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May 16, 2010

Climategate: A Battlefield Perspective

Annotated Notes for Presentation to Heartland Conference, Chicago

The Climategate Scandal – A Battlefield Perspective

Thank you for the opportunity to speak to you tonight. Climategate has obviously been big news and I've ended up with a pretty unique perspective.

Many issues are raised by the emails, but tonight I'm going to put the most notorious email - the "trick ... to hide the decline" – into context. I'll show how the trick arises from an apparent effort to show rhetorical coherence between the very different Mann and Briffa temperature reconstructions for the Third Assessment Report. I'll use this motif to connect this years later to Jones' equally notorious request to "delete all emails". Then on to Climategate itself and the half-hearted inquiries.

A few caveats. The trick itself, the obstruction of FOI requests, the whitewash inquiries and the seemingly obtuse reaction of the climate science community inevitably place this community in a bad light for audiences like this. Nonetheless, keep in mind that nothing that I'll say tonight disproves global warming. Nor does climate science as a whole stand or fall on proxy reconstructions. If we knew nothing about tree rings, we would still be obliged to assess the impact of doubled CO₂.

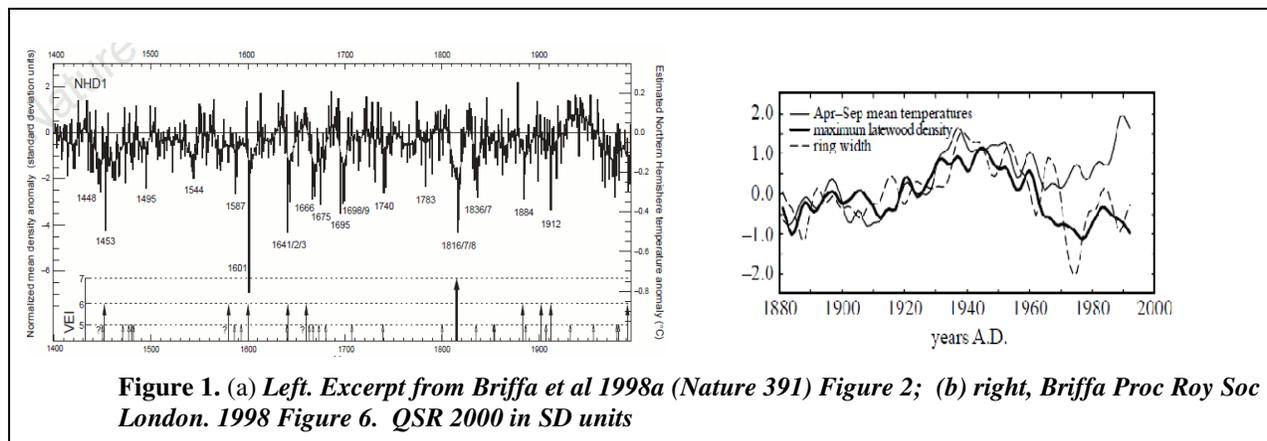
As a final preamble, there is far too much anger on both sides of the debate. People are quick to yell "fraud" at the other side. In my opinion, use of such language is both self-indulgent and counter-productive. I do not apply these sorts of labels myself, do not permit them at Climate Audit and do not believe that they serve any purpose. This doesn't mean that you can't criticize authors – I do so all the time and will do so today. But you should be able to make any point that you want using facts rather than adjectives.

The trick email had its roots in the 1998 Mann and Briffa temperature reconstructions. Both were submitted independently in 1997 within only a few days of one another and published in 1998 within only a couple of months of one another. Both drew on very large tree ring networks, but their later 20th century results were diametrically opposite. Mann's went sharply up, while Briffa's went down. Disguising this inconsistency rather than explaining it led to much of the strange history in this field.

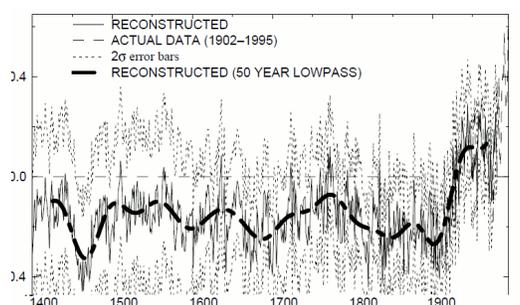
[SLIDE] The Briffa reconstruction was based on densities from an extremely large network collected in the early 1990s by Fritz Schweingruber from over 400 sites in northern Canada, Siberia etc selected beforehand as being temperature-limited due to altitude or latitude. To this day, it remains by far the largest sample of this type. Despite relatively little centennial variability, Briffa's reconstruction had a noticeable decline in the late 20th century, despite warmer temperatures. In these early articles¹, the decline was not hidden.

For most analysts, the seemingly unavoidable question at this point would be – if tree rings didn't respond to late 20th century warmth, how would one know that they didn't do the same thing in response to possible medieval warmth – a question that remains unaddressed years later.

¹ Briffa et al 1998a (Nature 391): During the second half of the twentieth century, the decadal-scale trends in wood density and summer temperatures have increasingly diverged as wood density has progressively fallen. The cause of this increasing insensitivity of wood density to temperature changes is not known, but if it is not taken into account in dendroclimatic reconstructions, past temperatures could be overestimated. See also Briffa et al 1998b (Nature 393) ; Briffa 1998 (QSR).



[SLIDE] The famous Mann reconstruction was published in April 1998², a month before Mann received his PhD. Mann also used a tree ring network of over 400 sites. But instead of limiting the network to temperature-limited sites, Mann included everything, even precipitation limited sites in the US southwest. Mann even included Graybill’s bristlecone pines, which had a pronounced 20th century growth pulse that the authors argued was due to CO₂ fertilization rather than temperature. Instead of using averages like Briffa, Mann used principal components – or rather his own adaptation of the method – a method that enhanced the contribution of bristlecones. In its first muddy version as shown here, it gave little hint of its later iconic status.



Contact between Jones and Mann commenced around this time³. The first letters are polite. In the fall, Jones, Mann, Briffa and Overpeck correspond about the merits of paleoclimate proxies and how to attract attention to the field.

September 1998 brought very different fortunes to Mann and Briffa. Despite his very junior status – only a few months from his PhD - Mann got a big boost by being appointed one of only eight Lead Authors of the important chapter 2 of the forthcoming IPCC Assessment Report⁴. Briffa, on the other hand, despite

² Mann et al 1998 (Nature)

³ 59. 0898099393.txt

⁴ 66. 0906042912.txt

practicing in the field for many years, was facing the bleak prospect of unemployment at the start of the new year⁵:

Mann to Jones, Sep 17 1998 : 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 ... 66. 0906042912.txt

Briffa to Bradley, Sep 18, 1998. Also I must write my application to NERC for a fellowship - if this fails Sarah and I are unemployed after December as things stand. God knows there is little chance of success but the application must be in by the end of September and I have not started it yet. This is a big deal for me and I am putting you down as my primary suggested scientific referee. 68. 0906137836.txt

A few weeks after his IPCC appointment, Mann submitted an extension of his reconstruction back to 1000⁶, including the first recognizable version of the famous hockey stick (see Figure 3.). It was published in February 1999, fortuitous timing since 1998, with its huge El Nino, been exceptionally warm. Mann wasted no time incorporating 1998 temperature into his graphic, and introduced the now familiar phrases that the “1990s were the warmest decade of the millennium, with 1998 the warmest year so far”. Co-author Hughes proclaimed the long-sought demise of the Medieval Warm Period. The findings caused a sensation both in the scientific and popular press. Mann’s newfound prominence enabled him to escape the precarious life of a post-doc, receiving a faculty position at the University of Virginia a couple of months later.

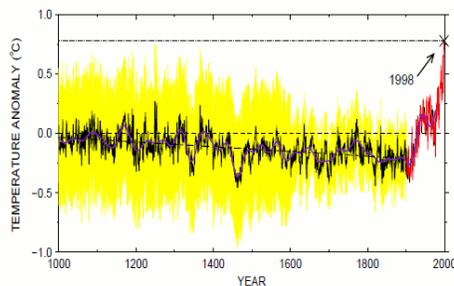


Figure 3. MBH99 Figure 5b

In May 1999, Briffa published the first assessment of Mann’s results⁷, containing what, to my knowledge, is the first spaghetti graph of reconstructions⁸ (see Figure 4). In this graphic, there is a new Briffa version – the one in pale blue – one which coheres much more closely to Mann’s. For the first time, values after 1960 were deleted. In retrospect, this article was the first bite of the poison apple of hide the decline. It seems to originate in an effort to minimize the 20th century discrepancy between the two reconstructions, since Briffa, like Mann, also believed that the 20th century was anomalously warm⁹. Briffa’s assessment also included a few sensible

⁵ 68. 0906137836.txt

⁶ Mann et al 1999 (GRL) together with press release at <http://www.umass.edu/newsoffice/newsreleases/articles/12577.php>

⁷ Briffa and Osborn 1999 (Science).

⁸ This is also the first reconstruction which contained Briffa’s Yamal chronology, later an extremely important individual proxy in spaghetti graphs, including IPCC AR4. The green series combines Yamal with two other later staples (Tornetrask and Taimyr.)

⁹ “On the basis of their analysis, Mann *et al.* conclude that the 20th century is anomalously warm. Even with the very limited data available and the problems associated with interpreting many of them as unambiguous measures of

caveats about reconstructions¹⁰, including the possible dependence of the Mann reconstruction on trees in the dry southwest US – an issue that McKittrick and I analysed in greater detail.

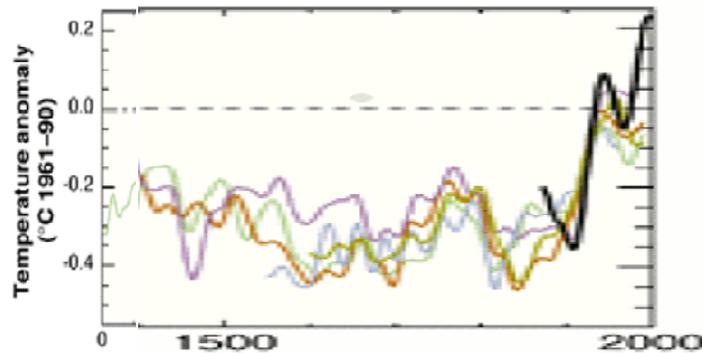


Figure 4. Briffa and Osborn 1999 (Science) Figure 1.

Prior to publication, Briffa had sent a copy of his short article to Mann for comment. Mann is now an entirely different figure from the year before. Mann made peremptory demands¹¹ that Briffa withdraw even very slight criticism. Presaging later conduct, Mann sent a demand to the editor of Science, saying that it was "Better that nothing appear, than something unacceptable to us"¹². Mann's supervisor, Raymond Bradley, immediately disassociated himself from these demands, which he described as "amazingly arrogant"¹³. Mann then tried to paper over the dispute with a non-apology to Jones and Briffa. Bradley's private comment: "excuse me while I puke"¹⁴.

Mann to Sciencemag Apr 18, 1999 : "Better that nothing appear, than something unacceptable to us" (99.0924532891.txt)

Bradley to Sciencemag Apr 18, 1999 : I would like to diasassociate myself from Mike Mann's view that "xxxxxxxxxxxx" and that they "xxxxxxxxxxxx". I find this notion quite absurd. ...As for thinking that it is "Better that nothing appear, than something unacceptable to us"as though we are the gatekeepers of all that is acceptable in the world of paleoclimatology seems amazingly arrogant. to a Science editor. (99.0924532891.txt)

hemispheric temperature change, this conclusion must surely be accepted." Briffa had previously expressed a similar view in Briffa et al 1995 (Nature) discussing the Polar Urals.

¹⁰ "Mann *et al.* state that one particular candidate predictor in their regression, the amplitude series relating to the first principal component of a group of high-elevation tree-ring chronologies in the western United States, is essential before A.D. 1400 for a verifiable NH reconstruction. Unfortunately, these trees display a progressive increase in growth from the middle of the 19th century, which may be wholly or partly due to rising atmospheric CO₂ levels. How can we distinguish the growth-promoting effects of warm temperatures from the possible influence of increasing CO₂ and perhaps even other anthropogenic growth enhancers such as nitrogenous pollution? All show positive trends over the 20th century, and each has the potential to increase tree growth alone or in combination with others (regardless of whether that growth is limited by moisture availability or temperature)".

¹¹ 96.0924030302.txt

¹² Referred to in Bradley email of Apr 18, 1999 to a Science editor. See 99.0924532891.txt

¹³ 99.0924532891.txt

¹⁴ 111.0926681134.txt

Mann to Sciencemag, Bradley, Jones, Briffa May 12, 1999: Thanks all for the hard work and a job well done. I like to think that may feedback helped here--so I take some pride here as well. (111. 0926681134.txt)

Bradley to Briffa and Jones May 12, 1999: Excuse me while I puke. . (111. 0926681134.txt)

Meanwhile, Mann's section of the Zero Draft of the Third Assessment Report included a new spaghetti graph giving pride of place to his own reconstruction. It had yet another Briffa version, one with relatively little variation, the characteristic 20th century decline and an unexceptional closing value.

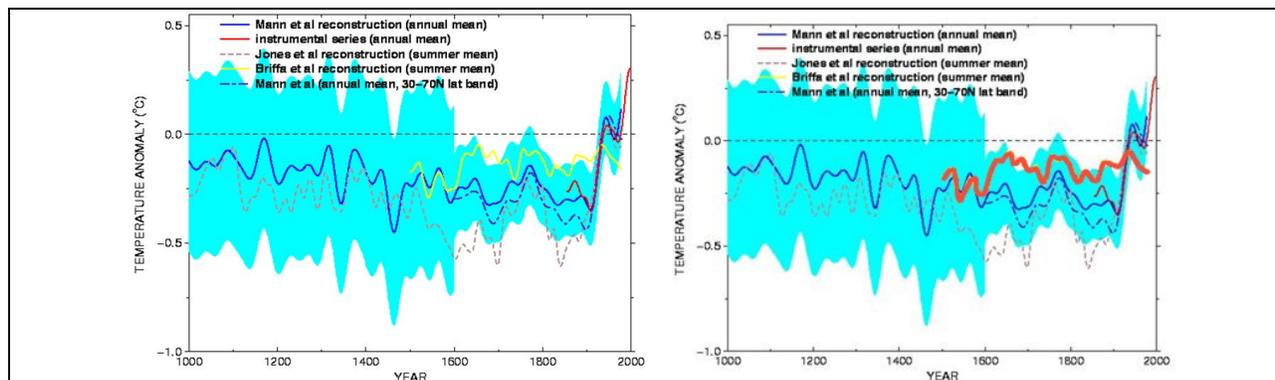


Figure 5. IPCC Zero Draft Figure 2.2.3a. Comparison of millennial Northern Hemisphere (NH) temperature reconstructions from different investigators (Briffa et al, 1998; Jones et al, 1998; Mann et al, 1998;1999a)... All the series were filtered with a 40 year Gaussian filter. **Briffa reconstruction is in yellow (left – original); re-colored for emphasis on right.**

This spaghetti graph seems not to have been well-received by senior IPCC figures at the IPCC Lead Authors meeting in Arusha, Tanzania¹⁵ in September 1999. In a post-mortem a few weeks later, Coordinating Lead Author Folland wrote that, although a proxy diagram was “a clear favourite for the Policy Makers summary”, the Briffa reconstruction “dilutes the message rather significantly”, adding that this was “probably the most important issue to resolve in Chapter 2 at present”. Mann wrote that “everyone in the room” agreed that the Briffa series was a “potential distraction/detraction from the reasonably consensus viewpoint we’d like to show”. Briffa recognized there was “pressure to present a nice tidy story as regards ‘apparent unprecedented warming in a thousand years or more’”, but expressed many caveats, in particular that the proxies were not responding the way that they were supposed to and that that the recent warmth was “probably matched” 1000 years ago.

Folland: 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 [i.e. the Briffa reconstruction] 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..(Folland, Sep 22, 1999, in 0938031546.txt)

Mann: Keith'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

¹⁵ see [Houghton](#), 929985154.txt and 0938018124.txt)

problem and a potential distraction/detraction from the reasonably consensus viewpoint we'd like to show w/ the Jones et al and Mann et al series. (Sep 22, 1999, 0938018124.txt)

Briffa: 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... [There are] 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. (Briffa, Sep 22, 1999, 0938031546.txt)

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. (Briffa, Sep 22, 1999, 0938031546.txt)

Mann worried that skeptics would have a “field day” if the Briffa reconstruction were shown, as this might undermine “faith in the paleoestimates”. Mann was convinced that doubt was not “scientifically justified” and didn't want to give “fodder” to the “sceptics”

Mann: 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! (Mann Sep 22, 0938018124.txt)

Matters settled down quickly, with Briffa apologizing to Mann for his temporary pangs of conscience. A couple of weeks later, Osborn (on behalf of Briffa) sent Mann a revised reconstruction¹⁶, one with more “low-frequency” variability but with the characteristic decline¹⁷. In the graphic here, I've shown the different rhetorical effect of including the deleted data.

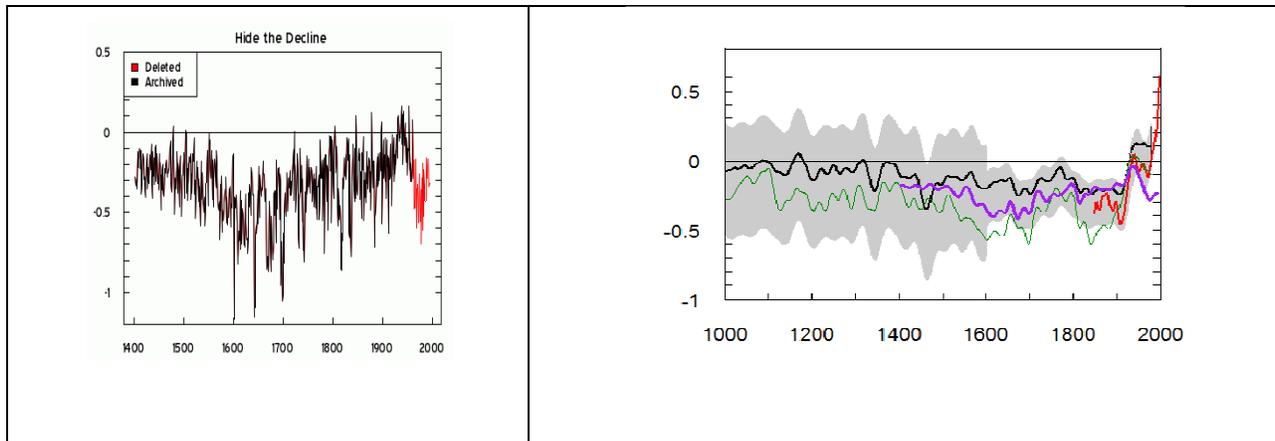


Figure 6. Left- Briffa reconstruction from Oct 5, 1999 Climategate email. Matches Briffa et al 2001 Plate 3 version up to 1960. Post-1960 values deleted in 2001 version shown in red; right – emulation of IPCC AR3 figure without trick. Briffa shown here in purple for emphasis.

¹⁶ Osborn, Oct 5, 1999, 0939154709.txt

¹⁷ A version identical up to 1960 (with post-1960 values deleted in the archived version at NCDC) was published a couple of years later (Briffa et al 2001(JGR)).

However, Mann’s spaghetti graph in the First Reviewer Draft a few weeks later¹⁸ (see Figure 8) had no inconvenient 20th century decline. It used what Jones later called Mike’s trick. While climate scientists later described the trick as “sophisticated”, its main element was very coarse – adverse data after 1960 was simply deleted. A second element of the trick was a little more subtle. Any smoothed series requires forward values to calculate the smooth. It appears that Mann substituted instrumental data for actual data after 1960 to calculate the smooth before truncating the smooth in 1960. This pulled up the end values of the smoothed series, further disguising the decline. The truncation was not reported and is not readily noticed in the tangle of spaghetti strands.

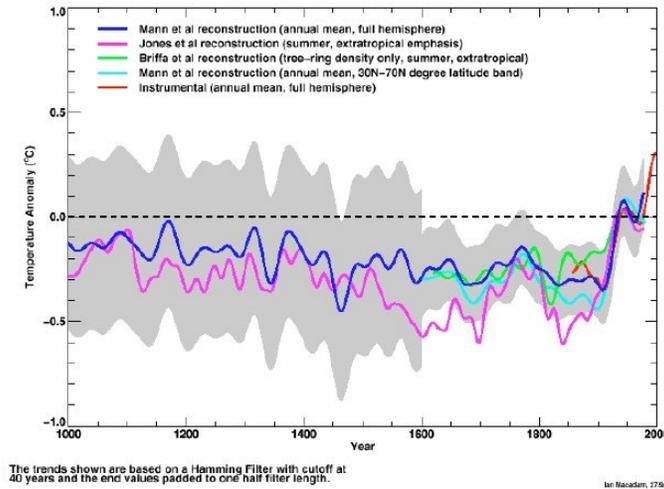
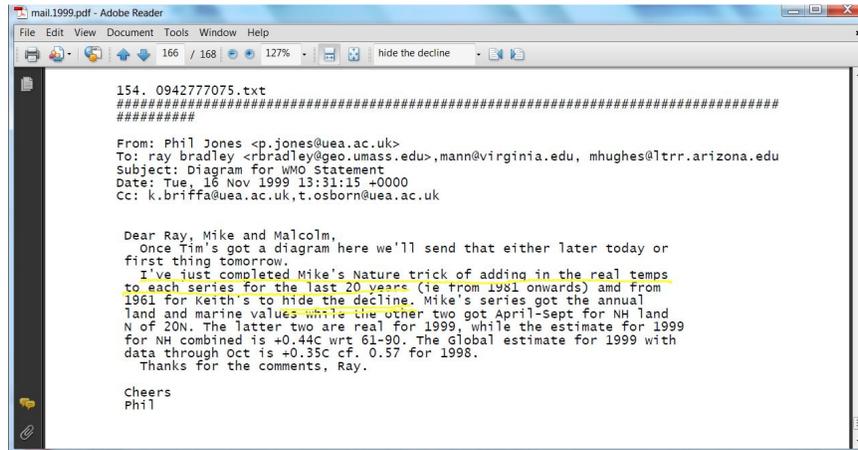


Figure 7. IPCC AR3 Figure 2.24. Green - Briffa reconstruction.

The notorious “trick” email¹⁹ came a couple of weeks later.



Jones did not entirely understand “Mike’s Nature trick” as used in the IPCC draft. Rather than then truncating the Briffa series in 1960 as Mann had done, Jones spliced the instrumental record onto the proxy record, a method that Mann later condemned at realclimate, asserting that “no researchers in this field have ever, to our knowledge, “grafted the thermometer record onto”

¹⁸ Oct 27, 1999
¹⁹ 154. 0942777075.txt

any reconstruction”, saying that this “specious claim” “usually originat[ed] from industry-funded climate disinformation websites”²⁰.

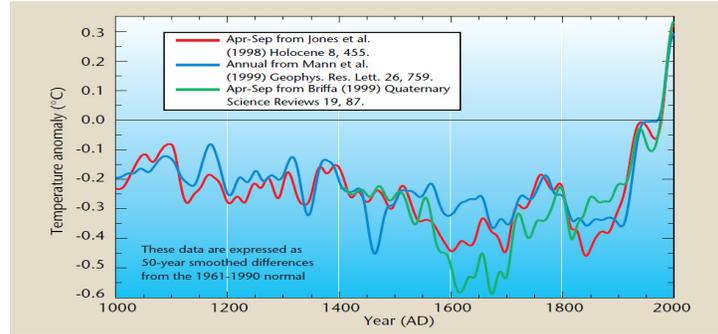


Figure 8. From WMO 1999 Annual Report.

The 1999 WMO Report was not widely cited or relied on. However, the Third Assessment Report obviously was and the Hockey Stick was more or less its logo – appearing no fewer than seven times in different guises²¹. It was even the backdrop for Houghton’s press conference in Paris unveiling the report.

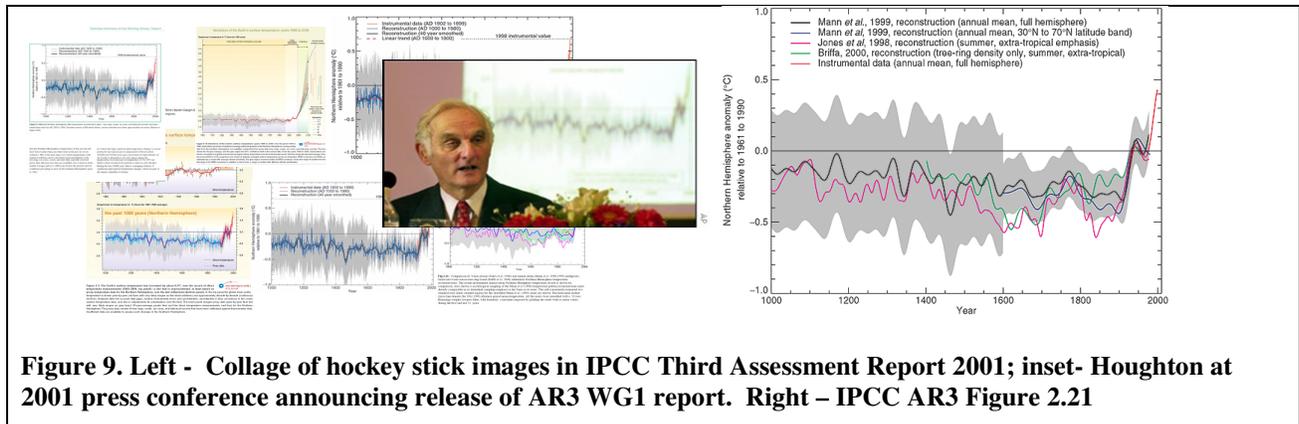


Figure 9. Left - Collage of hockey stick images in IPCC Third Assessment Report 2001; inset- Houghton at 2001 press conference announcing release of AR3 WG1 report. Right – IPCC AR3 Figure 2.21

After IPCC, the deletion of the decline became standard practice in spaghetti graphs. Briffa’s presentations in journal articles are a bit schizophrenic as he usually showed the decline in a

²⁰ <http://www.realclimate.org/index.php/archives/2004/12/myths-vs-fact-regarding-the-hockey-stick>

²¹ The Third Assessment Report did not disclose the deletion of post-1960 in the graphic. The running text coyly stated: “Furthermore, the biological response to climate forcing may change over time. There is evidence, for example, that high latitude tree-ring density variations have changed in their response to temperature in recent decades, associated with possible nonclimatic factors (Briffa *et al.*, 1998a). By contrast, Vaganov *et al.* (1999) have presented evidence that such changes may actually be climatic and result from the effects of increasing winter precipitation on the starting date of the growing season (see Section 2.7.2.2).” Defenders have argued recently (unsuccessfully in my opinion) that this was an acceptable alternative to plainly disclosing and explaining the data deletion.

standalone graphic, but then deleted it in the spaghetti version – which ends in 1960 here²². In this presentation, Briffa did not use Mann’s Nature trick and the dangling end is more noticeable.

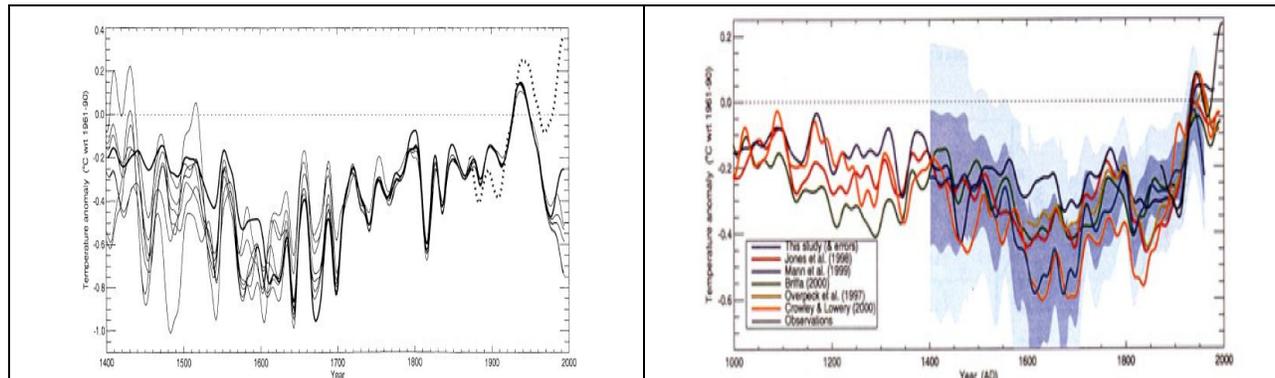


Figure 10. Left – Briffa et al 2001 (JGR) Figure 4; right- Briffa et al 2001 (JGR) Plate 3 – with post-1960 values deleted. In this smooth, post-1960 instrumental values are not used to align the smooth and the dangling end is more visible than the IPCC version.

Briffa and others made labored attempts to rationalize the situation. In 2002, Briffa made the remarkable statement that “in the absence of a substantiated explanation for the decline, we make the assumption that it is likely to be a response to some kind of recent anthropogenic forcing. On the basis of this assumption, the pre-twentieth century part of the reconstructions can be considered to be free from similar events and thus accurately represent past temperature variability.” Mann’s coauthor Hughes had previously drawn an even more remarkable conclusion - he said that the divergence problem proved that reconstructions were “even more reliable than previously thought”²³.

Briffa et al 2002 (Holocene): In the absence of a substantiated explanation for the decline, we make the assumption that it is likely to be a response to some kind of recent anthropogenic forcing. On the basis of this assumption, the pre-twentieth century part of the reconstructions can be considered to be free from similar events and thus accurately represent past temperature variability.

Hughes, July 1999. The recent marked weakening in the correlation between tree growth and temperature means that past climate reconstructions are even more reliable than previously thought, said Malcolm Hughes. ... “The recent weaker correlation between tree growth and temperature clearly affects the reliability of our reconstructions of the past. Actually, it means past climate reconstructions (before the 1960s) are better than we thought they were. And, as a result of this, it means that we underestimated the differences between the present century and past centuries,” Hughes said.

These quotations were partly responsible for my present involvement in climate science. In 2003, I had been casually wondering how they “knew” that 1998 was the warmest year of the millennium. When I read the above statement in April 2003, I could hardly believe that this sort of thing passed as science. So I emailed Mann for the underlying data in the Mann reconstruction, then the big dog in the IPCC world, the first step on a long road.

Dear Dr. Mann, I have been studying MBH98 and 99. I located datasets for the 13 series used in MBH99 ... and was interested in locating similar information on the 112 proxies referred to in MBH98 ... Thank you for your attention. Yours truly, Stephen McIntyre, Toronto, Canada

²² Briffa et al 2002 (Holocene)

²³ <http://www.sciencedaily.com/releases/1999/07/990707181851.htm> on the release of Vaganov et al 1999 (Nature).

Dear Mr. McIntyre, These data are available on an anonymous ftp site we have set up. **I've forgotten the exact location**, but I've asked my Colleague Dr. Scott Rutherford if he can provide you with that information. best regards, Mike Mann

Steve, The proxies aren't actually all in one ftp site (at least not to my knowledge). I can get them together if you give me a few days. Do you want the raw 300+ proxies or the 112 that were used in the MBH98 reconstruction? Scott [Rutherford]

Flash forward two years. In May 2005, in the early days of Climate Audit, I read or re-read the early Briffa articles – the ones that didn't hide the decline – and idly wondered why there was no corresponding decline in IPCC. I blew up the IPCC spaghetti graph (see Figure 11) and noticed that the truncation of the Briffa reconstruction – a truncation almost impossible to notice in the unzoomed version. I re-examined the IPCC text verifying that it didn't disclose the truncation anywhere. Climate Audit had a smaller audience then than now and the post²⁴ attracted only a few comments.

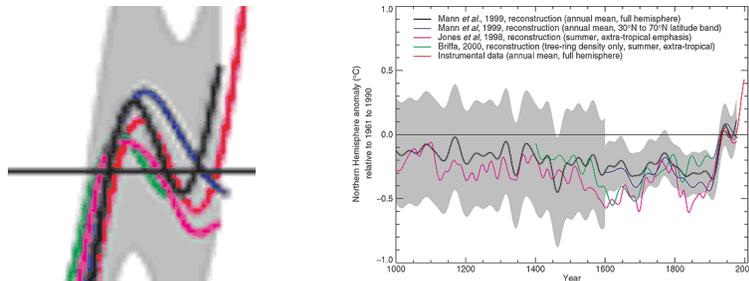


Figure 11. IPCC AR3 Figure 2-21 with Briffa reconstruction in green. Blow-up at left.

In 2006, as many of you know, two blue ribbon panels looked at the controversy. Neither examined conduct issues; when Mann implausibly told the NAS panel that he didn't calculate the verification r^2 statistic as that would be a “foolish and incorrect thing to do”, the NAS panel sat there like bumps on a log.

However, there were some interesting moments. Rosanne D'Arrigo astonished the NAS panel with a slide entitled Cherry picking, in which she attempted to defend reconstructions from criticism of biased proxy selection. D'Arrigo observed: you have to pick cherries if you want to make cherry pie²⁵. She also presented a figure from her reconstruction which had a late 20th century discrepancy between proxies and temperature - though not as striking as the Briffa network. Panelist Kurt Cuffey asked her about it; D'Arrigo answered “Oh that's the “Divergence Problem”. Cuffey reasonably wanted to know how you could rely on proxies to register possible past warm periods if they weren't picking up modern warmth. D'Arrigo could only say that the matter was being studied.

²⁴ <http://climateaudit.org/2005/05/01/a-strange-truncation-of-the-briffa-mxd-series/>. “Post-1960 values of the Briffa MXD series are deleted from the IPCC TAR multiproxy spaghetti graph. These values trend downward in the original citation (Briffa [2000], see Figure 5), where post-1960 values are shown. The effect of deleting the post-1960 values of the Briffa MXD series is to make the reconstructions more "similar". The truncation is not documented in IPCC TAR. In most cases, people would ask: who at IPCC truncated this series? why did they do so? who approved the truncation? what process was involved in approving the truncation?”

²⁵ <http://climateaudit.org/2006/03/07/darrigo-making-cherry-pie/>

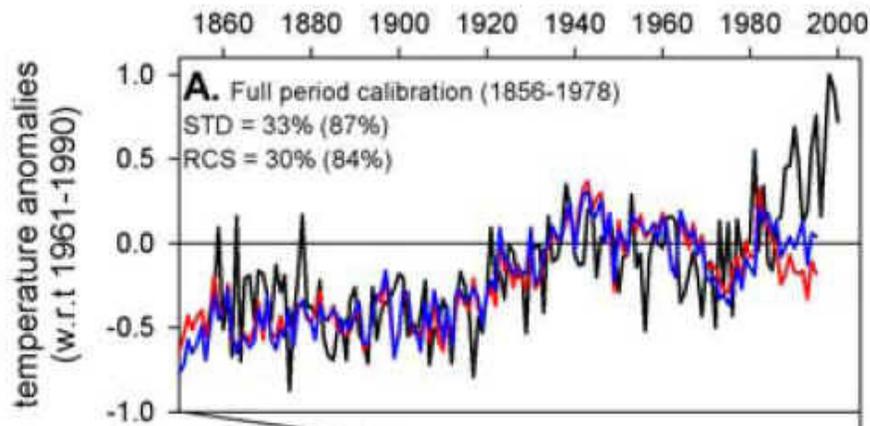


Figure 12. D'Arrigo et al Figure 5 (top).

Richard Alley, of Mann's soon-to-be employer Penn State, was in attendance, saw the worried faces of the NAS panellists and sent an urgent warning to key IPCC authors, including Overpeck and Briffa, that the NAS panel might come down hard on the divergence problem. Overpeck had already heard about "D'Arrigo's splash" and hoped that Briffa had good answers. Briffa and Ed Cook were urgently requested to write the NAS panel.

Alley, March 8, 2006. if the NRC[NAS] committee comes out as being strongly negative on the hockey stick owing to [D'Arrigo's] talk, then the divergence between IPCC and NRC will be a big deal in the future regardless. The NRC committee is accepting comments now (I don't know for how long)... As I noted, my observations of the NRC committee members suggest rather strongly to me that they now have serious doubts about tree-rings as paleothermometers (and I do, too...at least until someone shows me why this divergence problem really doesn't matter) 668. 1141849134.txt

Overpeck, March 8, 2006. I'm hearing about D'Arrigo's splash from other sources (Richard Alley) - hope Keith et al., have good counter arguments. 669. 1141930111.txt

Briffa's answer²⁶ probably didn't reassure the panel very much. He acknowledged that the issue needed work, but pointed out that he was "very busy with teaching and IPCC commitments" and lacked funds for a research assistant to study the topic. He explained that, as Lead Author of the relevant IPCC section, he had decided not to "overplay" the issue because of its "subtlety" and that the problem remained insufficiently studied to be of much concern.

the issue [the Divergence Problem] needs more work, this is only an opinion, and until there is peer-reviewed and published evidence as to the degree of methodological uncertainty, it is not appropriate to criticize this [D'Arrigo's] or other work. For my part, I have been very busy, lately with teaching and IPCC commitments, but we will do some work on this now, though again lack of funds to support a research assistant do not help.

It was my call not to "overplay" the importance of the divergence issue, knowing the subtlety of the issues, in the forthcoming IPCC Chapter 6 draft... This and the divergence problem are not well defined, sufficiently studied, or quantified to be worthy of too much concern at this point...

²⁶ 678. 1143661010.txt

Cook acknowledged²⁷ that it wasn't "surprising" that panellists might come to doubt the value of tree- reconstructions, but called the doubt "somewhat alarming". He conceded that doubt would be justified if the divergence were due to 20th century environmental conditions. However, he argued that the divergence was due to some still unknown anthropogenic cause. Cook conceded that the lack of a known cause was "unfortunate", but still insufficient grounds to question tree ring temperature reconstructions.

Cook, March 15, 2006 to NAS Panel: Perhaps not surprisingly, but also somewhat alarmingly, it is my understanding that some NRC committee members and other influential participants have come to the conclusion that the observed 20th century "divergence" calls into serious question the value of the tree-ring reconstructions of temperatures over the past millennium. The implicit assumption apparently being made is that the "divergence" being caused by environmental conditions in the 20th century could have also prevailed back during times like the Medieval Warm Period (MWP) some 800-1000 years in the past. If this were the case, then the concern raised by some at the workshop would be justified....

The lack of any known cause is unfortunate, but this would be true regardless of how the importance of "divergence" is interpreted. I am not aware of ANY evidence that demonstrates the occurrence of large-scale "divergence" between tree growth and climate prior to the 20th century. Indeed, the available evidence indicates just the opposite. .. In my opinion it is therefore unjustified to call into question the use of tree rings for reconstructing temperatures over the past millennium based on a naive and inappropriate extrapolation of the growth "divergence" problem into the past when it appears to be unique to the 20th century. The NRC committee members must consider this in their report if it is to have the necessary scientific credibility that is expected of it. 675. 1142469228.txt

As in other parts of its report, the 2006 NAS panel made stronger statements in its text than in the overall spin. They conceded that the Divergence Problem 'reduced confidence' in the correlation between ring widths and temperatures, but refrained from clearly stating the unavoidable corollary that this called the tree-ring reconstructions into question.

The NAS panel²⁸, as in other parts of its report, made stronger statements in details than the overall spin. They concluded that the Divergence Problem 'reduced confidence' that in the correlation between ring widths and temperatures, but refrained from clearly stating the unavoidable corollary that this called into question the validity of conclusions drawn from tree-ring based temperature reconstructions.

The observed discrepancy between some tree ring variables that are thought to be sensitive to temperature and the temperature changes observed in the late 20th century (Jacoby and D'Arrigo 1995, Briffa et al. 1998) reduces confidence that the correlation between these proxies and temperature has been consistent over time. Future work is needed to understand the cause of this "divergence," which for now is considered unique to the 20th century and to areas north of 55°N (Cook et al. 2004).

In a seminar at Texas A&M after the NAS panel, its Chairman, Gerald North, explained how these academic panels carried out their business: he said that they "didn't do any research", that they just "took a look at papers", they got 12 "people around the table" and "we just kind of

²⁷ 675. 1142469228.txt. See Cook's exposition of this line of reasoning in Cook et al 2004 (QSR) and my criticism <http://climateaudit.org/2006/03/14/cook-et-al2004-more-cargo-cult/>

²⁸ National Research Council, 2006. Surface Temperature Reconstructions for the Last 2,000 Years. Preprint at www.climateaudit.info/pdf/news/nas_2006/surface_reconstructions.pdf

winged it. That's what you do in that kind of expert panel". (Audio clip is at <http://www.climateaudit.info/pdf/news/NorthAudio30sClip.wma>²⁹)

Briffa definitely didn't "overlay" the divergence problem in the draft IPCC report. He didn't even mention it. Post-1960 values of his reconstruction were once again deleted. However, this time it did not pass unnoticed as I was an IPCC reviewer and made a sharply worded request that the decline be shown and explained as best they could^{30,31}.

Show the Briffa et al reconstruction through to its end; don't stop in 1960. Then comment and deal with the "divergence problem" if you need to. Don't cover up the divergence by truncating this graphic. This was done in IPCC TAR; this was misleading. (Reviewer's comment ID #: 309-18)]

This and other Review Comments occasioned an important exchange in the summer of 2006. IPCC rules, which Overpeck said were "very strict" in this regard³², required reviewers to be registered and approved by IPCC. By June 2006, the review period was over. Nonetheless, Briffa permitted Eugene Wahl, a coauthor of the Team response to our articles and not even a registered reviewer, to insert changes to responses to Review Comments and even to IPCC text. The exchange was very surreptitious³³ – both Briffa and Wahl emphasizing the need for secrecy. This correspondence was later the subject of the notorious "delete all emails" exchange³⁴.

In May 2007, the Fourth Assessment Report were published. Once again, the decline was not shown. However, after 10 years and no mention in either draft, they conceded in the running text that the divergence problem could "limit the possibility of reconstructing past warmth" – a disclaimer that hardly compensated for not showing the discrepancy in the spaghetti graph.³⁵

Several analyses of ring width and ring density chronologies, with otherwise well established sensitivity to temperature, have shown that they do not emulate the general warming trend evident in instrumental temperature records over recent decades, although they do track the warming that occurred during the early part of the 20th century and they continue to maintain a good correlation with observed temperatures over the full instrumental period at the interannual time scale (Briffa et al., 2004; D'Arrigo, 2006). This 'divergence' is apparently restricted to some northern, high latitude regions, but it is certainly not ubiquitous even there. In their large-scale reconstructions based on tree ring density data, Briffa et al. (2001) specifically excluded the post-1960 data in their calibration against instrumental records, to avoid biasing the estimation of the earlier reconstructions (hence they are not shown in Figure 6.10), implicitly assuming that the 'divergence' was a uniquely recent phenomenon, as has also been argued by Cook et al. (2004a).

Others, however, argue for a breakdown in the assumed linear tree growth response to continued warming, invoking a possible threshold exceedance beyond which moisture stress now limits further growth (D'Arrigo et al., 2004). If true, this would imply a similar limit on the potential to reconstruct possible warm periods in earlier times at such sites. At this time there is no consensus on these issues (for further references see NRC, 2006) and the possibility of investigating them further is restricted by the lack of recent tree ring data at most of the sites from which tree ring data discussed in this chapter were acquired.

²⁹ See <http://climateaudit.org/2006/09/05/gerry-north-presentation-on-nas-report/>. Original was downloaded from <http://www.met.tamu.edu/people/faculty/dessler/NorthH264.mp4> (subsequently deleted).

³⁰ <http://climateaudit.org/2009/11/26/ipcc-reviewer-show-the-decline/>; <http://climateaudit.org/2007/06/26/ipcc-and-the-briffa-deletions/>

³¹ <http://climateaudit.org/2006/02/19/536/>

³² 687. 1147982305.txt

³³ See especially 716. 1153470204.txt and 733. 1155402164.txt

³⁴ 891. 1212063122.txt

³⁵ The attentiveness of IPCC authors to the use of running text to avoid showing something in a graphic is evidenced for example in 709. 1153233036.txt.

In June 2007, after a Climate Audit campaign, IPCC placed the Review Comments and Author Responses online in June 2007, reversing their previous policy of limiting access to physical inspection at Harvard library³⁶. We then got to see how authors had justified not showing the decline – they said that it would be “inappropriate”. Nothing more. No explanation why it was “inappropriate” to show the data.

Show the Briffa et al reconstruction through to its end; don't stop in 1960. Then comment and deal with the “divergence problem” if you need to. Don't cover up the divergence by truncating this graphic. This was done in IPCC TAR; this was misleading. (Reviewer's comment ID #: 309-18)]

Rejected — though note divergence' issue will be discussed, still considered inappropriate to show recent section of Briffa et al. series.

In late 2007, Climate Audit reader David Holland noticed that IPCC procedures assigned its Review Editors the responsibility of ensuring that Review Comments were “afforded appropriate consideration” and that “genuine controversies are reflected adequately in the text.” Holland wondered how Chapter 6 Review Editor John Mitchell (of the UK Met Office) had discharged this obligation in respect to Briffa's refusal to show the decline and other unresponsive answers to Review Comments. Holland attempted to obtain Mitchell's comments first from IPCC, then from Mitchell and ultimately through an FOI request to the UK Met Office.

At first, Mitchell and the Met Office implausibly said that Mitchell's correspondence had been deleted. When asked to search backups, the Met Office changed its story, now making the equally implausible claim that Mitchell had attended IPCC meetings in a “personal” capacity and not in connection with his employment. When asked about expenses and salary, they changed their story again, now claiming that production of the Mitchell emails would prejudice relations between the UK and an international organization (the IPCC). For its part, despite its supposed commitment to openness and transparency, the IPCC secretariat refused to waive secrecy.

For my own part, I have not kept any working papers. There is no requirement to do so, given the extensive documentation already available from IPCC. (Feb 2008)

Any records and correspondence had already been deleted and the information is not held by the Met Office. (June 2, 2008)³⁷

I incorrectly stated that the Met Office held the information you seek and I apologise for this. Dr Mitchell acted as Reviewer of Chapter V1 of the Working Group 1 report in a personal capacity and not on behalf of

³⁶ In May 2007, after some months of trying to get access to the IPCC Review Comments – which were supposed to be made available – IPCC said that the Review Comments would be made available at Harvard library for personal inspection during restricted hours, with a copyright limit of 100 pages of photocopying. They refused to provide the Review Comments in searchable digital form; they refused. This led to one of my first FOI requests. Susan Solomon, Chairman of WG1, had used her NOAA email throughout. I asked NOAA for any digital versions of IPCC Review Comments that were in their possession. NOAA astonishingly claimed that they were not in possession of **any** IPCC documents. A few other Climate Audit readers submitted FOI requests as well. Within a few weeks, IPCC reversed course and placed the Review Comments and Author Responses online in June 2007. Although NOAA's excuse had been obviously untrue, the matter was now moot. See contemporary discussion at Climate Audit.

³⁷ <http://climateaudit.org/2008/06/20/fortress-met-office/>

the Met Office which means that none of the information actually falls within the scope of the Act...The information I sent to you was from Dr Mitchell's personal records which I was unaware of at the time. There is no requirement under the legislation to search back up data and as stated above, this information is personal to Dr Mitchell.(June 23, 2008)³⁸

This information is withheld in accordance with the following exemptions pursuant to FOIA: section 27(1)(b) FOIA – information likely to prejudice relations between the United Kingdom and any international organisation. (July 22, 2008)

The Met Office has liaised with the IPCC Secretariat and has been instructed that the Met Office is not to allow any emails to be released without discussion with and agreement from ALL participants concerned. The Met Office has learned that the majority of IPCC participants have stated categorically that they believed and still believe that these emails and correspondence are private and confidential because they were not, and were not meant to be, processed in accordance with the formal IPCC process.(Aug 19, 2008)

Holland also sent a freedom of information request to CRU for any IPCC review correspondence that had been kept out of the IPCC archive. Briffa's 2006 correspondence with Eugene Wahl fell in this category. Two days after Holland's request, Jones asked Wahl, Briffa, Mann and Ammann to delete any AR4 emails. Jones also emailed Briffa and FOI officer Palmer, saying that Briffa should tell Palmer that he didn't get "any additional comments other than those supplied by IPCC" – a claim that is obviously contradicted by the Wahl correspondence.

Jones to Mann, May 29, 2008: Mike, Can you delete any emails you may have had with Keith re AR4? Keith will do likewise. ...Can you also email Gene and get him to do the same? I don't have his new email address. We will be getting Caspar to do likewise. ...Cheers Phil 891. 1212063122.txt

Mann to Jones, May 29, 2008: Hi Phil, ... I'll contact Gene about this ASAP. His new email is: xxx@xxx. talk to you later, mike 891. 1212063122.txt

Jones to Palmer, May 28, 2008: ... **Keith (or you Dave) could say that for (1) Keith didn't get any additional comments in the drafts other than those supplied by IPCC.** On (2) Keith should say that he didn't get any papers through the IPCC process.either. I was doing a different chapter from Keith and I didn't get any. What we did get were papers sent to us directly – so not through IPCC, asking us to refer to them in the IPCC chapters. If only Holland knew how the process really worked!! Every faculty member in ENV and all the post docs and most PhDs do, but seemingly not Holland. So the answers to both (1) and (2) should be directed to IPCC, but **Keith should say that he didn't get anything extra that wasn't in the IPCC comments.** 888. 1212009215.txt

By 2009, these efforts were effectively stalemated. Then came Climategate. The "trick" email was spotted early on by Steve Mosher and was one of the first emails posted up on November 19. realclimate responded³⁹ the next day saying that the emails showed "people working constructively to improve joint publications", with the "trick" merely science-speak for a "good way to deal with a problem". For the most part, climate scientists seem to have adopted realclimate's viewpoint.

People working constructively to improve joint publications; scientists who are friendly and agree on many of the big picture issues, disagreeing at times about details and engaging in 'robust' discussions; Scientists expressing frustration at the misrepresentation of their work in politicized arenas and complaining when

³⁸ <http://climateaudit.org/2008/06/23/fortress-met-office-continued/>

³⁹ <http://www.realclimate.org/index.php/archives/2009/11/the-cru-hack/>

media reports get it wrong; Scientists resenting the time they have to take out of their research to deal with over-hyped nonsense. None of this should be shocking...

Scientists often use the term “trick” to refer to a “a good way to deal with a problem”, rather than something that is “secret”, and so there is nothing problematic in this at all. As for the ‘decline’, it is well known that Keith Briffa’s maximum latewood tree ring density proxy diverges from the temperature records after 1960 (this is more commonly known as the “divergence problem”—see e.g. the recent discussion in this paper) and has been discussed in the literature since Briffa et al in *Nature* in 1998 (*Nature*, 391, 678-682). Those authors have always recommend not using the post 1960 part of their reconstruction, and so while ‘hiding’ is probably a poor choice of words (since it is ‘hidden’ in plain sight), not using the data in the plot is completely appropriate, as is further research to understand why this happens.

However, the trick spread like wildfire into the media, even prompting its own video – Hide the Decline. Out of the early coverage, I thought that Jon Stewart’s take in the Daily Show was particularly acute. (The Stewart video is online at <http://texan2driver.wordpress.com/2009/12/04/jon-stewart-talks-climate-gate-while-the-rest-of-the-media-sleeps/> .)

After quoting from the famous trick email, Stewart:

It means nothing...He’s just using a trick to... hide the decline. It’s just scientist-speak for [speaking quickly] using a standard statistical technique for calibrating data in order to trick you...into not knowing aboutthe decline.

To date, four inquiries have been set up to inquire into the affair.

The first to report was Penn State. Penn State lived up to Stewart’s satire, stating that the trick was “nothing more than a statistical method used to bring two or more different kinds of data sets together in a legitimate fashion by a technique that has been reviewed by a broad array of peers in the field”... in order to trick you.... into not knowing about.... the decline.

The deletion of adverse data is obviously not an legitimate statistical method. They did not interview any critics or targets, take any submissions or provide statistical references. Despite the mute testimony of the emails themselves, Penn State decided that there wasn’t enough prima facie evidence to proceed to an investigation on most issues – even ones where the UK Information Commissioner had said that it was hard to imagine “more cogent prima facie evidence”.

Inquiry, Jan 2010: [The trick] is explained as a discussion among Dr. Jones and others including Dr. Mann about how best to put together a graph for a World Meteorological Organization (WMO) report. They were not falsifying data; they were trying to construct an understandable graph for those who were not experts in the field. The so-called “trick” was nothing more than a statistical method used to bring two or more different kinds of data sets together in a legitimate fashion by a technique that has been reviewed by a broad array of peers in the field. ... there is no basis for further examination of this allegation in the context of an investigation in the second phase of RA-10.

Mann, realclimate, Dec 2004⁴⁰: No researchers in this field have ever, to our knowledge, “grafted the thermometer record onto” any reconstruction. It is somewhat disappointing to find this specious claim (which we usually find originating from industry-funded climate disinformation websites) appearing in this forum.

The UK Parliamentary Inquiry took submissions and held a brief hearing. I made a submission that made specific reference to the trick but this analysis was not considered. The hearing was

⁴⁰ <http://www.realclimate.org/index.php/archives/2004/12/myths-vs-fact-regarding-the-hockey-stick/>

covered by parliamentary reporters and not just the usual science reporters. Many of the reporters were taken aback by Jones' testimony, expressing their discomfiture in stylish English prose. Quentin Letts of the Daily Mail, for example, described vice-chancellor Acton, as "a younger version of Professor Calculus from the Tintin books, who beamed and nodded at everything Professor Jones said. 'I think that answer was spot-on,' he cried, after listening to one response from the terror-stricken Jones." Dismayed by Jones' testimony, Letts hoped that politicians "sought second, third, even 20th opinions". Other parliamentary correspondents were equally acidic.⁴¹

Jones was accompanied by his university's vice-chancellor, Professor Edward Acton, who provided much-needed comic relief. Professor Acton, a younger version of Professor Calculus from the Tintin books, beamed and nodded at everything Professor Jones said. 'I think that answer was spot-on,' he cried, after listening to one response from the terror-stricken Jones.

Professor Acton's left eyebrow started doing a little jiggle of its own. His eyeballs bulged with admiration for the climate-change supremo. His lips were pulled so wide in wonderment they must nearly have split down the seams like banana skins.

Others, watching the tremulous Professor Jones, will have been less impressed. He may be right about man-made climate change. But you do rather hope that politicians sought second, third, even 20th opinions before swallowing his theories and trying to change the world's industrial output.

In a 3-1 split decision, the Parliamentary Committee⁴² concluded that the trick was simply "shorthand for the practice of discarding data known to be erroneous" ... in order to trick you... into not knowing about... the decline.

Their finding was entirely unsupported by the evidence. If the tree rings had, for example, been measured incorrectly, then such data could have been discarded as erroneous. But that's not what happened. No one, not even the University of East Anglia, even hinted that the tree ring data had been measured incorrectly. The problem was entirely different – the trees didn't do what Briffa anticipated. The Committee's finding was totally unjustified. In fairness, the Parliamentary Committee expressed its expectation that the Science Panel would weigh in on the matter, an expectation on which they were quickly disappointed.

7. Critics of CRU have suggested that Professor Jones's use of the word "trick" is evidence that he was part of a conspiracy to hide evidence that did not fit his view that recent global warming is predominately caused by human activity. The balance of evidence patently fails to support this view. It appears to be a colloquialism for a "neat" method of handling data. (Paragraph 60)

8. Critics of CRU have suggested that Professor Jones's use of the words "hide the decline" is evidence that he was part of a conspiracy to hide evidence that did not fit his view that recent global warming is predominantly caused by human activity. That he has published papers—including a paper in *Nature*—dealing with this aspect of the science clearly refutes this allegation. In our view, it was shorthand for the practice of discarding data known to be erroneous. We expect that this is a matter the Scientific Appraisal Panel will address. (Paragraph 66)

⁴¹ See links at <http://climateaudit.org/2010/03/02/opening-night-reviews-in-the-uk-press/>

⁴² **Report** <http://www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/387/387i.pdf>;
Evidence <http://www.publications.parliament.uk/pa/cm200910/cmselect/cmsctech/387/387ii.pdf>; **Archived Oral Testimony:** <http://www.parliamentlive.tv/Main/Player.aspx?meetingId=5979>

The Science Appraisal Panel was led by Lord Oxburgh⁴³, the chairman of a large wind power utility. The panel was announced on March 22 and reported on April 12; its report was only 5 pages long. They did not interview any critics or Climategate targets nor did they take any submissions. Even its terms of reference remain unclear. Their report stated that they were charged with assessing eleven supposedly critical papers that had been chosen on the “advice of the Royal Society. However, the Royal Society refused to answer question on who at the Royal Society supposedly selected these eleven articles and why they were chosen.

If they were trying to put questions to rest, the articles were singularly ill-chosen. The three articles on the Briffa reconstruction were the earliest three articles, all of which preceded the trick and none of which were in dispute. None of CRU’s own proxy reconstructions were studied, thereby precluding the opportunity to put concerns about cherry picking of proxies to rest. In fact, it seems that the eleven articles were selected by the University and not by the Royal Society. They proved to be the articles cited in the University’s brief to the Parliamentary Committee, a submission that was trying to put CRU in the best light by citing the articles in which the trick had not been used – not the ones where it had been used.

Lame as their report is, they contradicted the claim that the trick was a good way of handling data. Instead, they said that the IPCC “neglect” to present the data was “regrettable.” It was not a “good way” of dealing with the data after all. Of course, the IPCC authors in question had not “neglected” to present the data; they had done so intentionally. And of course, the IPCC authors in question could hardly be separated from CRU. But the Oxburgh inquiry left all of that out – its own version of the trick, I guess.

Recent public discussion of climate change and summaries and popularizations of the work of CRU and others often contain oversimplifications that omit serious discussion of uncertainties emphasized by the original authors. CRU publications repeatedly emphasize the discrepancy between instrumental and tree-based proxy reconstructions of temperature during the late 20th century, but **presentations of this work by the IPCC and others have sometimes neglected to highlight this issue.** While we find this **regrettable**, we could find no such fault with the peer-reviewed papers we examined.

There is still one inquiry to go – the Muir Russell inquiry⁴⁴, whose report is expected next month. It took submissions in a brief two-week window in February, but, like the others, hasn’t talked to any CRU targets or critics.

Unfortunately, it isn’t just the trick that the inquiries have failed to investigate. To an astonishing degree, they’ve totally avoided dealing with actual issues. This was recently recognized by the fourteen authors of the Hartwell Paper, including Mike Hulme of the University of East Anglia, who stated:” Hitherto, none of the specific critiques of this work by those auditing it have been adjudicated by reviews of the matter, and indeed were explicitly not investigated by the Oxburgh review”.

The seeming obtuseness of these inquiries is obviously very frustrating, as there doesn’t seem to be any accountability anywhere in the system;.

⁴³ <http://www.uea.ac.uk/mac/comm/media/press/CRUstatements/SAP>

⁴⁴ One of the members, Philip Campbell, editor of Nature, resigned the same day after he was discovered to have made prejudicial comments on the matter. Strange inconsistencies in the biography of another member, Geoffrey Boulton, quickly emerged, by Boulton toughed it out. See Climate Audit discussion in Feb 2010.

For the public, non-disclosure of adverse data, like the trick, seems like misconduct, but Pielke Junior, for example, has observed that there is little point in trying to fit non-disclosure of adverse data into academic misconduct, because the practice is widespread in the academic community – not just climate science. Academics seem unoffended by the trick.

But there's a price for not being offended, because the public expects more. If climate scientists are unoffended by the failure to disclose adverse data, unoffended by the trick and not committed to the principles of full, true and plain disclosure, the public will react, as it has, by placing less reliance on pronouncements from the entire field – thus diminishing the coin of scientists who were never involved as well as those who were. This is obviously not a happy situation at a time when climate scientists are trying to influence the public and many have lashed out by blaming everyone but themselves, using the supposed exonerations by these ineffectual inquiries as an additional pretext.

To the extent that things like the trick were sharp practice, the practices needed to be disavowed. The scientists do not need to be drummed out, but there has to be some commitment to avoiding these sorts of sharp practice in the future. George Monbiot suggested⁴⁵ early on perceived that apologies were necessary on the part of the climate scientists involved both to the targets and to the wider community – something that, in my opinion, would go a long way to achieving some sort of truth and reconciliation in a difficult situation. Right now, this seems less likely to happen than ever.

Despite the failures of the inquiries to do their job, I strongly disagree with Cuccinelli's recent investigation of potential financial abuse⁴⁶. Regardless of what one may think of the quality of Mann's work, he has published diligently. In my opinion, Cuccinelli's actions are an abuse of administrative prerogative that on the one hand is unfair to Mann and on the other provides an easy out for people to avoid dealing with the real issues.

I opened my comments with caveats and I'll close with more caveats. The critical scientific issue – as it has been for the past 30 years – is climate sensitivity and whether cloud and water cycle feedbacks are strongly positive or weakly negative or somewhere in between. This is territory of Lindzen, Spencer, Kininmonth and Paltridge. But persuasive as they may be, keep an open mind

⁴⁵ <http://www.monbiot.com/archives/2009/11/23/the-knights-carbonic/> Monbiot: It's no use pretending that this isn't a major blow. The emails extracted by a hacker from the climatic research unit at the University of East Anglia could scarcely be more damaging. I am now convinced that they are genuine, and I'm dismayed and deeply shaken by them. ... there are some messages that require no spin to make them look bad.

I have seldom felt so alone. Confronted with crisis, most of the environmentalists I know have gone into denial. The emails..., they say, are a storm in a tea cup, no big deal, exaggerated out of all recognition. It is true that climate change deniers have made wild claims which the material can't possibly support (the end of global warming, the death of climate science). But it is also true that the [emails are very damaging](#).

The response of the greens and most of the scientists I know is profoundly ironic, as we spend so much of our time confronting other people's denial. Pretending that this isn't a real crisis isn't going to make it go away. Nor is an attempt to justify the emails with technicalities. We'll be able to get past this only by grasping reality, apologising where appropriate and demonstrating that it cannot happen again. (Nov 25)

⁴⁶ See Climate Audit for more detailed discussion of issues, where, inter alia, the applicability of the Virginia statute is questioned. <http://climateaudit.org/2010/05/02/cuccinelli-v-mann/>; <http://climateaudit.org/2010/05/03/correspondence-with-the-university-of-virginia/>; <http://climateaudit.org/2010/05/03/the-virginia-statute/>

because many serious scientists do not agree with them and stand behind standard estimates of climate sensitivity to doubled CO₂ in good faith.

If the impact of doubled CO₂ is relatively small, it would be through sheer good luck rather than through good management. I do not subscribe to the belief that we need perfect certainty to make decisions – on the contrary, businessmen and politicians make decisions all the time under unquantifiable uncertainty and I, for one, have no philosophical objection to governments making decisions on climate policy. I think that there may be important practical situations where people who are primarily worried about the energy future can find common ground with people who are primarily worried about climate.

If I were a minister of the environment with policy responsibilities, regardless of what I felt personally, I would take scientific guidance from official institutions, rather than what I might think personally either as an occasional contributor to academic journals or as a blogger. Though, knowing what I know now, I would also try to improve the performance and accountability of these institutions.

People sometimes say to me – if the hockey stick is wrong, then the situation is worse than we think – arguing this on the basis that this would be evidence of greater climate sensitivity. My standard answer is – well, if that's the case, we should find out and govern ourselves accordingly. And give no thanks to people whose obstruction has delayed the resolution of the problem.