

Senator Jeffords's followup questions for Dr. Willie Soon

1. In testimony, you said that you did not know whether you submitted something for publication to Capitalism magazine. Here is the title and web address: "Global Warming Speculation vs. Science: Just Ask the Experts" by Sallie Baliunas & Willie Soon (Capitalism Magazine - August 22, 2002) <http://capmag.com/article.asp?ID=1816> Did you submit or approve submission of this article for publication?

With the benefit of your reminder, I hereby confirm that the above mentioned article in Capitalism Magazine was taken from the original article "Just Ask the Experts" by Baliunas and Soon originally published by the TechCentralStation.com at the link:

<http://www.techcentralstation.com/072302B.html>.

I did not submit the article to Capitalism Magazine.

2. In your testimony you indicated that your training is in "atmospherics." Could you please explain this term more fully, and indicate your formal training in paleoclimatic studies and analysis?

My PhD thesis¹ was on collisional-radiative properties of high-temperature, partially ionized nitrogen, oxygen, helium and hydrogen plasmas at conditions relevant to the Earth's atmosphere. This is why I mentioned that I had formal training in "atmospheric and space physics" in my oral remarks. If necessary, please consult my thesis advisor, Professor Joseph Kunc at kunc@usc.edu for further details about my educational background.

I would add that the quality of knowledge about climate science or any other subject of interest must be judged on its own merits, and does not and must not be determined by invoking the amount of formal schooling or consensus viewpoints adopted by particular interest groups.

My research interests and learning about paleoclimatology has been obtained mainly through the following individuals and sources:

- (1) Professor Eric Posmentier (Eric.S.Posmentier@Dartmouth.EDU), who is also my colleague.
- (2) Professor David Legates (legates@UDel.Edu), who is also my colleague.
- (3) Participation, both as a student and as lecturer, in numerous national and international workshops, conferences and summer schools including (a) the

¹ which was awarded the 1989 nation-wide IEEE Nuclear and Plasma Sciences Society Graduate Scholastic Award and the 1991's Rockwell Dennis Hunt Scholastic Award for "the most representative PhD thesis work" at the University of Southern California.

1993's NATO Advanced Research workshop on "Solar engine and its influence on terrestrial atmosphere and climate", (b) the 1994's NASA-NOAA Summer School on Processes of Global Change, (c) the 1996's (French) CNRS "Chaos et Fractales dans l'activite Solaire", (d) the 2000's "1st Solar and Space Weather Euroconference: The Solar Cycle and Terrestrial Climate," and other specialized meetings.

- (4) Many other scientists also have been helpful in my eager learning of the subject: the late Professor Jean Grove (Girton College, Cambridge University), Professor Jim Kennett (University of California Santa Barbara), Professor David J. A. Evans (University of Glasgow), Professor Lowell Stott (University of Southern California), Professor Hong-Chun Li (University of Southern California), Professor Reid Bryson (University of Wisconsin), Professor Henri Grissino-Mayer (University of Kentucky), Professor Emi Ito (University of Minnesota), Dr. ShaoPeng Huang (University of Michigan), Dr. Zhonghui Liu (Brown University), Dr. Ming Tang (Institute of Geology and Geophysics, Chinese Academy of Sciences), Dr. Yang Bao (Cold and Arid Regions Environmental and Engineering Research Institute, Chinese Academy of Sciences), and Professor Bin Wang (University of Hawaii).

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

The proxy-based temperature reconstructions for the Northern Hemisphere by Mann et al. (1998, *Nature*, vol. 392, 779-782) and Mann et al. (1999, *Geophysical Research Letters*, vol. 26, 759-762) extend from 1400-1980 and 1000-1980, respectively. So it is true that those proxy-based temperature series did not cover the 1981-2000 interval of the late 20th century.

Here is what close colleagues and co-authors (Bradley and Hughes) of Professor Mann admitted in their independent (i.e., without Prof. Mann as co-author) and updated publication, "A caveat to [our] conclusion [about northern hemisphere temperature change over the last 1000 years] is that *the 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* [emphasis added]. This is necessary because many paleo data sets were collected in the 1960s and 1970s, and have not been up-dated [NOTE: this statement by Bradley et al. (2003) referred primarily to the tree-ring data base from the International Tree-Ring Database.], so a direct proxy-based comparison of the 1990s with earlier periods is not yet possible." [p. 116 of Bradley et al., 2003, In: Alverson, K., R.S. Bradley and T.F. Pedersen (eds.) *Paleoclimate, Global Change and the Future*. Springer Verlag, Berlin, 105-149]

Agreeing with discussion on p. 260-261 of Soon et al. (2003), Bradley et al. (2003) cautioned that "in the case of tree rings from some areas in high latitudes, the

decadal time-scale climatic relationships prevalent for most of this century appear to have changed in recent decades, possibly because increasing aridity &/or snowcover changes at high latitudes may have already transferred the ecological responses of trees to climate (cf. Jacoby and D'Arrigo 1995; Briffa et al. 1998). For example, near the northern tree limit in Siberia, this changing relationship can be accounted for by a century-long trend to greater winter snowfall. This has led to delayed snowmelt and thawing of the active layer in this region of extensive permafrost, resulting in later onset of the growing season (Vaganov et al. 1999). It is not yet known how widely this explanation might apply to the other regions where partial decoupling has been observed, but regardless of the cause, it raises the question as to whether there might have been periods in the past when the tree ring-climate response changes, and what impact such changes might have on paleotemperature reconstructions based largely on tree ring data." (p. 116-117).

Bradley et al. (2003) also worried that "Paleoclimate research has had a strong northern hemisphere, extra-tropical focus (but even there the record is poorly known in many areas before the 17th century). There are very few high resolution paleoclimatic records from the tropics, or from the extra-tropical southern hemisphere, which leaves many questions (such as the nature of climate in Medieval times) unanswered." (p. 141). Bradley et al. continued "All large-scale paleotemperature reconstructions suffer from a lack of data at low latitudes. In fact, most "northern hemisphere" reconstructions do not include data from the southern half of the region (i.e. [missing comma] areas south of 30N). Furthermore, there are so few data sets from southern hemisphere that it is not yet possible to reconstruct a meaningful "global" record of temperature variability beyond the period of instrumental records. For the northern hemisphere records, it must be recognized that the errors estimated for the reconstructions of Mann et al. (1999) and Briffa et al. (2001) are minimum estimates, based on the statistical uncertainties inherent in the methods used. These can be reduced by the use of additional data (with better spatial representation) that incorporate stronger temperature signals. However, there will always be additional uncertainties that relate to issues such as the constancy of the proxy-climate function over time, and the extent to which modern climate modes (i.e., those that occurred during the calibration interval) represent the full range of climate variability in the past [i.e., similar unresolved research questions had been raised in p. 239-242 and p. 258-264 of Soon et al. 2003]. There is evidence that in recent decades some high latitude trees no longer capture low frequency variability as well as in earlier decades of the 20th century (as discussed below in Section 6.8) which leads to concerns over the extent to which this may have also been true in the more distant past. If this was a problem (and currently we are not certain of that) it could result in an inaccurate representation of low frequency temperature changes in the past. Similarly, if former climates

were characterized by modes of variability not seen in the calibration period, it is unlikely that the methods now in use would reconstruct those intervals accurately. It may be possible to constrain these uncertainties through a range of regional studies (for example, to examine modes of past variability) and by calibration over different time intervals, but not all uncertainty can be eliminated and so current margins of error must be considered as minimum estimates [meaning the actual range of error is larger than shown in Mann et al. 1999 or the IPCC TAR's charts]." (p. 114-115).

It is also very important to heed warnings and cautions from other serious researchers about not over stating the true confidence of a reconstructed climatic result based on indirect proxies. Esper et al. (2003, *Climate Dynamics*, vol. 21, 699-706) modestly apprised of the current situation in reconstructing long-term climatic information from tree rings: "Although these long-term trends agree well with ECS [i.e., Esper, Cook, Schweingruber in 2002, *Science*, vol. 295, 2250-2253], the amplitude of the multi-centennial scale variations is, however, *not* understood. This is because (1) no single multi-centennial scale chronology could be built that is not systematically biased in the low frequency domain, and (2) no evidence exists that would support an estimation of the biases either in the LTM [Long-term mean standardization] nor in the RCS [Regional curve standardization] multi-centennial chronologies. Consequently, we also avoided providing formal climate calibration and verification statistics of the chronologies. Note also that the climate signal of the chronologies' low frequency component could not be statistically verified anyway. This is because the high autocorrelations, when comparing lower frequency trends, significantly reduce the degrees of freedom valid for correlation analyses. We believe that a formal calibration/verification/transfer function approach would leave the impression that the long-term climate history for the Tien Shan [i.e., the location of Esper and five colleagues' study] is entirely understood, which is not the case. Further research is needed to estimate the amplitude of temperature variation in the Alai Range [south of Kirghizia] over the last millennium." (p. 705)

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

In Soon et al. (2003, *Energy & Environment*, vol. 14, 233-296), I and my colleagues cautioned that the regional temperature patterns resulted from Mann and colleagues' methodology are too severely restricted by the calibration over the limited interval of instrumental records of no more than 100 years. In particular, we are concerned that the regional (and hence larger spatial-scale averages) variability of temperature on multidecadal and centennial time scales deduced from such a method will be underestimated.

Recently, the methodology of Mann et al. (1998) has been seriously challenged by McIntyre and McKittrick (2003, *Energy & Environment*, vol. 14, 751-771) in that “poor data handling, obsolete data and incorrect calculation of principal components” were shown as the errors and defects of Mann et al.’s paper. The exchange between Mann and colleagues and McIntyre and McKittrick is ongoing, but the use of obsolete data is a clear case of misrepresentation of regional basis of change in Mann et al.’s work. Further problems in Mann et al. (1998) are outlined under question # 13 below. Additional documentation (including responses by Prof. Mann and his colleagues) and updates can be found in <http://www.uoguelph.ca/~rmckitri/research/trc.html>.

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

I have not seen any global temperature curves presented in the two earlier studies by Mann et al. (1998 and 1999). But please consider the deep concerns about the lack of proxy data especially over the tropics (30N to 30S) and the southern hemisphere raised by Soon et al. (2003) and even in the *independent* paper by Professor Mann’s close colleagues and co-authors (Bradley and Hughes), i.e., in Bradley et al. (2003), discussed under question # 3 above.

“Global” temperature estimates, based on indirect climate proxies, from 200-1980 were shown in Mann and Jones (2003, *Geophysical Research Letters*, vol. 30 (15), 1820) as Figure 2c. But I am unsure if the temperature series presented by Mann and Jones (2003) could adequately represent the variability over the whole globe since it was openly admitted that the proxies used covered only 8 “distinct regions” in the Northern Hemisphere and 5 for the Southern Hemisphere (see the coverage of proxies shown in Figure 1 of Mann and Jones, 2003).

More importantly, Soon et al. (2004, *Geophysical Research Letters*, vol. 31, L03209) showed that the 40-year smoothed instrumental temperature trend for the Northern Hemisphere shown as Figure 2a of Mann and Jones (2003) has a physically implausible high value at year 2000 (see more discussion in question # 6 below). We caution that the extremely rapid rate of warming trend of 1 to 2.5°C per decade implied by the published results by Mann and his colleagues over the last one to two years [comparing Mann and Jones (2003) with both Mann (2002, *Science*, vol. 297, 1481-1482) and Mann et al. (2003, *Eos*, 84(27), 256-257)], is most likely due to the artifacts of methodology and their procedure of trend smoothing. I am submitting the pdf file (SLB-GRL04-NHtempTrend.pdf) of Soon et al. (2004) for the record of the committee.

6. 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?

I am not sure about the meaning of this question. But when contrasted with borehole-based reconstruction, the Northern Hemisphere terrestrial temperatures produced by Mann et al. (1998, 1999) over the last 500 years may have been too warm by about 0.4°C during the 17th-18th century (see Huang et al. 2000, Nature, vol. 403, 756-758). Recent attempts by Mann et al. (2003, Journal of Geophysical Research, vol. 108. (D7), 4203) and Mann and Schmidt (2003, Geophysical Research Letters, vol. 30 (12), 1607) to rejustify and defend the Mann et al. (1998, 1999) results have been shown to be either flawed or invalid by Chapman et al. (2004, Geophysical Research Letters, vol. 31, L07205) and by Pollack and Smerdon (2003, Geophysical Research Abstract of EGS, vol. 6, 06345). The eventual fact will no doubt emerge with increased understanding, but Chapman et al. (2004) warned that "A second misleading analysis made by Mann and Schmidt [2003] concerns use of end-points in reaching a numerical conclusion. ... It is based on using end points in computing changes in an oscillating time series, and is just bad science."

With regard to instrumental thermometer data of the past 100-150 years, it is important to note that Soon et al. (2004) has recently shown that the 40-year smoothed Northern Hemisphere temperature trend shown in Mann and Jones (2003) has a physically implausible high value at the year 2000 endpoint especially when studied in context with previous published results by Mann et al. (2003, Eos, vol. 84 (27), 256-257) and Mann (2002, Science, vol. 297, 1481-1482). This important updated information, admittedly with the benefit of hindsight, together with the works by Chapman et al. (2004) and McIntyre and McKittrick (2003), showed clearly that the Northern Hemisphere temperature trends, either proxy-based or instrumental, derived by Mann et al. (1998, 1999) and Mann and Jones (2003) are not reliable.

7. 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?

Again, I am not sure if there are sufficient proxy data that would allow a meaningful quantitative estimate of annual mean global temperatures back six to nine thousand years. But in a new paper for the Quaternary Science Reviews, Darrell Kaufman and 29 co-authors (2004, Quaternary Science Reviews, vol. 23, 529-560) found that indeed there are clear evidence for warmer than present conditions during the Holocene at 120 out of 140 sites they compiled across the

Western Hemisphere of the Arctic. Kaufman et al. (2004) estimated that, at the 16 terrestrial sites where quantitative data are available, the local Holocene Thermal Maximum summer temperatures were about $1.6 \pm 0.8^\circ\text{C}$ higher than the average of the 20th century. The coarse temperature map sketched on the NOAA's Paleoclimatology web site:

<http://www.ngdc.noaa.gov/paleo/globalwarming/images/polarbigb.gif> suggests that the summer temperatures 6000 years ago may have been 2 to 4°C warmer than present in the other sector (Eastern Hemisphere) of the Arctic.

8. 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?

The theoretical requirement is fairly simple:

(a) find local and regional proxies that are sensitive to variations of temperature on timescales of decade, several decades and century;

(b) have sufficient spatial coverage of these local and regional proxies.

Then one would be able to compare the last 50 years of the 1000-year record with the previous 950 years.

Soon et al. (2003) had indeed initiated an independent effort in this direction and concluded that a truly global or hemispheric averaged temperature record for the past 1000 years is not yet forthcoming because of the large and disparate range of the indirect local and regional proxies to temperature such that a robust ability of different proxies in capturing all the necessary scales of variability cannot yet be confirmed. The main problem I foresee in having any definitive answers for now is related to the fact that the statistical association of each proxy to climatic variables like temperature can itself be variable and changing depending on the location and time interval. But I am not sure if the sole focus on temperature as the measure of "climate" is sensible if not unnecessarily narrow.

In Soon et al. (2003), we consider climate to be more than just temperature so we did not narrowly restrict ourselves to only temperature-sensitive proxies. For example, in addition to temperature, we are equally concerned about expansion and reduction of forested and desert-prone areas, tree-line growth limit, sea ice changes, balances of ice accumulation and ablation in mountain glaciers and so on. When studying the ice balance for a glacier, it is important to insist that although glaciers are very important indicators of climate change over a rather long time-scale, they are not simply thermometers as often confused by heated discussion pointing to evidence for global warming by carbon dioxide (see additional discussion on factors, especially atmospheric carbon dioxide, in determining Earth's climate and its change under questions # 19, 20, 25, 30 and 35 below). Examples include statements by Will Steffen, director of the

International Geosphere-Biosphere Program, "Tropical glaciers are a bellweather of human influence on the Earth system" (quoted in the article "The melting snows of Kilimanjaro" by Irion, 2001, *Science*, vol. 291, 1690-1691) or by Professor Lonnie Thompson, Ohio State University, "We have long predicted that the first signs of changes caused by global warming would appear at the few fragile, high-altitude ice caps and glaciers within the tropics ... [t]hese findings confirm those predictions. We need to take the first steps to reduce carbon dioxide emissions. We are currently doing nothing. In fact, as a result of energy crisis in California - and probably in the rest of the country by this summer - we will be investing even more in fuel-burning power plants. That will put more power in the grid but, at the same times it will add carbon dioxide to the atmosphere, amplifying the problem" (quoted in Ohio State University's press release, <http://www.acs.ohio-state.edu/units/research/archive/glacgone.htm>).

A clarification about the physical understanding of modern glacier retreats and climate change, especially those on Kilimanjaro, is necessary and has been forthcoming with important research progress. First, Molg et al. (2003, *Journal of Geophysical Research*, vol. 108 (D23), 4731) recently concluded that their study "highlights that modern glacier retreat on Kilimanjaro is much more complex than simply attributable to 'global warming only', a finding that conforms with the general character of glacier retreat in the global tropic [Kaser, 1999]: a process driven by a complex combination of changes in several different climatic parameters ... with humidity-related variables dominating this combination." In another new paper for the *International Journal of Climatology*, Kaser et al. (2004, *International Journal of Climatology*, "Modern glacier retreat on Kilimanjaro as evidence of climate change: Observations and facts", vol. 24, 329-339; available from <http://geowww.uibk.ac.at/glacio/LITERATUR/index.html>) provided clear answers that neither added longwave radiation from a direct addition of atmospheric CO₂ nor atmospheric temperature were the key variables for the observed changes, as revealed in this long but highly informative passage:

"Since the scientific exploration of Kilimanjaro began in 1887, when Hans Meyer first ascended the mountain (not to the top at this time, but to the crater rim), a central theme of published research has been the drastic recession of Kilimanjaro's glaciers (e.g., Meyer, 1891, 1900; Klute, 1920; Gilman, 1923; Jager, 1931; Geilinger, 1936; Hunt, 1947; Spink, 1949; Humphries, 1959; Downie and Wilkinson, 1972; Hastenrath, 1984; Osmaston, 1989; Hastenrath and Greischar, 1997). Early reports describe the formation of notches, splitting up and disconnection of ice bodies, and measurements of glacier snout retreat on single glaciers, while later books and papers advance to reconstructing glacier surface areas. ... Today, as in the past, Kilimanjaro's glaciers are markedly characterized

by features such as penitentes, cliffs (Figure 3a/b) [not reproduced here], and sharp edges, all resulting from strong differential ablation. These features illustrate the *absolute predominance* [emphasis added] of incoming shortwave radiation and latent heat flux in providing the energy for ablation (Kraus, 1972). A positive heat flux from either longwave radiation or sensible heat flux, if available, would round-off and destroy the observed features within a very short time ranging from hours to days. On the other hand, if destroyed, the features could only be sculptured again under very particular circumstances and over a long time. Thus, the existence of these features indicates that the present summit glaciers are not experiencing ablation due to sensible heat (i.e., from positive air temperature). Additional support for this is provided by the Northern Icefield air temperature recorded from February 2000 to July 2002, which never exceeded -1.6°C , and by the presence of permafrost at 4,700 m below Arrow Glacier on the western slope ... ”

Kaser et al. (2004) continue with this “synopsis of interpretations and facts”:

“A synopsis of (i) proxy data indicating changes in East African climate since ca. 1850, (ii) 20th century instrumental data (temperature and precipitation), and (iii) the observations and interpretations made during two periods of fieldwork (June 2001 and July 2002) strongly support the following scenario. Retreat from a maximum extent of Kilimanjaro’s glaciers started shortly before Hans Meyer and Ludwig Purtscheller visited the summit for the first time in 1889, caused by an abrupt climate change to markedly drier conditions around 1880. Intensified dry seasons accelerated ablation on the respectively illuminated vertical walls left in the hole on top by Reusch Crater as a result of *volcanic activity* [emphasis added]. The development of vertical features may also have started on the outer margins of the plateau glaciers before 1900, primarily as the formation of notches, as explicitly reported following field research in 1898 and 1912 (Meyer, 1900; Klute, 1920). A current example of such a notch development is the hole in the Northern Icefield (see Figure 2). Once started, the lateral retreat was unstoppable, maintained by solar radiation despite less negative mass balance conditions on horizontal glacier surfaces, and will come to an end only when the glaciers on the summit plateau have disappeared. This is most probable within next decades, if the trend revealed in Figure 1 continues. Positive air temperatures have not contributed to the recession process on the summit so far. The rather independent slope glaciers have retreated far above the elevation of their thermal readiness, responding to dry conditions. If present precipitation regime persists, these glaciers will most probably survive in positions and extents not much different

from today. This is supported by the area determinations in Thompson's et al. (2002) map, which indicate that slope glaciers retreated more from 1912 to 1952 than since then. From a hydrological point of view, melt water from Kibo's glaciers has been of little importance to the lowland in modern times. Most glacier ablation is due to sublimation, and where ice does melt it immediately evaporates into the atmosphere. Absolutely no signs of runoff can be found on the summit plateau, and only very small rivers discharge from the slope glaciers. Rainfall reaches a maximum amount at about 2,500 m a.s.l. [above sea level] (Coutts, 1969), which primarily feeds the springs at low elevation on the mountain; one estimate attributes 95% of such water to a forest origin (Lambrechts et al., 2002). The scenario presented offers a concept that implies climatological processes *other than increased air temperature* [emphasis added] govern glacier retreat on Kilimanjaro in a direct manner. However, it does not rule out that these processes may be linked to temperature variations in other tropical regions, e.g., in the Indian Ocean (Latif et al., 1999; Black et al., 2003)."

Lindzen (2002, Geophysical Research Letter, vol. 29, paper 2001GL014360) further added that "Recent papers show that deep ocean temperatures have increased somewhat since 1950, and that the increase is compatible with predictions from coupled GCMs [General Circulation Models]. The inference presented is that this degree of compatibility constitutes a significant test of the models. ... [But] it would appear from the present simple model (which is similar to what the IPCC uses to evaluate scenarios) that the ocean temperature change largely reflects only the fact that surface temperature change is made to correspond to observations, and says almost nothing about model climate sensitivity. ... It must be added that we are dealing with observed surface warming that has been going on for over a century. The oceanic temperature change [at depth of 475 m or so] over the period reflects earlier temperature change at the surface. How early depends on the rate at which surface signals penetrate the ocean." In other words, the recently noted warming of the deeper ocean is not a proof of global surface and atmospheric warming by increasing CO₂ in the air because the parameters of climate sensitivity and rate of ocean heat uptake are not sufficiently well quantified. In addition, if the earlier oceanic surface temperature warming mentioned by Lindzen were indeed initiated and occurred substantially long ago, then there would be no association of that change to man-made CO₂ forcing.

9. The IPCC has found that the late 20th century is the warmest period in the last 1000 years, for average temperature in the northern hemisphere. Does your paper provide a quantitative analysis of average temperatures for the

northern hemisphere for this specific time period - that is, for the later half of the 20th century?

It should be understood that (1) the conclusion of the IPCC Working Group I's Third Assessment Report (2001; TAR), (2) the evidence shown in Figure 1b of the Summary for Policymaker, (3) Figure 5 of the Technical Summary, and (4) Figure 2.20 in Chapter 2 of TAR were all derived directly from the conclusion in Mann et al. (1999) and Figure 3a of Mann et al. (1999). Therefore all comments and criticisms presented in this Q&A about Mann et al. (1999) apply to the IPCC TAR's conclusion. In addition, Soon et al. (2004) recently cautioned that the 40-year smoothed northern hemisphere temperature trend shown in Figure 2.21 of TAR (2001) cannot be replicated according to the methodology described in the caption of Figure 2.21. The failure in replication introduces a significant worry about the actual quality of scientific efforts behind the production of Figure 2.21 in TAR (2001).

The answer to the second part of your direct question is no. Here are the related reasons why a confident estimate of the averaged northern hemisphere temperature for the full 1000 years (including the full 20th century) is not yet possible, despite what had been claimed by Mann et al. (1999). First, several authors, including those detailed in section 5.1 of Soon et al. (2003) and those pointed out in question # 6, had shown that the 1000-year series of mathematical temperature derived by Mann et al. (1999) has significantly underestimated the multidecadal and centennial scale changes. Second, the focus of Soon et al. (2003) is to derive understanding of climatic change on local and regional spatial scales, instead of over the whole northern hemisphere per se, because those are the most relevant measures, in practical sense, of change. In addition, we provided the first-order attempt to collect all available climate proxies relevant for local and regional climatic changes, but not restricted to temperature alone. But more pertinent to your question is the fact discussed in Soon et al. (2003) that different proxies respond with differing sensitivities to different climatic variables, seasons, plus spatial and temporal scales, so that a convenient derivation of a self-consistent northern hemisphere averaged annual mean temperature for the full 1000 years, desirable as the result may be, is not yet possible.

10. Does your paper provide any quantitative analysis of temperature records specifically for the last 50 years of the 20th century?

Soon et al. (2003) considered all available proxy records with no particular prejudice. If the individual proxy record covers up to the last 50 years of the 20th century, then quantitative comparisons are performed, mostly according to the statements from the original authors. Please consider some of the detailed

quantitative discussion in section 4 of Soon et al. (2003) and the qualitative results compiled in Table 1 of that paper.

11. In an article in the Atlanta Journal Constitution (June 1, 2003), you were quoted as acknowledging during a question period at a previous Senate luncheon that your research does not provide a comprehensive picture of the Earth's temperature record and that you questioned whether that is even possible, and that you did not, "... see how Mann and the others could 'calibrate' the various proxy records for comparison." How then does your analysis provide a comprehensive picture of Earth's temperature record or have any bearing on the finding by the IPCC, that the late 20th century is the warmest in the last 1000 years?

Thank you for referencing the article. I must first state on the record that contrary to the claim in this Atlanta Journal Constitution (June 1, 2003) article <http://www.ajc.com/business/content/business/0603/01warming.html>, the writer, never, as claimed, conducted a telephone interview with me. No such conversation took place and I am rather shocked by this false claim. This fact has gone uncorrected until now.

The strengths and weaknesses of my research works are fully discussed in Soon et al. (2003). The paper documented detailed local and regional changes in several climatic variables to try to obtain a broader understanding of climate variability. We concluded that "Because the nature of the various proxy climate indicators are so different, the results cannot be combined into a simple hemispheric or global quantitative composite. However, considered as an ensemble of individual observations, an assemblage of the local representations of climate establishes the reality of both the Little Ice Age and the Medieval Warm Period as climatic anomalies with world-wide imprints, extending earlier results by Bryson et al. (1963), Lamb (1965), and numerous other research efforts. Furthermore, these individual proxies are used to determine whether the 20th century is the warmest century of the 2nd Millennium at a variety of globally dispersed locations. Many records reveal that the 20th century is likely *not* the warmest nor a uniquely extreme climatic period of the last millennium, although it is clear that human activity has significantly impacted some local environments."

The question on the difficult problem of calibrating proxies of differing types and sensitivities to climatic variables is discussed in Soon et al. (2003) and some criticisms on the weaknesses of the reconstruction by Mann et al. (1999) or the related IPCC TAR's conclusion are listed especially under questions # 6 and 9.

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

True progress in the field of paleoclimatology will certainly involve a better and more robust means of interpreting and quantifying the variations and changes seen in each high-resolution proxy record. The issue is not merely a problem awaiting solution through appropriate statistical methods like the EOF methodology adopted by Mann et al. (1998, 1999). On pp. 241-242 of Soon et al. (2003), we briefly outlined our straight-forward approach and contrasted it to the one used by Mann and colleagues that does not necessarily lead to results with physical meaning and reality.

13. In determining whether the temperature of the “Medieval Warm Period” was warmer than the 20th century, does your study analyze whether a 50-year period is either warmer or wetter or drier than the 20th century? If so, 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.

The detailed discussion behind our usage of the term “Medieval Warm Period” or “Little Ice Age” was described in Soon et al. (2003). We are mindful that the two terms should definitely include physical criteria and evidence from the thermal field. But we emphasize that great bias would result if those thermal anomalies were dissociated from hydrological, cryospheric, chemical, and biological factors of change. So indeed our description of a Medieval Climatic Anomaly (see a similar sentiment later reported by Bradley et al. 2003, Science, vol. 302, 404-405) in Soon et al. (2003) includes a warmer time that contains both drought or flooding conditions depending on the locations.

With regard to the last part of your question, I would answer by detailing only one example — Mann et al (1998). This influential study used both direct precipitation measurements and precipitation proxies as temperature indicators. This study was indeed applied by the IPCC TAR (2001). These include historical precipitation measurements in 11 grid cells, two coral proxies (reported in Mann et al. [1998] as precipitation proxies; see http://www.ngdc.noaa.gov/paleo/ei/data_supp.html for this and following references), two ice core proxies, 3 reconstructions of spring precipitation in southeast United States by Stahle and Cleaveland from tree ring data, 12 principal component series for tree rings in southwestern United States and Mexico reported as precipitation proxies by Stahle and Cleaveland (and Mann et al. 1998) and one tree ring series in Java - making a total of 31 precipitation series used as proxies in temperature reconstruction by Mann et al. (1998). In this peer-

reviewed article, for the precipitation data in a grid cell in New England, the researchers apparently used historical data from Paris, France (please see Figure 2 of McIntyre and McKittrick, 2003 and their discussion on pp. 758-759). For a grid cell near Washington DC, the researchers used historical data from Toulouse, France. For a grid cell in Spain, the researchers used precipitation data from Marseilles, France. Of the 11 precipitation series used in Mann et al. (1998), only one series (Madras, India) is correctly located. The precipitation data used by these authors cannot be identified in the source cited in paper Mann et al. (1998). While precipitation data and precipitation-related proxies can be instructive in providing information on past distribution of moisture and circulation patterns (and thus temperature), it is important to correctly identify the series used and important not to use data from the wrong continent for historical reconstructions.

14. 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?

The question raised here about the connection of any two 50-year periods in any two regions to be related from climatic point of view is both important and interesting. But the answer will be strongly dependent on the nature of forcings and feedbacks involved. If longer-term cryospheric or oceanic processes are involved then the answer would be yes.

15. 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?

The meaning of this question is not entirely clear to me. But I would say yes under the context of what is being said.

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

The results and conclusion of Soon et al. (2003) are best judged by the paper itself. Quantitative assessments of local and regional changes through the climatic proxies are discussed in section 4 of that paper as well as some qualitative picture described in Figures 1, 2 and 3 of that paper. Again, Soon et al. (2003) did not tried to distill all the collected proxies down to produce a strict temperature-only result since we are interested in a broader understanding of climate variability. Part of the answers given under questions # 9 and 11 can help elaborate what was done by Soon et al. (2003). I would also like to direct your attention to the two warnings listed under question # 3 by Bradley et al. (2003)

and Esper et al. (2003) concerning any undue, over confidence in promoting quantitative certainties in the reconstruction of past temperatures through highly imprecise black boxes of indirect proxies.

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

Specific authors and scientists that provided help in our work were listed in the acknowledgement section (p. 272) of Soon et al. (2003). We have also received generous help and comments from several scientists who are certainly highly qualified in terms of paleoclimatic studies. But the ultimate quality and soundness of our research shall always be our own responsibility.

In the September 5, 2003 Chronicle for Higher Education article (by Richard Monastersky), there were indeed two very serious accusations that suggested that Soon et al. (2003) had misrepresented or abused the conclusions by two original authors whose work we had cited. Our corrections and explanations to these unfortunately false claims can be studied from the documentation listed in the URL <http://cfa-www.harvard.edu/~wsoon/ChronicleHigherEducation03-d> (read especially Sep12-lettoCHE3.doc and Sep12-lettoCHE4.doc).

18. What was 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 concentration were at 500 ppm, which will occur around 2060 or so?

Co-answer to this question is listed under question # 19 below.

19. Please describe any known geologic precedent for large increases of atmospheric CO₂ without simultaneous changes in other components of the carbon cycle and the climate system.

My July 29, 2003 testimony was about the climate history of the past 1000 years detailed in Soon et al. (2003) rather than any potential (causal or otherwise) relationship between atmospheric carbon dioxide and climate. The fact remains that the inner working of the global carbon cycle and the course of future energy use are not sufficiently understood or known to warrant any confident prediction of atmospheric CO₂ concentration at year 2060. Please consider co-answer to this question under question # 25 below.

However, it is abundantly obvious that atmospheric CO₂ is not necessarily an important driver of climate change. It is indeed a puzzle that despite the relative low level of atmospheric CO₂ of no more than 300 ppm in the past 320-420 thousand years (Kawamura et al., 2003, *Tellus*, vol. 55B, 126-137) compared to the high levels of 330-370 ppm since the 1960s there is the clear suggestion of significantly warmer temperatures at both Vostok and Dome Fuji, East Antarctica, during the interglacials at stage 9.3 (about 330 thousand years before present; warmer by about 6°C) and stage 5.5 (about 135 thousand years before present; warmer by about 4.5°C) than the most recent 1000 years (see Watanabe et al., 2003, *Nature*, vol. 422, 509-512; further detailed discussion on environmental changes in Antarctica over the past 1000 years or so, including the most recent 50 years can be found in section 4.3.4 or pp. 256-257 of Soon et al. 2003).

But there are important concerns about the retrieval of information on atmospheric CO₂ levels from ice cores. Jaworowski and colleagues (1992, *The Science of the Total Environment*, vol. 114, 227-284) explained that:

“Ice is not a rigid material suitable for preserving the original chemical and isotopic composition of atmospheric gas inclusion. Carbon dioxide in ice is trapped mechanically and by dissolution in liquid water. A host of physico-chemical processes redistribute CO₂ and other air gases between gaseous, liquid and solid phases, in the ice sheets in situ, and during drilling, transport and storage of the ice cores. This leads to changes in the isotopic and molecular composition of trapped air. The presence of liquid water in ice at low temperatures [“even below - 70°C”] is probably the most important factor in the physico-chemical changes. The permeable ice sheet with its capillary liquid network acts as a giant sieve which redistributes elements, isotopes and micro-particles. Carbon dioxide in glaciers is contained: (1) in interstitial air in firn; (2) in air bubbles in ice; (3) in clathrates; (4) as a solid solution in ice crystals; (5) dissolved in intercrystalline veins and films of liquid brine; and (6) in dissolved and particulate carbonates. Most of the CO₂ is contained in ice crystals and liquids, and less in air bubbles. In the ice cores it is also present in the secondary gas cavities, cracks, and in the traces of drilling fluids.

The concentration of CO₂ in air recovered from the whole ice is usually much higher than that in atmospheric air. This is due to the higher solubility of this gas in cold water, which is 73.5- and 35-times higher than that of nitrogen and oxygen, respectively. The composition of other atmospheric gases (N₂, O₂, Ar) is also different in ice and in air inclusions than in the atmosphere. Argon-39 and ⁸⁵Kr data indicate that

36-100% of air recovered from deep Antarctic ice cores is contaminated by recent atmospheric air during field and laboratory processing. Until about 1985, CO₂ concentrations in gas recovered from primary air bubbles and from secondary gas cavities in pre-industrial and ancient ice were often reported to be much higher than in the present atmosphere. After 1985, only concentrations below the current atmospheric level were published. Our conclusion is that both these high and low CO₂ values do not represent real atmospheric content of CO₂.

Recently reported concentrations of CO₂ in primary and secondary gas inclusions from deep cores, covering about the last 160,000 years, are much below the current atmospheric level, although several times during this period the surface temperature was 2-4.5°C higher than now. If these low concentrations of CO₂ represented real atmospheric levels, this would mean (1) that CO₂ had not influenced past climatic changes, and (2) that climatic changes did not influence atmospheric CO₂ levels." (p. 272-273)

Additional historical evidence reveals natural occurrences of large, abrupt climatic changes that are not uncommon and they occurred without any known causal ties to large radiative forcing change. Phase differences between atmospheric CO₂ and proxy temperature in historical records are often not fully resolved; but atmospheric CO₂ has shown the tendency to follow rather than lead temperature and biosphere changes (see e.g., Dettinger and Ghil, 1998, *Tellus*, vol. 50B, 1-24; Fischer et al., 1999, *Science*, vol. 283, 1712-1714; Indermuhle et al., 1999, *Nature*, vol. 398, 121-126).

In addition, there have been geological times of global cooling with rising CO₂ (during the middle Miocene about 12.5-14 million years before present [Myr BP], for example, with a rapid expansion of the East Antarctic Ice Sheet and with a reduction in chemical weathering rates), while there have been times of global warming with low levels of atmospheric CO₂ (such as during the Miocene Climate Optimum about 14.5-17 Myr BP as noted by Panagi et al., 1999, *Paleoclimatology*, vol. 14, 273-292). A new study of atmospheric carbon dioxide over the last 500 million years (Rothman, 2002, *Proceedings of the (US) National Academy of Sciences*, vol. 99, 4167-4171) concluded that, "CO₂ levels have mostly decreased for the last 175 Myr. Prior to that point [CO₂ levels] appear to have fluctuated from about two to four times modern levels with a dominant period of about 100 Myr. ... The resulting signal exhibits no systematic correspondence with geologic record of climatic variations at tectonic time scales."

20. 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. Amman, W. M. Washington, J. S. Boyle, and W. Bruggemann 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?

It should first be noted that Pielke and Chase (2004, Science, vol. 303, 1771b; and see p. 1771c by Santer et al. and additional counter-reply by Pielke and Chase, with input from John Christy and Anthony Reale, available as paper 278b at <http://blue.atmos.colostate.edu/publications/reviewedpublications.shtml>) had criticized and challenged Santer et al.'s claim and conclusion that "[o]ur results are relevant to the issue of whether the 'real-world' troposphere has warmed during the satellite era. ... The direct evidence is that in the ALL experiment [i.e., climate model results that included changes in well-mixed greenhouse gases, direct scattering effects of sulfate aerosols, tropospheric and stratospheric ozone, solar total irradiance and volcanic aerosols; see more discussion below], the troposphere warms by 0.07°C/decade over 1979-1999. This warming is predominantly due to increases in well-mixed greenhouse gases. ... Over 1979-1999, roughly 30% of the increase in tropopause height in ALL is explained by greenhouse gas-induced warming of the troposphere. Anthropogenically driven tropospheric warming is therefore an important factor in explaining modeled changes in tropopause height."

In contrast, Pielke and Chase (2004) offered the observed evidence and concluded that "[g]lobally-averaged tropospheric temperature trends are statistically indistinguishable from zero. Thus, the elevation of the globally averaged tropopause report in [Santer et al., 2003] cannot be attributed to any detectable tropospheric warming over this period." In addition, "the climate system is much more complex than defined by tropospheric temperature and tropopause changes. Linear trend analysis [in Santer et al., 2003] is of limited significance. Changes in global heat storage provide a more appropriate metric to monitor global warming than temperature alone."

Soon and Baliunas (2003, Progress in Physical Geography, vol. 27, 448-455) had also previously outlined the incorrect fingerprint of CO₂ forcing observed in even the best and sophisticated version of climate models thus far. A more general and comprehensive discussion about the fundamental difficulties on modeling the effects of carbon dioxide using current generation of climate models is given in Soon et al. (2001, Climate Research, vol. 18, 259-271). Thus, the new paper by Santer et al. (2003) does not supercede or overcome the difficulties with respect to General Circulation Climate Models raised in Soon and Baliunas (2003).

Both the meaning and strength of the model-dependent results shown in Santer et al. (2003) remain doubtful and weak for several additional reasons.

First, Figure 2 of Santer et al. (2003) itself confirmed that the modeled changes in tropopause height are caused mainly by large stratospheric cooling related to changes in stratospheric ozone (they admitted so even though their note # 35 indicates that their numerical experiments did not separate tropospheric and stratospheric ozone changes) rather than by the well-mixed greenhouse gases that are supposed to be the subject of concern. Second, the model experiments of Santer et al. (2003) did not include changes in stratospheric water vapor which is known to be a significant factor for the observed stratospheric cooling (see e.g., Forster and Shine, 1999, *Geophysical Research Letters*, vol. 26, 3309-3312). Third, the failure to account for stratospheric water vapor contradicted the documented significant increases of stratospheric water vapor in the past half-century from a variety of instrumentations (e.g., Smith et al, 2000, *Geophysical Research Letters*, vol. 27, 1687-1690; Rosenlof et al., 2001, *Geophysical Research Letters*, vol. 28, 1195-1198; though Randel et al. [2004, *Journal of the Atmospheric Sciences*, submitted] recently noted that unusually low water vapor has been observed in the lower stratosphere for 2001-2003). Fourth, the model experiments by Santer et al. (2003) had clearly neglected (see note # 18 of that paper) the role of the Sun's ultraviolet radiation that is not only known to be variable (e.g., Fontenla et al. 1999, *The Astrophysical Journal*, vol. 518, 480-499; White et al., 2000, *Space Science Reviews*, vol. 94, 67-74) but also known to exert important influence on both the chemistry and thermal properties in the stratosphere and troposphere (e.g., Larkin et al., 2000, *Space Science Reviews*, vol. 94, 199-214).

Finally, the physical representation of aerosol forcing (which should not be restricted to sulfate alone) in Santer et al. (2003) is clearly not comprehensive and at best highly selective. Early on, Russell et al. (2000, *Journal of Geophysical Research*, vol. 105, 14891-14898) cautioned that “[o]ne danger of adding aerosols of unknown strength and location is that they can be tuned to give more accurate comparisons with current observations but cover up model deficiencies.” Anderson et al. (2003, *Science*, vol. 300, 1103-1104 and see also exchanges in Crutzen et al., 2003, vol. 303, 1679-1681) recently cautioned that:

“we argue that the magnitude and uncertainty of aerosol forcing may affect the magnitude and uncertainty of total forcing [i.e., “the global mean sum of all industrial-era forcings”] to a degree that has not been adequately considered in climate studies to date. Inferences about the causes of surface warming over the industrial period and about climate sensitivity may therefore be in error. ... Unfortunately, virtually all climate model studies that have included anthropogenic aerosol forcing as a driver of climate change (diagnosis, attribution, and projection studies; denoted

“applications” in the figure) have used only aerosol forcing values that are consistent with the inverse approach. If such studies were conducted with the larger range of aerosol forcings determined from the forward calculations, the results would differ greatly. The forward calculations raise the possibility that total forcing from preindustrial times to the present ... has been small or even negative. If this is correct, it would imply that climate sensitivity and/or natural variability (that is, variability not forced by anthropogenic emissions) is much larger than climate models currently indicate. ... In addressing the critical question of how the climate system will respond to this [anthropogenic greenhouse gases'] positive forcing, researchers must seek to resolve the present disparity between forward and inverse calculations. *Until this is achieved, the possibility that most of the warming to date is due to natural variability, as well as the possibility of high climate sensitivity, must be kept open.* [emphasis added]”

To further understand the complexity of calculating aerosol forcing, Jacobson (2001, Journal of Geophysical Research, vol. 106, 1551-1568) has to account for a total of 47 species “containing natural and/or anthropogenic sulfate, nitrate, chloride, carbonate, ammonium, sodium, calcium, magnesium, potassium, black carbon, organic matter, silica, ferrous oxide, and aluminium oxide” in his recent estimate of only the global direct radiative forcing by aerosols. (Jacobson [2001] found that the global direct radiative forcing by anthropogenic aerosols is only -0.12 W/m^2 while the forcing by combined natural and anthropogenic sources is -1.4 W/m^2 .) There are also the indirect aerosol effects. Temperature or temperature change is clearly not the only practical measure of effects by aerosols. Haywood and Boucher (2000, Reviews of Geophysics, vol. 38, 513-543) stressed the fact that the indirect radiative forcing effect of the modification of cloud albedo by aerosols could range from -0.3 to -1.8 W/m^2 , while the additional aerosol influences on cloud liquid water content (hence, precipitation efficiency), cloud thickness and cloud lifetime are still highly uncertain and difficult to quantify (see e.g., Rotstayn and Liu, 2003, Journal of Climate, vol. 16, 3476-3481). This is why one can easily appreciate the difficulties faced by Santer et al. (2003) because climate forcing by aerosols is not only known within a wide range of uncertainties but also to a large degree of unknown.

Therefore, I conclude that in addition to the fundamental issues related to climate model representation of physical processes, papers like Santer et al. (2003) have also failed the basic requirement for internal consistencies in the accounting for potentially relevant climatic forcing factors and feedbacks. This is why I cannot comment on the implication of this particular study and the meaning of the study for long-term climate history.

21. In your testimony, you discussed there being “warming” and “cooling” for different periods. If you did not construct an integral across the hemisphere or a real timeline, don’t your findings really just say there were some warm periods and cool periods, and therefore cannot speak to the issue of the rate of warming or cooling?

I am not sure about the meaning of this question and the quotes. My oral remark was merely referring to “making an accurate forecast that includes all potential human-made warming and cooling effects.” The detailed discussion about the climatic and environmental changes for the past 1000 years as deduced from the collection of proxies I had studied was given in Soon et al. (2003). I can certainly speak to the rate of warming or cooling at any given location or region when the available proxy, with sufficient temporal resolution, is known or proven to be temperature sensitive.

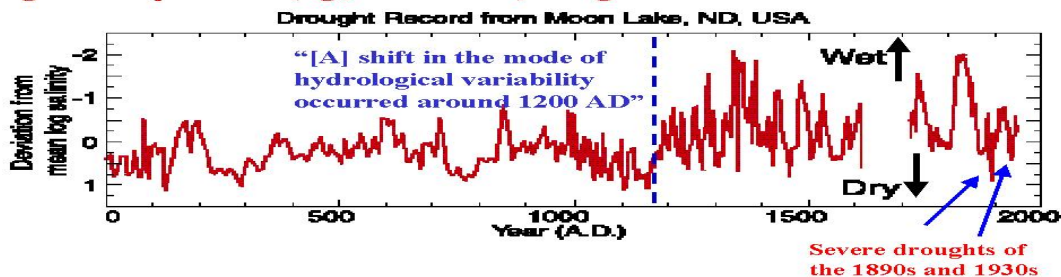
22. Is there any indication that regional climate variations are any larger or smaller at present than over the last 1000 years (with 2003, for example, perhaps being a case with large regional variations from the normal)?

I would not recommend considering the pattern of change from a single year, i.e., 2003, and called it a climate change. But the fact is that in Soon et al. (2003) we had carefully studied individual proxy records from various locations and regions. As an example, the 2000-year bottom-sediment record from Moon Lake, North Dakota, shows there is perhaps a distinct shift in the mode of hydrologic variability in the Northern Great Plain region starting around 1200 AD with the more recent period being more variable from the past. But, as indicated in the chart below, the author of this paper also noted that the severe droughts of the 1890s and 1930s around this area are “eclipsed by more extensive droughts before the beginning of the instrumental period.”

Greater drought intensity and frequency before AD 1200 in the Northern Great Plain, USA

(Laird et al., 1996, *Nature*, vol. 384, 552-554; Alverson and Oldfield, 2000, *PAGES News*, vol. 8, no. 1, p. 9)

“The severe droughts of the 1930s and 1890s (positive inferred salinity) are well reconstructed, but are eclipsed by more extensive droughts before the beginning of the instrumental period. There is an abrupt change in drought variability around AD 1200. Before that time, the high plains were characterized by much more regular and persistent (e.g., interdecadal) droughts.”



23. In your oral presentation, you talked about “[h]aving computer simulation.” Could you please explain what you [as in your original] computer simulation or modeling to which you are referring, and, a) Has this model gone through the appropriate set of model intercomparison studies like the various other global models? b) What forcings have been used to drive it? c) How does it develop regional climate variations, and are these comparable to observations? and, d) How does it perform over the 20th century, for example?

I apologize for any potential confusions.

In my oral remark, I said

“The entirety of climate proxies over the last 1,000 years shows that over many areas of the world there has been, and continues to be, large climate changes. Those changes provide challenges for the computer simulations of climate. The full models, which explore the Earth region by region, can be tested against the natural patterns of change over the last 1,000 years that are detailed by the climate proxies. Having computer simulations reproduce past patterns of climate, which has been influenced predominantly by natural factors, is key to making an accurate forecast that includes all potential human-made warming and cooling factors.”

So in the context of what I said, this question is clearly misdirected by someone who did not understand my remark. I was speaking on the potential application of works like Soon et al. (2003) for improving our ability to calculate with confidence the potential effects from man-made factors by first and foremost having a climate model that can at least reproduce some of the observed local and regional changes of the past.

Personally, I am also conducting my research through the help of several climate models (both simple and complex types) appropriate for my interests and I would certainly apply what I found in Soon et al. (2003) to my own future studies using climate models. Any additional comments will be beyond the simple context of my oral testimony. But, it may be useful to take note of the comments by Green (2002, *Weather*, vol. 57, 431-439): “It has always worried me that simple models of climate do not seem to work very well. Experts on numerical models say that this is because the atmosphere is very complicated, and that large numerical models and computers are needed to understand it. I worry because I do not know what they have hidden in those models and the programs they use. I wonder what I can compare their models with. Not with each other because they belong to a sort of club, where to have a model that disagrees with everyone else’s puts you outside. That is not a bad system, unless

of course they are all wrong. Another curiosity of complicated models is that their findings are rarely used to improve the model that preceded them. I would have expected that the more complex model would show where the simpler one had got it wrong, and allow it to be corrected for that misrepresentation.”

24. Based on the various comments of your scientific colleagues regarding your paper, including the methodological flaws pointed out in that paper by the former editor-in-chief of *Climate Research*, are you planning any reworking of your study or any further studies in the paleoclimatic area?

The use of a phrase like “methodological flaws” is a very convenient attempt to dismiss the weight of scientific evidence presented in Soon et al. (2003) but unfortunately without any clear nor confirmable basis. Thus far, the only formal criticism of Soon et al. (2003) was by Mann et al. (2003, *Eos*, vol. 84(27), 256-257) and we had provided our response to that criticism in Soon et al. (2003b, *Eos*, vol. 84(44), 473-476). My research interest and work to fully discern and quantitatively describe the local and regional patterns of climate variability over the past 1000 years or so will certainly continue despite this mis-characterization.

It should however not be left unnoticed that several very serious problems in Mann et al. (1998, 1999), Mann and Schmidt (2003) and Mann and Jones (2003) had been found recently. Those unresolved anomalies are outlined in my answers to your questions # 3, 4, 5, 6, 9 and 13. A careful reworking with a fully open access to all data as well as a fully disclosed transparency of the actual methodologies and detailed applications will be the next important step for paleoclimate reconstruction research.

25. You indicated that there would likely be relatively small climatic response to even substantial increases in the CO₂ concentration. Do you disagree with the radiation calculations that have been done and the trapped energy that they calculate, as per the peer-reviewed literature? If so, please explain.

First, please consider the above discussion on climate forcing factors and climate response sensitivities under question # 20 as part of the answers to this question.

Second, I do not believe that I had made any strong claim, one way or another, about the CO₂ forcing and potential response in any specific quantitative term during my testimony (since factually no one can). I do want to comment, as in my response under question # 19, that CO₂, as a minor greenhouse gas, is not a determinant of Earth’s climate and therefore not entirely obvious a driver of its change. Most calculations in peer-reviewed literature (or not) that focus on the CO₂ factor indeed would only like us to believe that CO₂, especially under the

realm of radiative forcing, is the predominant factor for driving *anomalous* climate responses, while the unavoidable and very difficult core subject about the actual dynamical state of Earth's "mean" climate is ignored.

Third, some ten years ago, Lindzen (1994, Annual Review in Fluid Mechanics, vol. 26, 353-378) pointed out a rather serious internal inconsistency regarding the role of water vapor and clouds when the physics of greenhouse effect is normally evaluated even among expert scientists or expert sources of information. (See e.g., the comment "without [the greenhouse effect], the planet would be 65 degrees colder" by Jerry Mahlman in the February 2004 issue of Crisis Magazine, <http://www.crisismagazine.com/february2004/feature1.htm>) and the description of Greenhouse Effect in the EPA's "global warming for kids" webpage: <http://www.epa.gov/globalwarming/kids/greenhouse.html>.) Lindzen notes the "artificial inevitability" for the predominance of CO₂ radiative forcing as a climatic factor in the following passage.

"In most popular depictions of the greenhouse effect, it is noted that in the absence of greenhouse gases, the Earth's mean temperature would be 255 K [about 0°F], and that the presence of infrared absorbing gases elevates this to 288 K [59°F]. In order to illustrate this, only radiative heat transfer is included in the schematic illustrations of the effect (Houghton et al. 1990, 1992) [IPCC reports]; this lends an artificial inevitability to the picture. Several points should be made concerning this picture: 1. The most important greenhouse gas is water vapor, and the next most important greenhouse substance consists in clouds; CO₂ is a distant third (Goody & Yung 1989). 2. In considering an atmosphere without greenhouse substances (in order to get 255 K), clouds are retained for their visible reflectivity while ignored for their infrared properties. More logically, one might assume that the elimination of water would also lead to the absence of clouds, leading to a temperature of about 274 K [or 278 K depending on what value of the solar irradiation factor is used] rather than 255 K. 3. Pure radiative heat transfer leads to a surface temperature of about 350 K rather than 288 K. The latter temperature is only achieved by including a convective adjustment that consists simply in adjusting vertical temperature gradient so as to avoid convective instability while maintaining a consistent radiative heat flux. ... " (p. 359-361)²

Hu et al. (2000, Geophysical Research Letters, vol. 27, 3513-3516) added that as the sophistication of parameterization of atmospheric convection increases, there

² A more pedagogical discussion of the greenhouse effect is given by Lindzen and Emanuel (2002) in *Encyclopedia of Global Change, Environmental Change and Human Society*, Volume 1, Andrew S. Goudie, editor in chief, p. 562-566, Oxford University Press, New York, 710 pp.

is a tendency for climate model sensitivity to variation in atmospheric CO₂ concentration to decrease considerably. In Hu et al. (2000)'s study, the change is from a decrease in the averaged tropical warming of 3.3 to 1.6°C for a doubling of CO₂ that is primarily associated the corresponding decrease in the calculated total atmospheric column increase in water vapor from 29% to 14%.

26. If you accept those radiation calculations as valid, please explain why you seem to believe that the energy trapped by the greenhouse gases will have a small effect whereas you seem to believe that small changes in solar energy will have very large climatic effects?

In addition to my answers under questions # 19, 20 and 25 above, I would like to point out that the Sun's radiation is not only variable but it varies in the ultraviolet part of the electromagnetic spectrum often by factors of 10 or more. The question about the relative effects of anthropogenic greenhouse gases and the Sun's radiation in terms of radiative forcing is certainly of interest but it does not add much to my current research quest to understand the Earth's mean climatic state and its nonlinear manifestations.

27. Please explain why you think the physically based climate models seem to quite satisfactorily represent the seasonal cycles of the climate at various latitudes based on the varying distributions of solar and infrared energy, but then would be so far off in calculating the climatic response for much smaller perturbations to solar radiation and greenhouse gases?

As indicated below, the first part of this sentence about a satisfactory representation of seasonal cycles of climate by computer climate models is not any assured statement of fact. This is why the follow-up question cannot be logically answered.

For example, E. K. Schneider (2002, *Journal of Climate*, vol. 15, 449-469) noted that "[a]t this writing, physically consistent and even flux-corrected coupled atmosphere-ocean general circulation models (CGCMs) have difficulty in producing a realistic simulation of the equatorial Pacific SST [sea surface temperature], including annual mean, annual cycle, and interannual variability. Not only do the CGCM simulations have significant errors, but also there is little agreement among models." In a systematic comparison of the performance of 23 dynamical ocean-atmosphere models, Davey et al. (2002, *Climate Dynamics*, vol. 18, 403-420) found that "no single model is consistent with the observed behavior in the tropical ocean regions ... as the model biases are large and gross errors are readily apparent." Without flux adjustment, most models produced annual mean equatorial sea surface temperature in the central Pacific that are too cold by 2-3°C. All GCMs except one simulated the wrong sign of the east-west SST

gradient in the equatorial Atlantic. The GCMs also incorrectly simulated the seasonal climatology in all ocean sections and its interannual variability in the Pacific ocean.

28. In regard to your answers to the previous questions, to what extent is your indication of a larger climate sensitivity for solar than greenhouse gases due to quantitative analysis of the physics and to what extent due to your analysis of statistical correlations? Is this greater responsiveness for solar evident in the baseline climate system, or just for perturbations, and could you please explain?

Please see my answers to question # 26 above and 30 below.

29. Please explain why you seem to accept that solar variations, volcanic eruptions, land cover change, and perhaps other forcings can have a significant climatic influence, but changes in CO₂ do not or cannot have a comparable influence?

Please see my answers to question # 30.

30. Could you please clarify why it is that you think the best way to get an indication of how much the climate will change due to global-scale changes in greenhouse gases or in solar radiation is to look at the regional level rather than the global scale? How would you propose to distinguish a natural variation from a climate change at the local to regional level?

Questions # 28, 29 and 30 seem to be based on the unreasonable presumptions that some special insights about the effects of solar irradiation or land cover changes or even volcanic eruptions must be invoked or answered in order to challenge the role of carbon dioxide forcing in the climate system. That presumption is illogical. My basic view and research interest about carbon dioxide and the ongoing search for the right tool for modeling aspects of the Earth's climate system can be briefly summarized by my answers to questions # 19, 25, 26, 27 and perhaps 20.

As to your specific question on distinguishing a natural variation (either internally generated or externally introduced by solar variation or volcanic eruption) from a climate change by anthropogenic factors like land cover changes or carbon dioxide at the local to regional level, there is possibly a somewhat surprising answer. If one wish to single out the potential effects of man-made carbon dioxide against other natural and anthropogenic factors as hinted by your question, then the answer is clear – the CO₂ effect is expected to be small in the sense that its potential signals will be likely be overwhelmed

when compared with expected effects by other factors. It is a scientific fact that the signal of CO₂ on the climate may be expected only over a very long time baseline and over a rather large areal extent. For example, Zhao and Dirmeyer (2003, COLA Technical Report # 150; available at <http://grads.iges.org/pubs/tech.html>), in their modeling experiments that attempt to account for the realistic effects of land cover changes, sea surface temperature changes and for the role of added atmospheric CO₂, found that “[w]hen observed CO₂ concentrations are specified in the model across the 18-year period, ... we do not find a substantially larger warming trend than in CTL [with no change in CO₂ concentration], although some small increase is found. The weak impact of atmospheric CO₂ changes may be due to the small changes in specified CO₂ during the model simulation compared to the doubling CO₂ simulation, or the short length of the integrations. It is clear that the relatively strong SST [sea surface temperature] influence in this climate model is the driver of the [observed] warming.” Please also consider the point made by Lindzen (2002) under question # 8 above concerning the difficulties in linking the observed warming trend of the deep ocean (without challenging the quality and error of those deep ocean temperature data) to anthropogenic CO₂ forcing. Finally, I wish to note that Mickley et al. (2004, Journal of Geophysical Research, vol. 109, D05106) managed to use climate model simulations results to demonstrate “the limitations in the use of radiative forcing as a measure of relative importance of greenhouse gases to climate change. ... While on a global scale CO₂ appears to be a more effective ‘global warmer’ than tropospheric ozone per unit forcing, regional sensitivities to increase ozone may lead to strong climate responses on a regional scale.”

31. How does your recent article relate to your assignments at the Harvard Smithsonian Observatory? Is paleoclimate part of the task of this observatory?

The publications of Soon et al. (2003) or Soon et al. (2004) are possible because of research grants that I and my collaborators obtained through competitive proposals to several research funding sources. I am a trust-fund employee at the Harvard-Smithsonian Center for Astrophysics and the support of my position and research work here is mainly through my own research initiative and proposal application. The scientific learning about paleoclimatic reconstruction presented in Soon et al. (2003) is related to my research interest in the mechanisms of sun-climate relation, especially for relevant physical pathways and processes on multidecadal and centennial time scales. Additional fruit of my independent research and labor in the area of sun-climate physics, funded or unfunded, is exemplified by the March 2004 book “The Maunder Minimum and The Variable Sun-Earth Connection” (see <http://www.wspc.com/books/physics/5199.html>) by W. Soon and S. Yaskell

(published by World Scientific Publishing Company). It might also be instructive to note that paleoclimate researchers have been speculating about long-term variability of the sun as the cause of centennial- to millennial-scale variability seen in their proxy records.

32. In your testimony, you said that “climate change is part of nature.” Please describe what you meant, since obviously, climate change have occurred due, in part, to changes in various forcings, such as solar, continental drift, atmospheric composition, asteroid impacts, etc. rather than being just completely random events. Could you provide estimates of how large you consider future forcings might be and how big the climate change they might cause could be?

In this occasion, I am referring to the fact that any change or variability in climate is most likely a rule, rather than the exception, of the climate system. But I was not speaking about or trying to imply the factors of change, either naturally produced or man-made. I apologize for any potential confusion. It is certainly reasonable to suggest that those climatic changes may arise from “forcings” but it would be unwise to rule out internally generated manifestations of climatic variables that could be purely stochastic in origin. I would strongly recommend the pedagogical discussion by Professor Carl Wunsch of MIT in Wunsch (1992, *Oceanography*, vol. 5, 99-106) and Wunsch (2004, “Quantitative estimate of the Milankovitch-forced contribution to observed Quaternary climate change”, working manuscript downloadable from <http://puddle.mit.edu/~cwunsch/>).

I cannot speculate on future climate forcings and resultant climatic changes because I found no basis for doing so.

33. Please provide a comparable estimate, with some supporting examples from the past, of how big you think the decadal (or 50-year if you prefer) change in the hemispheric/global climate could be due to natural variability? If you prefer to focus on the regional scale change, could you provide an indication of any expected change in the degree of regional variability about the hemispheric and global values, and what the mechanism for this might be?

This question seems a related question trying to get at a quantitative comparison of how large natural climate variability on regional or hemispheric scale can be under the shadow of expected future changes. Again, with no intention to devalue this interesting question, I do not have sufficient knowledge nor ability to venture such an estimate. In fact, I would go so far to say that if the estimates of variability for both the past and future are known within a reasonable range of uncertainties, then the actual scientific research program to address questions about the role of added carbon dioxide no longer require further funding or

execution since we have obtained all the relevant answers. But you may have judged from my answers given throughout this Q&A that much remains to be quantified and understood and the hard scientific research must continue.

34. Please explain the scientific basis for your testimony that “one should expect the CO₂ greenhouse effect to work its way downward towards the surface.”

Co-answer to this question is given under question # 35.

35. Do you believe that there is greater greenhouse trapping of energy in the troposphere than at the surface and that the atmosphere has a low heat capacity? If so, how big is this temperature difference?

It is broadly agreed and assumed that carbon dioxide, when released into the air, has a tendency to get mixed up quickly and so is distributed widely throughout the whole column of the atmosphere. The air near the surface is already dense and moist, so addition of more carbon dioxide will introduce very little imbalance of radiation energy budget there. In contrast adding more carbon dioxide to the thinner and drier air of the troposphere will cause a chain of noticeable effects. First, the presence of more carbon dioxide in the uppermost part of the atmosphere will cause more infrared radiation to escape into space because there are more carbon dioxide molecules to channel this infrared radiation upward and outward unhindered. Part of that infrared radiation is also being emitted downward to the lower parts of the atmosphere and the surface where it is reabsorbed by carbon dioxide and the thicker air there. The layer of air at the lower and middle troposphere, being more in direct contact with this down-welling radiation, is expected to heat more than air near the surface. Thus, adding more carbon dioxide to the atmosphere should cause more warming of the air around the height of two to seven kilometers. (Please consider for example the discussion by Kaser et al. (2004) under question # 8 about the ineffectiveness of an added longwave radiation from a direct addition of atmospheric CO₂ or atmospheric temperature change in explaining the modern retreat of glaciers at Kilimanjaro.) In other words, the clearest impact of the carbon dioxide greenhouse effect should manifest itself in the lower- and mid-troposphere rather than near the earth's surface. Here, I am mostly speaking on the basis of expectation from pure radiative forcing considerations.

Such a qualitative description is not complete, even though that is roughly what was modeled in the most sophisticated general circulation models (see e.g., Chase et al., 2004, *Climate Research*, vol. 25, 185-190), because it misses the key roles of atmospheric convection and waves as well as all the important hydrologic processes (please see e.g., Neelin et al., 2003, *Geophysical Research Letters*, vol. 30 (no. 24), 2275 and consider additional remarks about water vapor

and atmospheric convection under question # 25 as well as discussion on climate forcing factors and climate response sensitivities under question # 20). Some theoretical proposals expect a warming of the surface relative to the low- and mid-troposphere because of nonlinear climate dynamics (Corti et al., 1999, *Nature*, 398, 799-802). That expectation is because of the differential surface response with the pattern of Cold Ocean and Warm Land (COWL) that becomes increasingly unimportant with distance away from the surface (rather than just the difference in heat capacity mentioned in your question) [see Soon et al., 2001 for additional discussion]. Nevertheless, no GCM has yet incorporated such an idea into an operationally robust simulation of the climate system response to greenhouse effects from added CO₂. In the latest “global warming” work, Neelin et al. (2003), for example, still distinctly differentiate between mechanisms for tropical precipitation that are initiated through CO₂ warming of the troposphere and through El Nino warming rooted in oceanic surface temperature and subsurface thermocline dynamics. (Further note that their model experiments [see Figure 2b+2c and 10b+10c of Chou and Neelin, 2004, “Mechanisms of global warming impacts on regional tropical precipitation” in preparation for *Journal of Climate*; available at <http://www.atmos.ucla.edu/~csi/REF/>] also clearly shown that the troposphere warmed significantly more than surface with the doubling of atmospheric CO₂ as discussed by Chase et al. 2004 below.)

But it is worth noticing that the current global observation shows that, at least over the 1979-2003 interval, the lower tropospheric temperatures are not warming as fast as the surface temperatures (see Christy et al. 2003, *Journal of Atmospheric and Oceanic Technology*, vol. 20, 613-629; for additional confidence on the results derived by the University of Alabama-Huntsville group, please see Christy and Norris, 2004, *Geophysical Research Letters*, vol. 31, L06211). This observed fact is in contradiction to the accelerated warming of the mid and upper troposphere relative to surface simulated in current models (Chase et al. 2004). Chase et al. (2004) arrives at the following conclusions, upon examining results from 4 climate models in both unforced scenarios and scenarios forced with increased atmospheric greenhouse gases and the direct aerosol effect³:

- Model simulations of the period representative of the greenhouse-gas and aerosol forcing for 1979-2000 generally show a greatly accelerated and detectable warming at 500 mb relative to the surface (a 0.06°C decade⁻¹ increase).
- Considering all possible simulated 22 yr trends under anthropogenic forcing, a strong surface warming was highly likely to be accompanied by accelerated warming at 500 mb [i.e., 987 out of 1018 periods or 97% of the cases had a larger

³ Such a study should also be consistently challenged by the discussion under question # 20 about the adequacy of studying responses from a combination of incomplete forcings — though my primary purpose here is to illustrate the theoretical expectation of CO₂ forcing deriving from state-of-the-art climate models.

warming at 500 mb than at the surface] with no change in likelihood as forcings increased over time.

- In simulated periods where the surface warmed more quickly than 500 mb, *there was never a case* [emphasis added] in which the 500 mb temperature did not also warm at a large fraction of the surface warming. A 30% acceleration at the surface was the maximum simulated as compared with an observed acceleration factor of at least 400% the mid-troposphere trend.
- In cases where there was a strong surface warming and the surface warmed more quickly than at 500 mb in the forced experiments, there was never a case in which the 500 mb-level temperatures did not register a statistically significant ($p < 0.1$) trend (i.e., a trend detectable with a simple linear regression model). The minimum p value of approximately 0.08 occurred in the single case in which the significance was not greater than 99%.
- It was more likely that surface warmed relative to the mid-troposphere under control simulations than under forced simulations.
- At no time, in any model realization, forced or unforced, did any model simulate the presently observed situation of a large and highly significant surface warming accompanied with no warming whatsoever aloft." (p. 189)

36. 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?

All sources of funding for my and my colleagues' research efforts that resulted in the publication of Soon and Baliunas (2003) and Soon et al. (2003) were openly acknowledged. In other words, all sources of funding were disclosed in the manuscripts when they were submitted for publication; all sources of funding were also disclosed to readers in the printed journal articles. I am not the principal investigator for some of the grants we received (e.g., the NOAA grant was awarded to Professor David Legates), so I am not in the privilege position to provide exact quantitative numbers. But throughout the 2001-2003 research interval in which our work was carried out, the funding we received from the American Petroleum Institute was a small fraction of the funding we received from governmental research grants.

The primary theme of my research interest is on physical mechanisms of the sun-climate relationship. This is why researching into the detailed patterns of local

and regional climate variability as published in Soon et al. (2003) is directly relevant to that goal. Please also consider my research position listed under question # 31 above.

37. 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.

I have not knowingly been hired by, nor employed by, nor received grants from any such organizations described in this question.

38. Please describe the peer review process that took place with respect to your nearly identical articles published both in *Climate Research* and in *Energy and Environment*, including the number of reviewers and the general content of the reviewers' suggested edits, criticisms or improvements.

The *Climate Research* paper (Soon and Baliunas, 2003, *Climate Research*, vol. 23, 89-110) was submitted for publication and went through a routine peer-review process and was eventually approved for publication. The main content of the review was to propose: (a) reorganizing of materials including elimination of discussions on ENSO and GCMs; (b) removing "tone" problems by eliminating criticisms of previous EOF and superposition analyses; (c) reducing quotes especially those by Hubert Horace Lamb to improve readability; and (d) reviewing changes in each region with same thoroughness. The July 3, 2003's email (as Attachment I below) from the director of Inter-Research, Otto Kinne, who publishes *Climate Research* is enclosed below to confirm that the review process was fairly rigorous and all parties involved had carried out their roles and duties in this time-honored system properly.

The extended and more complete paper by Soon et al. (2003, *Energy & Environment*, vol. 14, 233-296) was submitted to *Energy & Environment* for consideration together with the accepted *Climate Research* manuscript. *Energy & Environment's* editorial decision was to send our manuscript for review, and after acceptance, include in its editorial in *Energy & Environment*, volume 14, issues 2&3, a footnote referring to the *Climate Research* paper.

Finally, we wish to correct that the false impression introduced by Professor Mann both during the testimony and in public media that his attack on the papers by Soon and Baliunas (2003) and Soon et al. (2003), in a FORUM article in the American Geophysical Union *Eos* newspaper (Mann et al., 2003, *Eos*, vol. 84, 256-258), were either rigorously peer-reviewed or represented widespread view

of the community. Contrary to Professor Mann's public statements, a FORUM article in Eos is said to be only stating "a personal point of view" (http://www.agu.org/pubs/eos_guidelines.html#authors). Whatever peer-reviewing that was done did not include soliciting comments from the authors of the papers being criticized. We first learned of this FORUM article from the AGU's press release No. 03-19 "Leading Climate Scientists Reaffirm View that Late 20th Century Warming Was Unusual and Resulted From Human Activity" (http://www.agu.org/sci_soc/prrl/prrl0319.html). See Soon et al. (2003b, Eos, vol. 84 (44), 473-476) for our own response to the Mann et al. FORUM article.

Attachment I for question # 38

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Date: Thu, 3 Jul 2003 13:06:25 +0200
From: Inter-Research Science Publisher <ir@int-res.com>

To
CLIMATE RESEARCH
Editors and Review Editors

Dear colleagues,

In my 20.06. email to you I stated, among other things, that I would ask CR editor Chris de Freitas to present to me copies of the reviewers' evaluations for the 2 Soon et al. papers.

I have received and studied the material requested.

Conclusions:

- 1) The reviewers consulted (4 for each ms) by the editor presented detailed, critical and helpful evaluations
- 2) The editor properly analyzed the evaluations and requested appropriate revisions.
- 3) The authors revised their manuscripts accordingly.

Summary:

Chris de Freitas has done a good and correct job as editor.

Best wishes,
Otto Kinne
Director, Inter-Research

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