

CLIMATE CHANGE

Response to Simmonds and Steffen

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We thank Drs Simmonds and Steffen for their interest in our paper. In response, and at the outset, we confirm that the paper under discussion¹ represents the combined and considered professional views of its authors. Beyond that, we do not wish to respond to suggestions that we have not studied the literature thoroughly, not understood some of the concepts under discussion, or not given consideration to the wide range of legitimate views that can defensibly be held on the global warming issue—towards one end of which lies the Inter-governmental Panel on Climate

Change (IPCC) orthodoxy that Simmonds and Steffen so spiritedly espouse. Here, in the interests of brevity, we restrict our remarks to those points amongst the matters that they raised that we view as particularly important or informative.

Peer review

In our parallel paper in this issue,² we respond to other comments on peer review and offer one practical improvement that could be put in place immediately.

The science-quality problem is in fact wider than the peer review process itself, and extends to the need for comprehensive audit of the quality of the science-based information on climate risk that is currently being used by governments to set public policy. Though the IPCC was set up with precisely the aim of summarizing the science for policymakers, many independent climate experts have observed that—administered under the overarching United Nations Framework Convention on Climate

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¹ Carter, R. M., C. R. de Freitas, I. M. Goklany, D. Holland, and R. S. Lindzen (2006), 'The Stern Review: A Dual Critique—Part I: The Science', *World Economics*, 7(4): 167–198.

² Carter, R. M., C. R. de Freitas, I. M. Goklany, D. Holland, and R. S. Lindzen (2007), 'Climate Science and the Stern Review', *World Economics*, 8(2): 161–182.

Change (UNFCCC) which presumes a dangerous human influence on climate—the IPCC has turned into an alarmist advocacy body whose influence is then strengthened by other reviews which accept its science advice, such as that of Sir Nicholas Stern. Even worse, the IPCC is alarmist only about global warming and completely ignores the possible threat of climatic cooling. It is also a matter of public record that some scientists have withdrawn from the IPCC process because of dissatisfaction with its probity and methods. Valuable though it might be for IPCC to continue to provide summaries of the science of climate change, it is simply no longer credible, if indeed it ever was, to pretend that the IPCC is acting as an adequate audit body.

One solution being actively considered to deal with this problem is the establishment of a new and independent audit body, for instance as a secretariat of the AP6 climate partnership. Such an audit committee would not summarize science or engineering proposals per se, but would instead carry out a rigorous audit regarding the reliability of technical advice that was tendered to the AP6—the IPCC, and any other interested parties, would be welcome to submit reports on climate change, or shorter summaries of them, should they wish, but in the full knowledge that they would then be rigorously audited.

Models

Simmonds and Steffen say that we appear antagonistic “toward models

and their use in understanding of the workings of past, present and future climates”. This is not the case, in the sense that we accept that General Circulation Models (GCMs) serve a valuable heuristic function. Carter *et al.* (2007) have dealt at greater length with climate models and show how models do indeed aid our understanding of how the atmosphere cools. But as recently confirmed also by two IPCC Working Group 1 lead authors, we do assert that GCMs are not suitable for use as predictive tools. For example, Kevin Trenberth indicates (*Nature*, Climate Feedback, June 4, 2007) that,

The state of the oceans, sea ice, and soil moisture has no relationship to the observed state at any recent time in any of the IPCC models. There is neither an El Niño sequence nor any Pacific Decadal Oscillation that replicates the recent past; yet these are critical modes of variability that affect Pacific rim countries and beyond...

and adds,

...In fact there are no predictions by IPCC at all. And there never have been. The IPCC instead proffers ‘what if’ projections of future climate that correspond to certain emissions scenarios.

In a second example, a New Zealand audit recently showed that the National Institute of Water and Atmosphere’s (NIWA) climate predictions were right only 48% of the time, i.e. no better than chance. This fact prompted NIWA’s Jim Renwick to comment, “Climate prediction is hard, half of the variability in the climate

system is not predictable, so we don't expect to do terrifically well".

Therefore, our view is that for the foreseeable future GCMs will continue to be limited to helping our understanding and cannot be used as evidence or proof of human interference with the climate. We note that this view is consistent with that of the IPCC, which says (Chapter 14.2.2.2 of WGI of IPCC, 2001):

In climate research and modeling, we should recognize that we are dealing with a coupled non-linear chaotic system, and therefore that the long-term prediction of future climate states is not possible.

Given the large uncertainty that remains over the effects of aerosols, and other known or potential forcing factors, we dispute the claim of Simmonds and Steffen that climate models have an "extraordinary performance and level of skill" and point to the continuing inability of the current models to simulate important aspects of the climate. Models may be based on well-established equations, but they are hardly the same as these equations. Numerical approximations never are, and the inevitable differences tend to add up with time.

As we have developed in the conclusions to Carter *et al.* (2007), the fact that climate is inherently unpredictable is the prime reason why the response to undesirable climatic events needs to be adaptive. In a similar fashion, humanity deals reactively with other unpredictable natural events such as earthquakes or volcanic eruptions. Attempting to "stop climate

change" without a sound scientific basis for predicting whether it will get warmer or cooler is neither a rational nor a responsible public policy.

Equally important, given that uncertain projections of climate change are then used as inputs to simplified biophysical and/or socioeconomic models to divine impacts, we have even less confidence in the ability, and the propriety of using impact assessments to drive trillion dollar policy decisions. We note that to the best of our knowledge there has never been a quantitative assessment of the confidence intervals surrounding impacts estimates that considers how uncertainties propagate through the chain of assumptions and models used to develop impacts. This long chain starts with the economic assumptions used to 'derive' emission scenarios and proceeds to estimates of the concentration of each greenhouse gas, to its radiative forcing, to the resulting climate changes at the relevant local or regional scales, to the outputs of biophysical models and then, more often than not, to outputs of socioeconomic models. We note in passing that many of the assessments also ignore or simplify feedbacks between and within the various models.

Tree-ring temperature reconstructions

Simmonds and Steffen take issue with our treatment of the 'hockey stick' reconstruction of historic global temperatures and suggest that Mann *et al.* presented their results with caution. Perhaps they did, but the IPCC

certainly didn't. They trumpeted them, and without any restraint or caveat being added by Mann *et al.* We made clear on page 173 of Carter *et al.* (2006) that our comments related to the use of this study in IPCC, 2001, and we make the additional point here that Dr Mann was a lead author of the chapter that gave prominence to the 'hockey stick' conclusion. The lengths to which the 2001 IPCC report goes to highlight the message of 1,000 years of steady global temperature leading up to the 20th century is in stark contrast to the review of temperature reconstructions made clear in the summary of the first IPCC assessment report in 1990, which says:

We conclude that despite great limitations in the quantity and quality of the available historical temperature data, the evidence points consistently to a real but irregular warming over the last century. A global warming of larger size has almost certainly occurred at least once since the end of the last glaciation without any appreciable increase in greenhouse gases. Because we do not understand the reasons for these past warming events, it is not yet possible to attribute a specific proportion of the recent, smaller warming to an increase of greenhouse gases.

The Mann *et al.* tree-ring studies and the others as presented with it in IPCC, 2001 had a pivotal role in changing the perception of the state of scientific knowledge from the cautious IPCC, 1990 conclusion, quoted above. If it could be proved beyond doubt that it is currently warmer now than at any earlier time, while this would still not be scientific proof of anthropogenic global warming, it would be

easier to argue the case for it, and for massive policy changes. The 'hockey stick' was widely portrayed as the 'smoking gun'. Gerald North, who chaired the NRC panel we discuss later, had no doubt of its importance. After seeing the draft IPCC Third Assessment Report, he was reported to have said:³

There are too many independent pieces of evidence, and there's not a single piece of contradictory evidence ... The planet had been cooling slowly until 120 years ago, when, bam!, it jumps up ... We've been breaking our backs on [greenhouse] detection, but I found the 1000-year records more convincing than any of our detection studies.

While it may be argued that the IPCC could not be held responsible for the flaws in the Mann *et al.* studies, it is responsible for the prominence it was given in the TAR and the subsequent process of releasing its findings, and the way in which it presented them, along with two other tree-ring studies, in a graph which it used to substantiate the claim that "the 1990s are likely the warmest decade, and 1998 the warmest year, in at least a millennium".⁴ The graph is deceptive. Though it does not admit it, the curve for the Briffa (2000) reconstruction after 1960 and the instrumental temperature record before 1902 are not plotted. The recent 2007 IPCC report does now show the full instrumental temperature curve and, in discussing the 'divergence' problem, offers this,

³ Kerr, R. A. (2000), 'Global Warming: Draft Report Affirms Human Influence', *Science*, 288: 589-590.

⁴ Figure 2.21, IPCC, 2001, WGI, Chapter 2, page 134.

still unacceptable, explanation for the continued omission of the curve for Briffa's post-1960 data from the reconstructions it now shows:

Briffa et al. (2001) specifically excluded the post-1960 data in their calibration against instrumental records, to avoid biasing the estimation of the earlier reconstructions (hence they are not shown in Figure 6.10).⁵

The Briffa (2000) reconstructed temperature curve falls after 1960 while instrumental temperatures rise. Had this been shown in IPCC, 2001, and the 'divergence' problem more clearly explained, critical readers would have realized that none of the reconstructions simulates the instrumental temperature record closely after 1950, and none goes beyond 1980. Had the instrumental temperature curve from 1860 to 1902 been plotted, it would also be clear that no reconstruction simulated that period either. When all the missing data are shown,⁶ the inadequacy of the reconstructions is revealed. If this type of selective data presentation had been undertaken in a prospectus for public funding, or the report of a medical trial, the authors would undoubtedly have been called to account by their professional bodies.

In defence of modelling in general, Simmonds and Steffen say "it could be argued that the whole of science is based on "models" of reality", with which we do not disagree. In fact, we believe this point should be kept in

mind before uncritically accepting such studies if they could influence the course of dramatic policy actions. In tree-ring studies, the response of trees to temperature and other factors is modelled in order to derive the relationship between temperature and growth. The relationship is complex, with numerous confounding factors. The evidence is plain that, over the supposedly known instrumental temperature record, the models used are reliable for less than half the time, during periods when temperatures rose fairly linearly. For the remainder of the record, the models fail or are not demonstrable. In particular, the reconstructions are unable to simulate the current warm period and we are entitled to say, on that basis, they can not be relied upon as indicators of the magnitude of other warm or indeed cool periods. Tree-ring studies demonstrate yet again "unwarranted credence to model projections over firmly established data and findings."

The 'hockey stick' falsification

The falsification of the 'hockey stick' is important for several reasons. First, it invalidates any conclusion, not only based upon it but on any similar studies, that the late 20th century warming was exceptional. Second, it demonstrates the inadequacy of the peer review system in ensuring proper disclosure; and third, it exposed the 'cherry picking' and lack of independence that exist in parts of the palaeoclimatic field. The affair also demonstrates the failure of the IPCC to fulfil its role of assessing "on a

⁵ IPCC, 2007, WGI, Chapter 6, page 473.

⁶ Stephen McIntyre shows the effect of plotting omitted data at: <http://www.climateaudit.org/?p=1737#more-1737>

comprehensive, objective, open and transparent basis the scientific, technical and socio-economic information relevant to understanding the scientific basis of risk of human-induced climate change...⁷

Simmonds and Steffen take exception to our factual statement in Carter *et al.* (2006), where we say,

Two recent US reports, one by the National Research Council⁸ (NRC) and one by Edward Wegman,⁹ Chair of the National Academy of Sciences Committee on Applied and Theoretical Statistics, have invalidated the 'hockey stick' conclusion.

After questioning whether we had read the actual 157-page NRC report, they quote at length, not from it, but from a 4-page summary of it. While this does not contradict the full NRC report, it is like all summary documents, prone to be the product of cherry picking. It mentions that it is warmer now than 400 years ago, which is not in dispute but fails to mention any of the technical faults in the Mann *et al.* studies that the full report carefully details. The NRC panel was drawn from the same milieu as the IPCC. Most if not all of the NRC panel were well acquainted with Dr Mann and were on record as

supporters of the IPCC consensus. They are all highly respected eminent scientists and, however politely they stated them, each of their findings separately invalidated the hockey stick, as indeed does the very limited criticism that Simmonds and Steffen select from the summary:

Even less confidence can be placed in the original conclusions by Mann *et al.* (1999) that "the 1990s are likely the warmest decade, and 1998 the warmest year, in at least a millennium."

However, Simmonds and Steffen are dismissive of Wegman *et al.*, and ignore their findings except:

Overall, our committee believes that Mann's assessments that the decade of the 1990s was the hottest decade of the millennium and that 1998 was the hottest year of the millennium cannot be supported by his analysis.

A full reading of the two reports, and of the transcript of the Hearings of the US House of Representatives,¹⁰ leaves no room for any ambiguity. Ralph Cicerone, president of the National Academy of Sciences agreed that Dr Wegman, who is chairman of the NAS Committee on Applied and Theoretical Statistics, had credibility in these matters.¹¹ Gerald North, chairman of the NRC panel said, "In fact, pretty much the same thing is said in

⁷ Principles Governing IPCC Work: <http://www.ipcc.ch/about/princ.pdf>

⁸ NRC (2006), Committee on Surface Temperature Reconstructions for the Last 2,000 Years, National Research Council, National Academies Press.

⁹ Wegman E. J. *et al.*, Ad Hoc Committee Report on the "Hockey Stick" Global Climate Reconstruction, commissioned by the US Congress House Committee on Energy and Commerce, 2006. Available at: http://republicans.energycommerce.house.gov/108/home/07142006_Wegman_Report.pdf

¹⁰ Hearings before the Subcommittee on Oversight and Investigations of the Committee on Energy and Commerce of the United States House of Representatives, 109th Congress, 2nd Session, July 19 and July 27, 2006. Available (150 Mb) at: http://frwebgate.access.gpo.gov/cgi-bin/getdoc.cgi?dbname=109_house_hearings&docid=f:31362.pdf

¹¹ Hearing transcript page 735.

our report.”¹² These documents invalidate the ‘hockey stick’, and expose fundamental problems not only in the science, but also in the way it was conducted, which included a lack of proper disclosure and independence between studies. The Wegman *et al.* report reinforced and supported the earlier conclusions of McIntyre and McKittrick, whose papers had led to the hearings, saying on page 48:

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. We comment that they were attempting to draw attention to the deficiencies of the MBH98-type methodologies and were not trying to do paleoclimatic temperature reconstructions.

In response to the letter from Chairman Barton and Chairman Whitfield, Dr. Mann did release several websites with extensive materials, including data and code. The material is not organized or documented in such a way that makes it practical for an outsider to replicate the MBH98/99 results. For example, the directory and file structure Dr. Mann used are embedded in the code. It would take extensive restructuring of the code to make it compatible with a local machine. Moreover, the cryptic nature of some of the MBH98/99 narratives means that outsiders would have to make guesses at the precise nature of the procedures being used.

The divergence problems and cherry picking

The NRC report also addressed an important matter beyond falsifying the ‘hockey stick’, and that is the previously largely ignored ‘divergence problem’. Having had the problem publicly presented to them,¹³ the NRC panel correctly identified it as seriously limiting the confidence that can be placed upon the reconstructions used by the Stern Review and the IPCC to suggest that current warming is exceptional in the last 1,300 years. While studies on ‘divergence’ differ on the extent of the problem and speculate as to possible causes, what NRC (2006) say on page 110 is:

The observed discrepancy between some tree ring variables that are thought to be sensitive to temperature and the temperature changes observed in the late 20th century (Jacoby and D’Arrigo 1995, Briffa et al. 1998) reduces confidence that the correlation between these proxies and temperature has been consistent over time. Future work is needed to understand the cause of this “divergence,” which for now is considered unique to the 20th century and to areas north of 55°N (Cook et al. 2004). For tree ring chronologies, the process of removing biological trends from ring-width data potentially obscures information on long-term changes in climate.

NRC (2006) does not have a great deal to say on how the proxies are

¹² Hearing transcript page 85.

¹³ McIntyre and McKittrick (2006), Presentation to the National Academy of Sciences Expert Panel, ‘Surface Temperature Reconstructions for the Past 1,000–2,000 Years’. Available at: <http://www.climateaudit.org/pdf/NAS.M&M.pdf>

selected, but we can look at what researchers in the field say. For instance, on pages 90–92 of Esper *et al.* (2003),¹⁴ they say:

Before venturing into the subject of sample depth and chronology quality, we state from the beginning, “more is always better”. However as we mentioned earlier on the subject of biological growth populations, this does not mean that one could not improve a chronology by reducing the number of series used if the purpose of removing samples is to enhance a desired signal. The ability to pick and choose which samples to use is an advantage unique to dendroclimatology. That said, it begs the question: how low can we go?

One answer comes from Jacoby and D’Arrigo (1989)¹⁵ where they state on page 44 that they sampled 36 northern boreal forest sites within the preceding decade, of which the ten “judged to provide the best record of temperature-influenced tree growth” were selected. In climate science, it is not always the case that how the samples are selected is reported, and that the samples analysed but not used are archived. Responding to requests from critics for data analysed but not actually used, Dr Jacoby replied, *inter alia*:¹⁶

Most of our research has been mission-oriented, dendroclimatic research. That means to find climatically-sensitive, old-aged trees and sample them in order to extend the quantitative record of climatic variations.

If we get a good climatic story from a chronology, we write a paper using it. That is our funded mission. It does not make sense to expend efforts on marginal or poor data and it is a waste of funding agency and taxpayer dollars. The rejected data are set aside and not archived ... As an ex-marine I refer to the concept of a few good men. A lesser amount of good data is better without a copious amount of poor data stirred in.

Thus it can be seen that proxy reconstructions are highly selective. In a presentation to the NRC panel, Rosanne D’Arrigo was reported to have put up a slide about ‘cherry picking’ and explained to the panel that that’s what you have to do if you want to make cherry pie. Also disconcerting is the evidence that, as well as having authors in common, certain proxy series are used repeatedly in supposedly independent temperature reconstructions. Wegman *et al.* show, on page 46 of their study, that of twelve tree-ring studies they examined, all twelve share ‘Polar Urals’ and ‘Tornetrask proxies’. Eight use Jacoby’s ‘Mongolia’ and seven use ‘Jacoby’s Treeline’. No fewer than eight of the twelve use the Bristlecone/Foxtail PC1 of Mann *et al.* which NRC (2006) say, “should be avoided” and which Wegman *et al.* say may not be valid as a temperature indicator.

Tree-ring studies are based on the premise¹⁷ that by processing a

¹⁴ Esper, J., Cook, E. R., Krusic, P. J., Peters, K., and Schweingruber, F. H. (2003), ‘Tests for the RCS Method for Preserving Low Frequency Variability in Long Tree-Ring Chronologies’, *Tree Ring Research*, 59(2): 81–98.

¹⁵ Jacoby, G. C., and D’Arrigo, R. D. (1989), ‘Reconstructed Northern Hemisphere Annual Temperature Since 1671 Based on High Latitude Tree-Ring Data from North America’, *Climatic Change*, 14: 39–59.

¹⁶ Response to request to the journal, *Climatic Change*, reported at <http://www.climateaudit.org/?p=29>

¹⁷ See NRC (2006), page 47.

measurable characteristic, such as the width of the ring, a correlation with instrumental temperature is obtained *which is presumed to have been constant in historic times* up to a thousand or more years ago. However, as just discussed, not all samples, and in the case mentioned only a minority, tell the desired story—and then mostly for less than half the time for which we have instrumental temperatures.

IPCC, 2001, and particularly the Summary for Policymakers failed to emphasise the known concerns over tree-ring reconstructions and failed to show data that might have alerted critical readers to the fact that confidence in the ‘hockey stick’ was misplaced. IPCC, 2007 failed to fully accept the mistakes and lack of disclosure of IPCC, 2001, made no reference at all to Wegman *et al.* and only the most cursory to NRC (2006).

Conclusion

We do not doubt the sincerity and professionalism with which Simmonds and Steffen defend their support for the consensus view of the IPCC, but our professional judgement is different. We are not against models,

per se, but against unjustified confidence that they can be used predictively.

We do not believe that one can begin with a conclusion, as the UNFCCC does, and expect that good science will follow to justify it.

In approaching the uncertainty of the future states of the climate, the economy, or for that matter, any other aspect of life “over 50, 100, 200 years or more”, we also believe that “modelling requires caution and humility, and the results are specific to the model and its assumptions”.¹⁸

There are some simple, inexpensive precautions that can be taken. One is to be tolerant and civil to dissenting views. Another is to insist upon high standards for the disclosure of data and methodology, and to insist upon disclosure of any lack of independence between different studies. A third is to appropriately and objectively characterize confidence intervals surrounding estimates from chains of models using assumptions and simplified model components (modules). These precautions are routine in many scientific fields, and it is greatly to the detriment of climate science that they are not always applied there too.

¹⁸ The Stern Review, Executive Summary, page x.

