between paleo and instrumental record, etc. Care to add to the list?
> Other big questions we have to contribute to include
> 1) **The SPM and Technical summary. Who from our chapter can do this? It needs someone who is broad and knows the whole chapter, and probably is NOT you or me as we have too much else to do that overlaps. Frankly I am not sure I can truly recommend anyone. So a possible option is to use a review editor (Susan suggested Brian Hoskins). Here is my quick take:**
> **David P contributes, but does not have strong views and does not speak up enough.**
> **Brian S. has the knowledge but has been disappointing in failure to interact and contribute outside of tasks assigned.**
> **Jim R. might be possible but not very strong**
> **David E. has not done much or anything outside of 3.3.**
> **Klein Tank: might be possible, but I have been disappointed thus far.**
> **Has not grasped everything, too many things in 3.8 at odds with elsewhere.**
> **Roxana, Matilde, Peter, Fatema, Pan Mao would be out of their depth.**
> 2) The synthesis report. This is another troublesome item that will go on in parallel with AR4 report and makes it tough to do both. Need broad people and ones who will speak up and take issue with the WG2 and WG3 people who have political agendas that go beyond the science.
> Requires a commitment. You or me might be possible but will we have time?
> So 4 things to think about.
> Kevin
> --

4.2 Hurricanes: The Landsea episode, J&T then struggle to find right replacement

Summary:
Month/Date/Email #/From/To/Contents

10	21	890	Landsea	Trenberth	Please don't overstate matters on hurricanes, there is no science to back up press conference headline. Mears pulled out, Trenberth went ahead
10	28	1219	Trenberth	Jones	I did a presser to oppose Landsea. Japan has had some bad storms, maybe we can try to find a Jap who'll say what we want.
11	1	1219	Jones	Nicholls	Can you find someone? Diff't perspective from Landsea
11	2	2815	Jones	Kondo	We're looking for someone to write about hurricanes. Who writes about changing numbers, and often talks to media?
11	2	2815	Jones	Kondo	More detail: views expressed by US scientists in Miami but we're looking for something other
11	3	1219	Nicholls	Jones	My suggestion is Chan, but he might end up agreeing with Landsea
11	3	1219	Jones	Nicholls	Well Kevin doesn't think much of him.
11	4	3967	Jones	Trenberth	Neville suggested Chan , but he'll just say the same thing as Landsea
11	5	890	Landsea	Tank	Trenberth has poisoned the well on this, IPCC can't be objective
11	8	890	Tank	Jones	We've got a problem, Landsea wants assurance of objectivity
11	8	890	Jones	Tank	KT told me ahead of time about presser. Best to go ahead without Landsea. We're trying to get a Jap on our side
12	8	1150	Landsea	RKP	Trenberth spoke on behalf of IPCC and said stuff that isn't true
12	9	3946	Trenberth	Manning	I think Landsea should be fired for not considering GW may be affecting hurricanes
12	15	4697	Trenberth	group	Trenberth claims Landsea was fired by Susan Solomon

Following this episode, help arrived in the form of a new study by Emanuel of MIT claiming a link between hurricanes and GW; Jones and Trenberth seized on this and made it the focus of their summary.

Some email extracts:

#1219 (Jones to Neville Nichols. Note that hearing from the "same old" source is not considered a problem in any other section)

```
On 1 Nov 2004 at 13:55, Phil Jones wrote:
>
>   Neville,
>      I said to Kevin I'd contact a couple of contacts in Japan, but do
>      you
> know anyone
>   in Japan or China, who could write this box. I know Chris Landsea
>   could
> do it, but he
>   always does. It would be good to get a different perspective.
>
>   Cheers
>   Phil
>
```

#2815 (Jones to Kondo. Note criterion Jones uses: find someone who often talks to the media)

```
>  Dear Hiroki,
           With Kevin Trenberth, I'm putting together the Atmospheric
 Observations Chapter
   of the next IPCC report (due in 2007).  We are trying to find a Japanese
 scientist (or maybe
   a Chinese one) who could write a small box (say 500-100 words) about
 tropical storms
   (not just for East Asia, but other regions around the world) and whether
 the number is
   changing. I am aware of the high number that have affected Japan this
 year, so I was
   wondering if you know of someone in Japan, who writes on their changing
 number
   and often talks to the media. Any help with a contact name would be most
 appreciated.
   Best Regards
   Phil
 Prof. Phil Jones
```

#170 (Jones, responding to fwd from Trenberth of Emanuel email, referring to new study that suggests a link between AGW and hurricanes (tropical cyclones or TCs). Note that Jones decides if study is worth citing depending on whether it supports position he wants to express. Meanwhile they are dealing with appearance of BAMS paper with large author list denying clear link between TCs and AGW.)

date: Fri Jul 8 11:43:30 2005
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Your hurricane article
to: Kevin Trenberth <trenbert@ucar.edu>

Kevin,
Seems that this potential Nature paper may be worth citing, if it does say that GW is having an effect on TC activity.
Most odd therefore, the arguments here and those of the BAMS paper, with Pielke Jr and Landsea..
I got the attached from Tom Peterson yesterday. Shows that if you adjust adequately you get the same trends as good stations that haven't changed any routines. I presume some of these poorer sites are those that Pielke Sr. has pictures of. He'll no doubt respond at some time to say that people use the

raw data - but clearly one needs to be in possession of all the facts, and not just throw up ones arms and say all is wrong. The US does have good metadata, he will likely say many other countries don't. That is why homogeneity assessments are done. They take a long time, they aren't sexy science and don't get reported in detail.

If we wanted a figure for one of the Appendix on this subject (which we don't) this would be a good one to use. When I'm reading there later I'll see if a ref could go in. Problem is that citing one example, opens us to others showing more plots of raw temperatures. Best probably to talk in general terms.

Cheers
Phil

5. Doubts about IPCC competence

5.1 Personnel

#649

Subject: Re: Procedures for LAs
Date: Mon, 10 May 2004 08:57:04 -0600
From: Kevin Trenberth [1]<trenbert@cgd.ucar.edu>
To: Phil Jones [2]<p.jones@uea.ac.uk>
References: [3]<5.2.1.1.0.20040510114711.04470060@pop.uea.ac.uk>

Hi Phil
The initial guideline for Chapter 3 is 70 pp from an earlier document I have.

The material I wrote was done before I knew who the LAs would be. **I am a bit dismayed that we have 5 from developing countries none of whom I know, although I know a bit about 2 of them. I still do not even have their names straight let alone what expertise they supposedly have. I think we will have to give them all assignments, but I suspect it may be prudent to pair them all with someone from the developed countries or one of us?** As CLAs it is best if we assign all of the chapter contents to the LAs and we hold ourselves mostly in reserve and/or work directly with the LAs on getting them going and making sure they are on track.

The main task in Sept is to make writing assignments and discuss extensively the Contributing authors and agree what they will be asked to do and who will contact them. Examples of my letters from the TAR are attached: the first to Phil Rasch has the outline, the second to Kerry Emanuel is about his expected contribution. Note each letter is tailored. Collectively we need to exploit the community as best we can. To do this best, however, means having the LAs come to Trieste with an idea of their assignment and suggestions for CAs and what they would contribute. It would also help if the TSU gave us info on CAs that were nominated!!!!

#1365 (Houghton disparaging Yuri Izrael; may be because Izrael made some fairly skeptical speeches)

cc: John Ashton <john.ashton@co2.org>, chris.anastasi@british-energy.com, Peter Stott <peter.stott@metoffice.com>, m.hulme@uea.ac.uk, "'Jenkins, Geoff'" <geoff.jenkins@metoffice.com>, michael.grubb@imperial.ac.uk, V.McGregor@uea.ac.uk, "'Cox, Peter'" <peter.cox@metoffice.com>, N.W.Arnell@soton.ac.uk, King MPST <MPST.KING@dti.gsi.gov.uk>
date: Thu, 24 Jun 2004 10:29:59 +0100
from: Sir John Houghton <john.houghton@jri.org.uk>
subject: Re: Yuri Izrael
to: Nick.Grout@dti.gsi.gov.uk

<x-flowed>
NOT TO BE COPIED ON PLEASE

Just a note re Yuri Izrael whom I have known well for 25 years or so both in the WMO and the IPCC. He is a chairman's nightmare in that he is extremely persistent, wants to dominate and even in very large

meetings succeeds in talking for
up to one third of the time (I have calculated the proportion more than once when sitting in meetings!). **He is not generally well informed but he likes to press extremely hard for a few points (he has a very high personal ego)
- sometimes they are relevant sometimes not.** If he fails to get his way he is extremely persistent and repetitive. A common tactic in meetings is to speak so often and so long that time and opportunity for others is severely reduced.

I gather he has cancelled going to an important IPCC meeting in Geneva (he is still a Vice Chair of the IPCC) in order to be present at our meeting in Moscow. For him to cancel a meeting in Geneva implies that he has some clear reason
and a strong personal agenda for the Moscow meeting.

For the IPCC 1990 Report he was the Chair of Working Group 2 on Impacts. So he reckons to know about Impacts and we have gathered so far he intends to lead on Impacts in Moscow.
I suspect much of what he knows about Impacts now may well be based on the 1990 Report! - he has never kept up with the science and doesn't do a lot of homework.

For instance in the 'Izrael' document he says rather little about impacts except to emphasise their great uncertainty (a substantial 1990 emphasis) and to mention CO2 and bioproductivity (also in 1990 report) and other positive impacts
especially for Russia. I suspect he will home in on these points about great uncertainty and positive impacts in his presentation to us.

May I suggest that Mike and Nigel might look at the 1990 Impacts report to see where Izrael might be coming from and include mention of the large advances in Impacts work over the last decade and how uncertainties have been reduced for
instance in our understanding of CO2 fertilization and its limitations.

Best wishes

John

#5212 (Though note that Jones' view of incompetence of other authors ties to how closely they agree with him on surface data set)

date: Fri Dec 3 09:51:51 2004
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Your IPCC thoughts for UCAR Highlights
to: "Bob Henson" <bhenson@ucar.edu>

Bob,
Reply brief as we're at the stage of trying to get the zeroth-order draft together for Dec 15.
Despite most of my fellow scientists thinking I've been involved in the IPCC process before, this time for AR4 is my first….
As Kevin may have said to you, we have a very mixed bag of LAs in our chapter. Being the basic atmos obs. one, we've picked up a number of people from developing countries so IPCC can claim good geographic representation. This has made our task harder as CLAs as we are working with about 50% good people who can write reasonable assessments and 50% who probably can't. Getting them all involved has been a challenge, and we've not really succeeded.
Our LAs are unlikely to cause us much of a problem. Problems will start when the first order draft (after our next meeting in May) goes our for review by all and sundry (any scientists anywhere) - sometime in the late summer. This is when the skeptics and scientists who'll think we've misrepresented or ignored their views get a chance to tell us. We have to respond to all. We have an excellent group of Review Editors to help us here - when we meet in NZ in Dec 05.
I expect that will be an interesting meeting. Getting our less good LAs involved will be an issue. Susan Solomon is keen for

them to be involved, but many lack global perspectives and a sense of what the big issues are.
Issues are (and could have been predicted before we started) :
1. How much has the world warmed? Errors attached to all observations.
Issues of representativeness - why urbanizaton and land-use changes are not that important. A small group of skeptics will likely have a go at us on this. I hope we have all our bases covered.
2. **Surface warming yet lower troposphere not warming as much.** A US report (CCSP) Kevin can tell about will help here, but our likely conclusion that this issue is resolved will likely come in for lots of flak. **Explaining why we think we're right will be the biggest issue - making sense of diverse datasets and saying why we think some are right and some have problems.**
3. Extremes. There is a lot more information out there this time from initiatives made by earlier assessments. Bringing all this together is the challenge here, and saying defensible statements.
4. We have a chance this time to go into a lot more detail about indices (ENSO,NAO etc) and their roles.
We will be endorsing GCOS initiatives to improve the network and saying that reanalyses in the future have to really consider issues of changing data inputs. They can do this by running periods with/without specific datasets to see effects. Getting people to think this way is coming, but resources are an issue. Computers getting faster,but we must use this to address the above issues,rather than using the additional speed to improve resolution. We can do both but we need good planning and it will all take time.
If you want to clarify anything then email me again.
We are 7 hours ahead and I tend to work 8 till 4.30 which makes catching me difficult.You can call me at home say 7-9pm (UK, so noon-2pm yours) most nights (except Tuesday and Fridays). Home phone is +44 1953 605643 .

Cheers
Phil

5.2 Oldenborgh comments

In January 2006, a couple of weeks before the AR4 SPM was due to be published, Geert Jan Oldenborgh sent an extensive set of criticisms about the graphical material in the draft. We have some of the subsequent responses from Jones and the IPCC, as well as Oldenborgh's rejoinders, but do not know how it was resolved.

1	19	1276	Oldenborgh	Klein Tank	I can't replicate many of the trends etc. in the chapter, there is misleading labeling in NA, SA, Europe, etc; Sahel cherrypicked…
1	19	3535	Jones	Oldenborgh	You're confused b/c you don't have figures from the Chapters, by the way they're confidential; Old: but you are misleading readers
1	19	3535	Oldenborgh	Jones	Other data set has decreasing trend yet SPM claims certainty on increasing trend
1	24	1358	Jones	Oldenborgh	Jones: these are the model aggregations; Old: but readers don't have that info, the result is misleading

#1276

From: Oldenborgh van, Geert Jan
Sent: Friday, January 19, 2007 12:05 PM
To: David.R.Easterling@noaa.gov
Cc: Klein Tank, Albert; Ulden van, Aad
Subject: Precipitation trends statement IPCC 4AR SPM

Dear David Easterling,

as climate researcher at the Royal Dutch Meteorological Institute (KNMI) I had been asked to contribute to the government review of the SPM. One of my points, which made its way into the Dutch review, was that I could not find back many of the trends in precipitation stated in the SPM. I checked all of these in a few datasets that I have available on the KNMI Climate Explorer (http://climexp.knmi.nl). Based on my view of the data, only two of the nine trends mentioned are clearly visible and significant in the observations, and these are slightly mislabelled. Two other highly significant trends are not mentioned. Could you comment why the SPM is so different from my trend maps? I have attached

a very rough analysis for internal use, with lots of figures. The main

points are listed briefly below, in the order of the SPM final draft (SPM-6 line 5-10).

1) The eastern North America trend seems weak, confined to a small area in Canada, so labelling it "eastern North America" is misleading.

2) The South American trends are poorly specified; if the trends in Argentina are meant, why use the phrase "eastern"? It is also absent in the GPCC datasets.

3) The trend in Northern Europe is in the winter only, this should be mentioned.

4) The North Asian trend is not a trend but a discontinuity in 1940, which looks suspiciously like a change in the observing system.

5) I see no significant trends in Central Asia except for 3 stations in the far west of China.

6) The trend in the Sahel is only significant when you start late and finish early; rainfall has increased substantially again since 1995. Given the large decadal variability in the first half of the century, and the attribution to aerosols of the drought in the 1970s and 1980s, I would hesitate to call the remaining trend "significant". Also, it is only the western Sahel that has a trend, not the eastern Sahel.

7) In the Mediteranean there is only a significant trend in North Africa, there is no significant trend on the northern shores. Labelling it "Mediterranean" is therefore misleading.

8) I see a drying trend in southern Africa only in the Zambia, I do not know the quality of the data there. Averaged over all of southern Africa as implied in the text there is no trend.

9) Parts of southern Asia. Which parts?

Two trends that are not included, but highly significant in all datasets are an increase in precipitation in western Australia, and a decrease in western coastal Africa, see the maps in the attachment.

Could you shed some light on this discrepancy arises, and what can be done to close the gap?

Greetings from calm & sunny Holland (after a big storm),

Geert Jan van Oldenborgh

#3535 (Oldenborgh replies to Jones' response)

cc: Aad van Ulden <uldenvan@knmi.nl>, "Klein Tank, Albert" <Albert.Klein.Tank@knmi.nl>
date: Fri, 19 Jan 2007 18:34:41 +0100
from: Geert Jan van Oldenborgh <oldenborgh@knmi.nl>
subject: Re: Precipitation trends statement IPCC 4AR SPM
to: Phil Jones <p.jones@uea.ac.uk>, David.Easterling@noaa.gov

<x-flowed>
Dear Phil, David,

thank you you for the figure, which clarifies a lot.

Phil Jones wrote:
> If you've just seen the SPM, then you will not know about a figure

> within Chapter 3. These are figures 3.14 and 3.15. I'm not supposed to
> send these out, so you got them from Albert. Don't pass on to
> anyone else.
>
>   So the SPM bullet points are based on these. There are also trend
> maps by seasons for 1979-2005 and the year for 1901-2005 and 1979-2005,
> and global land series time series for 1901-2005 from various databases
> - many more than just GHCN and CRU.

However, the reader of the SPM will not know these maps either, and assume something else from the names than you mean. Also you do not follow this consistently: "Western Africa" with a clear trend is arbitrarily replaced by the Sahel, with only a trend in the western half, and "Southern South America" is replaced by "eastern SOuth America".

>   We chose the regions in the chapter to show precip differently from
> how it
> had been done in previous IPCC reports. The regions were defined in a
> paper
> by Giorgi and someone else (from about 2001/2002) that is used in Ch 11
> (Table 11.1).

Why are the regions not defined based on the signal? This way one groups together regions with and without trends (e.d., in the Mediterranean, with no significant trends on the European side).

>   The regions are large, take no account of rainy seasons or rainfall
> regimes,
> so they have very little climatological content. They use a lot of the
> gridded
> data though and there are some surprising similarities and dissimilarities
> between them. We chose annual, as we only had space for one Figure.

This seems like a wise choice, especially since some of the observed changes are shifts in the rain seasons, as in the southern African rain season moving backwards. A fixed window like JJA would show a decrease when in fact there is none.

>   Now the important point - the SPM. If you've read the SPM you'll have
> noticed that hardly any country is mentioned. This is deliberate and we
> refer to large regions. This is because we would likely not get the
> text past
> the govts in Paris the week after next if we were that specific.

I agree that this makes sense, however, I disagree with choosing the regions first and making statements as if the observed trend applies to the whole region, rather than parts of it. The reader of only the SPM will conclude that rainfall has decreased everywhere in the Mediterranean, when in fact it has not in half; same with Central Asia, Eastern North America, etc.

Coming back to some individual regions mentioned in the SPM I still do not understand most of the claims made in the SPM statement.

1) Eastern North America: your figure shows as well as my maps that a significant increase is only seen in the easternmost provinces of Canada. This should not be labelled "Eastern North America"; there is no trend in New York City and Washingtonn D.C. to name a few populous and politically important places, whereas to the reader this is implied. If this small a region cannot be mentioned it should be left out.

2) Eastern South America is not even defined in your figure. Southern South America is, with a clear trend in Fig 3.14 (which is much weaker in the GPCC data), but this is not included in the SPM.

3) There is a clear trend in northern Europe, but as we all know that it is only in winter and the summer has in fact an opposite trend, would it be possible to add the word "winter"?

4) The North Asian trend. Looking at the data from individual GHCN stations, almost all of them have lots of missing data around 1940, when

the averaged series shows a big jump. What is the evidence that this is not caused by chances in the observing system? I find step function always quite suspect. The VasclimO dataset, which the authors claim has better homogenization, has a decreasing trend for the period 1951-2000! This does not seem the kind of certainty that warrants inclusion in the SPM.

5) I still see no significant trends in Central Asia except for 3 stations in the far west of China and in Russia (see plot). Do you want to make a sweeping statement "Central Asia is getting wetter" based on these three station series? Wulomoi shows 1.5 decadal cycle that imitates a trend, Dulan and Irtyssk have barely significant trends (p=0.04). There are many other stations with no trends.

6) I do not see an area labelled Sahel on your Figure in Chapter 3. Why is it then included here?

The trend in the Sahel is only significant when you start late and finish early; rainfall has increased substantially again since 1995. Given the large decadal variability in the first half of the century, and the attribution to aerosols of the drought in the 1970s and 1980s, I would hesitate to call the remaining trend "significant". Also, it is only the western Sahel that has a trend, not the eastern Sahel.

7) In the Mediteranean there is only a significant trend in North Africa, there is no significant trend on the northern shores. The trend in the time series of Fig 3.14 is not very convincing by eye, it is much better if you take only the southern half, i.e., North Africa. Claiming the "Mediterranean" is receiving less rainfall as a whole is again misleading.

8) From your plot (and mine on www.knmi.nl/adrica_cenarios) I see very strong decadal variability in southern Africa, and no significant trend. We could just happen to have had a downward cycle near the end. What value for the autocorrelation was used to determine the significance of the trend? The judgement by eye agrees with the map, which does not show strong brown colours either.

9) From your map, this concerns Butan/Assam only; the rest of the subcontinent is getting wetter. I see why the restriction on naming countries causes problems here... In the GHCN dataset I find only one station with >70 years of data there with a significant downward trend, Darjeeling, and only a half dozen with >50 years between many more stations with no trend. Again, you are basing a very important statement on very little actual data, and this statement will doubtless be interpreted to mean that large parts of teh subcontinent are drying. I think it would be better to leave it out.

To my great surprise "Western Africa" is included in Fig. 3.14, with a steep decline, but this is not mentioned in the SPM!

Western Australia also shows up very clearly in the colours, but is ignored. Why? Because Giorgi used northern and southern Australia?

So, in spite of the background information I still do not understand how this statement follows from the observations.

Greetings,
Geert Jan

#1358

cc: "Klein Tank, Albert" <Albert.Klein.Tank@knmi.nl>
date: Wed, 24 Jan 2007 17:58:24 +0100
from: Geert Jan van Oldenborgh <oldenborgh@knmi.nl>
subject: Re: Precipitation trends statement IPCC 4AR SPM
to: P.Jones@uea.ac.uk

<x-flowed>

Dear Phil,

P.Jones@uea.ac.uk wrote:
> Geert Jan,
> The bullet points come from Fig 3.14 not
> from the maps. The time series show large area
> averages. This is what the models give.

I thought the models gave gridded output (Fig SPM-6), and we are discussing observations here, not model output.

The boxes are not defined for someone just reading the SPM, without access to the full report that will be released much later. A reader will interpret the statement that the Mediterranean became drier as meaning that all of the Mediterranean area became drier. If you see on the map (not available to the reader of the SPM) that it only pertains to the southern half, why not make the more accurate statement that Northern Africa became drier?

> To get every grid box in one of the regions
> to all show the same sign of a trend is impossible.
> We used the large regions to show the bigger picture
> as I said earlier.

I am not complaining about not all grid boxes having the same sign. I am complaining about the whole statement based one or two grid boxes in a large area, with the reader who is not yet immersed in Chapter 3 interpreting this as a trend in the whole area, when in fact this is not the case. Especially when the grid boxes have a much higher climatology this can easily happen.

I am also curious why you left out the big trends in southern South America (shown explicitly in Fig 3.14) and western Australia. Is this because they are not in the model results?

> Talk to Albert!!!!!!!!!!!!

He told me to contact you.

Geert Jan

6. IPCC pursuing foregone conclusions

6.1 Houghton on the hockey stick

#5024 (Houghton tells reporter that over-emphasis on hockey stick in TAR could not have happened because of review process)

From: Sir John Houghton [mailto:john.houghton@jri.org.uk]
Sent: 04 February 2005 11:47
To: Regalado, Antonio
Cc: Chris Folland
Subject: Re: Hockey stick.

Dear Antonio Regalado

Thank you for your email. I am copying this to Chris Folland as he was a convening lead author of the chapter to which you refer and will be able to reply to the queries your skeptic has raised much better than I can.

The only points I would make are

(1) the discussions around the Mann and other diagrams were entirely scientific in nature; the Mann diagram was the one that was included in the Summary because we believed it to be the best data available at the time. I remember a significant

entirely scientific debate regarding its quality at the time.

There was no inappropriate bias or 'conspiracy' attached to its inclusion - indeed no such bias would have survived the thorough and open IPCC refereeing procedures.

(2) whether or not the MWR was warmer than colder in global average terms than 1998 would make no difference to the IPCC's 2001 report's conclusions

about the 20th century record and the contribution of greenhouse gases.

With best regards

John Houghton

#1104 (Yet they did receive warnings about over-reliance on it, and Lead Authors like Mann simply dismissed them)

date: Tue, 08 Mar 2005 10:41:54 +0100
from: Heinz Wanner <wanner@giub.unibe.ch>
subject: Hockeystick
to: k.briffa@uea.ac.uk

Dear Keith,

I am quite amused about the fact that everybody wants to express his/her concern about the hocheystick story. It is in a certain sense ridiculous, on the other hand we have to define our position.

I had to give several interviews (TV, radio, newspapers) but tried just to explain science. Now an old story is warmed up. I was a reviewer of the IPCC-TAR report 2001. In my review which I can not find again in its precise wording I critcized the fact that the whole Mann hockeytick is being printed in its full length in the IPCC-TAR report. In 1999 I made the following comments:

1. The spatial, temporal (tree-ring data in the midlatitudes mainly contain "summer information") and spectral coverage and behaviour of the data is questionable, mainly before 1500-1600 AD.
2. It is in my opinion not appropriate already to make statements for the southern hemisphere and for the period prior to 1500 AD.

My review was classified "unsignificant" even I inquired several times. Now the internationally well known newspaper SPIEGEL got the information about these early statements because I expressed my opinion in several talks, mainly in Germany, in 2002 and 2003. I just refused to give an exclusive interview to SPIEGEL because I will not cause damage for climate science. I just told a woman from SPIEGEL that I do carefully follow the activities and the forthcoming of the next IPCC report and I will then take position concerning the paleoclimate chapter there. I thought it is meaningful to infomr you about this fact.

Cheers, Heinz

6.2 Looking to draw conclusions even if data not available

#1925 (We need a clear statement about the SH even if we don't have the data: contrast to #2600 a month later)

cc: Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
date: Fri, 24 Jun 2005 11:52:25 -0600
from: Jonathan Overpeck <jto@u.arizona.edu>
subject: Re: First draft of FOD
to: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi gang - I still have to weigh in on the great figs/text that Keith and Tim have created, but here's some feedback in the meantime.

I agree that a mean recon isn't the thing to do. Let me think more before I weigh in more on the fig. Working to get other LAs to get their stuff in.

As for the Southern Hem temperature change fig (and caption and a little text), I agree that you (Ricardo in the lead) should do it as you've proposed. **We need a clear S. Hem statement, and although it should stress that the data are too few to create a reliable S Hem recon, we should show the data that are available.** Thus, PLEASE proceed Ricardo on this tack. Also, can we include the borehole recon series from S. Africa and Australia (e.g., Pollack and Huang, 98)? I'm sure Henry Pollack would provide fast - cc Huang too, since he might be even faster. Keith and Tim, does that make sense?

Please note that I think we can find room for the above, regardless, if it is compelling enough.

As for ENSO, we will need to address for sure - based mainly on the more direct coral data rather than teleconnected (e.g., tree-ring) relationships. The latter don't seem to be definitive enough at this time - as I think we discussed in China. The same holds true for NAO/AO/PDO etc., and I think that we (Keith and Tim) will need to have this in their section - in a appropriately short manner. I'll provide more feedback on this soon, so don't sweat it for now.

Main thing is to go ahead on the S Hem temp fig/caption/short text., independent of ENSO etc discussions.

Thanks, Peck

#2600

cc: "Keith R. Briffa" <k.briffa@uea.ac.uk>, <eystein.jansen@geo.uib.no>
date: Tue, 19 Jul 2005 15:35:39 -0300
from: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
subject: Re: the regional section and MWP Figure
to: "Jonathan Overpeck" <jto@u.arizona.edu>, "Edward R. Cook" <drdendro@ldeo.columbia.edu>

Dear Keith and Ed,

Please, find attached the new version of the SH figure for the IPCC. I have now included the New Zealand record. All the records have been scaled to 4 °C amplitude. Variability in the Tas record is reduced compared to New Zealand and Patagonian records. The reference lines is the mean used for the calibration period in each record, 15 C for New Zealand, 14.95 C for Tasmania and 0 C for the Patagonian records (they show departures). Please, let me know if you want to introduce some changes in the figure. **The opposite phase in the Patagonia-New Zealand records is so clear before 1850, which is consistent with our previous TPI. For instance, in the instrumental record the 1971 and 1976 are the coolest summer in northern Patagonian during the past 70 years, but the warmest in New Zealand reconstruction!! This out of phase relationship between regions in the Southern Hemisphere points out to the difficulty of using few records to get a hemispheric average.** Cheers,

Ricardo

#479 (Overpeck: It would be cool to have a figure showing modern uniqueness, but we don't have data… think hard about this)

date: Mon, 27 Jun 2005 22:14:09 -0600
from: Jonathan Overpeck <jto@u.arizona.edu>
subject: the Med Warm Period Box - Peck comments/edits
to: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>
Gentlemen - attached is the ZOD Med Warm Period Box with my edits/comments. I don't see anything sent since then, so hope I'm not editing the wrong thing. In any case, the Box was pretty nice as is, so I only made a few changes. Obviously, some updating w/ new studies is needed. The big issues are two:

1) the recent Wall Street Journal editiorial that is creating all the crap in the US actually showed a time series from the IPCC FAR - if you don't have it, or Eystein can't send, I can scan it in (my Republican Dad sends me these things, although he's an increasingly rare breed of moderate Republican). My thought is that it might we worth adding a couple lines documenting how the view of the MWP changed with each assessment and new knowledge. In doing so, it could be made very clear that there is a reason that scientists don't show those old plots anymore. We need to move the debate beyond the FAR, SAR and TAR on this issue!

2) **it would be cool to have another figure that made the point about no single synchronous period warmer than late 20th century.** This is where I get soft with respect to Tom's plot. If it is published to the extent we need it, and if the composite or large-area average recon is the same as you are showing in your great new Fig 1, then it seems that it would be reasonable to show Tom's fig as part of the Box - just to show the same thing in a different way, and to hammer in one more nail. That said, I'm not sure if my two conditions above are met (I emailed Tom, no response yet - you might have insight), and I believe you just don't like Tom's fig for some - probably good - reason. **But, I wanted us to think extra hard about whether there is SOME fig that might work?**

That's it for tonight. Will finish editing your main text next work session tomorrow I hope.

Best, Peck

#5298 (Five months later, Overpeck still searching)

cc: Eystein Jansen <eystein.jansen@geo.uib.no>
date: Mon Nov 21 11:00:18 2005
from: Keith Briffa <k.briffa@uea.ac.uk>
subject: Re: extra request for Christchurch
to: Jonathan Overpeck <jto@u.arizona.edu>, t.osborn@uea.ac.uk

Think we can do - but both in Switzerland all week , so will look next week
Keith
At 03:42 18/11/2005, Jonathan Overpeck wrote:

Hi Keith and Tim - Susan has put a map showing sites w/ available proxy data for the last 2000 years in the Tech Summ. as a placeholder. We agreed that chap 6 should look into the feasibility of including such a fig in chap 6 and the TS, and **would like to ask if you could produce such a figure (perhaps with some interation w/ us to make sure it's on the mark)** in time for us to use in our chap 6 plenary presentations. Below, see the example pulled by Susan for the TS - **hopefully, we can do better than this?**
Also, is there anything else NEW (since our FOD) and exciting that we might want to share with the entire WG1 team in our Christchurch plenary? For example, a figure illustrating a new compelling reason to have faith in the recons for the last 1000

years? Something related to the M and M controversy?
thanks for helping with this extra request. We are asking several of our LAs to help generate new graphics for Christchruch, and figure in each case that it is work that has to be done sooner or later before the SOD, so we might as well do it before Christchuch and get much more credit and feedback. Thanks again!
Cheers, Peck and Eystein

#2049

cc: "Jurgen Willebrand" <jwillebrand@ifm-geomar.de>, "Peter Lemke" <plemke@awi-bremerhaven.de>, "Phil Jones" <p.jones@uea.ac.uk>, "Brian Hoskins" <b.j.hoskins@reading.ac.uk>, "Martin Manning" <mrcjmanning@comcast.net>, mmanning@al.noaa.gov, "Matilde Rusticucci" <mati@at.fcen.uba.ar>
date: Sun, 7 Jan 2007 15:47:58 -0700 (MST)
from: "Kevin E Trenberth" <trenbert@ucar.edu>
subject: Re: Science presentation for Paris
to: "Susan Solomon" <Susan.Solomon@noaa.gov>

Susan
Many thanks for the feedback. My comments and explanations follow. …

> The comments make clear that we are going to be
> queried on the increases in heat waves statement
> as being too weak and only backed up in the FAQ.
> I personally like the European example but if you
> could also possibly put some text on that slide
> to help back it up more broadly, that will help
> to avoid challenges (please see the comments).

I included slide 22 which shows the shift in distribution of hot days and cold nights, and I thought this might be better than the Alexander et al maps. Again we run into too any slides. The change in hot days of course relates to heat waves, because the change in extremes relates to the whole pdf. The term heat waves is very subjective and the time scale is not always clear. There was a heat wave on east coast (New York 71F yesterday) although part of a month long warm period. The other main discussion of heat waves in our text is for Australia and I took out the slide of Australia temperatures vs precipitation in the first version (that Brian and Matilde have not seen). **There is not much we can do here. The preponderance of evidence from all the statistics and studies demonstrates a clear increase in heat waves, even if there is not a definitive study just on heat waves. That is what we have to say.**

Regards
Kevin

#2142 (Overpeck likes new post-1600 J. Lean solar reconstructions because they show little increase in solar output. Would like to extend them back to AD1000. Proposes a contrivance to do so by fitting them to a different reconstruction and using it to run them back.)

cc: Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Eystein Jansen <Eystein.Jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Anders.Levermann@pik-potsdam.de, Gian-Kasper Plattner <plattner@climate.unibe.ch>, Thomas Stocker <stocker@climate.unibe.ch>
date: Wed, 4 Jan 2006 17:32:22 -0700
from: Jonathan Overpeck <jto@u.arizona.edu>
subject: Re: [Fwd: Re: [Wg1-ar4-ch06] Follow-up from Christchurch]
to: Fortunat Joos <joos@climate.unibe.ch>

<x-flowed>
Hi Fortunat and friends - **I suggest that we (Fortunat, can you do this?) ask Thomas Stocker since he has lots of experience w/ IPCC and knows what we're trying to do too.** Is this ok?

If it's ok (and I'm guessing that it might not be ok to use an unpublished extended solar series,

as Fortunat suggest - but it would be more
comparable to other results in the same figure
(our old 6.10)), I think scaling to Bard would be
better since this is what has been done more in
the other simulations published and in the old
Fig. 6.10 - am I correct?

If we can't scale Judith's new recon back to
1000, then we'll just have some simulated series
back to 1610.

Again, thanks Fortunat for figuring it all out.

best, peck

>Hi Peck,
>
>Thanks for your thoughts. We will try to have a complete forcing series next
>week.
>
>Stefan and Anders are you happy with time series of radiative forcings in W/m2
>for a) solar - b) volcanic - c) CO2 -d) sum of non-CO2? Is it correct that you
>do not need concentrations and burdens for individual gases and anthropogenic
>and natural (volcanic and others) aerosols?
>
>For extrapolation of the Lean series it might be possible to use the Bard et
>al., Tellus, Be-10 record as it has been used widely. Another option would be
>to use 14C-derived solar modulation (Muscheler et al). This is more
>sophisticated, but solar modulation has up-to-date not been used in climate
>models. **In any case, extrapolation of the Lean**
>serie might be challenged in the
>IPCC context as we are leaving the area of published results.
>
>Regards,
>
>Fortunat
>
>
>Quoting Jonathan Overpeck <jto@u.arizona.edu>:
>
>> Hi Fortunat, Stefan and gang - **Have you given any**
>> thought to scaling the new solar forcing
>> estimates from Lean (sent w/ this email - thanks)
>> in some way (e.g., to 14C/10Be) so that the new
>> simulations could cover the last 1000 years,
>> rather than the last 400? This would be nice
>> given that we'll plot the new runs in a fig with
>> the existing/published runs (old fig 6.10). Might
>> take a little more work for someone, but could
>> you, for example, take an old solar series used
>> in a recent simulation shown in the old Fig 6.10,
>> and calculate the amplitude reduction implied by
>> the new Lean data over the last 400 years, and
>> then apply that same reduction (assuming it's
>> relatively constant - I'm being lazy here and not
>> ready up) to the old solar forcing back to 1000
>> AD?
>>
>> Might be a stupid idea, so it's ok to say so.
>> Please let me know what you think - again, it
>> would be good if both groups could use the same
>> forcing.
>>
>> Thanks again, peck

Continues in #2757 (MM= Maunder Minimum)
>> Eva Bauer wrote:
>>

>>>
>>> Dear Jonathan, dear Fortunat:
>>>
>>> Happy New Year!
>>>
>>>
>>> Stefan, Anders and me just have discussed how to set up our
>>> CLIMBER2/3alpha runs, to produce something useful for the IPCC WGI
>>> chapter 6. This chapter appears to touch the impact on the NH
>>> temperature related to low and high solar forcing.
>>>
>>> For a reasonable comparison, we think two 1000-year simulations
>>> differing only by a low and a high solar forcing, conducted with both
>>> CLIMBER models, would be ideal. **To do so, we would have to extend the**
>>> solar forcing time series based on Lean (GRL, 2000) and on Wang et
>>> al. (2005) distributed in previous e-mails back to the year 1000. This
>>> would require some splicing as was done, for instance, by Crowley.
>>>
>>> I'm thinking of some scaling applied to a series of Crowley (say the
>>> data called Be10/Lean splice in Science, 2000) such that the amplitude
>>> of the solar variability from the 11-year cycle is conserved after
>>> ~1720. I have to check but it appears that the variation in the TSI
>>> due to the 11-year cycle contained in the Crowley series agrees
>>> perfectly with the 11yr-cycle data in the file based on Lean (2000).
>>> Before starting such an exercise I like to ask you what you think
>>> about. We would be happy to receive your response quite soon to be
>>> able to finish the calculations with our slow model in time for the
>>> IPCC report.
>>>
>>> Could you please also comment on the other forcings we should include,
>>> namely the volcanic forcing and the CO2 forcing. For the present study
>>> we suggest to use the forcing as in Bauer et al (2000) but omitting
>>> the land-use. This means, using the volcanic forcing from Crowley,
>>> 2000 and the CO2 forcing based on Etheridge et al 1996 and Keeling and
>>> Whorf, 1996. (If you wish we can distribute these data series.)
>>>
>>> Also, thinking beyond the IPCC study, the model results may become
>>> interesting enough to be discussed in a 3-model comparison study!?
>>>
>>> Looking forward to your reply.
>>>
>>> Best wishes
>>>
>>> Eva
>>>

>> Dear Eva,
>>
>> We are working on the forcing series and they should be ready by the
>> end of the week. Stefan assured us that you can run this within a few
>> hours.
>>
>> What we are preparing are the following series of radiative forcing in
>> W/m2:
>>
>> a) RF from atmospheric constituents (well-mixed GHGs (CO2, CH4, N2O,
>> many Halocarbons) tropo and strato Ozone, various anthropogenic
>> aerosols) as used in the Bern CC TAR version and the TAR (see Joos et
>> al., GBC, 2001; pdf is on my homepage and TAR appendix).
>> b) volcanic from Crowley, Sci, 2000
>> c) solar based on Lean and Bard et al.
>>
>> For the solar we will prepare 3 combinations:
>>
>> c1) original serie from Lean (2005) provided to you already
>> c2) **Bard et al., Be-10 record linearly scaled to match the Maunder**
>> Minimum   Average of Lean-AR4
>> c3) Bard et al., Be-10 scaled to a MM reduction of 0.25 permil, i.e.
>> the low case in the Bard et, Tellus, publication corresponding to the
>> Lean et al, 1995 scaling

>>
>> For the RF by atmospheric components two cases are foreseen:
>> a1) standard case with reconstructed evolution over past 1150 years
>> a2) RF kept at 1765 value after 1765, i.e. a simulation with natural
>> forcings only.
>>
>> This will yield in total 6 simulations 3 over the full length from 850
>> AD to 2000 and 3 brach-off simulatons from 1765 with natural only
>> forcing.
>>
>> **An important point in IPCC is that things are published, consistent**
>> among chapters, and it helps if approaches are tracable to earlier
>> accepted and approved IPCC work. The arguments for these series are as
>> follows:
>>
>> a) Considering as many components relevant for RF as possible (more
>> than just CO2). The series are fully compatible with TAR and that the
>> setup is tracable to the TAR for the industrial era increase. The same
>> series will be used in the projection chapter 10 for the SRES calculation
>>
>> b) volcanic: a widely cited record
>>
>> c) solar: c1) and c3) are published series; c2 follows the same
>> approach and spirit as used to derive c3, i.e. scaling the Be-10 serie
>> linearly with a given Maunder Minimum reduction. The impact of the
>> 11-yr solar cycle can be looked at in the original Lean-AR4 serie.
>>
>> I hope this help.
>>
>> With kind regards,
>>
>> Fortunat

Jonathan Overpeck wrote:
> Dear Eva and Fortunat - thanks for working on getting things moving. It
> seems that the detailed forcing recommendations laid out below by
> Fortunat build nicely on what Eva first suggested, and that going with
> the forcing series suggested below by Foortunat (and the 6 simulations)
> is going to be just right for the IPCC AR4 Chap 6 needs. Does everyone
> agree?
>
> Thanks Fortunat for preparing/sharing the standard forcing series.
>
> Best, peck

6.3 Massaging the message

#4578 (How to make the trend look bigger)

date: Mon Jul 18 14:25:52 2005
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Text and CQ stuff
to: "Parker, David (Met Office)" <david.parker@metoffice.gov.uk>, Kevin Trenberth
<trenbert@ucar.edu>

Kevin,
Even without smoothing it is possible to get a trend of nearer 0.75 if the trend starts around 1920 (especially if the cold year of 1917 is at the start). The periods chosen for Table 3.2.2 had some justification, so we need to be a little careful. As a schematic for CQ2 though, it will be a different way of showing the same data.
I'll talk it over with David.
Cheers
Phil
At 14:03 18/07/2005, Parker, David (Met Office) wrote:

Kevin
I will discuss with Phil when he comes. We could ask John Kennedy to do

a plot. However, sub-period linear trends are already in Table 3.2.2
and, despite not being matched exactly to the sub-periods you suggest,
lead to a similar conclusion (ca 0.75C warming overall).
Regards
David
On Sat, 2005-07-16 at 22:59, Kevin Trenberth wrote:
> Hi all
>
> I have started going thru the text a bit more thoroughly. At present
> the description of the global mean temperature record is for a warming
> of 0.6C during the 20th Century. That is the linear reprducible
> value. But it is not a useful value as the trend is not linear. **In**
> the recent paper by Raper et al on SST they make a point to give
> values for both the linear trend and the change from the low pass
> filtered record. The latter is quite a bit bigger. I would like to
> see us adopt something similar. The question then is how to
> characterise the record. Here is my attempt: words
>
> However, the record is best characterized as level prior to about
> 1920, a warming to 1940 or so, leveling out or even slightly
> decreasing until 1970, and a fairly linear trend since then. Going by
> the low pass filtered data, the overall warming through 2005is 0.75Â°C,
> with 0.5Â°C increase occurring after 1970.
>
> To illustrate this I tried to capture the sense of this in the
> accompanying ppt. There are two slides. Make sure you go into slide
> show mode to view them. You will see the first has a smoothed trend
> the second has linear segments that join. The idea is to also capture
> the overall error bars to a reasonable degree, as you can see. In
> fact this could be linked to the modeling and attribution chapter to
> say that the warming in the first part of the 20th century was partly
> due to solar, the cooling from 1940 to 1970 to increased aerosol, and
> the warming after 1970 to the increasing GHGs.
>
> This could work very well as part of the CQ2.
>
> Ideally the background global mean values should not have the red bars
> on it but should just be a time series with error bars. The curves
> which I fitted by eyeball using power point should be done more
> rigorously, perhaps using a cubic spline fit with strong tension., or
> a series of segments with divides at 1940 and 1970. Then a linear
> value with the given starting point could be determined for both the
> mean and both end of the error bars.
>
> I am seeking feedback on this idea. 1) Is it a good idea and has your
> support? 2) Any comments or suggestions?
> 3) Any volunteers to do it more rigorously? Any such person would
> need the mean and error bars to do this from David or Phil?
> 4) Do you prefer the straight lines or smoothed values?
>
> Thanks
> Kevin

#2124 (Trenberth: one of the FAQs is supposed to say something about urbanization. Jones: I ignored it since it just confuses the message)

date: Tue Jul 26 17:13:55 2005
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: [Fwd: Natural GH effect]
to: Kevin Trenberth <trenbert@ucar.edu>

Kevin,
I remember the discussions, but I've not seen this attachment before. It might
have been posted on the WGI web site?
I was aware that CQ3.2 was supposed to include urban issues. I've been ignoring it as it just confuses the message the QACC is trying to convey.
I'm off home now. I'll think about this overnight. Each QACC would be better
after each section, like the boxes. This might mean renumbering. It will
also mean moving some figures and their renumbering?

Cheers
Phil
At 16:07 26/07/2005, you wrote:

Hi all
I just found out I am supposed to help do another CQ: on water vapor and the natural greenhouse effect. Help!
Anyway, I was also sent this attachment, which I don't think I had seen before. I recall the meetings on this in Beijing.
Several points affect us immediately
1) The CQ's are now labeled : QACCS (Questions about Climate Change Science) - that reeks of a committee decision, doesn't it?
2) In item 4 there are suggestions for CQ3.2 to add stuff on urban effects. So for the moment I have taken the short para from the exec summary and put it in there as a place holder.
3) In item 5 it suggests placing the QACCS in the text at the appropriate places, rather than at the end. This is a bit weird in that the order and numbering is fixed and so QACCS 3.2 will come before QACCS 3.1.   It is also apt to disrupt the text somewhat although it makes the figures more complementary. Si maybe we can place it at the end of major sections, not really embedded?
Please comment about the latter. I gather we don't have to do this, but perhaps we should? It also means relabeling all the references to CQ to QACCS. Henceforth you will not see CQ any more.
Kevin

APPENDIX B

Major Themes in Climategate 2.0 emails from 2008 and 2009

1. Blogs are Untrustworthy compared to Journals.

A general theme running through many of Phil Jones' recent emails is that information from blogs should not be trusted and that only information from peer-reviewed journal articles is reliable. This is somewhat ironic considering that Jones and colleagues conspire to tightly control the contents of the scientific journals seeking to gain favor for their papers and disfavor for the papers critical of the mainstream thought (i.e. skeptics). Rather than run this formidable gauntlet, many prefer blogs as an outlet for not only scientific commentary, but scientific findings as well. And, many people recognize the role that blogs and other outlets made available by new technologies (like the internet) have to play in moving science forward. By demeaning blog science, while gatekeeping journal science, Jones (and colleagues) control the course of science, rather than allow it to flow freely—a potentially dangerous situation.

Below are some examples from the emails, expressing disdain for blogs and dedication to peer-reviewed journal articles (see the original emails for the full exchange).

- In email **1338.txt** (Aug 24, 2009) Jones is expressing his frustration at Steve McIntyre's efforts and his Climate Audit blog tells Howard Ambler that "`Science advances through publications in scientific journals.`"

- In email **1892.txt** (Sep 24, 2009), Jones tells a perspective MA student in Scientific Journalism that it is "`very important to determine what people think they are experts in. They need to have publications in climate journals.`"

- In email **4373.txt** (Sep 29, 2009), Mike Mann tells Andy Revkin (science writer, *New York Times*) that "`A necessary though not in general sufficient condition for taking a scientific criticism seriously is that it has passed through the legitimate scientific peer review process. Those such as McIntyre who operate almost entirely outside of this system are not to be trusted.`"

- In email **0333.txt** (Sep 30, 2009), Jones tells Mike Mann and Gavin Schmidt that "`Another issue is science by blog sites - and the then immediate response mode. Science ought to work through the peer-review system..... sure you've said all these things before.`"

- In email **4832.txt** (May 8, 2008), Jones writes that "`You only get sound science in the proper climate science journals. These are the ones peer-reviewed by climate scientists. Journals have what is called an Impact Factor based partly on citation counts. If they don't mention this they aren't worth reading.`"

It is worth considering Jones' comments set against the recent Commentary of Jerome Ravetz of the Institute for Science, Innovation and Society at the University of Oxford that was recently published in *Nature* magazine, in which he describes how scientific standards in published are evolving to become more open and include amateurs, on-line discussion and blogs, etc.

http://www.nature.com/nature/journal/v481/n7379/full/481025a.html

Excerpt:

Society of Science: Keeping Standards High

Science is unique among areas of organized activity and production in that it has an informal quality-assurance system: peer review, publication and replication. The system has worked well since its inception in the seventeenth century, when the scientific journal came into being. But it is now being challenged as technology changes social practices of science. How might it evolve?

Some trends are apparent. The rise of digital media has revolutionized the management of information and created opportunities for broader involvement in science's production. Collaborations are growing ever larger, transforming the concept of authorship. Prepublication discussions of research on blogs dilute a principal author's claim to discovery. And the public is increasingly involved.

Amateurs are returning to mainstream research after an absence of generations. By completing online tasks, from classifying galaxies to solving complex protein-folding problems, anyone can become a co-creator of scientific knowledge. Such a widening of participation might be liberating, but it also risks lowering standards. Not everyone shares the ideal that intellectual integrity comes before personal gain.

As a result of these developments, the product of research is becoming more fluid. The journal is losing its status as the sole gatekeeper — simultaneous guarantor of quality, certifier of property, medium of communication and also archive. Other means of sharing material, assessing quality and screening out the incompetent or fraudulent are emerging to fill the gap, but ultimately the professional monopoly on quality assurance of science will have to be modified.

New gatekeepers

In response to these trends, some individuals are becoming self-appointed gatekeepers. During the polarized 'climategate' debates in 2010, for example, climate scientists stepped

> in to defend the work of a reputable colleague from criticism by a 'mere' mining engineer. That critic, Steve McIntyre, claimed on his blog simply to be applying the standards of the business world to climate data.
>
> Although scientific expertise presents a bar to interference, concerned outsiders have a legitimate and useful role."

2. Gatekeeping at Journals to Exclude Contrary Work:

In email **4666.txt** (Oct 23. 2009), Mann is specifically asked about allegations of being a "gatekeeper" by a *Wall Street Journal*-Europe reporter. In the email thread Mann lashes out at the reporter and copies their correspondence to a large number of other people: "`I've taken the liberty of copying this exchange to a few others who might be interested in it, within the broader context of issues related to the history of biased reporting on climate change at the Wall Street Journal Europe.`"

WSJ-Europe reporter Anne Jolis asked Mann:

```
"-How would you respond to the critique that, as a key part of the review
processes of publications in the field of climate science, as something of a
"gatekeeper," you have rejected and otherwise sought to suppress work that
contradicted your work. Is this fair? Why or why not? How would you
characterize your selection process for work that is or is not worthy of
publication?
```

To which Mann replied:

```
"I won't dignify that question with a response, other than to say that it
betrays a deep naivety about how the peer review process in science works,
and it buys into what I consider to be rather offensive conspiracy theories
that impugn the integrity of editors, reviewers in general, and myself in
particular."
```

Yet, compare that indignant response from Mann to this comment from him in email **2469.txt** (Apr 24, 2003)—note that Mann was an editor for the *Journal of Climate* (from 2000-2002):

```
"While it was easy to make sure that the worst papers, perhaps including
certain ones Tom refers to, didn't see the light of the day at J. Climate, it
was inevitable that such papers might slip through the cracks at e.g. GRL--
there is probably little that can be done here, other than making sure that
some qualified and responsible climate scientists step up to the plate and
take on editorial positions at GRL."
```

Mann fully admits that the peer review process is not perfect (when it comes to allowing paper to be published that he does not agree with), but bristles at the suggestion that the peer review process is being biased by he and his colleagues—despite numerous examples in the emails the indicate/suggest otherwise. He is blinded by his own preconceptions.

3. Influencing Journal Editors

There are many instances in the Climategate 2.0 emails in which the email authors are taking actions (either directly or indirectly, explicitly or implicitly) aimed at influencing the editors of scientific journals in order to gain favor for their submissions, to achieve disfavor for the submission of papers that they don't agree with, or to influence the editorial policies of the journals. None of these situation are the best practices to achieve the open, unbiased flow of science. Here are some examples.

a. Influence to gain favor for their papers

In the thread contained in email **2288.txt** (Jan. 14, 2009) Phil Jones complains to Glenn McGregor, the editor of the *International Journal of Climatology* (IJoC), that McGregor recently rejected two of Jones' submissions and that on this basis, he'll likely not submit any more paper to IJoC. (This seems like a threat). After a back and forth with McGregor, Jones finally convinces McGregor to rescind the rejection and to seek a third reviewer—and Jones provides suggestions of who that reviewer should be. Later, about 6 months later, in email **2452.txt**, Jones queries McGregor about the status of the third review, telling McGregor that some people are commenting about a program that Jones is involved in (the major science of which was described in Jones submission to the IJoC—if has to do with climate forecasting in the U.K.) had not been peer-reviewed. In other words, Jones has a need for his paper to be published in order to answer some criticism of it. Later in the same email thread (**2452.txt**), McGregor provides Jones with the third reviewer's comments, which were bad as well. But, in an apparent favor for Jones, McGregor chose instead of rejecting the paper, to allow Jones to resubmit it with major revisions. (Apparently, Jones chose to take his paper elsewhere, rather than make these changes, as a similar paper was eventually published by Jones in a different journal [*Nonlinear Processes in Geophysics*, 2011]).

****

In email **1423.txt** (Mar. 19, 2009) Jones tells Ben Santer that Jones send an email to the chief executive (Paul Hardaker) of the Royal Meteorological Society (RMS) complaining about how an editor of the RMS journal *Weather* was handling a submission by Jones, and threatening not to send any more papers to RMS journals and to resign as a RMS member.

```
date: Thu Mar 19 17:02:53 2009
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: See the link below
to: santer1@llnl.gov

    Ben,
…

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

In email **4632.txt** the editor of *Weather* (Bob Prichard) capitulates to Jones and tells him that "in order to bring this saga to an end" that if Jones will submit revised version of the manuscript that Prichard will accept it for publication.

```
Dear Professor Jones,
     Following your recent emails and in order to bring this saga to an end,
if you care to submit a revised article that you are happy with, we will
publish it.
     Please follow Weather's style guidelines to the extent that you are
aware of them as it otherwise makes for a lot of extra work at the proofing
stage.
     Yours sincerely
     Mr. BOB PRICHARD
     Editor, Weather
```

In the same email (**4632.txt**) Jones tells the chief executive of the RMS that he needn't worry about Jones' issues with the editor of *Weather*, that the issue as "almost been resolved"—obviously to Jones' liking:

```
date: Fri Mar 20 08:32:51 2009
from: Phil Jones <p.jones@uea.ac.uk>
subject: READ THIS ONE FIRST
to: "Chief Exec" <chief.exec@rmets.org>

    Paul,
```

```
       Hope you have read this one first. The issue I have with Bob Prichard
has been almost resolved - see below. So ignore that. Apologies for the
earlier email, but he just seems to wind me up at every step. I'll have to
try to be more restrained. I'll resubmit a revised version trying to take all
the points on board.
…
Cheers
    Phil
```

****

In email **1493.txt** (Jul 30, 2009) Mike Mann is discussing the submission of a paper that he is involved with that is a rebuttal to a paper published in the *Journal of Geophysical Research* (JGR) authored by McLean et al. Mann thinks the original review of the McLean et al. paper was poorly handled JGR and wants to make sure that the same editor doesn't handle the (anti-McLean et al.) paper that he and colleagues are submitting, suggesting that the chief editor of JGR should handle the piece.

```
We probably need to take this directly to the chief editor at JGR, asking
that this not be handled by the editor who presided over the original paper,
as this would represent a conflict of interest. if we are told that is not
possible, then we would at least want the chief editor himself to closely
monitor the handling of the paper.

 I too am happy to sign off at this point,

 mike
```

In email **3500.txt** (Aug 5, 2009), Jones discusses which reviewers they should suggest for their upcoming submission to JGR (the rebuttal to McLean et al.). Instead of suggesting unbiased reviewers, Jones suggests a list of people (all close colleageus) "All of them know the sorts of things to say - about our comment and the awful original, without any prompting." (Does this imply that Jones sometimes has to 'prompt' people reviewing his papers what to say?)

```
cc: "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan
<jdannan@jamstec.go.jp>, b.mullan@niwa.co.nz, Gavin Schmidt
<gschmidt@giss.nasa.gov>, Mike Mann <mann@meteo.psu.edu>,
j.renwick@niwa.co.nz
date: Wed Aug  5 16:14:34 2009
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: ENSO blamed over warming - paper in JGR
to: Kevin Trenberth <trenbert@ucar.edu>, Grant Foster <tamino_9@hotmail.com>

    Hi all,
       Agree with Kevin that Tom Karl has too much to do. Tom Wigley is semi
retired and like Mike Wallace may not be responsive to requests from JGR.
      We have Ben Santer in common !  Dave Thompson is a good suggestion.
```

I'd go for one of Tom Peterson or Dave Easterling.
To get a spread, I'd go with 3 US, One Australian and one in Europe.
So Neville Nicholls and David Parker.
All of them know the sorts of things to say - about our comment and the awful original, without any prompting.

Cheers
Phil

****

Ben Santer and colleagues are preparing a rebuttal to a piece published in the *International Journal of Climatology* (the IJoC editor is Glenn McGregor) by David Douglass et al. that showed a major discrepancy in the temperature trends in the tropical atmosphere between climate model projections and actual observations. The Douglass et al. paper was an important "skeptic"paper as it called into question the veracity of climate model projections. In email **4483.txt** (Jan 10, 2008), McGregor responds to an inquiry from Tim Osborn as to whether IJoC would be interested in publishing Santer's rebuttal under a set of conditions set forth by Santer (talk about influencing the journals!). McGregor tells Osborn that he would like to see the rebuttal published in IJoC and that he

"will do everything in my power to get their paper online asap" and that "I must confess that I think I made a misjudgement in letting the offending paper through as Francis Zwiers was not impressed with the paper having reviewed it but left it up to my judgement which on reflection was misplaced."

In email **2624.txt** (Jan 10, 2008), Jones discusses which reviewers they should suggest to McGregor in order to get a speed review, including at one person who has been on the emails correspondence list concerning the development of the Santer paper! (talk about an inside job).

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.

In email **4399.txt** (Jan 11, 2008) Jones asked Tim Osborn if he would go back to McGregor and see if he will agree to "a few conditions" that Santer insists upon prior to his submission of the paper to IJoC, including one that insures that Santer gets the "last say" in any reply/comment exchange that his paper may generate from Douglass et al. If McGregor doesn't agree to Santer's conditions, Santer will submit the paper with another journal.

Tim,

```
    I spoke to Ben last night. He elaborated a bit on the email below.
  In the light of this, can you send an email to Glenn to see if he
  will agree to a few conditions. Could say can we clarify a few things?

  1. Can the paper be considered as a new submission and not as a comment on
the Douglass et al paper? Ben will likely go for GRL if Glenn won't agree to
this. The issue is that he doesn't want Douglass to have the last say. Ben
happy for Douglass et al to respond, but he then gets the final say in any
reply.
```

In email **4316.txt** (Jan 11, 2008), Osborn pitches Jones/Santer requirements to McGregor, including using Francis Zwiers (the person who has been involved in the discussion of Santer's efforts and a critical reviewer of the original Douglass et al. paper). McGregor agrees.

```
date: Fri, 11 Jan 2008 13:26:33 -0000
from: "Glenn McGregor" <glenn.mcgregor@kcl.ac.uk>
subject: RE: Update on response to Douglass et al.
to: "Tim Osborn" <t.osborn@uea.ac.uk>

Tim

thanks for your comprehensive response

I have no problem treating the Santer et al contribution as a full paper. I
just assumed that they wanted to publish a comment. So if you would like to
relay this to BS I would be grateful. Needless to say my offer of a quick
turn around time etc still stands
…
Best
Glenn
```

In **email 4235.txt** (Jan 11, 2008) Osborn tells Santer that McGregor has agreed to Santer's conditions, and has even said that he would delay the print publication of Douglass et al. so that Santer's rebuttal could be published along side of it 9at this point the Douglass et al. paper had only appeared on-line at IJoC), but Osborn asked Jones not to share that information with anyone.

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

<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

In email **4149.txt** (Jan 11, 2008), Osborn gets back to McGregor telling him that Santer has agreed to send the paper to IJoC (under the conditions previously discussed) and Osborn includes a list of potential reviewers (including Zwiers).

From: Tim Osborn [mailto:t.osborn@uea.ac.uk]
Sent: Friday, January 11, 2008 5:33 PM
To: Glenn McGregor
Subject: Santer et al. manuscript

Hi again Glenn (sorry if I'm interrupting holiday time... I forgot to
ask if you were there for work or holiday),

Ben has decided to submit to IJC -- the right decision I think! --
and would like it treated as a independent submission. He said it
would be ready in about a week.

With regards potential reviewers... Francis Zwiers seems appropriate,
if he's willing. I guess you already have his contact
details. Others with appropriate expertise of tropospheric
temperatures and/or model-data comparisons would be:

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

or

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

Regards

Tim

Dr Timothy J Osborn, Academic Fellow
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ, UK

****

In email **0240.txt** (Jan 28, 2009), Phil Jones tells Eugene Wahl that Keith Briffa has made some arrangements with the journal *The Holocene* (for which Briffa is an editor) for a quick review of a paper Jones and Wahl are co-authors on. It seems that a published paper would help justify some funding.

```
Keith's arranged with the Holocene to get the whole thing reviewed
 quickly, so we'll pick up time. It seems though that Larry
 wants something to justify his funding of the Wengen meeting.
```

Jones further mentions Briffa's arrangements to fast track this paper in emails **3937.txt** and **3293.txt**.

****

In email **0372.txt** (Nov 14, 2008) Keith Briffa tells Ed Cook that Briffa has suggested Cook as a reviewer of a proposal that he has submitted, and schools Cook as to what kind of reviews he would like him to give (apparently , these folks do prompt reviewers!)

```
as a suggested referee - NERC often ignore these suggestions - but if you get
it you need to grade it top for importance and quality etc. More later on
this.
   Have to rush
   Keith
```

****

b. Influencing editors against skeptic papers

In email **5112.txt** (Feb. 19, 2009), there is a conversation between Phil Jones and Chet Ropelewski (editor of the *Bulletin of the American Meteorological Society,* (BAMS)). Ropelewski asks Jones if he will review a paper. Jones seems to indicate that he is too busy, but if Ropelewski could send him the abstract and author list, that Jones could perhaps makes some suggestions as to who would be good to review it. Ropelewski provides this information to Jones, and upon finding out that Roger Pielke Sr. is one of the authors, Jones proceeds to warn Ropelewski that Pielke Sr. will be hard to deal with, that Pielke Sr. makes unsubstantiated claims, that another Pielke Sr. paper had been recently published that had been commented on (negatively) by David Parker (so Jones recommends Parker as a reviewer of the

BAMS submission), and also provides a sort of informal review for which Ropelewski thanks him and comments "I fear this submission is going to be a struggle."

Roger Pielke Sr., at his blog, adds some comments and more behind-the-scenes interactions with Ropelewski concerning this paper (that was eventually published in BAMS). Pielke Sr.'s comments are detailed at this link:

http://pielkeclimatesci.wordpress.com/2011/11/28/inappropriate-interaction-between-an-ams-bams-editor-and-phil-jones/

Pielke Sr. ends his comments with this: "This-mail exchange shows how much of an "old boys" network, the review process is."

****

In email **2070.txt** (Jan. 29, 2009) Phil Jones tells Ben Santer that Steve McIntyre (who Santer refers to in this email exchange as "`Mr. Mc "I'm not entirely there in the head"`") and Ross McKitrick have just submitted a paper to the *International Journal of Climatology* (and editor Glenn McGregor) that is critical of a paper recently published in IJoC by Santer. He tells Santer that if Glenn McGregor contacts Jones, that he'll be sure to suggest some reviewers for the McIntyre and McKitrick submission (reviewers that Jones, no doubt, expects will be highly critical of anything McIntyre submits).

****

In email thread **1778.txt**, Kevin Trenberth discusses with Jim Salinger writing to the American Geophysical Union (AGU) "`publications board`" telling them that "`that there was a breakdown in the editorial oversight and reviewing process`" and suggesting "`that the commission should approach the responsible editor to upgrade his/her practices.`" This is in response to the publication of a paper by McLean et al. (the same Mclean et al. paper referred to in previous comments) in the AGU publication *Journal of Geophysical Research* that Salinger and colleagues took a dislike to (and later published a rebuttal). Later in the same email (**1778.txt**), Salinger tells Trenberth that he is happy with Trenberth's suggested approach.

date: Tue, 01 Sep 2009 13:50:03 +1200
from: Jim Salinger <j.salinger@auckland.ac.nz>
subject: RE: McLean et al. 2009 our response
to: trenbert@ucar.edu

<x-flowed>
Greetings Kevin

From a hot Geneva night - although I have just calculated that August in NZ was the warmest ever: 10.4 which is +1.7 deg C and equal to average September.

I did look up the AGU structure and Alan currently chairs the Atmospheric Sciences part. And Tim Kileen is the past president, the new president having just come and being a geologist. I am more than happy with the approach you suggest but, as doubtless you know, the deniers are really publicising it.

Hope all is well with your parents...I am back properly Thursday next week, but in NZ from Tuesday (at RSNZ workshop in Wellington workshop on acidification Wednesday)

Best

Jim

Quoting Kevin Trenberth <trenbert@ucar.edu>:

> Hi Jim
> I am in chch. Not sure that this is the right thing to do. Look up the
> AGU structure and see if there is a publications board or commission.
> They are the ones who should be written to if anyone. Wouldn't hurt to cc
> Alan. Also you should say that you believe there was a breakdown in the
> editorial oversight and reviewing process. Do not say "shocked". Hint
> that the commission should approach the responsible editor to upgrade
> his/her practices.
> Kevin

****

c. Influence as to the policies of journals

In **email 1423.txt** (Mar 19, 2009) Ben Santer tells Jones that if the Royal Meteorology Society establishes a data transparency policy that requires that authors make all of their data freely available, that he will no longer submit any more papers to RMS journals (like the *International Journal of Climatology*).

> Thanks, Phil. The stuff on the website is awful. I'm really sorry you have to deal with that kind of crap.
> If the RMS is going to require authors to make ALL data available - raw data PLUS results from all intermediate calculations - I will not submit any further papers to RMS journals.

```
        Cheers,
           Ben
```

****

d. Influencing the general viewpoint of the journal editors

In email **0845.txt** (Jan 15, 2008) Mike Mann tells Phil Jones that they should contact the chief editor of *Science* (David Kennedy) to complain about a "news" piece that was published in *Science* that Mann didn't like.

```
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.
```

4. Bad editor practices

As can be seen in the email samples above, Phil Jones and colleagues have acted to exert their influence over the editors of various scientific journals. In some cases, emails exist to illustrate that the influence was successful and that editors succumbed to the desires of Jones and colleagues. This is bad practice. The example below shows more bad practice from journal editors.

In email **0366.txt** (Nov 2, 2008) Glenn McGregor ask Tim Osborn for a quick review of a paper submitted to *the International Journal of Climatology* (for which McGregor is an editor), and gives Osborn the already completed review by another reviewer. It is impossible for a reviewer to be unbiased in a situation where he has another set of reviews. This practice is unacceptable.

```
date: Sun, 2 Nov 2008 01:50:09 -0400 (EDT)
from: g.mcgregor@auckland.ac.nz
subject: JOC-08-0099 - Invitation to Review
to: t.osborn@uea.ac.uk

02-Nov-2008

TIM: COULD YOU HELP OUT WITH A QUICK REVIEW OF THIS. I HAVE PASTED BELOW THE
COMMENTS FROM THE OTHER REVIEWER FYI
```

5. Pal Review/Inside Help

Peer review of articles submitted to scientific journals is one of the pillars of the building of scientific knowledge. In most climate journals, peer review is single blind—that is, the reviewer knows the identity of the authors, but the authors don't know the identity of the reviewers. The reviewers are chosen by the journal editor who is overseeing the process through which a submitted article is reviewed, how the authors respond to the reviews, and ultimately, based upon this review/response, makes a decision as to whether or not the submitted article is published in the journal. The policy for most journals is that the reviewers should not be too closely affiliated with any of the authors of the paper, nor predisposed to have an opinion about the paper. These conditions are to insure a fair and unbiased review, not dictated by interpersonal relationships. Involvement of the latter impinges on the free growth of scientific knowledge, an instead steers science in a particular direction favored by the reviewers. Often times, the reviewers think that they are performing nobly by keeping the "junk" out of science, and making it purer in the process. But such is a dangerous game, for if the "truth" doesn't lie down the pathway that scientific understanding is being guided, it's ultimately discovery may be unduly delay for an indefinite period—the this misfortune of all.

Commonplace throughout the Climategate emails, are examples where conditions necessary for the free flow of scientific knowledge are not met. These include situations where reviewers have a close relationship with the authors of the papers they are reviewing ("pal review"), situations where the authors are looking to colleagues who may have some "pull" with particular journals/editors ("inside connections"), and situations where authors are seeking special favor directly from journals/editors ("special favor")

a. Pal Review

In email **3611.txt** (Mar. 4, 2009) *Science* magazine invited Phil Jones to be a reviewer for a paper submitted by Mike Mann. Jones agrees (but by that time, *Science* already had located enough other reviewers. As Jones and Mann are close collaborators, Jones should have declined the invitation to review.

****

In email **0608.txt** (Aug. 20, 2009) *Geophysical Research Letters* (GRL) invited Jones to review a reply/comment involving an article previously published in GRL by Michael Mann. As Jones and Mann are close collaborators, Jones should have declined the invitation to review.

****

In email **2938.txt** (Sep. 11, 2009) Jones agrees to be a reviewer of a paper submitted to the *Journal of Geophysical Research* (JGR) by Matthew Menne et al. on station siting issues and their effect on temperature trends. In email **5209.txt** (Sep. 18, 2009), Phil Jones submits his review. Jones is not an unbiased reviewer. In fact, he kind of has a horse in this race and, in email **3739.txt** (May 15, 2009) Jones told a colleague of Menne's that Jones had hoped that he (Menne's colleague Tom Peterson) had persuaded Menne do an analysis in order to counter Anthony Watts' claims that station siting has a potential impact on temperature trends. The paper that Jones reviewed and recommended for publication was, in fact, that analysis.

Excerpt from email **3739.txt** (May 15, 2009) from Jones to Tom Peterson asking Tom to persuade Menne to do an analysis looking into Anthony Watts' hypothesis. This shows that Jones should serve as an unbiased reviewer of Menne's submission to JGR.

```
date: Fri, 15 May 2009 09:31:24 -0400
from: Thomas C Peterson <Thomas.C.Peterson@noaa.gov>
subject: Re: Parker on Pielke
to: Phil Jones <p.jones@uea.ac.uk>

   Very cute, Phil. I've passed your suggestion on to Matt.
           Tom

Phil Jones said the following on 5/15/2009 9:19 AM:

      Tom, David, John,
         Here's the first paper to cite it!  As we know they didn't realise
the significance of Figure 1!

          I hope you've persuaded Matt Menne to do that USHCN split (into the
watts-up-that categories).
          You could then have a title.
      Watts-up with this - no differences in US average for stations in
different categories
      Cheers
      Phil
```

****

b. Inside Connections

Email **2254.txt** (Jan 9, 2009):

date: Fri, 09 Jan 2009 08:41:55 -0500
from: David Easterling <David.Easterling@noaa.gov>
subject: paper
to: Phil Jones <p.jones@uea.ac.uk>

Content-type: text/plain; charset=windows-1252; format=flowed
X-MIME-Autoconverted: from 8bit to quoted-printable by
ueamailgate02.uea.ac.uk id n09DfWar002348

<x-flowed>
Hi Phil,

Michael Wehner and I have written a very short paper in response to all this garbage about the climate "cooling" since 1998 (attached). We wrote it for either Science or Nature, but Science balked at it claiming it is too specialized (what a crock) and should go to a specialty journal. We feel they are gun-shy about publishing controversial papers due to some lawsuit a contrarian filed against them. I would like to get it into Nature as a short contribution but its not clear to me how to do it since it is not a Letter and it looks
like most of these kinds of papers are solicited by Nature. Do you have any connections there such as the editor in charge of climate and can help out here? Maybe its not newsworthy enough, but we sure get enough grief from lots of places due to the bloggers, etc. and I feel it is very timely.

Cheers,
Dave

--
David R. Easterling, Ph.D
Chief, Scientific Services Division
NOAA's National Climatic Data Center
151 Patton Avenue
Asheville, NC 28801
V: +1 828 271 4675
F: +1 828 271 4328
David.Easterling@noaa.gov

****

c. Special favor

Email **4217.txt** (Oct 4, 2008).

Keith,
I have gone through this paper (Tardif) and it is very good.
They produce empirical evidence to show that the occurence of thin latewood vessel walls are not related to absolute temperature but to relative

changes (if mean tempertaure is more than 2 degrees below recent mean) presumed due to aclimation of trees. By implication this suggests that MXD cannot retain long-timescale variance which makes the paper important, i.e. you should review it.

As it comes from Prentice and we may want a favour from him later it ought to be done sooner than later.

Tom

6. Personal Attacks on Skeptics

Throughout the Climategate 2.0 emails are examples of disparaging remarks made towards climate skeptics, often expressing biases against these people and hopes that their science is wrong. Below are exmaples of such expressions, often made to a wide audience of people copied on the emails.

a. Fred Singer

Email **0624.txt** (May 18, 2009) Malcolm Hughes says:

"By the way, does he know Fred Singer is an AGU Fellow? It made me think about renouncing(!#@!) my fellowship."

b. McIntyre/Climate Audit

Email **0208.txt** (Jul 29, 2009) Mann writes to Phil Jones:

"I've been trying to no avail to get some journalist to look into their funding, industry connections, etc. they need to be exposed--badly!"

c. Patrick Michaels

In emails **0549.txt** (Oct 13, 2009), **0452.txt** (Oct 14, 2009) Tom Wigley attacks Michaels' PhD (completed nearly 30 years prior):

You may be interesting in this snippet of information about Pat Michaels. Perhaps the University of Wisconsin ought to open up a public comment period to decide whether Pat Michaels,PhD needs re-assessing?

Michaels responds to Wigely's attacks at *Forbes.com*:

http://www.forbes.com/sites/patrickmichaels/2011/12/02/climategate-ii-an-open-letter-to-the-director-of-the-national-center-for-atmospheric-research/2/

d. John Christy/Roy Spencer (compilers of the University of Alabama-Huntsville (UAH) satellite record of the temperature history of the lower atmosphere)

Email **3028.txt** (Jul 23, 2009) Michael Mann writes [possibly to Seth Borenstein of AP]:
Christy and Spencer continue to produce revised versions of the MSU dataset, but they always seem to show less warming than every other independent assessment, and their estimates are largely disregarded by serious assessments such as that done by the NAS and the IPCC.

Email **4833.txt** (Oct 5, 2009), Jones to Wigley:

```
It would of course, at this and any other time, be very nice to show that UAH
is wrong.
```

Email **0005.txt** (Mar 4, 2008), Jones to Mann:

```
If only RSS could definitively show that the UAH is wrong.
```

Email **3707.txt** (Jul 30, 2009), Santer to Tom Karl:

```
Thanks for forwarding the message from John Christy. Excuse me for being so
blunt, but John's message is just a load of utter garbage.
```

e. Judith Curry

Email **0810.txt** (May 30, 2008), Mann to Jones:

```
I gave up on Judith Curry a while ago. I don't know what she think's she's
doing, but its not helping the cause, or her professional credibility.
```

7. Recent Lack of Warming

Interspersed throughout the Climategate 2.0 emails are comments about the recent 10 to 15 year period during which the rise in global temperatures has slowed considerably. Most discussions are about the influence of natural variability, although some suggest other anthropogenic influences. Jones, at least for one, expresses his desire that the pace of warming picks up again very soon.

In email **4671.txt** (Jan. 3, 2009), Mike McCracken is getting tired of hearing that natural variability is to blame for the slowdown and suggests anthropogenic sulfate emissions are the true cause:

```
From: Mike MacCracken [mailto:mmaccrac@comcast.net]
Sent: 03 January 2009 16:44
To: Phil Jones; Folland, Chris
Cc: John Holdren; Rosina Bierbaum
Subject: Temperatures in 2009
…
But, I have one nagging question, and that is how much SO2/sulfate is being
generated by the rising emissions from China and India (I know that at least
some plants are using desulfurization--but that antidotes are not an
inventory). I worry that what the western nations did in the mid 20th century
is going to be what the eastern nations do in the next few decades--go to
tall stacks so that, for the near-term, "dilution is the solution to
pollution". While I understand there are efforts to get much better
inventories of CO2 emissions from these nations, when I asked a US EPA
representative if their efforts were going to also inventory SO2 emissions
(amount and height of emission), I was told they were not. So, it seems, the
scientific uncertainty generated by not having good data from the mid-20th
century is going to be repeated in the early 21st century (satellites may
```

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

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

Best, Mike MacCracken

In email **4671.txt** (Jan. 3, 2009) Chris Folland comments on McCracken's email and admits that the GHG+aerosols warming rate is only 0.15C/decade (note: the IPCC expects about 0.20C/decade of warming):

cc: <p.jones@uea.ac.uk>
date: Sat, 3 Jan 2009 21:31:27 -0000
from: "Folland, Chris" <chris.folland@metoffice.gov.uk>
subject: FW: Temperatures in 2009
to: "Johns, Tim" <tim.johns@metoffice.gov.uk>, "Smith, Doug" <doug.smith@metoffice.gov.uk>

Tim and Doug

Please see McCrackens email.

We are now using the average of 4 AR4 scenarios you gave us for GHG + aerosol. What is the situation likely to be for AR5 forcing, particularly anthropogenic aerosols. Are there any new estimates yet? Pareticularly, will there be a revision in time for the 2010 forecast? We do in the meantime have an explanation for the interannual variability of the last decade. However this fits well only when an underlying net GHG+aerosol warming of 0.15C per decade is fitted in the statistical models. In a sense the methods we use would automatically fit to a reduced net warming rate so Mike McCracken can be told that. In other words the method creates it own transient climate sensitivity for recent warming. But the forcing rate underlying the method nevertheless perhaps sits a bit uncomfortably with the absolute forcing figures we are using from AR4. However having said this, interestingly, the statistics and DePreSys are in remarkable harmony about the temperature of 2009.

Any guidance welcome

Chris

****

In email **3408.txt** (Jan. 10, 2008), Jones tells Santer "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!"

****

Email **1878.txt** (Aug 29, 2008), Mike Mann writes:

```
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...
```

****

Email **1878.txt** (Aug 29, 2008), Tom Karl writes:

```
> Curt,
>
> At this point the leveling off is more of a Blog myth than any change
> point scientific analysis
>
> Tom
```

****

8. Divergence Problem /Paleo Climate Uncertainty

In the last two years-worth of emails (from 2008 and 2009) there is a fairly large amount of discussion about the "divergence" issue in paleoclimatology—that is, that in some parts of the world, temperature reconstruction based on tree rings do not faithfully track observed warming in recent decades. This is a major problem for the reliability of the tree-ring reconstructions for it means that similar behavior may have existed in the past and therefore, tree-ring temperature reconstructions may underestimate temperature increases in the pre-instrumental time period. This problem was only briefly discussed in the IPCC AR4 which concluded that it was "likely" that the temperatures during the last 50 years of the 20th century in the Northern Hemisphere were the highest during the last 1,300 years. If the divergence problem was large, such a conclusion would be unfounded.

While the IPCC AR4 downplayed the problem, there has been a lot of subsequent research into just how large the divergence issue really is and what may be the cause. In fact, an entire project of the U.K.'s National Environment Research Council (NERC) that was proposed by Keith Briffa (one of the Lead Authors of the IPCC AR4 chapter on Paleoclimate) that has been dedicated to the "Divergence Problem."

In email **2836.txt** (Jul 29, 2009), Tim Osborn describes the issue in a nutshell:

```
date: Wed Jul 29 16:56:48 2009
from: Tim Osborn <t.osborn@uea.ac.uk>
subject: Re: The Dendroclimatic Divergence Phenomenon NERC

Palaeoclimate reconstructions extend our knowledge of how climate varied in
times before expansive networks of measuring instruments became available.
These reconstructions are founded on an understanding of theoretical and
statistically-derived associations acquired by comparing the parallel
behaviour of palaeoclimate proxies and measurements of varying climate.
Inferences about variations in past climate, based on this understanding,
necessarily assume that the associations we observe now hold true throughout
the period for which reconstructions are made. This is the essence of the
uniformitarian principle. In some northern areas of the world, recent
observations of tree growth and measured temperature trends appear to have
diverged in recent decades, the so called "divergence" phenomenon. There has
been much speculation, and numerous theories proposed, to explain why  the
previous temperature sensitivity of tree growth in these areas is apparently
breaking down. The existence of divergence casts doubt on the uniformitarian
assumption that underpins a number of important tree-ring based
(dendroclimatic) reconstructions. It suggests that the degree of warmth in
certain periods in the past, particularly in medieval times, may be
underestimated or at least subject to greater uncertainty than is currently
accepted. The lack of a clear overview of this phenomenon and the lack of a
generally accepted cause had led some to challenge the current scientific
consensus, represented in the 2007 report of the IPCC on the likely
unprecedented nature of late 20th century average hemispheric warmth when
viewed in the context of proxy evidence (mostly from trees) for the last 1300
years.
   This project will seek to systematically reassess and quantify the
evidence for divergence in many tree-ring data sets around the Northern
Hemisphere. It will establish a much clearer understanding of the nature of
the divergence phenomenon, characterising the spatial patterns and temporal
evolution. Based on recent published and unpublished work by the proposers,
it has become apparent that foremost amongst the possible explanations is the
need to account for systematic bias potentially inherent in the methods used
to build many tree-ring chronologies including many that are believed to
exhibit this phenomenon.
```

In email **0232.txt** (Sep 17, 2009), Keith Briffa writes to announces that the NERC has approved the proposed "Divergence" project.

```
Dear Colleagues and Friends,
```

```
We are writing now to inform you that our application to the UK NERC for
support to investigate the so-called "Divergence" phenonomen in
temperature-sensitive trees over a range of geographical and ecological
situations has formally been approved.
```

****

In email **2881.txt** (Oct. 14, 2009) Tom Melvin lays out a list of "Challenges Posed by Divergence" with the conclusions that "`Lots of work to do to clarify the situation`":

```
CHALLENGES POSED BY DIVERGENCE

1. Problem with curve-fitting e.g. Hugershoff  (Briffa 1998) and trend
distortion - part solution Signal free.
2. Problem with mixing sloping and horizontal curve fitting in Arstan (e.g.
D'Arrigo 2004) - part solution RCS.
3. End effect problems with RCS (Briffa - Hughes book)  - e.g. sample bias
4. Problem with updating chronologies (TTHH and Grudd 2008, Tornetrask)
5. Potential problem with Crown dieback (e.g. responders / non responders)
6. Potential MXD in sapwood problem ????
7. Potential competition problem - tree density changes RCS shape (Helama
2006)
8. Problem with non-linear response / skewed index distribution (Barber,
Wilmking etc)
9. Remove all these and residual is real divergence - problem with
identifying cause:
      CO2 change / Nitrogen fertilisation / Global dimming / UV light /
Drought stress/
Conclusion - Lots of work to do to clarify situation.
```

9. Uncertainty issues in paleoclimate and proxy temperature reconstructions

The divergence problem is just one part of the many uncertainties that plague paleoclimate reconstructions. The emails include several discussions about other uncertainties which may have a large influence on the reliability/interpretation of proxy temperature reconstructions. Many admissions in the emails seem to be more candid than appear in the scientific publicaitons (or the IPCC AR4).

In email **1583.txt** (Aug 18, 2009) dendro researcher Rob Wilson describes "uncertainty" in paleo climate reconstructions, and how the picture is not pretty—in fact, the level of uncertainty may be increasing.

```
My one real worry is the use of the term "reducing uncertainty".
The palaeo-world has become a much more complex place in the last 10 years
and with all the different calibration methods, data processing methods,
proxy interpretations - any method that incorporates all forms of uncertainty
and error will undoubtedly result in reconstructions with wider error bars
than we currently have. These many be more honest, but may not be too helpful
```

for model comparison attribution studies. We need to be careful with the wording I think.

****

Email **1578.txt** (Aug 24, 2009) is a lengthy thread including various participants discussing an NERC Proposal for further study of paleo/proxy reconstructions. Here are a few highlights which illustrate that the many members of the paleo/proxy community think that the science is far from being settled when it comes to existing paleo-proxy temperature reconstructions and the uncertainties that they reflect.

Phil Jones writes:

A parallel thrust could be emphasizing the uncertainties in all the reconstructions. As Rob says this is quite difficult with the proxy data as each discipline has a specific set of limitations. I'd also expect the uncertainties to expand, as we brought more things in.

…

Not sure where this is taking us. There are a lot of good scientific issues when considering combining proxies. In reconstructions like MBH, which ones do the work and which are superfluous. The longer instrumental records that are coming along - on both land and sea will enable many of these issues to be addressed, enabling the robustness of large-scale reconstructions to be quantified.

Groups all around the world are trying to do this at local-to-regional scales with some looking more globally. What is needed is co-ordination of these efforts, bringing together all the contacts each of us has.

Better quantified reconstructions should eventually lead to reductions in climate sensitivity, but it will be a long process.

Tim Osborn writes:

In agreement with some others' comments, it is unconvincing to say that a major aim is to determine climate variations over last 500 years with greatly reduced uncertainties. (a) Uncertainties of large-scale reconstructions are not fully estimated, so difficult to claim that we will reduce something when we don't know how big it is to begin with. (b) I don't think we'll "greatly" reduce them anyway.

****

In email **3925.txt** (Aug 24, 2009) Rob Allen puts the final touches on the NERC Proposal and tells everyone that they would be surprised by just how much data is still “out there” that that has not been incorporated into current analyses and which may have a significant impact.

cc: Rob Wilson <rjsw@st-andrews.ac.uk>, Gabi Hegerl <gabi.hegerl@ed.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Sandy Tudhope <sandy.tudhope@ed.ac.uk>, Chris Turney <turneychris@gmail.com>, simon Tett <simon.tett@ed.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Chris Jones <chris.d.jones@metoffice.gov.uk>, Philip Brohan <philip.brohan@metoffice.gov.uk>, "Bass, Catherine" <C.J.Bass@exeter.ac.uk>, "ewwo@bas.ac.uk" <ewwo@bas.ac.uk>
date: Mon, 24 Aug 2009 13:43:30 +0100
from: Rob Allan <rob.allan@metoffice.gov.uk>
subject: Re: NERC Consortium Bid - the 2 pager
to: "Cox, Peter" <P.M.Cox@exeter.ac.uk>

Peter,
I've added in a few changes and corrected a couple of typos in the attached.

Re the historical reanalyses concern, I've toned that down re teleconnections in data sparse regions, but would say simply that I think that you will all be rather surprised to see just how much additional surface terrestrial and marine data are 'out there' and will be going into these reanalyses. There are as much marine surface instrumental weather observations around a good part of the globe prior to World War 2 to be recovered, digitised and assimilated into the reanalyses back into the mid-19th century as already exist in the international data bases, such as ICOADS. The improvement in the Pacific will be greater than I think any of us imagined would be possible.

Cheers, Rob.

****

In the email thread **1910.txt** (Sep 29, 2009) Phil jones is discussing with Raphael Neukom some of the intricacies of developing a paleo temperature reconstruction from proxies and how sensitive the reconstruction is to a variety of data and methodological issues. The importance of this exchange it that is shows that the researcher, through making particular data/method choices can carefully craft the ultimate outcome (i.e. the shape and character of the temperature reconstruction).

Jones writes:

date: Tue Sep 29 15:35:53 2009
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: South American Temp. reconstruction paper draft
to: Raphael Neukom <neukom@giub.unibe.ch>

Ralphi,
Jones, P.D., Raper, S.C.B., Cherry, B.S.G., Goodess, C.M. and Wigley, T.M.L., 1986: A Grid Point Surface Air Temperature Data Set for the Southern Hemisphere, 1851-1984, U.S. Dept. of Energy, Carbon Dioxide Research Division, Technical Report TR027, 73 pp.
I'm not saying you should use these, but they might do better than those in GHCN.

```
   Hopefully it won't take long to check. I suspect there will be little in
it, so you might be able to use what you already have.
     I'm fully aware of how sensitive a PCR program can be to the
addition/deletion of one site!
    Cheers
    Phil
```

****

In email **1734.txt** (Oct. 2, 2009), Phil Jones explains some of the issues with tree rings as a climate proxy, while ultimately concluding that he has always done things the right way, unlike some other proxy researchers.

```
You've made clear that the chronology is built first - then we
> look at the climate response. A few dendro types have been caught
> only putting in individual cores that agree with the instrumental but
> this isn't the way we've ever worked.
```

****

In email **4622.txt** (Oct 16, 2009) tree-ring research Rob Wilson and Phil Jones have a discussion about the many uncertainties in tree-ring temperature reconstructions. Jones writes:

```
As you're fully aware there are lots of local non-climatic factors that can
influence trees. Even though all the sites are all with a few hundred km of
each other, you can't just cut some out and add others in. You could on these
scales with temperature sites, but trees are different.
```

Wilson writes:

```
I am busy developing a large network of pines from Scotland at the moment and
this species is incredibly sensitive to site differences, management
influences etc. I am not surprised that there could be some 'odd' sites in
the Russian data.
```

****

In email **0418.txt** (May 18, 2008), Rob Wilson details some of the many problems with proxy temperature reconstructions:

```
8. Finally, w.r.t. to NH reconstructions, individual constituent TR
chronologies should be assessed for their `climatic relevance' at the local
scale ONLY - i.e. they are robust estimates for local/regional climate. It
does NOT matter how they correlate with large scale NH temperatures. The
Jacoby/D'Arrigo principle of only looking for those series that express some
sort of mythical large scale signal is wrong and biased. Gaspe is a good
example of this. Do not pass this on to Gordon/Rosanne. :-)
```

(Gaspe is a part of the hockey stick, *see* http://climateaudit.org/2008/04/07/the-mbh-ad1450-network/, and Wilson is suggesting that it was improperly created.)

In email 4401.txt (June 2, 2008), Keith Briffa responds to Wilson in the affirmative.

```
      Rob
     I agree with virtually everything you say - thanks.
 …Cheers
     Keith
```

****

In email **3317.txt** (Jul 6, 2008) paleoclimate researcher Mark Bateman is commenting on a research proposal on paleoclimate and the high degree of inherent uncertainty that is still present and the progress required to try to reduce them.

```
[5] couldn't see the point made anywhere that whilst being more open
about all uncertainties in palaeoclimate data may be painful in the
short-term (and lead people to wonder whether any interpretations are
possible) the longer-term better understanding of uncertainty and
application of statistical approaches will be able to better resolve the
uncertainty and give probabilistic information of different interpretations.
```

****

In email **1823.txt** (Sep 18, 2008), Hakan Grudd discusses a recent paper pointing out uncertainties in paleo-temperature constructions with Keith Briffa. The paper that Grudd is commenting on was published by Craig Loehle (a skeptic), and while Grudd duly notes this, he still agrees with Leohle's major conclusion.

```
By the way, what is your opinion of the paper by Craig Loehle recently
published online in Climatic Change? I note that he has published several
papers in Energy & Environment, which I guess makes him some sort of
"sceptic". He has a point though. He puts in print some worries I have had
since first starting off with the tree rings: The non-linear growth response,
which we all know is there but which we do not really account for in making
reconstructions.
     Cheers,
     Håkan
```

****

In email **2657.txt** (Sep 30, 2008), Tim Osborn discusses the presentation of uncertainty of paeleotemperature reconstructions in the IPCC AR4:

```
this way of visualising the published results) as a rather ad-hoc approach
and subject to various lines of attack (e.g. should we really combine
reconstructions that represent rather different things [annual vs. summer,
full NH vs. land], what does it mean if the published uncertainty ranges
overlap from multiple studies if some of those studies have overlapping input
proxy series and others have few overlaps?).
   I don't want to put you off, and our IPCC chapter co-authors didn't seem
put off despite our (Keith and mine) prior expectations that they would.  I
just wanted to make sure that you're clear about the possible criticisms.
```

****

In email **5096.txt** (Oct 1, 2008), Ed Cook discusses the Medieval Warm Period and how it may not be fully expressed in proxy reconstructions.

```
The whole issue of whether or not the MWP was more spatially heterogeneous or
not is a huge "red herring" in my opinion anyway. A growing body of evidence
clearly shows that hydroclimatic variability during the putative MWP (more
appropriately and inclusively called the "Medieval Climate Anomaly" or MCA
period) was more regionally extreme (mainly in terms of the frequency and
duration of megadroughts) than anything we have seen in the 20th century,
except perhaps for the Sahel. So in certain ways the MCA period may have been
more climatically extreme than in modern times. The problem is that we have
been too fixated on temperature, especially hemispheric and global average
temperature, and IPCC is enormously guilty of that. So the fact that evidence
for "warming" in tree-ring records during the putative MWP is not as strong
and spatially homogeneous as one would like might simply be due to the fact
that it was bloody dry too in certain regions, with more spatial variability
imposed on growth due to regional drought variability even if it were truly
as warm as today. The Calvin cycle and evapotranspiration demand surely
prevail here: warm-dry means less tree growth and a reduced expression of
what the true warmth was during the MWP.
```

****

10. Politics in the IPCC AR4

```
date: Thu Jan  8 14:50:24 2009
from: Phil Jones <p.jones@uea.ac.uk>
subject: RE: FW: Temperatures in 2009
to: "Folland, Chris" <chris.folland@metoffice.gov.uk>

    Chris,
       I sent it. He says he'll read the IPCC Chapters! He hadn't
    as he said he thought they were politically biased. I assured
    him they were not. The SPM may be, but not the chapters.
       From other things in his email though, he won't be convinced.
    Cheers
    Phil
```

****

11. Thompson paper on errors in the CRU global temperature record

David Thompson identified a problem with the global temperature record compiled and maintained by the CRU (i.e. Phil Jones' record). The problem was identified as having to do with changing ships' records of sea surface temperatures in several decades following the end of World War II. While the error had little impact on the magnitude of the warming over a century time-scale, it did impact the amount of warming observed over the last 50 years (it lessened it), and it also showed that there were still fairly large data issues inherent in the global temperature analysis—which has been pieced together from, in Jones' words (**email 4040.txt**) "`a measurement system that is not designed to measure climate.`"

As this was a major finding, which was published in *Nature* magazine (and which included Phil Jones as a co-author), there was a lot of effort through the authors, and others (such as the weblog Real Climate) to as much as possible, control the media spin on the paper when it was released. The Climategate 2.0 email document much of this discussion, as well as the discussion of the work as it progressed (that process was not elaborated on in this summary). However, despite that attempt at controlling the media, other scientists realized the underlying implications—that the compiled observed temperature histories were not as reliable as they are often assumed to be.

In email **5017.txt** (June 1, 2008), paleoresearcher Ed Cook commented on the Thompson et al. results to colleague David Frank:

```
Hi Dave,

    Thanks for the paper as well. I heard about the extremely shocking goof
in the instrumental records from Phil Jones in Tahiti. Frankly, I'm amazed
that such a shoddy, amateurish mistake could have been made by the British
Met Office. The skeptics will have a field day with this paper, honestly, as
they should. Maybe the global change community is getting too smug.

    Ed
```

Later in the same email thread (**5017.txt**), Cook continues to be critical of the persons who claim that the problems with the proxy reconstructions lie only with the proxies themselves, when in fact, there is still a lot of uncertainty in the instrumental records to which the proxies are trying to be calibrated. If the instrumental record is inaccurate, the proxy reconstructions must be as well (but this may not be a problem with the underlying proxy data itself).

```
“Hi Dave,

   I just downloaded your powerpoint presentation from your server and looked
at it. Very nice job! It really covers many of the issues regarding proxy
uncertainty and tree rings. It is also really important not to let the
instrumental people off the hook, especially after that debacle just
published on by Thompson et al. in Nature concerning the SST corrections or
lack there of. The recent Eos article by Vecchi likewise shows how much
uncertainty remains in the instrumental SST fields. So it is increasingly
clear to me, as I believe it is to you, that the climate data homogenization
methods used can contribute significantly to the uncertainty in the
reconstructions even when the proxies are typically assigned pretty much all
blame. So while we need to be completely honest about the many large
uncertainties in our tree-ring data and reconstructions, the instrumental
data mob needs to be equally honest and upfront about how they are
contributing significant uncertainty to the reconstructions as well. This is
especially important at the lower frequencies, which makes time-scale
dependent calibration even more difficult to objectively assess.

   Cheers,

   Ed
```

****

12. Miscellaneous

This section highlights some notable comments in the Climategate 2.0 emails that did not readily fit into the categories outline above.

****

From an October 24, 2007 email (**1656.txt**), Climate Research Unit (CRU) scientist Douglas Maraun is putting together a seminar and suggests topics he’d like to see discussed, including

```
“How should we deal with flaws inside the climate community? I think, that

“our reaction on the errors found in Mike Mann's work were not especially

honest.”

date: Wed, 24 Oct 2007 11:05:20 +0100
from: "Douglas Maraun" <d.maraun@uea.ac.uk>
subject: Informal Seminar TODAY
to: cru.internal@uea.ac.uk

Dear colleagues,
```

```
I'd like to invite all of you to todays discussion seminar, 4pm in the
coffee room:

"Climate science and the media"

After the publication of the latest IPCC, the media wrote a vast
number of articles about possible and likely impacts, many of them
greatly exaggerated. The issue seemed to dominate news for a long time
and every company had to consider global warming in its advertisement.
However, much of this sympathy turned out to be either white washing
or political correctness. Furthermore, recently and maybe especially
after the "inconvenient truth" case and the Nobel peace prize going to
Al Gore, many irritated and sceptical comments about so-called
"climatism" appeared also in respectable newspapers.
Against the background of these recent developments, we could discuss
the relation of climate science to the media, the way it is, and the
way it should be.

In my opinion, the question is not so much whether we should at all
deal with the media. Our research is of potential relevance to the
public, so we have to deal with the public. The question is rather how
this should be done. Points I would like to discuss are:

-Is it true that only climate sceptics have political interests and
are potentially biased? If not, how can we deal with this?
-How should we deal with flaws inside the climate community? I think,
that "our" reaction on the errors found in Mike Mann's work were not
especially honest.
-How should we deal with popular science like the Al Gore movie?
-What is the difference between a "climate sceptic" and a "climate denier"?
-What should we do with/against exaggerations of the media?
-How do we avoid sounding religious or arrogant?
-Should we comment on the work/ideas of climate scepitics?

If you have got any further suggestions or do think, my points are not
interesting, please let me know in advance.

See you later,
Douglas
----------------------------------------------------------------------
Dr. Douglas Maraun
Climatic Research Unit, University of East Anglia
+44 1603 59 3857
http://www.cru.uea.ac.uk/~douglas

*******************
```

In email **1253.txt** (Sep 18, 2008) Jones says that he is undecided about his participation in the IPCC AR5 report, but that he may do so to spite the skeptics:

```
date: Fri, 19 Sep 2008 16:46:14 +0200
from: Thomas Stocker <stocker@climate.unibe.ch>
subject: Re: Congratulations!
to: Phil Jones <p.jones@uea.ac.uk>
```

<x-flowed>
Thanks, Phil for your kind email. Yes, for the first time there was a
real election at the IPCC Session. I hope we can count on you in
whatever role in AR5! The skeptics are indeed mounting their pressure.

Best regards,

Thomas

Phil Jones wrote:
>
> Thomas,
> I've been meaning to email you with congratulations, but have been
> too busy with meetings and the start of term. I heard that
> it came down to you or Francis, so the science did win out.
>
> Not decided yet if I want to be involved again. The more the skeptics
> get at me, the more I want to do it again!
>
> Cheers
> Phil

****

In email **0782.txt** (Sep 30, 2009) researcher John Grace write to a long list of people to announce the publication of some new findings relating tree-ring growth to galactic cosmic rays. He does so with some trepidation of reprisal.

>Dear Colleagues
>
>We have found a correlation between tree rings and galactic cosmic
radiation:
>
>http://www3.interscience.wiley.com/journal/122597017/abstract
>
>This is an unexpected result, for which we don't
>yet have a good explanation. I hope doesn't
>result in scientific excommunication!
>
>I thought it would be good idea to circulate
>this reference to relevant scientific friends- so here it is.
>
>We have one other data set for a different
>species, but spanning many more years. We'll be investigating this case.
>
>Best wishes
>
>John Grace

****

In email **0896.txt** (Apr 14, 2009), Jones is having a discussion with a co-author about a paper they are working on together trying to quantify the magnitude of the impact of urban warming on the long-term observed temperature history of China. A different research team has identified more urban warming in China than Jones thinks is there and is looking for a way to counter those results.

```
cc: d.lister@uea.ac.uk
date: Mon Apr 14 13:53:36 2008
from: Phil Jones <p.jones@uea.ac.uk>
subject: Revised paper
to: <liqx@cma.gov.cn>

    Qingxiang,
         Attached is a revised paper and also the file with the responses to
the reviewers that I will send back when we resubmit.
        I have made all the alterations except the final ones that relate to
how we interpret Ren et al versus what was done in the paper.  There is some
tentative text in at the moment on this issue.
   In the revised paper I've marked text in the following way:
    red/orange - I will leave this for the reviewers or the editor to see.
This is in response to all the other questions. yellow highlighted text in
the abstract and conclusions needs to be modified once the text in blue at
the end of section 3.3 is agreed.
      I am still unsure how to interpret Ren et al (2008) and what we should
say.
     I hope you will be sent the series from Figure 3 in Ren et al. over the
next few days.
    In the meantime have a look at what I've written. I think the urban-
related warming should be smaller than this, but I can't think of a good way
to argue this. I am hopeful of finding something in the data that makes by
their Figure 3.
     I think ours should be smaller as we include west China, but as you say
the south should be affected as much as the north.
      There is no rush to read this. I have an extension to resubmission to
May 21.
    I am also away in Vienna the rest of this week after tomorrow. I will be
in Geneva
    all next week.
    Best Regards
    Phil
```

****

In email **1839.txt** (Jan 15, 2008) Jones discusses preparing a response (at the encouragement of the IPCC WGI co-chair Susan Solomon) to all the influential skeptical papers. In this discussion he offers up this peculiar request: "Why can't people just accept that the IPCC is right!!"

```
date: Tue Jan 15 12:45:58 2008
from: Phil Jones <p.jones@uea.ac.uk>
subject: Re: Fwd: Re: Fwd: FYI: Daggers Are Drawn
to: Jean Jouzel <jean.jouzel@lsce.ipsl.fr>
```

Jean,

There are lots of other poor papers appearing at the moment. Susan is encouraging us all to write responses to them. I'm trying to do one, Ben Santer another and maybe David Parker a third. All are wrong, but it just takes time to put something useful together.

Why can't people just accept that the IPCC is right!! In Britain we have people saying that the evidence is accepted - we've won the war, now let's act! I'll see if I can persuade someone to follow up on the Science editorial.

I did talk to the journalist, mostly trying to persuade him not to run with the story.

Cheers

Phil

****