Response to the Hon. Jay Inslee

Question 1. I assumed that the anomaly for 2009 would be in the same range as the 2008 anomaly because of the La Nina (cold phase of El Nino) conditions extant at the time of my testimony and NOAA consensus forecasts that the ENSO index would (will) remain slightly negative for calendar year 2009. This is also consistent with Keenlyside et al. (*Nature*, 2008), who projected continued relatively low temperature anomalies in both the North Atlantic and Tropical Pacific for several years after their publication.

Question 2. The IPCC models fits are retrospective and largely driven by a combination of radiative effects from sulfate aerosol and greenhouse gases. As is obvious from the IPCC report, there is a very large uncertainty with the aerosol effect, nearly two watts/meter squared. This makes fitting the observed record rather easy, and has been commented on by several individuals. I find the candor of the *second* IPCC report more accurate, which stated that GCM's tended to predict too much warming unless a sulfate cooling is assumed OR the sensitivity of temperature to carbon dioxide has been overestimated. That sensitivity is very hard to deconvolve from with *a priori* logic, as has been attempted, because the changes that we are inducing today are quite different from those in the prehistoric climate. Rather, my approach is to let the sensitivity speak for itself. After all, we have increased the atmospheric concentration of carbon dioxide by about 38% over preindustrial, and there is additional positive forcing from methane and chloroflurocarbons.

I think it is logical to assume, however, that the functional form of the models is largely correct. As you know, the response of temperature to changes in carbon dioxide is logarithmic, while the change in concentration is (in all but the politically unrealistic B2 scenario) exponential. It is not difficult to see why so many models tend to produce linear or quasi-linear future warmings.

Indeed, this is what I took advantage of in my testimony. It is very obvious that the warming since 1977 is best fit by a straight line. It is also obvious that the central tendency of the A1B models is also a straight line, especially in the near term. Consequently, the overlap between the two, and the fact that both rates are constant indeed allows for a robust test based upon the distribution of model results. I must tell you that I was disappointed that I was not given enough time to answer your rather strong criticism of my analysis, but I also understand the nature of the process. So I hope this clears that up!

My work compared the HadCru3 (East Anglia, Hadley Center, "IPCC") temperature history with the A1B scenarios. It was prepared specifically for my testimony, for which, as I am sure you know, I had a total of four work days for preparation. Fortunately, that record is the one most cited through the history of the IPCC and makes for an apples-to-apples comparison. I would like to look at other scenarios and am doing this in my so-called spare time with an eye towards publication as soon as it is done. My working hypothesis is that the only scenario—oddly enough—that will

accommodate the IPCC temperature history within the timeframes analyzed will be B2. In other words, even though we have A1B concentration changes going on, we are getting a B2 response—further evidence that the sensitivity has been overestimated.

The IPCC temperature record is also the most transparent one—at least as can be gleaned from the background literature. It has been subject to continual upwards revision detailed in my recent book "Climate of Extremes", but it does not seem as unstable as Hansen's GISS record, which really has a lot of unexplained and quirky changes.

Question 3. While you state that "Temperature is only one of the possible outputs by which one could evaluate the results of a climate model", it is the *driving* metric. Everything else follows. So I believe it is best to look at that. With regard to the studies you mention, there is an interesting disconnection. Sea-ice records are only comprehensive back to 1979 with the advent of satellite coverage, and it is clear that there is a statistically significant negative trend in the northern hemisphere and a statistically significant positive one in the southern. The combined effect is that we are currently right around the 1979-2000 average, given by the University of Illinois' *Cryosphere Today*. Given that the record begins right at the end of the coldest period in the arctic record since the early 1920s, the overall stability of the global oceanic cryosphere should be encouraging, not discouraging. We have warmed some .48degC (trended value, IPCC record) and in the global cryosphere have precious little to show for it. This is one major reason why I think it is important to proceed cautiously on this issue, despite some very loud voices arguing otherwise.

Response to the Hon. Peter Welch

Thank you for your follow-up questions from the hearing entitled *The Climate Crisis: National Security, Public Helath, and Economic Threats.*

My testimony at the hearing focused on a comparison of the distribution of modeled warming trends from the IPCC's midrange suite of general circulation models versus observed trends of from five to 20 years in length in the HadCru3 (Hadley Center, East Anglia "Climate Research Unit", or "IPCC") temperature history. I concluded, based upon that analysis, that the observed trends are largely lying on or beyond the 95% confidence level of this suite of models, which would normally be grounds for rejection of those models. I advised that climate scientists should work to revise those models for better fit, as calculations of the costs and benefits of various climate policies require acceptable models.

I am sending as a separate file my revised C.V., which reflects 2008 and 2009 publications. This is a standard academic C.V., which I submitted along with my testimony and federal grant disclosure form in accordance with Committee rules. A standard academic C.V. includes all peer-reviewed publications, symposium presentations, books and book chapters, but does not included nonacademic publications like op-ed articles or web postings, which are open-source materials.

As I indicated in my testimony at the hearing, my testimony represented no official position of the Cato Institute or the University of Virginia and was tendered as an individual statement under the tradition of academic freedom. Accordingly, I received no specific compensation from any source for the testimony I provided.

Response to the Hon. Joe Barton and the Hon. Fred Upton

Trenberth's first statement that "there are no predictions by IPCC at all" is true. Instead, proponents of IPCC's climate model output confuse "predictions" with "scenarios". The two are quite different. It is painfully obvious to anyone with a cursory knowledge of history that projections of future energy type or use in the 100-year time frame are simply unreliable. To call them "educated" guesses gives far too much credit. As an example, consider how different this world is from 100 years ago. In 1909, who would have honestly anticipated—and had the science to back it up—thernmonuclear explosions, transport of a billion people by aircraft, or a small box in your pocket that can access virtually all the information there is in the world?

Consequently, projections for our societal energy structure some 100 years from today, which are the basis for the IPCC's forecasts of temperature change, are, not to put too fine a word on it, *silly*. Basing our Nations' energy course upon such conjecture, attempting to manipulate it with financial incentives, or subsidizing politically favored technologies of any type with significant resources seems foolhardy.

With regard to climate, given that all change from climate models is driven by change in the energy input, it would seem that any projection for 100 years out must be viewed with suspicion.

Question #2: I continue to be mystified by the "tipping point" notion. As an example, note the dozens of climate model/scenario combinations illustrated on page 763 of the 2007 Working Group I report of the United Nations' Intergovernmental Panel on Climate Change (IPCC). It is very clear from looking at these (I included one scenario/multiple model combination in my testimony) that there are no forecast "tipping points", which would appear as a large discontinuity or discontinuities over time. Consequently, if Dr. Schrag and Mr. Woolsey want to speak of "tipping points", that is fine, but it is not within the model consensus of the IPCC. So, in order to do so with confidence, they must somehow invalidated the entire suite of IPCC models.

This issue has also arisen with regard to the so-called "synthesis report" of the U.S. Climate Change Science Program (CCSP), which has gone through two reviews. I noted the use of the words "tipping point" in bullet-point type material at the beginning of the report (i.e. the only part that is likely to be read by a busy non-expert), but then searched the entire body of the report for "tipping point" in an attempt to find the basis for such an assertion. In fact, in both drafts, the words "tipping point" never appear in the subsequent text.

Unless something has more backing than a mere philosophical hand-waving or editorial assertion, I think it is wise for policymakers to stay away!

Question 3. It is a highly dubious exercise to predict regional climate changes in the United States based upon global averages. However, it is more appropriate to examine what *has* happened in the United States as global temperatures warmed for two periods in the 20th century.

In general, precipitation increased over the 20th century, by about 10%, or roughly three inches over the century. Even using somewhat debatable national temperature histories, the amount of concurrent warming was far to little to largely evaporate this increased precipitation. This means that the surface of the United States is, by and large, wetter than it was 100 years ago. This, in turn, and assuming no other great changes, that the nation is greener than it was and produces more food than it would had there been no change.

Question 4. I believe that the models used by the IPCC and the CCSP are useful inasmuch as they tend to predict constant (rather than exponentially increasing) rates of warming. However, the frequency distribution of the IPCC midrange models, as noted in my testimony, indicates that they are at or beneath the normal confidence limits that science uses as a test of a model or a hypothesis. This almost certainly means that the average warming rate predicted by the midrange emissions models is an overestimation, which in turn defuses much of the alarm that is currently associated with this issue.

The lack of confidence that we must place in these models, given the near-equivalence between their 95% confidence range and the IPCC's Hadley Center temperature record, means that they are nott confident estimators of costs and benefits of climate change versus climate policy. I wish this were not true, but it is.

Question 5. The answer for this follows from Questions 3 and 4, above. The short answer is that these models are not working well enough to provide confident answers about local and regional changes in individual weather elements.

Question 6. If an increase in recent droughts were predicted by climate models projecting recent warming, then the models would be in error, at least for regions of the earth where we have good historical precipitation data (which basically means Europe and North America). In both Europe and North America there is no increasing tendency towards persistent drought. Clearly, the most severe and extended drought period in the instrumental record in North America was in the 1930s. Few if any people would seriously related that drought to increases in atmospheric carbon dioxide, for if that were true, the frequency of severe drought would have to be dramatically increasing in recent decades. It is obvious from the history of the Palmer Drought Severity Index over the United States (data available from the U.S. National Climatic Data Center) that there is simply no trend in this important variable back to when systematic records began some 113 years ago.

Question 7. Our climate policy should be based upon rates of change, particularly in vulnerable environments. However, we really don't have accurate models to measure

this at this point in time. Absent any evidence for dramatic change (i.e. the "tipping point" notion discussed above) it seems prudent to wait for better information rather than to bill a large (and unspecified) amount of expenditures to taxpayers with little or no estimate of what benefits, if any, will accrue.

Indeed, if climate change turns out to be much more severe than is currently indicated (given observed rates of warming that tend to be below modeled values), it would seem prudent to have saved money that can be used for investment and adaptation, rather than having spent that money in a futile attempt to stop or something that couldn't be significantly changed.

Whatever the climatic future holds, it should be clear that a vibrant economy contains more capital for investment by individuals in the energy technologies of their choice, and that these investments will be made in larger amounts if such capital is not taken away before it is needed.

Question 8. It is simply impossible to reduce global carbon dioxide concentrations with or without China and India. If one wants to significantly change the rate of increase (an increase in concentration that will still occur for at least another 50 years), one must include China and India in any schedule of binding targets and timetables for emissions reductions. Further, the limits on their emissions must be as severe as those that are being proposed for the United States—otherwise their emissions (as well as their job growth) will swamp that of the now-industrialized world.

Question 9. If all of the world's nations with Kyoto "obligations" met them, the reduction in planetary warming would be 0.07°C per fifty years. This assumes that the sensitivity of temperature to a doubling of atmospheric carbon dioxide is 2.5°C. This is based upon a calculation published by Tom Wigley of the U.S. National Center for Atmospheric Research in the journal *Geophysical Research Letters* in 1998.

For the second part of this question, I assume you are asking what the effect on global warming is if the United States reduces its emissions to zero while everyone else continues "business as usual". Using the Wigley calculation as a basis, this would result in approximately 0.11°C less warming in 2050, and 0.15°C less in 2100.

Looked at another way, how quickly would a complete and immediate shutdown of all U.S. emissions be "made up" by the rest of the world? The answer is in about 6-8 years.