

CLIMATEGATE

Untangling Myth and Reality Ten Years Later

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INTRODUCTION: MAKING THE MYTHS

It is now 10 years since the Climategate emails were released. The issues they raised continue to reverberate; even figuring in a [decision](#) last week of the United States Supreme Court to allow Michael Mann's (US) defamation suits to proceed (see the [dissent](#) by Justice Alito), and in an August 2019 [decision](#) of the BC Superior court dismissing a similar suit (on which see more below). The immediate reaction at the time to the emails was visceral, even among "green" reporters, including George Monbiot as follows:

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

UK reporter Fred Pearce, who covered story for the *Guardian* and who, unlike Oxburgh or Muir Russell, had actually read the emails, wrote in *The Climate Files*:

The evidence of scientists cutting corners, playing down uncertainties in their calculations and then covering their tracks by being secretive with data and suppressing dissent suggests a systemic problem of scientific sloppiness, collusion and endemic conflicts of interest, but not of outright fraud. (p. 241)

Given the importance of climate science in today's society, all of us expect more of climate scientists than merely that they not commit "outright fraud." Exoneration at such a low threshold would be small exoneration indeed.

However, rather than confronting the corruption and misconduct apparent throughout the Climategate emails, the climate academic community shut their eyes to the affair, eventually even persuading itself that the offending scientists were victims, rather than offenders.

This re-framing was made possible by numerous myths propagated about the affair, of which the following were especially important:

Myth #1: The Climategate scandal arose because "cherry-picked" emails were taken "out of context".

Myth #2: The Climategate correspondents were “exonerated” following “thorough” and impartial investigations.

Myth #3: Scientific studies subsequent to Climategate have “confirmed” and “verified” the original Mann hockey stick.

These are only the major myths from a veritable tsunami of disinformation from the academic community. The myths are untrue and, in this article, we will explain why.

Yamal: Climategate in a Nutshell

A good illustration of the three myths is provided by the Yamal story. In many ways it was at the [heart of Climategate](#), yet very few commentators picked up on its centrality. Most tree ring temperature proxies do not have a hockey stick shape. A few do: some bristlecone pine records from the US (on which Mann’s hockey stick depended) and a few larch records from the Yamal peninsula of northwest Russia. The Yamal record was the [key ingredient](#) for virtually all the supposedly independent confirmations of the Mann hockey stick.

The first email in the Climategate archive, dated March 6 1996, was from a Russian scientist (Shiyatov) to Briffa requesting money to support their efforts to collect more tree ring data from the Yamal peninsula. One of the last Climategate emails (dated October 5, 2009, just a month before the Climategate release) was from scientist Rashit Hantemirov to a UEA colleague asking for advice on how to respond to a Finnish journalist who was investigating Briffa’s use of the Yamal data, based on findings Steve had published at Climate Audit. Yamal was, in fact, the [most-repeated theme](#) in the emails, even though it never captured public attention. The emails provided extensive context for a controversy that had long been raging.

Since 2005, Steve had [regularly criticized](#) Briffa and the CRU for concealing an updated version of a proxy record from the Polar Urals which, unlike the original published in 1995, showed a strong Medieval Warm Period. The cold medieval segment of the Polar Urals series was critical to a few early hockey stick-like reconstructions, so much so that using the revised series would have overturned the original conclusions.

Briffa resisted disclosing the updated Polar Urals data and Steve only obtained it after a lengthy dispute with *Science* magazine. When it finally became available, the CRU scientists promptly [dropped it](#) from their studies and substituted one from the nearby Yamal peninsula instead. Whatever the stated reasons for doing so (and at the time none were given), the effect was to remove a proxy that now had a medieval warm period and replace it with one with a very strong hockey stick shape, especially due to a big jump after 1990.

The two series (Yamal and updated Polar Urals) gave [contradictory information](#) about the climate of the region in the medieval era, something not disclosed to readers of the very studies and reports that placed great emphasis on the importance of being able to make precise claims about the relative warmth of the medieval era. Over an 8-year period Briffa used the Yamal series repeatedly in his papers, but never published the data. Steve’s various requests for the data were ignored, but in 2008 Briffa published a study based on Yamal in a journal (*Philosophical Transactions of the Royal Society*) that had adopted strict data disclosure rules. After Steve asked the journal for the data the editor demanded Briffa [provide it to Steve](#).

What immediately became apparent was that the post-1990 jump was based on a sampling flaw: the sample size collapsed at that point to fewer than the minimum number of trees required and the series should have been terminated prior to 1990. Also, Briffa had not used a large set of

nearby tree cores that would have allowed the full interval to be covered, but doing so would have yielded a different overall profile, one with [no hockey stick shape](#). Although numerous post-Mann hockey stick studies relied on the Briffa Yamal series to provide a supposedly independent confirmation of the Mann result, in 2013 the CRU quietly [abandoned](#) the Briffa Yamal series and substituted one based on a larger sample that looked a lot like the one Steve had computed back in 2009 by combining Briffa's data with the larger nearby data set.

None of this misconduct was dealt with by the various inquiries. The Oxburgh panel ignored it entirely (see below). In Steve's evidence to the Muir Russell panel (on which also see below) he showed the published version of Briffa's Polar Urals series, with its cold Medieval series, and the updated resampling in which the medieval era was now very warm by comparison to the present. He also discussed Briffa's secrecy and refusal to publish his data, which thwarted discovery of the weaknesses of his temperature reconstructions. The Muir Russell panel dismissed all these concerns on the basis that they were not published in academic journals. This was ridiculous reasoning since, first, much of the battle involved getting the journals to enforce their own data disclosure policies but this typically does not lead to an article in the journal, and second, by refusing to disclose the data Briffa was making it impossible for papers critical of his analysis from being published.

MYTH #1: THE EMAILS WERE TAKEN OUT OF CONTEXT

Climate academics repeatedly assert that the emails were taken "out of context" to create controversy, yet the reality is the exact opposite: the controversies already existed; the emails provided the disquieting context that exposed the depth of malfeasance. The most notorious emails (e.g. "hide the decline", "dirty laundry") concerned issues and controversies which had already been raised at "skeptical" blogs (especially Climate Audit). The emails provided background detail which was then analysed extensively in contemporary blog posts at Climate Audit. Rather than coming to terms with the revelations, the climate community has simply chanted "out of context!", but never demonstrated that there exists an alternative context in which the emails were less damning.

We will show this for several of the most prominent and controversial emails, but the potential list is far more extensive.

Example 1: "Dirty Laundry"

In a remarkable [2003 email](#), which was discussed in detail at [Climate Audit](#) within the first two weeks of Climategate, Mann sent some undisclosed calculations from the Mann et al 1998 temperature reconstruction to Tim Osborn, a "trusted colleague", telling Osborn that the series were his "dirty laundry" and needed to be kept strictly confidential so that they didn't fall into the wrong "hands".

CLIMATEGATE: Myth and Reality

From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: reconstruction errors
Date: Thu, 31 Jul 2003 11:18:24 -0400

Tim,
Attached are the calibration residual series for experiments based on available networks back to:
AD 1000
AD 1400
AD 1600
I can't find the one for the network back to 1820! But basically, you'll see that the residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them up. In any case, the incremental changes are modest after 1600--its pretty clear that key predictors drop out before AD 1600, hence the redness of the residuals, and the notably larger uncertainties farther back...
You only want to look at the first column (year) and second column (residual) of the files. I can't even remember what the other columns are!
Let me know if that helps. Thanks,
mike
p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things...

The email sounds bad enough on its face, but, *in context*, it is even worse. Nor was Mann's withholding of data an issue that originated with the Climategate emails: it was a longstanding controversy to which the Climategate email added additional and disquieting context. It also touches issues which, due to Mann's libel lawsuits, linger on to this day.

Reconstruction Residuals

The "dirty laundry" data series are called *residuals*. They are the differences between the proxy reconstruction estimates of past temperature and observed temperature records during the model estimation ("calibration") and testing ("verification") periods. Since the residuals measure the goodness-of-fit of the model, they are essential for computing verification test scores. In this email Mann was supplying residuals for reconstructions (which he grandiosely calls "experiments") based on the post-1000, post-1400 and post-1600 intervals. The first two were critical since they determine whether it is legitimate to do the reconstruction back that far.

Numerous statistical authorities, including those¹ cited in Mann et al 1998, recommend testing reconstruction validity using several different scores based on the residuals. Mann stated in his 1998 paper that he had computed two such scores, the Reduction of Error (RE) statistic and the r2 score. But in his paper and in the accompanying archive he only listed the RE values. He had not (and has never) released the r2 scores. Nor could they readily be computed from information disclosed with the original publication because, contrary to widespread belief among climate scientists, Mann's archive omitted the complete reconstructions for each time step. For the signature Northern Hemisphere (NH) reconstruction, Mann only archived the *spliced* reconstruction segments in which, at each time step, the results of a later step were printed over results from earlier steps. Without the residual series no one could compute the unreported r2 scores.

¹ Fritts 1991; Cook and Peters, 1994

What Was Being Hidden

In late 2003, only a few months after the “dirty laundry” email, we asked Mann to provide the residual series for the AD1400 step of his reconstruction. He refused. We filed a Materials Complaint to *Nature*, which had published the 1998 study, under their disclosure policies for either the residual series or the reconstruction steps. To their shame and discredit, *Nature* refused. We also requested the US National Science Foundation to require Mann to provide this data. To their discredit, they also refused.

Despite disinformation to the contrary, the results of Mann’s individual steps remain unarchived to this day.

We discovered the reason why Mann was so adamant about withholding his “dirty laundry” in 2004 – long before Climategate. By early 2004, despite many obstacles, we had been able to replicate Mann’s peculiar and poorly documented methodology well enough to calculate the residual series (and verification statistics) for the AD1400 step.

We discovered, to our considerable surprise, that the verification r^2 statistic for the AD1400 step was disastrously low (0.018). The verification r^2 is a commonplace statistic, which ought to be easily passed by any reconstruction purporting to have statistical “skill”. It is not a guarantee of model validity, but failure is more or less a guarantee of model invalidity. We reported our discoveries in two widely-discussed 2005 articles.² At the time, we didn’t know for sure whether Mann had overlooked calculation of verification r^2 values (implausible but possible) or whether he had calculated the values, discovered that they were disastrous and withheld them. Both alternatives were disquieting.

The dispute was prominently reported on in 2005, including a frontpage article in the Wall St Journal which attracted the attention of the US House Energy and Commerce Committee. They sent a set of questions to Mann including ones about source code and verification r^2 statistics. These provoked vigorous protests from AAAS, AGU and other science institutions. Ralph Cicerone, then chair of the National Academy of Sciences, [wrote](#) to the House Energy and Commerce Committee offering their services, including, specifically, examination of the verification r^2 . Two studies were commissioned by congressional committees: the 2006 [National Academy of Science](#) and [Wegman](#) reports.

In partial response to the Committee questions, Mann archived some (but not all) source code for Mann et al 1998. While incomplete, it confirmed our surmise that Mann had calculated verification r^2 statistics for each step of the signature NH reconstruction and had withheld them.

Subsequently, in 2006, Wahl and Ammann, both Mann allies and associates, did [their own](#) replication of the various steps: we were quickly able to reconcile their calculations to ours. They replicated the poor verification r^2 for the AD1400 step and discovered that the score for the AD1600 step was even worse – perhaps the worst verification r^2 in any scientific study ever published. Despite reconciling exactly to and confirming our results, their abstract misleadingly asserted that they had verified Mann’s results, when, in fact, they replicated ours – a point made at the time by Professor Wegman.

To this day, Mann has never archived the NH reconstructions for individual steps, the equivalent residual series (the “dirty laundry”) or even the verification r^2 results.

² McIntyre and McKittrick, [GRL 2005](#) and [E&E 2005](#).

Ongoing Repercussions

Although this was one of the longest standing and most contentious Climategate issues, it was not addressed by any of the inquiries.

Subsequent to Climategate, the issue has been resurrected by Mann’s libel lawsuits against Tim Ball and Frontier Institute in British Columbia, Canada in 2011 and against National Review, Mark Steyn, Competitive Enterprise Institute and Rand Simberg in DC in 2012. These cases frequently turn on the question of whether Mann has withheld important data. In the BC lawsuit, defendants asked Mann to provide verification r2 statistics for the individual steps of his reconstruction. Mann failed to do so. In 2019, after eight years, Mann’s lawsuit was dismissed by the B.C. court for failure to advance the proceedings, with costs awarded against Mann. In the DC lawsuit, seven years have passed without discovery taking place. One presumes that Mann will once again be asked to provide verification r2 statistics and will be challenged on why the adverse results were withheld.

The bottom line is that the “dirty laundry” email was never taken “out of context” by climate skeptics. On the contrary, it provided additional and disquieting context for Mann’s well-known withholding of the results of the individual steps of the Mann et al 1998 reconstruction. Rather than the email being innocent when placed in context, it was exactly the opposite.

Example 2: “Pressure to Present a Nice Tidy Story”

Long before the Climategate emails, it was well known at Climate Audit and other skeptic blogs that an inconvenient part of Keith Briffa’s temperature reconstruction had been deleted from a famous IPCC diagram. A series of Climategate emails provided an unexpectedly detailed and very troubling context for this excision.

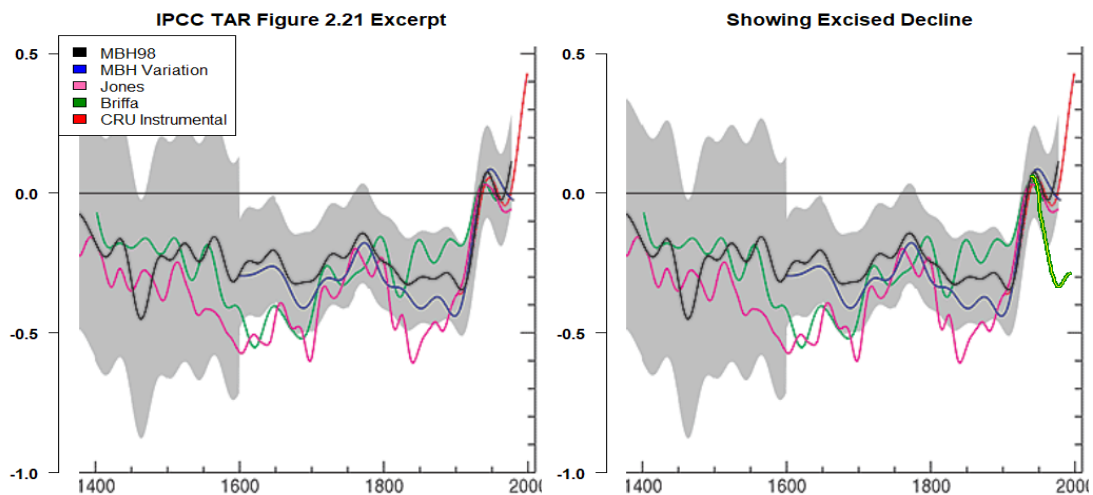


Figure 1. LEFT: Portion of hockey stick diagram as published by IPCC in 2001 (AR3 Fig 2.21). RIGHT: Actual source data. Notice the declining green line at the end.

The Mann and Briffa reconstructions were both shown in a famous diagram (Fig 2.21) in the IPCC (2001) 3rd Assessment Report in a section of which Mann was lead author. The diagram (excerpt shown in left panel above) gave a strong rhetorical impression of coherence among the proxy reconstructions in the 20th century. In order to achieve this rhetorical message, the authors excised the declining portion of the Briffa reconstruction (after approximately 1940). Without this deletion (as shown in right panel), the Briffa reconstruction had a very dramatic decline in the last half of the 20th century. By hiding the decline in the Briffa reconstruction, the authors gave a false impression of coherence in the proxy reconstructions. Had the decline been shown, the obvious question for readers would have been: if the tree ring proxies gave inconsistent results in the 20th century, with a large population going the “wrong” way, how could anyone preclude similar behavior in the past? Making matters worse, the deletion of the adverse portion of the Briffa reconstruction was not discussed or even disclosed in the IPCC report.

The Briffa Data Deletion

The underlying problem for all efforts to estimate temperature for the past one or two millennia using tree ring widths is that the data is widely and unfixably inconsistent. In the 1980s and 1990s, Fritz Schweingruber had carried out a massive collection of tree ring data from sites expected to be proxies for temperature (high altitude and/or high latitude). However, he discovered, to the chagrin and puzzlement of the climate community, that the resulting series went down in the last part of the 20th century (the “decline”). Schweingruber’s data was published in a series of articles with lead author Keith Briffa, including an article in *Nature* almost exactly contemporary with Mann et al (1998).

The data used in Mann et al consisted mainly of tree rings. Mann included sites believed to be responsive to precipitation, as well as sites from the Schweingruber network. However, unlike Briffa’s reconstruction, Mann’s reconstruction went up in the famous Hockey Stick.

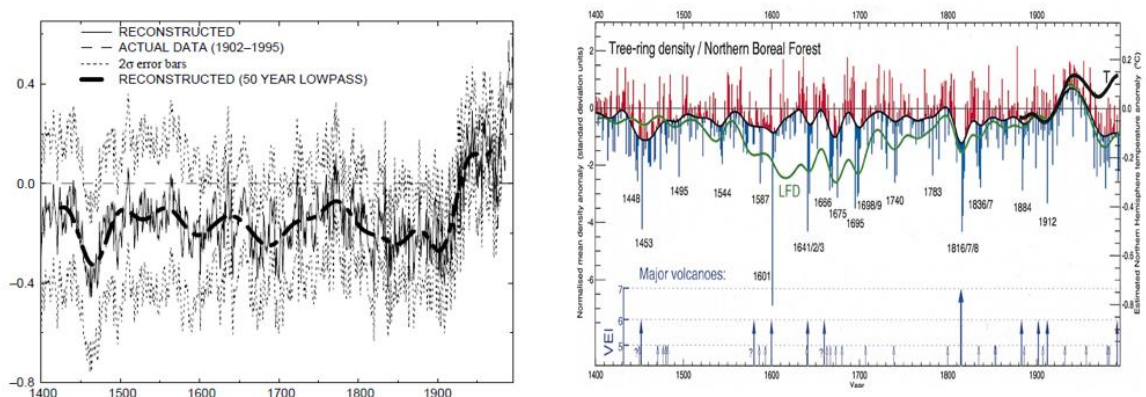


Figure 2. LEFT: Mann reconstruction (Mann et al, 1998); RIGHT: Briffa reconstruction (Briffa 2000)

The deletion of the Briffa decline from the canonical IPCC diagram was publicly unnoticed until 2005, when it was reported in an article at [Climate Audit](#). At the time, values after 1960 were

missing in the only archive of this version of the Briffa reconstruction, adding an additional layer of concealment and making it much more difficult to estimate exactly what was excised. The missing data first became available in a Climategate email.

This seemingly unscrupulous deletion of adverse values was incomprehensible to readers of Climate Audit, especially those outside academia who had to deal with customers and regulators in real life. It would be unimaginable, for instance, for a manager of a mutual fund to delete a declining portion from a graph of the fund history.

The following year (2006), both of us acted as reviewers of the IPCC Fourth Assessment Report. In that capacity, we asked that IPCC show the decline. Briffa was lead author of this section and curtly refused, merely stating that it would be “inappropriate,” without, needless to say, explaining why.

September 1999: Arusha, Tanzania

The Climategate emails provided extensive and unexpected **context** for the Briffa deletion. In September 1999 there was a meeting of IPCC lead authors in Arusha, Tanzania to consider the initial draft of the IPCC 2001 Assessment Report. At the meeting, Mann, a section lead author, presented a first draft of the figure that later became Figure 2-21, in which a discrepancy between the Briffa and Mann reconstructions was clearly visible.

Three weeks later, IPCC Coordinating Lead Author Chris Folland sent a note to Mann, Jones and Briffa saying that the senior IPCC officials wanted to make prominent use of the diagram, but were concerned that the Briffa reconstruction “dilutes the message” (emphasis added):

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 [Briffa’s] somewhat contradicts the [Mann] multiproxy curve and **dilutes the message** rather significantly.

Phil Jones’ suggestion was that they show the Briffa reconstruction in a separate diagram in a different section, thereby making it “awkward” to compare the discrepant reconstructions. Briffa objected to his series being marginalized. He said that he thought that the “the recent warmth was probably matched about 1000 years ago” and then added:

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.

When Mann came online later that morning, he noted that inclusion of the Briffa series “raise[d] a conundrum” that had been apparent to “everyone in the room” at the IPCC authors’ meeting: that the Briffa reconstruction was “a problem” and a “potential distraction” from the viewpoint that they wanted to show. Mann observed that, if they showed the Briffa series, then they would

have to explain the inconsistencies and that the skeptics would “cast doubt on our ability to understand the factors that influence these estimates and, thus, can undermine faith” in IPCC temperature reconstructions. Mann stated that he didn’t want to be the one to give “fodder to the skeptics”.

The next day, Briffa apologized to Mann for his temporary pangs of conscience and, a few days later, Briffa’s colleague, Tim Osborn, sent Mann a revised version of the Briffa reconstruction with more “low-frequency” variability. This version included data to 1994 but he noted in his email that they “usually” stop the series in 1960.

By this time, Mann was up against a deadline for chapter revisions since a draft for reviewers was scheduled to be sent out only 3 weeks later. For this draft, Mann developed a revised figure in which the last ~50 years of the Briffa reconstruction (the decline) were simply deleted. According to John Christy, also a lead author of the same chapter as Mann, Mann did not discuss (or even disclose) the deletion of adverse data to his fellow lead authors. Indeed, Christy didn’t know that it had been done until it was pointed out at Climate Audit in 2005.

Example 3: “To Hide the Decline”

These events also provide essential **context** for the famous email from November 1999 – just two months after the Arusha meeting - in which Jones told Mann and Briffa:

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) and from 1961 for Keith’s to hide the decline.

The email discussed a diagram prepared by Jones, Mann and Briffa for the 1999 World Meteorological Organization annual report: all three temperature curves – even the Briffa one – rose dramatically towards the end – in even more dramatic unison than the IPCC diagram prepared the previous month. Jones had prepared his version by splicing proxy data with temperature data – splice point at 1980 for Jones and Mann reconstructions and 1960 for the Briffa reconstruction.

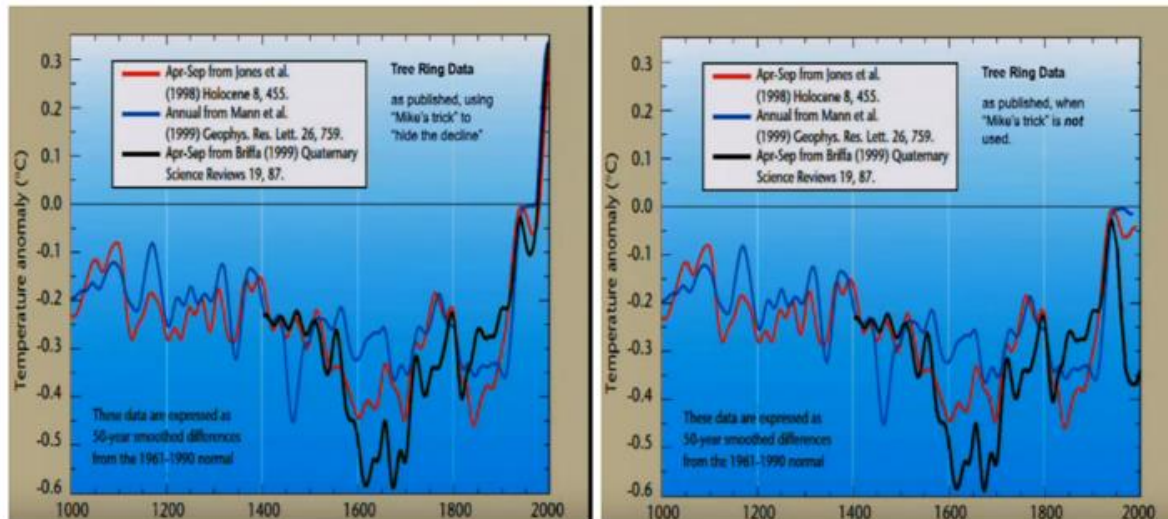


Figure 3. LEFT: WMO diagram as prepared by Jones; RIGHT how the data ought to have been presented according to Richard Mueller of Berkeley in a widely disseminated lecture

“Mike’s Nature trick” in this context is the splicing of proxy and instrumental data into one series – a technique used in Mann et al 1998 for the construction of the smoothed proxy curve in the IPCC report. The admission of such splicing in a Climategate email caused some smiles in the skeptic community since Mann had [vociferously denied](#) such splicing several years earlier:

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.

Mann himself claimed that his “trick” was a graphical method in which the temperature estimate using proxies (reconstruction) and observations were plotted on the same panel – an explanation which was credulously accepted by the climate community, despite the fact that the whole point was to prevent the proxy data from being plotted alongside temperatures to conceal their divergence. As for plotting observations and estimates on the same panel, this is a commonplace technique in use from the beginning of statistical analysis – not something that Mann discovered.

Further Examples: “We know with certainty that we know fuck-all”

Innumerable other examples can be given to demonstrate that the controversial quotes were not taken out of context. Out of all the examples, most topical are probably those emails that show that, in private, the scientists had many doubts on key topics, which they kept hidden from the public in order to make their analyses seem far more robust and certain than they really were.

Here are some examples, presented as a list without discussion, because a full exposition would require a book length treatment.

Paleoclimate

we can probably say a fair bit about <100 year extra-tropical NH temperature variability (at least as far as we believe the proxy estimates), but honestly know fuck-all about what the >100 year variability was like with any certainty (i.e. we know with certainty that we know fuck-all).

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!

the recent warmth was probably matched about 1000 years ago

Controversy about which bull caused mess not relevant. The possibility that the results in all cases were heap of dung has been missed by commentators.

[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!

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 he has produced over the last few years , and ... (better say no more)

There has been criticism by Macintyre of Mann's sole reliance on RE, and I am now starting to believe the accusations.

Is the [Mann] PCA approach robust? Are the results statistically significant? It seems to me that in the case of MBH the answer in each is no. It is not clear how robust and significant the more recent approaches are.

The IPCC

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.

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 include it? This is truly embarrassing.

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.

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.

All these decisions about IPCC chairs and co-chairs are deeply political

I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow - even if we have to redefine what the peer-review literature is !

Models

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.

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

If the spread of individual model results is large enough and at least 1 model overlaps the observations, then one cannot claim that all models are wrong, just that the mean is biased. My own gut feeling is that models as a group probably do indeed have a significant bias in simulating upper air temperature trends

The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't.

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.

MYTH #2: THE INQUIRIES EXONERATED THE SCIENTISTS

There was never any proper inquiry into Climategate. This was very regrettable as impartial inquiries could easily have avoided much of the subsequent controversy. Instead, they were whitewashes, and they were seen at the time as whitewashes by outside observers. They didn't fool outside observers at the time, and they don't deserve to be treated as credible investigations these many years later.

There was a certain bureaucratic artfulness in how the whitewash was applied. A key element was the terms of reference for each investigation: none of the investigations was charged with investigating all aspects. Each investigation had relatively narrow terms of reference and these terms were interpreted even more narrowly, opening up gaps large enough for most of the key controversies to fall through. Secondly, the investigations never included panelists who were familiar with skeptic criticisms. Third, the investigations consistently neglected to interview skeptics who understood the context of often long-standing controversies. Fourth, investigations made findings that were plainly contradicted by known facts. **None**, we repeat, none of the inquiries actually investigated the email context of the key allegations, including the "dirty laundry" and "diluting the message"/"fodder for the skeptics" threads discussed above.

Penn State Inquiry Committee and Investigation Committee

The most important – and arguably the most flawed – investigation was the first stage (Inquiry Committee) of the Penn State academic misconduct proceedings, which, in violation of important [Office of Research Integrity guidelines](#), removed the most important issues from the terms of reference of the actual Investigation Committee, leaving the Investigation Committee with an absurdly irrelevant remit. Criticisms of the Penn State Inquiry were [specifically mentioned](#) by Justice Alito as being at issue in the recent Supreme Court ruling on the Mann defamation suits.

Penn State, like most other US universities and in keeping with federal Office of Research Integrity policies, had a [two-stage procedure](#) for academic misconduct:

- The purpose of the first stage, an *Inquiry Committee*, is "preliminary information-gathering and preliminary fact-finding" to "determine if the allegation ... has sufficient substance to warrant an investigation". According to Office of Research Integrity guidelines, "in general, absent full admissions, inquiries should **not** be used to make findings on whether research misconduct in fact occurred."
- The second stage, the Investigation Committee, is supposed to conduct "a thorough review and analysis of all relevant facts to reach a conclusion as to whether research misconduct has occurred, who was responsible, and how serious any misconduct was".

This is not what Penn State did.

Procedural Mayhem

On November 24, 2009, only 5 days after the emails were released, Eva Pell, then Senior Vice President for Research and Dean of the Graduate School, initiated an investigation under university policy RA-10 governing academic misconduct. At the time, Pell was in her final few days

at Penn State: she left Penn State at the end of 2009 (only five weeks later) for a new job at Smithsonian Institute and was not involved in the final report of the Inquiry Committee.

She met with four university officials, one of whom was William Easterling, then Mann's supervisor. The subsequent report stated that Easterling "recused himself from the inquiry for personal reasons", but, according to an email that we received from someone familiar with intimate details of the inquiry, this was untrue. Despite the claimed recusal, Easterling apparently continued to influence the inquiry and was responsible for their failure to interview us (as the most prominent critics of Mann's work).

Over the next 5 days, Pell purported to "synthesize" the various controversies into the following four allegations:

1. Did you engage in, or participate in, directly or indirectly, any actions with the intent to suppress or falsify data?
2. Did you engage in, or participate in, directly or indirectly, any actions with the intent to delete, conceal or otherwise destroy emails, information and/or data, related to AR4, as suggested by Phil Jones?
3. Did you engage in, or participate in, directly or indirectly, any misuse of privileged or confidential information available to you in your capacity as an academic scholar?
4. Did you engage in, or participate in, directly or indirectly, any actions that seriously deviated from accepted practices within the academic community for proposing, conducting, or reporting research or other scholarly activities?

In the first "synthesized" allegation, read carefully, Pell did nothing more than a restatement of a standard clause (falsification) in Penn State and most university academic codes of conduct. However, unlike virtually every other academic misconduct case, Pell didn't particularize how Mann was alleged to have participated in suppression or falsification of data, even failing to mention longstanding controversies about withholding adverse verification statistics, the deletion of adverse portion of Briffa reconstruction in the IPCC diagram etc.

The Inquiry Committee could have easily informed itself of the allegations against Mann by interviewing critics who had been invited to the National Research Council hearing and/or House Energy and Commerce Committee hearing in 2006, but didn't do so. Even though the Office of Research Integrity had warned that such preliminary inquiries "should **not** be used to make findings" on research misconduct, the Penn State Inquiry did exactly that. Instead of interviewing critics to particularize the allegations, they interviewed Mann who, according to their report, "explained that he had never falsified any data, nor had he had ever manipulated data to serve a given predetermined outcome".

On January 26, 2010, the inquiry committee met, "along with University counsel, Mr. Wendell Courtney, Esq. in case issues of procedure arose." By this stage, there had been numerous violations of Office of Research Integrity required procedures, but university counsel Courtney appears to have remained silent.

The Inquiry Committee ignored the “dirty laundry” email and the ones related to the “nice tidy story” in the IPCC diagram. They limited their analysis to the email about the “trick” in the WMO diagram, which they explained away as follows:

While a perception has been created in the weeks after the CRU emails were made public that Dr. Mann has engaged in the suppression or falsification of data, there is no credible evidence that he ever did so, and certainly not while at Penn State. In fact to the contrary, in instances that have been focused upon by some as indicating falsification of data, for example in the use of a "trick" to manipulate the data, this 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"(1) 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.

Decision 1. As there is no substance to this allegation, there is no basis for further examination of this allegation in the context of an investigation in the second phase of RA-10.

Having failed to familiarize themselves with and address already-known issues, their finding in respect to the WMO diagram had no basis. The splicing in question had not been “reviewed by a broad array of peers in the field”—it was done in secret and was unknown before Climategate. The Muir Russell panel later contradicted the Penn State finding, finding that the deletion of data in the IPCC diagram and deletion followed by splicing in the WMO diagram showed an “intent to paint a misleading picture.”

Deleting IPCC Review Records

The IPCC AR4 included a section authored by Keith Briffa which resolved the hockey stick controversy in Mann’s favor using text which had not been shown to reviewers during the report-writing process. After the publication of the AR4, a retired engineer in the UK named David Holland began digging into where the text in question came from. After some diligent sleuthing he came to realize there must have been offline communication between Briffa and Mann’s allies Eugene (Gene) Wahl and/or Caspar Ammann, and on May 27, 2008, he submitted a Freedom of Information (FOI) request to the University of East Anglia to get the IPCC AR4 review emails. On May 29, apparently aware that the FOI request had been submitted, Jones emailed Mann:

Mike,

Can you delete any emails you may have had with Keith re AR4?

Keith will do likewise. He's not in at the moment - minor family crisis.

Can you also email Gene and get him to do the same? I don't have his new email address.

We will be getting Caspar to do likewise.

Mann replied right away:

I'll contact Gene about this ASAP. His new email is: xxxx@yahoo.com
talk to you later,
mike

On June 4, Jones [wrote to Jean Palutikoff](#), then of the IPCC Technical Support Unit, that IPCC Review Editor John Mitchell “conveniently lost many emails” but had to turn some over in response to a FOI request, whereas Briffa and Osborn “have moved all their emails from all the named people off their PCs and they are all on a memory stick.”

The Penn State Inquiry Committee did not refer the email deletion allegation to the Investigation. Once again, it disposed of it based only on a preliminary investigation - without complying with Office of Research Integrity policies. Although Mann’s forwarding of Jones’ request to Wahl fell squarely within the terms of Pell’s question 2, the Inquiry Committee dismissed the allegation without obtaining evidence from Wahl. (Later, the incident was also explored by NOAA, by whom Wahl was then employed. NOAA took no action on the grounds that Wahl’s destruction of emails took place a few months before he became a NOAA employee.)

The Inquiry Committee took falsification and email destruction issues off the table but did recommend an Investigation regarding potential violations of Policy AD47 on professional conduct. The Investigation Committee did not interview us or other critics familiar with Mann’s work, instead interviewing Mann’s supporters or uninvolved bystanders. It did not mention Policy AD47. Instead it asked puffs such as:

Do you believe that the perceived hostility and perceived ulterior motives of some critics of global climate science influenced your actions with regard to the peer review process, particularly in relation to the papers discussed in the stolen emails?

The investigation noted that Mann had been very successful at obtaining grants and cited that success as supposed proof that Mann’s conduct was within professional standards.

Writing in the Atlantic, Clive Crook later expressed incredulity at the Penn State inquiry as well as all the others.

I think climate science points to a risk that the world needs to take seriously. I think energy policy should be intelligently directed towards mitigating this risk. I am for a carbon tax. I also believe that the Climategate emails revealed, to an extent that surprised even me (and I am difficult to surprise), an ethos of suffocating groupthink and intellectual corruption....

I had hoped, not very confidently, that the various Climategate inquiries would be severe. This would have been a first step towards restoring confidence in the scientific consensus. But no, the reports make things worse. At best they are mealy-mouthed apologies; at worst they are patently incompetent and even wilfully wrong. The climate-

science establishment, of which these inquiries have chosen to make themselves a part, seems entirely incapable of understanding, let alone repairing, the harm it has done to its own cause.

The Penn State inquiry exonerating Michael Mann -- the paleoclimatologist who came up with "the hockey stick" -- would be difficult to parody. ... In short, the case for the prosecution is never heard. Mann is asked if the allegations (well, one of them) are true, and says no. His record is swooned over. Verdict: case dismissed, with apologies that Mann has been put to such trouble.

Much later, the Inspector General of the National Science Foundation ("NSF") stated that NSF had instructed the Committee in writing to "review sufficient relevant documents and interview a sufficient number of knowledgeable individuals who may provide credible information about the allegations," and found that, despite this instruction, the Inquiry Committee had not "adequately review[ed]" falsification allegations and failed to "interview any of the experts critical of [Mann's] research."

Penn State President Graham Spanier [praised](#) the work of the Inquiry Committee at a meeting of the University Board of Trustees on Jan 22, 2010 [as follows](#):

I know they've taken the time and spent hundreds of hours studying documents and interviewing people and looking at issues from all sides.

Spanier's assurance to the Board of Trustees was false: the Inquiry Committee had made no effort to "look at issues from all sides". On the contrary, the Inquiry Committee carried out zero interviews with critics nor had it looked at issues "from all sides".

In 2012, Spanier and university counsel Courtney (who had advised the Inquiry Committee on procedure) were both fired for negligence in a different misconduct scandal, with Spanier additionally receiving a [criminal conviction](#) and jail time.

Since the UK investigations limited their remit to UK persons, none of them investigated the longstanding controversies related to Michael Mann's hockey stick, including the issues discussed above.

UK House of Commons Inquiry

The UK House of Commons Science and Technology Committee sent a letter to the University of East Anglia (UEA) on 1 December 2009 requesting an explanation regarding what had taken place and the steps the university was following to investigate the matter. The university responded by promising two inquiries, one (Muir Russell) to investigate the allegations against the scientists and one (Oxburgh) to examine "important elements of the published science" of the CRU.

The Commons Committee decided to conduct its own inquiry and issued a call for evidence. They then held a single hearing with 5 panels of witnesses. The only critics of the CRU who were invited to appear were Nigel Lawson and Benny Peiser of the Global Warming Policy Foundation, who were uninvolved with and had no firsthand knowledge of the Climategate issues.

This hearing was Phil Jones' only public appearance during the crisis and exposed Jones to hard-bitten political reporters who were more searching than the domesticated environmental reporters that Jones was used to. Quentin Letts was [so unimpressed](#) by Jones that he expressed his personal hope that "politicians sought second, third, even 20th opinions before swallowing his theories and trying to change the world's industrial output" (emphasis added):

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.**

Other reporters on the hearing were [equally severe](#).

The Committee's investigation was cut short by an election campaign. The only falsification issue they considered was the "trick" email, on which the committee split 3-1. Even on this point, the report was not "unequivocal"; the Committee stated its expectation that the Science Panel would "address" the matter.

The Parliamentary Committee was particularly concerned about email-deletion charges, as a former Information Commissioner [had stated](#) that it was "hard to imagine more cogent prima facie evidence" of an offence under the Freedom of Information Act, but that it was outside the jurisdiction of the Information Commissioner because of a very short (6 months) statute of limitations from the time of the offence (rather than time of discovery):

The prima facie evidence from the published emails indicate an attempt to defeat disclosure by deleting information. It is hard to imagine more cogent prima facie evidence. Given that this was in the public domain and has been discussed in the media and on various websites over a number of weeks, the ICO's view, as I indicated when we spoke yesterday, is that the University must have understood that the question whether an offence under section 77 had been committed would be looked at. In the event, the matter cannot be taken forward because of the statutory time limit.

Notwithstanding this limitations period, the Committee directed Muir Russell and the Information Commissioners' Office to ensure that the matter was "resolved conclusively" stating that "much of the reputation of CRU hangs on the issue." The Muir Russell inquiry ignored this and other directions from the Parliamentary Committee (see below). Frustrated, the Parliamentary Committee recalled Muir Russell and Vice-Chancellor Acton to obtain an explanation. MP Graham Stringer discovered that Muir Russell had not even **asked** Jones about the email deletion controversy. Muir Russell, who had just made this admission, [said](#) that he not done so because it would have been asking Jones whether he had committed a crime:

Q (Graham Stringer): I find it a bit surprising, that you didn't ask directly when a lot of the controversy had been about the request to delete e-mails. You didn't personally ask Professor Jones ...directly whether he had deleted those emails?

Sir Muir Russell: That would have been saying, "Did you commit a crime?", and we would have had to go into a completely different area of the relationship and formal role for the inquiry.

Vice Chancellor Acton then stepped in and claimed that he had asked the question that Muir Russell had failed to ask and (falsely) re-assured the Committee that nothing had been deleted after all – notwithstanding several emails directly exchanging information on deletion. The Committee more or less [threw up its hands](#) in frustration with the university, closing their investigation without resolving the email destruction question that had originally concerned them:

We find it unsatisfactory that we are left with a verbal reassurance from the Vice-Chancellor that the e-mails still exist. He told us "Can those e-mails be produced? Yes, they can. Did those who might have deleted them say they deleted them? No. They say they did not".

MP Stringer [spoke out](#) in frustration, echoing Clive Crook's description of the Penn State inquiry: that it was "beyond parody":

In a situation that is almost beyond parody Muir Russell stated that he didn't ask Jones whether he had deleted the e-mails because they would have had to interview Jones under caution. What was the solution then? The Vice Chancellor asked Jones whether he had deleted the e-mails. This rather negated the purpose of having an independent Inquiry when the only person to ask the crucial question was the Vice Chancellor who saw his prime responsibility to the good name of the University. The accused investigating themselves again.

Subsequently, it turned out that Vice Chancellor Action's reassurances that nothing had been deleted were untrue. Following up on that supposed reassurance, one of us (McIntyre) submitted an FOI request for the attachments to the emails which Jones had sought to destroy, attachments which contained the surreptitious and hidden edits to the IPCC report by Mann's associates **after** close of external review. The University reported that the documents about which Acton had reassured the Committee did not exist after all.

The Committee's inquiry was severely criticized. *The Guardian* stated that the "climate inquiry ha[d] dodged key questions," while another observer found that "[t]he aim of the MPs' investigation was not to uncover the truth, but to defend the moral authority of climate-change alarmism."

Oxburgh Inquiry

In February 2010, in press releases and in their written submission and oral testimony to the Parliamentary Committee, the University of East Anglia announced their intention to form an inquiry to re-appraise CRU's science. This resulted in the *Science Appraisal Panel*, chaired by Lord Oxburgh, a panel which demonstrated its independence of the University by the creation of a logo combining the House of Lords insignia with the University Registrar's email address. Oxburgh was CEO of the Carbon Capture and Storage Association and Chairman of Falck Renewable Resources, both companies with strong vested interests in promoting climate policy, and UK Vice-Chair of GLOBE International, an industry-NGO-government consortium that lobbies for global warming policy.

The Oxburgh inquiry did not interview any CRU critics, nor, in their interviews with Jones and Briffa, did they take transcripts or minutes. Jones and Briffa were interviewed on April 7 and the morning of April 8; Oxburgh wrote the report on the afternoon of the 8th and it was released a few days later.

Soon afterwards, McIntyre learned from a reliable source that Jones had apparently admitted in his interview that it was "probably impossible" to do the 1000-year reconstructions with any accuracy", a key admission was not included in the Oxburgh report though it was the most contentious issue in the entire Hockey Stick debate. I wrote to Oxburgh, requesting that he issue an addendum recording this important information. He refused, saying that "the science was not the subject of his inquiry". On learning of that the remit of the *Science Appraisal Panel* did not include "the science", Phil Willis, former chairman of the Parliamentary Committee, [accused](#) the University of "sleight of hand". MP Graham Stringer called it a "whitewash".

The Commons Committee called Oxburgh in and asked why he hadn't done the review they had been promised. He said, in effect, because he was the wrong guy to ask for such a report.

If you wanted to validate the science, you would have a different panel. You wouldn't appoint me as chairman. You'd appoint experts from the field. It's a very different activity. I was quite clear that what we took on was to look at the integrity of the researchers.

Oxburgh [later said](#) that the announcement by the UEA about him providing an "independent external reappraisal of the science" was "inaccurate."

The 11 papers selected for examination by the Oxburgh team were [never ones](#) that had been controversial. The list also omitted the paleoclimate papers that had been subject to controversy, such as the Tornetrask and Yamal papers by Keith Briffa, and even all the 'hockey stick'-related paleoclimate papers from CRU. By focusing only on journal articles, the Oxburgh panel avoided the key question of whether CRU staff had suppressed uncertainties in WMO and IPCC Reports. Oxburgh sought to absolve the CRU scientists of doing so by pinning the blame on a nameless group of others who were guilty of omissions and oversimplifications:

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. For example, 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.

It apparently didn't occur to Oxburgh that the "IPCC and others" were the CRU scientists themselves acting in their alternate capacity as report-writers.

The [notes](#) of Panelist Michael Kelly (Professor of Physics) were among documents eventually obtained through FOIA requests. They indicate that he was far more critical in private than the text of the report conveyed.

Up to and throughout this exercise, I have remained puzzled how the real humility of the scientists in this area, as evident in their papers, including all these here, and the talks I have heard them give, is morphed into statements of confidence at the 95% level for public consumption through the IPCC process. This does not happen in other subjects of equal importance to humanity, e.g. energy futures or environmental degradation or resource depletion. I can only think it is the 'authority' appropriated by the IPCC itself that is the root cause.

Our review takes place in a very febrile atmosphere. If we give a clean bill of health to what we regard as sound science without qualifying that very narrowly, we will be on the receiving end of justifiable criticism for exonerating what many people see as indefensible behaviour. Three of the five MIT scientists who commented in the week before Copenhagen on the leaked emails, (see <http://mitworld.mit.edu/video/730>) thought that they saw prima facie evidence of unprofessional activity.

Muir Russell Inquiry

The Muir Russell inquiry held no public hearings and did not interview any CRU critics, relying instead on written submissions under a very short deadline. Roger Harrabin of the BBC reported that panel members had told him that, if they wanted to hear from McIntyre, they could read his blog. Muir Russell purported to excuse their failure to interview critics on the following theory of "natural justice":

We recognise that natural justice requires that those in respect of whom findings will be made should have an opportunity to be heard: this does not apply to the authors of submissions and other parties,

We doubt that many people will agree that this is a plausible interpretation of natural justice, but, regardless, it was not a sensible approach for an inquiry purporting to put issues to rest or with the initial expectations of the inquiry and ultimately undermined any credibility.

Although Muir Russell had been appointed in December 2009 and was due to report in spring 2010, as of the start of April, nobody at CRU had been interviewed on anything to do with the Hockey Stick or IPCC. The one and only Muir Russell interview with Jones and Briffa on the Hockey Stick and IPCC took place on April 9 (a few days after the Oxburgh interview). However, Muir

Russell (and two other panelists) didn't bother attending these interviews. Nor were the interviews recorded or transcripts taken. In fact, Muir Russell does not appear to have even met with Jones or Briffa after the unveiling of the Muir Russell panel in February.

One of the Muir Russell panel's assignments was examination of other emails on the CRU server. However, as of the beginning of April, Muir Russell had taken no steps to do so. When he then attempted to do so, the University, which had previously promised total independence, now negotiated conditions on Muir Russell's access, resulting in further delay. A forensic analyst hired by the university got access on May 14 and reported three days later that it would take several weeks to do an analysis. With the report deadline looming, the analysis was abandoned, sabotaged by the combined inertia of Muir Russell and the university.

Muir Russell on Email Deletion

On May 27, 2008, David Holland had submitted his FOI request covering Briffa's back-channel emails that circumvented the IPCC review process. The next day, Phil Jones emailed his CRU associates and university FOI officers to say that Briffa should say that there was no such correspondence. Then, Jones emailed Mann and other associates requesting that they delete all such back-channel communications. The Information Commissioner stated in February 2010 that it was impossible to contemplate "more cogent prima facie" evidence of an offence. But despite obvious facts known to thousands of people familiar with the story, Muir Russell *made the plainly untrue finding that there had been no deletion of emails requested under FOI*. The public may not be able to judge the fine points of principal components, but they were well able to judge this sort of fiasco. Fred Pearce wrote in the Guardian:

[t]his is all, we may hope, cock-up rather than conspiracy. . . . None of the inquiries have cleared the air."

Muir Russell on Hide the Decline

The review swept aside the whole issue of the deletion of Briffa's post-1960 data in the IPCC 3rd Assessment Report by saying they would only look at the 4th Assessment Report. The Briffa deletion was done in that report too but Muir Russell dismissed that by noting that it was discussed in the text. They took no note of the fact that the only reason it *was* discussed in the AR4 was as a result of Steve's protests as IPCC reviewer. It was not voluntarily done by the CRU authors, as was explained in the evidence, since early IPCC drafts did not provide the discussion. Moreover, the whole defence is irrelevant to the original case of the IPCC Third Assessment Report where it was not disclosed, and it contradicts the validity of a report for policy makers to have key evidence of the weakness of a statement in the Summary buried deep in the body of the report.

In respect to IPCC Figure 2.21, the WMO Report cover and the "trick" email, Muir Russell contradicted the Penn State and Parliamentary Committee reports. It found that the figures were "misleading" and that there was "evidence of intent to paint a misleading picture."

Other Findings

The Muir Russell inquiry did not address any of the MBH98 falsification issues, as its terms of reference were limited to actions by University of East Anglia employees.

The Muir Russell's review's other findings, including Jones' involvement in a plot with Kevin Trenberth to keep Ross' (and others') papers on contamination patterns in surface temperature data out of the IPCC reports and to fabricate evidence that they were statistically insignificant, followed a pattern of bending over backwards to make excuses for the CRU, or focusing on irrelevancies, or taking CRU excuses at face value without subjecting them to cross-examination.

MYTH #3: MANN HOCKEY STICK "CONFIRMED"

In the past 10 years, Climategate apologists have propagated the myth that the Mann Hockey Stick has been "verified" or "confirmed" by multiple **independent** studies, most notably the PAGES2K multiproxy networks (2013, 2017 and 2019), and that the original controversy is therefore entirely moot. This is untrue.

We discussed the reliance on the Yamal series in the introduction. Going further, there has been no scientific progress in this field since Climategate. Instead, during the past 10 years, climate academics have doubled down on the defective techniques exposed in Climategate emails:

- PAGES2K and similar studies remain primarily dependent on problematic and inconsistent tree ring data, many of which go down in the last half of the 20th century. In order to extract a Hockey Stick shape from inconsistent tree ring data, climate academics, including PAGES2K, have resorted to ad hoc methods (ex post screening, ex post orientation) which are condemned by mainstream statisticians and in statistical literature, but which enhance the hockey stickness of the resulting reconstruction. The ex post screening and manipulation even extends to data used in seemingly technical reports
- Use of tree ring widths as a temperature proxy is made even more problematic by the impact of the extraordinary worldwide "[greening](#)" during past 30 years, primarily attributed by specialists to carbon dioxide fertilization, on ring widths – an effect which is not disentangled in PAGES2K.
- The controversial stripbark bristlecone series, relied upon by Mann et al 1998, continue to be used in PAGES2K reports, even though the 2006 NAS panel recommended that such data be "avoided" in temperature reconstructions.
- In their zeal to obtain a hockey stick, PAGES2K authors, like Mann et al 2008, have introduced sediment series without taking care to ensure a physical link, leading to a series of embarrassing gaffes arising from series contaminated by construction run-off and even used upside down.

Ex Post Screening/Orientation of Inconsistent Tree Ring Data

Even before Climategate emails, there had been considerable controversy over Mann-style reconstructions using tree ring data.

As early as 2006, Valerie Masson-Delmotte, an eminent scientist and now the chair of IPCC Working Group 1, told us privately that she found persuasive our critique of multiproxy temperature reconstructions, primarily relying on tree ring data, and that, in her opinion, the only

way forward for the field was the development of new and better proxies – a process that, in her opinion, could easily take 20 years. Although 14 of the 20 years have now passed, the PAGES2K multiproxy reconstructions continue to predominantly rely on tree ring data. Tree ring chronologies made up 83% of the PAGES2K (2013 network used in IPCC AR5 (PAGES 2017 – 60%).

If there were a consistent “signal” in tree ring site “chronologies” (ring width time series), the “signal” could be easily recovered by simple averaging. But that’s not what was done in PAGES2K, which has thrashed about frenetically in the impossible task of extracting temperature estimates from inconsistent tree ring data.

PAGES2K North American Network

In their North American network (an important dataset which contained the stripbark bristlecones), there were 146 supposedly carefully-selected tree ring chronologies in the 2013 network. But only 4 years later they threw out 124 of them and brought in 129 new ones – see [Table 1 below](#). The turnover did not result from new series becoming available between 2013 and 2017 (other than a couple), but from different systems of ex post screening and orientation.

**Table 1. North American Tree Ring Proxy Network,
PAGES 2013 and PAGES 2017**

	PAGES (2013)	PAGES (2017)	Combined
PAGES 2013 only	124	.	124
PAGES 2017 only	.	129	129
Both	22	22	22
TOTAL	146	151	275

The reason for the rejection of so many PAGES2K (2013) chronologies was the introduction of the following ex post screening criterion in the 2017 network (emphasis added):

To be included in the current database, tree-ring data were required to **correlate positively ($P < 0.05$) with local or regional temperature** (averaged over the entire year or over the growing season).

In other words, 85% of the “carefully chosen” North American tree ring chronologies used in PAGES2K (2013) did **not** correlate positively with temperature, even with four chances (entire year or growing season, local or regional). Some of these series came from the sites used in the original Briffa reconstruction – with its disquieting decline in the last half of the 20th century.

In order to replace these 124 chronologies, PAGES2K (2017) authors foraged through the ITRDB database for replacement chronologies with a positive correlation, locating 129. They did not report the number of chronologies which they canvassed (though this is highly relevant for determination of statistical significance). There are approximately 3100 North American tree ring measurement datasets in the ITRDB database, from which 151 (4.9%) were selected as having a

$P < 0.05$ correlation with temperature – approximately the same percentage as one would expect from random data.

Remarkably, the majority of tree ring chronologies which were used in both PAGES2K (2013) and PAGES (2017) were the controversial [stripbark bristlecone](#) chronologies, which had imprinted the Hockey Stick shape on the Mann et al 1998 reconstruction and which the 2006 National Academy of Sciences had specifically said to “avoid” in temperature reconstructions. Thus, in respect to these most controversial proxies, the PAGES2K were not **independent**, but continued to rely on the same data which had caused earlier controversy.

Effect of Ex Post Screening

While screening on the basis of correlation to temperature superficially seems to make sense, the error is easily understood if you hypothesize a pharmaceutical scientist using ex post screening: imagine a drug study which only reported results for patients who got better. Or a financial fund manager who only reports the investments that go up in value. Such a technique would be risible and the results misleading. The fallacy, in broader statistical literature, is commonly called *screening on the dependent variable*; studies which use this fallacious technique have no statistical validity.

There is nothing wrong with hypothesizing *ex ante* that (for example) black spruce ring widths at treeline sites are a temperature-sensitive proxy. But once you have done so, you take your sample of all the trees you believe to be suitable and use all the resulting data. If you exclude sites in which ring widths go down in the 20th century, you can't use the fact that the resulting series goes up in the 20th century as proof of anything, because such a technique will impart a hockey stick shape even to data which, on average, has no trend.

The bias of ex post screening is illustrated in the diagram below³, showing four series, which, when averaged, produce a straight line. But when screened according to whether the series go up in the 20th century (red circle), their average is a hockey stick.

³ h/t Charles Rotter

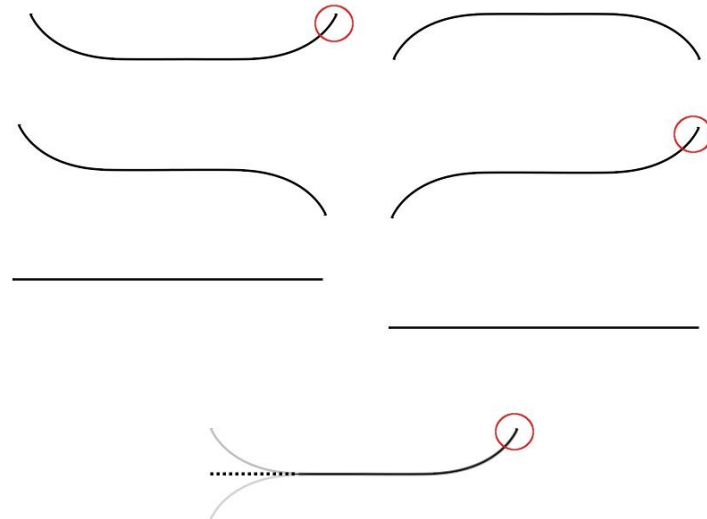


Figure 4. Diagram showing effect of ex post screening

The pernicious effect of ex post screening has been pointed out over and over on skeptic blogs and noted in McIntyre and McKittrick (PNAS, 2010), but has been vigorously denied by multiproxy authors. However, the point was privately conceded in one of the Climategate [email threads](#) (emphasis added):

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 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**

However, that private admission has been ignored by PAGES2K and IPCC authors.

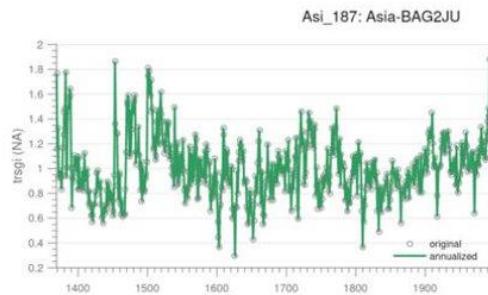
Ex post screening is not the only method by which hockey stick reconstructions can be produced from random data (or slight hockey-stickness enhanced to a big blade.) PAGES2K (2013) had used a different but equally problematic technique: they determined the sign (orientation) of each individual chronology by the sign of ex post correlation to temperature. If the series went down in the 20th century (negative correlation to temperature), they inverted the sign of the series.

Industrialized Cherry Picking in PAGES2K (2019)

PAGES2K (2019) carried out even more ruthless ex post screening of the PAGES2K (2017) proxy network. When examined in detail, the decision-making consistently lacked ex ante justification. A few examples will be shown.

The 2017 Asia tree ring network contained two nearby juniper (JUSP) series from the Bagrot Valley in Pakistan only 450 meters apart in altitude. Ex ante, the higher altitude series would be expected to be more temperature sensitive and a superior proxy for temperature. However, the higher (3750 m) series went down in the late 20th/early 21st century, while the lower altitude (3300 m) series went up. Needless to say, it was the lower altitude series which was included by PAGES2K (2019) authors.

Bagrot Valley JUSP, 3300 m



Bagrot Valley JUSP, 3750 m

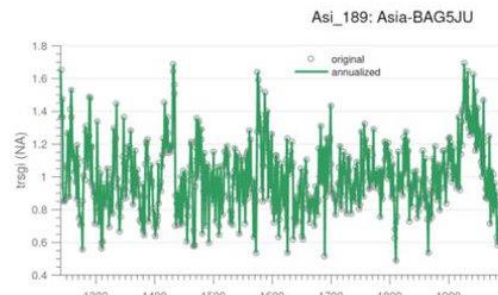


Figure 5. Two Pakistan tree ring chronologies from same location. LEFT: used in PAGES2K(2019), RIGHT: omitted from PAGES2K (2017).

One of the most dramatic individual hockey stick shaped series in PAGES2K (2019) resulted from manipulation (ex post screening) of data at a site level to manufacture a hockey stick shape⁴. In this study, the authors constructed a “divergence-free” chronology in which they manually excluded individual trees which had decreasing ring widths in the 20th century, leaving only trees with increasing ring widths (“positive responders”). Unsurprisingly, the composite, later used by PAGES2K, had a hockey stick shape that was not present in the overall data.

⁴ Porter, T.J et al, 2013. *Quaternary Research*, 80(2), 167-169.

https://tspace.library.utoronto.ca/bitstream/1807/73470/1/ring_width_based_reconstruction.pdf

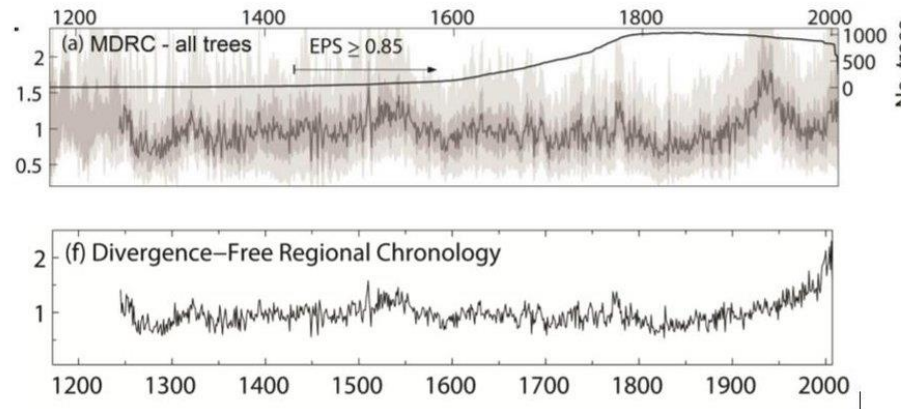


Figure 6. Excerpt from Porter et al, 2013, comparing all-tree and "divergence-free" chronologies

Greening and Carbon Dioxide Fertilization

One of the most astounding developments since Climategate emails is the Nyneni et al (2016) report of dramatic “greening” of virtually the entire globe over the past 30 years, amounting to a “an increase in leaves on plants and trees equivalent in area to two times the continental United States.”

From a quarter to half of Earth’s vegetated lands has shown significant greening over the last 35 years largely due to rising levels of atmospheric carbon dioxide, according to a new study published in the journal *Nature Climate Change* on April 25. An international team of 32 authors from 24 institutions in eight countries led the effort, which involved using satellite data from NASA’s Moderate Resolution Imaging Spectrometer and the National Oceanic and Atmospheric Administration’s Advanced Very High Resolution Radiometer instruments to help determine the leaf area index, or amount of leaf cover, over the planet’s vegetated regions. The greening represents an increase in leaves on plants and trees equivalent in area to two times the continental United States.

The increase in leaf area was [primarily attributed](#) to carbon dioxide fertilization.

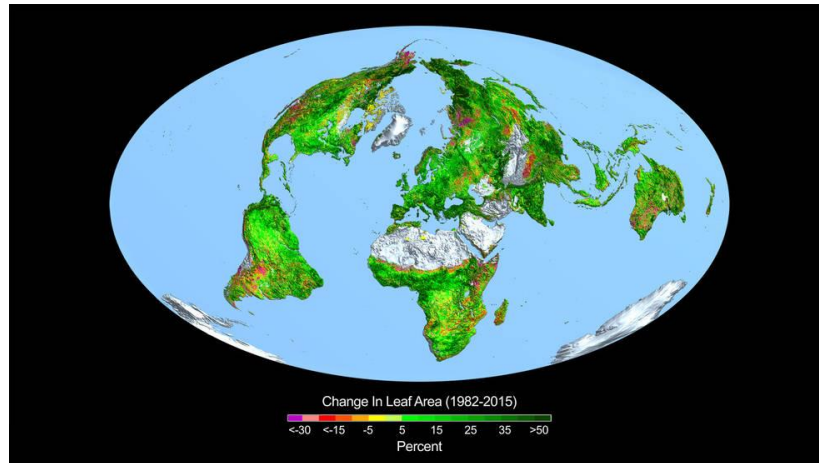


Figure 7. Change in leaf area 1982-2015. From Nyneni et al (2016)
<https://www.nasa.gov/feature/goddard/2016/carbon-dioxide-fertilization-greening-earth>

In order to use tree ring widths as a consistent proxy for *temperature*, it would be necessary to somehow disentangle the direct impact of carbon dioxide on growth from the indirect impact via increased temperature. This issue has been entirely neglected in the PAGES2K articles.

Ironically, Lamarche et al (1984)⁵, one of the earliest, if not the earliest, analyses of carbon dioxide fertilization, based their analysis on the remarkable increase in 19th and 20th century ring widths in stripbark bristlecones. This analysis was further developed in Graybill and Idso (1993), which introduced many of the distinctive Hockey Stick shape stripbark bristlecone chronologies, which later imprinted the Mann et al 1998 reconstruction, as discussed at length in our 2005 *Environment and Energy* article. They postulated that the impact of carbon dioxide fertilization was particularly dramatic in the very high (thus even lower CO₂) and very dry locations of bristlecone pines. We are inclined to think that the distinctive increase in stripbark ring widths in late 19th and early 20th century may result from mechanical deformation resulting from stripbark initiation in the mid-19th century⁶, but either issue is fatal to their use as a temperature proxy.

Statistical “Skill”

Finally, no subsequent study, however constructed, can repair the false assertions that the MBH98 reconstruction possessed statistical “skill” and “robustness” - assertions which led to its widespread acceptance. The failed verification r² values of the MBH98 reconstruction are a permanent monument to its lack of statistical “skill”. Subsequent reconstructions have refrained from similar grandiose claims, but, even if they did, that would not rehabilitate the false

⁵ Lamarche et al, 1984. Increasing Atmospheric Carbon Dioxide: Tree Ring Evidence for Growth Enhancement in Natural Vegetation. *Science* 225, 1019-1021; Graybill, D. and S. Idso, 1993. Detecting the aerial fertilization effect of atmospheric CO₂ enrichment in tree-ring chronologies. *Glob Biogem Cycles*. <http://www.climateaudit.info/pdf/others/graybill.idso.1993.pdf>

⁶ <https://climateaudit.org/2007/10/23/more-on-almagre-tree-31/>;
<https://climateaudit.org/2007/10/17/almagre-strip-bark/>

assertions about the MBH98 reconstruction, any more than any future cloning would rehabilitate the false cloning claims published in *Science* in 2006.

CONCLUSION

The rhetorical importance of the Hockey Stick itself is perhaps best summarized in one of the Climategate emails. In September 2005, the BBC⁷ came to East Anglia to interview Keith Briffa for a documentary they were preparing on climate change (shown in 2006 as “Meltdown”). In preparation, producer Jonathan Renouf [explained](#) that their narrative proposed to use the Mann Hockey Stick to “prove” that the recent climate change is “NOT just another of those natural fluctuations we've seen in the past” and would be used to stage a phony “conversion” of journalist Paul Rose from a doubter to a believer in climate change. Renouf:

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.

The Meltdown documentary⁸ showed interesting evidence of past warm periods, including long abandoned Bronze Age settlements on presently inhospitable Dartmoor. Narrator Rose purported to seriously consider Bronze Age warmth, then segueing to consideration of the Medieval Warm Period, before being “persuaded” by the Hockey Stick curve that “present warming is unprecedented in speed and amplitude, something like that”.

Briffa himself was privately dubious about the Mann Hockey Stick but was prepared to suppress his doubts in the name of public persuasion, always acquiescing to Mann’s bullying.

⁷ <https://wattsupwiththat.com/2011/11/27/climategate-2-impartiality-at-the-bbc/>

The BBC aired a program called Meltdown

http://www.youtube.com/watch?v=qkhJQDD1_ys&feature=watch_response;

<https://www.youtube.com/watch?v=ztgcK85EqPw;>

http://www.bbc.co.uk/pressoffice/pressreleases/stories/2006/02_february/14/climate.shtml

⁸ <https://www.youtube.com/watch?v=4GshAUZteZc&t=19s>

Climategate did not arise from a few emails being taken “out of context”. It was exactly the opposite. The emails provided behind-the-scenes and very disquieting **context** for troubling statistical and scientific practices which had, for the most part, already been identified by us and others in published articles in scientific journals and blogs.

The contemporary whitewashing and ultimate sanitization by climate academics is itself an interesting and mostly untold story. Climategate exposed bad practices; the fake inquiries whitewashed them, and now the story is being retold so the villains are not only innocent but are to be embraced as heroes. It is an almost classic example of what Alexander Pope famously observed in his *Essay on Man* (1733) nearly three centuries ago:

Vice is a monster of so frightful mien
As to be hated needs but to be seen;
Yet seen too oft, familiar with her face,
We first endure, then pity, then embrace.