Chapter 12: Panelist Responses

WILLIAM NORDHAUS:

Politicians will do their own thing, but we have our own responsibility to get the economics and science right. I'll just suggest three areas.

The first is on what I'd call high resolution and high frequency science. A striking thing about the latest IPCC report is how little the baseline scenarios have changed since the first report was published. But at the edges, there are major issues: abrupt climate changes, issues of glaciology, of tempestology—hurricanes—and of regional resolution. These are some of the fine-grained details that are the tails of the distributions that we talked about earlier. We social scientists are downstream: we collect the debris from science as it comes by us, the good models, the bad models, the good studies, and the bad studies. But, basically, we can't get anywhere without high resolution and high frequency science in this area.

For social science research, I'll mention two issues. First, the scenarios that are now being used for the climate models are inadequate. They're the SRES scenarios (from the *Special Report on Emissions* of the Intergovernmental Panel on Climate Change), which are ten years old in their conception and emerged not from a committee or even a review, but from international negotiations. There is a need for serious long-term modeling of energy and the economy using a multi-country approach—not these little reduced-form models that you've heard about today, but serious modeling. Second, we understand less than we ought to about how technological change is induced, about what we call, in technical language, endogenous technological change. We don't have a firm understanding of how to encourage green technologies, of the role played by the market versus government subsides, government tax credits, and government performance. This is an area we need to study more.

Finally, in political science and law, our conception of what is needed is very limited. There's virtually nothing for guidance except for Scott Barrett's work on the political economy of international agreements. Though we know a lot about the political economy and legal aspects of international trade, for example, we have a far less robust basis for making international environmental agreements. The Kyoto Protocol, I think,

was recognized in 1997 as a conceptual disaster; it has no coherence politically or economically or environmentally. We also have to recognize that it is the outcome of bargaining, of a complicated set of negotiations among major parties. I think in the climate change area, a better conceptual framework for negotiating is absolutely critical to an understanding of how we go beyond the current inadequate arrangements.

WILLIAM CLINE:

First, I would like to see some research on whether we really expect the beginning of a reversal in atmospheric concentrations, as Nick Stern just said, after a 200-year time horizon.

Second, what kinds of probabilities and what gross quantifications of economic cost could one attach to seven meters or more of sea-level rise from the Greenland and West Antarctic ice shelf?

GARY YOHE:

One of the illustrations in the Stern Review is a matrix (crafted by Tom Downing and Paul Watkins) that has uncertainty on one axis and valuation metrics on another. Under uncertainty, it goes from projection to bounded risk to true surprises. Under valuation, it goes from market to non-market to social valuations in multiple metrics. The upper left-hand portion of that matrix has some numbers in it. And the lower right-hand portion has almost nothing in it. The right-hand column is almost vacant. The surprises part is almost vacant. You would be enormously surprised, perhaps, to learn how hard it is to get the scientific community to give us descriptions of what the impacts of global warming will be, and even to give us some idea of what we should monitor, and how timely the signal would have to be, for the world to avoid going over particular thresholds. As economists we need that sort of information if climate change is a risk problem. We need to know what happens in the tails. And if the tails are thick, we need to have some idea about their thickness and how far out they go.

In the definition of policy, there's a tension between, on the one hand, a policy environment that adjusts to new science as it comes on-stream and aims at goals that we believe are appropriate, and on the other hand, policy that's predictable, persistent, and

sets a stable environment within which investments can be undertaken. I don't think we know very much about that specific tension in policy design.

ROBERT MENDELSOHN:

We've spent a tremendous amount of money on the natural science part of the climate-change debate, linking energy use to final changes in temperature. We have spent very little looking at other aspects of the debate and, in particular, on the damage side, we've looked at very little that connects changes in temperature to actual damages. We know a little bit about what's going to happen in the United States, but much less about the rest of the world.

I also think it is very important to study high-consequence low-probability events. Scientists have predicted these, but there's virtually no social science analysis of what will happen if one actually occurs.

If there were a very inexpensive technological solution to climate change, I agree with Jeff Sachs that that would be the end of the story. But engineers have made mistakes in the past about how optimistic we should be about some technologies (such as nuclear power that was going to be too cheap to meter). And so it is very important to look at some of the proposed abatement technologies and see how feasible they are, and how expensive they're going to be.

It's also important to look at hidden costs and unexpected consequences. Abatement technologies have hidden environmental costs, and some of the things we're considering doing to stop climate change may have unexpected consequences. For example, suppose we do dramatic carbon sequestration and put all this stuff down in a pit and 20 years later it's all back in the atmosphere? That's a policy with no beneficial effect whatever. So, we need to study those things very carefully and try to understand what the consequences are.

One thing we definitely should develop is an emergency plan, some actions we wouldn't normally consider but would take if climate change turns out to be a lot worse than we expected and is running away from us. For example, we probably wouldn't want to send particles up into the upper atmosphere just as a regular policy. But if our

alternative is catastrophe here on the planet, then we want to have policies like that ready to go, and to understand how they would work.

Finally, one of the things we should recognize at this point is that the planet is going to warm no matter what we do. So one of the things we have to study is how to adapt. Most of the temperate countries may not have to do a whole lot, but for many low-latitude countries adaptation is probably a much more important question that will require both private and public sector attention. So, adaptation is an area we had better look at very carefully.

SCOTT BARRETT:

The problem of climate change is unprecedented. And a fundamental challenge it raises for research is to think about what kind of institutional designs might get us from here to there—to be able to prevent environmental catastrophe, when market forces favor business as usual—in enough time.

NICHOLAS STERN:

My list for research topics fits quite closely with those that we've just heard. I'll be brief as I outlined some key areas of research in this morning's presentation.

I would add—and it would be my first thing—that we need to think carefully about the ethics of climate change and responses to it. Much of policymaking in this area derives inevitably from approaches to what's right and what's responsible. And I think we ought to think about and discuss that directly and rigorously. We can think through, as Tjalling Koopmans argued, the consequences of different kinds of assumptions on the ethics and come to understand the ethics that way. It's a kind of thought-experiment approach to moral philosophy, which is quite fundamental, I think.

Second, I would look at the theory and practice of extreme events. The kind of work that Marty Weitzman has been doing is very instructive. I think there's a theory of ambiguity, a theory of uncertainty, a theory of genuine lack of knowledge about probabilities, that is just developing among mathematicians, where you relax the von Neumann-Morgenstern assumptions a bit and you lose some of the thrust and results of expected-utility theory, but you're still left with something. A lot of our problem arises

from not knowing what the probability distributions are, which makes it a bit presumptuous even to attach Bayesian priors to them. So I think that kind of theoretical research is important. So is empirical work to try to understand how societies cope, or do not cope, with the kinds of stress that could arise, and thus what kind of migration or conflicts might follow. For example, what we're seeing now in Darfur is partly the result of climate change, where 'mobile' pastoralists after long periods of drought are running into 'fixed' agriculturists.

Third, I think there's a lot to be learnt in the theory and practice of technological inducement. I would agree with Jeff Sachs on the importance of regulation on that front, as making things move quickly, but we'd have to think hard about whether going that route leads to efficiency losses.

Fourth, I would also be very happy to see pilot projects and much more research on deforestation. I suspect that the costs of deforestation will look quite small in some parts of the world and quite large in others, and there's a research issue to try to identify where they may be low and where they might be high.

Last, I think that studies on international action as proposed by Scott Barrett would be tremendous. International action is a very important part of what's needed. How we design, for example, international cap-and-trade schemes, how we give the kind of confidence that's required by setting targets for 2020 and 2050, for example, and how those targets would or could actually develop into treaties where appropriate, I think, are important subjects for research.