Response to David Henderson's "*Governments and Climate Change Issues: The Flawed Consensus*"

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1 Introduction

There are, sometimes, cheap shots taken at the UN Intergovernmental Panel on Climate Change (IPCC); there are commentators who, without having read any of its reports, simply attack it on political grounds. Unthinking hostility towards the IPCC is as harmful as unthinking adulation. The IPCC addresses itself to serious but very contentious scientific matters, and, in principle, aims to do so in a conscientious and professional way. But it is not guaranteed to be correct, nor do its procedures guarantee that it is unbiased and properly representative of the full spectrum of professional views. David Henderson's presentation in this conference volume politely but firmly calls upon governments to consider what would be the consequences if the IPCC reports are biased or flawed. We ought to approach such a question without a presumption of guilt, but we should not be afraid to ask the question. As the saying goes, *trust but verify*.

Questions about the competence and objectivity of the IPCC matter, not only because of the importance of the climate change issue, but because the IPCC occupies an institutional niche as the designated monopoly supplier of scientific advice for countries under the UN Framework Convention on Climate Change (UNFCCC). Wording in IPCC reports have major implications for global energy policy, and if conclusions were to be issued by the IPCC that trigger Article Two of the UNFCCC, then its conclusions would effectively carry the force of international law.

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Therefore I agree with David Henderson that critical scrutiny of the IPCC is much needed, with particular reference to oversight mechanisms. Considering the IPCC's international role, it is remarkable that the only oversight is managed by the IPCC itself. A question to motivate our line of inquiry is: Suppose a journalist or researcher uncovered incontrovertible evidence that the IPCC had deliberately falsified data during the final preparation of one of its Assessment Reports. What is the phone number of the agency that could be called upon to investigate and, if necessary, prosecute? And what court would have jurisdiction?

In my comments I will explain why I believe the core group that influence the IPCC's reports and conclusions is biased towards the view that greenhouse gases are the cause of major, deleterious global warming, and why I think this bias leads them to censor or even misrepresent opposing evidence. I will draw on my interactions with Working Group I over its use of my research and that of others with which I am familiar. But I think David's proposed remedies are not adequate. Any solution needs to create strong incentives for the IPCC to fix itself. After presenting evidence of IPCC bias, I will use some economic reasoning to propose a fix for the IPCC that would, I hope, not simply replace its biases with different biases, but would make it more truly objective and balanced.

2 Examples of bias in the IPCC process

There are many layers to the IPCC. At its core are a few dozen individuals who serve as IPCC Bureau members or Lead Authors, and who exert considerable control over the contents and conclusions of the report (chiefly the Working Group I contribution to the Fourth Assessment Report, or AR4, with which I am concerned herein). Around them, like concentric circles, are larger and larger groups of contributing authors, reviewers and government reviewers, until the widest circle encompasses many hundreds, perhaps thousands, of people. I served as an expert reviewer, by invitation rather than self-nomination, for what that's worth. I found—admittedly to my surprise—that many sections of the IPCC report are truly well-done. They reflect credible expert contributions from knowledgeable sources. However, these sections don't grind axes or tell scary stories: they report on advances in scientific understanding, while making clear how complex and difficult the study of climate remains. This is not the stuff on which the Summaries and subsequent headlines get built.

In key places the tone of the report changes. It becomes brittle, argumentative and slanted. Here the heavy hand of the core writing team takes control, on topics that dominate the overall conclusions and the Summary for Policy Makers (SPM). Since contributors and reviewers are never asked to vote on whether they agree with the SPM, any suggestion that the core writing team speaks for the thousands of people in the wide concentric circles is misleading.

I have published on some of these key topics, and I followed the IPCC's drafts on them closely. In every case, partisans on the alarmist side of current controversies were asked to summarize the debates, an obvious conflict of interest, resulting in tendentious and incomplete discussions. I will discuss a few examples, then in Section 3 I will propose how to make the policy process

robust to the possibility that the IPCC is wrong, and even give the IPCC an incentive to start getting things right.

2.1 Surface Temperature Record

It is intuitively obvious (and empirically well-established) that the growth of cities, and other transformations of the earth's landscape, can cause a rise in local temperatures. Climatic data is supposed to be adjusted so that it only measures a "pure" air temperature signal, in effect showing what the temperature in a region would have been if there had never been any human settlement in the area. Treating published climate data as if they accurately reveal this hidden information requires some heroic assumptions. Since climate models do not predict a spatial pattern of warming that matches the spatial pattern of industrial development, there is a simple test of how successful the data adjustment models are. If climate data are uncontaminated, the spatial pattern of warming trends over land should be uncorrelated with the spatial pattern of industrial development and other indicators of measurement quality. The IPCC and other authors have long asserted this independence. Indeed their conclusions about warming and the detection of a greenhouse gas influence presuppose it. But until a few years ago nobody had ever tested it.

There were three studies published after the Third Assessment Report, one coauthored by me (McKitrick and Michaels 2004) and two by the team of de Laat and Maurellis (2004, 2005) that tested the hypothesis. We worked independently and published our papers not knowing about each others' work. We used independent data sets and approached the topic with different statistical methods. We all concluded, with overwhelming statistical significance, that the IPCC's climate data are contaminated with surface effects from industrialization and data quality problems, which together add up to a large warming bias on the order of half the observed warming over land since 1980.

In its first two drafts, the IPCC simply ignored these papers. They referred instead to an 18 year-old paper by IPCC Lead Author Phil Jones that had failed to find an urbanization bias in a few regions of Asia and Australia, a similar study for the US by Peterson, and two recent papers by IPCC Lead Author David Parker which compared the strength of urban heat islands on windy and calm nights, finding little difference. On this basis they asserted quite categorically that their climate data is uncontaminated by a warming bias due to land surface changes or measurement problems.

This question goes to the very heart of the IPCC's position. If the locations of maximum warming coincide with the locations of socioeconomic development, and if this is not predicted by climate models as a consequence of greenhouse gases, it would imply that the fundamental data set used by the IPCC is contaminated, and would put into question a host of their conclusions about the extent of observed warming and its attribution to greenhouse gases. The topic deserved an extensive and prominent treatment, but instead it was hurriedly dismissed.

In my expert review comments I drew attention to my paper and those by de Laat and Maurellis, and rebutted the arguments based on the Jones paper. In the second draft, nothing changed. The

IPCC continued to ignore the topic, so I reiterated all my criticisms in the second round, leading to the following response by the Lead Authors (Chapter 3 Second Draft Review Comments, line 3-453):

> Rejected. The locations of socioeconomic development happen to have coincided with maximum warming, not for the reason given by McKitrick and Mihaels [sic] (2004) but because of the strengthening of the Arctic Oscillation and the greater sensitivity of land than ocean to greenhouse forcing owing to the smaller thermal capacity of land. Parker (2005) demonstrates lack of urban influence.

Elsewhere the Lead Authors dismissed my paper by saying it is "full of errors," without providing any evidence or details.

The appeal to the Arctic Oscillation to explain away the warming pattern in land-based surface temperatures across both hemispheres is ridiculous. Where the IPCC discusses causes of observed climate change they don't even invoke the Arctic Oscillation to explain *Arctic* warming, let alone warming in South America or Africa. In Chapter 9, which discusses attribution of climate change to anthropogenic factors, the IPCC makes only one mention of the Arctic Oscillation (p. 693), where they note that its exclusion from an analysis has no effect on the results. In discussions of factors behind Arctic temperature changes (pp. 694, 714, 716) it is not mentioned at all.

It is highly ironic that, confronted with published, peer-reviewed evidence of an anthropogenic, but non-greenhouse, effect on temperature trends, the IPCC denied it by appealing to natural causes. In the end they seemed to conclude that they were obliged to discuss the topic, but the text they published turned out to be misleading.

In the published version of the AR4 (Chapter 3, page 244), a paragraph was inserted regarding the three papers in question (though only the latter de Laat and Maurellis paper was cited).

> McKitrick and Michaels (2004) and De Laat and Maurellis (2006) attempted to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanisation and related land surface changes have caused much of the observed warming. However, the locations of greatest socioeconomic development are also those that have been most warmed by atmospheric circulation changes (Sections 3.2.2.7 and 3.6.4), which exhibit large-scale coherence. Hence, the correlation of warming with industrial and socioeconomic development ceases to be statistically significant. In addition, observed warming has been, and transient greenhouse-induced warming is expected to be, greater over land than over the oceans (Chapter 10), owing to the smaller thermal capacity of the land.

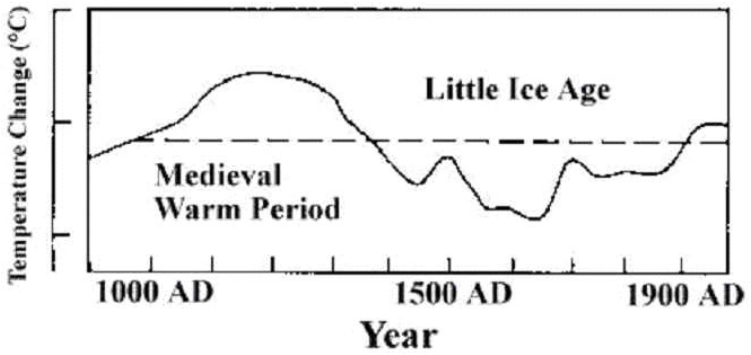
Note the slanted language: "*attempted to demonstrate…*" instead of "*showed*." This is an example of the subtle bias created when partisans get to write the report summarizing disputes of which they are a part.

The paragraph effectively admits the existence of evidence that the spatial pattern of warming coincides with the spatial pattern of industrial development, but attributes it to natural atmospheric circulation changes, referring the reader to two subsequent sections. Neither section shows any such thing—the overlap between warming patterns and socioeconomic development is simply not mentioned in those sections. The claim that our results become statistically insignificant when this effect is controlled for is a pure fabrication. Our papers show no such thing, nor do the cited sections, nor does the IPCC have any statistical evidence to back up their assertion, nor can they point to any peer-reviewed publication for support. Their claim is simply made up.

Thus, on one of the most important topics raised in the report—published evidence of significant contamination of the primary data set they use for measuring global surface warming over land and testing for an influence of greenhouse warming—the IPCC authors initially dismissed published, peer-reviewed evidence by appeal to an irrational, ad hoc speculation about the Arctic Oscillation, and then fabricated a claim that the evidence against their position was statistically insignificant. I consider this to be a serious failing on the part of the IPCC.

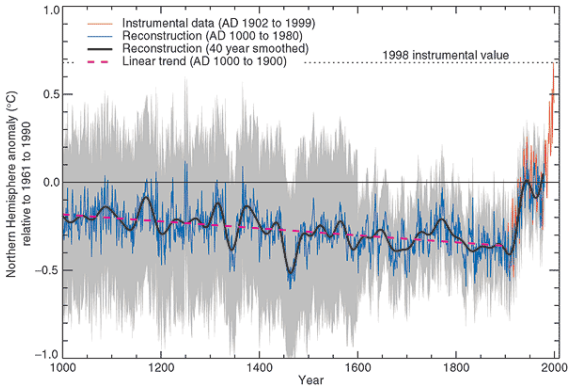
2.2 The Paleoclimate Record

In the First Assessment Report (1991), the IPCC declined to conclude that 20th century warming could be attributed to humans, citing an apparent warming interval in the medieval era as a significant obstacle to drawing such a conclusion. They published this schematic:



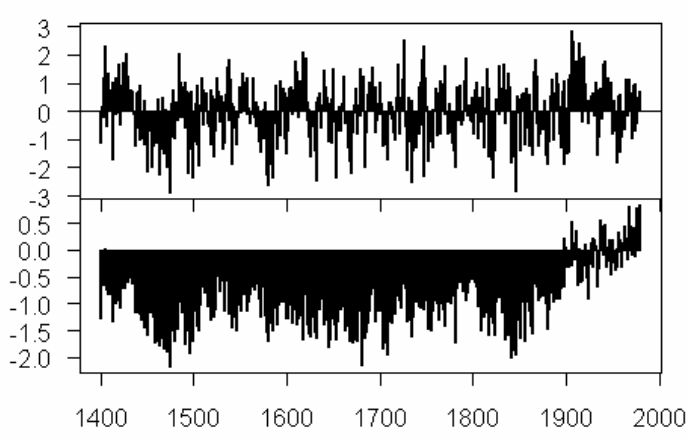
Source: *First Assessment Report*, Intergovernmental Panel on Climate Change, 1990

In the IPCC's Third Assessment Report, released in 2001, this history was swept away in light of the now-famous "hockey stick" graph of Michael Mann, which appeared prominently in the Summary for Policy Makers and at least four other places in the report. The graph appeared to show that there was no medieval warm period, and the 20th century climatic change was beyond all bounds of natural variability.



Source: *Third Assessment Report*, Intergovernmental Panel on Climate Change
http://www.grida.no/climate/ipcc_tar/slides/05.16.htm

The remarkable visual shape is not characteristic of Mann's proxy library. The simple average of the proxies in his data set looks nothing like a hockey stick, and doesn't even slope up in the 20th century. The hockey stick shape emerged solely as a consequence of the way the data were averaged.



Top – Simple average of 415 proxy series in Mann's data set.
Bottom – hockey stick reconstruction.
Source: McIntyre and McKitrick submission to the US National Academy of Sciences expert panel on paleoclimate reconstructions, http://www.uoguelph.ca/~rmckitri/research/NAS.M&M.pdf

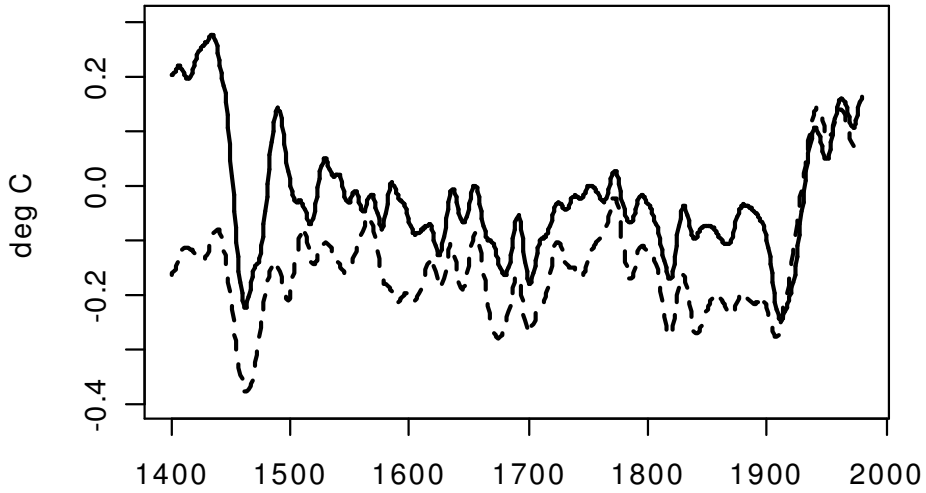
As is now well-known, and cannot be reviewed here in detail,[1] Stephen McIntyre and I published a series of articles that demonstrated calculation errors in the Mann analysis that undermined his conclusions.

[1] See http://ross.mckitrick.googlepages.com/#hockeystick for a collection of explanatory papers.

- We showed that he applied an incorrect principal component algorithm resulting in an over-weighting of a small set of tree ring indicators from bristlecone pines in Western USA, that had long been viewed as contaminated for the purpose of indicating historical temperature, due to the predominant influence of atmospheric chemistry rather than temperature on its growth rates.

- This error, in turn, masked the fact that his proxy model had no explanatory power for past temperature (i.e. was no more informative about the past than random numbers).

- It also masked the sensitivity of his results to the use of bristlecones.



The hockey stick (dashed) with the bristlecone influence removed (solid).
Source: Stephen McIntyre, pers. comm.

In light of the controversies raised by our findings, the US Congress asked the National Research Council and the Chairman of the National Academy of Sciences Committee on Theoretical and Applied Statistics to set up separate panels to review the matter.[2] Both panels accepted Steve's and my arguments and produced independent numerical replications to confirm our mathematical results. Both panels criticized the hockey stick, the latter in particularly severe terms. The NRC panel went into some wide-ranging examination of tree-ring based paleoclimatology and concluded, among other things, that the pre-1600 era is "murky" for the purpose of comparing to the present day. They specifically cautioned against relying on strip-bark formations in bristlecone pines, a type of proxy that underpins all paleoclimate climatic reconstructions used by the IPCC since 2001 to argue against a relatively warm medieval era. Without strip-bark

[2] Copies of the reports are available at http://www.uoguelph.ca/~rmckitri/research/trc.html.

samples, these reconstructions would all show the present climate to be unexceptional compared to the medieval warm period. They also accepted our findings that the claims of significant correlations between proxy series and temperature data were unfounded.

The sequence of IPCC drafts, reviewer responses, and the final wording, has been reviewed in a remarkable new paper by David Holland, to be published this year in *Energy and Environment*, which I strongly recommend for anyone who wants some insight into the real workings of the IPCC.[3] What Holland recounts, based on the publicly-available record of IPCC drafts and reviews, demands a verdict either of either profound stupidity or deliberate misrepresentation on the part of the IPCC Chapter 6 Lead Authors.

Despite having published five journal articles on the hockey stick controversy by the time the IPCC report was being drafted, the IPCC initially ignored all but our first paper. They falsely claimed that we had offered up a novel climate reconstruction that had failed model validation tests, and that we had been unable to replicate Mann's work because we omitted a key part of his data set. They also claimed that our results were rebutted in an unpublished paper by Wahl and Ammann, who had (they said) successfully replicated Mann's results. As we pointed out in our replies, none of this was true. We had repeatedly denied that we were presenting a new reconstruction, instead we were attempting to replicate Mann's reconstruction based on his stated methods and data. We showed that it was not possible to get his results using his stated data and methods. The IPCC failed to mention that we had proved to *Nature*'s satisfaction that the original disclosure of data and methods was, indeed, inaccurate, and a *Corrigendum* had been ordered.[4] Based on the amended disclosure of data and methods, the results of Wahl and Ammann were identical to *ours*, not to Mann's, and, like us, Wahl and Ammann had found that Mann's claims of finding statistical significance could not be replicated. But the draft version of the Wahl and Ammann paper submitted to the IPCC omitted the latter findings, which were included in the version they had submitted to a journal for publication. And their paper had not been published, which should have ruled out its usage by the IPCC in any case.

We (and others) submitted detailed critical replies rebutting the IPCC's summary. In their notes to the Review Editors the chapter authors appeared to concede the criticisms and said the text would be edited, but the Second Draft was almost identical to the first. The IPCC repeated its mischaracterization of our work and continued to claim Wahl and Ammann's (still unpublished) paper refuted our claims. They also added a grudging acknowledgment that our critique of the flawed principal component analysis "may have some foundation," but they dismissed it anyway, despite both the NRC and Wegman panels both having upheld our analysis.

We, and others, filed further objections to this section. The US Government Reviewer noted that the Wahl and Ammann paper had missed the IPCC's final, extended deadline for inclusion (it remains unpublished to this day), and ordered the removal of all references to it. The Lead

[3] It is available online at http://tinyurl.com/2szwh8.
[4] It was published in *Nature* on July 1, 2004, p. 105.

Authors ignored this request, flouting the IPCC rules in the process, and the Review Editors apparently rubber-stamped their decision.

The final, published text of the IPCC report thoroughly misrepresents the hockey stick debate, ignores published evidence against Mann's original results that had been upheld by two independent expert panels, relies on unpublished claims in the Wahl and Ammann paper while ignoring their replication of our results, etc. This whole section of the AR4 is indefensible and stands as a lasting testament to the bias of its authors, and the willingness of the IPCC process to indulge such biases.

2.3 Long Term Persistence

Detection of a trend in a data set, such as temperature records, requires more than simply fitting a line through it. The researcher has to decide if the trend is "large" or not, i.e. whether it is statistically significant, or outside the bounds of mere noise. This can be quite difficult to do since there are many properties of time series data that can cause the standard trend estimation formulas to give answers that are biased towards calling a trend significant when in reality it is not. For instance, most students of introductory time series analysis learn about the problem of *autocorrelation*, in which processes with an intrinsic "momentum" are slow to respond to random fluctuations. Autocorrelation can cause trend estimation methods to overstate significance.

A form of autocorrelation often observed in geophysical data is called Long Term Persistence (LTP). LTP models arose in hydrological studies of long-term Nile River depth records. Researchers noted that hydrological events (droughts, floods) tended to cluster together over time, causing long-term excursions in the data that appeared as pseudo-trends over short intervals. Standard time series analysis based on simple autoregressive models do not adequately capture this effect, so LTP models for geophysical data series have been developed. A substantial body of empirical work has been published in recent years showing that many basic climatic processes exhibit LTP, and classical statistical methods will lead to over-estimation of the significance of trends. Cohn and Lins (2005)[5] developed a test for the significance of trends in geophysical data that is robust to the presence of LTP. They showed that what appears to be a highly significant upward trend in a common 'global temperature' series under the autoregressive assumption falls to insignificance when the test allows for LTP. They conclude:

> "[With respect to] temperature data, there is overwhelming evidence that the planet has warmed during the past century. But could this warming be due to natural dynamics? Given what we know about the complexity, long-term persistence and non-linearity of the climate system, it seems the answer might be yes…natural climatic excursions may be much larger than we imagine."

[5] Cohn, T.A. and H.F. Lins, 2005: Nature's style: Naturally trendy. *Geoph. Res. Lett.*, **32,** L32402, doi:10.1029/2005GL024476.

While I have not (yet) published on LTP, I have served as referee for a few climatology journals on the topic. In my capacity as an IPCC expert reviewer I objected to the IPCC's use of simplistic, obsolete methods to evaluate the significance of trends in their temperature data sets. In response to the first draft of the report, I and another reviewer drew attention to the literature on LTP phenomena, asked that it be properly referenced, and that the temperature trend significance calculations be re-done using correct, up-to-date methods that followed the peer-reviewed literature. The chapter authors were antagonistic to this suggestion and refused to change their methods, but the second draft of the IPCC report did, at least, introduce a short discussion of the LTP issue as follows (Second Draft, page 3-9).

> Determining the statistical significance of a trend line in geophysical data is difficult, and many oversimplified techniques will tend to overstate the significance. Zheng and Basher (1999), Cohn and Lins (2005) and others have used time series methods to show that failure to properly treat the pervasive forms of long-term persistence and autocorrelation in trend residuals can make erroneous detection of trends a typical outcome in climatic data analysis.

A similar comment was inserted in the chapter appendix, though it included a disputatious and unsupported assertion that LTP models lack physical realism, to which I presented a counterargument in my second draft review comments.

The above paragraph makes an important point that has direct bearing on the overall conclusions of the IPCC report. Then without explanation, the above paragraph was deleted from the published edition. The entry in the Appendix was made even more disputatious, even though no supporting citations were provided for their dismissal of Cohn and Lins' results.

2.4 Level of Scientific Understanding

There are a few other areas where I have noted a pattern of bias in the IPCC, but which fall outside my own research areas. One is the evolution of "Scientific Understanding" ratings among drafts. There is a diagram in the Working Group I Summary for Policy Makers that sums up the estimated contributions of different planetary variables to the so-called "radiative forcing" of climate. Accompanying each one is an assessment of the "Level of Scientific Understanding" or LOSU. All the listed entries are Low, Medium or High. In the review comments on the Second Order Draft,[6] comment number 2-1273 read:

> It is notable (surprising?) that the level of scientific understanding for pre-satellite-era solar forcing which is based on proxies and models has jumped from "Very Low" in the TAR, to "Medium" in the AR4 figure. This should either be explained and highlighted here, or corrected including in this Figure which appears 3 times. In addition, this contradicts Chapter 2, page 6, lines 27-28!

[6] http://ipcc-wg1.ucar.edu/wg1/Comments/wg1-commentFrameset.html

The author response was:

> Changed to "low". Accepted

The difference between 'Very Low' and 'Medium' for a category as important as solar influence on climate implies quite a substantial difference in scientific understanding, yet is decided here by what amounts to haggling between a reviewer and an author. In other words, Lead Authors don't even claim enough scientific understanding to decide what the level of scientific understanding is. As will be pointed out below, the AR4 surveys historical solar proxies and finds a wide range of results with widely-varying implications for understanding the solar influence on climate. Had the reviewer not drawn attention to this item, the LOSU would have been listed as Medium; because of one objection it was scaled down to 'Low', suggesting that the authors had no basis for scaling it up so far in the first place.

	Level of Scientific Understanding (LOSU)			
Forcing Category	**1st Draft**	**2nd Draft**	**Final Draft**	**SPM**
Greenhouse gases	H	H	H	H
Stratospheric & Tropospheric ozone	M	M	M	M
Stratospheric water vapour from methane	L	L	L	L
Stratospheric water vapour from other	V. L	V. L	V. L	-
Tropospheric water vapour from irrigation	V. L	V. L	V. L	-
Aerosol scattering and absorbing	L-M	L	L-M	M-L
Cloud albedo effect	L	V. L	L	L
Cloud lifetime effect	V. L	V. L	-	-
Cloud semi-direct effect	V. L	V. L	-	-
Contrails and aviation cirrus	M	L-V.L	L	L
Solar	M	L	L	L
Cosmic Rays	V. L	V. L	V. L	-
Surface Albedo	L	L	M-L	M-L
Non-Albedo Surface	V. L	V. L	V. L	-
Volcanic	M	L	L	L
Proportion Listed as Very Low	***6 / 15***	***7½/ 15***	***4 / 15***	***0 / 8***

Table 1: Evolution of LOSU ratings in IPCC AR4 Table 2.11, and in the SPM Figure 2. '-' denotes *not shown.*

It is also interesting to look at the way the LOSU ratings were inflated between the second and final LOSU drafts—see Table 1. In the first draft, 6 out of 15 climate forcing categories were rated as Very Low scientific understanding. In response to reviewer comments, the second draft scaled down its certainty ratings so that 7½ out of 15 were Very Low (contrails includes two sub-categories, one Low and one Very Low). In other words, half the categories of major climatic forcings were subject to the lowest possible rating for scientific certainty. I did not find any review comments on the second draft saying this overstated the uncertainty, yet in the final, published report only 4 of 15 Very Low ratings are shown (with two categories deleted). And in the Summary for Policy Makers Figure SPM-2, none of the forcings in the Very Low categories

appear, creating the impression of greater certainty than was indicated in Table 2.11 at the close of scientific review.

2.5 Solar Reconstructions

The IPCC acknowledges that solar activity is high, and possibly exceptionally high, compared to the last 8,000 years. The two most prominent proxy-based reconstructions (from teams led by Solanki and Muescheler, respectively), differ on whether an interval in the 1700s included a spike comparable to today's but both agree that today's solar output is very high compared to most of the current interglacial era.

There have been many reconstructions of total solar activity based on sunspot counts, which began in the early 1600s. Up until recently, most reconstructions showed a strong upward trend in total solar irradiance since the 17th century, with low-frequency trends that track 19th and 20th century average temperatures reasonably well. Very recently, however, a different-looking reconstruction by Wang et al.[7] suggested that climatic forcing due to total irradiance had risen very little since the 1700s, implying an increase in solar output as little as one-tenth the size reported by other solar reconstructions. This would imply that solar changes could have little to do with climate change since 1600.

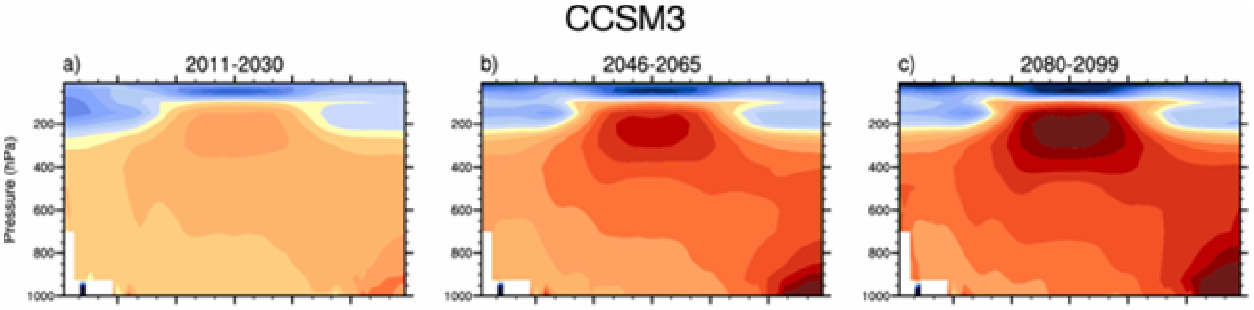
Rather than treating the Wang et al. results as one of a range of recent findings, it was presented as the sole and sufficient basis for a conclusive dismissal of the larger solar forcing estimates used in earlier IPCC reports, a dismissal that carried over into the Summary for Policy Makers, where it is concluded that solar influence on climate is much smaller than has been earlier proposed. Here the bias amounts to cherry-picking. Readers are told that one new result, standing at odds with a host of earlier studies, is the definitive word on the matter. Had the one new result found evidence for much stronger solar forcing than earlier thought, it is likely it would hardly have been mentioned, or it would have been presented in a disputatious and grudging aside.

2.6 Tropical Troposphere

GCMs implement a hypothesis in which strong infrared absorption by CO_2 raises the effective emissions altitude and forces a warming response centered in the tropical troposphere. IPCC Figures 9.1 and 10.7 confirm the importance of this mechanism by showing that repeated model runs, for historical and future intervals, are dominated by tropical tropospheric warming.[8] The following is an example.

[7] Wang, Y.M., J.L. Lean, and N.R. Sheeley (2005). Modeling the sun's magnetic field and irradiance since 1713. *Astrophysical Journal* 625: 522-538.

[8] For 10.7 see all 12 GCM runs at http://ipcc-wg1.ucar.edu/wg1/Report/suppl/Ch10/Ch10_indiv-maps.html;



Copied from http://ipcc-wg1.ucar.edu/wg1/Report/suppl/Ch10/Ch10_indiv-maps.html.

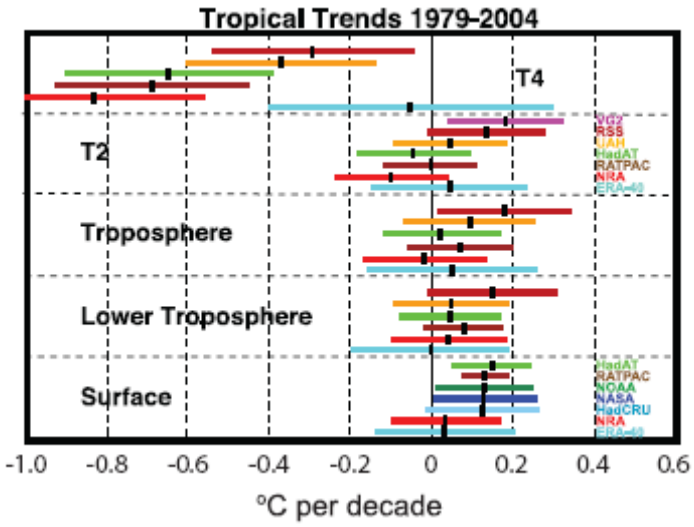
In each of these three panels, the horizontal axis shows latitude (left to right = South Pole to North Pole) and the vertical axis shows altitude, measured in atmospheric pressure, corresponding to about 25 km total height. The colour denotes the intensity of the warming trend. The 3 panels refer to 3 time intervals; in Figure 9.1 the IPCC generates a very similar profile as its prediction of what ought already to be observed in 20th century data.

The big red boil in the top middle is the tropical troposphere. Although a small region in the box, over the globe it takes up half the lower atmosphere. In all model runs, this is where the warming starts and runs strongest. Figure 9.1 indicates that this pattern is uniquely associated with greenhouse gas (GHG) effects. Amplified warming at the North Pole is associated with GHG emissions but is also associated with solar changes. The amplified warming over the tropics is only associated with greenhouse gases.

A similar hindcast is in the US Climate Change Science Program (CCSP) Report, page 25.[9] That report also shows the tropical troposphere should already be warming.

It is remarkable, therefore, that the IPCC did not plot the available data on the tropical troposphere in the same format as the model outputs above, to allow visual comparison and evaluation. The data they do show (AR4 Figure 3.18) is in a different and visually unobtrusive format. But its meaning is clear enough: across the six data sets there is essentially no evidence of significant warming in the tropical troposphere.

[9] http://www.climatescience.gov/Library/sap/sap1-1/finalreport/default.htm



Copied from IPCC AR4, Working Group I, p. 269.

The meaning of this is drawn out on page 11 of the US CCSP report on reconciling surface-tropospheric data (emphasis added)

> For global averages (Fig. 3), models and observations generally show overlapping rectangles. A potentially serious inconsistency, however, has been identified in the tropics. Figure 4G shows that the lower troposphere warms more rapidly than the surface in almost all model simulations, while, in the majority of observed data sets, the surface has warmed more rapidly than the lower troposphere. In fact, the nature of this discrepancy is not fully captured in Fig. 4G as **the models that show best agreement with the observations are those that have the lowest (and probably unrealistic) amounts of warming.**

Take out the "probably unrealistic" gloss and this is an admission of something pretty significant. The IPCC makes almost no mention of the issue. Had there been evidence of rapid warming in the tropical troposphere, the data would have been presented in a prominent format, accompanied by extensive discussion. The decision to downplay this topic is another indicator of the biased editorial hand guiding the final report.

3 How to Fix the IPCC

I could go on with other examples, but I hope by now to have justified my belief that the core writing team of the IPCC Report shares a single point of view, that its members are alert and pre-disposed towards evidence that confirms it, and they are unreceptive or openly hostile to evidence that contradicts it. Whether the reader agrees or not, take it as read, for the moment, that there is a problem of bias.

Now I'd like to turn to the question of how to fix the situation. David Henderson outlined some suggestions that seek to increase oversight and broaden the milieu. I agree that increased oversight may help. Together with Bruce McCullough, an econometrician at Drexel University, I have just finished a report for the CD Howe Institute[10] documenting the need for due diligence when empirical research is used in policy formation. While we mainly focus on the problem of pervasive non-reproducibility of economics research, the remedies we propose would also cover research from environmental scientists and others. Creating a mechanism for checking whether published research is reproducible, i.e. whether the data and methods have been accurately and fully disclosed, would catch a lot of climate science in its net.

But as for bringing in other branches of government to try and balance out the process, my concern is that the milieu simply swallows up new entrants, alive and whole. The incentives just aren't there for other divisions of the government to question the IPCC in any serious way. If a team of economists at, say, the US Bureau of Commerce, were asked to weigh in on the climate change issue, it would quickly become apparent to them that their troubles would be minimized by conceding the scientific grounds entirely and confining their comments to some narrow economic issues. If they were to weigh in on, say, statistical methods in greenhouse gas "signal detection" methods, even if they had as much or more qualifications to do so as their colleagues in an environmental ministry, they would know (or soon learn) that any challenge will be met with an extremely demanding counter-attack. They would have to devote all their energies for several months to answering a barrage of responses, usually lobbed with considerable public hostility and vituperation, and even if they succeed in answering them, the targets of their criticism would retain the option of carrying on as if nothing had changed, safe in the assumption that the IPCC will not demote their views as long as they continue to get them into print in any form, and, in a pinch, even if they can't get them into print.

The alternative of keeping one's head down and sticking to one's own knitting, would save a lot of bother and would be an irresistibly attractive alternative. Because these incentives would likely prevent any serious oversight from adjacent government agencies, I do not believe any solution will emerge from within the government or international government agencies.

Ultimately, reform cannot be imposed from the outside. The core IPCC leadership must *want* to be a neutral, accurate and honest information source. If they are ultimately interested in promoting a policy agenda, then they will always find a way around any attempt to force them to be balanced. So I would prefer a policy that would create incentives for accuracy in the IPCC.

My proposed solution will, at first glance, appear to have nothing at all to do with reforming the IPCC. It begins with a proposal for a carbon tax. Suppose a government—anywhere in the world, but preferably one with a large industrial economy like the USA—imposes a carbon tax *whose value is tied to the mean temperature of the tropical troposphere*, averaged over two or more of the data sources shown in Figure 3.18 of the AR4, Working Group I Report.

[10] McCullough, Bruce D. and Ross R. McKitrick. "The Case for Due Diligence when Empirical Research is Used in Policy Formation." C.D. Howe Institute Commentary, Fall 2007.

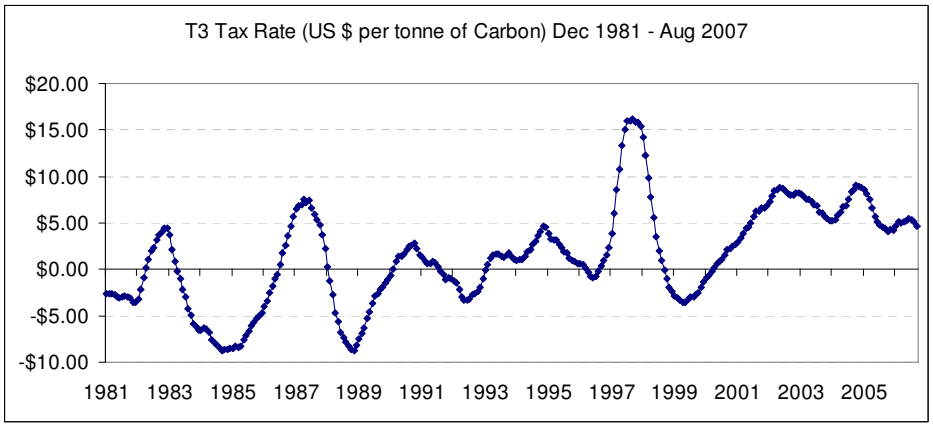
The IPCC has predicted that this region of the atmosphere is supposed to lead the global warming process, *if it is caused by greenhouse gases*. Other regions of the planet (especially at the Northern Hemisphere surface) might warm due to circulation changes, urbanization, or other factors. But IPCC climate models suggest that only one thing will cause a sustained, pronounced warming in the tropical troposphere, greenhouse gases.

So, suppose the US government (or any other government) implements a low carbon tax, with the revenue recycled locally, calibrated to that temperature measure. I would call it the T3 tax, for 'tropical tropospheric temperature.' If the mean tropospheric temperature starts going up, the T3 tax would go up, forcing emissions down. If the tropical troposphere does not warm up, the tax won't go up, nor should it.

I have spelled out the research behind this proposal in some essays available at ross.mckitrick.googlepages.com. Consider this formula:

$$T3 = 20 \times \tfrac{1}{12} \sum_{i=0}^{11} \tfrac{1}{2} \left(SC(t-i) + RSS(t-i) \right) \qquad (1)$$

where $SC(t)$ is the Spencer-Christy monthly mean tropical tropospheric temperature anomaly and $RSS(t)$ is the same from the RSS lab. Equation (1) says that the tax rate should be set equal to 20 times the twelve-month moving average of the mean of the RSS and UAH estimates of the mean tropical tropospheric temperature anomaly. By using a one-year trailing average the movements would be smoothed out, limiting spikes or drops due to, e.g. volcanic activity or strong El Nino events.



Formula (1), using the Spencer-Christy (University of Alabama) data and the Mears-Wentz (Remote Sensing Systems) data to compute the carbon tax.

Based on current data (as of August 2007) the T3 tax would be about US $4.70 per tonne of carbon, and it should be increasing by between $4 and $24 per decade, according to the range of IPCC projections. Its post-1980 historical trend is less than that, at around $3 per decade. It would exhibit far less volatility than the European carbon market price, which has swung between $0 and $30 (US) per year since inception.

Politically, an advantage would be that alarmists and skeptics alike should expect to get their preferred outcome. Alarmists will expect a rapidly accelerating carbon tax rate, whereas skeptics will expect to see a carbon price that stays low for a while then possibly drops when we enter solar cycle 25 around 2020, which is expected to be marked by diminished solar output.

Economically, it has the advantage of being a clear price mechanism. Other presentations at this conference by Gib Metcalf, Bob Mendelsohn and Pete Wilcoxen, will explain in more detail why choosing a carbon price is a particularly efficient way to achieve emissions reductions. This formula simply pins the rate to an objective measure of the effects of greenhouse gases. Whether the rate goes up or not, we will end up with the right outcome, without having to guess in advance what the right policy is.

As to the purpose of reforming the IPCC, note that the T3 tax would require firms to form long term expectations about future climate change, to guide today's decision-making. Someone building a pulp mill or a power plant would have to get the best information available about climate trends for the next ten or twenty years, in order to project the carbon price they will face. They do not want to know what today's value of the T3 tax is, they will want to know what it will be ten, twenty or thirty years down the road. This will create a market for *accurate* and *objective* climate analysis and forecasts. Firms with large investments on the line will have an incentive to get the tropospheric forecasts right. This will force them to peel away the layers of bias and drill down to objective science.

My conjecture is that a new consensus forecast would emerge which shows relatively little tropical tropospheric warming for at least the coming few decades. In other words I predict the *market* would say the IPCC is wrong. Its judgment will be based on the decision of neutral third parties, whose job is to forecast a tax rate that will guide hundreds of millions of dollars of private sector investment. Whoever holds that job will not have an inch of room to indulge his or her prior biases on global warming. Forecasters will have to get skeptical, dig into the data, ask hard questions of modelers, and convince their clients that they are giving trustworthy forecasts. Whether the forecaster is a true believer in global warming or not, he or she will know that clients need the right answer, regardless of whether it is politically correct or not, and the market will be looking for someone who can establish a track record of valid climate forecasting, regardless of what theory guides their modeling.

Over time, it is possible that the IPCC's analysis and forecasts would be vindicated. But I doubt it. I think that over time the IPCC would be seen as an outlier with a track record of exaggeration. At that point, the IPCC would have to reform itself or risk oblivion. Establishing a policy

framework which creates competition and rewards objectivity and accuracy will do more than anything to fix the problem of bias in an intellectual monopoly like the IPCC.