

Senator Inhofe's Follow-up Questions for Dr. Michael Mann

1. You have used the term "climate scientist" to distinguish certain individuals. What, in your view, does it take for one to earn the title "climate scientist"? What specific credentials, or the lack thereof, would lead you to refuse to recognize someone as a "climate scientist"?

The term "climate scientist" is used, in my experience, to describe an individual with specific training in oceanographic, atmospheric, and coupled ocean-atmosphere processes relevant to understanding climate variability and the behavior of the climate system. An individual might obtain this training through either an advanced degree in those areas of study, or through years of research in those areas associated with numerous publications in the peer-reviewed climate literature such as "Journal of Geophysical Research-Atmospheres", "Journal of Geophysical Research-Oceans", "Climate Dynamics", "The Holocene", "Geophysical Research Letters", "Paleoceanography" (or publication of climate papers in leading international science journals such as "Nature" and "Science"). I would not, for example, consider scientists with advanced degrees in Astronomy, Astrophysics, or Physics who have published primarily in those areas, as "climate scientists"--nor do I believe would most of my colleagues in the climate research community. In addition to training and publishing in a field, leading scientists would normally be expected to be actively interacting and collaborating in studies with colleagues and ensuring their understanding of cutting edge science through attendance and active participation in meetings convened by the leading professional societies and organizations.

2. Your work and testimony contends that the Little Ice Age was not global, but restricted to only portions of Europe. A forthcoming article by Shindell et al. (Shindell, D.T. et al., 2003: Volcanic and solar forcing of climate change during the pre-industrial era. *Journal of Climate*, in press), however, indicates the Little Ice Age could have resulted from a combination of solar and volcanic forcing. Do you agree with these conclusions from Shindell et al.? If so, how can solar and volcanic forcings generate climatic effects that are not observed across the entire hemisphere?

The statement is incorrect. I never testified that the "Little Ice Age was...restricted to only portions of Europe".

It should first be noted that many paleoclimatologists have questioned the utility of terms such as "Little Ice Age" and "Medieval Warm Period" which provide misleading descriptions of past climate changes in many regions. There is a complex pattern of climate variability in past centuries, and the lack of evidence for synchronous temperature variations worldwide in past centuries [e.g. Bradley, R.S., and P.D. Jones, "Little Ice Age" summer temperature variations: their nature and relevance to recent global warming trends, *The Holocene*, 3, 367-376, 1993; Hughes, M.K., and H.F. Diaz, Was there a 'medieval warm period', and if so, where and when?, *Climatic Change*, 26, 109-142, 1994]. The cited paper by Shindell et al (2003), of which I am a co-author, is fully consistent with such findings. The paper, rather than demonstrating globally uniform patterns of warming or cooling in past centuries, shows that surface temperature changes were dominated by regional overprints associated with the response of the "North Atlantic Oscillation" atmospheric circulation pattern to radiative forcing. This response leads to a pattern of cooling during the 17th/18th centuries in certain regions (not just Europe, but many regions throughout the Northern Hemisphere extratropics) and warming in other regions. The paper shows that this pattern of warming and cooling closely resembles the pattern of surface temperature change during that interval reconstructed by Mann and colleagues (MBH98). It is worth noting, moreover, that the tropical Pacific seems to have been in a warmer, rather than a "colder" state, during the conventionally defined "Little Ice Age" [Cobb, K.M., Charles, C.D., Edwards, R.L., Cheng, H., & Kastner, M. El Niño-Southern Oscillation and tropical Pacific climate during the last millennium, *Nature* 424, 271-276 (2003)]. Climate dynamists understand the importance of such phenomena in understanding the highly variable pattern of surface temperature changes in past centuries, and rarely, if ever, argue for the existence of globally uniform or synchronous temperature

change in past centuries. The response of the climate to solar and volcanic radiative forcing is known to involve dynamical responses associated with regionally differentiated temperature trends that overprint far smaller global mean responses. This contrasts strongly with the response of the climate to anthropogenic climate forcing, for which the integrated global mean radiative forcing is considerably greater, and the associated large-scale warming typically rises above the regional variability.

3. That same paper finds ³long-term regional response to solar forcing [that] greatly exceeds unforced variability...and produces climate anomalies similar to those seen during the Little Ice Age. Thus, long-term regional changes during the pre-industrial [era] appear to have been dominated by solar forcing.² You further state that ³For the few centuries prior to the industrial era, however, externally driven climate change is thought to have been forced primarily by only two factors: variation in solar output and volcanic eruptions...These forcings likely played a large role in the so-called Medieval Warm Period (MWP) and Little Ice Age (LIA) epochs of the last millennium, which saw significant climate changes on at least regional scales...² You then define ³regional² ³to mean continental in scale...² Do you claim that total solar irradiance change is the only solar forcing mechanism that has any significant climate effect? List your formal training in, plus courses you have taught, in solar physics. Do you agree with the paper's claim that the MWP and LIA exist on regional scales, in accordance with climate experts like R. Bryson and H. H. Lamb, starting with their work in the 1960s, and recently updated in summary in Soon et al. (2003)?

Expertise in "solar physics" is not the expertise required to evaluate what is happening to the Earth's climate—what matters are the changes in solar radiation at the top of the atmosphere and then down through it. As is made clear in our paper, we are considering not only "total solar irradiance" but also its spectral distribution. Indeed, because much of the change in solar radiation occurs in UV wavelengths, induced changes in stratospheric ozone can be lead to significant changes in atmospheric circulation in the troposphere. The model simulations indicate that such atmospheric circulation changes can, acting with other factors, lead to regional variations in the climate such as were observed over the last millennium. As a co-author of this paper, I of course, agree with its findings. However, the inference that this paper confirms the work of Soon et al. (2003) is very mistaken.

With respect to my training, teaching, I would encourage that my Curriculum Vitae, which I have provided separately, be included in the record to be compared to those of the other witnesses with respect to relevant expertise and standing in the climate research community.

4. In your testimony, you stated that you hold the $\text{\textcircled{E}}$ mainstream¹ view with respect to climate theory of air temperature trends over the past two millennia. Provide supporting citations in the refereed scientific literature that are not authored or co-authored by you or your colleagues, collaborators, students or former students, or associates (i.e., Phil Jones, Ray Bradley, Malcolm Hughes), where others hold this $\text{\textcircled{E}}$ mainstream¹ view.

The statement once again mischaracterizes my comments. As there is only one reconstruction of Northern Hemisphere annual mean temperature over the past two millennia, published only recently by Phil Jones and myself, it is hardly meaningful to discuss whether "other studies" support this finding. The peer-review and publication process typically unfolds on timescales of a year or longer, not months. Any careful reading of my comments would reveal that I was not referring to this one specific reconstruction in the comments I made characterizing what I believe to be the mainstream viewpoint of the climate research community. This review, as discussed in my testimony, refers rather to the widespread evidence that late

20th century warmth is unprecedented in a long-term context, anywhere from the past several centuries to nearly the past two millennia, depending on the timeframe of the particular study.

Secondly, the description of "collaborators, associates, former students", depending on how interpreted, is so broad a category as to include just about every leading scientist in the field. The following publications all come to the same conclusion that late 20th century Northern Hemisphere warmth is anomalous in a long-term context:

- Bauer, E., Claussen, M., Brovkin, V., Assessing climate forcings of the earth system for the past millennium, *Geophys. Res. Lett.*, 30 (6), 1276, doi: 10.1029/2002GL016639, 2003.
- Bertrand C., Loutre M.F., Crucifix M., Berger A., Climate of the Last millennium: a sensitivity study. *Tellus*, 54(A), 221-244, 2002.
- Bradley, R.S., and P.D. Jones, "Little Ice Age" summer temperature variations: their nature and relevance to recent global warming trends, *The Holocene*, 3 (4), 367-376, 1993.
- Bradley, R.S., Briffa, K.R., Crowley, T.J., Hughes, M.K., Jones, P.D, Mann, M.E., Scope of Medieval Warming, *Science*, 292, 2011-2012, 2001.
- Bradley, R.S., M.K.Hughes and H.F. Diaz., Climate in Medieval Time. *Science*, 302, 404-405, 2003.
- Pollack, H.N., S. Huang, and P.-Y. Shen, Climate Change Record in Subsurface Temperatures: A Global Perspective, *Science*, 282, 279-281, 1998.
- Briffa, K.R., T.J. Osborn, F.H. Schweingruber, I.C. Harris, P.D. Jones, S.G. Shiyatov, S.G. and E.A. Vaganov, Low-frequency temperature variations from a northern tree-ring density network. *J. Geophys. Res.*, 106, 2929-2941, 2001.
- Crowley, T.J., Causes of Climate Change Over the Past 1000 Years, *Science*, 289, 270-277, 2000.
- Crowley, T.J., and T. Lowery, How Warm Was the Medieval Warm Period?, *Ambio*, 29, 51-54, 2000.
- Gerber, S., F. Joos, P. Brügger, T. F. Stocker, M. E. Mann, S. Sitch, and M. Scholze, Constraining temperature variations over the last millennium by comparing simulated and observed atmospheric CO₂, *Climate Dynamics*, 20, 281-299, 2003.
- Hegerl, G.C., T.J. Crowley, S.K. Baum, K-Y. Kim, and W. T. Hyde, Detection of volcanic, solar and greenhouse gas signals in paleo-reconstructions of Northern Hemispheric temperature. *Geophys. Res. Lett.*, 30 (5), doi: 10.1029/2002GL016635, 2003.
- Huang, S., H. N.Pollack and P.-Y. Shen, Temperature Trends Over the Past Five Centuries Reconstructed from Borehole Temperature, *Nature* 403, 756-758, 2000.
- Jones, P.D., M. New, D.E. Parker, S. Martin, and I.G. Rigor, 1999: Surface air temperature and its changes over the past 150 years. *Reviews of Geophysics* 37, 173-199.
- Jones, P.D., T.J. Osborn, and K.B. Briffa, The Evolution of Climate Over the Last Millennium, *Science*, 292, 662-667, 2001.
- Overpeck, J., K. Hughen, D. Hardy, R. Bradley, R. Case, M. Douglas, B. Finney, K. Gajewski, G. Jacoby, A. Jennings, S. Lamoureux, A. Lasca, G.M.J. Moore, M. Retelle, S. Smith, A. Wolfe, and G. Zielinski, Arctic Environmental Change of the Last Four Centuries, *Science*, 278, 1251-1256, 1997.
- Pollack, H.N., S. Huang, and P.-Y. Shen, Climate Change Record in Subsurface Temperatures: A Global Perspective, *Science*, 282, 279-281, 1998.

5. Your work has been characterized as "global" in several venues, including the National Assessment. Is that a fair characterization, or are those sources confused by your use of Northern and Southern Hemisphere proxies in your Northern Hemisphere reconstruction. Can you explain why the National Assessment did not include error bars on your temperature reconstruction?

The proxy records on which our work is based represent conditions over much of the Northern Hemisphere and a small fraction of the Southern Hemisphere. While in any given year there can be some difference in the anomalies in the two hemispheres, the instrumental record indicates that over periods of a few decades or more, the anomalies in the two hemispheres are quite similar because of the thermodynamic and dynamic coupling between them. Thus, the major features of the temperature record, and in particular the unusual 20th century warming, are similar in the two hemispheres and thus global features. It was this

aspect of the record to which the text of the National Assessment report refers in presenting the overall significance of our study, and the report is correct in suggesting that the 20th century warming is global in nature. The caption for Figure 2 of Chapter 1 in the Foundation report (page 22 and page 544) states that “Although this record comes mostly from the Northern Hemisphere, it is likely to be a good approximation to the global anomaly based on comparisons of recent patterns of temperature fluctuations.” This accurately reflects the situation. In the Overview report (page 13), although the figure title says “Global CO₂ and Temperature Change,” the caption next to the figure says “Records of Northern Hemisphere surface temperatures, CO₂ concentrations, and carbon emissions show a close correlation. Temperature change: reconstruction of annual-average Northern Hemisphere surface air temperatures derived from historical records, tree rings, and corals (blue), and air temperatures directly measured (purple).” This quite clearly makes the point this is mainly a Northern Hemisphere temperature record.

With respect to the question about the presentation of the figure, it is misleading to imply that the term “error bars” indicates that the central line is off by this amount—rather the limits mean that there is only 1 chance in 20 that the actual value is outside this range. That is, what we are showing is the likely range within which the anomaly lies, with there being a 95% chance the value is within this range. The line that we present in many of our figures, and that was presented in the National Assessment report, is the most likely value within this range (rather a natural choice to display in explaining a complex issue to the public). In looking at the National Assessment report, the caption for Figure 2 of Chapter 1 in the Foundation report (page 22 and page 544) states that “The error bars for the estimate of the annual-average anomaly increase somewhat going back in time, with one standard deviation being about 0.25°F (0.15°C).” Quite clearly, the reader interested in investigating the accuracy of the records would follow-up by reading the original reference, which is cited in the text.

In that I was not involved in the National Assessment report, the questioner should consult the authors of that report for any further information or questions.

6. You testified that the late 20th century warming is likely caused by man-made CO₂ forcing on climate; what is your scientific proof for that claim? Please detail how you removed the potential effects from other factors including those of sulfate aerosols, tropospheric and stratospheric ozone, volcanic dust veiling, black soot, solar particle and wavelength-dependent variability, sea ice, land use, vegetation and other greenhouse gases?

The question inconsistently equates my statement of a "likely" causal relationship with the standard of "scientific proof". Scientists do not speak in terms of "proof". We speak in terms of likelihoods and the strength of evidence in support of a particular hypothesis.

A 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 have been demonstrated in the following peer-reviewed scientific articles:

- Wigley, T.M.L., R.L. Smith, and B.D. Santer, Anthropogenic Influence on the Autocorrelation Structure of Hemispheric-Mean Temperatures, *Science*, 282, 1676-1680, 1998.
- Tett, S.F.B., P.A. Scott, M.R. Allen, W.J. Ingram, and J.F.B. Mitchell, Causes of Twentieth-Century Temperature Change Near the Earth's Surface, *Nature*, 399, 569-572, 1999.
- Hegerl, G.C., P.A. Scott, M.R. Allen, J.F.B. Mitchell, S.F.B. Tett, and U. Cubasch, Optimal detection and attribution of climate change: sensitivity of results to climate model differences, *Climate Dynamics*, 16, 737-754, 2000.
- Crowley, T.J., Causes of Climate Change Over the Past 1000 Years, *Science*, 289, 270-277, 2000.
- Stott, P.A., S.F.B. Tett, G.S. Jones, M.R. Allen, J.F.B. Mitchell, and G.J. Jenkins, External Control of 20th Century Temperature by Natural and Anthropogenic Forcings, *Science*, 290, 2133-2137, 2001.
- Stott, P.A., S.F.B. Tett, G.S. Jones, M.R. Allen, W.J. Ingram, and J.F.B. Mitchell, Attribution of twentieth century temperature change to natural and anthropogenic causes, *Climate Dynamics*, 17, 1-21, 2001.

These conclusions, furthermore, were endorsed by the 2001 IPCC scientific working group report (Chapter 12), and the follow-up National Academy of Sciences report that endorsed most of the key IPCC conclusions.

7. A number of expert studies have produced individual proxy records that show the existence of a local Medieval Warm Period or Little Ice Age. Such studies cover a large portion of the globe. How do you reconcile your hemispheric reconstruction with these individual proxy records?

It is unclear to me what precisely the questioner means by "a number of expert studies?" or how he defines the "existence" of a "Medieval Warm Period" or "Little Ice Age". As discussed in my response to question 2, the regionally and temporally variable nature of climate changes in past centuries makes such descriptors of past climate change naive and often useless as a characterization of past changes. A sampling of some of the longest, high-quality best term proxy temperature estimates over the globe was provided in Figure 2 of the article: 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. This figure demonstrates the lack of evidence for any periods in earlier centuries that are comparable in terms of evidence for synchronous warmth to the late 20th century. This same conclusion was also demonstrated by the recent article in *Science*: Bradley, R.S., M.K. Hughes and H.F. Diaz., Climate in Medieval Time. *Science*, 302, 404-405, 2003.

8. Do you claim 22 proxies to be a sufficient sample of observations for reconstructing a Northern Hemisphere temperature? If not, why did you consider it sufficient for the 1400-1450 interval in your 1998 Nature paper? If you do, there are 29 proxies that continue to 1984 in the database you used for your 1998 paper. Why then did you terminate your temperature reconstruction at 1980? What efforts have you made to extend the proxy re-constructions up to the present?

The question is wrongly premised. The Mann et al (1998) study made use of almost 100 proxy series over the interval AD 1400-1450. The question appears to confuse the number of proxy series that was used, with the number of statistical indicators that were used to represent these proxy data. For example, the 70 series that make up the North American International Tree Ring Data Base date back to 1400, were represented in terms of their leading patterns of variance through a procedure known as "Principal Component Analysis". These patterns represented, however, a much larger number of underlying data. Most of the proxy records used in that analysis ended by 1980, limiting the useful upper limit to the calibration period used. A more recent paper (in press) extends proxy-based hemispheric temperature reconstructions through the mid

1990s, demonstrating the ability of the reconstruction to capture the accelerated warming evident in the instrumental record since 1980.

9. What are the patterns of temperature change in all proxies after 1980?

It is unclear what is meant by the question. Every proxy series which extends past 1980 exhibits its own particular pattern. A recent paper (in press), as referred to in question #8, demonstrates that a composite of proxy temperature indicators with reliable low-frequency variability that are available through the mid 1990s capture the accelerated warming after 1980.

All of the data used in our study have been available since July 2002 on the public ftp site:
<ftp://holocene.evsc.virginia.edu/pub/MBH98/>.

10. Do you have any external (not derived by you) method or data to provide verification of your temperature reconstruction? Please explain.

We used the method of cross-validation to independently demonstrate the statistical reliability of our reconstructions. This is detailed in MBH98 and MBH99. We did not derive the method of cross-validation--it is a well established statistical procedure, detailed in many introductory level statistics textbooks.

11. Are you aware of any errors in your data compilation for MBH98 or MBH99? If so, what are they?

We are not aware of any errors. We are, however, aware of recent spurious claims of such errors by the authors of an article published in the social science journal "Energy and Environment". These claims have already been widely discredited by a cursory analysis of the paper, and a manuscript detailing the numerous fundamental errors made in the Energy and Environment paper has been submitted to the peer-reviewed literature. We would be happy to provide a copy of the paper to be made part of the official Senate record once it is published.

12. Are you aware of any errors in any calculations that you made in MBH98 or MBH99? If so, what are the errors?

See response to question 11.

13. Vegetation grows as a result of a number of factors, including energy input, moisture supply, fire frequencies, and species competition. Do you claim it is possible to accurately remove the effects of these factors from your tree ring proxy datasets to produce a resulting time-series represents fluctuations in only air temperature? What is the magnitude of the error introduced in developing a procedure to remove these other effects? Please detail the analyses and list peer-reviewed works that specifically outline techniques to remove the effect of these other indicators for inferring past temperatures.

One of the co-authors of MBH98 (Malcolm Hughes) is among the world's foremost experts in dendroclimatology, so the team of MBH98 hardly needs to be informed of the processes that influence tree growth. The method of MBH98 does not "remove" various factors from tree ring proxy information (which would be a most unwise approach!) but, rather, uses multivariate statistical methods similar to those commonly used in climate and paleoclimate field reconstruction [see e.g. Cook, E.R., K.R. Briffa, and P.D. Jones, Spatial Regression Methods in Dendroclimatology: A Review and Comparison of Two Techniques, *International Journal of Climatology*, 14, 379-402, 1994; Smith, T.M., R.W. Reynolds, R.E. Livezey, and D.C. Stokes, Reconstruction of Historical Sea Surface Temperatures Using Empirical Orthogonal

Functions, *Journal of Climate*, 9, 1403-1420, 1996; Kaplan, A., Y. Kushnir, M.A. Cane, and M.B. Blumenthal, Reduced space optimal analysis for historical data sets: 136 years of Atlantic sea surface temperatures, *Journal of Geophysical Research*, 102, 27835-27860, 1997] to separate the information in the data that can meaningfully be related to surface temperature variations from that related to other influences.

14. Define the difference between variability and error in a statistical analysis. In EOF analyses, is the variation of the first principal component indicative of the uncertainty associated with the data? Why or why not?

Variability in an estimated quantity can be thought of as representing both 'signal' (the physical quantity one is interested in) and 'noise' (everything else). The definition of noise and signal depends on a number of assumptions regarding the nature of the process that generated the times series of interest and the specification of the statistical model for the data in question. Uncertainty, which is associated with the partitioning of data variance into 'noise' and 'signal', as defined above, depends on such detailed considerations. There are no general statistical principles that I am familiar with that relate uncertainty, thusly defined, to the first, or any other, principal component of a dataset containing both signal and noise contributions. Uncertainty is typically diagnosed by the analysis of residual variance from a statistical model based on a combined calibration/cross-validation procedure. Introductory text books such as "Statistical Methods in the Atmospheric Sciences" (D. Wilks, Academic Press) deal with this topic in detail.

15. Specifically, how do you construct regional patterns of temperature changes in past centuries when data are limited, either spatially, temporally, or both?

Our methods are described in detail in the following peer-reviewed scientific publications, which I would like to have made part of the official Senate record:

- **Mann, M.E.**, Jones, P.D., Global Surface Temperatures over the Past two Millennia, *Geophysical Research Letters*, 30 (15), 1820, 10.1029/2003GL017814, 2003.
- D'Arrigo, R.D., Cook, E.R., **Mann, M.E.**, Jacoby, G.C., Tree-ring reconstructions of temperature and sea level pressure variability associated with the warm-season Arctic Oscillation since AD 1650, *Geophysical Research Letters*, 30 (11), 1549, doi: 10.1029/2003GL017250, 2003.
- **Mann, M.E.**, Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., Optimal Surface Temperature Reconstructions Using Terrestrial Borehole Data, *Journal of Geophysical Research*, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.
- Rutherford, S., **Mann, M.E.**, Delworth, T.L., Stouffer, R., Climate Field Reconstruction Under Stationary and Nonstationary Forcing, *Journal of Climate*, 16, 462-479, 2003.
- **Mann, M.E.**, Large-scale climate variability and connections with the Middle East in past centuries, *Climatic Change*, 55, 287-314, 2002.
- Cook, E.R., D'Arrigo, R.D., **Mann, M.E.**, A Well-Verified, Multi-Proxy Reconstruction of the Winter North Atlantic Oscillation Since AD 1400, *Journal of Climate*, 15, 1754-1764, 2002.
- **Mann, M.E.**, Rutherford, S., Climate Reconstruction Using 'Pseudoproxies', *Geophysical Research Letters*, 29 (10), 1501, doi: 10.1029/2001GL014554, 2002.
- **Mann, M.E.**, Large-scale Temperature Patterns in Past Centuries: Implications for North American Climate Change, *Human and Ecological Risk Assessment*, 7 1247-1254, 2001.
- **Mann, M.E.**, Climate During the Past Millennium, *Weather* (invited contribution), 56, 91-101, 2001.
- Cullen, H., D'Arrigo, R., Cook, E., **Mann, M.E.**, Multiproxy-based reconstructions of the North Atlantic Oscillation over the past three centuries, *Paleoceanography*, 15, 27-39, 2001.
- **Mann, M.E.**, Gille, E., Bradley, R.S., Hughes, M.K., Overpeck, J.T., Keimig, F.T., Gross, W., Global Temperature Patterns in Past Centuries: An interactive presentation, *Earth Interactions*, 4-4, 1-29, 2000.

- Delworth, T.L., **Mann, M.E.**, Observed and Simulated Multidecadal Variability in the Northern Hemisphere, *Climate Dynamics*, 16, 661-676, 2000.
- **Mann, M.E.**, Bradley, R.S. and Hughes, M.K., Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations, *Geophysical Research Letters*, 26, 759-762, 1999.
- **Mann, M.E.**, Bradley, R.S., and Hughes, M.K., Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries, *Nature*, 392, 779-787, 1998.

16. Do you claim that the instrumental temperature record is known without error? If not, what error and uncertainty would you associate with the annual Northern Hemisphere averaged air temperature for 1900? For 1950? For 2000? How were these estimates incorporated into your analysis?

The claim made by Dr. Legates in his testimony that we present the instrumental record without uncertainty is incorrect. 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 small 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°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) indicates two standard error intervals. The decade of the 1990s is roughly two standard errors warmer (i.e., about 0.4°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.

17. Assuming a proxy record extended back to 1000 A.D., what specifically would be required to disqualify this proxy record from your analyses? Provide supporting evidence where others have disqualified such records from temperature analyses on these criteria.

It is unclear what type of "analyses" are being referred to here. I have used a variety of different statistical methods and data in various published studies describing paleoclimate reconstruction, so the question as worded is implicitly vague. In any case, our approaches do not "disqualify" proxy data. They use objective statistical criteria to evaluate the strength of the signal available for reconstruction of the particular climate field or index (be it related to surface temperature, atmospheric circulation, drought, or other variables) to be reconstructed. Such statistical approaches, and the related approaches used by other climate researchers, are described in the various publications listed above in the response to questions #13 and 15.

18. Have you made available via FTP the coefficients developed to relate proxies to principal components? If not, would you make those coefficients available at NGDC/paleo?

The question is based on two false premises. The first involves a naive view of what is required and expected of scientific researchers. It is unprecedented in my experience for any scientist to post in the public domain every single computational aspect of a complicated analysis. The methods of our study were adequately described in our paper and supplementary information, and the data used were made available in the public domain. Indeed, we made far more of our results, data, and methodological details available in the public domain than is provided in most similar scientific studies. The scientific funding agencies (DOE, NSF, and NOAA) would have informed us if we had not followed the appropriate protocols in the provision of data and results.

The second false premise is a technical one. A proper understanding of the methodology employed by MBH98 would reveal that there is no one fixed set of "coefficients" that relate a particular proxy record to a particular principal component. The relationship is determined based on time-dependent inverse problem for which the weights on different records are not fixed over time, as described in our published articles.

19. Have you made available via FTP any specialized computer studies, such as Matlab scripts, in connection with your temperature reconstruction? If not, would you make any such scripts used in developing the temperature reconstructions in MBH98 and MBH99 available through NGDC/paleo?

The methodologies have been described, and other climate researchers have independently, successfully implemented the methodology, e.g.: Zorita, E., F. Gonzalez-Rouco, and S. Legutke, Testing the Mann et al. (1998) Approach to Paleoclimate Reconstructions in the Context in a 1000-Yr Control Simulation with the ECHO-G Coupled Climate Model, *Journal of Climate*, 16, 1378-1390, 2003.

20. Do you claim your method of reconstructing past temperature from proxies is the only correct one? If not, please submit some published papers that use methods you consider to be correct as well. If you do consider yours the only correct method, can you provide a list of names of scientists whom you have contacted to tell them they are using the wrong methods in their work?

The question is based on the false premise that my colleagues and I use any one particular "method" of reconstructing past temperatures from proxy data. In fact, I have published on the application of at least five fundamentally independent methods for using proxy data to reconstruct past climate patterns in the peer-reviewed literature. Examples of the applications of different methods can be found in the following peer-reviewed scientific publications:

- 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.
- Mann, M.E., Jones, P.D., Global Surface Temperatures over the Past two Millennia, *Geophysical Research Letters*, 30 (15), 1820, 10.1029/2003GL017814, 2003.
- Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., Optimal Surface Temperature Reconstructions Using Terrestrial Borehole Data, *Journal of Geophysical Research*, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.
- D'Arrigo, R.D., Cook, E.R., Mann, M.E., Jacoby, G.C., Tree-ring reconstructions of temperature and sea level pressure variability associated with the warm-season Arctic Oscillation since AD 1650, *Geophysical Research Letters*, 30 (11), 1549, doi: 10.1029/2003GL017250, 2003.
- Mann, M.E., Bradley, R.S., and Hughes, M.K., Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries, *Nature*, 392, 779-787, 1998

On occasion, there are approaches used that are not adequate. For example, the approach of simply counting papers and not properly defining what constitutes an anomaly, as was the case for the paper by Soon et al. (2003), is most decidedly not adequate. Also, the analysis approach used by McIntyre and McItrick (2003) in which the authors attempted to reproduce the results of the previous study of MBH98 based on an analysis which used neither the same data (the authors eliminated the majority of data used by MBH98 for the first two centuries of the reconstruction), or method as the original authors, was woefully inadequate. In fact, this latter study was described as "seriously flawed" and "silly" in a recent article in USA Today ("Global Warming Debate Heats Up Capitol Hill", 11/19/03). When deeply flawed studies such as this are published, I am interested in determining what errors have been made and, if necessary as in this latter case, promptly submitting a rebuttal to the peer-reviewed scientific literature to ensure that the scientific community is not misled by the use of inadequate approaches. To my knowledge, I am not considered to be shy in offering criticism where criticism is due.

21. If there are other acceptable methods, did you try any of them on your data set prior to its publication to see what the results would be? If so would you please submit the results? If not, have you done so since? Why do you claim your multi-proxy results represent a ³robust consensus,² as you said in your *Eos* publication, if you have not verified that its results would also be obtained using other acceptable methods?

As demonstrated both in the *Eos* article, and the various references provided in my response to question 4, about a dozen different recent estimates based on a variety of data and approaches, published by different groups, yield statistically indistinguishable histories of Northern Hemisphere mean temperature changes in past centuries. I define such a result as characterizing a "consensus".

22. Did you at any time prior to publication compute the analysis up to 1984 or later? What were the results? If you did not, even though you had sufficient data, why not? If you did but you did not use those results, explain why. If the results were different, where did you publish a discussion of those differences? If they were the same why did you delete them? Why, in other words, did you throw out data for the period of maximum interest?

Most of the proxy records used in MBH98 and MBH99 ended by 1980, limiting the useful upper limit to the calibration period used. A more recent paper (in press) extends proxy-based hemispheric temperature reconstructions through the mid 1990s, demonstrating the ability of the reconstruction to capture the accelerated warming evident in the instrumental record since 1980. We would be happy to provide a copy of this paper to be made part of the official Senate record when it is formally published.

23. On your web site http://www.ngdc.noaa.gov/paleo/ei/data_supp.html where you explain the assembling of the data base for your 1980 paper you say: ³Small gaps have been interpolated. If records terminate slightly before the end of the 1902-1980 training interval, they are extended by persistence to 1980.² Does this mean you made up some observations to fill in blank spots in the data records? Have you ever provided a complete public listing of all the data you made up? Please provide such a listing now. Of the 112 proxies, in how many of them did you fill gaps? Why in some of them but not others? What is the longest interval of time over which you filled in missing observations?

Extension of missing values by 'persistence' of the final available value is a typical statistical approach to estimating small amounts of unavailable data at the end of a time series (see e.g. the textbook by Wilks, referred to in the response to question #14). The fact that this approach was used to infill a modest number of missing observations between 1972 and 1980 was described in the *Nature* supplementary information.

All of the data used in our study have been available since July 2002 on the public ftp site:
<ftp://holocene.evsc.virginia.edu/pub/MBH98/>.

24. What was the effect on your results of filling in the missing data? Did you run your analysis without it? Please submit the results when the filled-in data are dropped from the analysis. If it changes your results, where is that discussed? If it makes no difference, why did you do it?

The use of infilled data has essentially no effect on the reconstruction, as demonstrated by the fact that the same result is achieved if a 1902-1971 calibration period (which predates the use of any infilled proxy data) is used instead of a 1902-1980 calibration period. It is advisable to use the full 1902-1980 calibration interval, however, because the increased statistical constraint provided by the lengthening of the calibration period more than offsets the impact of the use of a modest amount of infilled data in a small number of series.

25. Do you agree that statistical methods based on linear extrapolation from data representing the far extreme of the line are associated with an added error/uncertainty? If so, how was this incorporated into the assessment of the error/uncertainty in your temperature reconstructions please provide citations from your publications. If not, please explain why the uncertainty envelope of a linear regression grows larger as a function of the distance from the mean of the data used to fit the parameters and why this was not included in your research.

The so-called "leverage effect" which the question appears to refer to, is taken into account through consideration of the spectrum of the calibration residuals, allowing for resolution of any enhancement of uncertainty as a function of frequency (see MBH99). Alternatively, the uncertainties can be evaluated from an independent sample (i.e., cross-validation, rather than calibration, residuals) that eliminates any influence of calibration period leverage in the estimation of uncertainties. Both approaches give similar results [e.g. Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, *Journal of Climate*, submitted, 2003].

26. Please describe the peer review process that took place with respect to your Forum article that appeared in EOS on July 8, 2003. If, according to the AGU, the EOS Forum contains articles stating a personal point of view on a topic related to geophysical research or the relationship of the geophysical sciences to society, how can you claim that your article is peer reviewed?

The article was independently reviewed and evaluated for suitability for publication by an editor who has expertise in the particular subject area. The associated process is correctly described as "peer review". Appropriate to the relatively short and non-technical nature of *Eos* "Forum" pieces, the associated peer review process is not as extensive as that employed for articles in the more technical literature such as *Geophysical Research Letters*, or *Journal of Geophysical Research*. I would suggest that the questioner contact representatives at AGU for more details on the peer-review process employed for their different journals and paper categories.

27. Do you claim that producing estimates of past climate states is an exact science? If so, explain why different authors can get such significantly different results when investigating and reconstructing past temperature, and detail the errors that other authors must have made. If not, explain how there can be, as you put it in your *Eos* article, a ³robust consensus² regarding the correct estimate of the climate state of the past millennium.

The term "exact science" is generally not used, or considered meaningful or appropriate by scientists, as science almost always involves the testing of hypotheses based on the use of intrinsically uncertain data or observations. Consistent with this fundamental aspect of nearly all scientific endeavors, my colleagues and I, and other researchers in the paleoclimate community, typically interpret the results of paleoclimate reconstructions within the context of sometimes substantial associated uncertainties. When a large number of estimates agree with each other within estimated uncertainties, and those uncertainties are modest enough to still allow for non-trivial conclusions (for example, that late 20th century warmth is anomalous in a long-term context), those conclusions can be considered as both "robust" and a "consensus".

28. Please describe the peer review process that took place with respect to your 1999 Geophysical Research Letters paper. What were the criticisms or improvements suggested by the referees? Why was no reference made to the anomalous global warming caused by the very strong El Niño event of 1997-98 in your paper? Is this 1999 paper a continuation of your 1998 paper in Nature where you stopped your reconstruction at AD 1400?

The comments of reviewers on a manuscript are considered a confidential matter, involving the editor, reviewers, and authors. Providing these comments for public record would be ethically questionable, and probably violates the confidentiality policies of the associated journals. Minor suggestions were made by the reviewers and editor, and addressed to their satisfaction prior to the acceptance and publication of the paper.

29. In Mann and Jones 2003 Geophysical Research Letters, did you change your methodology in the reconstruction of the hemispheric or global scale temperature from your prior publications? If so, why did you, and what is the rationale for the change of approach?

The question is wrongly premised, as it presumes, through the use of the language "change your methodology" that scientists only have one particular methodological approach that can be applied to a problem at hand. As discussed in my answer to question 20, my research has involved the use of a variety of different methods for reconstructing past climate patterns from proxy data. The paper by Mann and Jones (2003), for example, uses a coarser resolution proxy dataset than MBH98/ MBH99 and a compositing methodology that allows for the reconstruction of decadal, but not annual, changes, and the reconstruction of hemispheric mean, but not spatially-resolved, patterns of temperature in past centuries. In doing so, the study was able to make use of a more restricted set of temperature records available over a longer timeframe than those used in previous high-resolution proxy reconstructions of hemispheric temperature change.

30. Did IPCC carry out any independent programs to verify the calculations that you made in MBH98 or MBH99? If so, please provide copies of the reports resulting from such studies.

It is distinctly against the mission of the IPCC to "carry out independent programs", so the premise of the question is false. However, the IPCC's author team did engage in a lively interchanges about the quality and overall consistency of all of the papers as the chapter was drafted and revised in the course of review.

31. Did IPCC carry out any independent quality control on the data that you used in MBH98 and MBH99? If so, please provide copies of the reports resulting from such studies.

The IPCC doesn't "carry out studies", so the premise of the question is false. The IPCC instead depends that the normal scientific peer-review process, especially when done in a leading journal, has ensured an

acceptable level of quality. In addition, the IPCC does check to see if any criticisms have been raised post-review in comments and response to the journal articles.

32. Did IPCC carry out any studies to validate the statistical procedures and methodologies used in MBH98 and MBH99? If so, please provide copies of the reports resulting from such studies.

The IPCC doesn't "carry out studies", so the premise of the question is false. Instead, as indicated above, the IPCC relies on earlier stages of review to cover such matters.

33. Has any organization other than IPCC or your associates carried out any independent programs to verify the calculations that you made in MBH98 or MBH99? If so, please provide copies of the reports resulting from such studies.

I know of no "organizations" that carry out "independent programs" to verify calculations of individual co-authors. If the question is, have other scientists reproduced the basic results of MBH98 and MBH99, the answer is yes. Numerous other groups (see the dozen or so independent estimates of various groups shown in Figure 1 of: 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) have produced reconstructions that are remarkably similar to those of MBH98 based on a variety of data and methods. Refer back to my answer to question 4 for further details. I would like to see each of these papers made an official part of the Senate record.

34. Has any organization other than IPCC conducted independent quality control on the data that you used in MBH98 and MBH99? If so, please provide copies of the reports resulting from such studies.

The IPCC doesn't "carry out studies", so the premise of the question is false. The data used by MBH98 (and MBH99) were produced by other researchers, not Mann and colleagues. It is thus not clear what kind of "independent quality control" is being referred to here. However, it is fair to say that each of these papers has been subject to rigorous peer review in a leading scientific journal, which is considered by scientists to be an independent quality control process. We are aware of no criticisms of the datasets in the peer-reviewed scientific literature.

35. Has any organization other than IPCC carried out any studies to validate the statistical procedures and methodologies used in MBH98 and MBH99? If so, please provide copies of the reports resulting from such studies.

The IPCC doesn't "carry out studies", so the premise of the question is false. If the question were asked: Have other independent groups tested the methodology of Mann et al (1998) in a publication in the peer-reviewed climate literature, the answer would be "yes". I would refer the questioner to the following paper:

Zorita, E., F. Gonzalez-Rouco, and S. Legutke, Testing the Mann et al. (1998) Approach to Paleoclimate Reconstructions in the Context in a 1000-Yr Control Simulation with the ECHO-G Coupled Climate Model, *Journal of Climate*, 16, 1378-1390, 2003.

The paper arrives at the conclusion that the methodology of MBH98 performs well with networks of data comparable to those used by MBH98.

36. Have you ever received any communications that suggested that there might be computational errors in MBH98 or MBH99? Please provide such communications together with any responses.

I receive many emails, often from list-serves of self-professed "climate skeptics" making numerous spurious claims against my work and that of many of my colleagues. I have received no correspondence providing credible evidence of any errors in our work. Nor has any such credible evidence been published in the peer-reviewed scientific literature.

37. Did the peer reviewers for Nature in MBH98 carry out any independent quality control or validation studies? If so, please provide copies of such reports.

Neither I, nor authors of peer-reviewed journal articles in general, are made privy to the detailed analyses that peer reviewers may or may not have performed in the process of reviewing a manuscript. Authors only receive the comments that were selected to be made available to them by the reviewer and editor. This question is thus impossible to answer. Numerous other groups (see the dozen or so independent estimates of various groups shown in Figure 1 of: 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) have produced reconstructions that are remarkably similar to those of MBH98 based on a variety of data and methods. See my answer to question #4.

38. Did the peer reviewers for Geophysical Research Letters in MBH99 carry out any independent quality control or validation studies? If so, please provide copies of such reports.

See response to question #37.

39. How many people have requested the underlying digital information in MBH98? Please provide dates of such requests and dates of your reply.

My collaborators and I have not kept a specific record. The data has been provided to any scientific groups that have requested it, and has been made available on an open access basis through a public ftp site: <ftp://holocene.evsc.virginia.edu/pub/MBH98/>, since July 2002.

40. Were you one of the primary or lead authors of IPCC/TAR chapter 2?

The convening lead authors of chapter 2 of the IPCC TAR were Dr. Chris Folland and Thomas Karl. I was one of eight additional co-authors contributing to chapter 2.

41. In your capacity as IPCC/TAR author, did you prepare any drafts that referred to your own papers? Please provide all drafts that you prepared for IPCC.

I contributed to numerous sections of the chapter and provided contributions that referenced the work of the leading paleoclimatologists, which includes me and many of my colleagues. Those interested in drafts of IPCC chapters should inquire of the appropriate IPCC working group. I am not in possession of such drafts, and even if I were, I would not be at liberty to distribute the various drafts of the chapters of the report.

42. Was any language from your drafts referring to your own reports ultimately used by IPCC/TAR? Please provide highlighted versions from IPCC.

The wording of the question is unclear. If the question is, did I, in my contributions to the chapter, provide summaries that included references to my own work as well as that of other scientists, the answer is of course yes. Since each of the authors was asked to contribute sections related to their particular areas of expertise, and since the IPCC authors were chosen from among the leading scientists in the world, it would be distinctly odd if it were not the case that most authors referred to their work, as well as that of others, in their contributions.

43. Did IPCC/TAR have any policies governing how lead authors used their own work? Did IPCC/TAR have any quality control procedures in the event that a lead author used his own work? Please provide a short summary of your understanding of such procedures.

I am not a spokesperson for the IPCC. However, it is my understanding that 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 representing the full range of expert science. 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. (and in the case of the US government review, an invitation to submit comments to be considered to be forwarded to the IPCC is published in the Federal Register, enabling all to participate in this review). With the comments available at each stage of the review process, the 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

44. Did MBH98 and MBH99 use any proxy series, which were either unpublished or which resulted from unpublished calculations, which you carried out? If so, please identify, and detail how you verified those unpublished results.

MBH98 and MBH99, as many studies, made use of newly available data that had not yet been published by the original authors providing those data, and thus was provided to Mann and colleagues on a provisional basis that they not release the data until the authors had a chance to publish the records themselves. After all of the data used had been published, the full dataset used by MBH98 and MBH99 was made available in the public domain on the public website: <ftp://holocene.evsc.virginia.edu/pub/MBH98/>

45. Despite solar variability over the last two millennia, your analysis concludes the Northern Hemisphere average temperature has remained virtually constant. What mechanism or mechanisms are responsible for negating the influence of the sun? Do climate models (GCMs) exhibit the same lack of response to solar forcing that your analysis implies? If not, why are model simulations at variance with your conclusions and how does that limit their applicability for future climate scenario assessments?

The question is falsely premised on several levels. No reasonable description of the reconstructions that we or others have produced of temperature variations in past centuries would characterize them as "virtually constant". The reconstructions performed by my group and others indicate an amplitude of variability that consistent with expectations from models driven with estimates of past radiative forcing including solar and radiative forcing, and allowing for the added role of internal unforced variability [see e.g. Crowley, T.J., Causes of Climate Change Over the Past 1000 Years, *Science*, 289, 270-277, 2000]. Indeed, it has been shown that the model-predicted pattern of surface temperature response to solar forcing in past centuries

closely resembles that estimated from the temperature reconstructions that my colleagues and I have performed [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].

46. How did the temperatures of the mid-Holocene Optimum Period (6000 to 9000 BP) compare with those observed today? Was it a global or a local phenomenon? What was or were the cause or causes of any temperature anomalies in that period? What is the cause of the 104 to 105 year timescale changes in deuterium, oxygen isotope, etc., concentrations in ice core records? Are such changes global or local?

Paleoclimate experts have established that 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 about the same or a few tenths of a degree C cooler than today (the late 20th century) during this time period [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; 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, *Paleoceanography*, 17 (3), (19)1-13, 2002.]. That's a far cry from the very out-of-date claim made by Dr. Legates in his testimony. Dr. 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.

47. It has been observed that in the past, carbon dioxide concentrations have sometimes lagged air temperature trends; that is, changes in air temperature have subsequently sometimes resulted in changes in carbon dioxide concentrations. Do you agree with those results from expert researchers? Why or why not?

The question mis-characterizes the evidence that has been provided by paleoclimate researchers. The studies that the questioner appears to be alluding to, demonstrate a phase relationship between ice core CO₂ estimates and *local* temperature variations at the site of the ice core. Furthermore these local temperature estimates are indirectly inferred from oxygen isotopes, based on quite uncertain assumptions regarding oxygen isotope paleothermometry and neglecting possible biases due to the variable seasonality of local accumulation. As local temperature variations at the site of the ice core have an unknown relationship with global mean temperature variations (which are far more dominated by lower latitudes which occupy the majority of the Earth's surface area), the phase relationships between past CO₂ and global mean temperature variations are not known. In spite of these qualifications, it is not at all implausible that

the geologic record indicates that at some times the CO₂ increase may lag the initial temperature increase; such a situation would be expected, for example, if the change in climate was initiated by a change in the orbital geometry that affected the distribution of solar radiation, and then the slow warming drove CO₂ from the warming ocean into the atmosphere. It is because of the many possibilities for how different processes can interact that it is essential to not simply base a conclusion on an apparent correlation without evaluating the underlying physical mechanisms for that particular period.

48. Are there any time periods for which atmospheric CO₂ content has changed without a concomitant change in global air temperature? Are there periods when the atmospheric CO₂ content was relatively high but global air temperatures relatively low?

In his testimony, Dr. Legates indicated that there were historical cases where the temperature has gone up, but that CO₂ has fallen. It may well be 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. However, 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 a very long time period. 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.

49. Two independent and nearly direct measures of surface temperature (deep borehole reconstructions) over the past several millennia have been published for Greenland (Dahl-Jensen et al 1998) and the Middle Urals (Demeshko and Shchapov 2001). The local surface temperature at these locations is highly correlated with global temperature on 10-year time scales and longer ($r^2 > 50\%$ 10 yr with agreement increasing for longer averaging periods). Both reconstructions independently show their local surface temperatures were at least 1 °C warmer for century-scale mean temperatures around A.D. 900 than the latter half of the 20th century, translating into a global anomaly of at least +0.2 °C relative to today.

This further implies that even higher global temperature anomalies for shorter periods, such as half-century or decadal periods, were observed about 1000 years ago. Why do these two robust measures of local and global approximations differ greatly from Mann et al. 1999?

The question is wrongly premised on multiple levels. Firstly, the correlations cited are completely wrong. No citation to the peer-reviewed literature is provided, so it is difficult to determine how these numbers were arrived at. I therefore proceeded to analyze the appropriate surface air temperature gridpoint data from the Climatic Research Unit of the University of East Anglia myself. I found that only after 1922 is there adequate coverage (>50% areal coverage) to estimate a meaningful Greenland areal-mean temperature. For the period back to 1922, the linear correlation between the Greenland and Northern Hemisphere mean temperature is $r = -0.06$ (negative!), nor is there a significant correlation at decadal or

longer timescales. In fact, the trends in the two series during the latter 20th century are of opposite sign. So the numbers cited are completely spurious.

It is in fact well known by the climate community that there are fundamental physical reasons *why* temperatures in Greenland are, in general, poorly correlated with Northern Hemisphere mean temperature. Owing to the strong overprint of processes, such as the North Atlantic Oscillation, and changes in coupled ocean-atmosphere processes in the North Atlantic that impart a large regional overprint of temperature variation in this region, both negative and positive correlations with Northern Hemisphere mean temperature can be found, depending on the time period and region of Greenland analyzed. .

The Dahl Jensen et al Greenland borehole data may indeed be useful temperature proxy data for the regions they represent, and they have been used in reconstructions of Northern Hemisphere mean temperature, with caveats due to their extremely low temporal resolution (see Mann and Jones, 2003). While the two Greenland borehole records show significantly different histories over the past 1000 years (which is expected since temperature trends vary markedly depending on the region of Greenland in question), one of the two records does correlate well with the instrumental Greenland record over the period of mutual overlap. It shows the mid 20th century warm peak, followed by the latter 20th century cooling peak, just as the instrumental Greenland annual mean temperature record does. However, instrumental Northern Hemisphere mean temperature has, in contrast, *warmed* markedly during the latter 20th century.

The Greenland borehole temperature reconstruction may tell us something about temperatures in Greenland over the past few millennia even though the two different Greenland borehole records show some differences between them. But these results are unlikely to tell us much, if anything, about Northern Hemisphere mean temperature trends. Indeed, Dahl-Jensen et al have never, to my knowledge, claimed in their studies that that temperature variations in the two regions of Greenland reconstructed (which themselves show significantly different histories over the past 1000 years) are representative of Northern Hemisphere mean temperatures, and I would be surprised if the authors were comfortable in having their data represented as such.

50. In your view what should the federal government do in response of rising concentrations of CO₂? What would be the climate impact of this effort?

In my view, the congress and the Federal Government should be taking the scientific findings of the mainstream research community very seriously and should stop focusing so much attention on the poorly conducted and distracting nitpicking of the various contrarian scientists. The IPCC assessments represent the most authoritative reviews of the science and have been unanimously endorsed by all of the participating nations of the world—it is time to pay attention to their findings. Exactly what steps should be taken and how fast this should be done are policy questions that members of this body should be responsibly and thoughtfully addressing. The long residence timescales of anthropogenic greenhouse gases, and the lags in the response of the climate system (e.g. sea level rise) to already realized increases in greenhouse gas concentrations dictate, however, that there are potentially significant costs to delayed action.

51. Approximately what percentage of the temperature increase in the observational record over the last 100 years would you attribute to anthropogenic causes? What percentage would you attribute to increased urbanization? What percentage would you attribute to non-urbanized land use changes? What percentage would you attribute to natural (solar, volcanic, etc.) variability? What percentage would you attribute to ³internal² climate variability? What percentage would you say results from other or unexplained sources? Give estimates for the years 1900, 1940, 1980 and 2000.

A cursory review of the available 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. 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 potential 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 (e.g. the 1957 eruption). However, much of the overall warming of the globe during the 20th century (which is between 0.6° and 1.0°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, consistent with the conclusions of the IPCC Third Assessment Report that most of the warming of the past 50 years is attributable to human influences.

52. What was the earth's climate like the last time the atmospheric concentration of carbon dioxide was near today's level of about 370 parts per million (ppm) and what were past conditions like when concentrations were at 550 ppm? Detail the factors that cause the global carbon cycle to produce these high levels of atmospheric carbon dioxide.

It is not precisely known what the "earth's climate" was like the last time carbon dioxide levels were near 370 ppm (let alone 550ppm) because the available paleoclimate evidence available this long ago are quite uncertain and incomplete. That having been said, it is believed, based on the available proxy information and faunal/floral evidence, that global temperatures were probably several degrees higher than they are today when CO₂ concentrations neared 550ppm, roughly consistent with model simulation results. 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 a time when 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 a time when CO₂ levels were 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.

53. In your vitae, you indicate that you serve on the panel for NOAA's Climate Change Data and Detection (CCDD) program, while at the same time, you also have received large grants from this program. Please explain your role on the panel, how grant submissions are evaluated, and why there is no conflict of interest or impropriety associated with members of a panel receiving large grants from the program for which they serve.

I am not a spokesperson for NOAA, and would suggest that the questioner contact the appropriate NOAA agency officials for further information on their conflict of interest and disclosure policies. That notwithstanding, however, I would note the following points. Government funding agencies seek to draw upon the leading experts of the field in their panels. Inevitably, this means that specific science programs within NSF and NOAA invite to their review panels scientists who typically submit proposals themselves to those panels. Scientists are also asked to disclose any conflicts of interest they might have in reviewing a proposal, and are asked to recuse themselves from any participation in discussions related to proposals that they might have even peripheral involvement with. In my involvement in both NSF and NOAA panel

reviews, I have on many occasions recused myself from reviewing or discussing a proposal based on such considerations.

54. Do you receive any income or reimbursement (travel, speaking fees, etc.) 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 approximate amount of money that they represent.

All income or travel expense reimbursement funds that I have received to my recollection have come from academic institutions, government funding agencies such as NSF, NOAA, NASA, DOE, and scientific organizations such as the American Geophysical Union and University Corporation for Atmospheric Research (UCAR). I am not familiar with any advocacy positions that have been taken by any of these institutions or organizations regarding the UN Framework Convention on Climate change, or legislation before the United States Congress that would affect greenhouse gas emissions.