United States Senate Testimony
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Follow-up Questions from Senator James M. Jeffords

Do you maintain that the proxy-based temperature reconstructions of the Mann and colleagues studies do not extend into the latter half of the 20th century?

With regard to the Mann *et al.* (1998) article in *Nature* and the Mann *et al.* (1999) article in *Geophysical Research Letters*, I do maintain that their reconstructions do not extend past 1980, as is documented in their articles. Moreover, I would note that Dr. Mann's colleagues (Dr. Raymond Bradley and Dr. Malcolm Hughes) have stated, "[Mann *et al.*'s] current proxy-based reconstructions do not extend to the end of the 20th century, but are patched on to the instrumental record of the last 2-3 decades" (Bradley *et al.*, 2003:116).

Bradley, R.S., K.R. Briffa, J. Cole, M.K. Hughes, and T.J. Osborn, 2003: The climate of the last millennium. In: Alverson, K., R.S. Bradley, and T.F. Pedersen (eds.) *Paleoclimate, Global Change and the Future*. Springer Verlag, Berlin, 105-141.

Do you claim that the Mann study does not reconstruct regional patterns of temperature change in past centuries?

Dr. Mann, in the published literature discussed at the hearing, had only presented reconstructed time-series for the northern and southern hemispheres. More recently, Mann and Jones (2003), in their latest *Geophysical Research Letters* article, have provided a global time-series, but I have seen no time-series for regions smaller than a whole hemisphere. Note too that proxy data are quite limited spatially, such that detailed regional assessments are tenuous at best.

Do you maintain that the Mann study extrapolated global temperature estimates from the northern hemisphere?

I maintain, based on the documentation provided by the Mann *et al.* (1999) article in *Geophysical Research Letters*, that northern hemisphere temperature estimates were extrapolated from data taken from the *southern hemisphere*. I have no quarrel with data from the northern and southern hemispheres being used to estimate a global air temperature time-series – the egregious error committed here by Dr. Mann is to provide estimates, purporting to represent the northern hemisphere, that are determined from data located in the southern hemisphere. Of the twelve data sources used by Mann *et al.* (1999), four were located in the southern hemisphere, including two in the tropics and two in the middle latitudes (Tasmania and Patagonia). It is this extrapolation to which I objected in the hearings.

Do you maintain that historical and instrumental temperature records that are available indicate colder northern hemisphere temperature conditions than the Mann *et al*. northern hemisphere temperature reconstruction in the past centuries?

Yes. My concerns are best stated by Esper *et al.* (2004:113), "Briffa (2000) and Esper *et al.* (2002) display a pronounced [Medieval Warm Period] followed by a significant 200-300-year-long cooling trend associated with the Little Ice Age. Such a trend is broadly absent in Mann *et al.* (1999) and Jones *et al.* (1998)." The Briffa and Esper *et al.* studies utilize much of the same proxy records as the Mann *et al.* and Jones *et al.* studies and the work by Soon and Baliunas has clearly demonstrated that most of the proxy records indicate the Medieval Warm Period-Little Ice Age trend. Moreover, Esper *et al.* (2004) further demonstrate that the difference between Dr. Mann's reconstructions and those of Briffa and Esper *et al*. is *not* caused by insufficient data used by Esper *et al.* (a false charge leveled by Mann and Hughes in their 2002 article) *nor* is it due to differences in spatial coverage between the two data sources, as suggested earlier by Esper *et al.* (2002). They conclude it is "the use of different tree ring de-trending methods and their varying abilities to retain millennial-scale variations" (p. 113). This solidly illustrates that the research of Dr. Mann is the outlier, which does not agree with the preponderance of the evidence indicating the Little Ice Age and Medieval Warm Periods were at least hemispheric in scope.

Esper, J., D.C. Frank, and R.J.S. Wilson, 2004: Climate reconstructions: Low-frequency ambition and high-frequency ratification. *EOS, Transactions of the American Geophysical Union*, **85**(12):113,120.

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?

It is my understanding that the mid-Holocene optimum period was warmer as the conditions that existed during the 20th century. Reconstructions that far back are even more tenuous than those for the last millennium, but the current understanding is that it was a climatically warmer period – possibly 2°C warmer than present over most of the earth's surface and as high as 4°C warmer in some places.

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?

I will spare the statistical details of how it *should* be done from a scientific standpoint. However, any statistical analysis *must* appropriately assess the uncertainty in the reconstructions. Uncertainty (*i.e.*, the error) arises from several sources including

- the estimation of hemispheric temperatures from sparse and irregularly-distributed instrumental records that under-represent the oceans, high latitude regions, mountainous areas (*i.e.*, high altitudes), and non-populated landscapes,
- the estimation of large-scale proxy composites from a severely limited number of locations that exhibit locational biases even more drastic than those of the instrumental record,
- the ability of a single proxy location to represent the temperature of the local area,

- the statistical method used to 'match' the proxy record with the instrumental record over the overlapping period, and
- the observed variability of both the proxy record and the instrumental record.

Dr. Mann's research woefully under-represented the uncertainty in his reconstruction as he failed to take proper account of all possible sources of error.

Moreover, it requires an appropriate characterization of the trend in the instrumental record over the last 50 years. Recently, Drs. Soon and Baliunas and myself (2004) examined carefully the results presented by Dr. Mann and found that his characterization of the last 50 years has been exaggerated – and more so in his later publications. His representation of air temperature increases during the twentieth century is 0.95°C, or 0.35°C higher than that agreed upon by the IPCC and reiterated in Dr. Steven Schneider's congressional testimony before Senator McCain's committee. We examined the possibility that data padding (adding/inventing additional data beyond the end of the record to compute averages) could have been the reason for the erroneous characterization but concluded that we could not resolve the reason why Dr. Mann's figure is so much higher than that cited by the IPCC report.

Soon, W.-H., D.R. Legates, and S.L. Baliunas, 2004: Estimation and Representation of Long-Term (>40 year) trends of Northern-Hemisphere-gridded Surface Temperature: A Note of Caution. *Geophysical Research Letters*, **31**(3).

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 record?

I do not make such a claim. My claim is that the statistical methods employed by Dr. Mann are considerably flawed – to the point of rendering his claims scientifically invalid. Others, such as Briffa and Esper *et al.*, have attempted similar reconstructions and have been able to resolve the low-frequency (*i.e.*, the century-to-millenial scale variability). Thus, I have asked the question, "Why is the Mann *et al.* research the outlier, when it comes to resolving the Medieval Warm Period and the Little Ice Age?" Drs. Soon and Baliunas (and I) evaluated whether individual proxy records exhibit these two events and found that they are present in a large majority of the records. The independent work of Briffa and Esper *et al.* both show marked conditions that correspond to the Medieval Warm Period and the Little Ice Age – in complete agreement with the individual proxy records. The recent conclusion of Esper *et al.* (2004) is that the Mann *et al.* research is an outlier due to a flawed use of a tree ring de-trending method that is unable to retain millennial-scale variability. It is the inappropriate statistical analysis applied by Dr. Mann and his colleagues that cause his results to be at variance with virtually every other study (individual records or composite reconstructions) and even the conventional wisdom. Thus, I claim that appropriate statistical methods <u>*do*</u> exist, but that the methods used by Dr. Mann and his colleagues were inappropriate or that they were not applied correctly.

In determining whether the temperature of the "Medieval Warm Period" was warmer than the 20th century, does your paper analyze whether a 50-year period is either warmer or wetter or drier than the 20th century? Why is it appropriate to use indicators of drought and precipitation

directly to draw inferences of past temperatures? Please list peer-reviewed works that specifically support the use of these indicators for inferring past temperature.

With all due respect, I believe this question shows a misunderstanding of our research. Let me clarify. It was not our intent to show that a drought or above-normal precipitation implied air temperature; rather, we were properly characterizing the climatic response that is represented in proxy records.

Consider a simple example that I use in my introductory level course in climatology and hydrology. Take a potted plant and place it on your windowsill. Give it ample sunlight and keep the air temperature in the mid-70s (Fahrenheit) all year long. However, do not give it any water at all during the year. You will find that the plant does not grow. If you were to use that plant as a proxy indicator of temperature, would it be fair to characterize the temperature of your house as being 'below normal'? Of course not! Clearly, plants respond to moisture conditions in addition to temperature changes – and in many parts of the world, water and not air temperature is the limiting factor. Moreover, too much water can be detrimental to plant growth since plant roots need both water and air in the pore spaces of the soil (which is why your potted plant has holes at the bottom).

Thus, in our analysis, we could not simply assert that a lack of tree ring growth could be attributed to a decrease in air temperature or that significant growth implied above normal warmth. We correctly argued that a lack of growth could be representative of either below-normal temperature conditions or abnormal rainfall. Thus, it would have been easier for us to have simply taken the stance that tree-ring growth implies air temperature, but such a characterization would have been scientifically unsound.

On the other hand, Mann *et al.*'s 'hockey stick' includes many proxy records that are moisture-sensitive but Mann *et al.* contend that the observed changes represent only air temperature fluctuations. This is highly misleading and deceptive.

Do you maintain that any two 50-year periods that occur within a multi-century interval can be considered 'coincident' from a climatic point of view?

Despite Dr. Mann's characterization of air temperature estimates for each year over the last 1800 years (presented in his digital archive with a ridiculous resolution of 0.00001°C), it is extremely difficult to pinpoint such exact values from proxy records. Thus, we averaged over 50-year periods and examined a larger range (more than a century) in our paper. Given the uncertainty and given the nature of climatic forcings, I would argue the answer to the question is 'yes'.

Do your two recent studies employ an analysis (that is, a statistical or analytical operation performed upon numerical data) of a single proxy climate record?

I am not sure what is meant my 'two recent studies'. I believe that this is a question that was meant for Dr. Soon (indeed, he has received the same question), as I was a co-author on only one paper discussed at the hearing.

Does your study provide a quantitative analysis of average temperatures for the northern hemisphere for the last half of the 20th century?

Again, it is not clear what is meant here by 'your study'. If you are referring to the Soon *et al.* research that was published in *Energy and Environment*, the question is ill posed as we assessed proxy records to determine if the Medieval Warm Period and Little Ice Age could be found in the individual records. In earlier work (see below), I produced a global air temperature climatology at 0.5° of latitude by 0.5° of longitude resolution. That study quantitatively assessed average temperatures for the latter portion of the 20th century. It is this research that has given me insights into the statistical procedures by which individual records are averaged to produce hemispheric or global averages – and why I have the expertise to strongly argue that the statistical methods used by Dr. Mann and his colleagues are fatally biased and most assuredly the reason why the research of Dr. Mann is the outlier among the numerous other studies that show contradicting trends.

Legates, D.R., and C.J. Willmott, 1990: Mean seasonal and spatial variability in global surface air temperature. *Theoretical and Applied Climatology*, **41**(1):11–21.

Legates, D.R., 1987: A climatology of global precipitation. *Publications in Climatology*, **40**(1), 84pp. (Note: the air temperature climatology was a by-product of my dissertation to develop a global climatology of precipitation)

Has your analysis produced a quantitative reconstruction of past temperature patterns? Do you have a measure of uncertainty or verification in your description of past temperatures?

The goal of the Soon *et al.* (2003) paper was not strictly to provide a reconstruction of a past temperature time-series; but rather, to determine if the Medieval Warm Period and the Little Ice Age were present in the preponderance of the proxy records. Our analysis was based on evaluating the assessments made by the individual researchers (their assessment of uncertainty and verification) and not re-examining the proxy records directly. Thus, our paper does not reflect our assertion that these two events are present in the proxy records but reflects the assertion of hundreds of individual researchers who are intimately familiar with each of the proxy records.

Your study indicates that you have compiled the results of hundreds of previous paleo-climate studies. Have you verified the interpretation of the hundreds of studies with any of the scientists/authors involved in those studies? If so, how many?

We remained faithful to the comments and characterizations of the authors of the original study – we did not reinterpret their results. All scientists who have provided expertise were cited in the acknowledgements. As Dr. Soon was the first author on this paper, he can provide you with a more complete accounting.

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?

Our research did not focus on a cause-and-effect relationship between carbon dioxide concentrations and air temperature – we simply tried to document the significant air temperature fluctuations of the last millennium. The general understanding is that carbon dioxide concentrations and air temperature have been positively correlated, although the data seem to indicate that historically, air temperature has been the leading variable with changes in carbon dioxide concentrations following as a result. Ruddiman (2003), for example, describes the relationship between ice volume cycles (driven by air temperature fluctuations) and its impact on carbon dioxide concentrations at time scales greater than 10,000 years. Thus, one cannot explain all climate fluctuations on the basis of carbon dioxide alone – variations in solar insolation and volcanic activity also have a profound effect on the climate history of the world.

Ruddiman, W.F., 2003: Orbital insolation, ice volume, and greenhouse gases. *Quaternary Science Reviews*, **22**(15-17):1597-1629.

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?

Yes. A numerous have documented significant changes in air temperature that are not commensurate with changes in carbon dioxide concentrations. Between about ten and twenty million years ago, there are several periods where the correlation between air temperature and carbon dioxide concentrations are negative. Again, this serves to underscore the fact that CO_2 is not the sole driver of climate change.

According to a study published in Science magazine, [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?

In the March 19, 2004 issue of *Science*, Drs. Roger A. Pielke and T.N. Chase provide an enlightening commentary on the Santer *et al.* paper. Pielke and Chase examined the Santer *et al.* claim using the NCAR/NCEP† reanalysis data and concluded, "Globally-averaged tropospheric temperature trends are statistically indistinguishable from zero. Thus, the elevation of the globally averaged tropopause…cannot be attributed to any detectable tropospheric warming over this time period." Moreover, they examined Santer *et al.*'s figure and noted that it "shows little statistical significance…Therefore, in general, regional trends are statistically indistinguishable from zero and a tropospheric effect on tropopause height cannot be inferred." In their response, Santer *et al.* only argue against the use of the NCAR/NCEP reanalysis data, although it is widely used for climate variability analyses. They fail to comment on the lack of statistical significance for large regional areas in their original figure.

Thus, it appears that the conclusions suggested by Santer *et al.* are premature at best and that a thorough analysis must be undertaken before their claims can be taken as fact.

Pielke, R.A., and T.N. Chase, 2004: Comment on "Contributions of Anthropogenic and Natural Forcing to Recent Tropopause Height Changes." *Science*, **303**(5665):1771.

† National Center for Atmospheric Research/National Centers for Environmental Prediction

During the hearing, you stated that the "consensus" of the Association of American Geographers is that the IPCC takes a "rather strong viewpoint." Please explain what you mean by the term "consensus".

With all due respect, I never stated that the 'consensus' of the Association of American Geographers was that the IPCC takes a 'rather strong viewpoint'. My testimony was:

> "Having been president of the Climate Specialty Group of the Association of American Geographers, which is probably the largest group of climatologists available, I know from talking to rank-and-file members that generally my impression is that most climatologists agree it [the IPCC report] takes a rather strong viewpoint."

In fact, it was Dr. Mann who stated:

> "An article that appeared last month in the American Geophysical Union, which is actually the largest professional association of climatologists, showed that indeed that is the ***consensus*** viewpoint of the climate research community." (emphasis added)

I stand behind my statement that 'generally my impression is that most climatologists agree it takes a rather strong viewpoint'. I, however, would suggest this question be posed to Dr. Mann to have him explain what he meant by the term 'consensus' of the climate research community.

How does the membership of the Association of American Geographers compare to the membership of the American Geophysical Union in terms of numbers of professional climatologists.

Unlike the title, *Professional Engineer*, there is no such formal designation as a 'professional climatologist' (although there are about 700 *Certified Consulting Meteorologists*, of which I am one). Not all atmospheric scientists are climatologists either, since the study of weather and weather forecasting, for example, is covered under the rubric of 'atmospheric science' and does not fall under the purveyance of climatology. Nearly all geographers involved with atmospheric science are climatologists, whereas the American Geophysical Union has a large number of people who are not working in climatology or are, for example, biologists who whose research briefly touches on climate. Thus, I stand behind my assessment that the Association of American Geographers is among the largest group of climatologists, although I would argue that the American Meteorological Society is clearly the largest group of atmospheric scientists and not the American Geophysical Union, as Dr. Mann contends.

In your testimony, you said that "that the science [of global climate change] is completely unsettled." That runs contrary to statements from the IPCC, the National Academy of Sciences, the American Geophysical Union, and other highly esteemed scientific bodies. Please clarify what you mean by "completely unsettled."

With all due respect, I again contend that I never stated, "the science [of global climate change] is completely unsettled." In fact, it was Dr. Soon who commented, in a response to a question posed by Senator Thomas:

> "Although I am not able to comment on anything on public policies, I am certainly able to testify that the science is completely unsettled. There are just so many things that we do not know about how the climate really works and what are the factors that cause it to change, to really jump to the conclusion that it will all be CO2."

However, I object to the assertion that Dr. Soon's statement runs contrary to statements from 'highly esteemed scientific bodies' and challenge anyone to find a statement from such entities to argue that we know everything about how the climate works and the factors that cause it to change. In that sense, I fully agree with the above statement made by Dr. Soon that statements to the fact that all climate change is CO_2-induced are completely unfounded.

Do you agree that the carbon concentration increases in the atmosphere over the last century are a result of human activities?

By the middle of the 1900s, it was estimated that two-thirds of the carbon dioxide in the atmosphere came from biospheric sources and one-third from human emissions. The Second Scientific Assessment of the IPCC argues for an estimate of at least half from human emissions. It is likely, however, that most of the *increases* in carbon dioxide in the atmosphere over the last century are a result of human activities. The key question is, "how will these increases translate into changes in the climate"? That debate is still unsettled.

Do you agree that an increase in greenhouse gas concentrations in the atmosphere will lead to a warming influence? If not, please explain why the present concentrations of greenhouse gases has not led to a cooling influence?

Taken in isolation, an increase in greenhouse gas concentrations in the atmosphere will have a net warming influence. However, nothing happens in isolation and the key issues are the concomitant changes in other atmospheric variables, such as atmospheric circulation, cloudiness, and precipitation, as well as changes to the entire energy balance, not just the thermal infrared. It is the positive and negative feedbacks on the climate system that prohibit a simple statement as to the overall effect of increases in atmospheric CO_2.

Is there no agreement that a doubling of the CO2 concentration will cause a warming of about a few degrees, and if not, could you please explain how the natural greenhouse effect works in a way consistent with your explanation?

As with my statement above, a doubling of atmospheric carbon dioxide concentrations will likely lead to a slight warming, but the magnitude and spatial extent are still questionable. At present, the largest warming is likely to occur at night (when cloud reflectance of visible light is minimal) and in the coldest and driest air masses (where the specific heat is lowest and physical principles

show that the rate of change of air temperature as a function of energy input will be the greatest). It should be noted that there is far more to climate than the 'natural greenhouse effect', which focuses only on the longwave (thermal infrared) portion of the electromagnetic spectrum.

Do you agree that continuance of the increasing combustion of fossil fuels will continue the substantial rise in atmospheric CO2 concentration and those concentrations will persist for a century or more?

The IPCC provides a number of different 'plausible' scenarios for fossil fuel consumption. It is difficult to say which scenario is 'more plausible' or to predict advances in technology. In 1900, few could foresee our widespread use of fossil fuels or even imagine that energy could be extracted and harnessed from splitting the atom. Thus, I find it difficult to predict what is likely to be the case in a century or more.

Your testimony suggests that you believe that IPCC conclusions indicate that all climate changes will be due to CO2. What is the specific reference in IPCC documents supporting that position, given that IPCC considers a wide range of forcings?

I believe it is more correct to state that I believe Dr. Mann and his conclusions ***require*** all climate changes to result from increases in greenhouse gases. His 'hockey stick' representation of air temperature over the last two millennia shows no climate variability, save that resulting from anthropogenic influences. Thus, it is his analysis that argues for other forcings to be ignored.

As for the assertion that I believe that "IPCC conclusions indicate that all climate changes will be due to CO2", I provide the following from my opening remarks:

> "However, the IPCC report in the chapter with Dr. Mann as the lead author and his colleagues as contributing authors, also concludes that the research 'support the idea that the 15th to 19th centuries were the coldest of the millennium over the northern hemisphere overall.' Moreover, the IPCC report also concludes that the Mann and Jones research shows temperatures from the 11th to 14th centuries to be 'warmer than those from the 15th to 19th centuries.' This again is entirely consistent with our findings and in contravention of their own error assessment."

I agree with the IPCC that significant climate variability has occurred over the last millennium – climate variability that cannot be explained by carbon dioxide and greenhouse gases alone. It is Dr. Mann's work that dismisses all other forcings and runs contrary to the IPCC report and even the chapter on which he is a co-author. This, once again, supports our primary contention that the research of Dr. Mann is the outlier, which does not agree with the preponderance of the evidence supporting the widespread existence of the Little Ice Age and Medieval Warm Periods.

Could you please explain the basis for your testimony suggesting that "warmer surface temperature leads to more instability or rising air which leads to more cloudiness?"

It follows the basic laws of physics and is the primary reason why we have the possibility of afternoon and evening thundershowers in the summer. To form a cloud, moisture must condense

from the atmosphere. To force condensation of moisture, the air must be cooled to a temperature below the current dewpoint, since the Clausius-Clapeyron equation shows that at saturation, more water vapor will be present in warmer air than will be present in colder air. Most of the known mechanisms to produce clouds and precipitation – frontal lifting in the presence of air mass boundaries, cyclonic rotation, orographic lifting, and surface convection – require air to rise (an exception is advection fog where saturated air flows over a cooler surface). Surface convection occurs when the surface is heated, most often through afternoon solar insolation. This creates a situation where warmer and less dense air at the surface is capped by colder, denser air aloft. If the temperature gradient exceeds the moist adiabatic lapse rate (temperature decrease with height owing to decreasing pressure, offset by energy released by condensing moisture), then when the warmer air rises and cools by expansion (due to the decreasing atmospheric pressure with height) and it will be warmer than the air around it, which will enhance its upward motion. If the rising air reaches saturation, a cloud will form as the air continues to cool while it rises. Thus, afternoon cloudiness in the summer is a direct result of surface heating – solar insolation produces warmer surface temperatures, less stability of the atmosphere, and hence rising air motions, which leads to more clouds.

Do you agree that an increase in convective clouds is likely to reduce the extent of stratus clouds, which are the type of clouds that tend to cool the earth? If you agree, and the coverage of convective clouds, which imply upward and downward air motion, is smaller by comparison, what, in a global context, is the basis for suggesting more heating leads to more cloudiness?

No, I do not. Cumuloform (convective) and stratiform (*e.g.*, stratus) clouds arise as a result of different processes. In both, air reaches saturation, but there is considerable vertical lifting when cumuloform clouds are present. My assertion is that we may see more cumuloform cloud development due to enhanced surface heating when conditions are appropriate for cumuloform clouds to grow.

Convective clouds have a net zero effect overall, although the cooling effect influences mainly the earth's surface while the warming effect is confined to the atmosphere (Ramanathan *et al.*, 1989; Wielicki *et al.*, 1995). Thus, convective clouds provide a negative feedback to the global warming effect of greenhouse gases, by cooling the surface and warming the upper atmosphere – an exact opposite signal from the longwave contribution from an enhanced greenhouse effect. Global warming projections are for increased temperatures in the lower troposphere and decreased temperatures in the upper troposphere. This underscores the interconnectivity of the climate system and argues against a simple response that if greenhouse gases increase, a certain temperature response *must* be forthcoming. There is more to the greenhouse gas issue than just the terrestrial radiation budget.

Ramanathan, V., R.D. Cess, E.F. Harrison, P. Minnis, B.R. Barkstrom, E. Ahmad, and D. Hartmann, 1989: Cloud-radiative forcing and climate: Results from the Earth Radiation Budget Experiment. *Science*, **243**:57-63.

Wielicki, B.A., R.D. Cess, M.D. King, D.A. Randall, and E.F. Harrison, 1995: Mission to Planet Earth: Role of clouds and radiation in climate. *Bulletin of the American Meteorological Society*, **76**(11):2125-2153.

In your testimony, you indicated that "generally the idea is that warmer temperatures lead to more enhanced human activity; colder temperatures tend to inhibit." Please provide any historically accurate references with temperature, not precipitation, documentation that would support this theory.

The 'Holocene Climatic Optimum' (4500 to 7000 years ago) was a period where the temperature was as much as 2°C warmer than present conditions. With it occurred the rise of human civilization and the development of agriculture (see Whyte, 1995), which also had a positive impact (with precipitation changes) on the biosphere as well. The 'Medieval Warm Period' (about 800 to 1400 AD) was associated with warmer conditions globally that also had a beneficial impact on the biosphere. In fact, the conditions were so favorable such that some refer to it as the 'Little Climatic Optimum' and it ushered in the end of the 'Dark Ages' and began the period of 'Enlightenment' (significant increases in science, technology, and exploration). Between about 1600 and 1850 AD, the 'Little Ice Age' was associated with colder conditions and a concomitant decrease in activity of the biosphere. With colder conditions and a more pressing need to locate sources of food, science and technology lagged. Present-day conditions are warmer than those of the 'Little Ice Age' and are on a par with those from the Medieval Warm Period. We have seen an acceleration of developments in science and technology that have not been rivaled in the past. For a complete discussion, see Lamb (1995) who provides numerous examples.

It stands to reason that under warmer conditions, the biosphere tends to thrive (which is why there is any hope of using tree rings as proxy indicators of air temperature). With warmer conditions, agriculture feeds more people over a larger area, more lands are now inhabitable, and people are less concerned with food so that they can focus on enhancing their quality of life. Given that the three major warm periods in the last 10,000 years (*i.e.*, the 'Holocene Climatic Optimum', the 'Medieval Warm Period', and present-day conditions) were associated with enhanced human activity whereas the last major cold period (*i.e.*, the 'Little Ice Age') was associated with a lull in human activity, I continue to stand by my statement.

Lamb, H.H., 1995: *Climate, Change and the Modern World*, 2nd ed., Routledge, London, New York.

Whyte, I.D., 1995: *Climatic Change and Human Society*. Arnold, London, UK.

You stated, "We know historically that as the sun fluctuates in terms of its output, the climate does respond." Please discuss the uncertainty related to this statement.

Historically, the Milankovitch hypothesis – that the orbital variations of the sun, including precession, tilt, and obliquity – has been used to argue for a strong sun-climate connection. While we now understand that Milankovitch variables cannot explain the entire past climate record, a strong relationship between the sun and its effect on the climate of the earth cannot be ignored. Textbooks by Dr. Bradley (coauthor with Dr. Mann on his papers under debate at the hearing) and Dr. Ruddiman (former chair of Dr. Mann's department at the University of Virginia) assert that changes in solar insolation are a major factor that affects the climate on time scales beyond about 10,000 years. Thus, I agree with both of these authors on this topic.

As for my statement that the climate responds to solar variability, I do not think that there is any uncertainty in such a characterization. As more than 99.9% of the energy in the climate system originates from the sun, it is not surprising that the climate should respond to variations in solar insolation.

Bradley, R.S., 1999: *Paleoclimatology: Reconstructing Climates of the Quaternary*, 2nd Edition. Harcourt/Academic Press, 613pp.
Ruddiman, W.F., 2001: *Earth's Climate: Past and Future*. W.H. Freeman & Company, 465pp.

Your conclusion mentioned in the previous questions suggests that the climate response to solar fluctuations is certain. Why is that interaction certain while the science of climate change "is completely unsettled?"

Again, with all due respect, it was not my comment that the climate change issue "is completely unsettled." Dr. Soon made that statement, as I have indicated in my response to an earlier question. Furthermore, I have not asserted that the climate response to solar fluctuations is 'certain'; just more predictable. With respect to the climate history of the earth, a much higher correlation exists between solar variability and air temperature than between carbon dioxide concentrations and air temperature.

In your testimony, you indicated that, in terms of climate forcings, "the sun is the biggest game in town and is not controllable." Could you please summarize the estimates of the record of changes in solar radiation and indicate how they are derived?

As Dr. Soon is an astrophysicist (not an 'astronomer' as portrayed by Dr. Mann to demean Dr. Soon's credentials) and has recently written an excellent book on the topic, I will defer to Dr. Soon's experience. Dr. Soon's response:

> "Soon et al. (1996) had previously suggested that the total irradiance of the Sun could vary between 0.2% to 0.7% over the last 350 years and they showed that those changes can trigger and sustain significant global-scale temperature responses. The total solar irradiance estimates provided in the more recent publication by Bard *et al.* (2000) for the last 1000 years or so also suggest similar amplitudes but the actual physical constraints were based mostly from studies of solar-type stars; for example, as discussed in Soon *et al.* (1996). The emerging evidence from both paleoclimate reconstructions and studies of the Sun and solar-type stars of the last few years is documenting significant multidecadal and centennial scales variability as an important area of for a new research focus (e.g., Tan *et al.*, 2004; Hu *et al.*, 2003, Neff *et al.*, 2001). This is one of the reasons why we had embarked on our independent paleoclimatic study as published in Soon *et al.* (2003). It is important also to recognize the fact that the Sun is not merely a variable emitter in terms of its total light energy outputs but that a comprehensive study of the sun-climate relation must necessarily consider the variable solar outputs in term of its UV irradiance and its charged-particle outputs, especially when considered simultaneously as a modulator of the highly energetic charged particles from the Galaxy (see *e.g.*, Soon *et al.*, 2000).

Bard, E., G. Raisbeck, F. Yiou, and J. Jouzel, 2000: Solar irradiance during the last 1200 years based on cosmogenic nuclides. *Tellus Series B – Chemical and Physical Meteorology*, **52B**(3):985-992.

Hu, F.S., D. Kaufman, S. Yoneji, D. Nelson, A. Shemesh, Y. Huang, J. Tian, G. Bond, B. Clegg, and T. Brown, 2003: Cyclic variation and solar forcing of Holocene climate in the Alaskan subarctic. *Science*, **301**(5641):1890-1893.

Neff, U., S.J. Burns, A. Mangini, M. Mudelsee, D. Fleitmann, and A. Matter, 2001: Strong coherence between solar variability and the monsoon in Oman between 9 and 6 kyr ago. *Nature*, **411**(6835):290-293.

Soon, W. and S.H. Yaskell, 2004: *Maunder Minimum: The Variable Sun-Earth Connection*. World Scientific Publishing Co. http://www.wspc.com/books/physics/5199.html

Soon, W.H., E.S. Posmentier, and S.L. Baliunas, 1996: Inference of solar irradiance variability from terrestrial temperature changes, 1880-1993: An astrophysical application of the sun-climate connection. *Astrophysical Journal*, 472(2):891-902.

Soon, W., S. Baliunas, E.S. Posmentier, and P. Okeke, 2000: Variations of solar coronal hole area and terrestrial lower tropospheric air temperature from 1979 to mid-1998: Astronomical forcings of change in earth's climate? *New Astronomy*, 4(8):563-579.

Tan *et al.*, 2004: Sun-coupled climate connection between eastern Asia and northern Atlantic. *Geophysical Research Letters*, **31**, in press (paper 2003GL019085).

Please compare the relative magnitude of changes in solar radiation and other influences as the IPCC has, indicating where you would take exception with IPCC conclusions and the basis for that contention.

I provide Figure 9 from the Technical Summary of the IPCC Third Assessment Report (TAR) in Appendix 1. I will not comment on all forcings listed, but I will comment on the effect of land surface. The TAR suggests a slight negative forcing, about 0.5 Wm^{-2}, although the level of confidence is given as 'very low'. A recent article by Kalnay and Cai (2003) provides new insight on the effect of land use changes, including urbanization, agriculture conversion, and irrigation. Their results suggest, "that half of the observed decrease in diurnal temperature range is due to urban and other land-use changes. Morover, our estimate of 0.27 degrees C mean surface warming per century due to land-use changes is at least twice as high as previous estimates based on urbanization alone." (p. 528). Given this result, it appears that the IPCC estimate of a slight negative forcing of about 0.5 Wm^{-2} is of the wrong sign and that land use changes account for about half (0.27°C of the 0.6°C argued by the IPCC) of the observed warming over the twentieth century.

This supports my claim in the oral discussion where I argued that land use changes provide a net surface warming, to which Dr. Mann disagreed.

Kalnay, E., and M. Cai, 2003: Impact of urbanization and land-use change on climate. *Nature*, **423**(6939):528-531.

Your testimony referred to your work with the Association of State Climatologists. Please explain how this group would propose to get a better sense of future changes from an

unprecedented rate of change in atmospheric composition in an objective and quantitative way if not by reliance on global climate models.

With all due respect, nothing I submitted nor anything I said in testimony referred to my 'work' with the Association of State Climatologists. I have never attended any of their meetings nor am I a member of the Association.

Do you support the analytical approach whereby the change over the 21st century is estimated by a linear extrapolation of global average temperatures over the last three decades? If so, what is your estimate of uncertainty associated with this approach?

The Climate Model Intercomparision Project (CMIP), which examined nearly twenty different climate models, demonstrates that, once human-induced warming starts, it takes place at a constant rate. If one argues for an anthropogenic signal, as some have, then the scientific answer is that, according to the consensus of our models, it must take place at a constant, not an increasing rate. There are several papers in the scientific literature, by Allen *et al.* (2000), Michaels *et al.* (2002), and Hansen *et al.* (2000) all making a version of this argument.

However, the CMIP models are forced with a 1%/year increase in carbon dioxide, which is clearly an overestimate for the last several decades. Energy Information Administration data place the recent increases at closer to 0.4%/year, and even accounting for all the other positive and negative forcings, the effective change is around 0.7%/year, including chlorofluorocarbons, which will continue to decline. Also, EIA data indicate that global per-capita carbon dioxide emissions peaked around 1985; the decline since then is statistically significant. Hansen *et al.* (2000) and Michaels *et al.* (2002) have both argued that emissions are at or below the lowest limit of the IPCC scenarios. There is no reason to expect this to change. Hence the mean warming of the models is likely to be an overestimate.

In summary, if one accepts a human warming signal, then the most likely change is at the rate already established. If this is not true, then the sum-total of the many billions of dollars of effort of climate modeling would be that we could not even estimate the functional form of future warming. In that case, there is simply no scientific guidance for any policy options.

Allen, M.R., P.A. Stott, J.F.B. Mitchell, R. Schnur, and T.L. Delworth, 2000: Quantifying the uncertainty in forecasts of anthropogenic climate change. *Nature*, **407**(6804):617-620.

Hansen, J., M. Sato, R. Ruedy, A. Lacis, and V. Oinas, 2000: Global warming in the twenty-first century: An alternative scenario. *Proceedings of the National Academy of Sciences of the United States of America*, **97**(18):9875-9880.

Michaels, P.J., P.C. Knappenberger, O.W. Frauenfeld, and R.E. Davis, 2002: Revised 21st century temperature projections. *Climate Research*, **23**(1):1-9.

How does this analytical approach account for possible changes in future emissions, in the ratio of greenhouse gas and aerosol influences, and for any cases of changes in emissions that might be enacted?

I believe this question has been answered by my response to the question above.

The grants that are described as supporting your analysis seem to have much more to do with the sun or unrelated pattern recognition than with climate history (Air Force Office of Scientific Research – Grant AF49620-02-1-0194; American Petroleum Institute – Grants 01-0000-4579 and 2002-100413; NASA – Grant NAG-7635; and NOAA-Grant NA96GP0448). Could you please describe how much funding you received and used in support of this study, all of the sources and the duration of that funding, and the relevance of those grant topics to the article?

Of the grants listed, I am only associated with NOAA Grant NA96GP0448, so I am only able to comment on its contribution to our paper. It was a grant for $50,000 given to the University of Delaware to support research on the detection of climate change signals in climate time-series. Co-PIs on the grant were Dr. Robert E. Davis of the University of Virginia and Dr. Scott M. Robeson of Indiana University. For this grant, we examined research by Drs. Benjamin Santer and Thomas Wigley that purported to demonstrate an increasing correspondence between model-derived surface air temperature fields and observations – with the conclusion that the increasing correspondence showed that the 'preponderance of the evidence suggests a discernable human influence on the climate' (as claimed and authored by Dr. Santer in the Second IPCC Scientific Assessment). We showed that the statistical methodology used by Drs. Santer and Wigley and their colleagues was inherently flawed, using a simple example where an increasing correlation over time was observed when model-estimates and observations, in fact, diverged. Through the grant, we proposed an alternative approach.

While the research on the grant was essentially finished by the time that I began to work with Dr. Soon on this endeavor, the insight I gained by examining the flawed methodology that Drs. Santer and Wigley employed on their time-series analysis proved beneficial to understanding the flaws inherent in the work by Dr. Mann and his colleagues. Both Drs. Santer and Mann employed an assessment of a time-series to determine trends in surface air temperature and both inappropriately applied simple statistical techniques. Given that the grant had helped me better understand time-series analyses of air temperature as they were being inappropriately applied to make erroneous claims in the IPCC reports, I felt it appropriate to cite my NOAA grant as providing assistance to this project. To state that the grant simply focused on 'unrelated pattern recognition' is extremely misleading.

Have you been hired by or employed by or received grants from organizations that 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 organizations.

My research activities regarding climate and global change have been solely funded by federal organizations such as the United States Department of the Interior, the National Aeronautics and Space Administration, the National Oceanic and Atmospheric Administration, and the National Science Foundation.

Please describe the peer review process that took place with respect to your paper published in Energy and Environment, including the number of reviewers and the general content of the reviewer's suggested edits, criticisms or improvements.

As the senior author usually handles the interface with the editor, and Dr. Soon was the direct contact on this manuscript, Dr. Soon would be better suited to provide appropriate answers to your comments. However, I would like to state that *Energy and Environment* uses a true peer-review system, where independent reviewers are sought and provide feedback on the quality of the manuscript. By contrast, and despite the empty insistence by Dr. Mann, *EOS, Transactions of the American Geophysical Union* is not a refereed journal. The American Geophysical Union lists its journals on its website – EOS is not among them. Instead, EOS is listed as a *newspaper*, not a refereed journal, and the *EOS Forum*, where Dr. Mann and colleagues published their reply, is presented as an outlet for opinions. For Dr. Mann to state, as he has in his written testimony, that his *EOS* piece is a peer-reviewed article since the editor is an atmospheric scientist is intentionally misleading and blatantly undermines what is meant by the peer-review concept.

Appendix 1: Figure 9 from the Technical Summary of the IPCC Third Assessment Report.

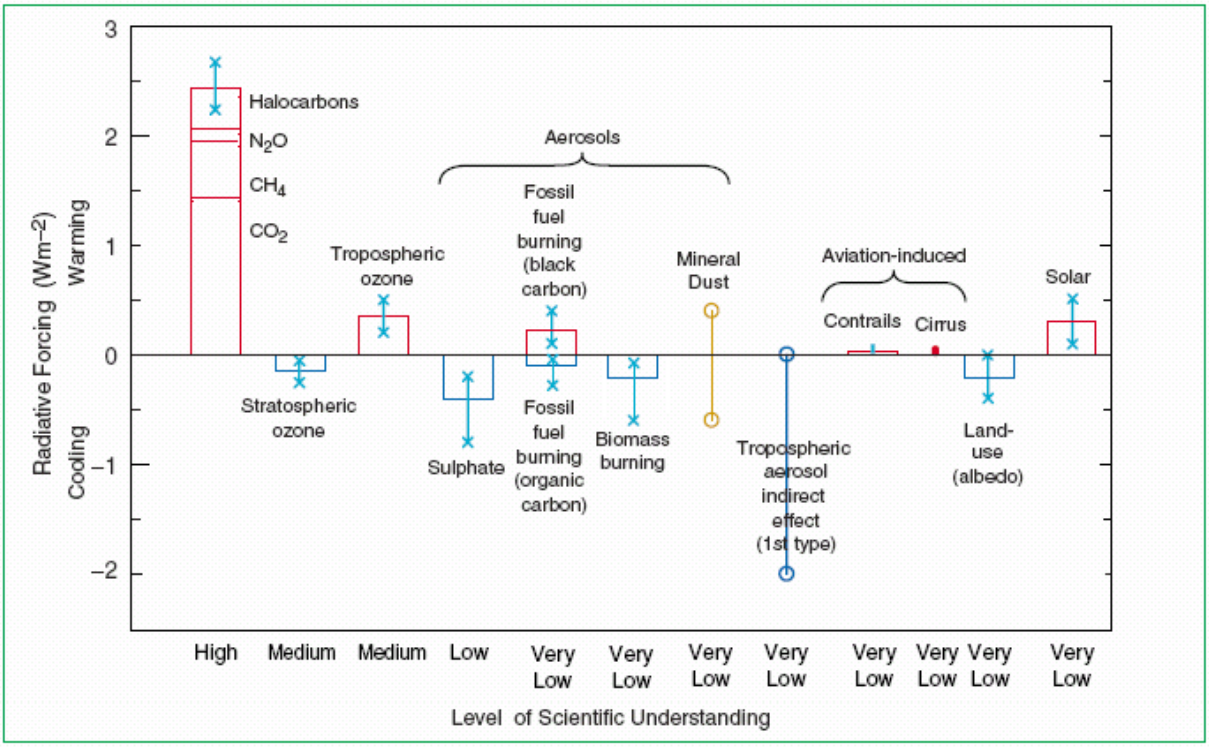

Figure 9: Global, annual-mean radiative forcings (Wm^{-2}) due to a number of agents for the period from pre-industrial (1750) to present (late 1990s; about 2000) (numerical values are also listed in Table 6.11 of Chapter 6). For detailed explanations, see Chapter 6.13. The height of the rectangular bar denotes a central or best estimate value, while its absence denotes no best estimate is possible. The vertical line about the rectangular bar with "x" delimiters indicates an estimate of the uncertainty range, for the most part guided by the spread in the published values of the forcing. A vertical line without a rectangular bar and with "o" delimiters denotes a forcing for which no central estimate can be given owing to large uncertainties. The uncertainty range specified here has no statistical basis and therefore differs from the use of the term elsewhere in this document. A "level of scientific understanding" index is accorded to each forcing, with high, medium, low and very low levels, respectively. This represents the subjective judgement about the reliability of the forcing estimate, involving factors such as the assumptions necessary to evaluate the forcing, the degree of knowledge of the physical/chemical mechanisms determining the forcing, and the uncertainties surrounding the quantitative estimate of the forcing (see Table 6.12). The well-mixed greenhouse gases are grouped together into a single rectangular bar with the individual mean contributions due to CO_2, CH_4, N_2O and halocarbons shown (see Tables 6.1 and 6.11). Fossil fuel burning is separated into the "black carbon" and "organic carbon" components with its separate best estimate and range. The sign of the effects due to mineral dust is itself an uncertainty. The indirect forcing due to tropospheric aerosols is poorly understood. The same is true for the forcing due to aviation via its effects on contrails and cirrus clouds. Only the "first" type of indirect effect due to aerosols as applicable in the context of liquid clouds is considered here. The "second" type of effect is conceptually important, but there exists very little confidence in the simulated quantitative estimates. The forcing associated with stratospheric aerosols from volcanic eruptions is highly variable over the period and is not considered for this plot (however, see Figure 6.8). All the forcings shown have distinct spatial and seasonal features (Figure 6.7) such that the global, annual means appearing on this plot do not yield a complete picture of the radiative perturbation. They are only intended to give, in a relative sense, a first-order perspective on a global, annual mean scale and cannot be readily employed to obtain the climate response to the total natural and/or anthropogenic forcings. As in the SAR, it is emphasised that the positive and negative global mean forcings cannot be added up and viewed *a priori* as providing offsets in terms of the complete global climate impact. [Based on Figure 6.6]