12 Science and Technology Policies for the Environment

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This chapter examines some of the problematic assumptions of our current approach to environmental research and development (R&D), and suggests a different way of thinking about the environment and science and the connection between the two. This different way of thinking is aimed at productively informing future choices about environmental science policy.

The fundamental environmental reality that we confront is the six billion people living on our planet. The decisions made by these six billion people determine the quality of the environment. The patterns of settlement, energy and material use, waste generation, agriculture, and travel—among many other activities—of these six billion decision makers will determine the future of the environment.

Our environmental science agenda focuses most of its resources, perhaps 90 percent, not on the principal agent of environmental destruction and change (that is, people) or on those who will make decisions about how to address such destruction and change (again, people), but on the affected environmental systems themselves. The agenda concentrates on understanding the basic physical, chemical and biological systems operating on the planet, and their interactions. The agenda addresses to a considerably lesser extent the ways that humans affect those systems; the social, cultural, political, economic, and cognitive determinants of those human effects, or, on the other side of the coin, how natural systems in turn affect humans.

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Until we direct our research efforts more closely to align with an understanding of the actual causes of environmental change—that is, the decisions of the six billion—we are not going to create the knowledge we need to help us move toward a sustainable global society.

The Current Approach

Obviously the current approach has a rationale, and it is a familiar one. The idea is that we cannot make good decisions about the environment without a comprehensive understanding of how it works. This formulation seems unobjectionable, but its translation into a science agenda has created some difficulties.

Environmental problems almost always involve competing interests and values. Making decisions almost always creates winners and losers. We call on science to help adjudicate this competition. The model is very simple. If we have more knowledge about a particular environmental problem, then we will be able to make better decisions because the competition between various interests or values will give way to a rational analysis of the problem.

This may seem sensible. If we could predict with perfect certainty the outcomes of particular decisions, then we could make perfectly transparent choices between alternative views of the future. The problem, of course, is that we can never possess such certainty, at least not until after the fact, and often not even then. The problem of uncertainty lies at the very heart of the rationale for publicly funded environmental science.

The language used to justify the public investment in environmental science, especially related to the global climate, usually rests on the explicit expectation that reducing uncertainty about the future will enable effective decision making.

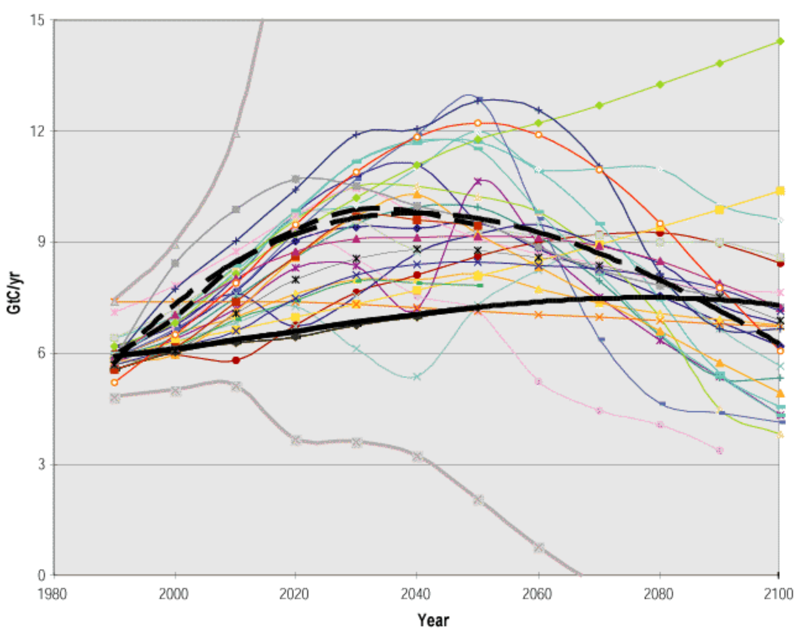
Climate Change

Climate change research constitutes about 25 percent of the entire federal environmental research portfolio in terms of budgetary expenditures—about $1.7 billion in a nearly $7 billion enterprise. If you add the energy

R&D that has some relation to reducing greenhouse gas emissions, then climate change research accounts for about 40 percent of the total environmental R&D portfolio.

Consider anthropogenic greenhouse warming. If we look at various projections of future emissions of greenhouse gases we would see that they all start at a point in the present where we have some certainty about what actual emissions are (Figure 1). Uncertainty is pretty low. But then the projections deviate all over the place into the future—even crossing one another at various points. Each projection is based on a different set of assumptions about how the climate works, how socioeconomic conditions will evolve, how energy and other technology will evolve and diffuse, how wealth and health will be distributed, how population will grow and how people will migrate, and other factors.

Figure 1
Projections of Future Emissions of Greenhouse Gases

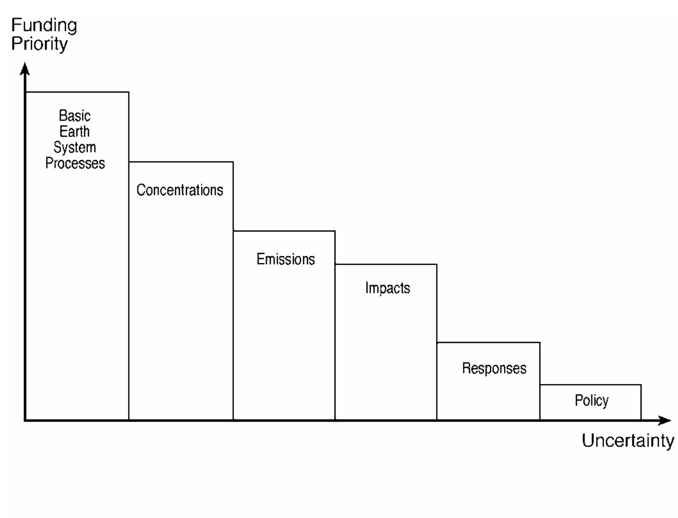


Source: Intergovernmental Panel on Climate Change (IPCC)

What we see is a known present fanning out into many possible futures. This fanning out of emission scenarios is the face of uncertainty. It is the reality that we have to deal with when we try to understand how to move forward into the future. The key point is this: However much knowledge we may have about the way the coupled ocean/atmosphere system works (and that is where the preponderance of our research dollars go), our capacity to confront, modify, mitigate, and adapt to that changing climate is going to be determined by the six billion people who make decisions on the planet every day. Climate is the dependent variable; society is the independent variable.

Another way to think about the problem is by considering what Steve Rayner (2000) has called the cascade of uncertainty (Figure 2). Our scientific funding priorities are inversely related to degree of uncertainty. There is, of course, a good reason for this. We may be able to reduce some uncertainties about Earth system processes, but we have very little prospect of reducing uncertainties about the future behavior of the six billion decision makers. Like the drunk searching for the key under the lamp, we focus resources where we have the scientists, the methods, the technologies to productively address the physical, chemical, and biological behavior of the planet. But whatever our capacity to reduce uncertainties about fundamental earth systems, this does not make us smarter about the fundamental determinant of global change: six billion decision makers.

Figure 2
Cascade of Uncertainty (Rayner 2000)



But the real problem is even worse than this. Although rarely if ever acknowledged in policy documents advocating more research on climate systems, we often see in reality that scientific uncertainties about the physical, chemical, and biological behavior of complex earth systems actually increase over time. We have certainly seen this with climate models over the last 20 or 25 years. There is a good reason for this: as the political stakes rise, the science must delve ever deeper into the complexity of the problem. New questions emerge, new phenomena are discovered, and new uncertainties are revealed. Knowledge is advancing, but uncertainty is also increasing. We are getting farther from the promise of decreasing uncertainty so that better decisions can be made.

But the challenge is really much more intractable even than this. Our research agenda, which focuses on reducing uncertainty about earth systems, and our policy agenda, which focuses on reducing emissions of greenhouse gases, are rooted in the belief that we can control the future behavior of the climate and (of greater importance) that we can control the future impacts of climate on humans by conscious action aimed at achieving desired outcomes.

But what, in fact, are we worried about? What is the problem with global climate change? This question has many possible answers, ranging from impaired ecosystem functions to reduced agricultural productivity to compromised water supply to public health threats to increased natural hazards. I will focus on the problem of hazards, although my general point applies to the other issues as well.

There is much concern that climate change (in particular greenhouse warming) will lead to an increase in the occurrence of extreme weather events that can have an adverse impact on society and the environment. The argument goes that if we reduce greenhouse gas emissions, we will reduce the incidence and severity of extreme weather events. I want to approach this simple formulation from a different perspective.

Two examples illustrate. First, in July 2000, an extreme weather impact event occurred in Manila, Philippines. A mountain of garbage ironically known as "The Promised Land" (because many people earned their livelihoods on it through scavenging) collapsed and killed about 200 people during a particularly heavy monsoon rain. Is this a climate event or is the problem that we have human beings living on garbage dumps and foraging on them for their living? Obviously, the event itself reflects a complex socioeconomic context—it is not simply a question of monsoon rains.

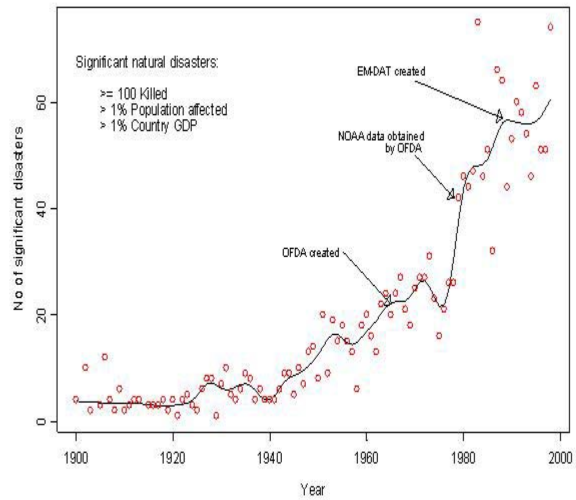
Another example is Hurricane Mitch, which in 1999 killed 10,000 people in Central America. Why did it kill 10,000 people? The rains were not unprecedented but they did, for example, saturate the soils of a deforested mountainside in Nicaragua. The mountainside collapsed in a catastrophic mudflow and killed 7,000 people in just a few minutes. Again, we can hardly characterize this as a problem of climate dynamics; climate played a role, but the socioeconomic context created the conditions for the human impacts.

If we look at trends for major disasters over the past century (Figure 3), we see a steep rise in the human and economic costs of such disasters, especially in the last 20 to 30 years. The reasons for this rise are not changes in the incidence of extreme events like hurricanes or floods; the reasons are increases in population, increases in socioeconomic exposure, increases in migration of population to coasts, and other factors.

Maybe this will change in the future. Maybe we really do need to worry more about the influence of climate change on extreme events. To test this idea, Roger Pielke, Roberta Klein, and I did a simple sensitivity analysis to compare the relative contributions of expected socioeconomic change (economic growth; population growth) and climate change (increasing incidence and severity of tropical cyclone events) to future losses from hazards (Pielke *et al.*, 2000). Using socioeconomic scenarios from the Intergovernmental Panel of Climate Change, and climate scenarios from the published literature, we compared losses under conditions of socioeconomic change and climate stability with those under conditions of socioeconomic stability and climate change (Figure 4). Based on a simple loss-extrapolation method, we learned that losses from extreme weather events due to socioeconomic change are projected to increase by 200 to 500 percent over the next 50 years, whereas losses due to climate change are projected to increase by at most about 50 percent.

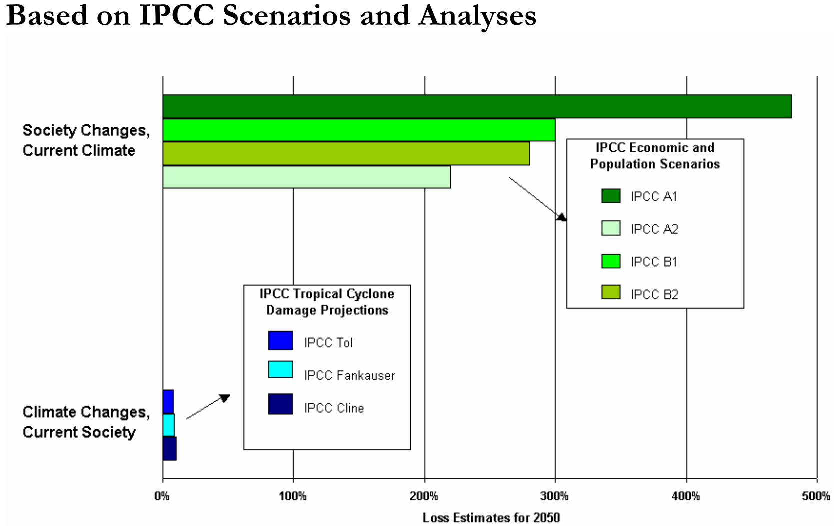
The point is not to minimize the potential impacts and the danger of emitting six billion tons of carbon into the atmosphere. The point is to find where the problems really lie, and from where solutions might most plausible emerge. Where do we need to focus our research efforts in order to best address the needs of the six billion people, and to minimize their impact on the population? If we are worried, for example, about natural

Figure 3
Trends for Major Disasters over the Past Century



Source: OFDA/CRED International Disaster Data Base

Figure 4
2050 Global Tropical Cyclone Loss Sensitivities
Based on IPCC Scenarios and Analyses



hazards and if we are worried about climate hazards in specific, what should be the balance between modeling the future climate and understanding how to increase societal resilience to events that are going to happen anyway? I am not suggesting any right answer to this. But I do suggest that the current allocation of resources heavily underfunds research to support societal resilience in the face of inevitable natural hazards, relative to research to support reducing uncertainty about the future of the climate.

The example of hazards illustrates a broader need. We need to think about environmental science policy in terms that are rooted not in the futile desire to control the future behavior of the environment through reducing scientific uncertainty, but in the conditions under which the six billion humans on Earth live and make decisions. This is not necessarily a brief for more money for social sciences, although that may well be necessary. It is an argument that whatever scientific tools we choose to bring to issues like global change, these tools must emerge from an understanding of the decision context open to human beings operating in a wide variety of scales in a wide variety of institutional settings.

One area of environmental research has already begun to confront this problem, even if it has not begun to move forward in a convincing way: ecological sciences. A century of failed efforts to manage ecosystems has begun to move ecologists away from command-and-control science toward adaptive management. In essence, many ecologists have accepted that ecosystems are so complex that their function cannot be predicted in detail and far in advance. The idea that you could use a scientifically prescriptive approach to making ecosystem management decisions was necessarily doomed to failure. The alternative is to integrate a process of scientific and policy experimentation, where decisions are based on the best science and the best practice that you have, but they are viewed not as solutions but as learning opportunities. The impacts of ecosystem management decisions are carefully monitored, and in a trial-and-error manner, ecosystem managers learn what works and what does not.

This sort of approach serves the decision making process in many ways. Because decisions have more modest expectations, the costs of winning and losing are much smaller, which means that the politics of making decisions, in principle at least, ought to be more tractable. Similarly, adaptive decision making does not commit you to long-term actions that could also commit you to long-term mistakes if your predictions are not correct.

For the last 10 years or so, adaptive management has been the buzzword in ecosystem management. Making it work in the real world is another question entirely. There are some good ongoing experiments, but the jury remains out. But I think this is probably the best example of a discipline embracing a different, nonprescriptive, heuristic approach to managing the environment. Reducing uncertainty is not necessary for action. On the contrary: action is used to reduce uncertainty.

Overall, I think it is fair to say that we do not have a good working model for developing an environmental science agenda that focuses on enhancing decision making rather than reducing uncertainty. One reasonable place to start, though, is with a report by the National Research Council: *Our Common Journey: A Transition Toward Sustainability* (Board on Sustainable Development, 1999). This report of the disbanded Board on Sustainable Development lays out a framework for sustainability science rooted in the conditions of the six billion, not in the need to understand the basic dynamics of the environment. It is a modest but hopeful beginning toward a redirection of environmental science priorities.

Conclusion

Let me suggest a few general principles that might help guide this redirection.

First, we need to focus more on observation and experimentation to support adaptive learning and less on comprehensive prediction. We need to focus more on science to support short-term adaptive decisions that can be revisited and changed, and less on science to support long-term commitments that are costly, controversial, and irreversible.

We need to focus more on science to support resilient social systems (that is, systems with reduced vulnerability to a wide range of possible outcomes, none of which can be predicted with certainty) and less on science to support specific control regimes (which can limit your ability to respond flexibly).

We need to understand the demand function for scientific information. I do not see how we can possibly design good environmental science policies if we do not understand the needs (vulnerabilities) and capabilities (absorptive capacity) of decision makers. This is a research problem. We need to know which decision makers are best positioned to influence a particular aspect of the environment (e.g., the carbon cycle). What are the decision options that they face? What are their capabilities for making decisions? What type of information will help them make those decisions? By how much does uncertainty need to be reduced, if at all, to allow them to take action? In many cases, it turns out that the issue is not reducing uncertainty. It is redefining a problem in a way that is politically tractable.

Our approach to environmental R&D has been backwards. We have focused on supply of knowledge without any comprehensive understanding of demand. Characterizing demand for information and know-how is a research problem, and we need to support it.

We will also need different types of research institutions, where the needs and capabilities of decision makers are internalized in the research-priority-setting process. There are many examples of such institutions in other areas of science, from the National Weather Service, to agricultural extension programs, to industrial research labs.

The final message: The goal of policy-relevant science needs to be good decisions and not good predictions. Moving in that direction will require significant changes in how we think about environmental science and its connections to policy-making. The place to start is with the six-billion.

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