Response to Revised Report from Deutsche Bank

Ross McKitrick
University of Guelph
Guelph ON N1G 2M5

November 8, 2010

1. Introduction

Deutsche Bank Group published a report purporting to respond to skeptic arguments. I took issue with a number of points in that document, and DB has responded with a revision to its original report (available at http://www.dbcca.com/dbcca/EN/_media/DBCCAColumbiaSkepticPaper090710.pdf) accompanied by 2 weblog entries at http://blogs.ei.columbia.edu/tag/climate-matters. I will discuss their response, indicating where some meeting of minds has occurred, as well as places where I think it falls short; also I would like to raise a few points not taken up earlier.

The original authors of the DB report were Mary-Ellen Carr, Robert F. Anderson and Kate Brash. The response was provided to me by the same three, via Madeleine Rubenstein, Research Coordinator at the Columbia Climate Center, who wrote the web entries. I will refer to this team as CABR for brevity. I appreciate the willingness of CABR to engage in this exchange of views.

2. Hockey Stick Controversy

CABR have graciously conceded some points concerning their summaries of the NAS and Wegman reports (p. 35):

> Professor McKitrick is correct that we mischaracterized the conclusions of the reports by the National Academy of Science (2006) and by Wegman et al (2006) with regards to the MM critiques of the publications of Mann, Bradley, and Hughes (1998, 1999, hereafter MBH).

They have agreed to amend some of their conclusions. However they have kept the original wording in the report itself, putting the amendments in an appendix. I would call this an unsporting way to amend a report: it would be better to change the report itself, perhaps with a footnote explaining the correction, rather than leaving the original text in place and putting the revision in an appendix that readers might not even read.

The Point of the Hockey Stick Chapter

In my earlier response I stated

> The DB refers to both Mann et al. hockey stick papers (the ones in *Nature* and *Geophysical Research Letters*) as well as a 2005 paper by Rutherford et al. But despite supposedly presenting a rebuttal of Steve's and my work on the hockey stick, the DB paper fails to cite our main publications (our 2005 *Geophysical Research Letters* and *Energy and Environment* papers) nor does it provide any summary of what those papers argued.

CABR argued that my criticism is misplaced since they did not intend to provide a review of the hockey stick controversy *per se*, merely to use it as an example of the perils of politicization. They state on their website (emphasis added):

> The claims that we addressed in this report are listed in both a table of the editorial section and in the executive summary- the hockey stick is not mentioned among these. This is because **our aim was not to rebut McIntyre and McKitrick's work (hereafter MM), nor to endorse the Mann, Bradley and Hughes studies (hereafter MBH), nor to summarize the bulk of the research of those who disagree or agree with critiques of the hockey stick.** As expressed on page 10 of the DBCCA/CCC report, the introductory section on the hockey stick controversy aimed to provide the reader with a historical overview of some events surrounding an "example controversy."

In the appendix of their revised report (p. 35) they make a similar point. However, regardless of their intention, their original report did attempt a rebuttal to the MM material, and did endorse the Mann et al. material. Having chosen to devote an entire chapter to what amounted to a brief for one side over the other, they are not in a position to say it was never their intention to do so. They are, of course, entitled to their point of view. But they should recognize that it limited the quality of argumentation they presented. To give one example, they said:

> Subsequent attempts to analyze, critique, and reproduce Mann et al's results have led to adjustments and refinements of the technique, while attempts to reproduce the work of McIntyre and McKitrick have shown their original claims to be largely spurious **(Rutherford et al. 2005)**.

This paragraph, which remains in the new version, depends on Rutherford et al. as the basis for a rather sweeping conclusion. But they do not mention that Rutherford's coauthors were Mann, Bradley and Hughes themselves, making it invalid to depend on it as independent confirmation of Mann, Bradley and Hughes' earlier work.

"Attempts to reproduce" MM

In addition, the phrasing of the paragraph implies that people have had difficulty reproducing our findings. Yet our data and code have been made available since the beginning. For example, the NAS report (p. 87) presented its own replication of the artificial hockey stick effect based on our submission:

STATISTICAL BACKGROUND 87

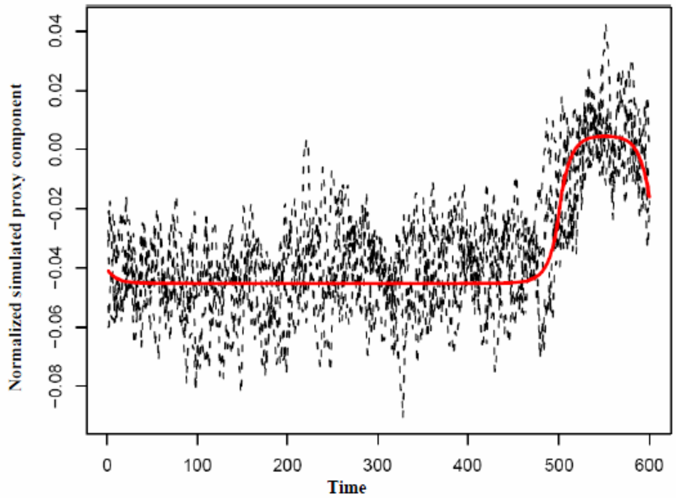


FIGURE 9-2 Five simulated principal components and the corresponding population eigenvector. See text for details.

The NAS (p. 86) described how they used our method to replicate the effect as follows:

> McIntyre and McKitrick (2003) demonstrated that under some conditions, the leading principal component can exhibit a spurious trendlike appearance, which could then lead to a spurious trend in the proxy-based reconstruction. To see how this can happen, suppose that instead of proxy climate data, one simply used a random sample of autocorrelated time series that did not contain a coherent signal. If these simulated proxies are standardized as anomalies with respect to a calibration period and used to form principal components, the first component tends to exhibit a trend, even though the proxies themselves have no common trend. Essentially, the first component tends to capture those proxies that, by chance, show different values between the calibration period and the remainder of the data. If this component is used by itself or in conjunction with a small number of unaffected components to perform reconstruction, the resulting temperature reconstruction may exhibit a trend, even though the individual proxies do not. Figure 9-2 shows the result of a simple simulation along the lines of McIntyre and McKitrick (2003) (the computer code appears in Appendix B).

Likewise the Wegman team stated (p. 47, emphasis added)

> In general, we find the criticisms by MM03, MM05a and MM05b to be valid and their arguments to be compelling. **We were able to reproduce their results** and offer both theoretical explanations (Appendix A) and simulations to verify that their observations were correct.

There is no hint that these teams found our claims to be "largely spurious."

Likewise the Wahl&Ammann papers cited by CABR are based on an identical reproduction of the results generated by Steve McIntyre's R code, which the authors were able to access directly, since it was published on the internet.

The conclusions of the various authors who have disputed our critique of the hockey stick were never based on difficulty reproducing our findings. They were always based on a different interpretation of the findings.

NAS Conclusions

On the question of what has been upheld or overturned in the MBH98/99 conclusions, it is important to understand that Mann et al. did not merely claim that they could produce a multiproxy reconstruction that yielded a relatively warm late-20th century. Since instrumental data were being combined with proxy records, such a construction would, on its own, be unremarkable, unless the model could be shown to have significant skill for extrapolation, as measured by cross-validation tests. What made the MBH claim noteworthy was (as emphasized in the 2001 IPCC Report, p. 233) that they produced a reconstruction "which had significant skill in independent cross-validation tests." We argued that their results lacked robustness since they depended on use of bristlecone pines, that the early portions of the graph yielded unreported cross-validation scores that in fact indicated statistical insignificance, and that the reported score using the RE statistic was incorrectly benchmarked. The NAS accepted the argument that bristlecones are unreliable for reconstructions and should be avoided, and they acknowledged that the MBH results, as reproduced by Wahl and Ammann, did not achieve statistical significance. This was stated in the quote that I presented in my original response (NAS p. 91):

> Reconstructions that have poor validation statistics (i.e., low CE) will have correspondingly wide uncertainty bounds, and so can be seen to be unreliable in an objective way. Moreover, a CE statistic close to zero or negative suggests that the reconstruction is no better than the mean, and so its skill for time averages shorter than the validation period will be low. Some recent results reported in Table 1S of Wahl and Ammann (in press) indicate that their reconstruction, which uses the same procedure and full set of proxies used by Mann et al. (1999), gives CE values ranging from 0.103 to –0.215, depending on how far back in time the reconstruction is carried.

Consequently they did not find our work to be "largely spurious", instead they reached the same conclusions: they just expressed them in an obscure and elliptical way.

In the website accompanying their revised report, CABR say that a quote I used from the NAS report was truncated:

> Professor McKitrick has truncated this quote and omitted the paragraph's significant conclusion: that while the methodology introduces a tendency to bias, that the bias does not invalidate the conclusion of the MBH studies. The NAS report goes on to conclude (emphasis added):
>
> "As part of their statistical methods, Mann et al. used a type of principal component analysis that tends to bias the shape of the reconstructions. A description of this effect is given in Chapter 9. In practice, this method, though not recommended, **does not appear to unduly influence reconstructions of hemispheric mean temperature;** […]" (p 113)

I had provided the quote (among many others) to dispute the CABR statement:

> Overall, National Academy of Sciences (2006) rejected the claims of McIntyre and McKitrick and endorsed, with a few reservations, Mann et al's work.

The NAS did not reject our claims, and they expressed more than a few reservations about the MBH work. The mitigation suggested by the NAS refers to the shape of the reconstruction, and is offered by the NAS without an actual demonstration, though it is possible to provide such a demonstration as we showed in our 2005 article in *Energy and Environment*, if you are willing to make the required assumptions. The problem with the flawed PC analysis in the MBH context, however, was not only that it distorted the shape of the PC1, but it also distorted the significance calculations and thereby understated the uncertainties of the reconstruction. As I have shown, the NAS endorsed these findings, notwithstanding the statement highlighted by CABR above.

The evidentiary value of the other studies cited by the NAS as independent support of the original MBH conclusions is diminished by the fact that they did not note the extent to which the other studies reused the same data or relied on the flawed MBH principal components series. The Wegman report did examine this issue (pp. 46-47) and stated (emphasis added):

> Indeed, the matrix outlined in Figure 5.8 illustrates the proxies that are used more than one time in twelve major temperature reconstruction papers. The black boxes indicate that the proxy was used in a given paper. **It is clear that many of the proxies are re-used in most of the papers. It is not surprising that the papers would obtain similar results and so cannot really claim to be independent verifications.**

MBH Corrigendum

In my original response I stated:

> First, as was acknowledged in the online supplement to the correction, the principal component analysis method used by Mann et al. was affected by the correction insofar as they used a flawed method without properly disclosing in their original paper what they were doing.

CABR responded:

> Finally, Professor McKitrick stated that the corrigendum by Mann et al. (2004) acknowledged a methodological flaw and provided modified principal components of the reconstruction. This is incorrect. There is no mention of any error or shift in methodology. In fact, the principal components provided in the supplementary material of the 2004 publication match those shown in Figure 5a of the 1998 paper.

CABR have made the mistake of interpreting the fact that there is no "mention" of any error or shift in methodology to mean that no such admission occurred. It is there, but it is heavily buried under verbiage and you have to know where to look. It is in one of the MBH SI files,[1] in which they provide the following information:

```
All predictors (proxy and long instrumental and historical/instrumental
records) and predictand (20th century instrumental record) were
standardized, prior to the analysis, through removal of the calibration
period (1902-1980) mean and normalization by the calibration period
standard deviation. Standard deviations were calculated from the
linearly detrended gridpoint series, to avoid leverage by non-stationary
20th century trends. The results are not sensitive to this step (Mann et
al, in review).
```

[1] http://www.nature.com/nature/journal/v430/n6995/extref/METHODS/AlgorithmDescription.txt

By contrast, their 1998 paper had said:

> The proxy series and PCs were formed into anomalies relative to the same 1902–80 reference period mean, and the proxy series were also normalized by their standard deviations during that period.

The difference between those two paragraphs constitutes the acknowledgement of methodological flaws. In the 1998 paper they implied that the principal components (PCs) were computed first and then centered on the 1902-1908 mean, which would be a valid procedure. In the Corrigendum they state that the raw data were centered on the 1902-1980 mean *prior* to computing the PCs, which is invalid. This reordering of the steps is where the artificial hockey stick effect was introduced, as we had argued to *Nature* and as was demonstrated in the NAS graph extracted above and at length in the Wegman report. CABR can be excused for not realising MBH were conceding the point since the concession was so well-disguised, but it does not justify them claiming that no admission of "error or shift in methodology" occurred: it just means they failed to spot it.

The decentering step did not introduce an artificial hockey stick in the MBH reconstruction itself; in that context it data-mines for the bristlecones and puts all the reconstruction weight on them. Where it created the artificial hockey stick was in the RE benchmarking step, explained by Mann in the same SI file as follows:

> An essential step in the procedure of Mann et al (1998), as described therein, was the use of conventional verification procedures to establish the level of skill in the proxy-based surface temperature reconstructions. Verification estimates based on correlation and Reduction of Error ('RE' or, 'beta' in the language of Mann et al, 1998) were established for each of the 11 separate procedures contributing to the stepwise reconstruction procedure, based on comparison of the proxy reconstructions.

The problem was that they used "conventional" RE critical values without taking into account the *unconventional* decentering step, and as a result they generated incorrect critical values for the RE score.

All these points have been discussed at considerable length in the relevant papers, in the NAS report and in the Wegman report. It is the technical core of the hockey stick controversy. CABR should have made the effort to come to terms with these details before publishing a survey on the topic and presuming to declare our work "largely spurious."

Nor did I state that Mann et al. provided new PC's in the revised corrigendum. The PC's in the corrigendum supplement were, as CABR point out, the same as in the MBH98 paper. What changed was the description of the methodology. However the wording I used in the paragraph they quoted was, indeed, ambiguous and awkward, so their confusion on that point was my fault.

Social Network Analysis

In my first response I did not raise an objection to their characterisation of the Wegman networking analysis, but I do so now, as it raises further questions about the diligence of CABR's reading of the underlying material. In wording that remains in their report, they state (emphasis added):

> The second assessment, commissioned by the House Energy and Commerce Committee and the Sub Committee on Oversight, was carried out by a team of statisticians (Wegman et al. 2006). They also concluded that the methodological errors in the original Mann et al papers had no impact on the scientific conclusion. **They carried out a social networking analysis of Mann's co-authorship network to evaluate whether "independent studies" could be unbiased. They interpreted the absence of McIntyre and McKitrick in Mann's co-author network (i.e. the authors who publish with the co-authors of Mann et al.) as evidence of bias,** and stated that Mann and co-authors were disproportionately influential in climate literature and the peer review system. Although Budd (2007, see below) subsequently refuted this claim of disproportionate influence, similar allegations have been made in the wake of the CRU emails stolen in fall of 2009.

Wegman et al. examined the coauthor network to examine the degree of independence among the different teams, not whether "'independent studies' could be unbiased" – a phrase whose meaning frankly escapes me. As I quoted above, the Wegman panel also examined the reuse of similar data sets across studies as part of this process, supporting their conclusion that the various papers did not exhibit clear independence.

More specifically, I am unable to find any statement in the Wegman report that their social network analysis was concerned with interpreting the absence of McIntyre and me in Mann's coauthor network. The network analysis is Section 5 of the Wegman report, occupying pages 37-46. Our names do not appear anywhere in that section. They do not even *mention* our absence from that network, much less "interpret" it as evidence of bias or anything else. Nor can I imagine anyone with even minimal familiarity with the subject bothering to do a computational social network analysis to understand why Mann et al. had never, in their careers, collaborated with McIntyre and me. Prior to the controversy we were complete outsiders to the field. Once the controversy was raging, there was, shall we say, a certain coolness between our groups that ruled out any form of collaboration.

On re-reading the paragraph several times I get the impression that CABR are suggesting that the Wegman team devoted a whole section of their report to answering a trivial and ludicrous question. I encourage CABR either to provide a page reference to substantiate this, or amend this paragraph.

3. East Anglia Emails

I took exception to the following paragraph in the original DB report.

> One of the emails mentioned a "trick" to plot long-term temperature records. Critics have argued that this indicates an attempt to mislead the public. In fact, the "trick" refers to the use of the instrumental record after 1960 instead of temperatures estimated from tree ring widths. The two sources were then labeled accordingly. Instrumental data were used after 1960 because some high-altitude tree ring records show declining growth after 1960 despite warming temperatures.

CABR say they "emphatically disagree" with my objections.

My first objection was that the "trick" was not, as they said, to "plot long-term temperature records," it was to "hide the decline." In their online entry CABR dispute this as follows:

> Our report asserted that Professor Jones was referring to hiding the decline in a graphical representation (we stated on p. 10 of the DBCCA/CCC report that the 'trick' referred to making graphs of long-term temperature records). We stand by our interpretation.

They then cite Jones' statement to the BBC:

> "The phrase 'hide the decline' was shorthand for providing a composite representation of long-term temperature changes made up of recent instrumental data and earlier tree-ring based evidence, where it was absolutely necessary to remove the incorrect impression given by the tree rings that temperatures between about 1960 and 1999 (when the email was written) were not rising, as our instrumental data clearly showed they were."

At the risk of belabouring the point, I fail to see how they have rebutted my claim. When they say

> Our report asserted that Professor Jones was referring to hiding the decline in a graphical representation

they are misquoting themselves. They didn't mention "hiding the decline," I did. They go on to point out that Jones felt it was necessary to hide the decline in tree ring records. He may very well have felt that way, but the point is he did not tell the reader what he was doing. Had he done so, the reader might well have asked why the tree ring data should be considered an accurate record of temperatures in previous centuries if it doesn't appear to be one in this century; and why we should assume the tree ring records would have tracked medieval warmth if they are obviously not tracking modern warmth. The decision to hide the decline prevented the reader from seeing the uncertainties and weaknesses in the model.

In their report, CABR state that "we can only speculate" that the email in question was referring to the cover of the WMO publication. However, Jones has had ample opportunity to indicate if the email referred to some other graph, and he has not done so, so I think the speculation is valid.

In their online report, CABR then say

> We agree that the graph is misleading, but it is speculative to equate a misleading graph with a deliberate attempt to mislead the public. By doing so, Professor McKitrick is making an unsubstantiated claim about Jones's intentions.

Why else produce a misleading graph, except to mislead? And, in any case, our views on Jones' intentions are substantiated by the email in which he states his intentions. It was not a clerical error. In his own words he did the "trick" to *hide the decline*.

CABR concede in their online discussion that the graph was misleading, but in their print report, they make less of a concession:

> When using different sources of information in a graph, it should always be appropriately labeled. However, the chart on the 1999 WMO cover was not labeled. We agree that this was a poor presentation of the data.

The data manipulation in question is not "poor presentation." That term describes graphs with a small font and a bad colour scheme. The WMO chart did not suffer poor presentation, it was, in fact, quite an attractive graph. The problem was that it was *misleading*, and in that sense the care that went into making it look compelling only compounds the problem.

At a certain point it becomes disconcerting that Deutsch Bank, which is among other things one of a few international banks qualified to act as a Primary Dealer for the New York Federal Reserve, and is thereby subject to particularly stringent requirements about the accuracy of commentary it publishes on economic and policy issues, is going to such efforts to excuse publication of misleading information.

In their written report CABR go on to say:

That said, other charts of long-term temperature and proxy temperature records with the same or similar data sources are properly labeled, such as those in the Third Assessment Report of the IPCC (IPCC 2001, pg. 3 or pg. 134).

One cannot excuse the publication of a misleading graph on the grounds that others, elsewhere, published graphs that weren't misleading. I cannot imagine that kind of defence succeeding in, say, a trial over securities fraud.

Worse, this example backfires because it proves the opposite point: CABR evidently don't realize that one of the IPCC graphs they appeal to also "hides the decline." In their online material they say:

As stated above, Professor McKitrik's critique of the WMO is valid: the labeling of the graph on the cover of the WMO report is misleading. Yet the cover of the WMO report is an exception; other similar graphs label the lines appropriately. In Briffa 2000 or IPCC TAR 2001 (see section 6), for example, the lines are labeled to clarify that different parameters are being plotted.

One of the two IPCC graphs they show is the following, about which they state "We disagree that there was 'no notice to the reader'" [regarding manipulations to hide the decline in the recent proxy data]:

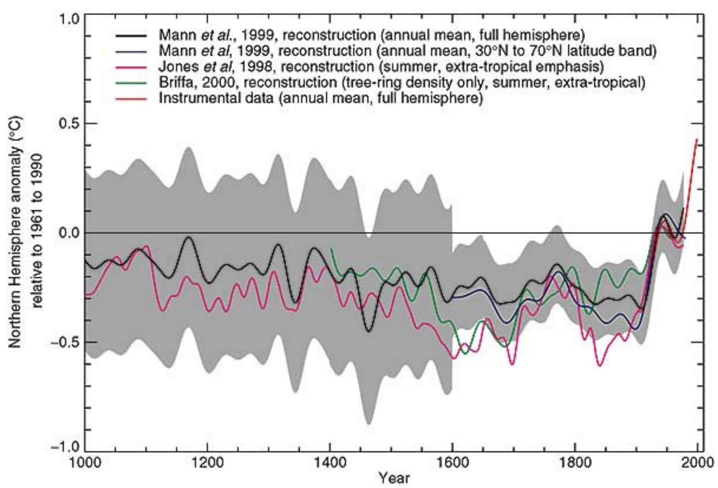


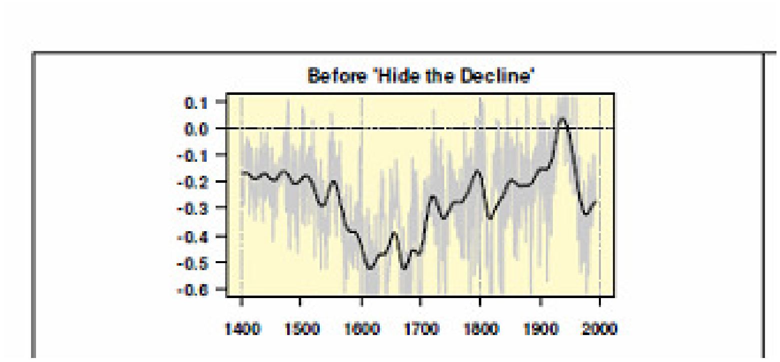
Figure 2.21. Comparison of warm-season (Jones et al., 1998) and annual mean (Mann et al., 1998, 1999) multi-proxy-based and warm season tree-ring-based (Briffa, 2000) millennial Northern Hemisphere temperature reconstructions.

By way of support they quote the IPCC text as follows:

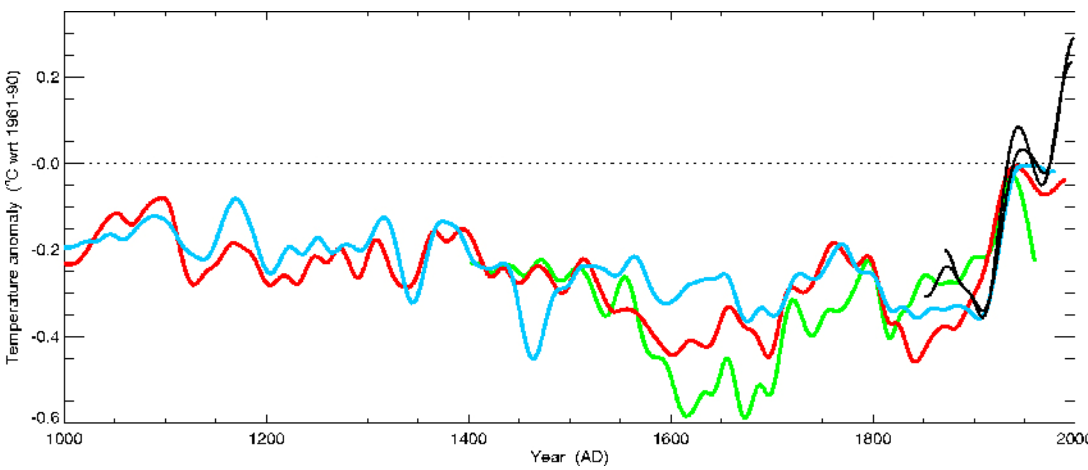
"Several important caveats must be borne in mind when using tree-ring data for palaeoclimate reconstructions. Not least is the intrinsic sampling bias…Furthermore, the biological response to climate forcing may change over time. There is evidence, for example, that high latitude tree-ring density variations have changed in their response to temperature in recent decades, associated with possible non-climatic factors…For these reasons, investigators have increasingly found tree-ring data most useful when supplemented by other types of proxy information in "multi-proxy" estimates of past temperature change." (IPCC 2001, p 132)

"All proxy information…require[s] careful calibration and verification against modern instrumental data." (IPCC 2001, p 133)

Nowhere in that paragraph do the IPCC authors point out *that they had deleted the post-1960 portion of Briffa's data*. The green line in the IPCC graph is not the same as the data from the publication they claim to have taken it from: the declining portion after 1960 has been deleted. It is disappointing that after all the times people like Steve McIntyre have exposed and explained the deletion of data from the above IPCC graph, we have yet another report from researchers denying that it happened. The Briffa 2000 data does not end at 1960, as shown in the IPCC graph, it ends at 1995.[2] The data Briffa collected looks like this:



If the full data set had been used, the IPCC graph would look like this:



(see http://climateaudit.org/2009/11/20/mike's-nature-trick/)

And here again we do not need to speculate about the intentions of the authors since they are revealed in the Climategate emails. The reason the published graph doesn't have the declining green portion is that IPCC authors like Mann were worried it detracted from the consensus picture they wanted to produce.

> 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 problem and a potential distraction/detraction from the reasonably concensus viewpoint we'd like to show w/ the Jones et al and Mann et al series.
>
> (email 0938018124.txt from Mann to colleagues Sept 22 1999)

---

[2] See http://climateaudit.org/2009/11/26/new-the-deleted-data/ and http://www.eastangliaemails.com/emails.php?eid=146&filename=939154709.txt.

It was shortly after this email went out that they deleted the post-1960 portion of Briffa's data. The fact that CABR failed to realize that this had been done in the IPCC graph as well as in the WMO graph only proves that the lines were not labeled "appropriately" and readers were not, in fact, notified of the change.

CABR correctly disputed my remark that the Briffa data were tree ring densities: they were in fact temperature estimates based on tree ring densities. My claim that the graph was the density data itself was incorrect.

CABR conclude this topic by saying:

> We feel it unwise to rely on emails, which were written casually, stolen, and taken out of context, to provide explanations and assign intention to these highly complex topics. Professor McKitrick has chosen to interpret the CRU researcher's language in the worst possible light. While he is entitled to do so, we would emphasize that this is not the only possible interpretation. Furthermore, we urge caution when drawing conclusions from these emails, especially since what has been made public lacks context. Clearer explanations are available from more authoritative sources.

A full understanding of the context does not mitigate the problems revealed in the emails, it confirms them. CABR failed to realise that the graph they relied on as an example of "appropriate" practice had also been distorted by the undisclosed truncation of data, suggesting that their investigation of these matters was uncritical, to say the least.

4. Other issues

I did not discuss other claims in the Deutsche Bank report about research in which I have been involved, but I would like to offer a few additional comments at this time.

CABR (p. 13):

> Satellite measurements of the temperature of the lower troposphere (the lowest 8 km, or 5 miles, of the atmosphere), which are free from biases due to urban heat island effects, indicate a comparable warming trend of 0.14 to 0.17°C/decade since 1979 (NOAA NCDC 2010c).
>
> Model simulations are consistent with these observations.

Model simulations are not consistent with these observations. The models state that almost all tropospheric warming will occur in the tropics. But very little warming has been observed there. Using both panel regressions and multivariate trend estimation with a non-parametric covariance matrix estimator, McKitrick McIntyre and Herman (2010) showed that modeled trends are 2—4 times higher than observed trends over the 1979-2009 interval, and the differences are statistically significant.

CABR (p. 15):

> Critics contend that scientists involved in the IPCC excluded reference to papers by **McKitrick and Michaels (2004)** and **Kalnay and Cai (2003)** that presumably contradicted their conclusions. The extremely complex multi-stage IPCC process, with multiple authors (450 lead and 800 contributing authors) and reviews (see below),makes it very difficult for a small group of individuals to influence it (IPCC 2010a). Most noteworthy, the two papers that were discussed in the CRU emails were in fact cited and discussed in Chapter 3 of the IPCC AR4 report, so any desire to omit them was not implemented.

It is misleading to cite the overall number of people involved in the IPCC when the section in question only involved a small number of lead authors and a small number of reviewers. As has been pointed out upon many times (such as my submissions to Muir Russell and the UK Select Committee), the McKitrick and Michaels paper (and the de Laat and Maurellis 2004/2006 papers) were, in fact, omitted from both of the drafts shown to reviewers during the "extremely complex multi-stage IPCC process." Then, after peer review was over, a false claim was inserted into the IPCC report to the effect that our results were statistically insignificant. This was disproven in McKitrick (2010); and more to the point was unsupported by any citation in the IPCC report in the first place, indeed it misrepresented the evidence in the papers themselves.

CABR (p. 24)

> Although the tropospheric "hot spot" is not a signature of GHG forcing, it is often mistakenly considered to be (e.g. Nova 2009). Until recently radiosonde and satellite observations did not show increased warming in the upper troposphere relative to the surface in the tropics. However, new observational datasets and updates of older ones are consistent with modeled warming trends at the surface and in the upper troposphere **(Santer et al. 2008)**….

Every model used for the IPCC report, without exception, shows a tropospheric hotspot in response to GHG changes. It is present in both the historical backcasts of 20th century warming and the forecasts of 21st century warming. Not one model deviates from this pattern. It could be argued that it is not a "unique" fingerprint, since a very large increase in solar flux might also create a tropospheric hotspot, but the model backcasts for the IPCC report do not suggest such an effect would have occurred in response to the observed increase in solar flux over the 20th century, nor is it expected to occur in response to solar variations in the 21st century. According to the models, the only forcing that would generate such a pattern, on such a scale, is GHG's.

> **Douglass et al. (2008)** reported that the discrepancy between modeled and observed trends in temperature at the surface and in the upper troposphere exceeded the uncertainty associated with either models or observations. They interpreted their results as evidence that computer models are seriously flawed, rendering climate projections untrustworthy. However, when **Santer et al. (2008)** applied a statistical test to the same data as Douglass et al., in addition to updated and new datasets, the observed temperature trends in the tropics were consistent with modeled ones at all heights. They noted that **Douglass et al. (2008)** had neglected the effect of interannual variability and used old versions of observational data. Improved tropical surface temperature estimates show a slightly reduced warming trend, while new inter-satellite comparisons lead to greater warming in the upper troposphere **(Santer et al. 2008)**, in both cases moving observational data closer to the relationship predicted by models.

The Santer 2008 paper did not use "updated" data, they used data ending in 1999. McKitrick et al. (2010) used updated data sets and, as noted above, found the claims of a significant discrepancy between models and observations in the tropical troposphere are correct.

REFERENCES CITED:

McKitrick, Ross R., Stephen McIntyre and Chad Herman (2010) Panel and Multivariate Methods for Tests of Trend Equivalence in Climate Data Sets. *Atmospheric Science Letters* DOI: 10.1002/asl.290.

McKitrick, Ross R. (2010) Atmospheric Oscillations do not Explain the Temperature-Industrialization Correlation. *Statistics, Politics and Policy*, Vol 1 No. 1, July 2010.