BIAS AND CONCEALMENT IN THE IPCC PROCESS: THE "HOCKEY-STICK" AFFAIR AND ITS IMPLICATIONS

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ABSTRACT

The climatic "hockey stick" hypothesis has systemic problems. I review how the IPCC came to adopt the "hockey stick" as scientific evidence of human interference with the climate. I report also on independent peer reviewed studies of the "hockey stick" that were instigated by the US House of Representatives in 2006, and which comprehensively invalidated it. The "divergence" problem and the selective and unreliable nature of tree ring reconstructions are discussed, as is the unsatisfactory review process of the IPCC Fourth Assessment Report that ignored the invalidation of the "hockey stick". The error found recently in the GISS temperature series is also noted. It is concluded that the IPCC has neither the structure nor the necessary independence and supervision of its processes to be acceptable as the monopoly authority on climate science. Suggestions are made as to how the IPCC could improve its procedures towards producing reports and recommendations that are more scientifically sound.

INTRODUCTION

Bias is an inevitable consequence of the belief we must have to be successful in whatever we do. Where there is a wide diversity of beliefs and the freedom and resources to pursue them, bias is less of a problem. In most scientific controversies the timescales and risks are such that we can let the passage of time settle them. In some, like medicine and climate change, we cannot. In medicine, despite centuries of study, there are few things absolutely safe or efficacious but we assume that most medical professionals would not propose medicines or procedures that they knew were poor in either respect. However, we have learnt through experience that it is unwise to allow pharmaceutical manufacturers, or others with a conflict of interest, to be the judges of these qualities. Carefully controlled studies are mandated specifically to avoid bias in the judgements as to which medicines and procedures are appropriate. High standards of record keeping and disclosure are enforced. It is inconceivable today that the developer of any medicine or procedure would be allowed to conceal test data or take a leading role in a review process that approved it. In comparison, climate research is in its infancy and almost entirely unregulated.

Many, particularly the conscientious young, have been persuaded that anthropogenic global warming is a very serious problem for mankind and one which governments can and should do something about. Sir David King, the UK's chief scientist said it was a more serious problem than terrorism[1]. So strong is the belief

[1]David King (2004): 'Climate Change Science: Adapt, Mitigate, or Ignore?' *Science* VOL 303 p. 176

among some that they are prepared to resort to civil disobedience, shut down power stations and disrupt major airports. It is by all measures as important a field of research as medicine, and ought to operate to standards at least as high, but it does not. On the contrary, it is steeped in bias, concealment and spin. The Stern Review[2] said "*The causal link between greenhouse gases concentrations and global temperatures is well established, founded on principles established by scientists in the nineteenth century*." This is both disingenuous and plainly wrong. Until the 1950's climate research was largely a branch of Meteorology, and what limited data were collected were largely to assist in weather forecasting. Similarly, the computer modelling that now dominates the climate debate evolved from the development of weather models. The dispute that has emerged is not over the basic science of the nineteenth century, or that a causal link exists between greenhouse gases and global warming, but concerns our ability to detect the contribution that a minor human increase in particular greenhouse gases makes to current climate change, and the possibility and economics of attempting to reduce it. Strong and well-founded scientific disagreement remains between those who say the limited observations since the beginning of the industrial era indicate a sensitivity of the Earth, to a doubling of carbon dioxide, of around 1°C or less, to which we can and should adapt, and those who say, based solely upon theory developed in numerical models, that the sensitivity is several times larger and that we must drastically reduce emissions.

This paper focuses on one strand of the dispute, the so-called "hockey stick" study, which suggested that little change occurred in global temperatures over the millennium that preceded the industrial era. Until recently, the "hockey stick" was strongly promoted as proof of human interference in the climate. The "hockey stick" story demonstrates that, contrary to what may be said elsewhere in this journal, much of the climate science in which we are invited to place trust is biased, sloppy and protected from exposure by concealment of the underlying data and methodology, and by a well organised "spin" process.

THE INTERGOVERNMENTAL PANEL ON CLIMATE CHANGE AND THE REPORTING PROCESS OF WORKING GROUP I

The IPCC, founded in 1988, is a complex and unusual organisation, with political, policy and scientific aspects. The panel itself is appointed by its member governments and therefore can hardly be policy neutral. It brings together a large network of experts who review and report on the technical and socio-economic information relevant for the understanding of climate change, its potential impacts and options for adaptation and mitigation. More detailed discussions of the IPCC structure and the interactions between the different milieus that comprise it can be found elsewhere[3]. This paper restricts itself to consideration of the process by which one of the three working groups, WGI, assesses the scientific basis of climate change. The WGI view is contained in four successive Assessment Reports between 1990/1 and 2007. We are frequently assured, without further explanation, that the WGI view represents the

[2]Nicholas Stern (2006): 'The Economics of Climate Change, The Stern Review'. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.

[3]David Henderson (2007): 'Governments and Climate Change Issues.' *World Economics* 8 (2). 183 228

consensus of a large number of experts. This claim needs to be treated with severe caution. Large numbers of persons are indeed involved, and may assent to the assessment as a whole, but individual chapters for each working group, each of which deals with many potentially controversial issues, and are written, reviewed and edited by much smaller groups. Other than naming the coordinating, leading and contributing authors, until recently little was published that gave any detailed indication of the IPCC process, beyond what the governing principles[4] laid down by its member governments say. These include some reassuring clauses:

> Clause 2 "*The role of the IPCC is to assess on a 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, its potential impacts and options for adaptation and mitigation. IPCC reports should be neutral with respect to policy, although they may need to deal objectively with scientific, technical and socio-economic factors relevant to the application of particular policies."*

> Clause 10 *"Differing views on matters of a scientific, technical or socio -economic nature shall, as appropriate in the context, be represented in the scientific, technical or socio-economic document concerned*."

> Appendix A, Clause 4.1 (3rd para.) "*All written expert, and government review comments will be made available to reviewers on request during the review process and will be retained in an open archive in a location determined by the IPCC Secretariat on completion of the Report for a period of at least five years*."

> Appendix A, 4.2.4.1 "*The Working Group/Task Force Bureau Co-Chairs should make available to reviewers on request during the review process specific material referenced in the document being reviewed, which is not available in the international published literature."*

However, as will be discussed, these entirely reasonable principles seem to be honoured more in the breach, and it may be that clause 3 of the principles makes it too easy for controversy to be brushed aside. It says:

> Clause 3 "*Review is an essential part of the IPCC process. Since the IPCC is an intergovernmental body, review of IPCC documents should involve both peer review by experts and review by governments*."

The IPCC undertakes no research itself and has few staff or resources but relies

[4]PRINCIPLES GOVERNING IPCC WORK**:** Approved at the Fourteenth Session (Vienna, 1 3 October 1998) on 1 October 1998, amended at the 21st Session (Vienna, 3 and 6 7 November 2003) and at the 25th Session (Mauritius, 26 28 April 2006). http://www.ipcc.ch/about/princ.pdf . See also Appendix A Procedures for the preparation, review, acceptance, adoption, approval and publication of IPCC reports. http://www.ipcc.ch/about/app a.pdf

upon those of its members. The USA is the biggest contributor of experts and resources, largely through the University Corporation for Atmospheric Research (UCAR). The $200 million of annual revenues of UCAR dwarf the IPCC's meagre annual budget of under $10 million. UCAR operates the Technical Support Unit for WGI, which largely organizes its meetings and provides the administrative support for its assessment reports. The UK, through the Climatic Research Unit, Hadley Centre and other publicly funded organizations is the second largest contributor of experts.

The IPCC relies on peer review for quality control. There is no common standard for this and the IPCC has no obvious procedures to guard against bias, undergoes no 'due diligence' checks on the validity of the science it summarizes and makes no checks to ensure that data and methodology of the science that it cites are available to critics. Indeed, as is discussed later, during the review process of the recent fourth assessment report[5] (AR4), the IPCC actually supported at least one author who had declined to release data. Authors of many of the key climate studies cited in the IPCC are brazen in their refusal to release data and methodology, as the governing principles seem rightly to require, and despite their leading roles in the IPCC process giving enhanced stature to their work. Repeatedly, the IPCC and its advocates strip the uncertainties from the science and present possibilities as strong probabilities or near certainties. Thus any belief that the IPCC review process is independent and trustworthy, and thus that it is an acceptable "auditor" of the state of climate research, is plain wrong.

THE FIRST IPCC ASSESSMENT

The science of climate change and anthropogenic global warming is immensely complex. Two issues that were identified in the first IPCC assessment report remain central to determining what part mankind may have played in the warming of the last three decades of the 20th century. They are the globally averaged instrumental temperature series, or "surface record", and the "reconstructions" of pre-instrumental temperatures from historic proxy records. The latter will be considered in some detail in this paper. While it reported model projections, which appeared alarming, IPCC, 1990[6] said,

> "*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.*"

[5]IPCC, 2007:Solomon *et al*., (eds.) 2007: 'Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change'. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA. (Available at http://www.ipcc.ch/)

[6]IPCC, 1990: '*Climate Change: The IPCC Scientific Assessment'* [Houghton, J. T *et al*., (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 362 pp.

The existence of earlier warm periods, supported by research documented, among others, by Hubert H Lamb[7] and accepted by the first IPCC assessment report, contradicted the alarming predictions from the models. At that time global temperatures were thought to have varied by more than 1°C over the previous 1000 years despite little change in greenhouse gas concentrations until the industrial era. It was also thought that it was still not as warm in 1990 as it was in the Medieval Warm Period. The extreme but frequently articulated view that, because of "positive feedback", a little further warming will lead to a "tipping point" [8] and "runaway" global warming was clearly unfounded in comparison with historic higher temperatures from which the earth has previously "recovered". Since it is argued that present carbon dioxide levels are higher now than for several hundred thousand years, any previous higher temperatures in that period must mean that factors other than human-emitted carbon dioxide were responsible.

CHANGING HISTORY: "WE MUST GET RID OF THE MEDIEVAL WARM PERIOD"

The second IPCC assessment report[9] in 1995 shows a change of mind over the issue of earlier warm periods, and ignores previous warmer interglacials and the well-established early-middle Holocene climatic optimum. Instead, the IPCC began to cast doubt on the Medieval Warm Period and Little Ice Age with this statement,

> *"based on the incomplete observations and paleoclimatic evidence available, it seems unlikely that global mean temperatures have increased by 1 deg C or more in a century at any time during the last 10,000 years."*

IPCC, 1995 Working Group I, also expressed increased confidence in numerical models because they then simulated more aspects of the climate than hitherto. The report attracted public attention for the subsequent complaint by one time president of the US National Academy of Sciences, Fred Seitz, who objected[10] to the editing of the scientists' input. Important statements of uncertainty concerning the modelling predictions in its Chapter 8, which had been agreed upon by the scientists, had been removed from the published version. The coordinating lead author for Chapter 8, Benjamin Santer, responded[11] justifying this change on the basis that it made the assessment clearer, thus ignoring clause 10 of the IPCC's governing principles which

[7]Hubert H. Lamb: 'Climate History and the Modern World'(1995) Routlidge, London and New York.

[8]Scientific papers normally use terms such as "dangerous climate change" but in his personal web page James Hansen has many references to "tipping points". See http://www.columbia.edu/~jeh1

[9]IPCC, 1996: '*Climate Change 1995: The Science of Climate Change. Contribution of Working Group I to the Second Assessment Report of the Intergovernmental Panel on Climate Change*' [Houghton *et al*. (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 572 pp

[10]Frederick Seitz: 'A Major Deception on Global Warming' *Wall Street Journal*, New York, June 12, 1996

[11]Benjamin Santer: 'No Deception in Global Warming Report'. Letters to the Editor. *Wall Street Journal*; New York; Jun 25, 1996.

requires the inclusion of such uncertainties. Edwards and Schneider[12] said that the removal of expressions of doubt were demanded by the politics of the day and were thereby justified. If true, the IPCC process had not delivered the policy neutral report that the governing principles required.

Meanwhile research continued into climate change and particularly into historic temperature reconstructions. Though some studies, like that of Keigwin[13], reinforced the earlier view of substantially warmer and cooler periods than the present, the main thrust appeared to be aimed at validating the IPCC suggestion that recent warming was exceptional. David Deming has told a US Senate hearing[14] that, some time after the publication of his 1995 *Science* paper on Borehole temperatures, he was approached by the media and other climate scientists interested in any anthropogenic warming implications. He claims to have been contacted by one climate scientist who expressed the view that "*we must get rid of the Medieval Warm Period.* "

THE "HOCKEY STICK"

In the IPCC Third Assessment Report[15] of 2001, the suggestion from its previous report that the 20th century might be exceptional was dramatically confirmed by the arrival of the "hockey stick" graph, which featured widely in the report, and became almost the trademark of the IPCC. The width and density of the tree rings that underpin the hockey stick reconstruction respond to many other things than temperature, but it was nonetheless asserted that with careful analysis the tree ring data could be used as a reliable indicator or proxy for ancient temperature. Some researchers alleged that tree ring proxies showed that the Medieval Warm Period and Little Ice age were not global in extent. Using complex statistical manipulations, these researchers concluded that for almost 900 of the last 1000 years the Northern Hemisphere had gently cooled until just before the 20th century. Then, apart from the mid 20th century cooling scare, the Earth had steadily warmed in lock step with greenhouse gas emissions. WGI Chapter 2 selected two particular studies, MBH98[16] and MBH99[17] of Michael Mann, Raymond Bradley and Malcolm Hughes, together with two other papers. Michael Mann who had only received his PhD in 1998, was a lead author of Chapter 2 and Bradley and Hughes were contributing authors. Blessing the work of these authors, the 2001 Summary for

[12]Edwards Paul N. and Stephen H. Schneider: 'The 1995 IPCC Report: Broad Consensus or "Scientific Cleansing"?' *Ecofable/Ecoscience* 1:1 (1997), pp. 3 9. Available at: http://www.si.umich.edu/~pne/PDF/ecofables.pdf

[13]Keigwin, L. D. (1996), 'The Little Ice Age and Medieval Warm Period in the Sargasso Sea', *Science*, 274: 1504 1508

[14]US Senate Committee on Environment and Public Works 12/06/2006 Statement of David Deming, http://epw.senate.gov/hearing statements.cfm?id 266543

[15]IPCC, 2001: *Climate Change 2001: The Scientific Basis. Contribution of Working Group I to the Third Assessment Report of the Intergovernmental Panel on Climate Change* [Houghton, J.T., et al. (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 881 pp.

[16]Mann, M.E., R.S. Bradley and M.K. Hughes, 1998: Global scale temperature patterns and climate forcing over the past six centuries. *Nature*, 392, 779 787.

[17]Mann, M.E., R.S. Bradley, and M.K. Hughes, 1999: Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations. *Geophys. Res. Lett.*, 26, 759 762

Policymakers (SPM) said on its first narrative page:

> *"New analyses of proxy data for the Northern Hemisphere indicate that the increase in temperature in the 20th century is likely to have been the largest of any century during the past 1,000 years. It is also likely that, in the Northern Hemisphere, the 1990s was the warmest decade and 1998 the warmest year"*

Using Mann *et al*.'s comparatively new and untested paleoclimatic science, the IPCC satisfied themselves that the exceptionally hot El Niño year of 1998 was likely the warmest for a thousand years. In MBH98 some 212 proxy series covering various periods back to 1400 AD were processed to provide estimated temperature data over the period up to 1980. After 1902 Mann *et al*. "spliced" or overlaid a temperature series that had been calculated from instrumental data. MBH99 extended the reconstruction to 1000 AD and suggested that from 1000 AD to 1900 temperatures had fallen gently by about two tenths of a degree C and from century to century had not varied by much more than about three tenths. This was less than a quarter of the natural variation previously believed, and allowed the computer model attribution studies to allocate a much higher sensitivity to greenhouse gases and hence project more alarming future warming.

On the second narrative page of the SPM the famous "hockey stick" graph appeared. The long flat shaft was mostly reconstructed from tree ring proxy data and the almost upright blade was instrumental temperature. The fact that it was a composite of two very different data sets spliced together, which is statistically unsound, was seldom pointed out, let alone made clear. Instead, the graph was relentlessly used to promote the IPCC's alarmist conclusions regarding a dangerous human influence on global warming. Sir John Houghton was photographed with the "hockey stick" graph as a background at his 2001 Cambridge Lecture[18]. In his lecture, the "hockey stick" graph from the SPM was used to reinforce the assertion that current warming is largely anthropogenic in cause.

THE INVALIDATION OF THE "HOCKEY STICK"

It has turned out subsequently that the Mann *et al*. studies were flawed from the start. However, few persons realized how heavily they would be promoted and how fiercely attempts to invalidate them would be resisted. After the publication of the Third Assessment Report, the "hockey stick" appeared everywhere together with apocalyptic pronouncements of what would happen if we fail to cut greenhouse gas emissions. Graphic designers competed over who could design the most alarmist rendering of this now infamous graphic, with fire-engine red a favourite bordering colour. This was a critical time for the Kyoto protocol. The USA had declined to ratify it and the spotlight fell on Russia who initially seemed unlikely to do so.

In October 2003, *Energy and Environment* published a paper (MM03)[19] by Stephen

[18]http://www.st edmunds.cam.ac.uk/cis/houghton/

[19]McIntyre, S., and R. McKitrick, 2003: Corrections to the Mann *et al*. (1998) proxy database and northern hemispheric average temperature series. *Energy and Environment*, **14**, 751 771.

McIntyre and Ross McKitrick, which showed MBH98 was a sloppy, poorly documented paper riddled with simple mistakes, unjustified assumptions, collation errors and incorrect methodology. Data, for instance reported to be from near Boston, Massachusetts actually came from Paris. Central England Temperature data was truncated eliminating its coldest period. Principle component analysis (PCA) had been done incorrectly. Drs Mann, Bradley and Hughes published a terse reply on the Internet rejecting out of hand the criticisms of MM03 and not admitting to a single error.

McIntyre and McKitrick were not criticising the science as such, but the methodology Mann *et al*. had used and their failure to make full disclosure. Many scientists from other fields, mathematicians and engineers were easily able to grasp the arguments that the IPCC's claims based on the "hockey stick" are without merit. The "hockey stick" came under great scrutiny on the Internet where both sides in the dispute set up 'blogs', which continue to attract large followings. After pressure from McIntyre and McKitrick, a corrigendum was published in *Nature*[20] in June 2004 by the MBH98 authors, who acknowledged the many non-technical errors but insisted, wrongly as is now clear, that their results were unaffected. Further details and data that had not previously been available were also made available at this time.

In 2005, McIntyre and McKitrick wrote further papers that demonstrated three major faults in the hockey stick analysis:

Inappropriate Bristlecone/Foxtail "strip-bark" proxies were used.
Incorrect PCA analysis was used
Verification statistics for the critical 15th century step were insignificant

IPCC REVIEW VERSUS THE US HOUSE OF REPRESENTATIVES

The dispute over the hockey stick related so far took place both publicly on the Internet, and less so within the IPCC - participants in which were already working on AR4. The debate was now sharpened and deepened by the decision of the US Congress to conduct an investigation and commission further studies into the matter. This provides an opportunity to assess just how "*comprehensive, objective, open and transparent*" the IPCC process was, in comparison with the congressional hearings that transpired.

The self-styled "hockey team" of supporters of Michael Mann were determined to face down what they correctly anticipated would be a spirited attack within both Congress and the IPCC on tree ring reconstructions. They therefore made determined efforts to get into press new papers that supported the "hockey stick" climate history concept. Scott Rutherford, a close associate of Michael Mann, was the lead author of one paper[21] that aimed to undermine the work of McIntyre and McKitrick. Its authors

[20]*Nature* Vol 430 page 105

[21]Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D.,2005: Proxy based Northern Hemisphere surface temperature reconstructions: Sensitivity to method, predictor network, target season, and target domain. *Journal of Climate*, **18**(13), 2308 2329.

are key members of the "hockey team". UCAR put out a press release[22] in May 2005 saying that their employee, Caspar Ammann, and Eugene Wahl had produced two new research papers submitted for review to the journals Geophysical Research Letters and Climatic Change, which conclude "that the highly publicized criticisms of the MBH graph are unfounded." Caspar Ammann was a former student of Michael Mann and also an AR4 contributing author. These **unpublished** and, at the time**, unrefereed** manuscripts were cited to discredit the work of McIntyre and McKitrick by the European Geosciences Union in a position statement dated 4 July 2005, to the US House of Representatives Committee on Energy and Commerce, and by Sir John Houghton in testimony to the US Senate Energy and Natural Resources Committee on July 21 2005. One, still not published in any print edition, is cited in IPCC, 2007.

When Michael Mann publicly refused[23] to "be intimidated" into releasing his computer code, the US House of Representatives Committee on Energy and Commerce through its subcommittee on Oversight and Investigations (the Whitfield Subcommittee) decided to hold hearings and investigate the "hockey stick". Chairman Barton requested the information being withheld. Those supportive of the "hockey stick" and the IPCC consensus view of climate change were outraged. This included the US House of Representatives Committee on Science who reacted by asking Ralph Cicerone, President of the US National Academy of Sciences, to empanel a balanced group of scientists to provide Congress with expert guidance on the current scientific consensus on the paleoclimatic record and the "hockey stick". Specifically asked by the House Committee on Science was "Has the information needed to replicate their work been available?"

THE NRC 2006 REPORT

The National Academy of Science instructed the US National Research Council (NRC) to undertake a study and report on the hockey stick "science". Providing balance, as requested by the Committee on Science, would prove very difficult with so many of the US scientists supporting the IPCC consensus. Moreover Michael Mann had become a very prominent climate scientist and was well acquainted with most of the potential panellists. Several UCAR scientists were also on the panel. Bette Otto-Bliesner, who is an AR4 author, had co-authored papers with Caspar Amman and was his immediate supervisor at UCAR. Douglas Nychka, also a UCAR employee, was at the time collaborating not only with Caspar Ammann but also with Michael Mann. The Panel Chairman, Gerald North, on first seeing the "hockey stick" graph, had been reported as saying[24],

> *"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*

[22]http://www.ucar.edu/news/releases/2005/ammann.shtml. NB: The paper submitted to *GRL* was not accepted.

[23]Antionio Regalado: In Climate Debate, The 'Hockey Stick' Leads to a Face Off. *Wall Street Journal* Feb. 14, 2005

[24]Kerr, R. A. (2000), 'GLOBAL WARMING: Draft Report Affirms Human Influence', *Science*, 288: 589 590.

> *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."* Further emphasising the fact that the "hockey stick" was more convincing than the models, Gerald North was also reported to have said in 2001[25], "*There are so many adjustables in the models and there is a limited amount of observational data, so we can always bring the models into agreement with the data*."

Panel member, Kurt Cuffey, had said in the San Francisco Chronicle of 9 October 2005,

> "*Mounting evidence has forced an end to any serious scientific debate on whether humans are causing global warming. This is an event of historical significance, but one obscured from public view by the arcane technical literature and the noise generated by perpetual partisans*."

Regardless of the clear bias of the panel towards the IPCC, and the prior acceptance of the "hockey stick" by most of its members, it included some of the most experienced, respected and professional scientists in the USA. Significantly it held its hearings in public and many witnesses with differing views were heard and questioned by the panel. A full reading of the comprehensive report[26] of the NRC panel, rather than the 4-page summary and press reports, leaves no room for any doubt that the Mann *et al*. "hockey stick" studies are invalid. It is important to note that the panel made no criticism in their public sessions or in their report of the McIntyre and McKitrick papers. Readers should not allow the mild and courteous language with which the panel gave its conclusions to minimise the import of what they were saying.

During its public hearings the panel were given a presentation[27] by Hans von Storch who had been a lead author of WGI Chapter 10 of IPCC, 2001 and was a critic of the "hockey stick". Hans von Storch was particularly critical of lead authors such as Michael Mann citing their own work and believed independent scientists should carry out the review. He was critical not only of the "hockey stick" study, but of the prominence that the IPCC had given to it. On page 16 NRC, 2006 says, "*Despite the wide error bars, [the IPCC "hockey stick" graph] was misinterpreted by some as indicating the existence of one "definitive" reconstruction with small century-to-century variability prior to the mid-19th century*."

On page 50 NRC, 2006 says, " …*'strip-bark' samples should be avoided for temperature reconstructions*." In various guises 'strip-bark' Bristlecone/Foxtail samples show up in most reconstructions and are responsible for the sharp up tick in the 20th century, which correlates with instrumental temperatures from 1900 to 1960. The 'strip-bark' data Mann *et al*. used had been collected by Graybill and Idso and

[25]Kerr, R. A. (2001), 'Global Warming: Rising Global Temperature, Rising Uncertainty', *Science*, 292: 192.

[26]NRC, 2006: Committee on Surface Temperature Reconstructions for the Last 2,000 Years, (2006) National Research Council, National Academies Press

[27]http://meteo.lcd.lu/globalwarming/von Storch/reconstruction of historical temp 060302.ppt

reported in their 1993 paper[28]. These are particular examples of Bristlecone and Foxtail tree-rings which generally had a pronounced growth spurt in the first half of the 20th Century, and which on page 82 the authors say could not be shown to be related to local temperature changes. Graybill and Idso were specifically looking for evidence of CO_2 fertilisation.

The most damaging aspect of the 'strip-bark' matter is that when the data for MBH98 study was finally released, a directory on Michael Mann's ftp server was found with the highly suggestive name "BACKTO 1400-CENSORED". It contained all but 20 of the 212 series used in the published paper. Nineteen were 'strip-bark' and the twentieth was inappropriate for reasons made plain in MM(2005b)[29]. The implication is clear that Michael Mann knew these proxies were of doubtful validity, and he had tested the reconstruction without them. He presumably knew but did not report that his reconstruction was significantly affected by the removal of these proxies.

NRC, 2006 shows, on page 86 onwards, that the non-centred PCA of Mann *et al*. does indeed produce "hockey sticks" from red noise, as shown earlier by McIntyre and McKitrick. The NRC panel produced their own worked examples to demonstrate that the process "mines" for "hockey sticks".

The issue of validation statistics had been heated and was possibly the reason for the reluctance of Michael Mann to disclose his computer programme code. McIntyre and McKitrick had realised that the R^2 verification statistics for the critical 15th century stage of Mann *et al*.'s reconstruction indicated that the results were statistically meaningless. Asked about it by Congressman Barton, Michael Mann still would not say if he had calculated R^2 but instead argued the merits of other verification statistics that he had used. Michael Mann did, as requested, release his computer code, which showed that he had indeed calculated R^2, but not reported it. This is a straightforward statistical issue, as indeed is that of non centred PCA, and in any other area of science it would have caused the instant withdrawal of the study. On page 107, NRC, 2006 says, and readers should note its generality to reconstructions:

> *"**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**."*

SCIENCE OR SORCERY – THE "DIVERGENCE" PROBLEM

Researchers had for some time been looking at datable samples, or proxies, that could be shown to indicate historic temperatures. Tree rings had, by their variation in width, provided an excellent means of dating archaeological artefacts and during the 70's and 80's a large archive of samples and ring width data were amassed. Plant growth is dependent, in part, upon temperature, and in some cases a good correlation between the growth of tree-rings and local instrumental temperature records was found. However,

[28]Graybill, D.A., and S.B. Idso. 1993. Detecting the Aerial Fertilization Effect of Atmospheric CO_2 Enrichment in Tree Ring Chronologies. *Global Geochemical Cycles* 7(1): 81 95.

[29]McIntyre, S., and R. McKitrick, 2005b: The M&M critique of the MBH98 Northern Hemisphere climate index: Update and implications. *Energy and Environment*, **16**, 69 99.

in others there may be none or even negative correlation. For instance, in one study, Jacoby and D'Arrigo (1989)[30], only 10 of 36 sites sampled were judged good enough. Thus selection, or cherry picking, is at the heart of tree ring temperature reconstructions, as it is in other parts of the scientific dispute over climate change. Jan Esper said:[31]

> "*The ability to pick and choose which samples to use is an advantage unique to Dendroclimatology.*"

By finding and combining enough "good" proxy series to give coverage over time and geography it was believed that hemispheric and globally averaged historic temperature reconstructions could be produced. One problem is that in deciding which series to use a subjective judgment has to be made and, in the absence of documented criteria and processes, it can be argued by critics that choices are made with a bias towards a warmer or cooler Medieval Warm Period, depending upon one's predilection. Another problem is that, the further back in time one goes, the fewer samples there are. In the case of Gaspé series, the 20th disputed proxy series used in the "hockey stick", **just one tree** is relied upon from 1404 to 1421, and the data from this single tree were extrapolated back to 1400 to enable the series to be used in the critical and disputed 15th century part of the study.

Other factors besides temperature determine plant growth, but in the case of samples that are thought suitable for temperature reconstructions, an assumption - "the uniformity principle" is made that their response to temperature has been constant historically. At the most basic level in science it has to be assumed, for instance, that the relationship between force, mass and acceleration remains constant over the range of values being considered. But in looking at phenomena such as tree ring growth over centuries, to which many variable conditions contribute, it is far from clear that the conditions remained uniform over distant historic times. The wide variation in historic temperature reconstructions based on tree rings shown in AR4 is a significant indication that there is no precise temperature information that can be deduced from them.

In 1998 Keith Briffa with co-authors including Philip Jones published a paper[32] with the title *"Trees tell of past climates: but are they speaking less clearly today?"* The answer they found is that, in many cases, tree rings series which correlated with the temperature rise from 1900 to 1960 "diverge" thereafter, indicating falling temperatures after 1960 while instrumental temperature measurements are generally believed to have risen. Figure 1 illustrates this. Thus at the time Mann, Bradley, Hughes, Briffa and Jones were writing the critical IPCC, 2001 Third Assessment

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[30]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.

[31]Esper J, Cook ER, Krusic PJ, Peters K, Schweingruber FH (2003) Tests of the RCS method for preserving low frequency variability in long tree ring chronologies. *Tree Ring Research* 59, 81 98.

[32]Briffa, K.R., Schweingruber, F.H., Jones, P.D., Osborn, T.J., Harris, I.C., Shiyatov, S.G., Vaganov, E.A. and Grudd, H., 1998 *"Trees tell of past climates: but are they speaking less clearly today?" Philosophical Transactions Royal Society London B* 353, 65 73 (R)

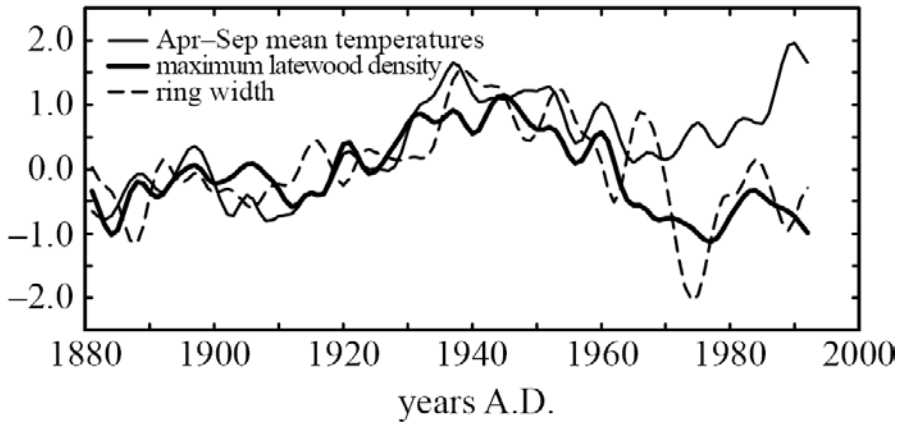

Figure 1. Illustration of the divergence problem from Briffa et al. (1998)

Report (TAR) Chapter 2, some of them certainly knew - and all should have known - that the data that they were using to assert that current warming is exceptional are inadequate to sustain that conclusion.

Figure 2.21 on page 134 of Chapter 2 of IPCC, 2001 WGI, shows a number of studies that are purported to corroborate the "hockey stick". Close examination reveals that none of the reconstructed temperature curves extend beyond 1980. **That is to say that most of the period, which the authors claim to be exceptionally warm, is not replicated in reconstructions that their claim relies upon**. Further examination shows that the IPCC truncated two of the datasets for their graphs. Briffa's reconstruction stops at 1960 even though the original study included data for later. When these post-1960 data are included, the divergence problem is immediately apparent and the reconstructions become far less convincing. Equally misleading is the fact that from 1850 to 1902 instrumental temperature, which elsewhere the IPCC treat as reliable, is also omitted from Figure 2.21. Had the IPCC figure been plotted with all the data shown, as it is in Figure 2 shown here[33], it would have been clear that we were being invited to accept the temperature estimates of a thousand years ago from these reconstructions when they **cannot even replicate current temperatures for half the known instrumental record**. Such manipulation of data and graphical presentation might charitably be viewed as "graphsmanship"; alternatively, it is fraudulent.

Plotting an instrumental record for the years for which no proxy reconstruction exists is in itself questionable as it is easily mistaken for an extrapolation of the proxies. I am unaware of any proxy study that fully simulates the instrumental record up to the end of the 20th century, whereas several studies show a clear mismatch between instrumental and proxy data.

NRC, 2006 concluded on page 111,

> *"For tree ring chronologies, the process of removing biological trends from ring-width data potentially obscures information on long-term changes in climate."*

[33] Adapted from http://www.climateaudit.org/?p 1737#more 1737

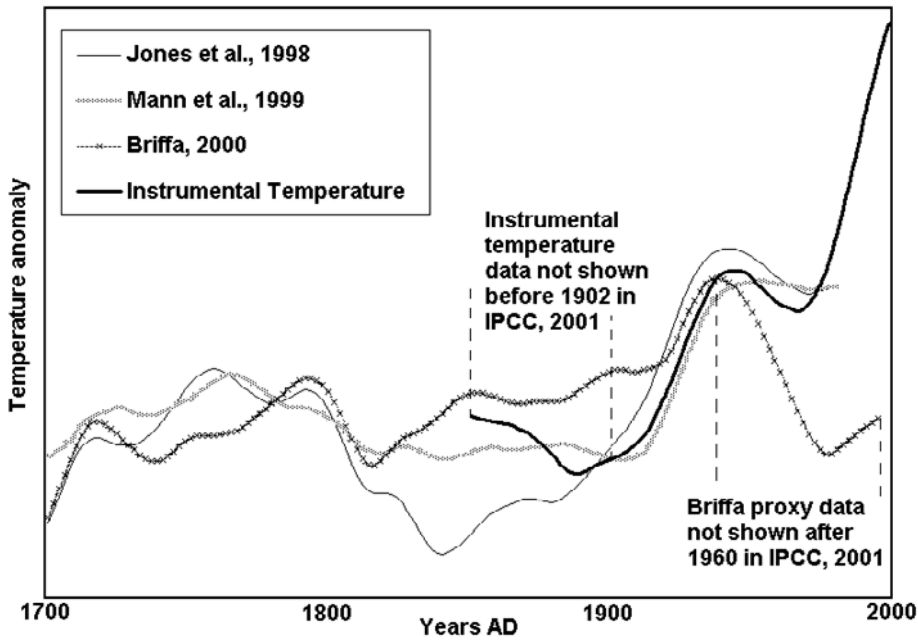

Figure 2. Last three centuries of IPCC, 2001 WGI Figure 2.21 with missing temperature and proxy data included.

This is a fundamental problem ignored by paleoclimatologists. On page 4, NRC, 2006 says critically,

> *"Furthermore, it would be helpful to update proxy records that were collected decades ago, in order to develop more reliable calibrations with the instrumental record. Improving access to data used in publications would also increase confidence in the results of large-scale surface temperature reconstructions both inside and outside the scientific community."*

WEGMAN ET AL., 2006

While the NRC panel was producing its report for the Committee on Science, the Whitfield Subcommittee had independently commissioned a study[34] from Edward Wegman who is chairman of the NAS Committee on Applied and Theoretical Statistics and a Fellow of the Royal Statistical Society. Wegman *et al*. do not show the same deference to Michael Mann as the NRC panel and their conclusion is unambiguous. They say on page 4 "*Overall, our committee believes that Mann's assessments that the decade of the 1990s was the hottest decade of the millennium and*

[34]Wegman *et al*., (2006): Ad Hoc Committee report on the "Hockey Stick" global climate reconstruction, commissioned by the US Congress House Committee on Energy and Commerce, 2006. http://republicans.energycommerce.house.gov/108/home/07142006 Wegman Report.pdf

that 1998 was the hottest year of the millennium cannot be supported by his analysis."

Wegman *et al*. found that the criticisms made by McIntyre and McKitrick of Mann *et al*. were justified 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*."

On the R^2 dispute Wegman *et al*. say on page 81,

> "*M&M also evaluated the MBH98 usage of the Reduction of Error statistic in place of the more reliable and widely used Monte Carlo Model to establish significant benchmarks. By using the Monte Carlo Model, M&M found that a more accurate significance level for the MBH98 procedures is .59, as opposed to the level of 0.0 reported in the original study. A guard against spurious RE significance is to examine other statistics, such as the* R^2 *and CE statistics. However, MBH98 did not report any additional statistics for the controversial 15th century period. The M&M calculations indicate that these values for the 15th century section of the temperature reconstruction are not significant, thereby refuting the conclusions made by MBH98*."

As well as clear analysis of the statistical issues and clear conclusions that the Mann *et al*. papers were incorrect, Wegman *et al*. looked at the "social network" of Michael Mann in which a group of authors collaborate with each other to write papers and share proxy data. On page 65 Wegman *et al* say,

> "*Based on the literature we have reviewed, there is no overarching consensus on MBH98/99. As analyzed in our social network, there is a tightly knit group of individuals who passionately believe in their thesis. However, our perception is that this group has a self-reinforcing feedback mechanism and, moreover, the work has been sufficiently politicized that they can hardly reassess their public positions without losing credibility*."

And on page 46,

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

The Wegman report was published two days before the Whitfield Subcommittee

held its first hearings and Michael Mann posted the following comment[35] on his website which is indicative of the practice of the "hockey team" in preferring to shift the argument onto peripheral issues rather than address the science.

> "*The un-peer reviewed report commissioned by Rep. Barton released today adds nothing new to the scientific discourse on climate change and is a poor attempt to further personalize and politicize what should be a matter of scientific debate not politics*".

A transcript of the hearing is available[36]. Michael Mann through his lawyer declined to attend the first day of the hearings at which it was explained that Wegman *et al*., had been peer reviewed in a similar way to NRC, 2006. It would appear that only peer review by the "hockey team" is acceptable to them. Several supporters of the "hockey stick" were witnesses at the hearings and were supported by sympathetic Members of the House, but apart from attacking its peer review no substantive dispute was made with the substance of Wegman *et al*.. Ralph Cicerone was asked if he considered Edward Wegman a credible person on the statistical issues his report addressed. Ralph Cicerone confirmed that he was[37]. Asked if he "*disputed the conclusions or the methodology of Dr. Wegman's report"*, Gerald North said he did not and added, "*In fact, pretty much the same thing is said in our report*."[38]

In summary two exhaustive and independent peer reviewed studies by professionals, reporting under oath to the US House of Representatives, had not only upheld the criticisms by McIntyre and McKitrick and identified several separate flaws that invalidated the Mann *et al*. "hockey study" but had given strong reasons why the field of paleoclimate should be treated with great caution. Anyone thinking that this might be reflected in the IPCC AR4 was to be disappointed.

IPCC, 2007 WGI

The two reports commissioned by the US House of Representatives were in the public domain in July 2006, almost six months before the IPCC began the release of AR4. It is inconceivable that the WGI authors were unaware of the conclusions of the congressional panels. For example, NRC panel member, Bette Otto-Bliesner, was a lead author of WGI Chapter 9 and a contributing author of Chapter 6. Of course, the IPCC must have cut off dates for material it considers or it would never complete its deliberations. On the other hand, it is absurd to ignore studies published well before the assessment's publication date that have an important bearing upon the matters in hand. In AR4 there is no mention whatever of Wegman *et al*. and only one citation of NRC, 2006 to which readers are referred for more information on the "divergence" problem. This is acknowledgment that the NRC report was considered, and it is scandalous that the WGI Chapter 6 authors ignored most of its substantive findings.

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[35]http://www.realclimate.org/?comments popup 324

[36]http://frwebgate.access.gpo.gov/cgi bin/getdoc.cgi?dbname 109 house hearings&docid f:31362.pdf

[37]Ibid, page 735

[38]Ibid, page 85

Despite the clear conclusions on page 110 of NRC, 2006 that,

> *"Largescale temperature reconstructions should always be viewed as having a "murky" early period and a later period of relative clarity. The boundary between murkiness and clarity is not precise but is nominally around A.D. 1600."*

And despite the clear analysis in Wegman *et al*. showing the lack of independence between the various temperature reconstructions, the authors of AR4 WGI Chapter 6 persisted with their reliance on a "spaghetti" diagram of reconstructions in Figure 6.10(b) to continue to justify the claim that "*Average Northern Hemisphere temperatures during the second half of the 20th century were likely the highest in at least the past 1,300 years*." As in the case of the previous report, the post-1960 data from Briffa (2000) are omitted. That of the new Rutherford, Mann *et al*. (2005) study, which largely rehashes MBH99, also terminates well before 1980 because after that divergence occurs. Again it can be seen that few of the proxies replicate instrumental temperatures for better than the period of relatively linear increase from 1900 to 1950. And again, despite the billions of dollars being spent on climate research every year, no proxy studies are shown by the IPCC that include the last 27 years, which they assert are exceptional.

THE IPCC REVIEW PROCESS AND DISCLOSURE.

The IPCC working group drafting and review process should, according to its governing principles, proceed on "*a comprehensive, objective, open and transparent basis*", but it does not. The review process, the pre-publication drafts of the report and the reviewers' comments are covered by confidentiality agreements, which in itself shows a lack of willingness to be open and transparent. The IPCC threatened legal action against one Australian web site for publishing the second order drafts of the WGI report when the SPM was first published.

Review comments for AR4 were submitted digitally in spreadsheet format and, presumably, held electronically by the IPCC for the chapter authors to see and respond to. The governing principles provide for reviewers to see all comments on request. When one reviewer[39] asked to see the reviewers' comments on the first order draft in 2005 he was sent a hard copy by post. If this were the case for all reviewers it would be very expensive and time consuming for the IPCC and not as useful as it could be for the reviewer. It suggests that reviewers do not generally ask to see the comments and do not follow the review process closely. The reviewer's request for the comments on the second order draft were ignored until after the end of the review process, whereupon the IPCC referred the reviewer to the curator of the Littauer Library at Harvard. On enquiring, the reviewer was invited to attend the library, with at least a week's advance notice, to see the comments. Freedom of Information Requests eventually obliged UCAR to release the drafts and reviewers' comments[40]. These were in electronic form, easy to search and analyse and reveal that many matters of

[39] See http://www.climateaudit.org/?p 1589

[40] The Review Comments are available at: http://ipcc wg1.ucar.edu/wg1/Comments/

climate science were professionally disputed by competent reviewers but were not subsequently mentioned in the text of the report - as is required by the governing principles of the IPCC.

The review process as operated by the IPCC is fundamentally unsatisfactory. Without knowing what other reviewers have said, and how the authors respond to the reviewers' comments, the ability to influence a team of determined authors is limited. Moreover, reviewers appear only to see the revised text when the next draft is issued. This means that the expert reviewers only have two opportunities to influence the report, and the result can be that changes make it even more unsatisfactory to the reviewers! An important example is described by Stephen McIntyre[41] relating to Table 3.2 in the published WGI Chapter 3; this table shows linear trends in land and ocean temperatures with estimates of statistical significance which were challenged with peer reviewed citations in both first and second order drafts - the second being by the Government of the United States. The final text has an added sentence saying, "The Durbin Watson D-statistic (not shown) for the residuals, after allowing for first-order serial correlation, never indicates significant positive serial correlation." No statistical literature was cited to justify what amounts to a new statistical test, and had it appeared in the earlier draft it would have been vigorously challenged.

The sections below that discuss the "hockey stick" are, inevitably, longer than the writer would prefer, but still represent only a fraction of the reviewers' critical comments, and relate to only a few of the many contentious issues. They show that in the controversy over the "hockey stick" the authors of Chapter 6 were determined to defend it and to ignore its peer reviewed invalidation. They also appeared to be determined, contrary to governing principles, that the final text should not give a clear account of the "*differing views on matters*."

THE IPCC WGI FIGURE 6.10(B) SPAGHETTI DISPUTE

The IPCC Figure 6.10(b) was vigorously protested by reviewers but to no avail. The Chapter 6 Second Order Draft Reviewers' Comments include:

> **6-700** Fig 6.10. I here repeat a point made in my comments on the FOD [first order draft]. It is statistically invalid and visually misleading to overlay the black instrumental line on this diagram. The coloured graph lines show proxy records that end at 1980. If you want a line that continues up to more recent years that then you must use the proxy records that continue past 1980, not switch to a different type of series. There are up to date proxy records available, but as I'm sure the authors of this chapter are aware, they depart from the surface instrumental record, many of them declining after 1980. By failing to show this, and including the surface temperature data in black, it constitutes a misrepresentation, since the black line is an invalid forward extrapolation of the proxy data. If the reason for not showing the updated proxies is that they are not considered to be good representatives of temperature anymore, then by what right does the Figure insinuate that they were good proxies 8-10 centuries ago? It is no defense to claim

[41]For a further important statistical example see http://www.climateaudit.org/?p 1805

that MBH99 established a statistically skillful relationship between the proxy network and the instrumental data, since that claim has been refuted, as discussed above. McIntyre and McKitrick (2005a,d) showed that the pre-1450 RE statistic was incorrectly benchmarked, yielding a spurious inference, and the r2 stat calculated by MB&H themselves, which showed the lack of skill, was simply not reported. The failure of the r2 and CE stats is confirmed by Wahl and Ammann. The squared correlation between the MBH long proxies and the instrumental record is nearly zero (MM05a,c). The mean correlation between the long NOAMER proxies and gridcell temperatures in the MBH98 data set (which dominate the pre-AD1450 portion) is -0.08 (McIntyre and McKitrick 2005c), and the RE significance benchmark is above the MBH98 RE score, using all available implementation of the Mann code (McIntyre and McKitrick 2005d). The surface instrumental record cannot be used as a statistically valid extrapolation for the proxies after 1980.
[Ross McKitrick (Reviewer's comment ID #: 174-35)]
Response "See responses to the specific points below. Plotting the instrumental data is appropriate here, and the caption and lines make it clear that the instrumental data are not the same as an extrapolation of proxy data."

The reviewer gives a cogent, indeed incontrovertible, reason why the instrumental data should not be plotted but it is simply ignored. Some of the specific points and responses are shown below. The chapter authors reject the points raised without any counter argument. No valid reason is given for concealing the divergence problem. The R^2 issue, which is a fundamental statistical issue upon which there can be no room for doubt or discussion, is simply ignored.

6-112 Show the Briffa et al reconstruction through to its end; don't stop in 1960. Then comment and deal with the "divergence problem" if you need to. Don't cover up the divergence by truncating this graphic. This was done in IPCC TAR; this was misleading and d [sic]
[Stephen McIntyre (Reviewer's comment ID #: 309-18)]
Response Rejected though note 'divergence' issue will be discussed, still considered inappropriate to show recent section of Briffa et al. series

6-113 I don't think that you should show the Rutherford et al 2005 reconstruction. First there is no "Rutherford et al 2005" reconstruction highlighted in their paper, but a variety of alternatives. The networks are duplicates of MBH98 and Briffa et al 2001, so [sic]
[Stephen McIntyre (Reviewer's comment ID #: 309-19)]
Response Rejected the purpose of showing this is to allow comparison with previous reconstructions but using a different spatial field reconstruction technique

6-114 If you do show Rutherford et al, you must show their values after 1960, as with Briffa et al 1960. Not to do so gives a very misleading impression.
[Stephen McIntyre (Reviewer's comment ID #: 309-20)]

Response Rejected Rutherford et al. did not use the tree-ring density data after 1960 so there are no data to show

6-709 This remark concerns the handling of the Mann- "hockey stick". Traditionally we have had a conflict between paleo climatologists and climatologists work with the present climate. Paleo archives consist of proxy data with different time resolution and different coupling to climate parameters. When Mann et al. presented their hockey stick 6-7 years ago they formatted paleodata in such a way that climate modellers could use it. But very few paleo climatologists agreed to the shape of the curve and now a days we have much better data to use. It is therefore natural to describe the Mann curve in a history of science perspective, but not as a valid data set. A good example of a good modern curve is the one presented by Moberg et al in Nature 2005. It can certainly be improved in the future, but it has at least the variation seen in almost all paleo climate records for the past millennia. In the present IPCC-text the view described is that we have the hockey stick and then later some scientists have raised critical voices. The basic meaning is that the hockey stick is still the number one description of the past millennia. This is not flattering and it certainly mis-credit the report. I believe that it is rather easy to go through the 5 pages and update the spirit of the text and perhaps make some adjustments in the figure captions.
[Per Holmund (Reviewer's comment ID #: 108-5)]
Response Rejected the Mann et al. curve is included for consistency and to maintain a historical context for the current state of the art. Also, the low frequency character of the Moberg et al. series is subject to very large uncertainty though it is also included to provide a comprehensive representation of the range of published results. The current text does not give uncritical support to the Mann et al (1999) curve it shows other reconstructions and discusses possible reasons (as far is currently possible) for the differences. Conclusions are then drawn on the bases of all the current data.

These breathtaking examples show that the IPCC panel is unable to grasp the obvious nonsense of a claim that historic reconstructions are evidence that one period is warmer than another when the reconstructions cited cannot replicate the instrumental record of the actual decades which they are alleging to be warmer. The evidence would be speculative if the divergence problem was unknown, but knowing it makes the evidential value of the reconstructions nil. The NRC said reconstructions prior to 1600 were "murky" but looking at AR4 WGI 6.10(b) we can see that this was a generous comment. In actuality, there is little consistency between the individual reconstructions, and before 1900 or after 1950 between any of them and instrumental data.

THE AR4 DEFENCE OF THE "HOCKEY STICK"

In any other scientific field the problems just discussed would have made any dispute over the statistical validity of a single study on the matter seem utterly irrelevant. However in the necessarily long section that follows the bias and arrogance of those

responsible for the published text is demonstrated by looking at the arguments over the short paragraph on page 466 of AR4 WGI Chapter 6 and the earlier drafts of it, together with just 10 of the 70 comments and responses. The IPCC paragraph in question is their defence of the "hockey stick" against the criticisms of McIntyre and McKitrick. In the first order draft, it is as follows:

> "McIntyre and McKitrick (2003), produced a Northern Hemisphere reconstruction that differs radically from that of Mann et al. (1999), in indicating a period of significant warmth in the 15th century, even though they attempted to employ the same method and candidate proxy climate predictors. However, they omitted several important proxy series used in the original reconstruction, and arrived at a regression model which did not 'verify', in the sense that it produced temperature estimates that did not agree with independent temperature observations sufficiently well to demonstrate any likely validity in their final reconstruction. The Mann et al. (1999) series was subsequently successfully reproduced by Wahl and Ammann"

The paragraph, as the authors must have known, is a disgraceful distortion of the McIntyre and McKitrick studies. Some of the more pertinent comments on the first draft are:

> **6-1316** McIntyre and McKitrick [2004] did NOT produce a NH reconstruction; they explicitly state that they do not endorse the proxies in MBH98. They showed the results using updated versions of MBH98 proxies and principal components calculated over the maximum period in which all proxies were available. [Stephen McIntyre]
> **Response** Noted see edited text
>
> **6-1317** Wahl and Ammann [2004] is not published yet. It does not reproduce MBH98 claims of statistical skill. [Stephen McIntyre]
> **Response** Noted see edited text
>
> **6-1318** McIntyre and McKitrick [2005a, 2005b, 2005c] showed that the MBH98 principal components methodology was biased towards selection of hockey stick shaped series; that the MBH98 reconstruction was not robust to the presence/absence of disputed Bristlecone pine series; failed R2 and other cross-validation tests; and that the seemingly significant RE statistic was spurious. In particular, they showed that the IPCC TAR claim that the MBH98 passed cross-validation statistical skill tests was false. [Stephen McIntyre]
> **Response** Noted see edited text
>
> **6-1319** The authors seem pretty uninformed about my work with Stephen McIntyre. For instance there is no mention of our 2005 GRL or E&E papers, even though these contain the bulk of our arguments; and indeed the paragraph shows that the chapter authors are unaware of what our arguments actually are. The paragraph trots out the straw man that we are selling an alternative climate history,

despite our repeated and persistent statements that we are not trying to offer "our" climate history curve. From the outset we have been trying to show what Mann's curve would look like if he had done what he said he had done, using the data he said he used. Lest any reader of this comment think it pejorative for me to suggest that the MBH98/99 data and methods were inaccurately or incompletely disclosed, the Corrigendum ordered by Nature and published July 1 2004 by Mann et al. should settle that. We filed a Materials Complaint with Nature in January 2004, Nature asked Mann to respond, and based on their review of his response Nature ordered a complete restatement of the data and methods of MBH98. The methodology described in the new MBH98 SI differs fundamentally from that presented in MBH98 itself, notably in its use of a highly irregular PC methodology and the splicing of proxy PCs in hitherto undisclosed segments. [Ross McKitrick]
Response Noted see edited text

6-1320 The last sentence is false. Mann's results have never been reproduced. Ammann and Wahl reproduced the reconstruction PCs of Stephen McIntyre to 9 decimal places (no great feat since his code was available on the internet) but got no closer to Mann's final results than McIntyre had, except for their introducing a rescaling step not disclosed in MBH98 but apparently used by Mann. Once added to McIntyre's code the Wahl-Ammann and Mcintyre reconstructions are identical but neither one agrees with Mann's. No one has ever reproduced Mann's results. I know of 3 teams that have tried: McIntyre-McKitrick, Ammann-Wahl and Cubasch, and all failed, but McIntyre and Ammann-Wahl published reasonably close approximations. [Ross McKitrick]
Response Noted see edited text

6-1322 Additionally this paragraph misses the whole issue of the bristlecone pines. The comment about how we "omitted several important proxy series" sounds like you got your material off the realclimate web site rather than from following the debate in the literature. We showed in our E&E2005 paper that the difference between high and low 15th century values is fully explained by the inclusion or exclusion of the Graybill-Idso Bristlecone pine series. Since in a proper PC analysis these only appear in PC4 and account for less than 8% of the explained variance of the NOAMER network, as opposed to appearing in PC1 and accounting for 37% in the erroneous Mann PC method, they cannot be considered a dominant climatic pattern. Moreover there is comprehensive evidence (surveyed in our E&E2005 paper) showing that their 20th century growth spurt is not a climatic signal, so they are not proper climate proxies. Yet their usage in the MBH data set swamps the rest of the data set and eliminates the high 15th century values that would otherwise result from the application of the MBH method on the rest of the data. Mann has never rebutted the dependence of his results on the bristlecone pine series, and is hardly in a position to do so since he did an unreported sensitivity analysis and discovered it for himself, but did not report it. So it is not that we "omit" some important proxies and end up with a lousy result, instead we

remove some lousy proxies and end up with an important result: the conclusions fall to pieces. The issue, as we have said over and over, is robustness. Mann's conclusions are not robust. They are not statistically robust, nor are they robust to removal of a small network of bristlecone proxies that are widely viewed among dendrochronologists (including Hughes himself in another paper) to be invalid as temperature proxies. What we have shown is not that the 15th century was "warm", but that Mann's results do not provide evidence that the late 20th century was climatologically exceptional.
[Ross McKitrick]
Response Noted see edited text

6-1330 More information on what was omitted (north american bristlecone pine?) - and why it should not be - would be helpful here. [**Susan Solomon**]
Response Text will be modified

Readers may think that "Noted see edited text" indicates that the chapter's authors intended to put right the obviously incorrect characterization of the "hockey stick" dispute and that the reasonable suggestion of Susan Solomon, the Co- Chairperson of WGI, would, as the response says, find its way into the text of second order draft which is as follows,

"McIntyre and McKitrick (2003) reported that they were unable to replicate the results of Mann et al. (1998). Wahl and Ammann (accepted) demonstrated that this was due to the omission by McIntyre and McKitrick of several proxy series used by Mann et al. (1998). Wahl and Ammann (accepted) were able to reproduce the original reconstruction closely when all records were included. McIntyre and McKitrick (2005) raised further concerns about the details of the Mann et al. (1998) method, principally relating to the independent verification of the reconstruction against 19th century instrumental temperature data and to the extraction of the dominant modes of variability present in a network of western North American tree-ring chronologies, using Principal Components Analysis. The latter may have some foundation, but it is unclear whether it has a marked impact upon the final reconstruction (Von Storch et al., 2004; Huybers, 2005; McIntyre and McKitrick, 2005). However, subsequent work using different methods to those of Mann et al. (1998, 1999), also provides evidence of rapid 20th century warming compared to reconstructed temperatures in the preceding millennium."

The second order draft, above, entirely ignores the comments, even that of Susan Solomon, and makes worse the misrepresentation by wrongly suggesting that, despite the litany of errors, the "hockey stick" is unaffected and that other studies support its conclusions. This version was also vigorously protested with little effect. Some of the more pertinent reviewers' comments and responses are,

6-1157 You say that Wahl and Ammann were able to "reproduce the original reconstruction" implying that they reproduced the "results". This is completely

false. They categorically failed to "reproduce" the MBH claims of statistical skill and MBH claims of robustness to presence/absence of dendro indicators. Their reproduction of a hockey-stick shape used a method almost identical to what we had previously used in our emulations, where e [sic] had been emulate [sic] the hockey stick shape but only with the flawed PC method OR using a lot of PC series - which enabled the bristlecones to imprint the result. [Stephen McIntyre (Reviewer's comment ID #: 309-53)]
Response The reviewers opinion is noted and in part accepted the text in this paragraph is intended to convey a brief and basic assessment of the current balance of evidence regarding the features and likely reliability of the original 'hockey stick'. It is not intended to provide a detailed elucidation of the criticisms or responses, but rather to provide an indication that aspects of the Mann et al (1999) methodology have been challenged and these challenges addressed. This list of references has been extended to include McIntyre and McKitrick 2005b and other minor wording changes made in response to other comments. The reader is also referred to the responses to comments 6-732, 6-734, 6-736, 6-1154 and to the comment 6-740 made by another reviewer.

6-1158 Wahl and Ammann 2006 did not meet several publication deadlines. Is it fair to use this study when other studies also not meeting publication deadlines were not used? It was not accepted by December 13-15. TSU did not have a preprint by late February. The version available for review was not the same as the accepted verion - in particular, the version made available omitted critical information that MBH98 failed cross-validation r2 and CE statistics.
[Stephen McIntyre (Reviewer's comment ID #: 309-119)]
Response Rejected- the citation is allowed under current rules.

6-750 The use of Wahl and Ammann (accepted) does not comply with WG1's deadlines and all text based on this reference should be deleted. WG1's rules require that all references be "published or in print" by December 16, 2005. Wahl and Ammann was "provisionally accepted" on that date, and not fully accepted until February 28, 2006, at which time no final preprint was available. Substantial changes were made in the paper between December 16, 2005 and February 28, 2006, including insertion of tables showing that the MBH98 reconstruction failed verification with r-squared statsistics, as had been reported by McIntyre and McKitrick in 2003. These tables were not available in the draft considered by WG1 when developing the second-order draft.
[**Govt. of United States of America** (Reviewer's comment ID #: 2023-415)]
Response See response to comment 6-1158.

The final published text on page 466 is,

"McIntyre and McKitrick (2003) reported that they were unable to replicate the results of Mann et al. (1998). Wahl and Ammann (2007) showed that this was a consequence of differences in the way McIntyre and McKitrick (2003) had

> implemented the method of Mann et al. (1998) and that the original reconstruction could be closely duplicated using the original proxy data. McIntyre and McKitrick (2005a,b) raised further concerns about the details of the Mann et al. (1998) method, principally relating to the independent verification of the reconstruction against 19th-century instrumental temperature data and to the extraction of the dominant modes of variability present in a network of western North American tree ring chronologies, using Principal Components Analysis. The latter may have some theoretical foundation, but Wahl and Amman[sic] (2006) also show that the impact on the amplitude of the final reconstruction is very small (~0.05°C; for further discussion of these issues see also Huybers, 2005; McIntyre and McKitrick, 2005c,d; von Storch and Zorita, 2005)."

Thus despite the detailed protests at the second order draft the published AR4 still suggests, quite falsely, that the **still unpublished** paper of Wahl and Ammann could reproduce Mann *et al*. (1998) whereas McIntyre and McKitrick (2003) reported they could not. Despite retaining the unpublished paper contrary to the clearly stated objection of the US Government's reviewer, the AR4 WGI Chapter 6 authors refused to mention that Wahl and Ammann, in the "in press" version of the paper, include a table of verification statistics, including R^2 for the disputed 15th century portion that corroborates that of McIntyre and McKitrick, and which shows the reconstruction to have no statistical merit. Far from adding the clarification, asked for by Susan Solomon at the first draft stage, on the disputed proxies, reference to this issue is replaced by referring to "differences in the way MM2003 had implemented the method of Mann et al. (1998)". For good measure the word "theoretical" is added to further blunt the criticism of the incorrect PCA method used by Mann *et al*. (1998)

In their apparent desperation to discredit McIntyre and McKitrick the final editors of AR4 WGI Chapter 6 seemed unable to admit in the published text that the Wahl and Ammann paper they use is still unpublished. In one line they cite its publication date as 2007 and a few lines later as 2006 but miss the second "n" from Ammann. The paper is described in the Chapter's references as "in press" but, by October 2007, had still not been published or listed in the contents of any scheduled print edition.

In concluding this section the question of how the responses to the comments previously illustrated were arrived at must be addressed. We are repeatedly told that the IPCC conclusions are the "consensus of thousands of scientists". There is no evidence, as yet disclosed, to say how the omission of inconvenient data from the spaghetti diagram was agreed or the "hockey stick" paragraph was decided against the many protesting comments and the well documented and peer reviewed studies initiated by the US House of Representatives. Just 16 "lead authors" are listed for Chapter 6 and it is unlikely that all were involved in these issues and also unlikely that any were not fully aware of the public hearings into the issue. Almost certainly within WGI there will be further annotated documents that show how and by whom these crucial matters were decided. In the interest of transparency all the documents should be published.

THE IPCC WGI SUPPORTING THE LACK OF DISCLOSURE

Numerous unpublished papers were included in AR4 though the Wahl and Ammann paper is the only one of any significance that remains unpublished and in fairness it should be said that its data and methodology were available. Stephen McIntyre describes[42] his experiences as an appointed IPCC reviewer in endeavoring to review two particular papers, Hegerl *et al*. (2006) and D'Arrigo *et al*. (2006) which were unpublished at the time. Given that one thrust of the paleoclimatic chapter of AR4 was a defense of the "hockey stick", against the criticisms of McIntyre and McKitrick, natural justice should require that if critics are invited to review papers they must have access to the data and methodology or the papers should not be cited. All efforts to examine the data were frustrated and Susan Solomon wrote to Stephen McIntyre forbidding him from further seeking to see the data, concluding her letter,

> *"Finally, we must insist that from now on you honour all conditions of access to unpublished, and therefore confidential, material made available for the IPCC review process. The IPCC rules for reviewing draft reports have served the scientific and policy communities well for numerous past international assessment rounds. If there is further evidence that you can not accept them, or if your intent is to use your access to the review process to challenge them, then we will not be able to continue to treat you as an expert reviewer for the IPCC."*

Susan Solomon's letter reveals a gross abuse of process, ignoring as it does her duty under Clause. 4.2.4.1 of Appendix A to the IPCC governing principles. Supporters of the "consensus" can bring into the IPCC process unpublished papers that are "accepted" by one journal or another and have passed its peer review process regardless of whether the data and methodology are archived. In general, journals do not require archiving of data and methodology much before publication and if, as was the case with these papers, the authors decline to disclose it, the IPCC is taking into account science that cannot effectively be challenged by critics or officially recognized reviewers.

Stephen McIntyre also wrote[43] to the president of US National Academy of Sciences asking him to use his influence to persuade scientists to make available the data they had used in many key papers relied upon by the IPCC in several of its reports. They included, Lonnie Thompson, Rosanne D'Arrigo, Gabrielle Hegerl, Jan Esper, Edward Cook, Tim Osborn, Keith Briffa and Michael Mann. Only a fraction of the data referred to has now been archived. Thus, despite its stated clear responsibility to make available to critical reviewers all the data and methodology of the science it reviews, the IPCC does not, and while the various national academies of science also exhort scientists to follow best practice, there is no enforcement.

THE SURFACE RECORD

In assessing if current warming is exceptional compared with estimated historic

[42] http://www.climateaudit.org/?p 640

[43] http://www.climateaudit.org/correspondence/cicerone.letter.final.doc

temperatures, the current temperature or "surface record" is just as important as historic estimates. It is tempting to imagine that with modern technology it is easy to accurately measure temperature, but here we are discussing changes of tenths of a degree in a fluid atmosphere worldwide. The readings from many weather stations are "averaged" to give the surface record, for the region, hemisphere or the globe. This process is not simple. Earlier temperature data were collected in differing ways and adjustments are made to many readings, which are first averaged over smaller grid cell areas. A key issue in dispute concerning the surface record is urbanization which is a form of anthropogenic warming but has little to do with greenhouse gases, and will not be prevented by their reduction. The effect is not insignificant. London, at times, is up to 8°C warmer[44] than the surrounding countryside. Even small communities and minor local changes in vegetation can generate local warming. Simply providing hard standing and shelter for researchers near to weather stations can alter readings.

Buildings and paving, as well as changes in agriculture, have a substantial warming effect and without question there have been major increases in these since the middle of the last century. Adjustments are made to many temperature measurements in an attempt to eliminate the effects of urbanization as well as changes in location and procedures. The adjustments in some cases are of the same order as the trend allegedly being detected a practice that most experienced scientists and engineers view with the greatest suspicion.

It is easy to overlook the dramatic changes in urbanization that have taken place recently. Figures[45] from the USGS for world production of cement, which is a likely predictor of urbanization, show world production of cement rising from 50 million tons in 1945 to over 2000 million tons today. Asphalt figures show a similar rapid growth. Residency of both cement and asphalt in the environment are high and it appears highly likely that urbanization effects have grown dramatically since 1970.

Events in August 2007 have put the surface record in the spotlight. NASA's GISS temperature record is cited in IPCC, 2007. Stephen McIntyre discovered[46] a significant error in the compilation of the GISS temperature record for the United States which, when corrected, reduced the post 2000 temperature anomalies by 0.15°C which is a substantial error as the GISS data show a rise of only about 0.5°C for the USA as a whole over the last century. One side effect of the correction is to the ranking of the hottest years in the USA, with 1934 now the hottest, slightly hotter than 1998. As part of his investigation Stephen McIntyre and others had been looking at the weather stations in the USA and finding that many fall way below the standards that we might expect. One used in the GISS series is in an asphalt car park in Tucson that has an "adjustment" of about 1°C.

The response from James Hansen[47] that the error makes a difference of only 0.01°C to the global statistics entirely misses the point. Until after the error was discovered

[44]London's Urban Heat Island: A Summary for Decision Makers. Available at http://www.london.gov.uk/mayor/environment/climate change/docs/UHI summary report.pdf

[45]http://minerals.usgs.gov/ds/2005/140/cement.pdf

[46]http://www.climateaudit.org/?p 1868#comments

[47]http://www.columbia.edu/~jeh1/distro LightUpstairs 70810.pdf

GISS had refused to publish the detailed computer programme code used and the error was only discovered by painstaking analysis of raw and adjusted data and "reverse engineering" in much the same way as the errors in the "hockey stick" methodology were found. No independent verification of the surface records used by the IPCC has been carried out and without full access to the computer programme code and data (all of which has been publicly funded) no critic has the resources to reverse engineer all of them. Given that significant programme code errors were found in the GISS temperature series, the "hockey stick" and the ClimatePrediction.net model, there is good reason to believe that full disclosure of computer programme code and data will reveal other faults in key studies long taken for granted. In the case of the GISS series, the station locations were identified and the data properly archived. Until October 2007, the more alarming surface record produced by the UK's Climatic Research Unit (CRU) was far more secretive with few of the details needed to verify it disclosed to outsiders. In the previously referenced presentation to the NRC panel Hans von Storch singled out Philip Jones for criticism for his response to one critic saying,

> "*We have 25 or so years invested in the work. Why should I make the data available to you, when your aim is to try and find something wrong with it*." (Jones' reply to Warwick Hughes, 21. February 2005).

As in the case of the "hockey stick", the AR4 WGI Chapter 3 text, which discusses surface records, and particularly urbanization, was vigorously opposed by some reviewers. The assertion, that urbanization is adequately adjusted for, is supported primarily by papers from two of the chapter's lead authors, Philip Jones and David Parker - which again raise the issue of bias. Readers can see from the reviewers' comments on this chapter that considerable controversy exists in this aspect of climate science. The studies from Parker are rather limited, dealing with a small sample size and focusing on windy versus calm nights. As with other studies on urbanization the datasets and methodology are somewhat opaque. Jones *et al*. (1990)[48] was recently the subject of a Freedom of Information Request in the UK to discover which weather stations were chosen in the paper's section on China and why. For the purpose of demonstrating the presence or absence of urbanization, homogeneous rural records over the period studied are essential. Location, instrumental or other changes can have as much effect as the urbanization being investigated. When pressed to explain the selection criteria for the Chinese stations used in Jones *et al*. (1990) CRU said[49],

> "*We do not have any information about why the sites for the 1990 paper were selected as Dr. Jones is unaware of how his collaborators selected the sites*."

When the records for the stations are examined it is clear that many had moved more than once in the study period and this has led to the validity of the paper's conclusions in respect of China being challenged[50].

[48]Jones, P.D., et al., 1990: Assessment of urbanization effects in time series of surface air temperature over land. *Nature*, 347, 169 172.

[49]Response from CRU available at: http://www.climateaudit.org/?p 1323

[50]http://www.informath.org/WCWF07a.pdf

CONCLUSIONS

Study of the partial release of the reviewers' comments and authors' responses dispels any notion that climate science is settled. It also shows that preparation of the IPCC AR4 did not proceed, as required, "*on a comprehensive, objective, open and transparent basis*." Furthermore, the disclosure of bad practice by the IPCC is only partial, because it is unclear how seriously the review process is taken by the chapter authors. The responses are in many cases vague or simply don't address the issue raised. When a seemingly simple and sensible suggestion from Co-Chair Susan Solomon, which appears to have been accepted, does not appear in the final text we are entitled to ask why. The fact that reviewers cannot see other reviewers' comments until after the review, and thereby realise that others shared their criticism, makes the review process entirely one sided and allows the lead authors to brush off cogent arguments. With the technology available to AR4, the review process could have been conducted on line with public viewing, in which case the assessment might have turned out very differently.

The many references in the popular media to a "consensus of thousands of scientists" are both a great exaggeration and also misleading. The validity and relevance of almost all of the vast IPCC report is dependent upon the determination of the key scientific question as to what extent, if any, human activity is responsible for recent warming. There is no agreed proven formula that gives the average global temperature for a given concentration of greenhouse gases. Mitchell *et al*. (2007) [51] say clearly "***It is only possible to attribute 20th Century warming to human interference using numerical models of the climate system.***" The alarming model projections depend upon many unproven assumptions and are only consistent with the assumption that current warming is exceptional compared with the past when greenhouse gas concentrations were lower. It is therefore the consensus on this matter alone, which is important.

Three chapters of the IPCC WGI assessment report are fundamental to any conclusion that humans are interfering significantly with the climate, and must be valid separately for the assessment as a whole to be valid. These are Chapters 3 - Observations, 6 Paleoclimate and 9 - Attribution. The total number listed as authors and editors in each chapter are respectively, 50, 56 and 80. The numbers of lead authors were respectively 12, 16 and 9 with the balance made up of contributing authors a total of only 180 (6 authors are involved in more than one chapter). Of this total 72 are listed as from the USA, 29 from the UK, 15 from France and the remainder from 20 other counties. There are certainly a very large number of scientific papers reviewed and cited, but the majority are peripheral to the much fewer disputed studies like the "hockey stick" and the surface records. The evidence is clear that in these cases there is substantial dispute and chronic lack of disclosure, which renders support for their conclusions mere opinion.

One key question to be considered is what would have been the consequence of IPCC, 2007 accepting that the "hockey stick" and other reconstructions that cannot simulate the instrument record from 1850 to 2005 were not scientifically reliable.

[51] John Mitchell, Julia Slingo, David S. Lee, Jason Lowe & Vicky Pope: 'CLIMATE CHANGE Response to Carter *et al*.', *World Economics*, 8 (1): 221 228.

Given their previous prominence, the IPCC would have to withdraw the conclusions that flow from them and could only claim, as did NRC, 2006 on page 3 of its report, that it is "plausible" that it is now warmer than a thousand years ago. Against that many and probably most qualified scientists would say that there is at least equally plausible evidence that it is not. The effect of such a conclusion on modelling studies would be to undermine the credibility of the more alarming predictions. Unless a warming of significantly more than 1°C can be shown to be likely over the next century it would be impossible to maintain public and political support for the massive costs that the current burgeoning "global warming industry" imposes. The IPCC and the "hockey team" in particular appear to recognise this, and are desperate to shore up the shaky foundations of the science and deny the many uncertainties, as these comments on the final SPM draft suggest.

> **SPM-19** More careful reference to uncertainty and lack of understanding is required. In several places the casual reader might think we understand very little but in reality we know a lot but not enough to quantify it. [**Govt. of United Kingdom** (Reviewer's comment ID #: 2022-93)]
> **Response**: Specific suggestions considered where offered

> **SPM-804** The authors of this chapter should request an explanation from the lead authors of the SPM of why there is not a single graphic from the chapter shown in the SPM. Every other major section of the SPM has at least one supporting graphic. The lack of a supporting graphic in the "A Paleoclimate Perspective" section is effectively a slap in the face to chapter 6 authors. It also sends a disturbing message that AR4 is somehow backing away from paleoclimate-based claims made in the TAR where the results from paleoclimate studies were highlighted. [comment continues]
> [**Michael Mann** (Reviewer's comment ID #: 156-55)]
> **Response**: Not every chapter has a figure. Figures depend upon merit and need. Text has been clarified regarding conclusions of TAR and new conclusions here, as well as uncertainties.

While no "hockey stick" made it into the SPM this time, the "hockey team" did a good job in ensuring that the UK Government's wishes were met, resisting the inclusion in the technical reports of most of the serious doubts over the integrity of some of the key studies that are used to support the hypothesis of anthropogenic global warming.

That the "hockey stick" should have been so comprehensively invalidated by two highly qualified, independent, peer reviewed studies and public hearings, and yet is retained in any guise by the IPCC in its latest AR4 report, indicates how insular and unscientific a body the IPCC has become. Despite substantial research over the last 20 years by paleoclimatologists at significant expense to taxpayers, there is no historic temperature reconstruction that can accurately replicate the instrumental temperature record from 1860 to 2000, let alone to 2007. Equally the errors recently exposed in the GISS surface record, and the refusal to disclose pertinent data to allow verification,

makes all surface temperature records questionable. Unless all important studies are independently verified, it cannot be said that the late 20th century warming was particularly exceptional. And especially so given that no global warming at all has occurred since 1998, a period of eight years over which atmospheric CO2 increased by 15 ppm (4%). It is crystal clear that natural causes are a possible explanation for the entire instrumental temperature record to date. Indeed, beyond that and in conformity with Occam's Razor, the appropriate null hypothesis for climate research is that the changes in climate that we measure are a result of natural forcing agents unless and until it can be demonstrated otherwise. So far as I am aware, there is no empirical evidence published in refereed journals that invalidates this null hypothesis.

Wegman *et al*. showed that the paleoclimate field is heavily influenced by "*a tightly knit group of individuals who passionately believe in their thesis*." Similar small groups almost certainly exist in other key areas of climate science, such as amongst those scientists who study the instrumental temperature series or who perform the computer model attribution studies. The IPCC WGI is effectively run by small groups of inbred scientists from UCAR, CRU and the Hadley Centre, who have a strong and disproportionate influence on its processes and agenda. Rather than the consensus of thousands of scientists, the IPCC conclusions represent the passionate belief of a small number of scientists whose funding and research careers depend heavily upon continuing alarm. The belief is then shared by a much larger number of environmentally and politically motivated individuals, organisations and also businesses that have evolved to service the emission reductions that the IPCC calls for. The vested interests of these groups are powerful sources of bias.

The IPCC has no quality control systems or supervision to ensure that its governing principles are properly observed by its working groups, that the science it assesses has been fully open to informed challenge by critics and that it fully discloses the process by which it reaches its conclusions. Its resistance to openness and transparency allows invalid science such as the "hockey stick" to be relentlessly promoted against a background of lack of disclosure that denies other scientists access to the information they need to conduct independent studies. The IPCC's governing principles are interpreted loosely, for example the strong scientific and statistical disagreements expressed by reviewers are not adequately, if at all, recorded in IPCC reports. Unpublished papers supporting IPCC orthodoxy are included even though their supporting data and methodology are not available. The use of non-disclosure agreements runs entirely counter to the IPCC's role. Far too much is made of "climate science" as a discipline. Only recently have there been formal courses in Climatology and most of the current senior practitioners gained their qualifications in other disciplines. More to the point, the shortcomings in many science papers used by the IPCC are not usually speciality-related but rather result from ignorance or misuse of advanced (and even standard) statistical methods, computer programming, basic scientific procedures and simple common sense.

Knowing the background and views of the 2006 NRC panel, many critics doubted that it would deal objectively with its investigation into the "hockey stick", but its report in large measure proved them wrong. It may well be that its open format with public presentations to independent professionals and cross-examinations of experts

by their peers is the model to which the IPCC should turn. With the increasing ease and low cost with which information can be made available, and the increasing use of video streaming, a format more conducive to scientific enquiry could be created. The idea that scientists who undertook the original studies, or are closely associated with those who did, should sit in judgement upon the question of which studies should be cited and relied upon is a recipe for horse trading, concealment and bias. Suitable amendments to the IPCC's governing principles might include:

- For each chapter an independent expert with appropriate technical experience but not directly associated with the chapter subject matter or any of the study areas should act as chairperson with the specific duty of ensuring that reviewers have every reasonable opportunity to challenge the proposed text and that where dispute exists and is supported in the professional literature it is properly recorded in the text.
- A formal appeals process needs to be provided by a panel of the independent chapter chairpersons for circumstances where authors or reviewers cannot agree on either the text or the interpretation of the governing principles.
- Only studies for which data, methodology **and computer programme code** are independently certified to be fully archived and available should be reviewed. The International Standards Organisation should be invited to develop standards for the peer review of important studies and archiving of datasets and programme code.
- Where sample selection is involved criteria must be explained, justified and the full dataset from which the selection is made must remain available.
- Where advanced statistical or numerical techniques are used, independent external verification of the methodology and computer programmes should be required before studies are accepted.
- The IPCC review process should be conducted openly so that when disputed issues are decided it can be seen why cogently presented arguments are rejected. Revised text should be resubmitted for review until a consensus is agreed or the differing views recorded in the text. It should also be clear if changes to text are made after the expert review process, who requested them, why they did and who agreed to them.
- Each chapter should have a quality control assessment in the same way as it now has a list of references. For the most contentious issues, archiving standards of the studies should be shown together with the degree of commonality the studies have in respect of authors, data and methodology.
- Those critical of the science should be fully included in the process and allowed to publish minority reports on the same dates as the main reports if they are unhappy with the representation of their views in the main report.
- Governments should be given an adequate time to preview the final reports so as to be able to write their own summaries, individually or collectively, but should not be able to alter the work of the expert panels or publish their summaries ahead of the reports themselves.

David Holland is an engineer, and a member of the Institution of Engineering and Technology. He has followed the scientific debate over the human contribution to global warming for many years, and is a co-author of 'The Stern Review: A Dual Critique, Part I The Science.' (2006) *World Economics* 7, 4. No funding has been sought or received in connection with this or any paper written by the author.