

INDEPENDENT CLIMATE CHANGE EMAIL REVIEW

Responses from Michael E. Mann

Allegations:

1. The allegation of ignoring potential problems in deducing palaeotemperatures from tree ring data that might undermine the validity of the so-called “hockey-stick” curve.

In the late 20th century, the correlation between the tree ring record and instrumental record of temperature change diverges from that for the earlier period. The cause of this divergence does not appear to be understood. If the method used to deduce temperatures from tree ring proxy metrics for the earlier tree ring record is applied to the late 20th century tree ring series, then declining temperatures would be deduced for the late 20th century. It is alleged that if the cause of divergence between the tree ring and instrumental temperature record is unknown, it may have existed in earlier periods. Therefore if tree rings had similarly failed to reflect the warming of the early Middle Ages, they may significantly under-estimate the warming during the Medieval Warm Period, thus falsely enhancing the contrast between the recent warming and that earlier period. (It is this contrast that has led to statements that the late 20th century warming is unprecedented during at least the last 1000 years.)

QUESTIONS TO ADDRESS:

- i. What method do you use to deduce palaeotemperatures from tree ring data?*
- ii. Does not the problem of divergence for the late 20th century record invalidate the deduction of tree ring palaeotemperatures for the period prior to the instrumental record?*
- iii. How open have you been about this issue?*
- iv. What attempts have you made to resolve it?*
- v. What is the evidence that the amplitude of warming during the Medieval Warm Period (MWP) is not underestimated by tree ring evidence?*
- vi. How does the tree ring evidence of the MWP compare with other proxy data? Have you showed how data from different sources compare or have you conflated them? If the latter, what is the justification?*
- vii. If tree ring proxies are removed from reconstructions, what evidence remains of the MWP?*
- viii. Have you been selective in utilizing tree ring evidence from Yamal in Siberia; and if so, what is the justification for selectivity and does the selection influence the deduced pattern of hemispheric climate change during the last millennium?*

RESPONSE

The question as stated, is somewhat problematic. If one means by the “hockey stick” curve, the work of Mann et al (1998) [Mann, M.E., Bradley, R.S., and Hughes, M.K., Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries, *Nature*, 392, 779-787, 1998] and Mann et al (1999) [Mann, M.E., Bradley, R.S. and Hughes, M.K., Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations, *Geophysical Research Letters*, 26, 759-762, 1999]—which is indeed from where the traditional usage of the term derives ---then most of the issues listed do not actually apply. The Mann et al (1998) and Mann et al (1999) “Hockey Stick” reconstructions were “multiproxy” reconstructions, based on a diversity of information (long historical records, ice cores, corals, and sediments in addition to tree-rings), rather than tree-rings alone.

The so-called “divergence problem” pertains to temperature reconstructions based on a specific subgroup of tree-ring data, maximum latewood tree-ring density measurements from sub-polar environments. The

problem with those particular data was identified in work by Briffa et al (1998) [Briffa, K. R., F. H. Schweingruber, P. D. Jones, T. J. Osborn, S. G. Shiyatov, and E. A. Vaganov (1998), Reduced sensitivity of recent tree-growth to temperature at high northern latitudes, *Nature*, 391, 678–682]. In that study, the authors noted a particular problem with that specific tree-ring data, wherein there is indeed an enigmatic decline in their response to temperature after about 1960—this is the so-called “divergence problem”, and it is why Briffa and coworkers do not use those data in reconstructing temperatures after about 1960, when the problem begins to appear.

This problem exists to a much lesser degree in other tree-ring data, and is not generally present in other types of proxy data. The Mann et al (1998; 1999) “Hockey Stick”, which used very few tree-ring density records at all, does not suffer from this problem. The “Hockey Stick” extends through to the end of the modern calibration interval in 1980. As explained in the original Mann et al 1998 study “*the training interval is terminated at 1980 because many of the proxy series terminate at or shortly after 1980*”. In other words, the termination of the Hockey Stick at 1980 has nothing to do with any potential problems involving tree-ring data, but simply the fact that many of the proxy records (ice cores, corals, as well as tree-rings) terminated in the late 1970s or early 1980s, and didn’t come up to the present.

Despite the fact that the thrust of this question is inapplicable to our own work (i.e. Mann et al, 1998; 1999), there are some other issues raised here that I am happy to speak to. For example, in more recent work [Mann, M.E., Zhang, Z., Hughes, M.K., Bradley, R.S., Miller, S.K., Rutherford, S., Ni, F., Proxy-Based Reconstructions of Hemispheric and Global Surface Temperature Variations over the Past Two Millennia, *Proc. Natl. Acad. Sci.*, 105, 13252-13257, 2008] my collaborators and I have examined the sensitivity of paleoclimate reconstructions to the use of tree-ring data. In that study, we performed reconstructions both with and without the use of any tree-ring data at all. Our findings were that essentially the same result was obtained with and without the use of tree-ring data, and that the conclusion that recent warmth is anomalous in a millennial or longer context was independent of the use of tree-ring data.

An independent study by Moberg et al published in *Nature* [Moberg, A., et al., 2005: Highly variable Northern Hemisphere temperatures reconstructed from low- and high-resolution proxy data. *Nature*, 433(7026), 613–617.] yielded the same finding. In that study, the authors use various types of proxy data, other than tree-ring data, to reconstruct long-term temperature trends. They too found that the recent warmth is anomalous in the context of their millennial temperature reconstruction.

It is worth noting that it is only over the past five years or so, as other key long, high-resolution, high quality proxy records have come available, that it has been possible to achieve reliable large-scale temperatures reconstructions a millennium or more back in time without using tree-ring data at all.

2. The allegation that CRU has colluded in attempting to diminish the significance of data that might appear to conflict with the 20th century global warming hypothesis.

The CRU group, in consultation with Professor Michael Mann, is alleged to have systematically attempted to diminish the significance of the Medieval Warm Period, evidenced by an email from Mann 4th June 2003: "I think that trying to adopt a timeframe of 2K, rather than the usual 1K, addresses a good earlier point that Peck made w/ regard to the memo, that it would be nice to try to "contain" the putative "MWP", even if we don't yet have a hemispheric mean reconstruction available that far back [Phil and I have one in review--not sure it is kosher to show that yet though--I've put in an inquiry to Judy Jacobs at AGU about this]." The use of the words "contain" and "putative" are alleged to imply an improper intention to diminish the magnitude and significance of the MWP so as to emphasise the late 20th century warming.

QUESTIONS TO ADDRESS:

- i. What does the word "contain" mean in this context?*
- ii. What involvement have you had in "containing" the MWP?*
- iii. How important is the assertion of "unprecedented late 20th century warming" in the argument for anthropogenic forcing of climate?*

RESPONSE

As the author of the email in question, I feel uniquely qualified to dispel some of the disturbingly false assertions that have been circulated over this particular email. While the choice of word "contain" was not my own (it is clear in the email that I was referring to a statement by a colleague "Peck"---Jonathan Overpeck, now at the University of Arizona), I can speak to how I interpreted his use of the term and how I was using it in the email in question.

In this email, I was discussing with potential co-authors a piece I had been solicited to submit to the journal *Eos* by AGU representatives to address highly questionable claims that had been made in a recent article by Soon & Baliunas [Soon, W., and S.Baliunas, Proxy climatic and environmental changes of the past 1000 years, *Climate Research*, 23, 89-110, 2003]. I stressed the importance of at least tentatively trying to extend hemispheric-scale paleoclimate temperature reconstructions far enough back in time that we could determine the onset and duration of any "Medieval Warm Period" ("MWP"). The existence of a well-defined MWP in hemispheric-wide temperatures was indeed "*putative*" as no scientists to date had yet extended hemispheric or global temperature reconstructions far enough back to "*contain*" the time interval (a multi-century time interval centered between about AD 950 and 1250 when there is regional evidence in some places, such as Europe or Greenland, for relative warmth) in question, or to establish whether there was enough temporal and spatial coherence to any evidence of warmth in different regions to meaningfully define a truly hemispheric or global-scale "MWP". This subject was indeed the very topic of discussion in our *Eos* article. Earlier work by Hughes and Diaz [Hughes, M.K. and H.F. Diaz, "Was there a 'Medieval Warm Period' and if so, when and where?" *Climatic Change*, 26, 109-142. 1994], and subsequent work by Bradley et al [Bradley, R.S., Hughes, M.K., Diaz, H.F., Climate in Medieval Times, *Science*, 302, 404-405, 2003] and more recently by Osborn and Briffa [Osborn T.J., Briffa K.R., The spatial extent of 20th-century warmth in the context of the past 1200 years, *Science* 311:841-844, 2006] continues to suggest that indeed there was no period during Medieval times that displayed evidence of the level of spatially-coherent warming that is witnessed in recent decades. There is an extremely large body of work, for example, that now indicates a "La Nina" like climate state in Medieval times, characterized by unusually cold temperatures in the tropical Pacific. Such regional cooling offsets warming in other regions, such as North America and Eurasia, when computing hemispheric and global mean temperature. The recent article by Mann et al in *Science* [Mann, M.E., Zhang Z., Rutherford, S., Bradley, R.S., Hughes, M.K., Shindell, D., Ammann, C., Falugevi, G., Ni, F., Global Signatures and Dynamical Origins of the "Little Ice Age" and "Medieval Climate Anomaly", *Science*, 326, 1256-1260, 2009] provides a good review. For such reasons, most climate scientists no longer use the potentially misleading term "Medieval Warm Period" and instead use the terminology "Medieval Climate Anomaly".

The *Eos* article was among the first to attempt to "contain" the Medieval period, by extending the hemispheric reconstructions, on a preliminary basis, far enough back to entirely contain the interval in question (i.e the interval of AD 950-1250 or so when there is evidence of warmth in some regions such as Europe). Both Keith Briffa and collaborators had a tentative extension based on regional data, while Phil Jones and I had a more representative hemispheric reconstruction that went back to AD 200, but it was not yet published. In the emails with AGU representatives, we were trying to determine whether it would be appropriate to show the latter in our *Eos* piece, given that it was currently in press in another AGU journal (GRL). To suggest, as some have, that the efforts discussed in this email, which were very clearly discussing the interest in extending reconstructions further back in time to include the Medieval interval, could somehow reflect an attempt to hide or obscure the so-called "MWP" simply defies logic.

In more recent work, such as the IPCC Fourth Assessment Report published in 2007, many paleoclimate reconstructions now stretch nearly 2000 years back in time, which is indeed far enough back in time to isolate or, in the words of my colleague Overpeck, "contain" this period in time. In every one of these reconstructions, the peak Medieval warmth does not match the warmth of recent decades. This is indeed why the most recent IPCC report concluded that recent Northern Hemisphere average warmth is unprecedented over at least the past 1300 years (there are fewer reconstructions that extend farther back than that, hence confident conclusions are limited to this time interval).

Despite the controversy that persists (much of it manufactured) over the issue of Medieval climate, and the Hockey Stick, such paleoclimate evidence does not in fact constitute one of the primary pillars of evidence for human-caused climate change. While all peer-reviewed scientific studies over the past decade investigating the issue find that peak large-scale warmth during Medieval times did not approach modern levels of warmth, whether or not this is the case is largely irrelevant to the question of whether human's are changing the climate. The primary pillars of evidence for that proposition are instead are (i) the basic, well established physics of the warming impact of greenhouse gases and (ii) the close match between model predictions of the patterns of warming expected from greenhouse gas concentration increases, and the observed patterns of change in climate over the period of roughly the past century during which widespread instrumental data are available.

3. It is alleged that proxy temperature deductions and instrumental temperature data have been improperly combined to conceal mismatch between the two data series.

An attempt to hide the difficulty of combining these two data series and to mislead is alleged to be revealed in the following sentence in a November 1999 email from Professor Phillip Jones which is alleged to imply a conscious attempt to mislead: "I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years (i.e. from 1981 onwards) and from 1961 for Keith's to hide the decline".

QUESTIONS TO ADDRESS:

- i. What is the meaning of the quotation from the 1999 email?
- ii. How do you justify combining proxy and instrumental data in a single plotted line?
- iii. What method do you use?

RESPONSE

Jones has indicated in published accounts that he was using the term "trick" (in apparent reference to our own 1998 *Nature* article) in the sense often used by people, as in "bag of tricks", or "a trick to solving this problem...", or "trick of the trade". Scientists use the term "trick" to describe a clever approach to a problem, rather than something inappropriate or nefarious.

In referring to our 1998 *Nature* article, Jones was simply pointing out the following: our proxy reconstruction ended in 1980 (when the proxy data set we were using terminates—see item #1 above) so, it didn't include the warming of the past two decades. We therefore also showed in our article the more recent instrumental temperature data which extended through 1995, so that the reconstruction could be viewed in the context of recent instrumental temperatures. The separate curves for the proxy-based temperature reconstruction and for the instrumental temperature data were clearly labeled, and the data for both curves were available in the public domain at the time of publication for anyone who wanted to download them.

Nature in an editorial published in Dec '09 stated the following (emphasis added): *The stolen e-mails have prompted queries about whether Nature will investigate some of the researchers' own papers. One e-mail talked of displaying the data using a 'trick' — slang for a clever (and legitimate) technique, but a word that denialists have used to accuse the researchers of fabricating their results. It is Nature's policy to investigate such matters if there are substantive reasons for concern, but nothing we have seen so far in the e-mails qualifies.*

The reference to "hide the decline" and "Keith" was to separate work by Keith Briffa and colleagues, already discussed in my previous response to item #1 above. The "decline" refers to the well-known post-1960 decline in the temperature response of the high-latitude tree-ring density measurements used by Briffa and colleagues. This is the so-called "divergence problem". Briffa and colleagues were very clear in their paper that the post-1960 tree-ring density data in their dataset should not be used in reconstructing temperatures because of that problem. This problem was indeed the main topic of their original (1998) article.

While "hide" was perhaps poor wording on Jones' part, he was simply referring to something that was very much out in the open—that the Briffa et al tree ring density dataset should not be used as a temperature record after 1960 because of the "divergence problem" with that particular dataset.

The graph that Jones was preparing was evidently to appear on the cover of a 1999 World Meteorological Report. It's a relatively obscure report (i.e. not nearly as prominent as the IPCC report), but it is generally read, to my knowledge, by non-specialists. I believe that Jones was trying to simplify the plot so that it conveyed the basic conclusions of paleoclimate temperature reconstructions to non-experts in a simple,

accurate, but not overly cluttered or technical manner. I suspect he didn't want to distract the readers by including data known to be unreliable (the post 1960 tree-ring density-based temperature reconstruction of Briffa et al) and that is all he really meant by "hide". While it does appear that Jones merged the proxy and instrumental datasets into a single set of curves in that figure, I believe he was simply trying to present the basic picture (what we know from the combined information from long-term proxy and more recent instrumental data) in simple terms, to a non-technical audience. *To my knowledge, neither Jones nor any other researchers have done this in any figures that have appeared in the actual scientific literature.*

5. It is alleged that there have been improper attempts to influence the peer review system and a violation of IPCC procedures in attempting to prevent the publication of opposing ideas.

It is alleged that there has been an attempt to subvert the peer review process and exclude publication of scientific articles that do not support the Jones-Mann position on global climate change. A paper by Soon & Balunias was published in the Journal *Climate Research* arguing that the 20th century was not abnormally warm. An email from Professor Michael Mann on 11th March 2003 contained the following: "I think we have to stop considering Climate Research as a legitimate peer-reviewed journal. Perhaps we should encourage our colleagues in the climate research community to no longer submit to, or cite papers in, this journal." The allegation is that journals might be pressured to reject submitted articles that do not support a particular view of climate change.

In an email to a fellow researcher in June 2003, Briffa wrote: "Confidentially I now need a hard and if required extensive case for rejecting (an unnamed paper) to support Dave Stahle's and really as soon as you can."

In an email to Mann on 8th July 2004, Jones wrote: "The other paper by MM is just garbage. [...] 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!" The allegation is of an attempt to prevent ideas being published and the author being prepared to subvert the peer review process for a journal and to undermine the IPCC principle of accounting properly for contradictory views.

Give full accounts of the issue in relation to the journal *Climate Research*, the June 2003 email, and the March 2004 email to Mann ("recently rejected two papers (one for *Journal of Geophysical Research* & one for *Geophysical Research Letters*) from people saying CRU has it wrong over Siberia. Went to town over both reviews, hopefully successfully. If either appears I will be very surprised". Are the first two instances evidence of attempts to subvert the peer review process? In relation to the third, where do you draw the line between rejecting a paper on grounds of bad science etc, and attempting to suppress contrary views? To what extent is your attitude to reviewing conditioned by the extent that a paper will set back the case for anthropogenic global warming and the political action that may be needed to mitigate it? What is the justification for an apparent attempt to exclude contrary views from the IPCC process?

QUESTIONS TO ADDRESS:

- i. Give full accounts of the issue in relation to the journal *Climate Research*, the June 2003 email, and the March 2004 email to Mann ("recently rejected two papers (one for *Journal of Geophysical Research* & one for *Geophysical Research Letters*) from people saying CRU has it wrong over Siberia. Went to town over both reviews, hopefully successfully. If either appears I will be very surprised".*
- ii. Are the first two instances evidence of attempts to subvert the peer review process?*
- iii. In relation to the third, where do you draw the line between rejecting a paper on grounds of bad science etc, and attempting to suppress contrary views?*
- iv. To what extent is your attitude to reviewing conditioned by the extent that a paper will set back the case for anthropogenic global warming and the political action that may be needed to mitigate it?*
- v. What is the justification for an apparent attempt to exclude contrary views from the IPCC process?*

RESPONSE

As I was centrally involved in the discussion in question over the flawed Soon & Baliunas "Climate Research" paper, I am more than happy to speak to this particular topic. The allegation that the exchange in question reflected some kind of effort to suppress a particular viewpoint or subvert the peer review process is both false and offensive. The allegation attempts to blur the essential distinction between one's views and the validity of one's arguments and reasoning. It has always been my philosophy that it is only the latter--and not the former--that should be considered in determining whether work is appropriate for publication in the peer-reviewed literature.

There were concerns among a number of climate scientists, as indicated in a series of email exchanges from which this particular exchange was taken, regarding a number of papers published by the journal "*Climate Research*" over a several year period, and involving a particular editor as discussed further below. The problem was not that the papers were contrarian in their viewpoint, but rather that they reflected atrocious scholarship and suffered from serious errors that should have been caught during any legitimate peer review process. Once published, the papers were in several cases used by the authors to argue that mainstream climate research findings were fundamentally flawed, and to support high-profile character attacks against mainstream climate researchers.

One striking example was a paper by Michaels and McKittrick [McKittrick, R., and Michaels, P.J. (2004). A test of corrections for extraneous signals in gridded surface temperature data. *Climate Research*, 26: 159–173]. Upon publication of the paper, Michaels wrote an article on the antiscience website "techcentralstation" billing it as a "bombshell" paper that would "knock the stuffing out of" the findings of the IPCC. Michaels billed the paper this way: "*After four years of one of the most rigorous peer reviews ever, Canadian Ross McKittrick and another of us (Michaels) published a paper searching for "economic" signals in the temperature record. ... The research showed that somewhere around one-half of the warming in the U.N. surface record was explained by economic factors, which can be changes in land use, quality of instrumentation, or upkeep of records.*"

After what Michaels called "one of most rigorous peer reviews ever", Australian researcher Tim Lambert easily determined that the authors mistakenly used degrees rather than the required radians in calculating the cosine functions used to spatially weight their estimates. This mistake rendered every calculation in the paper incorrect, and the conclusions invalid. There were still other independent and equally fundamental errors in the paper that would have rendered it entirely invalid anyway. To the journal's credit, it published a criticism of the paper by Benestad (2004) to this effect. The paper was handled by an editor from New Zealand named Chris de Frietas—I will return to this point below.

The *Climate Research* paper by astronomers Willie Soon and Sallie Baliunas of the Harvard University-affiliated Harvard-Smithsonian Center for Astrophysics and David Legates of the University of Delaware was similarly problematic. The paper claimed to demonstrate that 20th century global warmth was not unusual in comparison with conditions during Medieval times, using a fundamentally illogical argument. This deeply flawed study was immediately trumpeted by fossil fuel interests, sympathetic news outlets such as the Wall Street Journal, and politicians in the U.S. congress and the Bush administration as calling into question the entire basis of the science underlying human-caused climate change. The paper was published in its original, un-edited form in yet another journal "Energy and Environment" (something that is highly unusual and indeed, typically considered inappropriate). The managing editor of the American Geophysical Union weekly newsletter "*Eos*" contacted me regarding their concern over this paper, and requested that I draft a response for the "forum" section of "*Eos*": "*Dear Dr. Mann, I am the managing editor for Eos, the weekly newspaper of the American Geophysical Union. Late last week, the Eos editor for atmospheric sciences, Ellen Mosley-Thompson, asked me if Eos would publish what she called "a position paper" by you, Phillip Bradley, et al that would, in effect, be a refutation to a paper by Soon et al. noting that the article was picked up by the "Discovery Channel and other print and electronic media that reach the general public.*"

The Soon and Baliunas paper was refuted in an *Eos* piece written by me and a team of a dozen leading climate researchers, and in additional articles in *Science*, and elsewhere. However, it took some time for

the rebuttals to work their way through the slow process of the scientific peer review. In the meantime the study was quickly seized upon by those seeking to sow doubt in the validity behind the scientific consensus concerning the evidence for human-induced climate change.

The publication of the study had wider reverberations throughout the academic and scientific institutions connected with it. As described in several articles in the *Harvard Crimson*, the association of the study with the "Harvard" name caused some notable unease among Harvard faculty. John Holdren, the Heinz professor of environmental policy at the Kennedy School of Government stated that "the critics are right. It's unfortunate that so much attention is paid to a flawed analysis, but that's what happens when something happens to support the political climate in Washington." Professor Daniel Schrag of the Department of Earth and Planetary Sciences (a MacArthur "Genius" fellow) stated that he "did not think Soon and Baliunas' approach to finding a global average temperature was as honest as other approaches". According to Schrag "The bottom line is that this paper is suggesting that the unusually warm weather we've been having for the last 100 years is part of natural variability. We have observations to show that that's not the case." The reputation of *Climate Research* was deeply tarnished in the process. The editor at *Climate Research* that handled the Soon et al paper, Chris de Frietas, had a controversial record of past editorial practices as detailed in a subsequent article in *Scientific American*. In an unprecedented (to my knowledge) act of protest, *Climate Research* editor-in-chief Hans von Storch resigned when the publisher would not allow him to write an editorial indicating that the peer review process had clearly failed with the publication of the Soon et al paper. In an interview in the *Chronicle of Higher Education*, Von Storch indicated that he felt contrarians were submitting papers to this journal [for legal reasons in relation to publication of this submission, parts of Professor Mann's response have been redacted by the Climate Change E-mails Review] A detailed account of these events has been provided by the *Wall Street Journal*, *New York Times*, *Scientific American*, and *Nature*. [redacted]

Under these circumstances, I make no apologies for having asked a group of respected colleagues if we should consider not submitting papers to the journal or citing the increasingly bad papers that were appearing in the journal, and encouraging the same of other colleagues. Indeed, other colleagues suggested stronger actions (e.g. advocating the firing of the editor in question)—an action which I did not support. [redaction]. As is clear in my comments above, I was particularly disturbed by the fact that certain climate change contrarians, rather than choosing the noble path of making their case as rigorously and objectively possible by publishing in mainstream venues e.g. as direct comments on those mainstream findings they believed to be flawed, instead sought an "end run" around the peer-review system [redacted]

I support the publication of "skeptical" papers that meet the basic standards of scientific quality and merit. I myself have published scientific work that has been considered by some as representing a skeptical point of view on matters relating to climate change (for example, my work demonstrating the importance of natural oscillations of the climate on multidecadal timescales). Skepticism in the truest scientific sense of the word is good and is indeed essential to science. *Skepticism* should not be confused, however, with *contrarianism* that does not meet the basic standards of scientific inquiry.

6. The scrutiny and re-analysis of data by other scientists is a vital process if hypotheses are to [*sic*] rigorously tested and improved. It is alleged that there has been a failure to make important data available or the procedures used to adjust and analyse that data, thereby subverting a crucial scientific process.

It is alleged that there has been a systematic policy of denying access to data that has been used in publications, referring to an email from Jones to Mann on 2nd February 2005 which contains the following:

"And don't leave stuff lying around on ftp sites - you never know who is trawling them. The two MMs have been after the CRU station data for years. If they ever hear there is a Freedom of Information Act now in the UK, I think I'll delete the file rather than send to anyone. Does your similar act in the US force you to respond to enquiries within 20 days?—our does! The UK works on precedents, so the first request will test it. We also have a data protection act, which I will hide behind".

QUESTIONS TO ADDRESS:

- i. Do you agree that releasing data for others to use and to test hypotheses is an important principle?*
- ii. If so, do you agree that this principle has been abused?*
- iii. If so, should not data be released for use by those with the intention to undermine your case, or is there a distinction you would wish to make between legitimate and illegitimate use?*
- iv. If not, do others have reasonable access to the data at all levels and to the description of processing steps, in order to be able to carry out such a re-analysis?*
- v. Can you describe clearly the data-sets and relevant meta-data that have been released; what has not been released and to what extent is it in useable form? Where has it been released?*
- vi. Where access is limited, or not possible, or not meaningful, for legitimate reasons please explain why?*

RESPONSE

Other than having received the cited email above from Jones, I am not involved in the specific matters discussed here. I am nonetheless happy to comment on the matters under discussion more generally.

I believe it is important to release data used in a scientific study when at all possible. It has always been my policy with my own studies to place all data used in my scientific work in public depositories such as the NOAA world data centers (and on my own website) following the publication of the work. There are exceptions that inevitably arise to this rule. In more collegial times, before the science of paleoclimatology and the scientists who practiced it were besieged by the concerted attacks of professional climate change deniers, it was common practice for scientists to extend the courtesy to fellow scientists working in good faith, to make use of their own unpublished data. But this was done on a provisionally basis—certain scientists would allow us to use their unpublished data in our analyses on a proprietary basis. We could not distribute those data until the scientist who had provided them to us had the opportunity to himself/herself publish the data. Such arrangements insured that scientists whose work was aimed at compiling and synthesizing information from a large network of data, had the most up-to-date datasets with which to perform the analyses.

That is the way things were a decade ago, when I was first entering into this field. Over the years, as a concerted effort arose to undermine the science of climate change and to attack climate scientists personally, the attackers exploited this situation by making the disingenuous claim that climate scientists were unwilling to provide their data. In reality, we were providing all of the data that we at liberty to provide, without violating the agreements that we'd made with other scientists to use their unpublished data. In subsequent studies to my original work in the late 1990s, I have avoided this complication by only using data in my analyses that I was at liberty to distribute at the time of publication. While this limits me

to using somewhat out-of-date datasets, and makes any scientific work become outdated more quickly, the alternative---setting myself up for attack by those intentionally trying to undermine our science and our integrity, was unacceptable.

My understanding is that the complaints against CRU scientists over data availability are of a similarly dubious nature. As I understand it, the complaints regard relatively small amounts of instrumental climate data that CRU has been provided on a proprietary basis by other countries, under legal obligations not to release those data themselves. It appears, to me, that some third parties looking to make mischief engaged in a massive flood of frivolous FOIA attempts against CRU scientists demanding that they release these proprietary data--the attackers, almost certainly knowing, that CRU was not legally allowed to release those data.

To me, these incidents speak far more to the dishonorable intentions of who would bombard scientists and scientific institutions with frivolous FOIA attacks, than to any impropriety on the part of CRU scientists.