FOLLOW-UP QUESTIONS FROM SENATOR JEFFORDS
SENATE COMMITTEE ON ENVIRONMENT AND PUBLIC WORKS
7/29/03 HEARING ON CLIMATE HISTORY AND MERCURY SCIENCE

To Dr. Michael Mann,
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1) Is it your understanding that during the mid-Holocene optimum period (the period from 4000-7000 B.C.) that annual mean global temperatures were more than a degree C warmer than the present day?

This is an oft-repeated but patently false claim. Dr. Legates, who has no established expertise in the relevant field of paleoclimatology, indeed asserts that temperatures were warmer at this time. In fact, not only is that not the consensus of the paleoclimate research community, but just the opposite is believed to be true of global annual mean temperatures at this time. Paleoclimate experts know that the mid-Holocene warmth centered roughly 5000 years ago was restricted to high latitudes and certain seasons (summer in the Northern Hemisphere and winter in the southern hemisphere). Because much of the early paleoclimate evidence that was available (for example, fossil pollen assemblages) came from the Northern Hemisphere extratropics, and is largely reflective of summer conditions, decades ago some scientists believed that this was a time of globally warmer conditions. It is now well known that this is not the case. More abundant evidence now demonstrates, for example, that the tropical regions were cooler over much of the year. All of these changes are consistent with the expected response of surface temperatures to the known changes in the Earth's orbital geometry relative to the Sun during that time period and associated climate feedbacks, as detailed in peer-reviewed scientific publications [e.g., Hewitt, C.D., A Fully Coupled GCM Simulation of the Climate of the Mid-Holocene, *Geophysical Research Letters*, *25* (3), 361-364, 1998; Ganopolski, A., C. Kubatzki, M. Claussen, V. Brovkin, and V. Petoukhov, The Influence of Vegetation-Atmosphere-Ocean Interaction on Climate During the Mid-Holocene, *Science*, *280*, 1916-1919, 1998].

Climate model simulations indicate quite good agreement with paleoclimate evidence now available. These models calculate that global annual average temperatures were probably a few tenths of a degree C cooler than today during this time period [Kitoh, A., and S. Murakami, Tropical Pacific Climate at the mid-Holocene and the Last Glacial Maximum simulated by a coupled ocean-atmosphere general circulation model, *Paleooceanography*, *17* (3), (19)1-13, 2002.]. That's a far cry from the very out-of-date claim made by Legates. Legates' comments regarding climate changes over the past 1000 years reflect a similar lack of familiarity with a whole body of paleoclimate research, especially with the new insights gained through the augmented research program, during the past decade.

2) Why only focus on the past 1000 or 2000 years and not further back?

Large changes in climate certainly occurred in the distant past. If we look million years back in time, dinosaurs were roaming the polar regions of the Earth, and the globe was several degrees warmer than today. Carbon dioxide levels were probably several times their current level, slowly having attained such high levels due to changes in the arrangements of the continents ('plate tectonics') which influence the volcanic outgassing of carbon dioxide from the solid Earth. These changes occurred on timescales of tens of millions of years. Going back 10,000 years ago, large ice sheets existed over North America due to natural changes that occur in the Earth's orbit around the Sun on timescales of tens of thousands of years. Trying to study distant past climates for insights into modern natural climate variability is hampered by the fact that the basic external constraints on the system (the continental arrangement, the geometry of the Earth's astronomical orbit, the presence of continental ice sheets--what we call the 'boundary conditions') were significantly different from today. Focusing on the evolution of climate in *the centuries* leading up to the 20th century (i.e., the past 1000 to 2000 years) provides a perspective on the natural variability of the climate prior to the period during which large-scale human influence is likely to have occurred, yet modern enough that the basic boundary conditions on the climate system were otherwise the same. This provides us, in essence, a 'control' for diagnosing whether or not recent changes in climate are indeed unusual. Moreover, only during the past 1000-2000 years do we have adequate networks of proxy climate data with the required (annual)

resolution in time to compare and validate against modern instrumental records. Reliable quantitative reconstructions of large-scale surface temperature patterns further back in time are thus not, at present, possible.

3) One of the Northern Hemisphere temperature reconstructions in your Figure 1 (the green curve from a paper by Esper and colleagues from *Science* in 2001) shows larger swings in past centuries, marginally outside the uncertainty bounds of the other reconstructions and model simulations. Does this indicate internal inconsistency in our knowledge?

There is no inconsistency. *Esper et al* noted, in their paper, that their estimate, unlike that of my colleagues and mine (the Mann/Bradley/Hughes or "MBH" reconstruction), was not representative of the entire Northern Hemisphere. They explicitly noted this in their paper, where they pointed out the likely reason for differences is that the MBH reconstruction represents the full Northern Hemisphere (tropics, subtropics, and extratropics) while the Esper et al reconstruction only represents the restricted extratropical continents. In fact, in a *Science* article that appeared in the same issue as the Esper et al paper [Briffa, K.R. and T.J. Osborn, Science, Blowing Hot and Cold, 295, 2227-2228, 2002] Briffa and Osborn noted that much of the difference was due to an arguably inappropriate scaling that Esper et al used, and an inappropriate comparison of summer vs. annual temperatures:
'when we regressed the record of Esper et al. *against non-smoothed data (see the figure), this difference [with MBH] was reduced to about 0.4ºC. Recalibrating both curves against year-by-year warm season temperatures reduces this difference further to about 0.35ºC.'*
As shown in the "Eos" article discussed in my testimony, which represents a consensus of the leading researchers in the field [Mann, M.E., Ammann, C.M., Bradley, R.S., Briffa, K.R., Crowley, T.J., Hughes, M.K., Jones, P.D., Oppenheimer, M., Osborn, T.J., Overpeck, J. T., Rutherford, S., Trenberth, K.E., Wigley, T.M.L., On Past Temperatures and Anomalous Late 20th Century Warmth, *Eos*, 84, 256-258, 2003], a proper scaling of *Esper et al* record prior to comparison with other estimates, shows it only marginally outside the error estimates of the MBH reconstruction and the many other estimates that are in agreement with it. As noted in two articles in *Science* that shortly followed the Esper et al paper [Mann, M.E., Hughes, M.K., Tree-Ring Chronologies and Climate Variability, *Science*, 296, 848, 2002; Mann, M.E., The Value of Multiple Proxies, *Science,* 297, 1481-1482, 2002], it is likely that the emphasis of the Esper et al reconstruction on only the summer season and the extratropical continental regions, provides a biased estimate of the true pattern of annual, hemisphere-wide temperatures in past centuries, explaining the small differences between this estimate and other estimates. This conclusion has been verified in recent modeling studies [Shindell, D.T., Schmidt, G.A., Miller, R., Mann, M.E., Volcanic and Solar forcing of Climate Change During the Pre-Industrial era, *Journal of Climate*, in press, 2003]. It is thus clearly disingenuous when contrarians make the argument that the *Esper et al* result is in conflict with the mainstream conclusions of the climate research community with regard to the history of Northern Hemisphere mean annual temperature variations over the past millennium as embodied, for example, in the *Eos* article.

4) As a climatologist, can you explain what kind of quantitative analysis it takes to determine whether or not the last 50 years has been unusually warm compared to the last 1000 years?

Well, such an analysis requires the careful use of proxy data, because we don't have widespread instrumental temperature records prior to the mid 19th century. By 'careful use', I mean that one must first establish that the records actually resolve the changes of the past 50 years. This typically requires annually-resolved proxy records or the very circumspect use of records with decadal resolution. One must not, as in the Soon and Baliunas' studies, use records that do not resolve the trends of the past few decades. One must also establish the existence of an actual temperature signal in the available proxy data before using them to reconstruct past temperature patterns, and one must properly synthesize regional data, which typically all show different tendencies at any given time, into an estimate of the average temperature over the entire hemisphere or globe. There are a number of ways of performing such a synthesis, from the sophisticated pattern reconstruction approaches my colleagues and I have described in the technical literature, to the relatively straightforward compositing approach that many other paleoclimatologists, (including myself) have also used. In all cases, the estimates based on the proxy data must be calibrated against modern instrumental temperature measurements, to allow for a quantitative estimate of past temperatures. The estimate must then be independently verified or, what we call, 'cross-validated,' by showing that it independently

reproduces earlier instrumental data that were not used to calibrate the estimates. Finally, uncertainties must be diagnosed based on how well the reconstruction describes actual available instrumental measurements. Once such steps have been taken, it is possible to compare the recent instrumental record to the reconstruction within the context of the uncertainties of the reconstruction. This latter comparison allows us to gauge whether or not late 20th century temperatures are anomalous or not in a long-term context. The conclusion from legitimate such studies that late 20th century warmth is indeed anomalous in a millennial or longer-term context has been shown to be quite robust with respect to the details of the data set used, or the methodology used (as shown in exhibit 1 in my testimony, the first figure of the "*Eos*" piece). It is noteworthy that the Soon and Baliunas paper satisfies *none* of the required standards for a 'careful use of proxy data' specified above.

5) Do you claim that appropriate statistical methods do not exist for calibrating statistical predictors, including climate proxy records, against a target variable, such as the modern instrumental temperature record?

No. The statement belies a centuries-old field of statistics known as "multivariate linear regression" in which a set of candidate "predictors" (such as proxy data) are statistically related to a target variable or "predictand" (such as the instrumental temperature record) during a common interval of overlap (e.g., the 20th century). If done properly, this statistical method isolates the temperature information that is contained within the proxy data, and uses that information to reconstruct past temperature patterns from the proxy data. It is also well known to those properly trained in statistics that the "regression model" must be independently "validated" by showing that it successfully reproduces independent data (e.g. longer-term instrumental temperature records) that were not used in constructing the statistical model itself. The estimates by Mann and colleagues embrace each of these fundamental statistical principles..

Legates in his testimony seemed to claim that a composite estimate (e.g. of Northern Hemisphere mean temperature) should somehow resemble *each* of the individual predictors (e.g. the various regional temperature estimates). Such a result is, in general, a statistical impossibility. There is a basic theorem in statistics known as the "central limit theorem" which indicates a general tendency for a composite (i.e. average) of a large number of different individual estimates to cancel out in terms of the pattern variation and amplitude of variability evident in the individual estimates. If Legates were indeed (as he claims to be) familiar with the instrumental surface temperature record, he would know, for example, that individual instrumental thermometer records available for particular locations over the globe during the 20th century show very little in common with the 'composite' series constructed by averaging all of the individual records into a hemispheric or global estimates. Because season-to-season and year-to year fluctuations in the climate at regional scales often result from shifts in the atmospheric circulation, not every location experiences the same variation in any given year; for example, this summer, the western US and Europe are anomalously warm, while the East Coast is anomalously cool. The average series reflects a tendency for a cancellation of the various "ups" and "downs" in the different individual series that often occur at different times. This is simply a statement about the instrumental record itself, and requires no use of proxy data at all. It is discouraging that Legates and colleagues haven't performed even such simple analyses with available instrumental data that would expose the fundamental flaws in their supposed 'statistical' reasoning.

In this context, it should be noted that Legates testimony seriously misrepresents the statistical analyses used by Mann and co-workers. In his testimony, Legates claimed that my collaborators and I replaced the proxy data for the 1900s by the instrumental. The assertion is simply factually incorrect. Any reader of our published work knows full well that our proxy-based temperature reconstruction extends well into the late 20th century, through 1980 (the vast majority of published high-resolution climate proxy data are not available at later times than this). It is shown in our work that the reconstruction independently reproduces estimates from the instrumental surface temperature data record back through the mid 19th century and, in certain regions, back through the mid 18th century (i.e., our regression model is "validated" in the manner discussed above). Our Northern Hemisphere average temperature series reconstructed from proxy data is shown to agree with the instrumental Northern Hemisphere average record over the entire interval available for comparison (1856-1980). This successful validation of the reconstruction, furthermore, allows us to compare the proxy-based reconstruction to the entire instrumental record (available through 1999 at the time of our publication), taking into account the uncertainties in the reconstruction.

Legates further claim that we present the instrumental record without uncertainty is disingenuous. If Legates, for

example, were familiar with studies of the instrumental surface temperature record, he would understand that the uncertainties in this record during the 20th century are minimal compared to the uncertainties shown for our reconstruction [see e.g. Figure 2.1b in Folland, C.K., Karl, T.R., Christy, J.R., Clarke, R. A., Gruza, G.V., Jouzel, J., Mann, M.E., Oerlemans, J., Salinger, M.J., Wang, S.-W., Observed Climate Variability and Change, in *Climate Change 2001: The Scientific Basis,* Houghton, J.T., et al. (eds.), Cambridge Univ. Press, Cambridge, 99-181, 2001.]. Furthermore, all scientists with a proper training in statistics know that uncertainties add "in quadrature". In other words, you have to square them before adding them. This means that the relatively small uncertainty in the instrumental record makes a relatively small contribution to the total uncertainty. Legates claimed in his testimony that including the uncertainty in the instrumental record, which he estimates as 0.1^{o}C would change the conclusions expressed by us and other mainstream climate scientists that the 1990s are the warmest decade in at least the past 1000 years within estimated uncertainties. This claim is very misleading for several reasons, First, the standard error in Northern Hemisphere mean annual temperatures during the 1990s is far smaller than the amount cited by Legates [see again Folland et al, 2001 cited above]. Even more problematic, however, Legates claim indicates a fundamental misunderstanding of the statistical concepts of standard error and uncertainty. The shaded region shown along with the Mann et al reconstruction (and other similar plots shown in recent articles such as the aforementioned "*Eos*" article, and the IPCC report) indicate two standard error intervals. The decade of the 1990s is roughly two standard errors warmer (i.e., about 0.4^{o}C)]than any decade prior to the 20th century in the reconstruction. Based on a one-sided test for anomalous warmth, this translates to a roughly 97.5% level of significance. Modifying the uncertainties to include the small additional contribution due to uncertainties in the instrumental record itself would modify this only slightly, and would not lower the significance level below the 95% level . Though there is no such thing as an absolute estimate of uncertainty, despite Legate's implications to the contrary, a 95% confidence is often adopted as an appropriate criterion for significance. Legates statement that including instrumental contributions to the uncertainty would change the conclusions is thus clearly false.

6) In determining whether the temperature of the "Medieval Warm Period" was warmer than the 20th century, does your work analyze whether a 50-year period is either warmer or wetter or drier than the 20th century? Is it appropriate to use indicators of drought and precipitation directly to draw inferences of past temperatures?

No, the work of me and my colleagues does not follow the flawed approach used by Soon and Baliunas. It is fundamentally unsound to infer past temperature changes directly from records of drought or precipitation. The analysis methods used by my various collaborators and I (e.g. the 13 authors of the recent article in Eos) employ standard statistical methods for identifying the *surface temperature* signal contained in proxy climate records, and using only that temperature signal in reconstructing past temperature patterns. By contrast, Soon and Baliunas simply infer evidence for warm "Medieval Warm Period" or cold "Little Ice Age" conditions from the relative changes in proxy records often reflective of changes in precipitation or drought, rather than temperature. It is difficult to imagine a more basic mistake than misinterpreting hydrological evidence in terms of temperature evidence. This fundamental shortcoming in their approach is identified in the *Eos* article referred to in my testimony that was written by thirteen leading climate and paleoclimate scientists. Incidentally, the *Eos* piece was peer--reviewed, despite the claim by Legates otherwise in his testimony--an associate editor with training in the particular field corresponding to the submitted piece ("Atmospheric Science" in this case), reviews the content of *Eos* forum pieces prior to acceptance.

7) Can you compare the quantitative analysis that supports your conclusion that the climate is warming faster in recent years than at any time in the recent past with the analysis done in the Soon literature review?

Technically speaking, there is no actual "analysis" in the Soon and Baliunas review, in that they don't appear to have performed a single numerical or statistical operation upon a single time series at all. They do not provide any quantitative estimates of temperature changes, let alone any estimate of uncertainties. Instead, they claim to interpret the results of past studies mainly by counting the number of studies coming to some conclusion, no matter how they got there and whether there have been later interpretations. Science requires analysis—not just counting studies. Climate scientists whose records they analyzed have gone on record as indicating that Soon and Baliunas

misinterpreted their studies (e.g., the article by David Appell in the August 2003 issue of *Scientific American*) and numerous climate scientists have indicated (see same article) that Soon and Baliunas misinterpreted evidence of drought or precipitation as evidence of temperature changes, did not use records that resolve the climate changes of the late 20th century, and did not take into account whether or not variations in different regions were coincident or not. Soon and Baliunas also neglect undeniable evidence of substantial warming of hemispheric and global surface temperatures during the past few decades. So the Soon and Baliunas analysis fails just about every meaningful criterion that might be applied to determining the validity of an analysis that purports to evaluate current warming in the context of past temperature trends. This deeply flawed study thus contrasts sharply with other rigorous quantitative studies (as discussed in the *Eos* article) performed by numerous other scientists with appropriate training in the fields of climatology and paleoclimatology, which use proper statistical methods for inferring past temperature changes from proxy data, provide uncertainty estimates, and employ appropriate comparisons of current and past trends.

8) What was the earth's climate like the last time that atmospheric concentrations of carbon dioxide were at today's levels or about 370 parts per million (ppm) and what were conditions like when concentrations were at 550 ppm, which will occur around 2060 or so?

We have to go back far into the past to find carbon dioxide levels approaching today's levels. Ice core studies indicate that modern carbon dioxide levels are unprecedented for at least four glacial/inter-glacial cycles: in other words, for more than 400,000 years. Other evidence suggests that carbon dioxide levels are now higher than they have been for at least 10 million years. One probably has to go back roughly 40-50 million years ago (see chapter 3 of the 2001 IPCC working group 1 report) to find CO2 concentrations were in the range of 550 ppm (i.e., roughly double their pre-industrial concentration) and approximately 80 million years ago (i.e., the mid-Cretaceous period when Dinosaurs roamed the polar regions) to find CO2 levels in excess of 1200 ppm (a level that will be reached, at current rates of CO2 increase, within one to one-and-a-half centuries). Proxy evidence available for this period, tenuous though it is, suggests deep ocean temperatures 8-12°C warmer than present. State of the art climate model simulations performed by Bette Otto-Bleisner and colleagues using the National Center for Atmospheric Research (NCAR) global climate model, which incorporate such CO2 levels (and the continental configuration corresponding to the mid Cretaceous period), indicate significantly warmer sea surface temperatures, with tropical sea surface temperatures approximately 4°C warmer and polar sea surface temperatures approximately 6-14°C warmer than present. The simulations indicate an absence of perennial sea ice at even the most polar latitudes.

9) Are you aware of any scientists beside the authors of the Soon et al article who support using "wetness" or "dryness" as indicators of past temperatures, instead of actual temperatures or proxy data that reflects temperatures?

I am not aware of any other scientist who has made the mistake of interpreting paleoclimatic information in this way. As discussed above, trained paleoclimatologists typically use statistical methods to identify the strength of the temperature signal in a proxy record prior to using it in reconstructing past temperature patterns.

10) Is there any known geologic precedent for large increases of atmospheric CO2 without simultaneous changes in other components of the carbon cycle and the climate system?

There is not, to my knowledge, such an example. As discussed above, the geological record shows a clear relationship between periods of high CO2 and relatively high global mean temperatures. The study of the relationship between changes in CO2 and climate in the paleoclimate record is sometimes complicated by the fact that these relationships can be relatively complex during rapid transitions between glacial and interglacial climates such as those that occurred with the coming and going of ice ages that occurs on a roughly 100 thousand year timescale over the past nearly one million years. However, one can turn to periods of time when the climate and CO2 were not varying rapidly, and thus the climate was approximately in an "equilibrium" state, for insights into

the relationship between CO2 and climate. A perfect such example is the height of the last ice age, the so-called "Last Glacial Maximum" or "LGM" centered roughly 25 thousand years ago. At this time, CO2 was substantially lower than today (just below 200 ppm) and global mean surface temperatures were several degrees (about 4C or so) cooler than today. Such relationships between past CO2 changes and global temperature changes typically suggest a "sensitivity" of the climate system to enhanced CO2 of 1.5 to 4.5 C warming for each doubling of CO2 concentrations from their pre-industrial levels, similar to the range of sensitivities found in various climate models.

11) According to a study published in Science magazine recently [B. D. Santer, M. F. Wehner, T. M. L. Wigley, R. Sausen, G. A. Meehl, K. E. Taylor, C. Ammann, J. Arblaster, W. M. Washington, J. S. Boyle, and W. Brüggemann Science 2003 July 25; 301: 479-483], manmade emissions are partly to blame for pushing outward the boundary between the lower atmosphere and the upper atmosphere. How does that fit with the long - term climate history and what are the implications?

This is yet another independent piece of evidence confirming a detectable anthropogenic influence on climate during the late 20th century. This evidence is consistent with evidence for unprecedented surface warming during the past few decades--warming that indeed appears unprecedented for as long as we have records (i.e., for at least a thousand, and probably two thousand years). These changes, moreover, are consistent with predictions from climate models driven by known anthropogenic (human) forcing of the climate.

12) At this hearing, there were a number of calls for "sound science." Could you please explain what it is about the IPCC process that justifies respecting the IPCC results as the very soundest representation of the science of climate change?

The IPCC carries out a process for developing its summarization of the understanding of science that leads to one of the most rigorously peer-reviewed scientific documents in existence. Individual technical chapters are prepared by expert scientific teams that consider the full range of published papers in a subject area. This expert author team then solicits an initial peer review from a large number of other scientists in the field, drawing on those with the full range of views. The reports next go through a much wider review that is open to literally thousands of scientists around the world. Finally countries, NGOs, and professional groups (such as business groups) are provided the opportunity to send in review comments. At each stage, authors consider each comment and document their response. The meticulousness and fairness of the revision process by the authors in response to reviewer comments is evaluated by an independent pair of "review editors" who are themselves top international climate scientists who are not authors of the report itself. The National Academy of Sciences, at President George W. Bush's request, and other national academies around the world have independently reviewed the process and the validity of the scientific findings of the IPCC and endorsed them. To question the IPCC and the IPCC process, as does Dr. Legates thus not only does a disservice to thousands of the world's top scientists, but to the exceptional care and rigor of the process that has led to the unanimous adoption of all of the IPCC's assessments by representatives of the over 150 nations that participate in the IPCC process. The documents are very finely honed and carefully phrased. The scientific studies of those such as Dr. Legates are considered, as are their review comments, and it is terribly disingenuous, not to mention totally unacceptable to the international community, after all of the care and consideration put into these efforts to try to so cavalierly dismiss them.

In his testimony Legate's alleges that the IPCC report misrepresents what is known about climate change in past centuries and that it somehow replaces conventional wisdom with dramatically new conclusions. One must conclude that Legates either did not read the report, or if he did, he did not understand what he read, for if he had he would certainly have to recognize factually incorrect nature of his comments. The IPCC chapter dealing with paleoclimatic evidence discussed the full range of regional evidence described in the peer-reviewed as well as evidence from hemispheric composites that average the information from different regions. The paper by Soon and Baliunas is a dramatic throwback to the state of our knowledge many decades ago, while the IPCC report provides a far more up-to-date assessment of all of the available knowledge regarding past climate change. The Soon and Baliunas papers provide a glaring example of the very "unsound" science that Senator Inhofe claims to be concerned about, as

numerous mainstream climate researchers have now opined in the media, and in the peer-reviewed scientific literature.

13) In your opinion, how do the processes used by the IPCC and the National Academy of Sciences compare to the process used in the publication of the Soon and Baliunas paper and other papers by so-called "contrarians"? In the next IPCC assessment, would you expect that the Soon and Baliunas paper will be considered and cited, putting it into the context of other papers and findings and explaining why it has differences or similarities?

As discussed above, the IPCC is one of most rigorously peer-reviewed scientific documents in existence. By contrast, the contrarians rarely publish in the peer-reviewed literature and when they do, it is not uncommon to discover, as in the case of the Soon and Baliunas paper, irregularities in the peer-review process. Publisher Otto Kinne indeed indicated that the review process at Climate Research "failed to detect methodological flaws" in the Soon and Baliunas paper. I would indeed expect that in the next IPCC assessment, the Soon and Baliunas paper will be discussed and evaluated in the context of other available evidence and in the context of how it is faring in the literature when the IPCC review takes place (e.g., if there are as many criticisms about their work as at present, and they have not been seriously addressed by more careful followup studies by the authors,, I would think their conclusions will be rejected as scientifically unsound. I would not presume to know in detail what the result of the assessment will be, but I believe it fair to assume that the rigorous review provided by the IPCC assessment process will not fail to identify the methodological flaws that appear to have slipped through the cracks in their publication in the journal "Climate Research".

14) Could you explain the IPCC's lexicon for indicating relative levels of confidence and how you would suggest this relates to the information being "real" and "probable"? When IPCC says something is "very likely," just what do they mean?

To avoid the type of misunderstanding that often results from when scientists seek to convey scientific results to a non-technical audience, the IPCC specifically sought to employ a lexicon in which terms such as "likely" or "probable" or "very likely" had specific statistical meanings attached to them. A fairly conservative standard was typically employed in this process. Consider the conclusion in the IPCC report that the 1990s are the warmest decade in at least the past 1000 years for the Northern hemisphere average temperature. This conclusion is based on the fact that the average warmth of the 1990s exceeds that for any reconstructed decade in the reconstructed Northern Hemisphere series. To be more specific, the 1990s warmth exceeds any past decade by two standard errors. This corresponds to a roughly 97.5% probability based on standard statistical assumptions. Probabilities of 90%-99% are termed "very likely" in lexicon typically adopted by the IPCC report. However, this conclusion was offered as only "likely" (corresponding to a 66-90% level of probability) rather than the more stringent "very likely" because it was based on only a small number of independent studies at the time. Since that time, of course, several more studies have affirmed this conclusion, and one might imagine that a more stringent conclusion will be offered in the future. This example nonetheless illustrates the manner by which IPCC adopted conservative standards in their use of terms such as "likely" or "very likely". It is instructive to contrast that standard with the one taken in the Soon and Baliunas paper. The Soon and Baliunas paper does not provide a quantitative estimate of any quantity (such as average Northern Hemisphere temperature), or any assessment of uncertainty. It is thus not possible for the authors to attach any meaningful statement of likelihood or probability to any of their conclusions. They thus provide no basis for judging the validity of any of the claims made in their paper, in striking contrast to the rigorous standards adopted by IPCC, and by the work of my collaborators and me.

15) In his questioning, Senator Inhofe cited results regarding the potential costs of implementing the Kyoto Protocol from the Wharton Econometric Forecasting Association (WEFA). I realize that you are not an economist, but would you please comment as a scientist on the following two points:

(A) Senator Inhofe cited economic projections (e.g., 14% increase in medical costs; real income drop of $2,700 per household) going out a decade or so into the future and without any indication of uncertainty on these estimates implying an accuracy of two-significant figures. Senator Voinovich cited numbers to similar claimed accuracy going out 20-25 years, again without any indication of uncertainty. Could you please comment on what you think are the relative strengths and weaknesses of making climate projections based on use of physical laws versus economic extrapolations and what sorts of relative uncertainty should likely be associated with each type of estimate so that they can be interpreted in a comparative way by decisionmakers?

Indeed, it is somewhat remarkable that politicians who reject the validity, for example, of climate model simulations, which are based on solution of the laws of physics, can so uncritically accept precise economic projections based on economic forecasts based on untestable and unverifiable assumptions governing human decision making, and speculative future scenarios that depend on the unfolding of the political process. Specific numbers of "14%" or "$2,700" are of course entirely dependent on the assumptions that go into such forecasts. Because assumptions about future economic growth and about future government policies are necessarily uncertain, estimates of changes in costs or income from such models must also be quite uncertain, and so should have large uncertainties (or ranges) associated with them. The faith expressed in such poorly constrained economics estimates by the same individuals who express strong skepticism of results from far more physically based and testable climate simulation models, strikes me as a remarkable inconsistency.

(B) Because you may be familiar with the 1997 study by the World Resources Institute entitled "The costs of climate protection: A guide for the perplexed," which explains the important role of assumptions in leading to very different cost estimates from even one economic model, much less among different models, could you explain in a comparative fashion how robust the findings regarding the "hockey stick" behavior of the climate in the various studies carried out by you and fellow investigators may be to variations in the assumptions made. It's clear that varying the assumptions among reasonable possibilities in the economic models can change what is calculated to be a few percent impact on the economy to a small gain; would changes in the assumptions you are making change the indication of strong anthropogenic warming to an indication of human-induced cooling?

The millennial temperature reconstruction (or "hockey stick" as it was termed by former GFDL head Dr. Jerry Mahlman) is based on a rigorously validated statistical model with demonstrated predictive skill based on comparisons with independent data. The primary conclusions drawn from the reconstruction (e.g. the anomalous nature of late 20th century warmth) are based on a conservative appraisal of the uncertainties in the reconstruction, and are not strongly dependent on the assumptions made. The same conclusions have been affirmed now by several other independent empirical and model-based estimates. As discussed above, this contrasts with economic predictions which are necessarily far more sensitive to the assumptions that go into them.

16) In his opening statement, Senator Inhofe concluded that the Soon and Baliunas paper is "credible, well-documented, and scientifically defensible." By contrast, your testimony indicated that the experts in the field do not consider this to be the case. Does one have to be an expert in the field to understand the apparent problems with this paper? If not, could you summarize in terms for non-scientists what the key problems with the paper are?

The mainstream scientific research community has indeed rejected the approach, interpretation, and conclusions advanced by Soon and Baliunas as fundamentally unsound. The major flaws in the analysis, as described in earlier comments in more detail, are basic enough that they can be understood by a non-specialist. In short, the analysis by Soon and Baliunas is unsound because (a) they inappropriately interpreted indicators of past precipitation as evidence of past temperature changes, (b) they did not use an approach which takes into account the simultaneity and lack of simultaneity of variations in different regions, and (c) they did not employ a proper standard for evaluating recent changes (i.e., changes during the past few decades) in the context of past variations. Indeed, they also misinterpreted past published work, and did not provide any quantitative estimates, let alone estimates of uncertainty. It is difficult to find anything of scientific merit at all in their published work.

17) As I understand it, the data that both you and Soon and Baliunas draw from is the same, and it is not a question of the data being the problem. Instead, it is apparently the processing of the data that you are indicating has been done in a substandard way by Soon and Baliunas. Is this correct?

Keep in mind that Soon and Baliunas, unlike my collaborators and I, don't actually analyze any data at all. They simply claim to have 'interpreted' past studies (often incorrectly). Soon and Baliunas refer to a number of proxy data studies that describe data which we employed in our analysis. There are also many proxy data that we used in our study that the Soon and Baliunas do not discuss. However, as alluded to in the question, the real issue isn't what data were used. Numerous independent now-published studies employing widely different assemblages of proxy climate data sets have demonstrated (e.g. as in the *Eos* article discussed earlier) a similar pattern of past variations in hemispheric mean temperature. The real issue with Soon and Baliunas is indeed not the set of studies that they claim to have interpreted, but rather the approach that they took to interpreting those studies. As indicated in previous comments, Soon and Baliunas, unlike mainstream climate researchers, did not employ a method for isolating the actual temperature signal in the proxy records before using the records to draw conclusions regarding past temperature changes. Unlike mainstream researchers, they did not aggregate information in a way that addresses whether or not variations in different regions are simultaneous. Unlike mainstream researchers, they did not analyze the actual modern (late 20th century) warmth in the context of past variations. And finally, they did not even produce a quantitative estimate of past temperature variations, let alone an estimate of uncertainty.

18) The Soon et al literature review has been described as shifting the paradigm away from the "hockey stick" description of global warming. It seems that that review simply attempted to revive an older theory of climate change that has been discarded by NOAA, the USGCRP, the NAS, the IPCC, etc.. Please comment.

The mainstream scientific community has clearly and decisively rejected the Soon and Baliunas papers as scientifically unsound which undermines any claims made by industry-funded special interest groups or politicians, that the papers provide any valid scientific conclusions, let alone the basis for a shift of the "paradigm". Indeed, the Soon and Baliunas papers simply promote a number of long-discredited myths which have been replaced, in recent decades by far more rigorous and quantitative analyses as described by the IPCC, USGCRP, NAS, and other mainstream scientific organizations and funding agencies. In short, the Soon & Baliunas papers simply repackage myths that were discredited more than a decade ago.

19) In an opinion-editorial by former Secretary of Energy James Schlesinger, he suggested that "we have only a limited grasp of the overall forces at work,..." in terms of global climate change. Could you please summarize for us what the scientific community considers the key forces at work over the past 1000 years, how well these estimates are understood, and whether there is a general consistency or inconsistency between the various forcings and the climate estimates that you and colleagues have developed for the last 1000 years?

Mr. Schlesinger's assertions entirely mischaracterize the nature of our scientific knowledge, which has advanced tremendously during the past several decades. In fact, a very large number of peer-reviewed scientific studies have been published in the leading scientific journals such as *Nature* and *Science* in the past two decades elucidating the role of natural and anthropogenic factors in observed climate changes. Physically-based models have been developed and validated against observations, and these models reproduce complex climate phenomena such as El Nino. These same models have been driven with the primary "external" factors that are believed to govern climate variations on timescales of decades and centuries. These external factors include natural factors, such as the modest estimated variations in radiative output of the Sun, which varies by a fraction of a percent over time, variations in the frequency and intensity of explosive volcanic eruptions, which have a several-year cooling effect on the climate through the injection of reflective volcanic aerosols into the stratosphere, and very small changes in the Earth's orbit relative to the Sun that occur on multi-century timescales. These external factors also include the "anthropogenic" influences of increased greenhouse gas concentrations due to fossil fuel burning, changes in the reflective properties of the land surface due to human land use alterations, and the regional cooling effect of anthropogenic sulphate aerosols in certain industrial regions. When driven with these factors, these climate models have demonstrated a striking ability to reproduce observed global and hemispheric temperature trends during the 20th century, as well as longer-term trends in past centuries as reconstructed from proxy data. Such results are nicely summarized in the 2001 IPCC scientific working group report. Mr. Schlesinger would have benefited from a reading of this report, or the follow-up National Academy of Sciences report that endorsed the key IPCC conclusions, prior to writing his op-ed piece, which reflects a decades-old understanding of the state of the science.

20) In Dr. Soon's testimony, he speaks about there being "warming" and "cooling" for different periods. If he did not construct an integral across the hemisphere or a real timeline, can he say anything other than that there were some warm periods and cool periods?

Aside from not adequately distinguishing temperature changes from hydrological changes, Dr. Soon and his collaborators indeed did not even attempt to estimate contemporaneous patterns of past temperature change, let alone an integral across the hemispheric domain to assess hemispheric mean temperature changes. It is unclear what, if any, meaningful conclusions can be drawn from the Soon and Baliunas study.

21) Dr. Soon indicates that "local and regional, rather than global average changes are the most relevant and practical measure of climate change and its impact." Could you please comment on this, including the relative likelihood of identifying a signal of climate change amidst the local fluctuations? In what sense might local changes be the most practical measure? In that the primary forcings of the climate are global in scale, does it not make most sense to first determine how the large-scale rather than the regional climate might be affected?

Dr. Soon's comments are truly misguided. Firstly, the surface temperature reconstructions published by my colleagues and I explicitly resolve regional patterns of surface temperature, so it is entirely unclear why Dr. Soon believes that we don't address regional climate changes. Had Dr. Soon understood our papers, he would be aware that we do. However, unlike the study that Dr. Soon published, our reconstructions explicitly take into account the issue of the relative timing and simultaneity of surface temperature changes in different regions. Only through doing this it possible to form an integrated measure of temperature changes such as hemispheric mean temperature. Scientists with training in climatology, statistics, and other areas of research required in the study of paleoclimate reconstruction know that the signal-to-noise ratio of any surface response to global radiative climate forcing increases as the scale of spatial averaging increases. In discussions of climate change it is thus the integral of the surface temperature field over an entire hemisphere or globe, which constitutes the most useful single variable for detecting, and attributing causal factors to observed changes. The spatial signature of the surface temperature signal (both with respect to position on the surface of the Earth, and altitude in the atmosphere) can nonetheless help to distinguish one source of climate forcing (e.g. solar) from another (e.g. enhanced greenhouse gases). My colleagues and I have indeed used the spatial patterns of surface temperature changes in past centuries to identify the role of natural external forcing of climate [Shindell, D.T., Schmidt, G.A., Mann, M.E., Rind, D., Waple, A., Solar forcing of

regional climate change during the Maunder Minimum, *Science*, 294, 2149-2152, 2001; Waple, A., Mann, M.E., Bradley, R.S., Long-term Patterns of Solar Irradiance Forcing in Model Experiments and Proxy-based Surface Temperature Reconstructions, *Climate Dynamics,* 18, 563-578, 2002; Shindell, D.T., Schmidt, G.A., Miller, R., Mann, M.E., Volcanic and Solar forcing of Climate Change During the Pre-Industrial era, *Journal of Climate*, in press, 2003]. Both Dr. Soon and Dr. Legates advocate in their testimony a primary role of solar forcing in recent climate change, though they provide no quantitative justification for this assertion at all. In fact, nearly a dozen detailed "detection" and "attribution" studies published during the past decade by leading climate researchers in the premier international scientific peer-reviewed journals such as *Science* and *Nature*, have shown that the vertical and horizontal pattern of observed warming is inconsistent with the response of the climate to solar forcing, but is consistent, with the response of the climate to anthropogenic forcing. Thus a prudent use of spatial information, as described in various studies by leading climatologists, including my collaborators and I, can potentially help elucidate the roles of natural and anthropogenic factors. However, Dr. Soon's studies are deficient in their use of any such information, and provide no insights into the factors governing past climate change.

22) This year, the western US is anomalously hot and dry. The eastern US is wetter than it has been since approximately 1891 and cool. Europe is hotter and drier than it has been in about 150 years. If we assume for the moment that these types of anomalies would persist for 50 years, are these the types of anomalies that Soon and Baliunas would consider as being indicative of there being an equivalent to the Medieval Warming in the western US and Europe while at the same time there is the equivalent of the Little Ice Age in eastern North America? How would your type of approach vary in its analysis of the year 2003 compared to the apparently contradictory results that Soon and Baliunas would have?

Indeed, as my colleagues and I discussed in our peer-reviewed articles in "*Eos*" and more recently "*Geophysical Research letters*" [Mann, M.E., Jones, P.D., Global Surface Temperatures over the Past two Millennia, *Geophysical Research Letters*, 30 (15), 1820, doi: 10.1029/2003GL017814, 2003] the Soon and Baliunas approach is indeed internally contradictory in that it would separately identify anomalies for even a given year, such as 2003, as simultaneously supportive of conditions they would classify as associated with a "Little Ice Age" and a "Medieval Warm Period" anomaly. As outlined in the question, this year's pattern of climate anomalies is a perfect example. Trained climatologists and paleoclimatologists know that one must independently evaluate precipitation or drought information from temperature information in reconstructing past climate patterns. For example, colleagues of mine and I have developed reconstructions of patterns of drought over the continental U.S. in past centuries from drought-sensitive tree-ring data [Cook, E.R., D.M. Meko, D.W. Stahle, and M.K. Cleaveland, Drought Reconstructions for the Continental United States, *Journal of Climate*, *12*, 1145-1162, 1999; Zhang, Z., Mann, M.E., Cook, E.R., Alternative Methods of Proxy-Based Climate Field Reconstruction: Application to the Reconstruction of Summer Drought Over the Conterminous United States back to 1700 From Drought-Sensitive Tree Ring Data, *Holocene*, in press, 2003]. The drought reconstructions display a quite different pattern of behavior over time from reconstructions of Northern Hemisphere mean temperatures, just as patterns of drought over the continental U.S. during the 20th century as recorded from instrumental data show relatively little in common with instrumental Northern Hemisphere mean temperature estimates (for example, the most prominent drought episode was the 'dust bowl' of the 1930s, while the most prominent anomaly in the Northern Hemisphere temperature record is the late 20th century warming). Drought and temperature are essentially independent climate variables. The papers by Soon and Baliunas seem not to recognize this fundamental fact. Finally, there is an irony in the testimonies of Soon and Legates in that they seem to be criticizing my colleagues and me for supposedly only focusing on the reconstruction of temperature patterns, when in fact we, and not they, have published work reconstructing past patterns of drought, precipitation, and atmospheric circulation from proxy climate data. However, we have made careful use of the information contained in proxy data in independently reconstructing patterns of temperature and patterns of drought. By contrast, Soon and colleagues hopelessly convolute such information in their interpretations of past climate trends.

23) Could you provide a more detailed explanation for the apparent Northern Hemisphere cooling from the 1940s to 1970s? What is the general expectation of what would have happened to the climate in the absence of any human influences, so just

continuing on from the trend for the last 1000 years prior to human intervention?

In fact, this issue has been studied by quite a number of climate scientists for well over a decade. As I mentioned in my testimony, a statistically significant cooling trend from 1940s to the 1970s is not evident for the globe, but only for the Northern Hemisphere. Dr Legates testimony on this matter is incorrect in that regard. The observed record of global-mean temperature changes over the past 100 years indicates a warming to about 1940, little change from 1940 to the mid 1970s, and then further warming. Legates implies in his comments that these changes are inconsistent with our current understanding of the factors governing climate change. This is also incorrect. In order to understand these observed changes it is necessary to consider all likely causal factors, both anthropogenic and natural. Anthropogenic factors include the warming effects of greenhouse gases and the cooling effects of sulfate aerosols. Natural factors include changes in the output of the Sun and the effects of explosive volcanic eruptions (such as the El Chichon eruption in 1982 and the Mt. Pinatubo eruption in 1991). and internal variability associated with natural climate oscillations in the ocean circulation and various modes of coupled ocean-atmosphere variability (such as El Nino). When all of these factors are considered, models give an expected pattern of 20th century temperature changes that is in remarkable agreement with the observations – and the models clearly show the three phases noted above. In particular, the leveling off of the warming trend over 1940–1975 turns out to be explained largely by the relatively rapid increase in cooling effects of sulfate aerosols as the world emerged from the Depression and WWII (and perhaps a small contribution from natural, internal variations in ocean currents). This cooling temporarily offset the warming due to increasing concentrations of greenhouse gases. This was first pointed out in a paper by Dr. Tom Wigley of the National Center for Atmospheric Research (NCAR) in *Nature* in 1989 and has been verified by numerous additional studies since. This agreement between models and observations shows quite clearly that human factors have been the dominant cause of global-scale climate change over the past 50 years, contrary to the repeated assertions by Soon and Legates that they are a manifestation of natural climate variability. In the absence of anthropogenic factors, model simulations indicate that natural factors alone would have lead to a slight cooling trend of global temperatures over the 20th century [Crowley, T.J., Causes of Climate Change Over the Past 1000 Years, *Science*, *289*, 270-277, 2000], in stark contrast to the dramatic warming that has been observed.

24) It was suggested at the hearing that increased CO2 could enhance plant life, and that since plants produce oxygen, this could lead to more O2 and less CO2. Could you please comment on the likelihood of this and how large the percentage changes could possibly be, recognizing that as the CO2 decreased, this would presumably mean the plants would do less well and conditions would cool?

Those suggestions (for example, Legates testimony with regard to the role of the 'CO2 fertilization' effect) represent a misunderstanding of the factors governing carbon cycle dynamics and their interaction with climate. In fact, careful studies that have been performed with coupled climate/terrestrial carbon cycle models that take into account the internal coupled interactions between climate and carbon dioxide, accounting for multiple potential factors such as (a) the potential 'CO2 fertilization' effect in which productivity of plants increases in a higher CO2 environment, (b) the impact of climate on productivity in which higher surface temperatures favor enhanced plant growth, and (c) the feedback of CO2 back on surface temperature alluded to in the question [see chapter 3 of the 2001 IPCC working group 1 report]. Such studies show that changes in surface temperature, through their impact on biological productivity, have led to, at most, changes of 5 to 10 ppm in CO2 levels over the past 1000 years [see Gerber, S., Joos, F., Bruegger, P.P., Stocker, T.F., Mann, M.E., Sitch, S., Constraining Temperature Variations over the last Millennium by Comparing Simulated and Observed Atmospheric CO2, *Climate Dynamics*, 20, 281-299, 2003]. Such changes are minimal in comparison with the dramatic increases in CO2 concentrations of more than 80 ppm associated with human activity, suggesting that the 'CO2 fertilization' effect advanced by Legates in his testimony in reality has a minimal role, at best, in the modern changes taking place in CO2 concentrations and climate.

25) Could you please clarify your remarks regarding the FACE experiments? When you say that increased CO2 leads to more uptake and that they will rot, do you mean that all plants will grow and eventually die and decay, and that increased CO2 really only ties up a bit more carbon in the process?

The sequence of questions and time allotted did not allow me to adequately explain this basic, but important point. The point I was making in my testimony is that the supposed increase in the terrestrial carbon reservoir due to enhanced plant growth that is argued to occur in a higher CO2 concentration atmosphere (the so-called 'CO2 fertilization' effect) is not a long-term, sustained effect. It is only a short-term effect that lasts only over the

generational timescales of forest stands. Any depletion of the atmospheric carbon reservoir due to enhanced growth or productivity of plants argued to arise from higher CO2 concentrations is short lived, because the plant or tree eventually dies and gives its carbon back to the atmosphere either through microbial activity (rotting) or burning. In other words, when plants, with any potential additional organic carbon storage that might arise from enhanced biological activity eventually die, they don't simply pile up in place with their carbon reservoir intact (which is what is implicitly assumed by those who argue that 'CO2 fertilization' represents a potential long-term offset to anthropogenic CO2 increases). Instead, this carbon is acted upon by biological, chemical, or physical processes which serve to add the carbon back to the atmosphere. Thus, the so-called "CO2 fertilization" effect, cannot serve as a permanent offset to anthropogenic increases in the atmospheric carbon budget (i.e., atmospheric CO2 concentrations), as implied by Legates in his testimony. It may simply act to slow, slightly, the rate of CO2 increase in the atmosphere by slightly increasing the storage rate (but *not* the residence time) of carbon in the terrestrial biosphere.

Another way to estimate the potential influence is by considering the total amounts of carbon presently stored in vegetation. Present, about 600 billion tons of carbon are tied up in the aboveground vegetation. About 2-3 times this much is tied up in roots and below ground carbon, which is a more difficult carbon pool to augment. By comparison, scenarios for fossil fuel emissions for the 21st century range from about 600 billion tons (if we can keep total global emissions at current levels, which implies controls well beyond the Kyoto Protocol calls for) to over 2500 billion tons if the world increases its reliance on combustion of coal as economic growth and population increase dramatically. These numbers clearly indicate that sequestering a significant fraction of projected emissions in vegetation is likely to be very difficult, especially as forests are cleared to make way for agriculture and communities. While there are possibilities of storage in wells and deep in the ocean, stabilizing the atmospheric CO2 concentration would require gathering up the equivalent of 1 to 2 times the world's existing above ground vegetation and putting it down abandoned oil wells or deep in the ocean. While CO2 fertilization will help to increase above ground vegetation a bit, storing more than a few tens of percent of the existing carbon would be quite surprising, and this is likely to be more like a few percent of global carbon emissions projected for the 21st century.

26) Senator Thomas stated that "[t]he rise in temperature during the 20th century occurred between 1900 and 1940." Could you please provide an indication of how much change occurred during this period based on internationally accepted observations, and compare this to the total change during the 20th century? Also please comment on whether it is scientifically representative to calculate a change starting with a cold period due to volcanic eruptions and end it during a period devoid of volcanic eruptions and then compare it to the century long period, which had major volcanic eruptions in both the first and last decades of the century.

A cursory review of the actual evidence (see e.g. Figure 2.1 of chapter 2 of the 2001 IPCC Scientific Working Group report) indicates the following approximate attributes in the observed record of global-mean temperature changes over the past 100 years: a warming of approximately 0.3C to 1940, a statistically insignificant change (given the uncertainties) from 1940 to the mid 1970s, and then an additional warming of approximately 0.5C from 1970 to 2000. Senator Thomas' claim is thus clearly mistaken. As discussed in my answer to an earlier question, this pattern of behavior is reproduced closely by models driven with estimates of both natural and anthropogenic forcing of the climate during the 20th century. The period of relative stasis in global mean temperatures from 1940 to 1970, in these model simulations, appears to result from the cooling impact of anthropogenic aerosols (for which there was a large increase during that time period) as well as a cooling contribution from explosive volcanic eruptions that occurred during that period, which tended to offset the warming influence of increased greenhouse gas concentrations during that time period. However, much of the overall warming of the globe during the 20th century (which is between 0.6^{o} and 1.0 ^{o}C depending on the precise instrumental data set used, and the precise endpoints of the interval examined) is clearly a result of increased greenhouse gas concentrations, as established in these simulations.

27) Senator Thomas stated that "there is no real evidence" that the greenhouse gases are affecting the climate. Could you please summarize the available evidence explaining their probable effect? Please include in your answer a specific example of a proxy indicator such as tree rings and explain the various subtleties in deriving a temperature

indicator from such a record.

As discussed in my answers to previous questions, the fact that increased greenhouse gas concentrations have a role in 20th century warming is no longer considered as being in doubt by mainstream researchers. Even noted contrarians such as Patrick Michaels of the Cato Institute now agree with this conclusion. The only room for legitimate scientific debate concerns the relative role of greenhouse gas concentrations vs. other factors, and the rate of future warming that may occur. Evidence establishing the role of anthropogenic greenhouse gas increases in 20th century warming includes the agreement with the full spatial (horizontal and vertical) pattern of warming with predictions from model simulations, and the fact that only model simulations which include anthropogenic forcing can match the observations, as discussed earlier. Evidence for an anthropogenic influence on climate also comes from evidence of the anomalous nature of late 20th century warmth in a very long-term context (i.e., in at least the past millennium, and potentially the past several millennia or longer). One such source of evidence for this conclusion comes from proxy climate records (such as tree rings, corals, and ice cores) that can be used to reconstruct long-term temperature patterns based on a careful consideration of the temperature signal in those data, as discussed in my response to earlier questions. But other evidence of anomalous late 20th century warmth comes from indications of unprecedented melting of mountain glaciers the world over (including meltback in the Alps so dramatic that it recently revealed the now-famous "Ice Man" who had been trapped in ice for more than 5000 years), and evidence of unusual phenological changes (e.g. the timing of flowering of plants) during the late 20th century.

28) Senator Carper asked the other two witnesses if they thought it "possible to emit unlimited amounts of CO2 into our atmosphere without having any impact on climate or temperature?" What is your expectation of what would occur? That is, how much change in the CO2 concentration would cause how much of a response?

The response of global mean surface temperature to increased CO2 varies roughly as the logarithm of the CO2 concentration (meaning that increments in temperature scale with the percentage change in CO2 rather than the change in amount itself). This is a very well known, and long established result that follows both from basic theoretical considerations of radiative transfer theory, and is embodied in experiments using global climate models with varying levels of CO2 concentrations. The statistical relationship between estimated concentrations of CO2 and the admittedly crude estimates of global mean temperatures at various periods in the geological past or during past glacial intervals, conform relatively well to this theoretical relationship within estimated uncertainties [see e.g. the textbook, "Earth's Climate Past and Future", by W.F. Ruddiman (W.H. Freeman and Co), 2001]. I was extremely surprised when Dr. Soon indicated that he did not know how to answer Senator Carper's question, suggesting that he is not familiar with this fairly basic scientific knowledge.

This result implies, in the absence of any other factors, a linear increase in temperature over time in response to an exponential increase in CO2 (which is not a bad description of the character of the CO2 trend associated with exponentially increasing anthropogenic activity over the past two centuries). Climate models tell us that the "slope" of that linear increase is between 1.5 to 4.5 C for each doubling of the CO2 concentration. In this context, the testimony Dr. Legates that an arbitrary increase in greenhouse gases would lead only to a "slight" increase in termperature, seems especially disingenuous. Dr. Legates seems to be suggesting that the warming would be small despite the magnitude of the CO2 increase. Yet, both model-based studies and analyses of how climates changes in the past may have been influenced by changes in atmospheric composition suggest that a 1.5 to 4.5 C increase in temperature is likely for *each* doubling of the CO2 concentration. Thus, a quadrupling of the CO2 concentration, which is plausible if the world chooses to derive most of its future energy from coal, would be expected to be associated with a roughly 3 to 9 C increase in global mean temperature. Does Dr. Legates consider this a "slightly" increased temperature?

29) In his testimony, Dr. Legates indicated that there were historical cases where the temperature has gone up, but the CO2 has fallen. Do you agree there were such periods and how would you explain this?

It is certainly the case that this has happened in the past. However, it is hardly surprising, and certainly not inconsistent with our established understanding of the various factors that influence surface temperatures. The warming response to increased greenhouse gas concentrations lags the actual increase in greenhouse gas

concentrations in the atmosphere potentially by several decades, due to the sluggish response of the oceans, which have an enormous thermal capacity compared to the atmosphere, to increased surface radiative forcing. So warming is not expected to be contemporaneous with changes in CO2, but instead, to lag it by several decades. In addition, greenhouse gases are certainly not the only factor affecting the average surface temperature of the Earth. There are other anthropogenic factors, such as increased sulphate aerosols, which can have a cooling effect on the climate, and natural factors, such as volcanic activity, modest natural variations in solar output, and internal dynamics associated with climate events such as El Nino, which also influence the average surface temperature of the globe. At any particular time, these other factors may outweigh the warming effect due to increased greenhouse gases. For example, the relative lack of warming during the period 1940-1970 appears to be related to a combination of such factors, as discussed in my response to an earlier question. But while these other factors tend to cancel over time, the increased greenhouse gases lead to a systematic warming that will not cancel out over time. It is for precisely this reason that late 20th century warming now appears to have risen above the range of the natural variability of past centuries.

There are two myths commonly perpetuated by climate change contrarians with regard to the relationship between historical CO2 and temperature variations that are worth addressing in particular:
(1) Contrarians sometimes argue that the fact that the seasonal cycle in atmospheric CO2, which is opposite of the seasonal cycle in temperature in the Northern Hemisphere (maximum atmospheric CO2 levels over the course of the year occur during the Northern Hemisphere winter) implies a negative feedback of temperature on CO2 concentration. Such an argument is based on a most profound misunderstanding of the basic principles governing atmospheric chemistry. Properly trained atmospheric chemists know that the seasonal cycle in global atmospheric CO2 concentration is governed by the breathing of the terrestrial biosphere, which exhibits a hemispheric (and thus seasonal) asymmetry: there is a net uptake of atmospheric CO2 (and thus a drawdown of atmospheric CO2 concentrations) by terrestrial plants during the Northern Hemisphere summer growing season, owing to the vastly greater proportion of land in the Northern Hemisphere. This simple fact, and nothing else, dictates the relationship between Northern Hemisphere surface temperatures and CO2 concentrations on seasonal timescales.
(2) Contrarians sometimes argue that the relationship between atmospheric CO2 concentrations and temperature variations associated with glacial/interglacial variations over the past several hundred thousand years, as deduced from ice core measurements shows that CO2 is an effect, rather than cause, of climate variability. This reasoning is unsound for at least two fundamental reasons:
(a) Detailed measurements show that global atmospheric CO2 concentrations lead estimated polar temperature variations (as deduced from ice core oxygen isotope ratios) during the long phase of increased glaciation, consistent with greenhouse gas forcing of the atmosphere. There is some evidence that CO2 concentrations, however, lag estimated polar temperature variations during the rapid phase of deglaciation (melting of the terrestrial ice sheets at the termination of an ice age). This observation is the basis of the flawed argument summarized below. During this more rapid 'deglacial' phase, the climate system if far from being in an equilibrium state, and the dynamics of the climate system must be considered as representative of the a coupled interaction between surface temperature, atmospheric CO2, ocean circulation, and glacial mass. It is well known by glaciologists who study this problem that the relationship between CO2 and temperatures in such a scenario cannot be interpreted in terms of a simple cause-effect relationship.
(b) Even during the rapid deglaciation, the oxygen isotopes from the ice cores only provide an estimate of surface temperature variations in the proximity of the ice core (and a very imperfect one, owing to possible seasonal deposition biases and non-temperature influences on isotope fractionation). They certainly do not provide an estimate of hemispheric, let alone global, temperature variations. Thus, a comparison of ice core estimates of CO2 and oxygen o18 ratios cannot be used to confidently infer the relationship between CO2 concentrations and global mean temperatures

30) During the hearing, there was some contention over the issue of the effect of surface cover changes and urban influences on the climate? Could you please restate your position on the likely sign and magnitude of the influence of both factors?

Unfortunately misleading comments by Soon and Legates, and the complexity of the issues involved, made it difficult for me to convey, in the brief time allotted, the established science dealing with the various influences on Earth's surface radiation balance and changes therein in recent decades. Legates in his testimony confused and misstated the nature of both natural and anthropogenic influences on the Earth's surface energy budget and on the measurement of surface temperatures from surface-based stations. There are several different issues involved here, which I will attempt to clarify one at a time below:

1) The claim made by Legates that the location of thermometer measurements in urban centers biases estimates of global mean temperature from the available meteorological observations would be correct were this effect not already carefully accounted for. In particular, possible 'urban heat island' effects on global temperature estimates have been studiously accounted for in estimates that have been produced for more than a decade. See e.g. the 2001 IPCC report. This is unrelated to the issue of the influence of land-use changes on the surface radiation budget, though Legates testimony blurs the distinction between the two issues.

2) The implication by Legates that land-use changes (such as urbanization) are the dominant influence on changes in the absorptive properties of the Earth as a whole in recent decades is completely wrong for at least two reasons:

a) The primary factor impacting changes in the absorption of solar insolation by the Earth's surface in modern decades is the decrease in reflective snow and ice cover due to the warming of the Earth's surface. This represents a well-known positive feedback (the 'ice-albedo' feedback) associated with global warming in which warming leads to melting of snow and ice, which decreases the reflective properties of the surface, increasing surface absorption of radiation, and thus increased the surface temperatures themselves. This crucial positive feedback, which enhances the impact of greenhouse gas concentrations on the warming of the surface, is fully accounted for in the climate model simulations that I have referred to above and in my testimony.

b) While urbanization, as suggested by Legates, may lead to increased absorption of solar insolation in some urban areas, this is the more minor of the human land use changes impacting climate. There are far more extensive regions of the Earth where other changes in land use, such as conversion of forested land to agricultural land, have, instead, increased the reflective properties of the Earth's surface [Ramankutty, N., and J. A. Foley, Estimating historical changes in global land cover: croplands from 1700 to 992, Global Biogeochemical Cycles, 13, 997–1027, 1999.], tending to cool the surface, as I explained in my testimony. Scientists who have studied the influences of these effects have found that the latter cooling effect is the dominant of these two anthropogenic land-use influences on the Earth's surface properties. Thus, climate model simulations investigating the influence of land-use changes on hemispheric or global mean temperatures indicate that they have imposed a modest cooling influence [Govindasamy, B., P.B. Duffy, and K. Caldeira, Land use changes and Northern Hemisphere cooling, *Geophysical Research Letters*, *28*, 291-294, 2001; Bauer, E., M. Claussen, and V. Brovkin, Assessing climate forcings of the Earth system for the past millennium, *Geophys. Res. Lett., 30*, doi: 10.1029/2002GL016639, 2003] that partially offsets even greater warming that would have been realized during the 20th century due to anthropogenic greenhouse gas influences. Evidence, therefore, does not support the case, as argued by Legates, that the full range of human land use changes have had a net warming effect on the climate. They have had a modest cooling influence on the climate.

31) Do you receive any income from any sources which have taken advocacy positions with respect to the Kyoto Protocol, the UN Framework Convention on Climate Change, or legislation before the United States Congress that would affect greenhouse gas emissions? If so, please identify those sources and the relevant advocacy position taken.

I do not, nor have I ever, received any such income.