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Chairman Whitfield, ranking member Stupak, and committee members, I am John Christy, professor of Atmospheric Science and Director of the Earth System Science Center at the University of Alabama in Huntsville. I am Alabama's State Climatologist. I also served as a Lead Author of the chapter on Observations of the Intergovernmental Panel on Climate Change 2001 Assessment, a Lead Author of the Climate Change Science Program's report on temperature trends and as a Panelist on the National Academy of Sciences report on temperature reconstructions over the past 2000 years.

This written testimony covers a wide range of topics. I will discuss the idea of "consensus" in climate reports and how scientific results may be convoluted by that process. I will examine the issue of sharing computer code and data, and the way it led in our experience to a more reliable dataset. The issue of relative temperatures of the past 1000 years as stated in the IPCC 2001 will be addressed from my perspective as of one of the Lead Authors explaining that we chose words signifying a relatively low level of confidence. I also will note my disappointment with the exclusion of information that pointed to a more complex picture of temperature variability over the last millennium. I touch on the imperceptible climate impacts of energy policy options being considered nowadays and close with some comments about the unfortunate demonization of energy, the resource that has produced uncountable benefits in human health, longevity and freedom from deprivation.

Consensus Reports and Science

In describing the process of generating scientific reports by consensus I was quoted in the *New York Times* as saying it was the worst way to gather scientific information except for all the others.

Consensus at its heart is a political notion. It is a process of selecting words that don't offend the combined sensibilities of a particular set of the authors and reviewers, and is often done grudgingly. It is almost certain that a different set of authors and reviewers would select a different set of words and interpretation even if given the same scientific material.

One example from the first report of the Climate Change Assessment Program's (CCSP) on surface and atmospheric temperature trends comes to mind. This may provide a window into the "science-by-consensus" process. The report's main task was to reach conclusions about temperature trends measured at the surface and those measured in the lower atmosphere. Projections from theoretical climate models indicated atmospheric trends should be warming faster than the surface, especially in the tropics. However, several observational datasets did not support the models, suggesting flaws in the way greenhouse theory was being expressed in those models. Was this discrepancy real?

The original headline was made public in the near-final drafts as, “There is no longer evidence of this discrepancy.” This was constructed in a rather busy Chicago meeting in which various authors were working on finalizing their own chapters as well as dealing with this punchline. After sitting with this characterization for a few days I could not agree with its dogmatic tone of finality. The problem was that there was evidence for discrepancies within the report itself.

In terms of strict scientific defensibility the statement should have said, “The magnitude of the global discrepancies in trends is not significant.” I made known my view and our lead Editor, Dr. Tom Karl, instigated a special, last minute conference call with the authors to let me make my case. I was basically unsuccessful at persuading the others.

At one point I offered to have a footnote inserted that stated something like, “One author, John Christy, recommends the following version...” I didn’t mind being singled out in print as having a different view. That idea was not accepted because, I presume, it violated the notion of consensus. Rather, the punchline statement was massaged a bit to give a little less dogmatism in its meaning to, “This significant discrepancy no longer exists.”

The problem still for me is that discrepancies do indeed exist as clearly indicated in the raw numbers provided in the body of the report. However, error margins of the datasets included the *possibility* (not the *proof*) that there were no discrepancies. The difference in meaning of these two statements was apparent to me; rather than promoting a certainty of knowledge as does the first, my proposal acknowledged the uncertainty in our observations, and thus in model evaluation.

This example doesn't cast doubt on the credibility of the body of the report and the considerable information it provides. The many tables and figures display the real currency of science: numbers. The interpretation of those numbers, especially in the high profile Executive Summary, represents the political art of consensus, with the underlying knowledge that from this the headlines burst forth.

I often wonder what conclusions a completely different group of authors would have reached in the Executive Summary given the same scientific information. [That would be a very interesting experiment to perform!] My basic point is that one should recognize that scientific material *and* interpretation of that material are contained in these reports. The interpretation is difficult to test for accidental or even subtle bias. Specific statements may arise from the dogged advocacy of a small group and the fatigue of the remaining writers, but in the end is blanketed by the notion of "consensus." This leaves a murky path of accountability where "all" authors are accountable but at the same time "none" are.

I am risking something here. What future committee would ask me to serve if I might be tempted to later expose some deliberation to the public after all was said and done? I have been careful here to limit this example to one that involved only me, and that was made fully public in the process of final review. But, would the idea of public exposure and potential accountability constrain the typically free-wheeling discussions we as scientists enjoy in trying to reach conclusions? In any case, I hope this example will not threaten future opportunities for me while giving the committee a sense of the limitations of scientific consensus.

Consensus reports are not inerrant, nor infallible. And, as time goes on, new discoveries will demonstrate how science evolves and understanding improves. In the science of climate change we will never have the "Final Answer". I wish every one of these reports began with the line my high school physics teacher drilled into us, "At our present level of ignorance we think we know …"

<u>IPCC 2001, NAS and MBH99 (i.e. the "Hockey Stick")</u>

The National Academy of Sciences (NAS) recently released a report about surface temperature reconstructions of the past 2000 years. Regarding the NAS statement which evaluated MBH99 and how it was expressed in the IPCC 2001, I specifically recused myself from discussing that one paragraph since I was an author of the original IPCC statement. I did not want to be seen as having a conflict of interest and of opening the NAS to such a claim.

As one of the Lead Authors of the Observations chapter in IPCC 2001, I helped craft the now infamous statement regarding the relative warmth of the temperature of the decade of the 1990s and the single year 1998 in the past millennium. We selected the qualifying term "likely" warmest rather than "very likely" or "virtually certain". In other words we chose the term which represented a relatively low level of confidence, being two thirds chance of being correct. "Very likely" meant 90% confidence while "virtually certain" demanded 99% confidence.

Through consensus, and I've indicated the dangers of applying consensus, we settled on "likely", meaning the evidence indicated to several of us that there still remained considerable uncertainty surrounding proxy temperature reconstructions and their errors.

Some IPCC authors were concerned that MBH99 was new and had not had time to be exposed to independent analysis to confirm or revise the result. We also learned at that time that a key anchor for the early part of the record was a western tree ring series that explained only 5% of the overall temperature variability. I was specifically concerned that the unavoidable constraints on the length and certainty of the calibration and validation periods prevented confident assignment of the relative warmth of century-scale temperatures. We eventually chose “likely” based on such concerns. I also remember that we casually discussed the possibility that this figure would become a prominent result of our chapter, but had no idea that it would receive the level of notoriety it eventually did. I think the wide but improper use of the figure promoted an idea that nothing happened for 900 years, then all of the sudden everything happened, giving a false impression of how climate varies over time.

A more disappointing aspect of IPCC 2001 regarding temperatures of the last 2000 years was that some important work was not included. Specifically, the work of Dahl-Jensen 2000 et al., which I recommended to be included on a number of occasions, was completely missing in this section. At that time, this particular analysis of borehole temperature records from Greenland was probably the most confident assessment of relative regional temperature values over the last millennium. Thus, in at least one location of the northern hemisphere we had high confidence that 1000 years ago there was a relatively long period of warmer temperatures than observed in the most recent decade. And, though Greenland’s temperature may not be tightly connected to that of the entire northern hemisphere, Greenland in and of itself is important in dealing with claims of melting ice and sea level rise.

If Greenland were indeed warmer in the relatively recent past, as several proxy records indicated, what was its condition then? Was it melting around the edges in those earlier, warmer centuries as it appears to be melting now in our present cooler temperatures? I believe the IPCC 2001 missed an opportunity to show a more complex picture of climate variability on the planet by excluding this information in 2001.

Sharing data and computer code

Dr. Roy Spencer and I created the first satellite-based temperature dataset for climate studies in 1990. At present we are working on improvements for the 8th adjustment to the dataset brought about by the divergence of the most recent two satellites. Of the 7 previous changes in methodology, two were discovered by other scientists while the other 5 were discovered by us. Satellite instruments and data are complicated and affected by processes which no one really understands completely. Since we cannot go back in time with better instruments, we have to study the ones that were in orbit then and do the best we can to understand how confounding influences affect the measurements.

The computer code we employ consists of 6 complicated programs which at times run sequentially on 3 different machines. The raw datafiles are enormous. When asked, we have shared with others parts of the computer code that were important to understanding how our methodology worked as well as intermediate products which served as a test to check that are methodology was doing what it was intended to do.

When asked, we provided Remote Sensing Systems (RSS) a section of our code which calculated part of the adjustment for the satellites' east-west drift as well as files with the actual values of the adjustment to be sure that our intention in the code and the output matched. They believed our accounting of this particular adjustment was incorrect. Frankly, this was a difficult process from a personal standpoint. By sharing this information, we opened ourselves up to exposure of a possible problem in the code which we had somehow missed. Or worse, a simple disagreement which would lead to arguments about obscure technical aspects of the problem might arise for which there was no simple answer. However, and more importantly, if there was a problem, we certainly wanted to know about it and fix it.

Not knowing the outcome of their work, I received a request from RSS for permission to publish one of the files that we had sent to them. In my formal scientific response I wrote, "Oh what the heck" … " I think it would be fine to use and critique … that's sort of what science is all about."

And so it was that in August 2005 RSS published a clear example of an artifact in our adjustment procedure which created erroneous values in our tropical temperature trend (Mears and Wentz 2005). In *Science* magazine the following November we published information about our now-corrected temperatures and expressed our gratitude to RSS for discovering our error (Christy and Spencer

2005, below). The UAH dataset is better as a result. RSS has also generated a set of satellite temperature products which still differ from ours in some aspects and explanations of those differences are being explored and documented in soon-to-be published material.

The NAS report on temperature reconstructions made the point that when datasets and methods are fully exposed to independent eyes the results will carry more confidence within the scientific community. As best I can tell, this practice was not followed in the MBH99 situation, leading to the conflicts of the past few years.

This brings me back to the CCSP and the evaluation of climate model projections. It was a requirement in the CCSP that all observational datasets used in the report be publicly available in easy-to-access format. Some of us thought the same requirement should be applied to the time series of the global and tropical averages from the climate model simulations, especially since those results had already been published the year before.

In a curious email debate, those who did not want public access given to the climate model averages prevailed. I've encountered this asymmetry before in the field of climate science in which it has typically been very difficult to obtain climate model output in a useful format if at all. Progress has been made with the archiving of the "Climate of the 20th Century" model output at the Dept. of

Energy's Lawrence Livermore Laboratory, but the effort required to retrieve commonly used climate variables is still almost Herculean. Most investigators do not have the infrastructure and personnel to spend time acquiring the huge raw datafiles and then climb a very steep learning curve to process those files into the something useful.

Further, it appears to me that climate model evaluation to this point has been performed mostly by the modelers themselves. It is my view and recommendation that policymakers would learn much from independent, hard-nosed assessments of these model simulations by those who are not directly vested in the outcome. Some of this is going on, but the level of support is minimal.

<u>Science Panel Members – Vested or Non-Vested?</u>

This leads me to another point regarding the CCSP and the NAS reports. In the case of the CCSP report, we as authors were ourselves the builders of the datasets or those who directly performed climate model simulations and evaluations. The process of selecting words to describe the conclusions sprang from those who arguably had strong vested interests. On the other hand, the NAS report of surface temperature reconstructions was written by experts in climate, but who, as we say in Alabama, did not "have a dog in that fight."

After experiencing both situations in the past year, I prefer the approach of the National Academy of Sciences where, in theory, a better chance of producing unbiased and more critical statements is likely.

Global Warming

That greenhouse gases are increasing in concentration is clearly true and therefore the radiation budget of the atmosphere will be altered. In response, the surface temperature should rise due to this additional forcing. In our observational work however, we have not been able to show clear support for the manner or magnitude of this response as has been depicted by the present set of climate models (Christy, 2002, Christy et al. 2006a, Christy and Norris 2006, Christy et al. 2006b).

For policy makers this is an important point, as detailed in my testimony last week (20 July 2006) before the House Committee on Government Reform. We cannot reliably project the trajectory of the climate for large regions within the U.S. for example. It would be a far more difficult task to reliably predict the effects of a policy that altered by a tiny amount the emissions which act to enhance the greenhouse effect. Simply put, we cannot say with any confidence to you or to the American tax payer that by adopting policy X we will cause an impact Y on the weather of the climate system. The basic problem is that if policy X is similar to those being proposed today, the impact on emissions will be essentially imperceptible and thus the attempt to measure or predict its consequence on the climate will be essentially impossible.

To understand the scale of what we are dealing with the following serves as a rough example. We know that we on Earth benefit from 10 terawatts of energy production today. To achieve a reduction of the CO2 representing 10% (1 terawatt) of that production we would need 1,000 nuclear power plants now (1 gigawatt each). Massive implementation of wind and solar does not achieve this result and would not provide the baseload power needed by economies today in any case. (They of course are worthy of investment if costs are reasonable.) Thus, to have a 10% impact on emissions from energy (that is growing at the same time) will require a tremendous and difficult and expensive restructuring of energy supplies.

I believe we will slowly decarbonize energy production and eventually this issue will fade away. But that path of decarbonization should be done with care, being aware of where we are in human economic development as described below.

(However, there are other reasons, such as energy security, which may drive the nation to a different mix of energy sources for which economic outcomes may be more confidently predicted.)

Energy Policy

What I find disturbing in the policy sphere is the demonization of energy and its most common by-product, carbon dioxide (CO_2). It is difficult for me to call CO_2 a pollutant when as an atmospheric gas it is the source of life on the planet. The long history of CO_2 decline over the last millions of years is thought to have been leading to a slow starvation of the biosphere because CO_2 is, simply put, plant food.

But, as importantly, the extra CO_2 we have put in the atmosphere represents tremendous improvements in health, longevity and quality of human life. I suspect half of us in this Hearing room would not be here but for the benefits wrought by affordable and accessible energy. Energy has delivered to us longer and better lives. Energy use is not evil.

I feel I have some expertise not common to the average scientist that I believe is important in this whole discussion of energy and climate change. In the 1970's I taught science and math in Africa as a missionary teacher. I saw the energy system there. The "energy source" was wood chopped from the forest. The "energy transmission" system was the backs of women and girls, hauling the wood a U.N.-estimated average of 3 miles each day. The "energy use" system was burning the wood in an open fire indoors for heat and light. The consequence of that energy system was deforestation and habitat loss while for people it was poor respiratory and eye health. The U.N. estimates 1.6 million women and children die each year from the effects of this indoor smoke.

Energy demand will grow, as it should, to allow these people to experience the advances in health and quality of life that we in the U.S. enjoy. They are far more vulnerable to the impacts of poverty, water and air pollution, and political strife than whatever the climate does. I simply close with a plea, please remember the needs and aspirations of the poorest among us when energy policy is made.

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Correcting Temperature Datasets

We agree with C. Mears and F. J. Wentz ("The effect of diurnal correction on satellite-derived lower tropospheric temperature," 2 Sept., p. 1548; published online 11 Aug.) that our University of Alabama in Huntsville (UAH) method of calculating a diurnal correction to our lower tropospheric (LT) temperature data (v5.1) introduced a spurious component. We are grateful that they spotted the error and have made the necessary adjustments. The new UAH LT trend (v5.2, December 1978 to July 2005) is +0.123 K/decade, or +0.035 K/decade warmer than v5.1. This adjustment is within our previously published error margin of ± 0.05 K/decade (*1*).

We agree with S. C. Sherwood *et al.* ("Radiosonde daytime biases and late–20th century warming," 2 Sept., p. 1556; published online 11 Aug.) that there are significant, progressively colder biases in stratospheric radiosonde data, as we and others have noted (*1*, *2*). We further agree that many daytime radiosondes are plagued by spurious cooling in the troposphere as well (*3*). However, there are also instances in which spurious warming occurs in both day and night soundings. Such a circumstance is not properly accommodated by the day-minus-night (DMN) procedure, a possibility mentioned by Sherwood *et al.*, but not specifically addressed. For example, when the Australian/New Zealand network, prominent in the Southern Hemisphere in Sherwood *et al*'s Report., switched instrumentation from Mark III to Vaisala RS-80, both day and night warmed approximately 0.4 K [(*3*, updated], with tropospheric night readings warming more than day readings. On the basis of this relative difference, the DMN method assumes that a correction for spurious cooling should be applied, when in fact the real error is large and of the opposite sign.

DMN values are useful indicators for pointing out radiosonde changes, but they are often not useful in assessing magnitudes and in this case overestimate the trend. Further, the DMN-adjusted tropospheric trend for 1958–97 of +0.253 K/decade for the 75% of the globe south of 30°N is more than 2.5 times that of the surface (+0.092 K/decade) and thus very likely to be spuriously warm. [Note that B. D. Santer *et al.* ("Amplification of surface temperature trends and variability in the tropical atmosphere," Reports, 2 Sept., p. 1551; published online 11 Aug.) indicate a ratio less than 1.4.] Direct, site-by-site comparisons between radiosondes and UAH LT data at 26 U.S.-controlled stations (nighttime only) from tropics to polar latitudes yield a difference in trends of less than 0.03 K/decade, showing consistency with the more modest UAH LT trends (*1*) [(*3*), updated through 2004].

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