

House Science, Space and Technology Committee  
Examining the Process concerning Climate Change Assessments

31 March 2011

John R. Christy, University of Alabama in Huntsville

One Page Summary

1. Climate assessments like the IPCC have to date been written through a process in which IPCC-selected authors are given significant authority over the text, including judging their own work against work of their critics. This has led to biased information in the assessments and thus raises questions about a catastrophic view of climate change because the full range of evidence is not represented. Three examples follow.

1.A. Regarding the Hockey Stick of IPCC 2001 evidence now indicates, in my view, that an IPCC Lead Author working with a small cohort of scientists, misrepresented the temperature record of the past 1000 years by (a) promoting his own result as the best estimate, (b) neglecting studies that contradicted his, and (c) amputating another's result so as to eliminate conflicting data and limit any serious attempt to expose the real uncertainties of these data.

1.B. In the IPCC 2007 report, Dr. Ross McKittrick presented evidence that indicated warming processes other than greenhouse gas warming affected the popular surface temperature data sets. The IPCC authors were themselves producers of these data sets, yet as "final-say" authors they sat in judgment over this controversy, eventually denying McKittrick's evidence with what turned out to be (apparently) their own fabricated claim.

1.C. The EPA Finding misrepresented key evidence on the evaluation of climate models against real data. In IPCC-like fashion, the EPA gave authority to its hand-picked author team to respond to evidence which contradicted the Finding with assertions that were not based on reliable data or methods. The evidence shows the EPA overstated the agreement between models and observations when in fact there was disagreement.

2. Warming in surface temperatures is caused by many factors other than greenhouse gases, one reason they are poor proxies to depict greenhouse warming. Bulk atmospheric temperatures, a more direct proxy, show much less warming than models predict.

3. Because this issue has policy implications that may potentially raise the price of energy significantly (and thus essentially the price of everything else), the U.S. Congress should not rely exclusively on the U.N. assessments because the process by which they were written includes biased, false, and/or misleading information about one of the most murky of sciences – climate. In my opinion, the Congress needs at least one second-opinion produced by well-credentialed climate scientists but overseen by a non-activist team which includes those with experience in the scientific method, the legal aspects of "discovery," and who simply know what is important in answering the questions at hand.

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John R. Christy  
The University of Alabama in Huntsville

Written Testimony

I am John R. Christy, Professor of Atmospheric Science and Director of the Earth System Science Center at the University of Alabama in Huntsville. I am also Alabama's State Climatologist. My training and research have been almost exclusively in the area of climate studies. I built my first climate dataset when I was 15 in an attempt to understand and predict the interannual variations of rainfall in the San Joaquin Valley of California. It didn't work. Even so, climate science has been a passion of mine for almost 50 years.

I have served as Lead Author of the Third Intergovernmental Panel on Climate Change (2001) and a "Key" or "Contributing" Author on the others. I was chosen to receive a Special Award by the American Meteorological Society and NASA's Medal Exceptional Scientific Achievement for my work with Dr. Roy Spencer regarding the development of satellite-based climate datasets. I was elected a Fellow of the AMS in 2002. My main research deals with building climate datasets from scratch to understand what the climate has been doing and to test assertions made about the climate system.

I normally speak to congressional committees regarding the science of climate change as I did three weeks ago to the House subcommittee on Energy and Power. Those interested in that testimony are encouraged to access it (8 March 2011.) The question I was asked to address today relates to the *process* by which past climate change assessments were generated and how the final products of such efforts may be compromised. This is the same basic topic I addressed before the Inter-Academy Council (of Sciences) or IAC in Montreal last June. Some of the discussion below is contained in that testimony (Appendix A.) Additionally, Dr. Ross McKittrick provided information to the same House subcommittee three weeks ago and I wish to attach that as well (Appendix B) since I refer to it below. Finally, one of my responses to the EPA Endangerment Finding is discussed below and thus my full comment to EPA is attached as Appendix C.

In the following I will provide some general remarks on the shortcomings of the assessment process as I've experienced it, then provide three examples of how the process led to inaccurate information provided to policymakers, followed by a comment on temperature records and I will close with some concluding remarks.

### 1. General Remarks

The first basic problem with the entire issue here is that climate science is a murky science, not a classic, experimental science. As an emerging science of a complex, chaotic atmospheric and oceanic system, it is plagued by uncertainty and ambiguity in

both observations and theory. Lacking classic, laboratory results, it easily becomes hostage to opinion, groupthink, arguments-from-authority, overstatement of confidence, and even Hollywood movies. (For a formalized discussion of the uncertainties and ignorance in climate science see Curry 2011.)

The most prominent assessment of climate change science is produced through the Intergovernmental Panel on Climate Change or IPCC. These U.N. reports have appeared every few years, with the main reports coming out in 1990, 1995, 2001 and 2007. Understanding the selection and role of the authors is important for policymakers who want to understand the process.

In simplified terms, IPCC Lead Authors are nominated by their countries, and down-selected by the IPCC bureaucracy with help from others (the process is still not transparent to me – who really performs this down-select?) The basic assumption is that the scientists so chosen as Lead Authors (L.A.s) represent the highest level of expertise in particular fields of climate science (or some derivative aspect such as agricultural impacts) and so may be relied on to produce the most up-to-date and accurate assessment of the science. When these assessments are done, government organizations such as the U.S. EPA often adopt the reports in total, without investigation, to guide their agendas.

In one sense, the authors of these reports are volunteers since they are not paid. However, they do not go without salaries. Government scientists make up a large portion of the author teams and can be assigned to do such work, and in effect are paid to work on the IPCC by their governments. University scientists aren't so lucky but can consider their IPCC effort as being so close to their normal research activities that salary charges to the university or grants occur. Travel expenses were paid by the IPCC for trips, in my case, to Australia, Paris, Tanzania, New Zealand, Hawaii, and Victoria, Canada. Perhaps it goes without saying that such treatment might give one the impression he or she is an important authority on climate.

As these small groups of L.A.s travel the world, they tend to form close communities which often re-enforce a view of the climate system that can be very difficult to penetrate with alternative ideas (sometimes called “confirmation bias” or “myside bias”.) They become an “establishment” as I call them. With such prominent positions as IPCC L.A.s on this high profile topic, especially if they support the view that climate change is an unfolding serious disaster, they would be honored with wide exposure in the media (and other sympathetic venues) as well as rewarded with repeated appointments to the IPCC process. In my case, evidently, one stint as an L.A. was enough.

The second basic problem (the first was the murkiness of our science) with these assessments is the significant authority granted the L.A.s. This is key to understanding the IPCC process. In essence, the L.A.s have virtually total control over the material and, as demonstrated below, behave in ways that can prevent full disclosure of the information that contradicts their own pet findings and which has serious implications for policy in the sections they author. While the L.A.s must solicit input for several contributors and respond to reviewer comments, they truly have the final say.

In preparing the IPCC text, L.A.s sit in judgment of material of which they themselves are likely to be a major player. Thus they are in the position to write the text that judges their own work as well as the work of their critics. In typical situations, this would be called a conflict of interest. Thus L.A.s, being human, are tempted to cite their own work heavily and neglect or belittle contradictory evidence (see examples below.)

In the beginning, the scientists who wrote the IPCC assessment were generally aware of the new responsibility, the considerable uncertainties of climate science, and that consequences of their conclusions could generate burdensome policies. The first couple of reports were relatively cautious and rather equivocal.

In my opinion, as further assessments were created, a climate “establishment” came into being, dominating not only the IPCC but many other aspects of climate science, including peer-review of journals. Many L.A.s became essentially permanent fixtures in the IPCC process and rose to positions of prominence in their institutions as a side benefit. As a result, in my view, they had a vested interest in preserving past IPCC claims and affirming evermore confident new claims to demonstrate that the science was progressing under their watch and that financial support was well spent. Speaking out as I do about this process assured my absence of significant contribution on recent and future reports.

Political influence cannot be ignored. As time went on, nations would tend to nominate only those authors whose climate change opinions were in line with a national political agenda which sought perceived advantages (i.e. political capital, economic gain, etc.) by promoting the notion of catastrophic human-induced climate change. Scientists with well-known alternative views would not be nominated or selected. Indeed, it became more and more difficult for dissent and skepticism to penetrate the process now run by this establishment. As noted in my IAC testimony, I saw a process in which L.A.s were transformed from serving as Brokers of science (and policy-relevant information) to Gatekeepers of a preferred point of view.

A focus evolved in the IPCC that tended to see enhanced greenhouse gas concentrations as the cause for whatever climate changes were being observed, particularly in the 2001 (Third Assessment Report or TAR) which was further solidified in 2007, (the Fourth Assessment Report or AR4.) The IAC 2010 report on the IPCC noted this overconfidence when it stated that portions of the AR4 contained “many vague statements of ‘high confidence’ that are not supported sufficiently in the literature, not put into perspective, or are difficult to refute.” (This last claim relates to the problem of generating “unfalsifiable hypotheses” discussed in my recent House testimony.)

With an understanding of the power of the L.A.s in determining the content of the IPCC and thus EPA reports, I shall describe three situations, about which I am quite familiar, to support the claims made above.

#### 1.A.An Example from IPCC Third Assessment Report (TAR 2001) – the Hockey Stick

My experience as Lead Author in the IPCC TAR, Chapter 2 “Observed Climate Variability and Change”, allowed me to observe how a key section of this chapter, which produced the famous Hockey Stick icon, was developed. My own topic was upper air temperature changes that eventually drew little attention, even though the data clearly indicated potentially serious inconsistencies for those who would advocate considerable confidence in climate model projections.

First, note these key points about the IPCC process: the L.A. is allowed (a) to have essentially complete control over the text, (b) sit in judgment of his/her own work as well as that of his/her critics and (c) to have the option of arbitrarily dismissing reviewer comments since he/she is granted the position of “authority” (unlike peer-review.) Add to this situation the rather unusual fact that the L.A. of this particular section had been awarded a PhD only a few months before his selection by the IPCC. Such a process can lead to a biased assessment of any science. But, problems are made more likely in climate science, because, as noted, ours is a murky field of research – we still can’t explain much of what happens in weather and climate.

The Hockey Stick curve depicts a slightly meandering Northern Hemisphere cooling trend from 1000 A.D. through 1900, which then suddenly swings upward in the last 80 years to temperatures warmer than any of the millennium when smoothed. To many, this appeared to be a “smoking gun” of temperature change proving that the 20<sup>th</sup> century warming was unprecedented and therefore likely to be the result of human emissions of greenhouse gases.

I will not debate the quality of the Hockey Stick – that has been effectively done elsewhere (and indeed there is voluminous discussion on this issue), so, whatever one might think of the Hockey Stick, one can readily understand that its promotion by the IPCC was problematic given the *process* outlined above. Indeed, with the evidence contained in the Climategate emails, we have a fairly clear picture of how this part of the IPCC TAR went awry. For a more detailed account of this incident with documentation, see <http://climateaudit.org/2009/12/10/ipcc-and-the-trick/>.

We were appointed L.A.s in 1998. The Hockey Stick was prominently featured during IPCC meetings from 1999 onward. I can assure the committee that those not familiar with issues regarding reconstructions of this type (and even many who should have been) were truly enamored by its depiction of temperature and sincerely wanted to believe it was truth. Skepticism was virtually non-existent. Indeed it was described as a “clear favourite” for the overall Policy Makers Summary (Folland, 0938031546.txt).

In our Sept. 1999 meeting (Arusha, Tanzania) we were shown a plot containing more temperature curves than just the Hockey Stick including one from K. Briffa that diverged significantly from the others, showing a sharp cooling trend after 1960. It raised the obvious problem that if tree rings were not detecting the modern warming trend, they might also have missed comparable warming episodes in the past. In other words, absence of the Medieval warming in the Hockey Stick graph might simply mean tree ring proxies are unreliable, not that the climate really was relatively cooler.

The Briffa curve created disappointment for those who wanted “a nice tidy story” (Briffa 0938031546.txt). The L.A. remarked in emails that he did not want to cast “doubt on our ability to understand factors that influence these estimates” and thus, “undermine faith in paleoestimates” which would provide “fodder” to “skeptics” (Mann 0938018124.txt). One may interpret this to imply that being open and honest about uncertainties was not the purpose of this IPCC section. Between this email (22 Sep 1999) and the next draft sent out (Nov 1999, Fig. 2.25 Expert Review) two things happened: (a) the email referring to a “trick” to “hide the decline” for the preparation of report by the World Meteorological Organization was sent (Jones 0942777075.txt, “trick” is apparently referring to a splicing technique used by the L.A. in which non-paleo data were merged to massage away a cooling dip at the last decades of the original Hockey Stick) and (b) the cooling portion of Briffa’s curve had been truncated for the IPCC report (it is unclear as to who performed the truncation.)

In retrospect, this disagreement in temperature curves was simply an indication that such reconstructions using tree ring records contain significant uncertainties and may be unreliable in ways we do not currently understand or acknowledge. This should have been explained to the readers of the IPCC TAR and specifically our chapter. Highlighting that uncertainty would have been the proper scientific response to the evidence before us, but the emails show that some L.A.’s worried it would have diminished a sense of urgency about climate change (i.e. “dilutes the message rather significantly”, Folland, 0938031546.txt.)

When we met in February 2000 in Auckland NZ, the one disagreeable curve, as noted, was not the same anymore because it had been modified and truncated around 1960. Not being aware of the goings-on behind the scenes, I had apparently assumed a new published time series had appeared and the offensive one had been superceded (I can’t be certain of my actual thoughts in Feb. 2000). Now we know, however, that the offensive part of Briffa’s curve had simply been amputated after a new realization was created three months before. (It appears also that this same curve was apparently a double amputee, having its first 145 years chopped off too, see <http://climateaudit.org/2011/03/23/13321/>.) So, at this point, data which contradicted the Hockey Stick, whose creator was the L.A., had been eliminated. No one seemed to be alarmed (or in my case aware) that this had been done.

Procedures to guard against such manipulation of evidence are supposed to be in place whenever biases and conflicts of interest interfere with duties to report the whole truth, especially in assessments that have such potentially drastic policy implications. That the IPCC allowed this episode to happen shows, in my view, that the procedures were structurally deficient.

Even though the new temperature chart appeared to agree with the Hockey Stick, I still expressed my skepticism in this reconstruction as being evidence of actual temperature variations. Basically, this result relied considerably on a type of western U.S. tree-ring not known for its fidelity in reproducing large-scale temperatures (NRC 2006, pg. 52).

At the L.A. meetings, I indicated that there was virtually no inter-century precision in these measurements, i.e. they were not good enough to tell us which century might be warmer than another in the pre-calibration period (1000 to 1850.)

In one Climategate email, a Convening L.A., who wanted to feature the Hockey Stick at the time (though later was less enthusiastic), mentions “The tree ring results may still suffer from lack of multicentury time scale variance” and was “probably the most important issue to resolve in Chapter 2” (Folland, 0938031546.txt). This, in all likelihood, was a reference to (a) my expressed concern (see my 2001 comments to NRC below) as well as to (b) the prominence to which the Hockey Stick was pre-destined.

To compound this sad and deceptive situation, I had been quite impressed with some recent results by Dahl-Jensen et al., (*Science* 1998), in which Greenland ice-borehole temperatures had been deconvolved into a time series covering the past 20,000 years. This measurement indeed presented inter-century variations. Their result indicated a clear 500-year period of temperatures, warmer than the present, centered about 900 A.D. – commonly referred to as the Medieval Warm Period, a feature noticeably absent in the Hockey Stick. What is important about this is that whenever any mid to high-latitude location shows *centuries* of a particularly large temperature anomaly, the spatial scale that such a departure represents is also large. In other words, long time periods of warmth or coolness are equivalent to large spatial domains of warmth or coolness, such as Greenland can represent for the Northern Hemisphere (the domain of the Hockey Stick.)

I discussed this with the paleo-L.A. at each meeting, asking that he include this exceptional result in the document as evidence for temperature fluctuations different from his own. To me Dahl-Jensen et al.’s reconstruction was a more robust estimate of past temperatures than one produced from a certain set of western U.S. tree-ring proxies. But as the *process* stood, the L.A. was not required to acknowledge my suggestions, and I was not able to convince him otherwise. It is perhaps a failure of mine that I did not press the issue even harder or sought agreement from others who might have been likewise aware of the evidence against the Hockey Stick realization.

As it turned out, this exceptional paper by Dahl-Jensen et al. was not even mentioned in the appropriate section (TAR 2.3.2). There was a brief mention of similar evidence indicating warmer temperatures 1000 years ago from the Sargasso Sea sediments (TAR 2.3.3), but the text then quickly asserts, without citation, that this type of anomaly is not important to the hemisphere as a whole.

Thus, we see a situation where a contradictory data set from Greenland, which in terms of paleoclimate in my view was quite important, was not offered to the readers (the policymakers) for their consideration. In the end, the Hockey Stick appeared in Figure 1 of the IPCC Summary for Policymakers, without any other comparisons, a position of prominence that speaks for itself.

So, to summarize, an L.A. was given final say over a section which included as its (and the IPCC's) featured product, his very own chart, and which allowed him to leave out not only entire studies that presented contrary evidence, but even to use another strategically edited data set that had originally displayed contrary evidence. This led to problems that have only recently been exposed. This *process*, in my opinion, illustrates that the IPCC did not provide policymakers with an unbiased evaluation of the science, whatever one thinks about the Hockey Stick as a temperature reconstruction.

This story had a couple of postscripts regarding my involvement. First, The National Academy of Sciences contacted me shortly after the TAR appeared in 2001 for my views on the IPCC process. I indicated that the process was generally a pleasant experience, but that some things still bothered me. In my written submission to the NRC I stated that I believed too much emphasis was placed on the Hockey Stick.

*21 May 2001*

*To: Vaughan Turekian (NAS)*

*Subject: Question about IPCC*

*1000-year temperature record*

*This first concern arises from our chapter (2) for which I must accept as much blame as anyone. We (chapter 2 authors) are guilty of omitting information that indicated the temperature history of the past 1000 years is not as well known as is implied by the prominent figure in the SPM [Summary for Policymakers] (Fig. 1) and TS [Technical Summary] (Fig.5). At each of the Lead Authors meetings I pointed out that we should include mention of publications which strongly suggest the medieval warm period was warmer than the current century. In particular I mentioned the Dahl-Jensen et al. 1998 Science paper which I believe presents the most direct measurement of temperature and thus should be highlighted. Broecker (2001, SCIENCE) echoed the very concerns I had put forward in our meetings. In the final version of the text the Dahl-Jensen paper was not even cited in Section 2.3 - a fact I did not realize until last week when I read the report in detail (2.3 is the section on the temperature record of the past 1000 years.) Thus, its [Greenland's temperature] information was not carried forward in the TS or SPM. (The paper is only mentioned in passing regarding the warming 8 kybp in the TAR [Third Assessment Report].) I should point out that the final wording concerning the warmth of the 1990's and 1998 as "likely" the warmest of the past millennium (i.e. only 2/3 chance of being correct) tried to account for the lack of certainty in our knowledge of past temperatures. However, the very prominent placement of the time series of the last 1000 years in the TS and SPM overrules what tentativeness some of us actually intended. This is my personal view.*

*John R. Christy*

*University of Alabama in Huntsville*



Secondly, I served on the 2006 NRC panel that took another look at the temperatures of the past 2000 years and noted several findings about the Hockey Stick that had come to light since I wrote the above in 2001. That report stated that it was inappropriate to use the particular type of tree rings which dominated the early part of the Hockey Stick (p. 52), and that a key step in its mathematical method was so biased that even when a collection of random numbers were used for input, hockey stick shapes were produced (p. 91.) Overall, the NAS report concluded that methodological problems in reconstructions mean that “uncertainties of the published reconstructions have been underestimated” (p. 113.) For further critical analysis see the “Wegman Report” (Wegman et al. 2006). It is clear now, in my view, that the prominence accorded the Hockey Stick was inappropriate and that the IPCC failed to provide an accurate depiction of the state of climate science in this area.

Finally, you may hear that certain ad-hoc panels were assembled which examined these events and were claimed to have “exonerated” the scientists from major wrong doing. Please note that these reports have no true legal standing as the legal process was not followed, i.e. determining admissible evidence, discovery, cross-examination of the evidence and witnesses, the full inclusion of testimony by witnesses denigrated by these scientists, etc. A summary of this whole “exoneration” affair is given by Dr. Ross McKittrick in “response to climategate inquiries” at <http://rossmckitrick.weebly.com/submissionsresponses-to-govt-inquiries.html> .

#### 1.B.IPCC apparent fabrication of claims regarding surface temperature

The next two examples are well-described in the attached document supplied by Dr. Ross McKittrick of the University of Guelph, Ontario, sent to the House subcommittee on Energy and Power in relation to their hearing three weeks ago (Appendix B). The first situation I describe deals with an apparent fabrication of information regarding surface temperatures contained in the most recent IPCC AR4 (2007) and the subsequent usage of the information by the EPA in their endangerment finding. This is a situation encountered by McKittrick himself (Appendix B.1). The second incident focuses more on EPA’s mishandling of information, and I relate my own experience here (Appendix C.3.1a), but I direct you to McKittrick’s commentary in Appendix B.3 as an independent analysis of the same issue.

In the first case, a point of contention arose between McKittrick, an IPCC reviewer, and the IPCC L.A.s concerning evidence published by two independent groups which documented the contamination of the surface temperature record by industrialization and land-use change (De Laat and Maurellis 2004, 2006, McKittrick and Michaels 2004.) Numerous papers, including some by myself (e.g. Christy et al. 2009), have been published in this arena, but the two groups’ papers cited here specifically found patterns of warming over land that were statistically associated with patterns of socio-economic development, a correlation not predicted in model simulations of greenhouse warming. This of course would call into question the use of these surface datasets (maintained by some of the aforementioned L.A.s) as indicators of greenhouse warming of the planet.

*After the close of peer review*, the L.A.s inserted text into the IPCC report that described the findings pointed out by McKittrick, but then dismissed them by asserting that the correlations were due to natural circulation patterns, not industrialization, concluding that the “correlation of warming with industrial and socioeconomic development ceases to be statistically significant.” This claim was subsequently quoted by the EPA Finding, and thus, as demonstrated below, tarnishes that document as well. The problem? There was no evidence to support this claim made by the L.A.s - it was simply an assertion (perhaps a belief?) evidently invented to dismiss the offensive results.

McKittrick (2010) was later published which specifically tested the IPCC claim about the role of circulation patterns as the cause of the observed distribution of warming and found the IPCC claim to be false. Thus, the IPCC assertion had evidently been a fabrication. The key point here is that the IPCC process failed policymakers by not providing the complete picture of an issue and unfortunately produced not just misleading, but false information. Given that the IPCC L.A. team (a) exerted almost total control over the text, (b) were sitting in judgment of criticisms of datasets they themselves produced, and (c) were not required to accommodate alternate views, it is not difficult to see how such a failure could occur – a failure that can have significance for climate change policy. This, again, is an example of L.A.s acting as Gatekeepers, not as Brokers. Furthermore, the Climategate emails also shed light on the behind-the-scenes attempts by the L.A.s to squelch this important information – hardly the activity associated with an open and transparent process (see Appendix B.1).

### 1.C.EPA “Finding” relied on an IPCC-like review process

In its Finding (Part III.C.), the EPA essentially relies on climate model output to make claims about current and future climate changes being potentially dangerous and being caused by increases in greenhouse gases. The report, fundamentally, assumes that climate models are so precise in their depiction of the real climate that they are reliable for predictions and thus policy. In the public comment period, I was one of several who responded to this assertion with evidence to demonstrate that basic and fundamental features of climate model simulations do not effectively represent the real world.

A prominent signature of global warming due to greenhouse gases in climate models is a warming of the tropical upper atmosphere, generally between 8 and 12 km, that is much greater than the warming which models project for the surface. The signature in models is so prominent that it provides a relatively easy test against observations. Several studies have indicated that observations do not show this feature, which in turn casts doubt on climate model theory as representing greenhouse warming properly and on which the EPA Finding relied (e.g. Christy et al. 2007, Douglass et al. 2007).

In the review of the EPA draft, several responders, including me, informed the EPA that the EPA’s statement about agreement between observations and models had been improperly reported. We backed up our claims with published information. However, in their response to us, the EPA’s “authors” (themselves part of the establishment) in IPCC-

like fashion claimed “when uncertainties in models and observations are properly accounted for, newer observational datasets are in agreement with climate model results.” As far as we could tell, they did not give any serious consideration to contradictory evidence. This was another example of authors, who were utilized by the EPA, having the authority to ignore evidence that was clearly against their assertions. Rather than providing the range of views in the Finding, or at a minimum pointing out significant model uncertainty suggested by our results, the EPA authors acted as gatekeepers and mislead the readers (See Appendix C for my full review comments.)

In their response to our reviews, the EPA cited three papers which purportedly offered “new observations” to support their model vs. observations “agreement”, relying mainly on Santer et al. 2008. However, these “new” upper air data sets (RAOBCORE 1.3, 1.4, and Allen and Sherwood (2005) thermal wind derivation) and two of the “new” surface data sets (ERSST v2 and v3) had been shown to contain spurious trends when tested for accuracy and these versions are not used for trend estimation any longer. Santer et al., the EPA’s key citation, had done no testing of the observations as we had done. In my review, I went through the details of why Santer et al. 2008 had been incorrect in both their hypothesis test (where they neglected the pre-condition of surface trend agreement between models and observations – see bracketed note below) and with the data they used. However, the EPA simply allowed its own hand-picked authors to assert their conclusion. They did not objectively assess the conclusions of these contradictory studies or even acknowledge at a minimum that significant controversy continued on this issue. Further studies support the original comments of my review (e.g. Sakamoto and Christy, 2009, Klotzbach et al. 2009, Christy et al. 2010, McKittrick et al. 2010).

[I note here some technical points. Douglass et al. tested a hypothesis that depended on a specific condition. We addressed the question, “If models and observations have the same surface temperature trend, then do the models and observations have the same upper air trend?” In other words, we were testing the *relationship* between surface and upper air temperatures. For data 1979-2004, the answer was no. McKittrick et al. 2010 (and Santer et al. 2008) tested a broader question without the condition of surface agreement. Their question was simply, “Do upper air trends of models and observations agree?” (i.e. without the requirement that surface trends agree). Santer et al. used 1979-99, McKittrick et al. used 1979-99 and 1979-2009. Ending in 1999 was a clever way to tilt observations upward, to help them match the models’ warming, due to the massive 1998 El Niño whose impact fades as the time series is lengthened to 2004 and 2009. Even on this more general question, McKittrick et al. 2010 found the answer to be no, i.e. models and observations do not agree, and noted the difference in methodologies in their Supplementary Note 5.]

In my comments to the EPA on this issue I knew the agency would rely on the “establishment” in IPCC-like fashion to write its response, giving their hand-picked “authors” control of the process. So I included the flowing paragraph:

Warning: The EPA will be tempted to rely on scientists/appointees who are well-entrenched into a particular view of the issue of global warming

to review documents such as this, and who will (a) develop clever sounding rebuttals, and (b) are afforded the luxury of the “last word” to protect the current EPA consensus. Basic scientific inquiry should encourage EPA to listen to those of us who actually build these datasets (from scratch) as our message has equal if not greater credibility.

This plea to be objective and avoid an IPCC-like process (i.e. relying on hand-picked authors to give the last word) was to no avail. Again, this demonstrates that consensus reports like the IPCC and EPA can be resistant to dissenting scientific information in a science that is already murky. In this last case, not only were policymakers misled by the EPA’s consensus document, but the promised expensive regulations that are to follow must be viewed as being based, at least in part, on misleading or flawed information. This situation occurs when an institution follows a process that accords authors with veto-oversight of scientific information, who hold one type of perspective, and who are given total control over the output in a field plagued by uncertainty.

There are other examples of the shortcomings of the assessment process (see for example, McKittrick’s Appendix B.2 and my Appendix C.1, C.2 and C.3b), but these above are sufficient to show the problems with the process of generating consensus documents.

Before providing concluding remarks I will briefly address an issue requested by the committee regarding surface temperature datasets.

## 2. Temperature data sets

I have built temperature data sets for climate studies from satellite microwave sensors, balloon soundings, and traditional surface thermometers. My research as well as my experience as State Climatologist exposed me to problems with traditional surface measurements and led me to establish a new network of stations in Alabama with high quality, modernized instrumentation. However, these older stations provide the bulk of the measurements that are the basis of the popular surface temperature datasets today. My studies (and many others) have shown that popular land-surface temperature measurements are affected by many influences, most of them causing warming, which are unrelated to greenhouse gas increases (Christy 2002, Christy et al. 2006.) This is especially true for the daily low temperature which is utilized in the popular surface temperature datasets today (Christy et al. 2006, 2009.) As a result, these measurements, as used, are not adequate to detect what might be happening to the global climate as a result of greenhouse gas increases. (This is also related to the contamination issue raised by McKittrick described above and in my Appendix C.3.2.)

Two of the major problems with the traditional datasets today are determining the provenance of the raw data and reproducing the methodology that created the processed temperature products used in assessments. In the past, raw data were often held close to the product-producer and so results were difficult to independently investigate. “Just trust me” seemed to be the basis for acceptance by the IPCC.

There is an effort underway to create a data bank for surface temperatures that will be open and transparent, with the capability to trace the data to the original sources. From a data bank that is this comprehensive, many useful applications can be created (addressing not just climate change) with the full traceability of the product – from its original measurement with site photographs, to the final adjustments. In this way, for example, methods designed to deal with the contamination issues described above can be better studied and addressed by the community. Much of the effort of this project is led out of the National Climatic Data Center in Asheville NC.

Bulk atmospheric temperatures measured by satellites and balloons, from the surface to 35,000 ft., form a more robust parameter than surface measurements for detecting changes that might be caused by the enhanced greenhouse effect. These temperatures are also affected by transient events, like volcanoes, that tend to confuse the detection of what these extra greenhouse gases are doing to the climate. As described in my recent testimony, when these extraneous features are removed from the global bulk atmospheric temperatures, we find a rising temperature trend since 1979 that is significantly lower than what is being predicted from climate models as they try to quantify the effect of those greenhouse gases. To me, this demonstrates that the real atmosphere is not as sensitive to greenhouse gases as the climate models suggest.

### 3. Concluding remarks

While there are many examples of problems with the process of producing climate change assessments, I am not suggesting everything in these assessments is wrong. The point I raise here is that the *process* by which these assessments were created, whether intended or not, did not provide an expression of the full range of scientific information (and in some cases provided incorrect information) for some key conclusions. These conclusions were then adopted without question by regulatory authorities such as the U.S. EPA. These suspect conclusions include but are not limited to, (a) the notion that the popular surface temperature datasets can serve as a detection variable of the impact of enhanced greenhouse-gas concentrations (and that it is accurately measured), (b) the belief that climate models have precisely replicated natural, unforced variability (so natural variations can be ruled out as the cause for changes that occur), and (c) an overconfident view of how sensitive the climate is to human forcing.

With the IPCC process to date, we see Lead Authors sitting in judgment of information regarding their very own scientific results and those of their critics. This creates an unhealthy conflict-of-interest situation that unfortunately shortchanges the policymakers. To make well-informed decisions, policymakers depend on receiving the full range of scientific thought and evidence on any issue, especially one as contentious, murky, and as potentially expensive as climate change. The committee should understand that the IPCC presents one version of climate change science generated by an establishment that has evolved to largely reflect a particular point of view. As shown above, this point of view attempts to dismiss information that questions the belief that greenhouse gases are the dominant cause of observed climate change (as represented mainly by a rather poor surface temperature data set) with little effort expended on (a) other explanations for

change such as natural, unforced variability, (b) a critical assessment of the climate change variables utilized (including paleoclimate) or (c) a rigorous assessment of model sensitivity and fidelity to observations.

In my IAC testimony (Appendix A), I indicated that the climate “establishment” is so entrenched now, that our science is in need of “adult supervision.” If a new and independent report is called for, one idea is to use a leadership team composed of non-activists that includes, (a) physicists who understand that science advances by testing falsifiable hypotheses (and not by accepting popularized, untestable sentiments), (b) research engineers who understand what’s important to the issue at hand and (c) attorneys who understand the meaning of language, admissible evidence, and the legal process of discovery (transparency). With, hopefully, such objective eyes overseeing the process, the result may be much more humble and honest – revealing the lack of confidence and understanding we have on most climate issues, the lack of dramatic events attributable to humans now occurring in the climate, and the resilience of the Earth to human inputs.

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IAC 15 June 2010

Montreal

John R. Christy

Distinguished Professor of Atmospheric Science  
Director, Earth System Science Center  
Alabama State Climatologist  
University of Alabama in Huntsville

IPCC Lead Author: 2001 TAR  
Contributor: 1992 Supplement  
Contributor: 1994 Radiative Forcing of Climate Change  
Key Contributor: 1995 SAR  
Contributing Author: 2007 AR4, WG I and II

NASA Medal for Exceptional Scientific Achievement  
American Meteorological Society Special Award for satellite observations  
Fellow, American Meteorological Society

Mr. Chairman and members of the IAC panel, thank you for inviting me to offer my views on the IPCC process. Five years ago the *New York Times* quoted me saying that an IPCC-like process, "... is the worst way to generate scientific information, except for all the others." (23 Aug 2005) I now think I was a bit too generous.

A fundamental problem with the entire issue here is that climate science is not a classic, experimental science. As an emerging science of a complex, chaotic climate system, it is plagued by uncertainty and ambiguity in both observations and theory. Lacking classic, laboratory results, it easily becomes hostage to opinion, groupthink, arguments-from-authority, overstatement of confidence, and even Hollywood movies. When climate scientists are placed in the limelight because this issue can generate



compelling disaster scenarios, we simply don't want to say, "We just don't know."

I have been a contributor to the IPCC Assessments since 1992 and a Lead Author in the Third Assessment of 2001. Though I had some good things to say about the IPCC, I did respond in 2001 to the US National Academy of Sciences when they solicited information about certain problems (see Appendix A).

At the time, I was more concerned about the product rather than the process. The first objection I raised regarding the Third Assessment was that the fabled Hockey Stick was oversold as an indicator of past climate change. This was well before the critical work of the Wegman Report, National Academy of Sciences, McIntyre's papers and the East Anglia emails. *Indeed, I urge you in the strongest terms to engage Stephen McIntyre in your deliberations at a high level as he has accurately documented specific failures in the IPCC process, some of which I can attest to, as I was there.*

My second objection to the TAR was its overstatement of confidence in model projections.

My role in the Fourth Assessment of 2007 was limited to that of a Contributing Author. This means I submitted recommendations that were dealt with by the Lead Authors who tended to disagree with my published findings. Thus, their views carried the day in the report. In this process, the final result really boils down the opinions of those selected as Lead Authors, a point I will address below.

In March of last year, 8 months before the email fiasco, about 140 former IPCC Lead Authors gathered in Hawaii for a preview of what the Fifth Assessment might tackle. I was the only one there well-known to be essentially outside the IPCC “consensus.” I had come to the conclusion that the IPCC establishment demonstrated a disturbing homogeneity-of-thought regarding the hypothesized but unproven role that greenhouse gases might impose on the climate system. My short talk (Appendix B) and poster (Appendix C) at that meeting last year dealt with three science issues and offered a recommendation. The three issues were (1) the surface temperature record is flawed in many ways, but is flawed in particular as a metric to detect greenhouse-imposed warming, (2) direct tests of the so-called fingerprint of climate model temperature changes versus observations indicated significant differences, failing simple hypothesis tests, and (3) the critical value of climate sensitivity to greenhouse gases was overstated because it had not been properly calculated. All of these were supported by peer-reviewed publications which even now continue to appear.

In my view, the IPCC process had drifted away from allowing authors to serve as Brokers of climate science, in which various views are given attention, to becoming Gatekeepers of climate science in which one view is elevated and promoted. The IPCC Assessment had become a “consensus of those who agreed with the consensus.” Since “consensus” is a political notion, not a scientific notion, a goal of “consensus” in any forum is at its heart a political goal.

My recommendation last year was to include a chapter written by credentialed climate scientists who would provide evidence concerning these heretofore minimized issues, in particular the low sensitivity of the climate system. My assumption at that time was that the IPCC writing process would be the same, i.e. that the Lead Authors of this chapter, as the others, would be given the sacred right of being their own final reviewers to let a new voice be heard. No one at the meeting thought this was a useful suggestion, I believe, because it would allow the expression of reasonable alternatives to claims too entrenched in the message of looming climate disasters promoted with IPCC indulgence.

Since last March, much has happened to expose some of the scientists who dominated the IPCC, whom I call the establishment, as less than transparent, subject to bias, and who suppress alternative views while using the IPCC's perception as a near-sacred document to promote their own opinions. This establishment dominates not only the IPCC but also the review process of the peer-reviewed literature, making it extremely difficult for alternative evidence to even be published now. This happens when your type of science is rather murky to begin with.

In my view, the three fundamental flaws in the current IPCC process are (1) the two-step political filter by which Lead Authors are selected, (2) the review-authority granted the Lead Authors who write the chapters and synthesis reports, and, (3) the very limited word-count available for each topic, which encourages short and overconfident statements about questions that in truth are plainly nasty to deal with.

In February of this year, *Nature* magazine asked me for a brief discussion about the IPCC and a way forward (Appendix D, last page). My main concern there was to define a process that would let the world know that our ignorance of much of the climate system is simply enormous and we have much to do. Mother Nature has a tremendous number of degrees of freedom up her sleeves, many of which we don't even know about or account for.

So, I suggested a living, carefully-managed, wikipedia-style process. Important questions, most of which are already laid out in the IPCC manifest, would be addressed by teams of Lead Authors who would be far less constrained by the word-count rules, and so would allow fuller expression of uncertainty and disagreement – expressions contributed by the specific people who perform whatever research is being discussed. The Lead Authors main task would be to organize and summarize the information on each question, acting strictly as Brokers, not Gatekeepers. With web-based links to actual text (and data) the Lead Authors would be far less tempted to be biased. Lead Authors need to know they do not have to agree with the findings they report. I believe such transparency would spur the Lead Authors to be fairer and more humble in their summary comments.

Peer-reviewed research of course would dominate the source material, but other documents – whose source is clearly identified – could contribute to the discussion. I know there would be significant issues of managing such a process, but I believe it would be far better than producing big books every six years that are limited, biased and out-of-date when they are printed. We *are* in the 21<sup>st</sup> Century, and, to the despair of those who find comfort in

absolute answers, there are only continuously evolving levels of understanding (and ignorance) to most of the climate questions being asked. This situation begs for a dynamic assessment process.

The selection of Lead Authors through a two-step political process is a problem too. Presently, national governments nominate to the IPCC those who over the years, they can generally count on to be consistent with national policy. From this pool, the IPCC itself selects those it wants to be Lead Authors. To combat the political influence of governments and the U.N., to a small extent, I would recommend that Lead Authors be nominated by appropriate learned societies, such as yours, and selected for overlapping, rotating terms. I'm not completely comfortable with this as I'm aware that councils of science are deeply involved in political maneuvering which is why I state that to a "small extent" the political influence of governments and the U.N. might be mitigated.

Some Lead Authors could and should be scholars from other disciplines but who have a keen awareness of the hard rules of hypothesis testing, admissible evidence, and the power of language ... physicists, chemists, engineers and yes, even lawyers. As I told a colleague the other day, it is clear to me now that climate science needs some adult supervision.

I realize such a recommendation creates consternation among those who have controlled the process up to now and who believe deeply that the "science is settled" because they find comfort in easy and unimaginative answers to difficult questions. For example, why doesn't the IPCC report on (and funding agencies invest in) major research about the internal

dynamical properties of the climate system? At present these properties are incapably represented in climate models to date, and yet have been shown to be a major source of the variability we've seen. Why must we be so unimaginative that we just give up and claim that nothing else but enhanced greenhouse forcing explains most of the temperature rise in the past 50 years?

Others will complain that such an open process I describe will not generate the definitive statements necessary to drive policy. To those I say, "Welcome to climate science." If a specific policy is desired, climate science is a weak leg on which to stand which means a policy should have multiple, defensible reasons for adoption.

You will hear from those within the IPCC establishment that the IPCC does a terrific job of getting down to the truth about climate science and that the consensus reports are the best documents for policymakers. But as one mostly outside the "consensus", I can not agree, and I am far, far from being alone in that disagreement. I say this as a working-stiff climate scientist who builds datasets from scratch to create understanding and test assertions about the climate system. The process followed in the Fourth Assessment, in my view, simply did not provide to the world the true ambiguities, uncertainties and contentions of our fledgling science.

In summary, to me, the impediments to providing a more honest expression of our science to the world in the current IPCC process are (1) Lead Authors essentially having final review authority, (2) the Lead Author selection process which encourages government-approved, homogeneity-of-thought,

and (3) the limited size, the dead-line character, and the past-expiration-date of printed documents. Thank you.

## Appendix A

Response to National Research Council for IPCC TAR Comments  
John R. Christy

21 May 2001

Vaughan:

I suspect I will have a slightly different view on the SPM, TS and TAR Text of the IPCC than most other participants on the list.

~~I believe the IPCC effort was a good effort, but not a perfect effort. Attached is the text of an op-ed piece I was asked to write by the Atlanta Journal-Constitution who stated to me they (the paper) felt manipulated by the aggressive media push of certain of the IPCC leaders. The op-ed appeared on 11 March 2001. Note that in general I thought the IPCC body of work (main text) was fine, but that the media reports were not.~~

Here are three issues that specifically concern me regarding your message.

1. 1000-year temperature record

This first concern arises from our chapter (2) for which I must accept as much blame as anyone. We (chapter 2 authors) are guilty of omitting information that indicated the temperature history of the past 1000 years is not as well known as is implied by the prominent figure in the SPM (Fig. 1) and TS (Fig.5). At each of the Lead Authors meetings I pointed out that we should include mention of publications which strongly suggest the medieval warm period was warmer than the current century. In particular I mentioned the Dahl-Jensen et al. 1998 Science paper which I believe presents the most direct measurement of temperature and thus should be highlighted. Broecker (2001, SCIENCE) echoed the very concerns I had put forward in our meetings. In the final version of the text the Dahl-Jensen paper was not even cited in Section 2.3 - a fact I did not realize until last week when I read the report in detail (2.3 is the section on the temperature record of the past 1000 years.) Thus, its information was not carried forward in the TS or SPM. (The paper is only mentioned in passing regarding the warming 8 kybp in the TAR.) I should point out that the final wording concerning the warmth of the 1990's and 1998 as "likely" the warmest of the past millennium (i.e. only 2/3 chance of being correct) tried to account for the lack of certainty in our knowledge of past temperatures. However, the very prominent placement of the time series of the last 1000 years in the TS and SPM overrules what tentativeness some of us actually intended. This is my personal view.

2. Model confidence

Secondly, I view the whole modeling effort with more skepticism than most, perhaps because I do not receive funding to produce model results. Each global modeling group has had 20 years to look at the global surface temperature record and devise clever ways to reproduce what is in the record. This is "a posteriori" science in my view. No one has from first principles actually reproduced the record. The sulfate hypothesis is highly uncertain (as indicated by the IPCC itself) yet has become a critical component of modeling efforts in order to hold down the unrealistic temperature rise most models produce for the past century. Too, models have not reproduced the observed surface-tropospheric differential temperature trends (especially in the tropics), yet now are trying to do so. I'm somewhat confident that a model result will appear soon that announces a reproduction of the differential trend observations - but will it be based on correct physics? Modelers are



working to reproduce observations, and when a match is finally constructed, the insinuation is that the models are successful. In my view, this procedure is not a scientific success as much as an exercise in curve-fitting. Do we know whether the "match" is correct for the right reasons? I generally am comforted by the many references to uncertainty that the TAR contains. The magnitudes of those uncertainties do not convince me that the "science is settled" as several IPCC authors have stated (please define what "science" is settled!) or that we know what policy road to take.

### 3. SPM representation of surface/troposphere issue

Though I was the Lead Author of the discussion of the upper air temperature data, I was not able to influence a few phrases and statements in the SPM which appeared in the final version. For example, the following is a bullet from the SPM:

The lowest 8 kilometres of the atmosphere and the surface are influenced differently by factors such as stratospheric ozone depletion, atmospheric aerosols, and the El Nino phenomenon. ... In addition, spatial sampling techniques can also explain some of the differences in trends, but these differences are not fully resolved.

I do not believe these explanations have been objectively demonstrated in terms of explaining the trend differences between the surface and troposphere. Nearly all of the discrepancy in trends is found in the tropics. However, in the tropics there is the least amount of ozone depletion (some tropical regions actually show no decrease or a slight increase). And, the stratosphere (16 km and above) is separated by an 8 km layer from the lower troposphere (0 - 8 km), thus little influence would be expected. Too, examination of individual tropical sondes (which have maintained consistent instrumentation) shows the lower troposphere (850-500 hPa) has cooled relative to the upper troposphere. Thus, ozone depletion does not rise to more than speculation as a cause for the trend differences.

The aerosol effect is as yet an unproven hypothesis, and it is unclear that it has much influence at all in the tropics - again speculation. Michaels and Knappenberger (2000) have shown that the El Nino phenomenon has actually influenced the lower troposphere to warm relative to the surface, thanks to the 1997-98 event, not cool as suggested by the SPM above.

The statement that "spatial sampling techniques can also explain some of the differences" is less than fully accurate. The tropospheric data are fully global, thus spatial sampling errors apply to surface temperatures only. However, left as it is in this section the insinuation could be that the tropospheric data are suspect. The most substantive statement in this bullet is the last phrase, "... but these differences are not fully resolved."

In summary, my personal view is that there is a "spin" placed on some of the statements that "leads the witness" toward a conclusion that is not entirely justified. I found this also in many of my discussions with authors from the other chapters. I had a feeling of discomfort in trying to express a view that would diminish the human-related climate paradigm.

Overall, the interactions among the Lead Authors in Chapter 2 were quite open and congenial, and we produced a good document (now outdated a bit) but not a perfect document.

John C.

Vaughan Turekian wrote:

As you may know, the National Academies is conducting a fast-track study to examine a number of key questions about the science of climate change. As part of this study the committee requires information regarding the IPCC WG I report and summary preparation process. Owing your involvement in the IPCC WG I process, you may be able to provide some needed insight. Specifically, do you feel that the WG I SPM and the TS accurately reflected the information in the main body of the WG I report? Were there any instances where the WG I SPM (or the TS) did not accurately convey the information in the WG I report, or do you know of any situation where the body of the WG I report was altered to justify statements in the SPM or the TS?

Please note that any written response to these questions will be included in the study's public access file. If you would prefer to discuss this by phone, please provide contact information.

I thank you in advance for your help on this and look forward to your input. If you have any questions please do not hesitate to contact me.

Vaughan

Vaughan C. Turekian, Ph.D. Program Officer Board on Atmospheric Sciences and Climate  
The National Academies 202-334-3512 vtureka@nas.edu

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John R. Christy  
Director, Earth System Science Center voice: 256-961-7763  
Professor, Atmospheric Science fax: 256-961-7751  
Alabama State Climatologist  
University of Alabama in Huntsville  
<http://www.atmos.uah.edu/atmos/christy.html>  
Mail: University of Alabama in Huntsville, Huntsville AL 35899 Express: NSSTC/ESSC  
320 Sparkman Dr., Huntsville AL 35805

## **Appendix B**

### **Presentation at IPCC Lead Author's Meeting Honolulu Hawaii**

#### **Can the IPCC Allow a Section of Alternative Views Authored by Equally Credentialed Climate Scientists?**

**John R. Christy  
University of Alabama in Huntsville**

**I want you all to understand this: No one is holding a gun to my head and no one is paying me money either above or under the table to arrive at the conclusions I (and others) have come to. I propose that the IPCC allow for well-credentialed climate scientists to craft a chapter on an alternative view presenting evidence for low climate sensitivity to greenhouse gases than has been the IPCC's recent message – all based on published information.**

**In other words, I am proposing that the AR5 be a true Scientific Assessment, not a document designed for uniformity and consensus. In a scientific area as uncertain as climate, the opinions of all are required.**

**Three quick examples are on the poster.**

**First, the iconic mean surface temperature is a poor proxy for detecting greenhouse gas influences for reasons shown. And, this metric is not well-observed in any case.**

**Secondly, many of the so-called metrics of human-induced climate change are not changing at rates policymakers have assumed and the media promotes with the indulgence of the IPCC Leadership. And,**

**other variables showing change are still within the magnitudes of long-term natural variations.**

**Thirdly, confidence that the climate system is highly sensitive to greenhouse gases can be shown to be overstated due to assumptions about how the sensitivity is calculated. Latest measurements clearly suggest a strong negative feedback in the short wave – in other words, in warming episodes, clouds respond to cool the climate. Another problem with popular sensitivity estimates is the dependence on essentially one century of an oblique greenhouse-proxy (mean surface temperature) combined with the notion that all of the natural, multi-decadal variability can be defined so accurately that the left-over warming is assumed to be human-induced. The investigation rather should examine all levels of natural variability that have been observed and seek to defensibly eliminate those as possible causes.**

**An alternative view is necessary, one that is not censured for the so-called purpose of consensus. This will present to our policymakers an honest picture of scientific discourse and process. I submit this proposal because our level of ignorance of the climate system is still enormous and our policymakers need to know that. We have much work to do.**

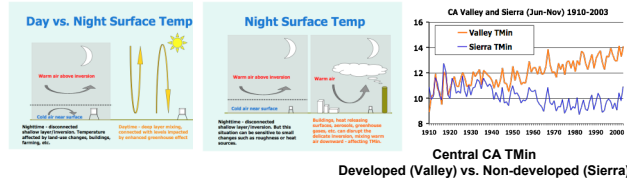
# An Alternative View Proposal for IPCC AR5

John R. Christy, University of Alabama in Huntsville

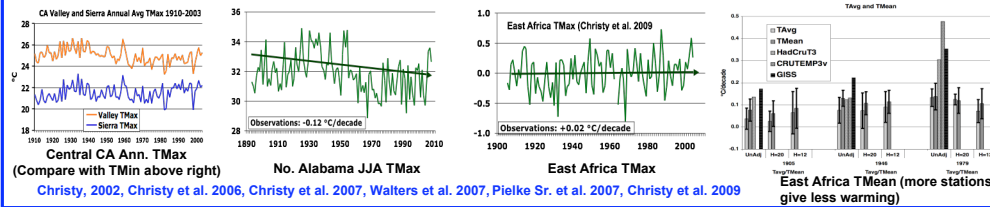
ChristyJR Appendix A

## Mean Surface Temperature: a Poor Metric for measuring response of climate to enhanced GHGs

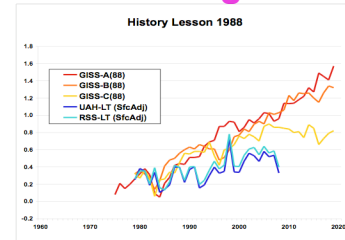
$T_{Mean} = (T_{Max} + T_{Min})/2$ .  $T_{Min}$  is heavily influenced by surface development and changing atmospheric constituents over time. The thermal radiation budget and boundary-layer mixing altered by these changes, introduce higher temperatures.  $T_{Max}$ , though not perfect, is better since its spatial mixing scale is much larger.



"Super-sampled" regions generate  $T_{Max}$  temperature trends near zero while under-sampled methods using  $T_{Mean}$  (e.g. GISS, HadCRUT, NCDC) do not. Thus  $T_{Mean}$  overstates the warming rate by (1) using  $T_{Min}$  and (2) using too few stations.

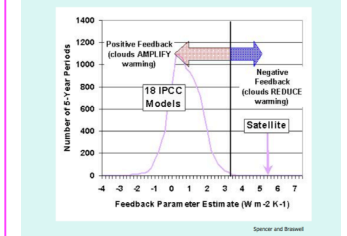


## Climate sensitivity to CO2 Forcing too high in Climate Models



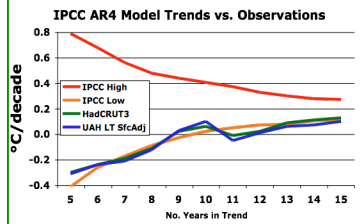
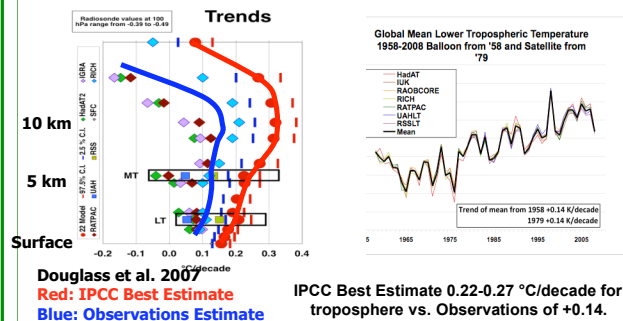
(Left) Hansen projected 3 scenarios in 1988, 2 of which (red, orange) had slightly lower GHG emissions than actually observed over the next 20 years, and one with drastically lower emissions (yellow). The climate sensitivity of the model was so high that all three scenarios, even the one with sharp cuts in emissions, significantly overshoot the observations (lower tropospheric temperatures adjusted for surface comparisons, CCSP 1.1 2006)

(Right) Longwave (LW) and shortwave (SW) feedback parameters ( $W/m^2/K$ ) were calculated for all 5-year periods from 18 IPCC AR4 transient simulations, and also from 5 years of Aqua CERES data. The satellite diagnosis indicated positive LW feedback, right in the middle of the model distribution of similarly computed feedbacks. But the observed reflected solar SW feedback was strongly negative, well outside the range of all 5-year SW feedbacks computed from the models. The total feedback parameter (seen here) is then the sum of both (LW+SW) individual parameters, which is also outside the range of all total feedbacks computed from the models.

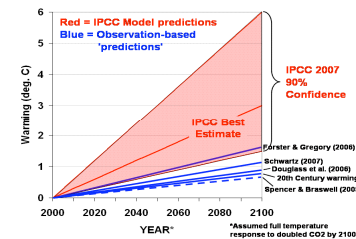


## Modeled climate change temperatures inconsistent with observed changes

When climate models generate the same tropical surface trend value as the observed tropical sfc temperature trend (below left), their upper air trends are significantly different from observations where GHG signal is largest. Global tropospheric temperature trends of the IPCC mid-range estimate (below right) are significantly higher than the mean of observations from seven sources.



Projections of 21 A1B IPCC Climate models' global trends for segment lengths shown (ending in model year 2020 and observation year 2008, HadCRUT3 and UAH LT - sfcAdj). Models' 95% range bounded by red (high) and orange (low). Results show observations are well below the "best estimate" ( $+0.20$ °C/decade) and along the edge of the "significantly different" region. Adapted from P. Michaels.



(Left) The range of solutions from the IPCC AR4 climate model simulations (pink). Empirically calculated model projections and current observed trend (blue) which by implication factor in the negative feedbacks of cloud responses. The rate of warming in these empirical models is much lower than the full blown coupled models.

Summary: An Alternative View Section written by well-credentialed climate scientists is needed in the IPCC AR5

If not, why not? What is there to fear?



## OPINION

ChristyJR Appendix A

# IPCC: cherish it, tweak it or scrap it?

As calls for reform intensify following recent furores about e-mails, conflicts of interest, glaciers and extreme weather, five climatologists propose ways forward for the Intergovernmental Panel on Climate Change. Their suggestions range from reaffirming the panel's governing principles to increasing the number and speed of its publications to replacing the volunteer organization with a permanently staffed structure.

## Split into three panels

**Mike Hulme**

Coordinating lead author, lead author, review editor (AR3), University of East Anglia, Norwich, UK

Much has changed since the late 1980s when the Intergovernmental Panel on Climate Change (IPCC) was designed, notably the nature of scientific practice and its relationship with society. How the world's knowledge communities are mobilized to enlighten policy deliberations also needs to be different. The assessments published by the IPCC have firmly elevated anthropogenic climate change to one of the major international political issues of our time. But they have made this impact by drawing in an ever-widening subset of the social, technological, environmental and ethical dimensions of climate change — well beyond the physical sciences.

The IPCC is no longer fit for purpose. It is not feasible for one panel under sole ownership — that of the world's governments, but operating under the delegated management of the United Nations Environment Programme (UNEP) and the World Meteorological Organization (WMO) — to deliver an exhaustive 'integrated' assessment of all relevant climate-change knowledge. As I remarked three years ago in these pages, "The IPCC needs a complete overhaul. The structure and process are past their sell-by dates."

My suggestion for radical reform is to dissolve the IPCC after the Fifth Assessment Report (AR5) in 2014. The work would be split into three types of assessment and evaluation, each rather different to the three existing IPCC working groups.

The first would be a Global Science Panel (GSP). An IPCC-like assessment process should continue to operate for the physical sciences that observe and predict the Earth system. Rather



STR/AFP/GETTY

An IPCC meeting: the panel will publish its Fifth Assessment Report (AR5), in 2014.

than comprehensive reports every six years, this panel would commission, on a rolling basis, a larger number of smaller, sharply focused syntheses of knowledge on fast-moving topics that have great scientific or policy salience. Perhaps two or three would be in production at any one time and each would be no more than 50 pages in length. These would need to be globally coordinated and could be governed either through an intergovernmental process as now, or devolved to a governing council of representative national academies of science.

The second group would be made up of Regional Evaluation Panels (REPs). The cultural, social, economic and development dimensions of climate change are essentially regional in nature. Each region — five to ten continental or sub-continental regions in all — should conduct its own evaluation of relevant knowledge. This should use the work of the GSP, but also draw in a much more diverse set of expertise, knowledge and scholarship. As well as being structured according to the concerns of

each region, the ownership and governance patterns of these REPs would vary regionally, but should ideally involve a consortium of national governments, civil-society organizations and businesses.

The third group would be the Policy Analysis Panel (PAP) — a standing panel of expertise, global in reach, with interdisciplinary skills and a diverse analytical capacity. Perhaps 50–100 strong, this panel would undertake focused and rapid (6–12 months) analyses of specific proposed policy options and measures that have global significance. These could be subjects such as environmental effectiveness of controlling black carbon, economic implications of carbon border tariffs or new financing options for reducing emissions from deforestation. The policy options to be analysed can be brought forward by UN bodies, non-governmental organizations (NGOs), businesses and groupings of national governments. The PAP could be governed by a council of women and men of international stature and strong cultural significance to represent the breadth of civil society around the world. Such high quality and transparent policy evaluation would broaden the options available

**"A new class of short, rapidly prepared, peer-reviewed reports is needed."**

ChristyJR Appendix A

for national and international deliberations.

This restructuring would allow clearer distinctions to be made in areas that have been troublesome for the IPCC: assessments of published knowledge versus policy analysis and evaluation; the globalized physical sciences versus more geographically and culturally nuanced knowledge; a one-size, top-down model of ownership and governance versus more inclusive, representative and regionally varying forms of governance. It would better serve the world, and its peoples, in understanding and responding to anthropogenic climate change.

## Independent agency needed

**Eduardo Zorita**

Contributing author (AR4), GKSS Research Center in Geesthacht, Germany

Like the financial sector last year, the IPCC is currently experiencing a failure of trust that reveals flaws in its structure. This presents the climate-change community with the opportunity to address these faults. The IPCC currently performs as a diffuse community of government-nominated academic volunteers occupying a blurred space between science and politics, issuing self-reviewed reports under great stresses and unmanageable deadlines. Its undefined structure puts it at the mercy of pressure from advocates.

The IPCC should be made stronger and independent. We do not need to reinvent the wheel; there are excellent examples of agencies that society has set up when credibility is of the utmost importance. The European Central Bank, the International Atomic Energy Agency (IAEA), the International Energy Agency and the US Congressional Budget Office all independently navigate their way through strong political pressures, delivering valuable assessments, advice, reports and forecasts, tapping academic research when necessary. These agencies are accountable and respected.

An international climate agency (ICA) along such lines would have a staff of around 200 full-time scientists who would be independent of government, industry and academia. Such an agency should be resourced and empowered to do the following: issue streamlined biennial state-of-the-climate reports; be a repository and quality-controller of observational climate data; advise governments on regional assess-

ments of climate impacts; and coordinate the suite of future-climate simulations by research institutes.

An ICA could be built, for instance, on the IAEA template, encompassing many more countries than the IAEA but with a smaller staff. ICA reports should be independently reviewed in a transparent process, draw only on established, peer-reviewed literature, and highlight research gaps. External reviews would then be incorporated into the reports to form white papers to include possible opposing views in a transparent way.

The process of moving towards such an ICA could start now, alongside the preparation of the next IPCC assessment report, and culminate after its completion. Those climate researchers in the IPCC Bureau who have widely recognized credibility could initiate this transformation, supported by lead authors and review editors more numerous and with a bigger say than presently. These review editors should be elected not by governments but directly by scientific unions, for instance the American Geophysical Union, the European Geosciences Union and similar associations from Asia.

As with finance, climate assessment is too important to be left in the hands of advocates.

## Apply best practice rules

**Thomas F. Stocker**

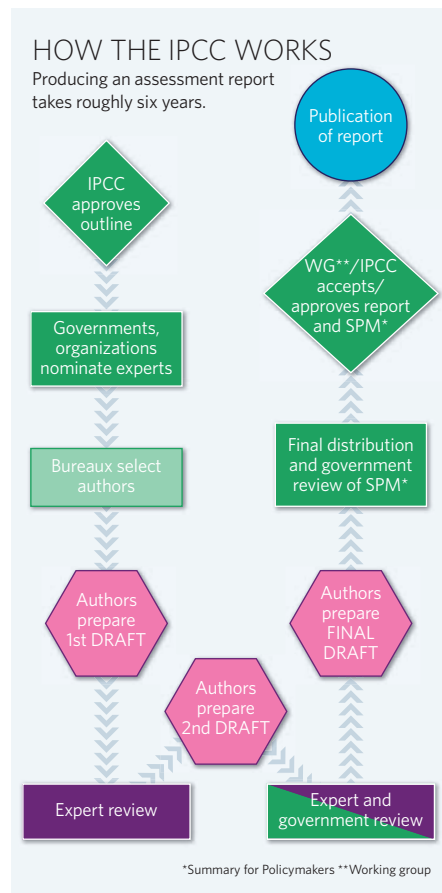
Co-chair IPCC Working Group I (AR5), coordinating lead author (AR3, AR4), University of Bern, Switzerland

The basis of the IPCC is the voluntary contributions of thousands of dedicated scientists from all over the world. The *Principles Governing IPCC Work* (IPCC, 1998) provide a clear framework for an open, transparent and robust process. This bottom-up endeavour is a unique model of providing scientific information, mainly from the peer-reviewed scientific literature, for decision-making on a challenging problem. It has worked extremely successfully for the past 21 years.

Recent controversies have demonstrated both the value and the limitations of these procedures. The team structure of the chapter authors, the multiple reviews by peers and governments, and the full and public documentation of this process largely eliminate personal views or biases in the science assessment. But procedures are only as strong as their enforcement at all levels of the assessment process. When I served as a coordinating lead author of Working Group I in the Third and Fourth Assessment Reports (AR3 and AR4), I was deeply impressed by the strict adherence to these principles by the co-chairs who ensured that these standards were applied at all levels. The combination of the best scientists and clear procedures constitute the authority of the IPCC.

Calls for reform of the IPCC have been made before. Changes were discussed after the completion of the Fourth Assessment Report in 2007. One possibility mooted was the production of more frequent assessments, more limited in scope. Fast-track assessments in support of the United Nations Framework Convention on Climate Change process were also considered. However, the panel concluded that the production of comprehensive reports roughly every six years is preferable because it ensures the robustness required for a thorough and rigorous assessment. Faster turnover would jeopardize the multi-stage review and thus compromise authority and comprehensiveness. In asking scientists to produce reports and assessments every year, say, we could lose their support rather quickly.

The IPCC has served as an honest broker in the past and will do so, hopefully, in the future. Now that the problem of climate change is on the



SOURCE: IPCC



## ChristyJR Appendix A

radar screen of the world, there are many NGOs and other groups, even groups of scientists and institutions, that provide climate-change information in various forms and quality, often lacking comprehensiveness and proper recognition of uncertainties. There is a strong pressure to provide 'just-in-time' scientific updates for policy-makers and stakeholders, as was the case in the preparations for the 2009 climate-change conference in Copenhagen. The IPCC must not yield to this pressure.

In this field of different and divergent forces, confusion may arise. An honest broker therefore is an asset. From my perspective, the IPCC has fulfilled this role with remarkable rigour and integrity. This role is now at risk, as the stakes are higher than ever before. The requirement that assessments are policy relevant but never policy prescriptive, as formulated in the *Principles Governing IPCC Work*, is of paramount importance. Our task is to inform the policy-makers and the public strictly in a 'what if' mode. Any other approach must be left to NGOs, negotiators or individuals. Only with strict adherence to procedures and to scientific rigour at all stages will the IPCC continue to provide the best and most robust information that is needed so much.

## Produce more reports faster

### Jeff Price

Lead author (AR3, AR4), director, climate-change adaptation, WWF United States

The IPCC is accepting nominations (until 12 March 2010) from governments and participating organizations for authors for its Fifth Assessment Report. One recommendation for the IPCC that could be implemented immediately is in how its coordinating lead authors and review editors are selected.

Currently, authors are selected to represent "a range of views, expertise, gender and geographical representation". However, given the importance placed on these assessments, the most senior positions should be filled by the nominees most expert in their field, regardless of balance. These authors should be the most knowledgeable nominee about the range of topics in their chapter, best able to cooperatively work with a team of international scholars. Preferably, they should have previously been involved in an IPCC assessment and be familiar with IPCC standards and methodologies. Geographic and gender balance should then

be used in selection of lead authors. The level of work required in preparing an assessment is large. Increasing the number of lead authors would provide better balance and give more scientists the ability to participate in the process.

A new class of short, rapidly prepared, peer-reviewed reports is also needed. At present, publication options include supplemental material (no peer review required), technical papers (based on existing assessments) or assessments and special reports that undergo two reviews (expert and government/expert, usually taking more than two years to complete). For topics of emerging importance or uncertainty, we need reports based on expert meetings and literature synthesis that undergo only a single round of extensive peer review with review-editor oversight before publication. The IPCC should also expand the number of specialist task forces, task groups and hold more expert meetings to provide additional scientific review and oversight for the broadening array of models (including model comparisons and validation) and methodologies used in emissions reporting, estimating and monitoring impacts, and in developing assessments and adaptation plans.

Finally, the current period between assessments is too long. One option would be for the IPCC, or another body, to produce an annual review, assessment and synthesis of the literature for policy-makers (for example, three annual review volumes with a synthesis chapter in each volume) prepared by experts in the field. Although the editors of the volumes should ideally be drawn from past IPCC authors and editors, the review articles could be submitted by any author, as they would for a journal, with appropriate peer review and assessment for publication.

## Open debate: Wikipedia-style

### John R. Christy

Lead author (AR3), University of Alabama in Huntsville, USA

Since 1992 I have served as an IPCC contributor and in 2001, as a lead author. My experience has left me of the firm conviction that the IPCC should be removed from UN oversight.

The IPCC selects lead authors from the pool of those nominated by individual governments. Over time, many governments nominated only authors who were aligned with stated policy. Indeed, the selections for the IPCC Fourth

Assessment Report represented a disturbing homogeneity of thought regarding humans and climate.

Selected lead authors have the last word in the review cycle and so control the message, often ignoring or marginalizing dissenting comments. 'Consensus' and manufactured-confidence ensued. The recent leaking of e-mails from the Climatic Research Unit at the University of East Anglia in Norwich, UK, put on display the unsavoury cycle of marginalizing different viewpoints. Now several errors of overstatement, such as that of the melting rate of the Himalayan glaciers, have been exposed.

Unfortunately, prestigious media, including *Nature*, became cheerleaders for these official reports, followed then by governments trying to enact policies that drastically reduced emissions to 'stop global warming' while increasing energy costs.

I recommended last year that the next IPCC report invites published authors to write about the evidence for low climate sensitivity and other issues. The IPCC then would be a true reflection of the heterogeneity of scientific views, an 'honest broker', rather than an echo chamber. My recommendation assumed a business-as-usual IPCC process.

However, voluminous printed reports, issued every six years by government-nominated authors, cannot accommodate the rapid and chaotic development of scientific information today. An idea we pitched a few years ago that is now worth reviving was to establish a living, 'Wikipedia-IPCC'. Groups of four to eight lead authors, chosen by learned societies, would serve in rotating, overlapping three-year terms to manage sections organized by science and policy questions (similar to the Fourth Assessment Report). The authors would strike a balance between the free-for-all of true science and the need for summary statements.

Controversies would be refereed by the lead authors, but with input from all sides in the text, with links to original documents and data. The result would be more useful than occasional big books and would be a more honest representation of what our fledgling science can offer. Defining and following rules for this idea would be agonizing, but would provide greater openness.

The truth, and this is frustrating for policy-makers, is that scientists' ignorance of the climate system is enormous. There is still much messy, contentious, snail-paced and now, hopefully, transparent work to do. ■

See also **Perspectives**, page 747.

Have your say on the future of the IPCC at [go.nature.com/orzWau](http://go.nature.com/orzWau).





DEPARTMENT OF ECONOMICS  
College of Management and Economics  
University of Guelph  
Guelph Ontario, Canada N1G 2M5  
(519) 824-4120 Ext. 52532  
<http://www.uoguelph.ca/~rmckitri/ross.html>  
rmckitri@uoguelph.ca

Ross McKittrick, Ph.D.  
Professor

March 9, 2011

To: Rep. Ed Whitfield, Chair Energy and Power Subcommittee  
cc: Rep. John Sullivan, Vice Chair Energy and Power Subcommittee  
Rep. Fred Upton, Chair Energy and Commerce Committee  
Rep. Joe Barton, Chairman Emeritus Energy and Commerce Committee  
Rep. Steve Scalise  
Rep. Morgan Griffith

**Re. Technical Problems with the EPA Endangerment Finding**

Dear Mr. Whitfield

I understand your committee is considering legislation to limit the EPA's ability to regulate greenhouse gases. I believe that the review process leading to the EPA Endangerment Finding was flawed, and I am writing to provide information that may be pertinent to your deliberations.

In its Proposed Endangerment Finding of April 2009 (74 FR 18886) regarding greenhouse gases, the Environmental Protection Agency stated that it relied primarily on the work of the Intergovernmental Panel on Climate Change, and the US Climate Change Science Program:

The [EPA] therefore relies most heavily on the major assessment reports of both the Intergovernmental Panel on Climate Change (IPCC) and the U.S. Climate Change Science Program (CCSP). EPA took this approach rather than conducting a new assessment of the scientific literature. The IPCC and CCSP assessments base their findings on the large body of many individual, peer reviewed studies in the literature, and then the IPCC and CCSP assessments themselves go through a transparent peer review process.  
(EPA p. 46)

Likewise in the December 2010 version of the Endangerment Finding the EPA reiterates its reliance on IPCC Reports:<sup>1</sup>

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<sup>1</sup> [http://www.epa.gov/climatechange/endangerment/downloads/Federal\\_Register-EPA-HQ-OAR-2009-0171-Dec.15-09.pdf](http://www.epa.gov/climatechange/endangerment/downloads/Federal_Register-EPA-HQ-OAR-2009-0171-Dec.15-09.pdf)

However, the Administrator is relying on the major assessments of the USGCRP, IPCC, and NRC as the primary scientific and technical basis of her endangerment decision for a number of reasons. (FR 74 page 66510)

The EPA Administrator claims the material therein is subject to a review process even more rigorous than that for academic journals.

Fourth, these assessment reports undergo a rigorous and exacting standard of peer review by the expert community, as well as rigorous levels of U.S. government review and acceptance. Individual studies that appear in scientific journals, even if peer reviewed, do not go through as many review stages, nor are they reviewed and commented on by as many scientists. The review processes of the IPCC, USGCRP, and NRC (explained in fuller detail in the TSD and the Response to Comments document, Volume 1) provide EPA with strong assurance that this material has been well vetted by both the climate change research community and by the U.S. government.

(FR 74 page 66511).

I was an expert reviewer for the IPCC Fourth Assessment Report. I explain herein two incidents that show the EPA's views of the IPCC process to be, at best, naïve. One concerns insertion of apparently fabricated evidence within the IPCC report regarding the quality of the surface temperature data, and another concerns deletion of peer-reviewed evidence about the uncertainty of global warming trends. In both cases the IPCC review process was subverted by making the text changes outside the expert review process. I submitted information on both items<sup>2</sup> to the EPA in response to the Advanced Notice of Proposed Rulemaking (a-and-rDocket@epa.gov, November 24, 2008) but, as I will show, the EPA failed to respond adequately to either matter.

To preface, the EPA reliance on the IPCC has already been put into question by the findings in the Inter-Academy Council's (IAC) August 2010 Report.<sup>3</sup> Human health and welfare impacts of climate change are discussed in the Working Group II volume of the IPCC Report, which has been widely discredited due to its extensive reliance on non peer-reviewed literature and its unsubstantiated conclusions. The IAC noted:

The Working Group II Summary for Policy Makers in the Fourth Assessment Report contains many vague statements of "high confidence" that are not supported sufficiently in the literature, not put into perspective, or are difficult to refute.  
(IAC p. 37)

And

Many of the 71 conclusions in the "Current Knowledge about Future Impacts" section of the Working Group II Summary for Policy Makers are imprecise statements made without reference to the time period under consideration or to a climate scenario under which the conclusions would be true....In the Committee's view, assigning probabilities to imprecise statements is not an appropriate way to characterize uncertainty. If the confidence scale is used in this way, conclusions will likely be stated so vaguely as to make them impossible to refute, and therefore statements of "very high confidence" will have little substantive value.  
(IAC pp. 33-34).

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<sup>2</sup> My submission is online at <http://rossmckitrick.weebly.com/uploads/4/8/0/8/4808045/epa-anprsubmission.pdf>.

<sup>3</sup> <http://reviewipcc.interacademycouncil.net/report.html>

My comments pertain to Working Group I, for which I served as an expert reviewer.

### **1. Reliance on Apparently Fabricated Evidence Concerning Problems in Surface Temperature Data Contamination**

The EPA relied on conclusions from IPCC modeling work as the basis of its scientific findings. They stated (April 2009 document, p. 59):

Most of the observed increase in global average temperatures since the mid-20th century is very likely due to the observed increase in anthropogenic greenhouse gas concentrations. Global observed temperatures over the last century can be reproduced only when model simulations include both natural and anthropogenic forcings, that is, simulations that remove anthropogenic forcings are unable to reproduce observed temperature changes.

This statement pre-supposes that there are no biases or contamination problems in the surface temperature record. In the April 2009 Technical Support Document (TSD) accompanying the Endangerment Finding, the EPA dismissed evidence of problems in the surface temperature record as follows (p. 22):

Biases may exist in surface temperatures due to changes in station exposure and instrumentation over land, or changes in measurement techniques by ships and buoys in the ocean. It is likely that these biases are largely random and therefore cancel out over large regions such as the globe or tropics (Wigley et al., 2006). Likewise, urban heat island effects are real but local, and have not biased the large-scale trends (Trenberth et al., 2007).

Wigley et al. (2006) is a reference to the 2006 Climate Change Science Program Report “Temperature Trends in the Lower Atmosphere: Steps for Understanding and Reconciling Differences” which did not address evidence of problems in surface temperature records in detail. Trenberth et al. (2007) is a reference to Chapter 3 of the 2007 IPCC Report. At the time of the report’s preparation, evidence had been published by two independent teams (of which I was a coauthor on one) in high-quality peer-reviewed journals<sup>4</sup> showing statistically significant evidence that contamination in the surface temperature record due to industrialization and related land-use effects had not been adequately removed from climatic data sets and it added a clear warming bias.

One of the Climategate emails is from IPCC Author Phil Jones to his colleague Michael Mann on July 8 2004, in which Jones confides that he and IPCC coauthor (Kevin) Trenberth were determined to keep this evidence out of the IPCC Report:

“The other paper by MM is just garbage. [...] I can't see either of these papers being in the next IPCC report. Kevin [Trenberth] and I will keep them out somehow — even if we have to redefine what the peer-review literature is!”

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<sup>4</sup> De Laat, A.T.J., and A.N. Maurellis (2004), Industrial CO<sub>2</sub> emissions as a proxy for anthropogenic influence on lower tropospheric temperature trends, *Geophys. Res. Lett.* Vol. 31, L05204, doi:10.1029/2003GL019024. McKittrick, R.R. and P. J. Michaels (2004), A test of corrections for extraneous signals in gridded surface temperature data, *Climate Research* 26(2) pp. 159-173, Erratum, *Clim. Res.* 27(3) 265—268. De Laat, A.T.J., and A.N. Maurellis (2006), Evidence for influence of anthropogenic surface processes on lower tropospheric and surface temperature trends, *Int. J. Climatol.* 26:897—913.

Consistent with that plan, all mention of the studies in question were kept out of drafts shown to reviewers. Then after the close of expert review a paragraph was inserted into the IPCC chapter that misrepresented the findings in the publications and made empirical claims with no supporting evidence:

McKittrick 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.

(IPCC 2007 Chapter 3 page 244, cited by the EPA as Trenberth et al. 2007, emphasis added).

The first highlighted sentence is false: neither of the cited report sections address the topic or show any information on the spatial pattern of industrialization or its overlap with the warming record. The second highlighted sentence is a fabrication. Both papers reported statistically *significant* correlations between warming patterns and the spatial distribution of industrialization; neither one offered any evidence that these results could be attributed to natural atmospheric circulation changes, nor does the IPCC present any such evidence, nor does any such evidence exist. In a 2010 paper, published in a peer-reviewed statistics journal, I specifically tested the IPCC's conjecture and showed it to be untrue:

- McKittrick, Ross R. (2010) "Atmospheric Oscillations do not Explain the Temperature-Industrialization Correlation." *Statistics, Politics and Policy*, Vol 1 No. 1, July 2010

I cited a preliminary copy of this paper to the EPA in my comment on the ANPR. Consequently, in this regard, the EPA's conclusions regarding the integrity of the surface temperature record can be shown to depend entirely on IPCC material that was fabricated and which was kept out of drafts shown to peer-reviewers—something which the Climategate emails showed not to have been inadvertent.

The EPA relied verbatim on the IPCC fabrication quoted above in its dismissal of comments on the Endangerment finding:

Commenters also point to recent papers (e.g., McKittrick and Michaels, 2007; de Laat and Maurellis, 2006) that attempt to demonstrate that geographical patterns of warming trends over land are strongly correlated with geographical patterns of industrial and socioeconomic development, implying that urbanization and related land surface changes have biased the temperature trends (and are, therefore, the cause of much of the observed warming). In the case of de Laat and Maurellis (2006) and an earlier paper by McKittrick and Michaels (2004), IPCC (Trenberth et al., 2007) assessed these papers and noted that the locations of greatest socioeconomic development coincided with those most warmed by atmospheric circulation changes, which are not limited to urban areas but rather have large-scale coherence. When this is taken into account, IPCC concludes that the correlation of warming with industrial and socioeconomic development ceases to be statistically significant.

<http://www.epa.gov/climatechange/endangerment/comments/volume2.html#2>

The last two sentences repeat uncritically the unsupported claims in the IPCC Report, which the EPA attributes to Trenberth et al. (2007). I should note that the IPCC claim was obviously an invention since it is presented with no supporting evidence and misrepresents the actual findings in the studies they cite. The EPA's failure to recognize this obvious fact is, in my view, prima facie evidence that their review of evidence was biased, cursory and inadequate.

In the years since publishing the second of my studies on this topic (McKittrick and Michaels 2007, cited by the EPA quotation above), a number of statistical criticisms have been advanced, chiefly in a 2009 paper by Gavin Schmidt of NASA, who made a series of claims about the reliability of our results without subjecting them to formal statistical modeling and testing. The EPA relied upon Schmidt's paper in another section of its rejection of comments on the Endangerment finding:

Neither IPCC nor CCSP assess McKittrick and Michaels (2007) which conclude that "that non-climatic factors, such as those related to land use change and variations in data quality, likely add up to a net warming bias in climate data, suggesting an overstatement of the rate of global warming over land." However we note a recent study by Schmidt (2009) that finds "The reported correlations [in McKittrick and Michaels, 2007]...are probably spurious (i.e. are likely to have arisen from chance alone). Thus, though this study cannot prove that the global temperature record is unbiased, there is no compelling evidence from these correlations of any large-scale contamination."

It is noteworthy that the TSD cites Gavin Schmidt as an expert reviewer (p. ii) but not anyone from the other side of the debate, indicating a lack of diligence on their part in obtaining balanced information on this issue.

Schmidt's paper makes the quoted assertions without subjecting them to formal statistical testing. In a recent peer-reviewed paper I have tested Schmidt's conjectures and showed them to be unfounded. Specifically I show that the evidence of data contamination is consistent across multiple combinations of surface and satellite data, that it is not an artefact of statistical modeling, and that it cannot be replicated by climate models:

- McKittrick, Ross R. and Nicolas Nierenberg (2010) "Socioeconomic Patterns in Climate Data." *Journal of Economic and Social Measurement*, Vol 35 No. 3-4 pp. 149-175.

Consequently I submit that the EPA's consideration of this issue is at best inadequate and at worst based on fabricated evidence. The reality of problems in the surface temperature record fundamentally impair the conclusions about the magnitude of warming and its attribution to greenhouse gases, since the studies that support EPA findings on this either directly or indirectly presuppose the absence of any contamination problems in the surface temperature record.

In addition to submitting the above information to the EPA in response to its Advance Notice of Proposed Rulemaking, I made submissions on the above information to both the UK House of Commons Science and Technology Committee investigation and to the Muir Russell Review of the Climate Change Emails,<sup>5</sup> neither of which disputed or rebutted any of the information, but neither of which addressed the implications either.

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<sup>5</sup> My submissions are online at <http://rossmckittrick.weebly.com/climategate.html>.

## 2. Deletion of Evidence on the Uncertainty of Warming Trends

One of the IPCC's most important topics is the measurement of modern warming trends. In the Second Order Draft of the Working Group I section of the Fourth Assessment Report, in the discussion of Table 3.2, which presents data on observed temperature trends at the global and hemispheric level, the following cautionary text was included on page 3-9 (emphasis added)

Table 3.2 provides trend estimates from a number of hemispheric and global temperature databases. 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.**

This paragraph was not in the First Order Draft and appears to have been inserted on the basis of technical comments received during expert review. There do not appear to have been any reviewer objections to this paragraph. A statement was also included in the Appendix of the Second Order Draft (p. 3-116) cautioning that the method used by the chapter authors to compute trends, called REML AR1, yields statistical significance levels that are "likely to be overestimated" (emphasis added):

As some components of the climate system respond slowly to change, the climate system naturally contains persistence, so that **the REML AR1-based linear trend statistical significances are likely to be overestimated** (Zheng and Basher, 1999; Cohn and Lins, 2005). Nevertheless, the results depend on the statistical model used, and more complex models are not as transparent and often lack physical realism.

The draft of the IPCC Report that was circulated on July 3 2006 (immediately after the close of expert review) still included the statements about erroneous trend detection:

17  
18 Table 3.2 provides trend estimates from a number of hemispheric and global temperature databases. Brohan  
19 et al. (2006) and Rayner et al. (2006) provide uncertainties on annual estimates, incorporating the effects of  
20 measurement and sampling error, and uncertainties regarding biases due to urbanization and earlier methods  
21 of measuring SST. We take these into account, although ignoring their serial correlation. Determining the  
22 statistical significance of a trend line in geophysical data is difficult, and many oversimplified techniques  
23 will tend to overstate the significance. Zheng and Basher (1999), Cohn and Lins (2005) and others have used  
24 time series methods to show that failure to properly treat the pervasive forms of long-term persistence and  
25 autocorrelation in trend residuals can make erroneous detection of trends a typical outcome in climatic data  
26 analysis (see more extensive discussion in Appendix 3.A).  
27

This version of the text was an attachment to an email released to David Holland in the UK in response to his 2010 Environmental Information Regulation request to the University of Reading for the records of IPCC Review Editor Brian Hoskins.

In the version of the IPCC report that was released to the public ten months later, in May 2007, the statement warning of erroneous trend detection had been deleted, and replaced with the following (p. 242) (emphasis added):



In Table 3.2, the **effects of persistence on error bars are accommodated** using a red noise approximation, which **effectively captures the main influences**. For more extensive discussion see Appendix 3.A

The text in the Appendix 3.A had been changed to the following (p. 336) (emphasis added):

As some components of the climate system respond slowly to change, the climate system naturally contains persistence. Hence, the statistical significances of REML AR1-based linear trends **could be** overestimated (Zheng and Basher, 1999; Cohn and Lins, 2005). Nevertheless, the results depend on the statistical model used, and more complex models are not as transparent and often lack physical realism. Indeed, long-term persistence models (Cohn and Lins, 2005) have not been shown to provide a better fit to the data than simpler models.

Hence the changes made to the IPCC report after the close of peer review were as follows.

- A caution about the likelihood of erroneous detection of trends, that had been inserted based on information received during expert review, was deleted.
- An unsupported claim was inserted into the chapter (p. 242) claiming that the chapter authors' method (REML AR1) "effectively captures the main influences," despite the warming in the Appendix to the Second Order Draft that this method likely overestimated the significance of trends.
- A caution in the Appendix that "linear trend statistical significances are likely to be overestimated" was changed to say merely that they "could be" overestimated.
- A sentence was added to the Appendix disputing the validity of persistence models, with no supporting citations.

In sum, the IPCC deleted evidence pointing to uncertainties in their claims, and also falsified the review record insofar as they added text in response to expert review, then led reviewers to believe that it had been inserted, then deleted it after the reviewers had no further access to the text.

I described these alterations to the IPCC text in my submission to the EPA, but to the best of my knowledge they are not addressed in the responses to comments as posted online at <http://www.epa.gov/climatechange/endangerment.html#comments>.

### **3. New Evidence Concerning Model-Data Mismatch in the Troposphere over the Tropics**

I also wish to draw your attention to a new paper, of which I am coauthor, regarding the tropical troposphere, that has direct bearing on a key claim relied upon by the EPA in its dismissal of some critical comments. The region in question is the vast section of atmosphere up to an altitude of 16 km, spanning 20 degrees North and South of the equator. The importance of this region is based on the fact that, ever since the first climate models were produced, and in all the modeling work done since, including for the IPCC in its 2007 Report, the theory of amplified greenhouse gas-induced warming implies that warming trends should reach a maximum there, specifically in the mid-troposphere over the tropics. A recent survey article by Thorne et al. (2011) summarizes the point as follows:

“Since the earliest attempts to mathematically model the climate system’s response to human-induced increases in greenhouse gases, a consistent picture of resulting atmospheric trends has emerged. The surface and troposphere (the lowest 8—12 km) warm with a local maximum trend in the upper levels in the tropics, while the stratosphere above cools.”

The IPCC also emphasizes that,<sup>6</sup> according to climate model predictions, warming due to greenhouse gases reaches a maximum in the upper troposphere over the tropics, and that all model runs suggest this pattern ought to be observable in current data.

But there is considerable empirical evidence that no such warming “hotspot” has been observed since the advent of satellite monitoring in 1979. Many commenters on the EPA Endangerment Finding pointed to the empirical evidence that the combined records from weather balloons and satellites does not support the model predictions of amplified warming in the tropical troposphere.<sup>7</sup> A significant discrepancy between models and observations on this point would imply a major failure on the part of climate models, directly undermining the soundness of, among other things, the EPA’s position. Indeed the 2006 CCSP Report on surface and satellite records, mentioned above, pointed to this problem, as follows:

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.  
(Wigley et al. 2006, p. 11)

In 2007, papers by two teams of authors (Christy, Norris, Spencer and Hnilo, and Douglass, Christy, Pearson and Singer) showed that observed data sets contained much less warming than even the lowest model-based predictions. The Douglass et al. paper specifically asserted that the model-data discrepancy is statistically significant. The EPA Response to comments on the Endangerment Finding (3-7) reveals some hesitation on their part concerning this matter:

EPA is aware of the emerging literature on this issue and the challenges in identifying the anthropogenic fingerprint in the tropics. The TSD’s characterization of this issue is consistent with the assessment literature as well as the most recent studies, which find that when uncertainties in models and observations are properly accounted for, newer observational data sets are in agreement with climate model results.

The new study, of which I was coauthor, specifically rebuts the latter statement.

The EPA responds to the evidence in the Douglass et al. paper by citing three sources. First, they refer to a paper by Haimberger et al. (2008) which uses a weather balloon series called RAOBCORE version 1.4, which apparently agrees with some model projections. However, Haimberger has since revised the RAOBCORE version 1.4 data to remove a spurious warming influence from an input data source.<sup>8</sup> The trend in the lower tropical troposphere in RAOBCORE 1.4 set is now 0.117 degrees C per decade whereas the average predicted trend in climate models for the same region is 0.272 degrees C per decade,

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<sup>6</sup> IPCC WGI pp. 763-764; also Figure 9.1.

<sup>7</sup> <http://www.epa.gov/climatechange/endangerment/comments/volume3.html>

<sup>8</sup> The problem apparently was in the ERA-40 reanalysis data.



more than twice as high. Clearly this data set cannot be the basis for setting aside the commenters' concerns about models overstating warming.

The second paper cited by the EPA is Allen and Sherwood (2008), who use *windspeed* data collected by weather balloons to infer temperature trends. They find higher trends than studies using thermometers to measure temperature trends. The EPA does not provide a discussion of the problems associated with using wind data to infer temperatures. A 2010 paper by John Christy and 8 coauthors in the journal *Remote Sensing* points out that until the advent of modern GPS systems, weather balloons tended to drift out of radio range at high altitudes on the windiest days, leading to an artificial depression of the highest windspeeds in the earlier years of the record, introducing a known source of bias in the trend over time. Also, windspeed data is very limited in the tropics compared to temperature data, and as Christy et al. point out, the temperature trend calculations by Thorne et al. imply windspeeds in the interpolated regions would have to be much higher than those observed in regions that do have data. Consequently, it was inappropriate for the EPA to place greater reliance on this study than on the many studies using direct temperature observations, especially since its method is new and rather speculative.

The third study cited by the EPA, and arguably the one that is key to their position, is a 2008 paper by Ben Santer et al., asserting that uncertainties in climate models and observations are sufficiently large with regards to trends in the tropical troposphere as to rule out a finding of inconsistency. They reach this conclusion by arguing that Douglass et al. used an incorrect statistical methodology to compare modeled and observed trends, and in the Santer et al. analysis they propose a slight improvement in methods, which they apply to data ending in 1999. They report the uncertainties in the model trends to be sufficiently large as to partially overlap with the uncertainties in the observed trends, leading Santer et al. to conclude that the models-data differences are not statistically significant.

In a paper published in fall of 2010, I and two coauthors showed that the Santer et al. conclusions fail on two grounds. First, neither Douglass et al. nor Santer et al. used modern statistical modeling techniques for comparing trends in data sets of the kind under dispute. We applied two different state of the art statistical methods for trend comparisons, both of which are well-established in the econometrics literature. Second, we extended the data up to the end of 2009 (the maximum extent available when we did the analysis). Ending the data at 1999 is a problem because there was a large El Nino event in 1998, temporarily boosting the observed trend so it appears to match models.

We found that on the full sample up to 2009, the satellite and weather balloon data sets were not significantly different from each other, but were significantly different from models. In particular, the models predicted two to four times more warming, on average, than is observed in the data, and the differences are statistically very significant.

Our paper is

- McKittrick, Ross R., Stephen McIntyre and Chad Herman (2010) "Panel and Multivariate Methods for Tests of Trend Equivalence in Climate Data Sets." *Atmospheric Science Letters*, DOI: 10.1002/asl.290.

In light of these updated findings, the EPA's reliance on Santer et al. (2008) is unsound, as is their claim that

"when uncertainties in models and observations are properly accounted for, newer observational data sets are in agreement with climate model results."

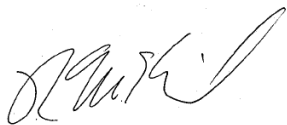
The statement should read:

“when uncertainties in models and observations are properly accounted for, the most up-to-date data sets indicate a statistically significant discrepancy between observations and climate model results.”

If, like Santer and Douglass, we had terminated our sample in or around 1999, we would find the model-observation difference is only marginally significant, partially supporting the Santer findings. But there is also no significant warming trend in the balloon and satellite series if the data are truncated at 1999, something not mentioned by the EPA in its reliance on the Santer et al. results. When the data are extended up to 2009, some of the observational series indicate a significant warming trend, but it is very small compared to model predictions, and the model-observation discrepancy is statistically significant. Thus we affirm the 2006 observation of the CCSP Report of a “potentially serious inconsistency” between models and data. The continuing importance of this issue is attested by the Thorne et al. review paper I mentioned earlier, which points out that if observations fail to support the tropospheric warming projected by models this would have “fundamental and far-reaching implications for understanding of the climate system.”

The EPA claims to place great importance on the rigor of the peer review process. Our paper went through three rounds of intensive review involving five different referees. One of our referees was Ben Santer, who signed his report. His review comments were quite extensive, but he did not put forward any objections that the Editor did not later decide we had rebutted. As with all of my papers, upon publication I archived all my data and code online to allow any reader the ability to check our calculations.

Yours truly,



Ross McKittrick  
Professor of Economics

Docket ID No. EPA-HQ-OAR-2009-0171

Response to Federal Register/ Vol. 74, No. 78, April 24 2009 / Proposed Rules

**Responder:**

**John R. Christy**  
**ESSC/Cramer Hall**  
**University of Alabama in Huntsville**  
**Huntsville AL 35899**

[Christy@nsstc.uah.edu](mailto:Christy@nsstc.uah.edu)  
**256 961 7763**

Brief biosketch

I am John Christy, Ph.D., Professor of Atmospheric Science and Director of the Earth System Science Center at the University of Alabama in Huntsville. I have served as Lead Author of both the IPCC and CCSP reports, have published widely on climate science, have served on NRC and NAS panels, have testified several times before congressional hearings and have received a number of awards including NASA's Medal for Exceptional Scientific Achievement. I also appeared as an expert witness, unpaid, in Federal Court to testify as to the impact of California AB 1493 (auto emission standards) on the climate system. Both sides and the Judge accepted my conclusions.

Introduction

There are many aspects about the proposed rules that should be challenged, but I will focus only on the part about which I have considerable experience and a considerable publication record. In general, I am deeply troubled that the EPA has accepted an alarmist set of assumptions as "facts" when the truth is that our ignorance about the climate system is still enormous. I will demonstrate that assertions held by the EPA are highly questionable because they are based on "consensus reports", poor data and poor model projections. I concentrate on three major EPA assertions (the third has two parts) each of which will be introduced by a condensed statement of the assertion, then the alternate conclusion I supply ("Bottom Line"), followed by a rather detailed explanation.

### **Part III.A.3 The Administrator's Proposed Endangerment Finding (18896)**

**Condensed EPA Assertion 1:** "... most of the observed global and continental warming can be attributed to this anthropogenic rise in greenhouse gases. The information presented here builds on these facts ..."

**Bottom Line 1:** Attribution of climate variations is a function of faith in climate model projections and not in direct observations (i.e. "facts").

#### **Explanation 1:**

"Facts" about the climate system are only "assertions" until proven beyond doubt to be real (with error bars) by testing. The EPA has relied almost exclusively on consensus documents (e.g. IPCC and CCSP) as the origin of their "facts". I have served as a Lead Author of both the IPCC and CCSP reports and will demonstrate with published data that these reports are not always "factual" but written (a) to give the impression of certainty where large uncertainty is the reality or (b) to actually suppress results which run counter to the more alarming conclusions. And, more importantly, the "consensus" exercise is a false scientific process because the authors tend to write about their own publications and are given the final review-authority of the products (i.e. this is not a peer-reviewed process in the sense that the product could have a relatively high probability of being rejected by independent reviewers. The selected authors KNOW their words will be published since they have the "last word".)

Indeed, the great majority of the IPCC authors were, on the one hand, not climate scientists and were, on the other hand, pre-approved by their governments in a political process (this is a pattern followed by the CCSP reports as well.) This should lead to considerable caution when interpreting their statements – the reports had as their final editors those who were appointed by the political process. Thus, scientific results deemed inconsistent with personal views of the authors were far less likely to be considered in the reports.

A fundamental notion contained in the IPCC and CCSP reports, and stated in the EPA quote above, is that climate models are capable of producing "facts" when in fact they cannot. They are models – which means they are the sum of the assumptions and prejudices of the organizations building the models (and do rather poorly when measured against the real world as shown later.) Here is a simple fact:

There is no instrument that can measure Earth's temperature change which can unambiguously determine what part of the temperature change might be due to humans and what part might be due to nature.

[It should be noted that the IPCC AR4 stated there was a 90% probability that most of the surface warming in the past 50 years was human-induced. Thus to characterize human-induced warming as "fact" (see quote from EPA under assertion 1) is to say an idea that is only 90% confident (from the IPCC itself) is a fact. I do not know of any context in which a 10% probability of being incorrect would be considered a "fact".]

Claims as to how much of the change is due to humans are found only in model assumptions and simulations ... not in direct observation. Therefore, it is faith in model simulations (and their assumptions) that drives the notion that major variations in the climate are due to greenhouse gases. I will demonstrate below that the "facts" relied on by the EPA endangerment finding are not "facts" but assertions for which there is considerable contradictory information.

### **Part III.C. The Administrator's Proposed Finding That the Air Pollution Endangers Public Health and Welfare**

#### **Part 1. Evidence of currently observed climatic and related effects (18898)**

**Condensed EPA Assertion 2:** Popular surface datasets, which show warming, represent the effect on the climate caused by greenhouse gas increases.

**Bottom Line 2:** Popular surface datasets have poor station selection and are contaminated by warming of nighttime temperatures caused by surface development, not greenhouse warming.

#### **Explanation 2:**

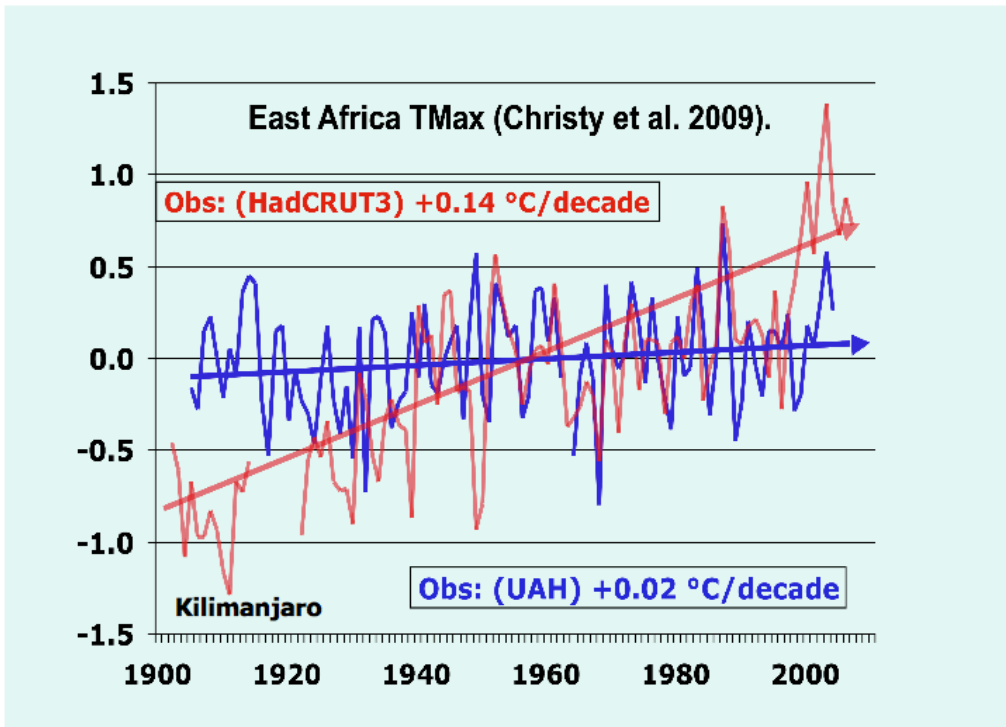
The major “fact” used by the EPA (derived from the IPCC) is that the popular surface temperature datasets (HadCRUT3v, GISS, NOAA/NCDC) are accurate representations of the part of the climate system which is affected almost exclusively by rising greenhouse gases. Thus, the reasoning goes, as the surface temperature rises, this becomes an indicator primarily of greenhouse effects with all other effects being fairly minor, including natural variability. This is an assertion which is highly questionable and likely false.

As a culmination of several papers and years of work, Christy et al. 2009 demonstrates that popular surface datasets overstate the warming that is assumed to be greenhouse related for two reasons. First, these datasets use only stations that are electronically (i.e. easily) available, which means the unused, vast majority of stations (usually more rural and more representative of actual trends but harder to find) are not included. Secondly, these popular datasets use the *daily mean* surface temperature (TMean) which is the average of the daytime high (TMax) and nighttime low (TMin). In this study (and its predecessors, Christy 2002, Christy et al. 2006, Pielke Sr. et al. 2008, Walters et al. 2007 and others) we show that TMin is seriously impacted by surface development, and thus its rise is not an indicator of greenhouse gas forcing. Some have called this the Urban Heat Island effect, but, as described in Christy et al. 2009, it is much more than this and encompasses any development of the surface (e.g. irrigated agriculture). For scientists reading this response, I would encourage you to read Christy et al. 2009 for a discussion of how the delicate nocturnal boundary layer formation process is disrupted by surface development, leading to an increase in TMin which is unrelated to greenhouse gas forcing. The evidenced supplied in this paper is supported by several other studies from the observational, theoretical and boundary-layer modeling arenas.

(In some of the examples below, we will look at trends from 1979 forward since this is the period of surface warming in the popular datasets blamed on greenhouse forcing. Since there was no warming from 1950-1979, the IPCC 2007 assertion that “most” of the warming since 1950 is due to greenhouse gases relies only on the post-1978 period).

Below is an example for East Africa (grid square bounded by 5°S – Eq, 35°E – 40°E). Here we see the two flaws in the popular datasets (HadCRUT3v and GISS NASA). First, the reliance on a very few stations leads to large errors (popular trends > +0.3 °C/decade vs. +0.1 when all stations are used with rigorous techniques) and secondly, the use of TMean which incorporates the warming effects of surface development in TMin which are mistakenly assumed to be driven by greenhouse gases (Christy et al. 2009).

East Africa 1979-2004	Trends °C/decade			
Dataset	TMean	TMin	TMax	No. Stations
HadCRUT3v	+0.31	-	-	2
GISS NASA	+0.35	-	-	4
Christy et al.	+0.11	+0.16	+0.05	45



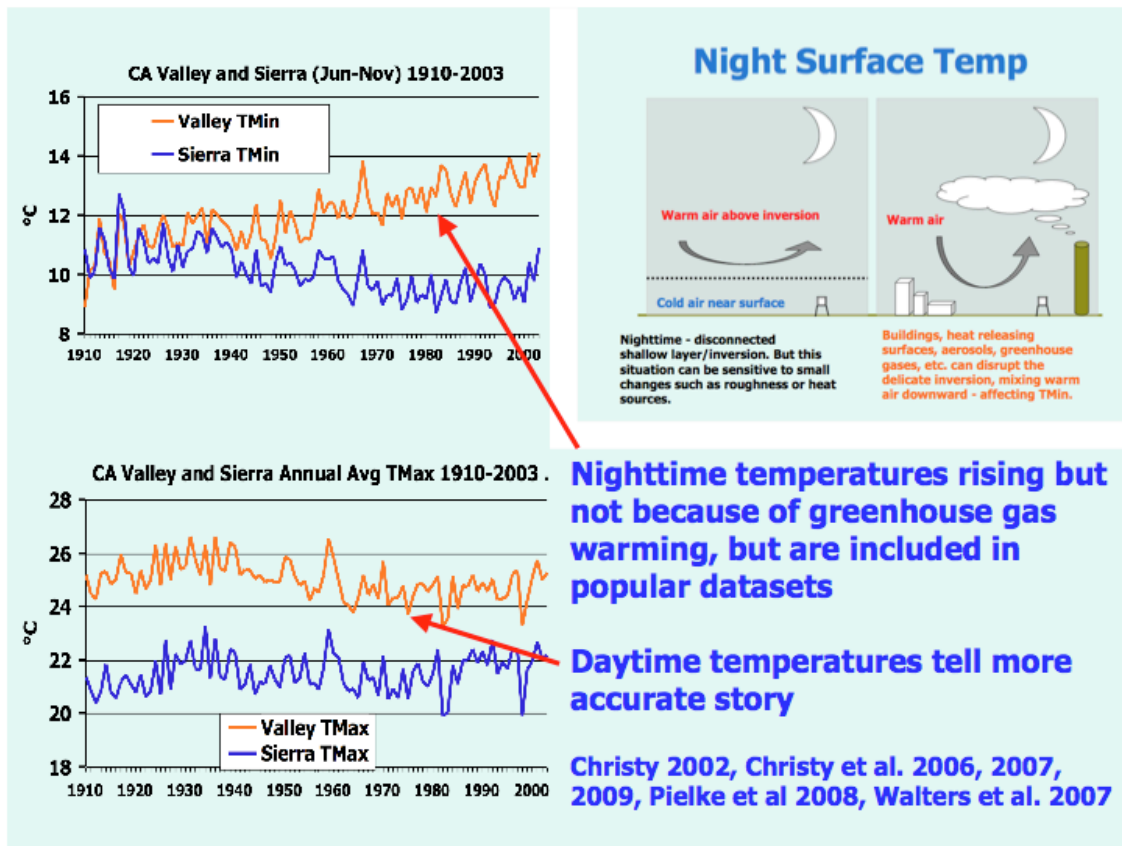


The figure above shows the difference between the results of HadCRUT3v (surface TMean) and our published study for the 100 year period 1905-2004 (surface TMax, the earlier Table looked at 1979-2004 only). It is clear that the popular dataset (HadCRUT3v) overstates the physical warming of the surface (and atmosphere.)

A second example below is given for Central California as reported in Christy et al. 2006. Here we have a different experiment in which we built two datasets, one of the San Joaquin Valley which has experienced extensive surface development (urbanization, irrigated farming, orchards replacing desert, etc.), and one of the adjacent Sierra Nevada mountains, where little development has occurred.

California	Trends C/decade			
1910-2003	TMean	TMax	TMin	Stations
Valley	+0.07	-0.10	<b>+0.25</b>	18
Sierra	-0.02	+0.05	-0.08	21

The figure below is a summary of the results for California, demonstrating the misrepresentation of incorporating TMin in popular datasets.



Here, we see that the only significant temperature trend is for TMin in the developed Valley. The “control experiment” (TMin in Sierras) shows no warming, thus greenhouse gases could not be the cause of the Valley warming because greenhouse gas warming, according to models, would warm the entire area, and in fact warm the Sierra more than the Valley. A second “control” experiment, TMax, is not warming in either region, further indicating TMax is the preferred metric for detecting larger-scale phenomenon like the enhanced greenhouse effect (which apparently is having no impact.) The paper also reports that the popular datasets show higher TMean warming rates than even shown here due to their small selection of stations used (+0.10 °C/decade warmer, Christy et al. 2006, pg 562). An objection was raised about a single one of the sixteen time series created in our paper by Bonfils et al. 2007, but Christy et al. 2007 demonstrated the objection was incorrect.

The basic result of all of these studies is that the few stations selected for calculations in the popular datasets are spuriously warm over the land and that they include TMin, which is warming more than TMax, but not because of greenhouse gas forcing. The best surface metric for detecting atmospheric warming is TMax (daytime high) as it generally occurs with the deeper mixing of the atmosphere and is not nearly as impacted by boundary-layer disruptions (see studies cited above and upper right hand figure above.) An even better metric is tropospheric temperature change (satellite data.) The recent paper by

Voss et al (2005) which appeared to show that more recently TMin was warming at a rate not quite as slowly as TMax did not have the opportunity to include most of the developing world (as we have shown for East Africa, for example), and thus has generally missed the part of the world where such effects would be observed.

One criticism of our work might be that we have examined only a few locations around the earth. This is a legitimate point. However, these studies are excruciatingly detailed and time-consuming (thus we have only completed these). We have selected three very different surface temperature climates and found the same result in all – the popular surface datasets overstate the warming in these areas as compared with our carefully-constructed *and published* results. However we can point to global results too. When comparing the global land trends of the satellite (i.e. atmospheric) and surface datasets, the trend differences are physically explainable only if the land surface measurements are contaminated by local development activities. Thus we have demonstrated in three locations what is consistent with the global-land temperature trend differences between the surface and the atmosphere.

Warning: The EPA will be tempted to rely on scientists/appointees who are well-entrenched into a particular view of the issue of global warming to review documents such as this, and who will (a) develop clever-sounding rebuttals, and (b) are afforded the luxury of the “last word” to protect the current EPA consensus. Basic scientific inquiry should encourage EPA to listen to those of us who actually build these datasets (from scratch) as our message has equal if not greater credibility.

## **Part III.C. The Administrator's Proposed Finding That the Air Pollution Endangers Public Health and Welfare**

### **Part 1. Evidence of currently observed climatic and related effects (18898)**

**Condensed EPA Assertion 3:** Climate models have enough precision to allow EPA to make the assertions stated in this section, i.e. "... most of the observed global and continental warming can be attributed to this anthropogenic rise in greenhouse gases." And "... changes are occurring now that can be attributed to the anthropogenic rise in atmospheric greenhouse gases ..."

**Bottom Line 3.1:** Climate model output has failed to reproduce current tropical changes, a key greenhouse detection region, significantly overstating the very modest warming. The information in CCSP 1.1 (Karl et al. 2006) is biased, but more importantly, out of date.

#### **Explanation 3.1:**

That climate models have serious shortcomings is not a new scientific finding. However, it is well known that the clearest signal of model-projected greenhouse warming is found in a rapidly warming tropical troposphere. This issue has been examined by both the IPCC and CCSP (SAP 1.1, Karl et al. 2006) with disappointing analysis. I want the EPA to know that those who write these consensus reports are people who often serve as gatekeepers of these issues. I have served on these panels and have witnessed the heavy-handed tactics of the authors. The majority of these authors are selected by their governments for their specific view on climate change, not because of their scientific productivity on the issue at hand. I struggled with the other CCSP lead authors, as detailed in my House Testimony of 2006 (Christy 2006), for a more accurate rendering of the summary statements, but was unsuccessful. Thus "consensus" is less than what it appears to be.

With that as a background, the fundamental issue here is that climate model simulations produce temperature changes in the tropics that show the upper air warms more than the surface as a very distinct signature of the enhanced greenhouse effect. At certain altitudes, the warming is twice (or more) that of the surface in the models. So, a simple hypothesis test can be performed which compares the upper air temperature trends to the surface using observations and models. Models show that the upper air layer-average

trend is 1.3 times that of the surface. The factor of 1.3 is often called an amplification factor or amplification ratio.

Let me say here that one point of confusion occurs immediately. One can say that the surface and tropospheric trends are consistent (i.e. not statistically different) in the sense that their magnitudes are similar (i.e. an amplification ratio of 1.0). However, the real scientific discussion deals with the fact that in the tropics climate models indicate that tropospheric trend should be about 1.3 times greater than the surface if models have greenhouse theory correctly simulated. Thus when someone says the discrepancy between the surface and tropospheric trends has been resolved with no difference between them, this becomes a misleading statement because it also implies that the troposphere is warming no more than the surface, which is therefore *inconsistent* with model greenhouse theory on which the current EPA relies.

We have continued to look at this issue beyond CCSP (and the IPCC which simply followed the CCSP findings) and now have even further evidence to demonstrate that this well-known discrepancy is indeed real and that the models have erred significantly. In Christy et al. 2007, the most detailed analysis to date was performed on all balloon stations in the tropics (20°S - 20°N) in comparison with all datasets available at the time and concluded the observed upper air tropical trend was not 1.3 times that of the surface (it was less).

While much was in the paper, one interesting result was that a satellite dataset produced by Remote Sensing Systems (RSS, which indicated a warmer temperature trend than the other datasets) contained a discontinuity in 1992 that was especially strong in the tropics. This feature was confirmed in three other studies which used different tests to demonstrate also that the trend of RSS was spuriously too positive (Christy et al. 2006, Randall and Herman 2008 and Christy and Norris 2009.) In this last paper (Christy and Norris 2009) we also demonstrated that the new NOAA-produced satellite dataset (STAR) has serious problems due to errors in correcting diurnal problems and intersatellite biases. These results were not included by the CCSP or IPCC panels to influence the “consensus” (the publications were after CCSP and thus IPCC had closed), but their results remain unchallenged and should be accepted by the EPA as peer-reviewed, published findings. [Note: A different paper, Douglass et al. 2007, which demonstrated the model failures, was challenged as will be discussed below.]

A simple way to look at this basic issue is that models show an amplification of temperature trends through the troposphere caused by greenhouse gases, so that whatever the trend is at the surface, the upper air trends warm by up to a factor of 2 (and more) by 12 km altitude. The average factor for the layer (which satellites measure) is 1.3, i.e. the layer measured by satellites should warm by a factor of 1.3 faster than the surface according to the greenhouse theory in models. The results of Christy et al. 2007 indicate the factor is not 1.3, but 0.7 to 1.0 (when RSS is discounted) – i.e. no amplification and thus models over-warm the atmosphere. Douglass et al. 2007 (I was a co-author) followed up with a detailed comparison of observations and models to demonstrate a significant difference between the two using both satellites and balloons, or that the

model hypothesis of an amplification factor was falsified – important because that is the key signature of greenhouse gases in the models.

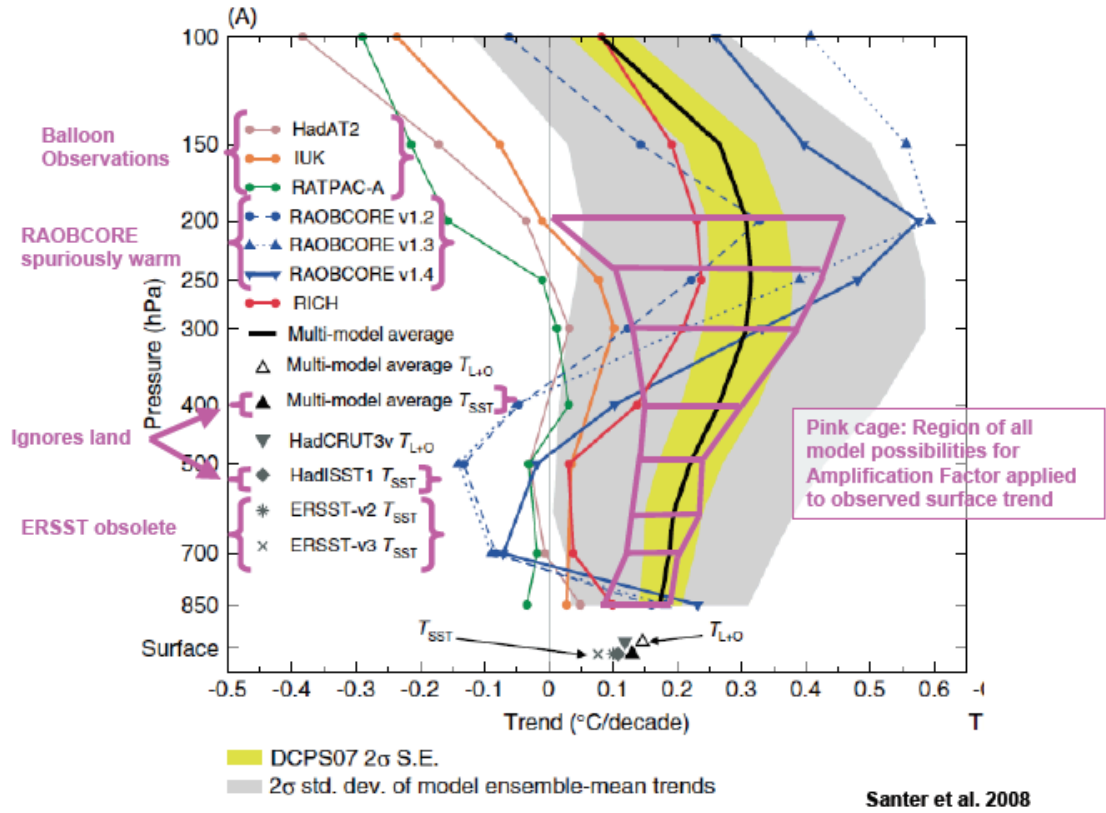
It didn't take long for the “consensus” side, which earlier dominated CCSP 1.1 (Karl et al. 2006), to respond. Santer et al. 2008 reconfirmed the numerical results of the question addressed by Douglass et al. 2007. Our question was simply, “When the models and the observations have the same surface temperature trend, do the models and observations agree in the troposphere?” The answer was no. In other words, Santer et al. reproduced the results of Douglass et al. 2007.

However, Santer et al. then asked a different question, which might have interest to some, but was not our question as stated above. They asked something like this, “When individual model trends of the surface are allowed to be examined, whether they agree with the observations or not, do upper air trends between models and observations agree?” Not surprisingly, because some individual model trends are quite bizarre, they could answer in the affirmative, but only for models whose surface temperature did *not* match the observed surface trend. In other words we compared apples to apples and Santer et al. compared apples to oranges. When going back to the fundamental issue of whether models overstate the atmospheric amplification factor, the answer is clearly yes from the observations and models we have. (And in an ironic result, had Santer et al. used UAH satellite data through the most recent year, the models would have failed their test in any case.)

In the analysis, Santer et al. used some “old”, “modified” (i.e. SSTs only) and “new” datasets that (a) revealed less surface warming or (b) more upper tropospheric warming. By using these datasets, the apparent discrepancy could be reduced (i.e. cooling the surface or warming up the troposphere in the observations). Then, one unorthodox trick was added - the use of Sea Surface Temperatures (SSTs) only and ignoring the warming of the land temperatures as if they did not matter (which is incredulous since the upper air resides over land too.) Regarding the SST datasets, they used a “new” one –ERSST - which indicated less warming at the surface so when multiplied by the model-calculated factor of 1.3, implies less warming in the upper air – which then was closer to our upper air observations. However, the version of ERSST used in the paper is now obsolete (obsolete trend was +0.076, new trend is now +0.126 °C/decade – 65% warmer!), so the consistency arguments of Santer et al. based on the old ERSST are obsolete as well.

The figure below, from Santer et al. 2008 but supplemented with pink comments, is quite complicated, but contains much of the information described herein. This is a diagram of the vertical atmosphere and superimposed are trends for 1979-1999 from various balloon observations and IPCC AR4 model results. The key point here is that the pink cage represents the entire range of model trends under the assumption they produced the observed surface trend (i.e. this gives an apples to apples comparison between models and observations). As can be seen, the observations (brown, red, green, orange lines) lie to the left (cooler) than the coolest of the model trends for the bulk of the lower atmosphere (700 – 400 hPa). Only part of the RICH (red) trends penetrate the cage, though, RICH is influenced by the ERA-40 model forecast scheme which has a clearly

demonstrated spurious warming due to improper assimilation of HIRS channel 11 (which renders RAOBCORE v1.2-1.4 obsolete, see below.) The other balloon datasets are not affected by that problem.



In another curious avoidance, Santer et al. did not include surface datasets generated by NOAA/NCDC and NASA/GISS to confuse the overall picture again. When these datasets are used (with their higher surface trends pointing to higher upper air trends when multiplied by 1.3), they indeed more closely support the results of Christy et al. 2007 and Douglass et al. 2007 that upper air trends of models and observations are significantly different.

Regarding the upper air trend datasets, Santer et al. included RAOBCORE v1.2, v1.3 and v.1.4, which appeared to show a fairly rapidly warming in the upper tropical troposphere (see Fig.) However, the RAOBCORE datasets, which rely on the ERA-40 forecast cycle, have been shown to be spuriously warm in the upper air due to an error in the assimilation of HIRS channel 11 in 1991-2 (noted in earlier papers, but specifically identified in Sakamoto and Christy, 2009). Rather, Christy et al. 2007 and Douglass et al. 2007 used the latest version from the RAOBCORE group - RICH, which was also affected by the spurious warmth in 1991-2 but not as much, and yet found the inconsistency with models was indeed upheld for the layer-average.



Again, relying on the various datasets, *which have been tested for accuracy*, we find no evidence to contradict the results of Christy et al. 2007 and Douglass et al. 2007. (Note the caveat, “which have been tested for accuracy” – papers such as Santer et al. 2008 do no testing, but simply assume that all datasets are equal, such as “new” ERSST or “old” RAOBCORE v1.2, v1.3 and v1.4, and thus ignore the publications which have provided the evidence which document significant errors in the ones they prefer.)

There is much, much more available on this topic, but I will leave it here. Please contact me for more information/clarification if needed.

[Repeated] Warning: The EPA will be tempted to rely on scientists/appointees who are well-entrenched into a particular view of the issue of global warming to review documents such as this, and who will (a) develop clever-sounding rebuttals, and (b) are afforded the luxury of the “last word” to protect the current EPA consensus. Basic scientific inquiry should encourage EPA to listen to those of us who actually build these datasets (from scratch) as our message has equal if not greater credibility.

**[Repeated] Condensed EPA Assertion 3:** Climate models have enough precision to allow EPA to make the assertions stated in this section, i.e. “... most of the observed global and continental warming can be attributed to this anthropogenic rise in greenhouse gases.” And “... changes are occurring now that can be attributed to the anthropogenic rise in atmospheric greenhouse gases ...”

**Bottom line 3.2:** Climate models overstate the global surface warming over the past 30 years, the period when temperatures were predicted to start warming more rapidly.

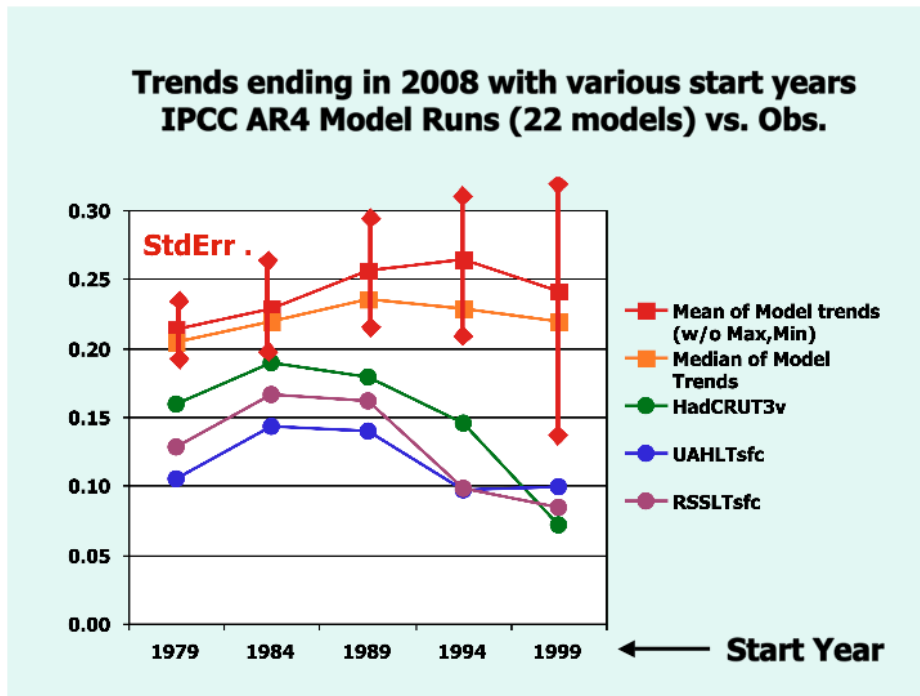
### **Explanation 3.2:**

The reason that the past 20-30 years are important is due to the IPCC assertion that most of the surface global warming since 1950 is caused by enhanced greenhouse gases. However, there was no rise in temperature from 1950-1979 so the assertion depends on the temperature rise of the last 30 years only.

I have tested all of the IPCC AR4 models against observations in two ways to demonstrate that the models overstate the warming that has occurred. The clear implication of this result is that the models have an assumed sensitivity to CO<sub>2</sub> that the real world does not. This is really the crux of the modeling problem and the whole of the predictive value of models depends on the magnitude of this “sensitivity” (i.e. temperature response to GHG forcing.)

In the figure below, I have calculated the mean and standard error of the global surface temperature trends of all of the IPCC AR4 models (A1B scenario) for periods beginning

in the year shown and ending in 2008. From the left, 1979 indicates the trend from 1979-2008 in the models is the red square ( $+0.22$  °C/decade) with a standard error of the mean of  $\pm 0.03$  °C. In other words, there was little spread in the model results for 30-year periods. As the time period becomes shorter, the standard error increases until the last period shown, 10-year or 1999-2008, becomes  $+0.24 \pm 0.11$  °C/decade. Ten-year periods can have quite a bit of variation in their trends.



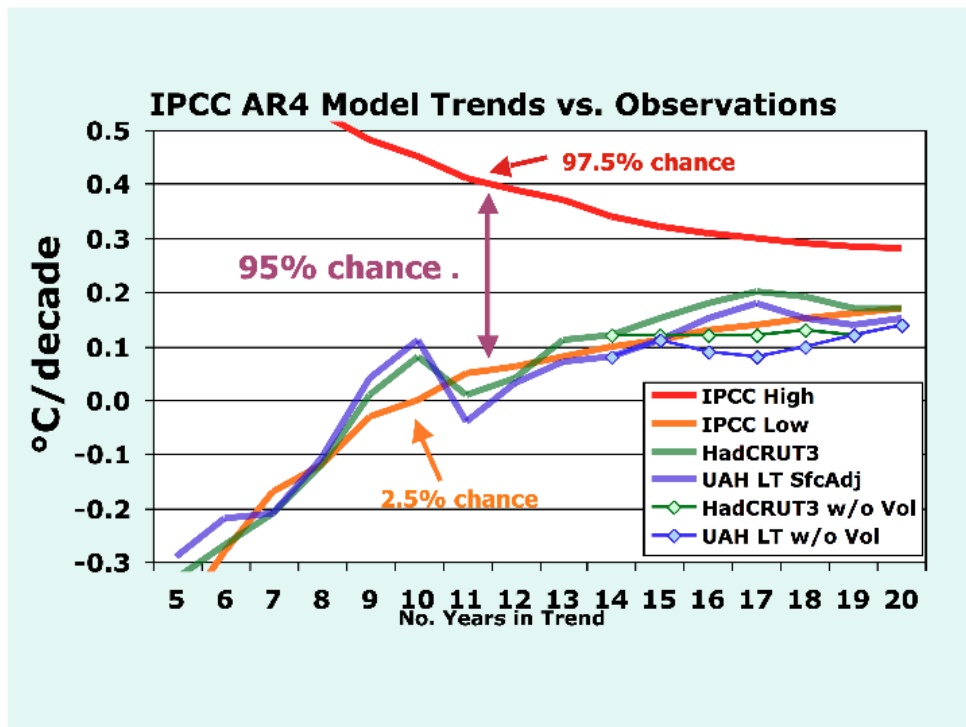
Included with these model results on the chart are the same global trends for three observational datasets. HadCRUT3v (green), UAH (blue) and RSS (purple). UAH and RSS are satellite datasets, but have global coverage. Their variations have been adjusted by the global amplification factor of 1.2 to mimic the surface variation, so that an apples to apples comparison can be made (recall the tropical amplification factor was 1.3). Note two points previously made: (1) for most of the trends, HadCRUT3v is warmer than the two satellite datasets due to excessive land-surface warming as we have demonstrated above, and (2) RSS shows warmer trends than UAH for the longer periods due to the documented spurious shift described earlier. However, even with these highly likely spurious warming effects in HadCRUT3v and RSS, the mean model trends are still significantly warmer than the observations at all time scales examined here. Thus, the model mean sensitivity, a quantity utilized by the IPCC as about  $2.6$  °C per doubled  $\text{CO}_2$ , is essentially contradicted in these comparisons. The basic meaning here is that the models are too sensitive to  $\text{CO}_2$  forcing and thus overstate the warming response.

[Note: I will mention here but will rely on submissions from others to identify the key model weakness – clouds. All models cause “reflecting” or “cooling” cloud-cover to

shrink as GHGs rise, allowing the sun to heat the Earth (positive feedback). Thus, it is the reduction of cloudiness that causes the main warming in models, not the direct action of GHGs. In the real world, my colleague Roy Spencer and others, have found that cooling-clouds actually *expand* when the Earth warms, thus creating a thermostatic cooling affect (negative feedback). Hence, the temperature impact of rising GHGs is much less due to this apparent significant negative feedback – and this fits very well with the relatively slow current rate of atmospheric temperature increases.]

A second way to look at the overstated model warming is below. Here, a colleague, Dr. P. Michaels, has calculated the 95% confidence interval for trends from all of the AR4 models over the current 20-year period 2001-2020 (in the models, obviously). The 97.5% high boundary of the range is red and the 2.5% low is orange. As noted above, trends over 5-year periods will have large variations, while longer term trends will not. I have added to this chart the trends of the observations over the last 5 to 20 years (again, UAH has been adjusted to mimic the magnitude of surface variations.)

At first glance, the observations are shown to appear right at the lowest edge of the model range, meaning they are borderline significantly different (solid lines) from the models. However, the observations experienced a major volcano in 1991 (Mt. Pinatubo) while the model simulations did not. Thus, removing the effects of the volcano on the observations (diamonds), we see that for the longest periods (16 to 20 year trends) the observations are indeed significantly different from the model range (orange line).



A recent paper by Easterling and Wehner (2009) looked only at 10-year periods for IPCC AR4 models (though it was not clear which models were used.) In the analysis above, we have calculated all of the possible trend realizations from all 22 of the models for the current time period – a much more robust and direct test of the models. In our results above, we have demonstrated what the probability truly is for these various trend periods, performing the calculations for up to 30 years (not 10) in the two figures, again making the results more robust and significant. Our results also focus on the current decades when GHGs are thought to have their largest impact on the global temperature.

Therefore, what has been shown with fairly simple statistical analysis is (1) that the mean of the IPCC AR4 model runs, often described as the “best estimate” and which represents the best estimate of climate sensitivity, significantly overstates the current global temperature change and (2) that the observations fall outside of the 95% range of the IPCC AR4 model trends and are thus significantly different from the full spectrum of model outcomes. The main point here is that the model projections should not be utilized as confident indications of the true trajectory of the climate system because the evidence shown strongly suggests that climate models are much too sensitive to GHGs.

## Summary

The EPA has relied on “consensus” documents which reflect the views of selected authors who (a) have review-authority (i.e. gatekeeper status and luxury of the “last word”) and (b) often do not consider the broad range of scientific inquiry into this subject.

The popular surface datasets cited by the EPA as indicators of greenhouse impact are poor representatives of the part of the climate system that is indeed affected by greenhouse gases. Rather they largely represent the impact of surface development over land which, then, is misinterpreted as greenhouse warming.

Atmospheric datasets which monitor regions where the GHG impacts should be easily detectable, indicate significantly less warming than models portray, implying that models in general are more sensitive to greenhouse gases than is the real world.

Thus, the foundation of the notion that humanity is threatened or endangered by the climate-consequences of additional CO<sub>2</sub> in the atmosphere (which by itself has considerable benefits for the biosphere) is based on (a) inadequate surface datasets and (b) model projections that fail hypothesis testing as they overstate the warming that is occurring.

John R. Christy  
University of Alabama in Huntsville

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