Response to Misinformation from Deutsche Bank

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1. Introduction

Deutsche Bank Group has published a report entitled "Climate Change: Addressing the Major Skeptic Arguments." The document is dated September 2010 and is available online at http://www.dbcca.com/dbcca/EN/_media/DBCCAColumbiaSkepticPaper090710.pdf.

The authors are Mary-Ellen Carr, Robert F. Anderson and Kate Brash, all of Columbia University, and the report is published under the imprimatur of the Deutsche Bank Climate Change Advisors, Deutsche Bank Group.

Unfortunately the document was issued in a locked PDF that prevents copying and pasting quoted portions, so I have reproduced the section on the hockey stick as a JPG extract in the appendix, and I have retyped specific items I want to critique.

Readers who are familiar with the various issues will recognize that the Deutsche Bank (DB) report is one-sided. The weakness of its argumentation is partly due to its failure to properly quote the material it purports to rebut, so that its arguments are frequently shallow and unconvincing. In this rejoinder I will focus only on two items: The Hockey Stick controversy and the report's treatment of the "Hide the Decline" email. These should suffice to illustrate the weakness of the DB report.

2. Hockey Stick Controversy

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.

The DB paper states that Mann et al. published a correction in 2004 after our initial publication, and claims "none of the results or analyses were affected." This is a misleading claim. 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. Second, subsequent analyses, including those of

the Wegman and National Academy of Science panels both concluded Mann's flawed methods biased the results. The DB paper misrepresents both these reports, as I will show below.

The DB paper then reports a political timeline without dates or details as follows:

> Until this point the controversy followed the standard pattern of scientific discourse: discovery, publication, attempts at replication, criticism, adjustment and re-publications. The debate entered the political arena when McIntyre and McKitrick met with Senator James Inhofe (R-OK), an outspoken denier of anthropogenic climate change; shortly afterward, Congressman Joseph Barton (R-TX) write to Michael Mann, demanding that he share all his data, methods and associated information with critics and congressional staff (Eilperin 2005).

We made a presentation on our work in Washington in November 2003, during which time we briefly met Senator Inhofe, among others. Barton's letter to Mann and his coauthors was not issued until in July 2005, and it was in response to Mann's statement in a *Wall Street Journal* article of 14 February 2005 affirming his refusal to share his computer code with us. Contrary to the DB paper, apart from our brief meeting in fall 2003, McIntyre and I did not meet with Senator Inhofe during the debate over our work and, in particular, during the lead-up to the issuance of Barton's letter.

NAS Report

The DB paper then claims that the 2006 report of the National Academy of Sciences

> rejected the claims of McIntyre and McKitrick and endorsed, with a few reservations, Mann et al.'s work.

This is a misrepresentation. It is quite notable that no citations to the NAS report are given to support this statement. The NAS report endorsed every technical criticism we made.

In our presentation to the NAS panel we explained that the hockey stick method, and the test statistics used to validate it, systematically underestimated the uncertainties in the data. The NAS panel concluded as follows (p. 107)

> Regarding metrics used in the validation step in the reconstruction exercise, two issues have been raised (McIntyre and McKitrick 2003, 2005a,b). One is that the choice of "significance level" for the reduction of error (RE) validation statistic is not appropriate. The other is that different statistics, specifically the coefficient of efficiency (CE) and the squared correlation (r^2), should have been used (the various validation statistics are discussed in Chapter 9). Some of these criticisms are more relevant than others, but taken together, they are an important aspect of a more general finding of this committee, which is that uncertainties of the published reconstructions have been underestimated.

We said the hockey stick failed key verification tests. In subtle wording they agreed (p. 91): Mann's data set does not have enough information to verify its 'skill' at resolving the past, and has such wide uncertainty bounds as to be no better than the simple mean of the data (p. 91). The NAS reported the failure of Mann's reconstruction in a roundabout way, by discussing the results in a replication exercize rather than the original Mann paper itself (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.

We argued that the flawed principal component methodology used in Mann et al.'s work biased their results. The NAS panel concluded (p. 106):

> 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.

Although they argued that other authors who had not used the same method had obtained qualitatively similar results, the NAS agreed with us that the problem with Mann's method was it loaded too much weight on a small number of bristlecone pine series from the western US, which were inappropriate series for use as temperature proxies. The NAS concluded (p. 107):

> The more important aspect of this criticism is the issue of robustness with respect to the choice of proxies used in the reconstruction. For periods prior to the 16th century, the Mann et al. (1999) reconstruction that uses this particular principal component analysis technique is strongly dependent on data from the Great Basin region in the western United States.

And in their examination of the data in question, they warned that these strip-bark series should not be used in this type of research (p. 50).

Wegman Report

The DB paper summarizes the Wegman panel findings with regards to the hockey stick as follows:

> They also concluded that the methodological errors in the original Mann et al. papers had no impact on the scientific conclusion.

The DB Report went on to say

> While the uncertainty associated with assessments of past climate might have been understated and there were minor methodological errors in the Mann et al. studies, both NAS (2006) and Wegman et al. (2006) confirmed the soundness of the research and concluded the primary conclusions were unaffected by any methodological problems.

In addition to misrepresenting the NAS findings, this is a wholly false misrepresentation of the findings of the Wegman report. The Wegman Report concluded as follows.

> While the work of Michael Mann and colleagues presents what appears to be compelling evidence of global temperature change, the criticisms of McIntyre and McKitrick, as well as those of other authors mentioned are indeed valid. Because the error and uncertainty involved in climate reconstructions is magnified with each preceding year, the ability to make certain conclusions about the climate at the beginning of the millennium is not very robust. This is even

> less robust considering the inability to actually calculate an accurate uncertainty for these reconstructions. (p. 26)
>
> The papers of Mann et al. in themselves are written in a confusing manner, making it difficult for the reader to discern the actual methodology and what uncertainty is actually associated with these reconstructions. (p. 26)
>
> It is not clear that Dr. Mann and his associates even realized that their methodology was faulty at the time of writing the MBH paper. The net effect of the decentering is to preferentially choose the so-called hockey stick shapes. (pp. 28-29)
>
> Our committee believes that the assessments that the decade of the 1990s was the hottest decade in a millennium and that 1998 was the hottest year in a millennium cannot be supported by the MBH98/99 analysis. (p. 49)

East Anglia Emails

The DB report provides a cursory review of the problems revealed in the East Anglia Emails. Their discussion of the notorious "hide the decline" email is as follows.

> 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.

Every sentence in this paragraph is untrue or misleading. I will take them one-by-one.

1. *One of the emails mentioned a "trick" to plot long-term temperature records.*

No, one of the emails mentioned a "trick" to *hide the decline*. The reference is to email 942777075.txt wherein Jones says

> 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.

2. *Critics have argued that this indicates an attempt to mislead the public.*

It is not merely critics who have argued this, but the Muir Russell Inquiry as well, which summarized the issue as follows (p. 60, emphasis added).

> In relation to "hide the decline" **we find that, given its subsequent iconic significance (not least the use of a similar figure in the TAR), the figure supplied for the WMO Report was misleading** in not describing that one of the series was truncated post 1960 for the figure, and in not being clear on the fact that proxy and instrumental data were spliced together. We do not find that it is misleading to curtail reconstructions at some point *per se*, or to splice data, but we

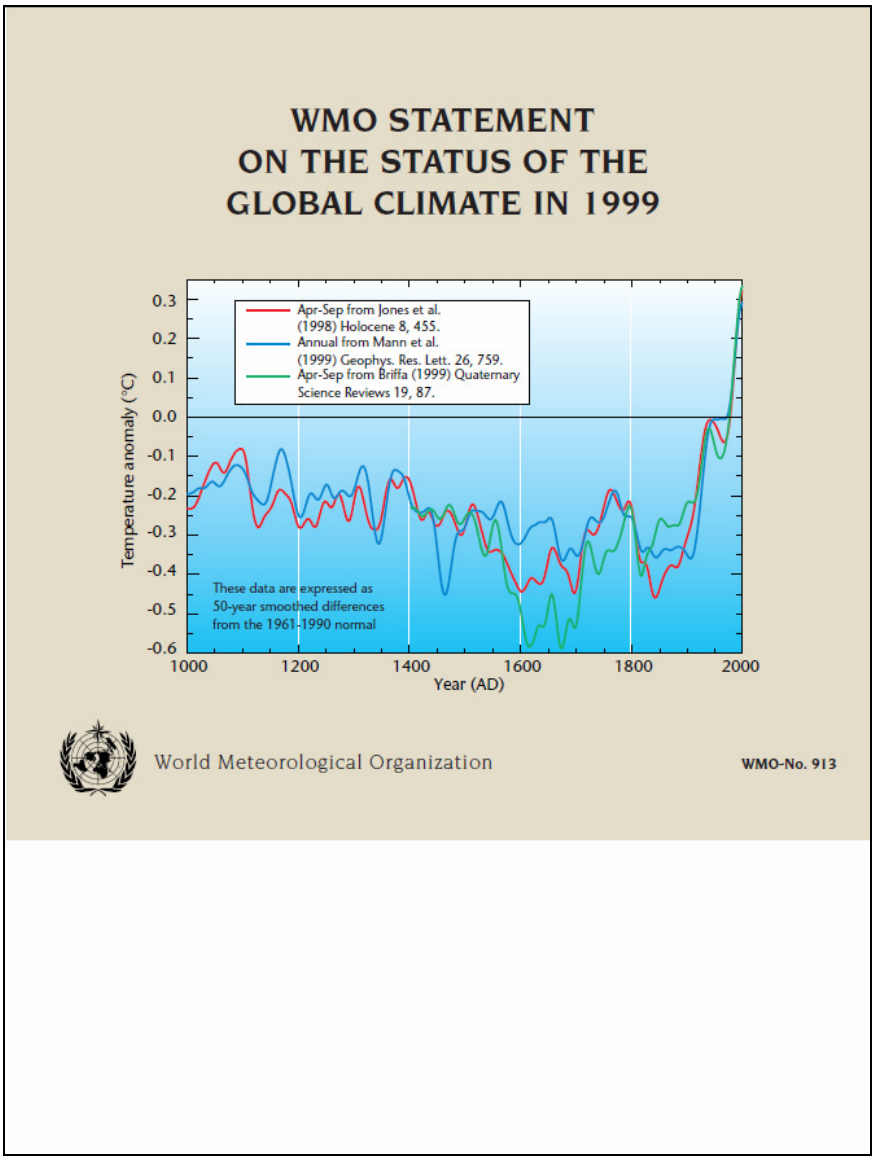
believe that both of these procedures should have been made plain – ideally in the figure but certainly clearly described in either the caption or the text.

3. In fact, the "trick" refers to the use of the instrumental record after 1960 instead of temperatures estimated from tree ring widths.

The graph in question shows tree ring widths in two series, and temperatures estimated from tree ring widths in one series. The substitution of temperature data replaces one of the tree ring series (Briffa's). In other words, the instrumental record is used to replace tree rings themselves, not "temperatures estimated from tree rings."

4. The two sources were then labeled accordingly.

False. The Figure that appeared on the cover of the World Meteorological Organization is:



(see http://www.wmo.int/pages/prog/wcp/wcdmp/statemnt/wmo913.pdf).

There is obviously no labeling of the data swap.

5. Instrumental data were used after 1960 because some high-altitude tree ring records show declining growth after 1960 despite warming temperatures.

This is sheer speculation on the part of the DB paper authors, since no explanation was provided in the report as to the rationale for the trick. Likewise, in the IPCC Report that was produced 2 years later, Briffa's divergent data was truncated at 1960 with no notice to the reader. The only explanation that appears to have been recorded at the time was in Jones' email: to "hide the decline."

APPENDIX: Extract from DB paper

2. The Hockey Stick Controversy

Understanding past climate is key to our ability to interpret the climate of the present and to project future climate conditions. In the absence of an instrumental record prior to the 19th century, scientists recur to what are known as proxy records. Proxies for climate are biological or chemical markers within the Earth system that are affected by changes in climate, and thus provide indirect information about climate of the past. Some climate proxies include the width of tree rings, the isotopic composition of ice, or representative fossil micro-organisms in sediment cores. Analyzing and assembling data from proxy sources to create a credible picture of past climate is a difficult process and, like any scientific study, one that is open to differences of opinion and criticism. Thus, it is no surprise that the use of proxy data is at the root of one of the best known climate change controversies.

The controversy began with two articles on long-term temperature trends based on proxy records, which were published in the scientific journals *Nature* and *Geophysical Research Letters* (**Mann et al. 1998, 1999**) and included a graph of average northern hemisphere temperatures over the last millennium. The graph (and the data it represented) showed that northern hemisphere temperatures were currently higher than they had been in the last 1000 years and that the sharpest rise in temperature (the last 200 years) coincided with increasing greenhouse gas emissions from human activity.

After the figure was prominently displayed in the IPCC's Third Assessment Report (IPCC 2001), it became known as the "hockey stick," where the shape describes the sharp temperature increase toward the end of the record. Appearing in articles around the world, the hockey stick was seen as "visually arresting scientific support for the contention that fossil-fuel emissions are the cause of higher temperatures" (Wall Street Journal 2005).

However, Steve McIntyre, a Toronto-based minerals consultant, and Ross McKitrick, an economist at Canada's University of Guelph, argued in the social science journal *Energy and Environment* (**McIntyre and McKitrick 2003**), that the temperature increase depicted in the hockey stick graph resulted from flawed methodology, including "collation errors, unjustifiable truncations of extrapolation of source data, obsolete data, geographical location errors, incorrect calculations of principal components, and other quality control defects." In response, Mann et al. (2004) published a correction in *Nature*, acknowledging errors in the list of proxy data sets provided in the Supplementary Information as part of **Mann et al. (1998)**; none of the results or analyses were affected.

Until this point the controversy followed the standard pattern of scientific discourse: discovery, publication, attempts at replication, criticism, adjustment, and re-publication. The debate entered the political arena when McIntyre and McKitrick met with Senator James Inhofe (R-OK), an outspoken denier of anthropogenic climate change; shortly afterward, Congressman Joseph Barton (R-TX) wrote to Michael Mann, demanding that he share all his data, methods and associated information with critics and congressional staff (Eilperin 2005). While Mann considered the request, House Science Committee Chairman Sherwood Boehlert (R-NY) asked Barton to withdraw what Boehlert called a "misguided and illegitimate investigation," arguing that the purpose of the investigation seemed to be "to intimidate scientists rather than to learn from them, and to substitute congressional political review for scientific review" (Eilperin 2005).

This led to two independent government-commissioned assessments of the "hockey stick." The House Science Committee commissioned the US National Academy of Science (NAS) to review the original Mann et al. study, while Barton and the House Energy and Commerce Committee asked Edward Wegman, chair of the NAS statistics panel, to investigate the statistical merit of the critiques made by McIntyre and McKitrick.

The National Academy of Sciences report (2006) supported Mann's conclusion that temperatures of the latter half of the twentieth century were the highest in the record, but asserted that the authors should have better communicated the uncertainty of data; namely a specific year or decade (1998 and the nineties) could not be identified as the warmest because of the uncertainty associated with proxy values for individual years or decades (especially prior to 1600). Overall, National Academy of Sciences (2006) rejected the claims of McIntyre and McKitrick and endorsed, with a few reservations, Mann et al's work.

2. The Hockey Stick Controversy

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.

While the uncertainty associated with assessments of past climate might have been understated and there were minor methodological errors in the Mann et al. studies, both NAS (2006) and Wegman et al. (2006) confirmed the soundness of the research and concluded that the primary conclusions were unaffected by any methodological problems.

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)**. Recent studies using various independent proxies and different statistical approaches continue to support the original conclusion of Mann et al; see for example **Kaufman et al. (2009)** discussed below in 3.2.b.

Ongoing support in the peer review literature for a similarly shaped temperature record of the past millennium could be questionable if, as Wegman et al. (2006) asserted, dissenting opinions were denied fair consideration due to bias from mutually reinforcing networks of likeminded scientists. However, **Budd (2007)** argues that by focusing on co-authors Wegman et al.'s social networking analysis was specious and did not demonstrate bias. Since "co-authorship is an intentional act," connectedness between authors is to be expected. McIntyre and McKitrick are from different scientific communities than Mann and thus naturally belong to mutually exclusive author networks.

This example demonstrates that despite the uncertainties associated with the scientific study of climate (discussed in more detail below), the scientific process is well suited for discovering and correcting errors through peer-review. Taking the scientific discussion into the political arena can succeed only by including a balanced and broad-based representation of views, which can be difficult in a political context (e.g. **Budd 2007**).