26 October 2003

Questions for Dr. Stephen H. Schneider, Professor, Department of Biological Sciences, and Co-Director, Center for Environmental Science and Policy, Stanford University

QUESTION 1) You were also highlighted by Senator Inhofe as being a critic of the Intergovernmental Panel on Climate Change (IPCC) results. Would you care to clarify your comments concerning the IPCC range of expected temperature increases over the next 100 years?

Answer:

A **general comment** first on Senator Inhofe's overall approach to discussing the climate issue, then I will specifically respond to the temperature increase issue you raise.

It is indeed correct as the Senator suggests that I have criticized a few aspects of IPCC assessments, but then so too have nearly all scientists or policy analysts who honestly observe the complex issues involved. In fact, IPCC Lead Authors themselves are among the most vigorous critics of their own evolving assessment reports—as they should be. That is why IPCC assessments undergo several rounds of rigorous internal and external peer review—to maximize the likelihood of balanced assessment.

However, criticizing pieces of a whole hardly constitutes disagreeing with the principal conclusions and the overarching credibility of most of the IPCC analyses. It is misrepresenting my views to characterize them as even implying that IPCC has exaggerated or failed to describe the state of the science fairly at the time the assessment reports were completed in 2000. In fact, work of my own (see Testimony of Stephen H. Schneider to Senate Commerce committee on Oct 1, 2003) or by the MIT group Senator Inhofe mentions, shows we all believe that IPCC may have *underestimated* the potential for large climate change by restricting itself to existing climate models available in 2000, and that several more recent papers (e.g., Andronova and Schlesinger, 2001 or Forest et al, 2001) show that climate may indeed be both more or less sensitive to greenhouse gas increases than IPCC could have known in 2000 when the Third Assessment was prepared. Thus, if anything, since the 2000 assessment—which may in fact be *conservative in its conclusions*--new research could as well increase the likelihood of higher warming as lower warming in 2100. Such is the nature of complex problems in which humans dump their wastes into the atmosphere at a faster rate than science can understand the consequences.

In addition, to criticize the international scientific community for not providing "a definitive scientific answer" to the question of what constitutes "dangerous" global warming is to misunderstand the nature of sound science—in which it is rare to have "definitive" knowledge of any complex system (and to so claim would be dishonest,

which IPCC did not do)**.** For example, in his July 28, 2003 floor speech, Senator Inhofe says:

> According to the U.N.'s Intergovernmental Panel on Climate Change, Kyoto will achieve "stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system."
>
> What does this statement mean? The IPCC offers no elaboration and doesn't provide any scientific explanation about what that level would be. Why? The answer is simple: thus far no one has found a definitive scientific answer. (Inhofe, 2003)

This statement is erroneous in several aspects, and calls into question the credibility of the research apparatus Senator Inhofe relies upon to determine what is sound science. First of all, it was the UN Framework Convention on Climate Change (UNFCCC—see: http://unfccc.int/), signed by President Bush (Sr.) and ratified by the US Senate about 10 years ago, which stated the object of the Convention was to stabilize greenhouse gas concentrations so as "to prevent dangerous anthropogenic interference…" IPCC has steadfastly insisted that what is "dangerous" is a value judgment, not a scientific conclusion, so the Senator's quote that "thus far no one has found a definitive scientific answer" about what is "dangerous" is a contradiction in terms since it is not science, but policymakers, that must decide what constitutes a "dangerous" threat. The job of scientists—such as in IPCC or US National Research Council assessments--is to discuss what consequences might occur at various greenhouse emissions levels and what is the likelihood and impacts of such potential consequences. That is responsible scientific assessment. The above quote is both incorrect in its assertion about what IPCC said about Kyoto and is not about science, let alone "definitive" science, an impossibility in principle about a complex problem assessing future possibilities.

Specific response on warming to 2100.

Senator Inhofe refers to my Commentary in Nature (Schneider, 2001), correctly noting that:

> In his article, Schneider asks, "How likely is it that the world will get 6 degrees C hotter by 2100?" That, he said, "depends on the likelihood of the assumptions underlying the projections." (Inhofe, 2003)

The Senator continues:

> But as Schneider wrote, the group drafting the IPCC report decided to express "no preference" for each temperature scenario.
>
> In effect, this created the assumption that the higher bound of 5.8 degrees Celsius appeared to be just as likely as the lower of 1.4 degrees Celsius. "But this inference would be incorrect," said Schneider, "because uncertainties compound through a series of modeling steps."
>
> Keep in mind here that Schneider is on the side of the alarmists. (Inhofe, 2003)

This is an unfortunate mischaracterization of the context of my commentary, since it implies that IPCC has overestimated the likelihood of future temperature rises, when in

fact they made no probability estimates—and I expressed a wish that they had tried, as difficult as that exercise would be. First of all, I praised the IPCC for have a wide range of possible emission scenarios—to honestly represent the divergence of possible futures that are reflected in the scientific and policy literature. Second, I also approved of the IPCC using several climate models to estimate the warming from each emission scenario, as using only one model would be misleading. However, the "incorrect inference" that Senator Inhofe quotes me saying was my concern about the assumption some analysts *outside of the IPCC* might make: namely, that if there was a uniform probability distribution implied over both emissions scenarios and climate sensitivity estimates, that some might misinterpret that as implying a uniform probability distribution of 2100 temperature warming estimates. What I actually said was:

> The most typical assumption is a uniform probability distribution across storylines (scenarios). This might seem to imply a uniform probability distribution in an outcome that really matters to policymakers: the likelihood of a given temperature rise in 2100. But this inference would be incorrect, because uncertainties compound through a series of modeling steps. Uncertainties in emissions scenarios feed into uncertainties in carbon-cycle modeling, which feed into uncertainties in climate modeling, which drive an even larger range of uncertain climate impacts. This 'cascade of uncertainties' (7) is compounded by the very wide range of emissions offered by the SRES authors. (Schneider, 2001)

In other words, I did not assert that IPCC overestimated anything, just that they put the burden on outsiders to estimate probabilities, and in my view the excellent expertise IPCC assembled should undertake the exercise in the next assessment. Moreover, I showed that under two differing sets of assumptions, there would be about a 20% or 40% probability of 2100 temperatures exceeding a large warming threshold (I used 3.5 ^{o}C to be conservative, though IPCC (2001) noted warming over 1.5 ^{o}C raises serious potential threats for some systems and regions). I said that such different probabilities imposed a burden for decision makers to interpret. I also said quite emphatically and explicitly that the probability distributions I showed in my Figure 1 in that paper were simply illustrations of the potential for misinterpretations—I even put quotation marks around the word "frequency" to be sure nobody misinterpreted the graph as being based on subjective probabilistic analysis, rather than being what is was: a demonstration of how it is too easy for there to be misinterpretations. This is what I actually said about that:

> The sensitivity of the likelihood of threshold crossing occurrences is thus quite sensitive to the particular selection of scenarios and climate sensitivities used. Arbitrary selection of scenarios or sensitivities will produce distributions that could easily be misinterpreted as containing subjective probabilistic analysis when in fact they do not--until judgments are formally made about the likelihood of each scenario or sensitivity. For this reason the word "frequency" appears with quotation marks on Figure 1, as it is not a justifiable probability distribution given that the subcomponents are arbitrarily chosen without a "traceable account" (Moss and Schneider, 2000) of the logic of the selection process. (Schneider, 2001)

I concluded my commentary in Nature by expressing my concern:

> The special report leadership was not wrong, of course, about how difficult it would be to attempt to assign subjective probabilities to radically different visions of the future. However, in the probability vacuum that followed its assertion that all scenarios were "equally sound", we are facing the even more worrying prospect of dozens of users selecting arbitrary scenarios and climate-model sensitivities to construct frequency charts that purport to enlighten policymakers on

> the likelihood of "dangerous" warming. In the risk-management dilemma that constitutes climate-change policymaking, I would definitely put more trust in the admittedly subjective probability estimates of the SRES team than the myriad special interests that have been encouraged to make their own selection. Meanwhile, while we wait for IPCC to decide whether to reassemble the team for this controversial labor, climate policymakers and advisers will have to be vigilant, asking all advisors to justify the threshold they choose for predicting "dangerous" climate change, as well as to provide a "traceable account (Moss and Schneider, 2000) of the basis of their selection of emissions scenarios and climate-model sensitivities, as these jointly determine the probability of future risks. (Schneider, 2001)

Thus, Senator Inhofe's interpretation that "Schneider's own calculations, which cast serious doubt on the IPCC's extreme prediction", is not a proper characterization of my intent or analysis. In fact, I specifically argue in a number of places (summarized in my Oct 1, 2003 Testimony to the Senate Commerce Committee) that the sword of uncertainty cuts two ways: IPCC is as likely to have underestimated the likelihood of climate change crossing dangerous thresholds as having overestimated it, and that a better characterization of probabilities would be a useful exercise for the next IPCC assessment.

Finally, I fail to see how my very conservative approach to characterizing openly the uncertainties of climate projections--and my advice on how to improve the situation published in highly visible journals (like Nature)--could possible be characterized fairly by anyone as "Schneider is on the side of the alarmists" or is "an outspoken believer in catastrophic global warming", the ad hominem assertions of Senator Inhofe. I am indeed an "outspoken believer" that *both* mild or catastrophic global warming outcomes remain plausible. That is why I advocate the use of more probabilistic formulations of the potential risks (e.g., Schneider, 2001) and consideration by the policy communities of possible hedging strategies against the more serious possibilities—just like most business, medical or military assessment groups would do when they face deep uncertainties and an uncomfortable chance of potentially risky outcomes.

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QUESTION 2) You mentioned in your statement the next area of research is climate sensitivity probabilities. You also mentioned that MIT has started work in this area. Can you explain the importance of this area of research? Today, we get the weather forecast in terms of probabilities. Is this an attempt to get climate data in the same format?

As noted in my answer to question 1 above, one important question policy makers ask of climate scientists is by how much might it warm up at some future time given various levels of greenhouse gas concentrations—precisely the question posed in the UNFCCC "dangerous interference" quote. At least two factors contribute greatly to uncertainties over the amount of warming projected: scenarios of greenhouse gas emissions and the climate sensitivity (i.e., by how much the temperature will rise given a fixed—usually doubling—increase in the concentration of CO_2). Up to about 2000, most assessments tried to bracket uncertainties in these factors by providing scenarios of emissions and ranges of climate sensitivity. That is what led IPCC to give its well-cited 1.4-5.8 ^oC temperature increase range for 2100. Several authors/groups have argued that more than

ranges are needed for helping policy makers, since the likelihood of any particular warming is also necessary to make informed risk-management decisions. Thus, a probabilistic analysis is desirable, if possible, to produce credible risk assessments. Several early attempts have been made since the IPCC Third Assessment in 2000. These include: Wigley and Raper, 2001, Schneider, 2001 and recently Webster et al, 2003.

An important part of the attempt to provide probabilistic assessments is estimation of climate sensitivity as a probability distribution. Recently, several groups have attempted to derive such distributions, by matching the range of emissions of greenhouse gasses and aerosols and comparing them to the actual temperature rises over the past 50 years. Such attempts to scale climate sensitivity by actual observed temperature changes has resulted in a substantial expansion in the range of climate sensitivity from most previous assessments. Up to 2000, it was typically believed the most likely range for climate sensitivity was 1.5 to 4.5 ºC warming for a doubling of CO_2. Now, two studies, for example, Andronova and Schlesinger, 2001 (from the University of Illinois at Champagne-Urbana) and Forest et al 2001 (from MIT) derive climate sensitivities well above and below the heretofore "canonical" range of 1.5-4.5 ºC. That is why I said in my formal testimony to the Commerce Committee on Oct 1, 2003 (and in answering question 1 above), that recent analyses have actually expanded uncertainty, not reduced it, for the climate sensitivity assessment. Of course, eventually more research will narrow uncertainties, but at the moment we face an even larger range of potential warming outcomes and thus the probability of exceeding thresholds that some might consider "dangerous" has actually *gone up*, not down since the Third Assessment Report.

The Webster et al 2003 work (from MIT) has tried to incorporate several major uncertainties via probability distributions and produced, like Schneider, 2001 and Wigley and Raper, 2001, a probability distribution for warming in 2100, that essentially encompasses the IPCC range, but shows the possibility of both greater than 5.8 º C or less than 1.4 ºC warming. I believe that this new approach—expressing the important elements of projected climate change in probabilistic terms--will become the method of choice for the research community over the next decade. Early results still show a very wide distribution, but hopefully over the next two decades uncertainties can be substantially narrowed. Of course, whether the now-expanded possibility for warming over several degrees should motivate policy actions—what I personally believe constitutes a sound hedging strategy—is the value judgment policy makers will be facing in the decades ahead. Hopefully, the new probabilistic presentations will put risk-management judgments on a firmer scientific basis.

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