

## COMMENTS ON THE ISSUES PAPER OF THE INDEPENDENT CLIMATE CHANGE EMAIL REVIEW

Malcolm K Hughes  
Regents' Professor and Director Emeritus  
Laboratory of Tree-Ring Research  
University of Arizona  
Tucson, AZ 85721  
USA

Submitted: February 28, 2010

I write in response to your call for comments on your issues paper. In particular I will address item 1.1 of the document, as a scientist who has been active in related areas for four decades, and who was the writer or recipient of some of the hacked emails. Please do not hesitate to contact me to seek clarification of any of the points I make here.

I note that it is indicated in the "Issues for examination" document that "*The Team stresses that its remit does not involve re-evaluation of the scientific conclusions of the CRU work.*" I was, therefore, somewhat puzzled to read the preamble of point 1 of "Issues arising on Para 1.1 of the terms of reference" which deals exclusively with scientific questions, and contains some unfortunate errors and evidence of confusion regarding technical matters.

### **Point 1 in "Issues..."**

#### **The "hockey-stick".**

The term "hockey-stick" curve was initially applied to a graph of reconstructed Northern Hemisphere annual temperature for the period back to AD 1400 published by Michael Mann, Ray Bradley and myself in *Nature* in 1998, and then extended tentatively by us back to AD 1000 in a 1999 paper in *Geophysical Research Letters*. Our results were based on several kinds of records, including those from coral annual bands, annual layers in ice cores, with the largest single group being tree-ring records. This was an almost entirely different data set to that developed and used by Professor Keith Briffa and his colleagues, which was based on the maximum latewood density of conifer tree rings from alpine and boreal forests. It is important to note that not only were the data used by Professor Briffa and by Mann et al different, but that the methods used by us (Mann, Bradley, Hughes) differed fundamentally from those used by the CRU group. Please understand that these are complementary approaches, both with clearly acknowledged advantages and limitations. Further, our (Mann, Bradley, Hughes) methods of calculation have been subject to unusually intense scrutiny, including by a panel of the US National Academy of Science (USNAS) that was requested by the US Congress in response to claims by lay critics see [http://www.nap.edu/catalog.php?record\\_id=11676#description](http://www.nap.edu/catalog.php?record_id=11676#description)

The USNAS panel concluded in its 2006 report that:

*"The basic conclusion of Mann et al. (1998, 1999) was that the late 20th century warmth in the Northern Hemisphere was unprecedented during at least the last 1,000 years. This*

*conclusion has subsequently been supported by an array of evidence that includes both additional large-scale surface temperature reconstructions and pronounced changes in a variety of local proxy indicators, such as melting on icecaps and the retreat of glaciers around the world, which in many cases appear to be unprecedented during at least the last 2,000 years.*” Residual issues concerning our methods raised by the same lay critics were shown comprehensively to be without merit in a peer-reviewed paper by Wahl and Amman published in *Climatic Change* in 2007. I describe our work and the NAS report because it supports the general results also found by Professor Briffa.

In 2008 and 2009 we published further work in *Proceedings of the National Academy of Sciences* and in *Science* respectively. Far more data of various kinds were used, this time including a recent version of the tree-ring density dataset kindly made available by Professor Briffa, Dr Osborn and colleagues. Multiple methodological approaches were tested. We concluded that “*Recent warmth appears anomalous for at least the past 1,300 years whether or not tree-ring data are used.*” We characterized this finding as “*likely correct*”, that is the probability of the assertion being true is estimated as between 66% and 90%, and by “*recent*” we meant the decade prior to publication.

### **Divergence**

Professor Briffa and colleagues reported in a widely cited 1998 *Nature* paper that some, but far from all, tree-ring records in their tree-ring dataset showed less sensitivity to interdecadal variations in summer temperature after about 1960 than before. This concern was hardly hidden by Professor Briffa! In fact, it was he and his colleagues who most prominently drew attention to this as a potentially widespread problem, and in their subsequent work continued to refer to it. Perhaps most importantly in the context of your second point in “Questions to Address”, the Mann et al. 1998 and 1999 results did not suffer from this problem, probably because we used different tree-ring data, many of which were prepared in different ways. We published a comparison of our results including and excluding tree rings in the online journal *Earth Interactions* in 2000 [http://www.ncdc.noaa.gov/paleo/ei/ei\\_nodendro.html](http://www.ncdc.noaa.gov/paleo/ei/ei_nodendro.html) showing that the decadal course of reconstructed temperatures was largely insensitive to the inclusion or exclusion of tree-ring data, including after 1960. At that time (in 2000), it was possible to make this comparison only for the period AD 1750 to 1980 because many of the records we used ended by 1980, and because, although there were plenty of tree-ring records before 1750, that was not true of the non-tree-ring data at that time. It should also be noted that this question has been addressed elsewhere, notably by Cook et al (page 2069) in a 2004 *Quaternary Science Reviews* paper, who produced evidence that the tree-ring data they used had not “*similarly failed*” to reflect the warming of the early Middle Ages, and so did not “*underestimate the warming during the Medieval Warm Period*” as you put it. D’Arrigo et al (2008) note that Cook et al.’s “*observation that the divergence phenomenon appears confined to recent decades strongly suggests an anthropogenic cause [for the divergence phenomenon. MKH]*”. Finally, in a 2009 paper in the journal *Global Change Biology*, using data from many locations in Russia, including many used by Professor Briffa and colleagues, Esper et al. concluded that “*divergence*” is largely an artifact of particular combinations of tree-ring data processing and the procedures of calibration against instrumental data. They found no need to invoke other factors such as

environmental nitrogen or fertilization by carbon dioxide. I am aware that Professor Briffa and colleagues have also been engaged in a major exercise to get to the bottom of this question. This is a classic example of scientific progress, characterized by the successive improvement of methods and data sets, in which Professor Briffa has played a leading and entirely commendable role.

### Questions

The peer-reviewed papers of Mann et al. referred to above, supported by a full explanation of the methods and data used, and accompanied by the release not only of all data used, as in our earlier work, but also of all computer code, provide answers to several of your “Questions to address”. Their relevance to your questions should become clear below. Specifically:

#### QUESTION

*“Does not the problem of divergence for the late 20<sup>th</sup> century record invalidate the deduction of tree ring paleotemperatures for the period prior to the instrumental period?”*

#### RESPONSE

As shown by Mann et al 2008, clearly not. Our reconstructions made using tree ring records, including those also used by Professor Briffa, are very largely similar to those we made excluding tree ring data.

#### QUESTIONS

*“What is the evidence that the amplitude of warming during the Medieval Warm Period (MWP) is not underestimated by tree ring evidence?” “How does the tree ring evidence of the MWP compare with other proxy data?” “If tree ring proxies are removed from reconstructions, what evidence remains of the MWP?”*

#### RESPONSE

Again, the evidence that the use of tree ring data, including the data set associated with Professor Briffa, does not produce an underestimation of warming in the so-called MWP, or of cooling in the so-called Little Ice Age, may be seen clearly in Mann et al 2008. In particular see Figure S7 in the online Supplementary Information, showing reconstructions both using and excluding tree-ring data. Furthermore Chapter 6 of Working Group 1 of the IPCC AR4 shows ample evidence of convergence between reconstructions based on various data sources, including tree rings, and runs of physically based climate models driven by estimates of natural and anthropogenic forcings for the last millennium [http://www.ipcc.ch/publications\\_and\\_data/ar4/wg1/en/ch6.html](http://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch6.html) This is not just about statistical techniques – our developing understanding of the climate of the past 2000 years rests on both a growing and improving body of data and improving understanding of physical climatology and the mechanisms forming records such as tree rings.

#### QUESTION

*“Have you been selective in utilizing tree ring evidence from Yamal in Siberia....”*

#### RESPONSE

Professor Briffa has already responded to this convincingly, comprehensively and professionally at <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

## **Point 2 in “Issues...”**

### QUESTION

*“What does the word “contain” mean in this context?”*

### RESPONSE

It means “include”. There is no other credible interpretation of the quoted email. If not, why would the writer be encouraging the use of a 2000 year-long period, rather than a 1000- year period? Further, “*putative*” is entirely appropriate here, as you can see from many papers in the literature where authors, notably Professors Briffa and Jones and Dr Osborn actually try to investigate what happened, rather than sticking a label on the period. In 1994, I wrote a paper with Henry Diaz in the journal *Climatic Change* with the title “*Was there a Medieval Warm Period and so where and when?*” reviewing the then available evidence for a global warm period during the European Middle Ages. We found the evidence to be equivocal and spotty. In recent years, as knowledge of many aspects of climate over the last two millennia has expanded, it has emerged, especially recently, that the several centuries before the fifteenth century were far more varied and interesting than the MWP label would imply, which is why that term has largely fallen out of use in the specialist literature concerned with climate variability. It is now often replaced with the term “*Medieval Climate Anomaly (MCA)*”.

### QUESTION

*“What involvement have you had in “containing” the MWP?”*

### RESPONSE

As my response to the previous question shows, in the context of the preamble in Point 2, this question is meaningless.

### QUESTION

*“How important is the assertion of unprecedented late 20<sup>th</sup> century warming” in the argument for anthropogenic forcing of climate?”*

### RESPONSE

The argument for anthropogenic forcing of climate does not depend on the assertion of unprecedented late 20<sup>th</sup> century warming. That case rests on understanding of physical climatology. The one feature of the recent warming of relevance is its explanation. What is relevant is the convergence of the preponderance of evidence from multiple reconstructions of Northern Hemisphere temperatures, including those developed by Professor Briffa and his colleagues, and the forced climate model runs referred to earlier in my responses. The point is that although variability in forcing from solar receipts and the effects of climatically effective explosive volcanic eruptions can account for much of the reconstructed temperature variability of the past thousand years, it cannot account for the recent remarkably synchronous warming or its latitudinal and vertical distribution. The only known factors that can do that are those associated with human activity, primarily increased concentrations of greenhouse gases and certain particulate components of the atmosphere.

### **General comments**

Contrary to the impression given in much uninformed comment in the news media and in weblogs, it has been my experience that colleagues at CRU have always been generous in sharing data, and as transparent as humanly possible in documenting their analyses, according to contemporaneous standards. In particular, I have always admired the honest caution with which Professor Briffa reports his results, and evaluates those of others, including myself. It is important to remember that not all data shared with CRU scientists has been theirs to pass on. This is not only so in the case of meteorological data provided by weather services from some countries, but also for some proxy data such as those derived from natural archives such as tree rings. A worker, perhaps in another country, may be willing to share a precious data series only on condition that it is not passed on to third parties. This is because they may not yet have had the chance to publish all of their results and interpretations from the shared data. This rarely presented a major problem in the past, because it was understood that a third party wishing to access the shared data could see whose data they were from the citations in the paper reporting the shared data, and a visit to the library or the internet would yield the address of the data owner so that permission to use the data might be sought directly. Because of his leadership role in multinational collaborations, Professor Briffa has been caught between this accepted professional manner of dealing with data requests on the one hand and on the other hand unreasonable and insatiable demands for instantaneous access to data not his to distribute.

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