Chapter 4
EPISTEMIC COMMUNITIES, SCIENCE, AND INTERNATIONAL ENVIRONMENTAL GOVERNANCE

SCIENTISTS AND THEIR FINDINGS mattered in the development of international environmentalism. The very nature of global environmental problems as uncertain and complex ensures that technical expertise is called upon for understanding and advice, and that scientific discovery can bring previously unknown problems to the attention of policymakers. It would be truly remarkable if scientists played no role—akin to suggesting that agricultural experts played no role in world agricultural or food programs or that medical doctors or researchers played no role in world health programs. If the research question of interest was simply "did scientific and technical knowledge on specific environmental problems influence international cooperation on those problems," or "did the growth in scientific knowledge or a rationalized scientific culture play some role in the increase in international efforts to address environmental concerns," the answer would surely be yes, or at least a qualified yes given that other factors also played a role.[1] But the research question that guides this study is not on the rise of environmentalism generally, nor is it mainly to explain cooperation on a specific environmental problem. Rather, the question is why did the *content* of global environmental policy, or the *appropriate* way to understand, address, or manage global environmental problems, evolve as it did? In other words, why did some ideas prevail over others to guide global environmental governance toward liberal environmentalism?

I begin with a focus on scientists, or expert groups more generally, because so much of the scholarship on global environmental problems either assumes or sets out to show the central importance of scientific communities and knowledge to environmental governance. This scholarship also contains an implicit prescriptive element. Whereas very few scholars naïvely argue, as Underdal (2000a:5) puts it, that "science is seen as carrying the torch of light, guiding what Plato referred to as 'philosopher kings' in their altruistic search for the common good," many authors who view rational science and expertise as necessary for understanding nature, and for the effective management of international environmental problems, do seem to pin their hope for improved environmental governance on the progressive influence that scientific communities can exert to modify state interests.[2] The question of whether this hope is well founded, as well as the rationalist basis of this hope, both deserve to be critically assessed.

As a lens through which to address these questions, this chapter tests in detail a prominent explanation for the evolution of environmental governance that focuses on the influence of expert or "epistemic" communities (Haas 1989, 1990, 1992a, 1992b, 1996; Adler and Haas 1992). These communities are bound together in a common policy enterprise and empowered by shared causal and principled beliefs. They need not be composed strictly of natural scientists, but their legitimacy, and thus their power to affect governance, must stem from shared notions of validity, or shared criteria for weighing and validating knowledge in the domain of their expertise (Haas 1992c:3). The assumption is that the commitment to professional methods and norms of scientific inquiry gives policymakers and the public confidence in the autonomy and integrity of scientists (Underdal 2000a:10). Thus, this explanation argues that scientific communities can influence and shape international policies because policymakers and governments recognize epistemic communities as legitimate sources of knowledge and expertise, and thus call on them to achieve policy goals in issues characterized by uncertainty and complexity. The global environment is clearly such an issue.

I test this hypothesis in particular for the three reasons listed in the introduction: its clear explanatory framework of how scientific knowledge translates into changed patterns of state behavior and international interactions; its influence on literature on the role of ideas in international environmental coordination and international relations more generally;[3] and because environmental governance should be a most-likely case, indeed, the paradigmatic case for the hypothesis. Thus, its failure in this case would provide strong evidence against the general validity of the hypothesis, since the expectation would be for it to also fail in less hospitable circumstances.

The epistemic communities literature also offers a good entry point into a broader discussion about the role of science and scientists in environmental governance. While the chapter specifically sets out to rigorously test a specific hypothesis of importance to political scientists, the insights this test generates will be of interest to anyone interested in the possibilities and limits of the influence of scientific knowledge on global environmental policymaking and international affairs more broadly. As will be seen, the findings of this chapter strongly contradict the conventional wisdom. They demonstrate that the central place of scientists in constructing action on issues that require technical and scientific expertise has been greatly exaggerated. Moreover, contrary to expectations in much of the mainstream scholarship, I find that scientific research and the uses to which it is put are as strongly shaped by existing social structures as vice-versa.

Given these shortcomings, the next chapter examines the role of economic ideas and puts forward an alternative explanation that better accounts for the interaction of ideas and social structures in global governance. It focuses on social structural pressures that favor the selection of some ideas over others in the institutionalization of international norms. In many ways, the next chapter can be read as an attempt to recapture the core insight from the broader agenda of the epistemic communities literature, that agency, legitimacy of ideas, and broader “epistemes” (dominant ways of looking at social reality or a set of shared symbols and references) can be important sources of continuity and change in international politics and can even (re)shape understandings of state interests.[4] However, the application of the epistemic communities literature, as it evolved to focus mainly on agency and the problem of cooperation or policy coordination, has suffered because of its inattention to the constraints of the international system. Recasting some of its insights in a broader theory of the interaction of ideas and international social structure offers a fuller understanding of the opportunities and constraints that new ideas face.

The separation of scientific and economic ideas may at first appear arbitrary or artificial. For example, economists and natural scientists sometimes interact in the policy process and even in the technical work on ecological problems. Also, individual scientists and economists might be influenced by ecological ideas and both groups might be considered "epistemic communities" in their own right. However, treating both equally as epistemic communities undermines the logical basis of the explanation—that a single community is granted legitimacy based on its claim to authoritative and policy-relevant knowledge in a certain issue area. If more than one such community exists, the reason for adopting the position advocated by such a group could not be accounted for simply by looking at its privileged position owing to its knowledge claims. Thus, in this chapter, I focus on the strongest group identified in the literature—scientists, and especially those scientists loosely considered sympathetic to "scientific ecology."

The chapter begins with a discussion of the precise claims of the epistemic communities literature in order to clarify how evidence should be evaluated. The remainder of the chapter traces the influence of scientists, determines whether they constituted an epistemic community, and assesses the influence of such a group in the lead-up to and deliberations during the three key turning points in environmental governance in 1972, 1987 and 1992. By focusing on what scientists actually did and the effects of those actions, I also hope to illuminate the interaction of science and global environmental governance rather than merely offer a critique of the epistemic communities hypothesis.

EXPLAINING NORM CREATION AND CHANGE WITH EPISTEMIC COMMUNITIES

Stated formally, an epistemic communities explanation asserts *that scientific consensus within an epistemic community, "politically empowered through its claims to exercise authoritative knowledge and motivated by shared causal and principled beliefs,"*(Haas 1992a:41) *and its promotion of norms derived from that consensual knowledge, leads to the adoption of its ideas over others as guides to appropriate behavior.* In this context, the relevant question is, did ideas promoted by key epistemic communities prevail over other ideas? Or, more generally, to what extent do expert groups determine the observed content of governing norms or specific policies those norms support?

The clearest substantive application of the epistemic communities hypothesis to explain international environmental governance comes from Peter Haas, who argues that an epistemic community formed around a "scientific ecology" research program (1989, 1990, 1992a, 1996). Scientific ecology is "distinguished by its systems perspective on environmental, social and economic problems; reflecting a multi-sectoral approach and a normative commitment to environmental preservation." Furthermore, members of this epistemic community have "sought to develop social laws from their understanding of the laws of nature" (Haas 1996:27–28). Thus, according to this argument, experts not only provided technical advice, which is undoubtedly true.[5] More importantly, Haas's argument is that the legitimacy of their knowledge-claims led to political empowerment and the content of such claims, and "social laws" derived from them, shaped governance.

Testing an epistemic communities hypothesis entails three steps. First, a community of experts, privileged by its claim to authoritative knowledge in the issue area and with a shared policy enterprise that generates a set of norms that stem from a scientific consensus within the group must be identified. Given the outcome in this case, such norms would need to establish linkages between environment and development for the hypothesis to hold. Second, the group must attempt to influence the political process through the promotion of its ideas. Third, relevant actors or institutions must eventually adopt those norms over alternatives (Haas 1992b:34).

I will show that the explanation fails each criterion for success in this case, demonstrating the need to move beyond the theory and substance of this argument for a fuller explanation. First, I find little evidence that a coherent epistemic community formed around "scientific ecology" or that scientists agreed on "social laws" derived from that research program. Second, scientific communities have a mixed record in influencing policy. Science and "scientific ecology" certainly played a role in identifying environmental problems and influenced thinking in international organizations such as UNEP and the IUCN. Those organizations in turn helped to disseminate ecological concerns to state governments, NGOs, and publics. Not surprisingly, science also played a role in supplying technical knowledge that helped in the formulation of some specific policies in response to perceived crises and in the face of uncertainty. So, an epistemic community approach tells part of the story. However, it performs far less well on the core political issue of consensus on what should be the appropriate responses generally to global environmental problems. Consensus on the nature of environmental problems was often weak, particularly among hard scientists when it came to values, management norms, or specific responses in key cases. Also, most hard scientists came late, if at all, to development concerns.

Finally, and most importantly, the ideas behind liberal environmentalism simply did not originate among scientists, ecological or otherwise—a point I return to in the next chapter. In fact, it appears the causal arrow often ran the other way. Ideas around the norm-complex of liberal environmentalism appear to have increasingly influenced scientific work that feeds into global environmental research, rather than vice-versa.

Before going over the evidence for these findings, it is important to have a clear understanding of the policy process that underpins the epistemic communities hypothesis. The epistemic communities literature asserts that the success of ideas depends on whether *consensus* emerges within such groups (Haas 1992a:41). Accordingly, a high degree of consensus within the community makes the ideas it supports more likely to influence policy and to facilitate coordinated state action around such ideas. Low consensus diminishes the influence of the group and makes agreement on matters related to the community's expertise less likely. Consensual knowledge embodied in epistemic communities can help states identify interests under conditions of uncertainty, frame issues for collective debate, propose specific policies, and identify salient points for negotiation. Thus epistemic communities' activities can lead to international policy coordination and ultimately determine the content of governing norms and policies.

Haas is not alone in focusing on scientific consensus as a key ingredient of successful influence (for example, see Benedick 1991; Döös 1991). However, others, such as Döös, focus more on the difficulty in achieving such consensus given problems of observation, measurement, and prediction of human effects on the environment. Furthermore, Döös (1991:4–7) argues that scientific consensus, while important, can be thwarted because governments may encourage negative feedback loops once the political process gets underway.[6] Writings on scientific policy generally echo the point that a variety of factors in the political arena may thwart or redirect expert knowledge. Some philosophers and sociologists of science go further, arguing that the conduct of disciplinary scientific research can never be fully exempt from politics over the internal construction of knowledge and scientific activity. In this view, the broader context of scientific research always interacts with societal and, in the case of policy-related science, governmental structures.[7] Whatever one thinks of the deep critique of knowledge construction, the question should be addressed of how does the formulation of policy interact with an apparent scientific consensus? In other words, what happens in the process of scientific influence that leads not only to positive and negative feedback loops for action, but also to the kind of action that is deemed appropriate?

The epistemic community hypothesis in its pure form appears to posit a fairly linear relationship between scientific consensus and policy outcome, with only minor institutional hurdles to overcome. In other words, once a sufficient level of scientific consensus is achieved—though it must navigate through national bureaucracies, convince leaders, and respond effectively to critics—it should provide the substantive basis on which to build agreement. The literature emphasizes channels of communication and influence in national governments, identifying these as the main political hurdles. Following this logic, the influence of an epistemic community depends in part on its privileged access to officials and leaders of national administrations or international secretariats, or to its members joining such bureaucracies themselves. Membership in such organizations helps to ensure the institutionalization of ideas carried by the epistemic community and the socialization of governments to the norms promoted by the group (Adler and Haas 1992:374).

The literature as a whole is somewhat ambiguous on the necessity of community members actually becoming government personnel. However, if a community's influence stems from the legitimate authority granted to it by virtue of its expertise and its policy activities are driven by principled beliefs around the issue at hand, then epistemic communities should be considered autonomous groups from the governments they influence, with their own set of interests and priorities. According to Haas, "The members of a prevailing community become strong actors at the national and transnational level as decision makers solicit their information and delegate responsibility to them. A community's advice, though, is informed by its own broad worldview" (Haas 1992c:4). If governments manipulate the activities of such groups according to government interests, their autonomy is compromised and the analytic weight that can be attached to epistemic community influence is diminished. Furthermore, if the evidence supports a very different understanding of the policy process than that just presented, the hypothesis fails.

An epistemic communities hypothesis also goes beyond a simple argument that an influential network of interested actors promoted ideas they preferred. Otherwise, concepts from the comparative public policy and transnational relations literature would suffice. Concepts such as "policy network/community," "advocacy coalition," "issue network," or "transnational social movement organization" all identify networks of actors involved in a policy, either owing to a common interest or shared policy enterprise.[8] Haas distinguishes epistemic communities by, in addition to their shared causal and principled beliefs and common policy enterprise, their "authoritative claim to policy-relevant knowledge in a particular domain . . . based on their recognized expertise within that domain" (Haas 1992c:17).

Furthermore, epistemic communities are more than mere purveyors of consensual knowledge. The consensual knowledge literature focuses almost exclusively on the uses of knowledge during negotiations. It pays less attention to the actors that carry such knowledge, their own goals and sources of legitimacy, or how such knowledge may affect interest-definition as on ongoing process outside of negotiations (Rothstein 1984; Sjöstedt 1994). In contrast, the epistemic community hypothesis draws its power from the special status accorded to the community's expertise, which gives it legitimacy. The focus on legitimacy of ideas gives the hypothesis its causal weight and analytic strength compared to other concepts. Epistemic communities are not united simply by interests; they form around specific knowledge claims and values that, to have force, must stem from those knowledge claims.

One should answer the following set of questions in the affirmative in order to have confidence in an epistemic community explanation: Was consensual cause-effect knowledge necessary for responses or action? Did knowledge come from an identifiable network or group acting with a particular value orientation? Was such a group autonomous from state actors and were the members self-recruited? Did the group push states in a policy direction they might otherwise have not taken? And, have other intervening factors been discounted (Haas 1992b; Haas 1992a, 44–45; Haas and Haas 1995:260)?

Most commonly, scholars utilizing this approach attempt to explain outcomes on discrete issues, such as ozone depletion or whaling. Such studies can easily identify single research communities, measure community influence by following how individuals move into domestic bureaucracies, and delineate the ins and outs of all relevant negotiations. Given the timeframe and breadth of this study, however, this approach is inappropriate. Instead, as Haas does in more recent studies (which are the logical extension of the approach), I will use broader strokes to focus on key ideas and the access given to major players in the relevant scientific communities who might qualify as members of an ecological epistemic community. I will look at the influence of ideas they championed and their own activity over time to determine their influence (Haas 1996; Haas and Haas 1995). To address the criticism that I have not carefully examined the influence of an epistemic community in a particular case, I also examine some specific cases in more detail, notably those where one might expect it to perform best, such as ozone and climate change.

In addition, it might be objected that it would be unfair to argue a monocausal explanation for the broad expanse of normative evolution examined in previous chapters. I would not expect an epistemic communities explanation to perform to the same standard as in a study of a specific treaty outcome or discrete environmental problem. However, the literature does assert that an ecological epistemic community holds *the* privileged position in the broader development of global environmentalism as well. This community holds such a position because of its commitment to examining cause-effect relationships through the scientific method, its allegedly holistic approach, and its commitment to environmental preservation. So it remains a worthwhile exercise to assess whether an epistemic community was either necessary or sufficient for the normative development identified in previous chapters and, if not, then to make some observations about just what role science did play in the evolution of governing norms.

THE ROLE OF SCIENTISTS AND SCIENTIFIC ECOLOGY

Scientific Ecology

I begin with an assessment of Haas's assertion that "scientific ecology" ideas dominated thinking in epistemic communities active in international environmental research. The assumption requires critical examination to avoid the circular reasoning that if outcomes reflect some ecological concern, they do so because of consensus within the relevant epistemic community on "scientific ecology."

Contrary to Haas, an examination of the work of ecologists suggests "scientific ecology" is an unlikely candidate to form the basis of epistemic consensus and values. The problem stems first from Haas's various definitions of ecology, which conflate ecological thought and the work of scientific ecologists, who, using scientific methods, simply study how living matter interacts with its environment. As a result, his description of an ideal-type scientific ecologist mixes facts and values from different branches of ecology, other disciplines, and the environmental movement. The links between a specific set of values and what ecologists actually do, or what their findings suggest for norms of human behavior, are simply far less direct than he suggests.

For Haas (1992a:43) ecology "has been described as a framework that assimilate[s] other scientific disciplines." It does so because it studies the interaction of living (the biosphere) and nonliving realms (the atmosphere, geosphere, and hydrosphere). So ecologists, who come from a variety of backgrounds, "share a common belief in the need for an holistic analysis that is sensitive to the possible feedback and synergistic relationships among a variety of variables." This ideal-type description of scientific ecology, however, masks sharp disagreements among ecologists, not to mention among many natural scientists who study environmental problems but do not necessarily subscribe to an ecological philosophical position. Two faulty implications stem from this definition: first, that ecologists uniformly adopt a systems approach; second, that "scientific ecology" dominated international environmental discourse. I challenge each assumption in turn.

The ecology Haas describes most closely resembles ecosystem and systems ecology, the latter pioneered in the work of Eugene Odum and, to a lesser degree, his brother Howard.[9] Haas may also have in mind earlier pioneering work of influential ecologists such as Soviet geochemist Vladimir I. Vernadsky, who took a holistic approach to the subject. Vernadsky was one of the first to use the term biosphere and to stress biogeochemical cycles to understand the interrelationship between living and nonliving systems. Key elements of system ecology include its emphasis on the ecosystem concept and the flow of energy through them, the self-regulatory/functional properties of living systems, and the existence of negative feedback loops in nature. System ecology also conceives of nature as composed of "innumerable, partially overlapping systems" (Hagen 1992:131). This holistic branch of ecology did indeed influence international scientific study of the Earth's environment in the late 1960s through the International Biological Program (IBP), a large-scale transnational research program on ecosystems, although the fissure between evolutionary and systems ecology split scientific support for IBP (Hagen 1992).

That division highlights the second of two difficulties that arise with the focus on systems ecology. First, some attempts to apply its insights to social and political systems were largely discredited within the broader ecological community because they suggested an extreme version of social control that appeared anti-democratic. Howard Odum's (1971) semi-popular *Environment, Power, and Society* exemplified this trend. Early chapters on concepts of ecosystem ecology, systems modeling and the limits of industrial growth were highly regarded. However, Odum's application of his systems approach to politics and religion, and the simple control loops of his energy diagrams to explain voting, public opinion, taxes, and even revolutions and war, suggested the need for a coercive system of social control, not the democratic choice he claimed to promote (Hagen

1992:135; Bowler 1992:540). In general, systems ecology has been characterized by a strong management orientation that makes many environmentalists uncomfortable, especially when it comes to the global environment (Hagen 1992:138–140; Finger 1993:42; Hawkins 1993). Thus, one is hard pressed to find a consensus that the "ecological discipline . . . does not appear to reflect and reproduce patterns of inequality and dominance that may exist in domestic and international society more broadly" (Haas 1992a:43). The evidence thus seems to contradict the vision supposedly put forward by the scientific ecology epistemic community as presented in Haas's writing.

The second problem, noted above, is that stark divisions within ecology arose in the 1960s and 1970s. This split does not inspire confidence that consensus existed on ecological ideas that supposedly informed thinking in transnational scientific communities. As one prominent ecologist who has worked extensively on transnational environmental issues put it, "I know a lot of ecologists and if two of them share the same perspective, I will eat my hat."[10] The deepest and most acrimonious splits occurred just when ecology saw a huge increase in numbers of practitioners, money, and interest from governments, and high expectations from the public who looked to it for insights into environmental problems.

A complete history of the split is not necessary here, but a few aspects merit highlighting. The main split came from population ecologists who challenged the holistic approach of the Odums, opting instead for a more evolutionary stance where individualistic competition determines the structure of a region's ecology and the evolution of species themselves. A series of more technical debates about the use of mathematical modeling and the like also arose. The splits were not merely disciplinary debates, but affected how ecologists saw the application of their discipline to human behavior. As one historian of science puts it: "Many ecologists accept theoretical models that are quite explicitly opposed to the holistic perspective of the radical environmentalists. The development of scientific ecology cannot be equated with the rise of environmentalism, nor have the tensions between these two areas diminished in the modern world (Bowler 1992:536–537)." Or, as another author observes, "Ecosystem ecology provided the ideal perspective for examining critical environmental problems, but for many evolutionary ecologists this perspective lacked an acceptable intellectual foundation" (Hagen 1992:163).

Here the problem of values must be confronted head on. True, ecology as a system of thought does imply many of the values Haas identifies. For example, Tim Hayward (1994:31–32) lists three core values of ecological thought: live in harmony with nature (humans are a part of nature, not separate from it); overcome anthropocentric prejudice; and recognize intrinsic value in beings other than humans. But Hayward harbors deep skepticism about the ability to derive these values from ecological science. "My conclusion, then," he writes, "is that the normative regulation of human affairs cannot necessarily be derived from ecological insights, at least to the extent that they follow a logic which ecology is insufficiently equipped to illuminate." Rather, an ecological lens has been attached to values that come from elsewhere (1994:34). Unsurprisingly, Bowler (1992:536) finds that ecology has been used as frequently by the industries that some environmentalists criticize as by environmentalists themselves, each finding support within ecological science for their position—and controlled exploitation is as much a part of ecology as environmental protection. Hence, Haas's (1992a:43) ultimate claim linking scientific ecology to environmental preservation as "an absolute end" is incorrect. Given these divisions, Bowler's (1992:504) caution seems appropriate to keep in mind: "The very word 'ecological' has come to denote a concern for the environment. In science, however, 'ecology' is merely the discipline that studies the interactions between organisms and their environment. History shows that such studies can be undertaken within a variety of different value systems." My critique makes no judgment on the merit of ecological values *qua* values. It only questions the claim that they arise as the epistemic community literature suggests, based on a scientific and normative consensus among ecologists.

Finally, the following claim also has more to do with ecological thought than ecological science: "[Ecologists] do not view environmental policies in terms of opportunity costs, as some economists commonly do. Consequently, when involved in international environmental negotiations, they have encouraged behavior that is different from previous patterns of collective action" (Haas 1992a:43–44). If that were true, one would have to conclude from chapter 3 that the ecological epistemic community has been only marginally successful. While environmental protection is certainly present in the complex of norms governing international activity on the environment, the goal of economic growth for human needs, albeit a more environmentally friendly growth, remains paramount. Furthermore, environmental policies are indeed evaluated as much if not more by the costs and benefits of various actions than from the position of an environmental protection ethic as expressed above.

By contrasting environmental ecology to economics, Haas correctly points out the different values and assumptions of ecological thought and classical economic thought. The difference comes from environmental economics' basic starting point, consistent with classical economics, of valuing the environment to ensure the costs/benefits of human activity are properly considered. The basic underlying value is that "taking care of the environment is in humans' interest" (Haas 1994:102). But human interest is dependent on a prior set of values of what humans want and need, and hence comes from socioeconomic and historical circumstances. The environment is instrumental, thus important, and ought to be brought into economic models.

Ecological values, as portrayed by Haas and as commonly understood in philosophical literature, imply a much more radical position with implications for the way policies ought to be framed. That perspective gives value to the environment as an end in itself, not related to its use by humans. The contrast is important to assess critically and accurately the source of ideas of environmental governance. Otherwise, it would be tempting to equate just any concern with the environment with the influence or success of a scientific ecology epistemic community. Scientific ecology does not reflect just any concern with the environment, and stands in contrast to fundamental tenets of the norm-complex endorsed by the Brundtland Commission in 1987, and certainly of liberal environmentalism as articulated at UNCED in 1992. These findings contradict the existence of a coherent set of norms produced by policy-oriented ecological scientists with the authority of epistemic consensus.

In the absence of an ideal-type epistemic community, scientists may still influence policy. Below I identify scientific communities directly involved in negotiations or policy and conduct a process trace of their attempts to influence normative outcomes. Indeed, leading scientists at the cutting edge of global change research are generally not concerned about the philosophical or disciplinary perspective from which they come, but often simply go where the science takes them (William Clark and James Bruce, author's interviews). Admittedly, the science agenda for global environmental change as it developed in the 1980s has increasingly focused on the interaction of biological, chemical, and geophysical processes and their relationship to human activity. But for most scientists, this has little to do with a particular value orientation, ecological perspective, or conscious policy enterprise.[11] To simplify matters then, in the remainder of the chapter I focus specifically on the scientific communities most directly involved in global governance and ask first whether they represented a "scientific ecology" approach, and second what role, if any, they played in providing the foundation for international environmental norms.

The Scientific Community

Caution is warranted in determining what group of individuals might be properly called a scientific ecology epistemic community. For example, simply counting Maurice Strong, Jim MacNeill, Peter Thacher, Mustafa Tolba,[12] and others who took leadership roles in key UN environmental organizations and processes would unfairly stretch the use of the term. Even though some members of this group have science backgrounds (Tolba, for example) others do not. Moreover, some do not base their goals or values on cause-effect relationships in ecological science, even if they see the value of scientific research for environmental governance. This group, and its allies in domestic bureaucracies, might better be termed "knowledge brokers"—intermediaries between original researchers and policymakers or those involved in the policy process (Litfin 1994, 4, 37–40)—or more generically as policy entrepreneurs. Caution is also warranted when attaching analytic value to such labels, however, since "knowledge broker" still implies that the source of legitimacy for such a group rests on its use of scientific knowledge rather than the promotion of a particular set of values. While many key individuals used science to back their claims, it was not always primary in their attempts to influence others or shape the discourse around environmental governance. The epistemic community hypothesis, as I argue above, must therefore not merely focus on this group of like-minded influential individuals—indeed its value added is to provide a causal link between the authority of the knowledge claim and policy change. An epistemic community is a group with particular expertise who draw on that expertise to formulate not only technical advice or scientific research programs, but also goals and proposals that could potentially shape behavior of governments or other groups who partake in governance structures.

To locate possible members of an epistemic community, I briefly map the terrain of environmental science and international environmental policy. The range of environmental negotiations that require scientific inputs, and the number of actors and their interactions involved, make for a dense network of interactions and feedbacks that appears to make identification of an epistemic community daunting. For example, the Canadian Global Change Program (1996:91–94) identified more than 150 different research programs and organizations involved in global change activities, the majority of which involve scientific research. While global change research is currently the most prominent international environmental research program, one could make similar lists for specific concerns, ranging from big issues such as biodiversity, forestry, or ocean pollution to a myriad of specialized environmental or conservation issues on the international agenda.

Despite the plethora of actors and organizations, key umbrella institutions or groups closest to international environmental negotiations and policy processes can be identified. Among the influential nongovernmental scientific organizations are ICSU (International Council of Scientific Unions) and some of its prominent programs and member organizations such as SCOPE (Scientific Committee on Problems of the Environment) or IGBP (International Geosphere-Biosphere Programme); IUCN (World Conservation Union which includes governmental and nongovernmental representation); and IIASA (International Institute for Applied Systems Analysis)—an east-west think tank and product of the Cold War that does high-profile, interdisciplinary, and policy-relevant research on global problems. Some intergovernmental organizations have also played central roles, such as UNEP, UNESCO, and WMO (World Meteorological Organization). The latter organization became associated more closely with environmental problems as atmospheric issues gained ascendancy on the international agenda.

On any given issue, particular organizations (or scientists within those organizations) often assume leadership or coordinating roles, and organizations frequently collaborate to create specialized bodies to conduct research on specific topics. For example, ICSU and WMO collaborated to create the Global Atmospheric Research Programme and then the World Climate Research Programme. Similarly, UNEP and WMO jointly sponsor the Intergovernmental Panel on Climate Change (IPCC). Of all these organizations, UNEP and IIASA probably best represent the value-orientation of "scientific ecology," although neither has used the label. As I will show, however, these organizations, especially IIASA, were not necessarily the most successful in influencing the content of policy. In addition, both organizations have themselves adapted their research to provide a closer fit with liberal environmentalism.

With the backdrop of key organizations in mind, a process trace of the influence of scientific ideas and knowledge from these organizations (and individuals) can determine the strength of the epistemic communities hypothesis, even accepting that an ideal-type epistemic community did not exist.

THE INFLUENCE OF SCIENTISTS AND SCIENTIFIC ECOLOGY

The evidence shows that scientists played a remarkably minor role in the lead-up to and activities of the three major norm-articulating events examined. Scientists had more success in bringing particular problems to the attention of governments and some environmental scientists helped shape ideas about international environmental governance through institutions such as IUCN and UNEP. However, even within these primarily environmental organizations, environmental governance faced pressure to respond to development concerns and these organizations were unable to develop ideas that successfully bridged that tension in order to forge a focal point for normative consensus.

Science and Ecology in the Lead-up to Stockholm

A counterfactual example helps illustrate the limited influence of science and ecological ideas at Stockholm. Had UNESCO's 1968 Biosphere Conference in Paris set the pattern for environmental norms rather than Stockholm, the epistemic communities hypothesis would find strong support. As much as any other attempt to coordinate global environmental action since, the Intergovernmental Conference of Experts on a Scientific Basis for Rational Use and Conservation of the Biosphere endorsed an ecological approach to global environmental problems and reflected concerns associated with ecosystem or systems ecology. However, it did not set the trend. Instead, many of the environmental ideas that spurred global research and raised expectations among scientists fell into the background at Stockholm, except when it came to recommendations that dealt specifically with further research itself.

The Biosphere Conference did attempt to apply ecological ideas to the development process and bring environmental concerns to developing countries (Caldwell 1990:44–45; Adams 1990:30–36; McCormick 1989:88–90). Systems ecology, with its emphasis on management of ecosystems, provided a way to move beyond traditional concerns of conservation and endangered species, which did not interest the UN Economic and Social Council (Adams 1990:32–33). The conference successfully set in motion attempts by IUCN, UNESCO, and other conservation organizations to develop ecological principles for development. For UNESCO and IUCN, though, that focus came from the new development discourse among Third World countries, not from ecological ideas themselves. Still, these efforts came to fruition in statements such as the 1973 publication of IUCN's "Ecological Principles for Economic Development" (Adams 1990:32, 143–145). Many of these ideas can be traced to a scientific ecology epistemic community involved in the IBP, some members of which directly participated in the Biosphere Conference. The IBP had a major influence on thinking there, especially its call for the establishment of an interdisciplinary and international program of research on the rational use of natural resources to deal with global environmental problems.[13] It should also be noted that the values expressed were much more anthropocentric than those put forward in the early environmental movement, and therefore represented the management orientation of systems ecology more than simply an environmental preservation outlook.

The influence of the IBP, however, did not come from a consensus on a proper ecological approach *per se*, but on the support within the IBP for a research program that fit with global environmental concerns. These concerns drew especially from Odum's work and from supporters who pushed systems ecology, at least in part because they hoped it would raise the status of ecology to match more established disciplines such as molecular biology. Hence, although it had some other elements, "for all intents and purposes, [the IBP was] an international study of ecosystems" (Hagen 1992:170). The ecological community itself split in terms of support for the IBP, as many ecologists from a non-systems perspective were suspicious of the "big science" orientation of IBP projects and its focus on ecosystem research (as opposed to evolutionary ecology, for example). Many prominent ecologists felt that large-scale ecosystem studies were not the best part of ecology (Hagen 1992:172).

The most concrete outcome of the Biosphere Conference was UNESCO's Man and Biosphere Programme (MAB) launched in 1971. Mandated to study global relationships between human activity and the environment, MAB clearly took an ecosystem management approach and many projects it sponsored linked natural ecosystems and human use in single research projects. However, older nature preservation outlooks also remained, particularly in MAB's Project 8, which created "biosphere reserves." These reserves were often renamed areas already protected, did not really reflect ecological selection criteria, and did not succeed very well in creating protected spaces in developing countries (Adams 1990:33–36).

Scientific ecology did also influence the organization of scientific research and framing of conservation concerns (especially of animals and plants) at the international level. However, during this time period, most problems in practice were still treated as discrete problems of purely nature preservation or conservation, received a relatively low profile on the international agenda, and required relatively little coordination among states. The plans that came out of the Biosphere Conference such as MAB remained largely removed from international governance (that is, the realm of state or other actors rights and obligations, regulation, and so forth). Hence, in the long run, those plans had a limited influence on governing norms.

The Stockholm Conference

Scientists played a much greater role in the Biosphere Conference than at the UN Conference on the Human Environment at Stockholm. The latter reflected some ecological ideas, but mainly demonstrated the wide disjuncture between the concerns of scientists and of diplomats (Caldwell 1990:44–45). Scientists did not provide an ideational basis for the norms articulated at Stockholm and little consensual knowledge existed among the hard scientists who served as the main advisers to the secretariat or national delegations. Finally, at both the national and transnational level, scientists involved were more often reactive than proactive in conference preparations, with only a few exceptions.

Two isolated examples illustrate the haphazard way scientists did and did not influence events. The first is the fact, pointed out in chapter 2, that Maurice Strong replaced Swiss biologist Jean Moussard as secretary-general in 1971 after it became clear that Moussard did not possess the political savvy necessary to make the conference a success. Although the reasons for the replacement go beyond Moussard's scientific background, it indicated the difficulties that scientists would face in maintaining primacy when bringing environmental concerns into the mainstream of the multilateral agenda, or shaping it.

Second is the story of Svante Odén, the Swedish scientist who almost singlehandedly convinced his government of the need for an international conference to promote cooperation on acid rain. Despite little training in atmospheric science, he successfully used his own theory to convince politicians and the Swedish people that lakes and rivers in Sweden were becoming acidic partly as the result of sulfur from smokestacks in other countries, adequate responses to which would require international cooperation. Largely as a result of Odén's efforts, Sweden proposed the Stockholm conference.

The scientific story on acid rain is telling because it worked in a way quite different than an epistemic community hypothesis would suggest. The earliest related research dated back all the way to 1661 when investigators in England noted that industrial emissions affected plant and animal health and that England and France exchanged windborne pollutants. The term "acid rain" itself dates to 1872 when a British official who monitored pollution wrote about increasing acidity of rain as one got closer to industrial centers.[14] The subject remained dormant for nearly 100 years until research programs started to examine the effects of acidity in precipitation on various living organisms. While a number of independent researchers studied related phenomena that would be brought together in later research programs, the interest of policymakers was a result mainly of the efforts of Odén, a soil scientist and adviser to the Swedish government, who also happened to star in a popular television show. Odén was the first to publish a complete theory of acid rain, in which he linked air pollutants containing sulfur and nitrogen to increased acidity in rain that fell large distances away from their industrial sources. He also identified ecological consequences of acid rain that ranged from changes in the chemistry of lakes to accelerated damages to materials. Interestingly, Odén first published his theory of acid rain in 1967 in a prominent Stockholm newspaper, not a scientific journal (although the next year he published an article in *Ecology Committee Bulletin* that stimulated interest in the scientific community).

As a result of his work, Sweden sponsored a scientific study to try to determine the extent of the problem. It presented the study at the Stockholm conference, which then acted as a catalyst for expanded research programs in other countries. Odén's success stemmed from his personal access to policymakers, entrepreneurial efforts to spread word of his theory, and public popularity, rather than scientific consensus. This story, although it is but one example, lends little support to an epistemic communities hypothesis, which emphasizes the need for consensual scientific knowledge.[15] Instead, it illustrates the nonlinear relationship between scientific knowledge and political action on the problems such knowledge addresses.

In terms of the broader issues of governance, the normative compromises that arose at Stockholm came largely independently of scientific input. Ideas that framed the conference stemmed mostly from the entrepreneurial leadership of conference Secretary-General Maurice Strong who brought together strands of environmental and development discourse. Ultimately these compromises were political, but were facilitated by Strong's organization of two key meetings prior to the conference. These meetings helped forge a consensus among developing country economists that environmental protection could be accommodated, at least to some degree, while still making economic development a priority.

The first meeting could be considered as loosely comprising members of a scientific ecology community. The second, discussed in the next chapter, comprised members of the development community who had some interest in the environment (with some overlap). Strong himself ranks the latter meeting as being the more significant of the two in shaping the Stockholm agenda (author's interview). Even the first meeting did not involve ecologists as such, but people with related technical expertise who were sympathetic to the broad goals of ecology. The main link with ecology among the group was systems theory, which some ecologists had adopted. Three members of this meeting had been involved with the MIT team that worked on *Limits to Growth*, which used computer simulations grounded in systems theory.

Strong called the meeting shortly after his official appointment took effect in January 1971. The meeting of a small group of experts at MIT mainly concerned environmental issues (see chapter 2 for a list of participants), although Strong also wanted to link issues of environmental control with economic development in both industrial and developing countries. Strong (author's interview) said a major theme was to move thinking about the environment beyond a simple concern with pollution to a view that looked at industrial society and its effects on the environment in more systemic terms. As indicated in chapter 2, however, the meeting mainly emphasized environmental protection and a conservation ethic that emphasized sustainability for the sake of future generations.

One scientific meeting did directly address the relationship between environment and development—a SCOPE working party meeting in Canberra, Australia (Aug. 24–Sept. 4, 1971).[16] ICSU (1969:25–32) set up SCOPE in 1969 to report on the "problem of the human environment that humankind is altering." The report by a working party on basic environmental issues in developing countries, composed mostly of scientists from Africa, Asia, and Latin America, stressed the importance of what it called an "ecological approach" to environmental problems (SCOPE 1972). This approach would emphasize determining the "carrying capacity" of ecosystems, which depends both on plant and animal species, and the socioeconomic values of the society. It thus supports the incorporation of ecological concerns into development planning decisions and owed much to ideas present in the IBP and that came out of the Biosphere Conference.

Strong attended the meeting, but said it had less influence on the conference secretariat and the agenda of Stockholm than the two meetings already mentioned. As Strong saw it, "the Canberra meeting was more of a scientific meeting discussing the broader issues in scientific terms, not so much trying to write the Stockholm agenda" (author's interview). The Canberra meeting mainly aimed to bring Strong and the secretariat up to speed on the scientific issues. Strong had been looking for a source of scientific advice and SCOPE fit the bill. So, he paid for the meeting after discussions with SCOPE Secretary-General Tom Malone (an American scientist), and SCOPE became a key source of scientific advice for the conference (R.E. Munn, author's interview).

However, SCOPE did not directly address problems of environmental management—virtually no social scientists were involved, and, although SCOPE aimed in part to bring together individuals from different scientific unions, consensual knowledge was not a key component of the exercise. Scientists from different disciplines often talked past one another, although some learning did occur as ICSU and SCOPE scientists tried to understand the language of scientists involved in IBP, for example (R.E. Munn, author's interview). Indeed, some scientists came to SCOPE from the IBP or MAB, although turf wars between government and nongovernmental scientific programs were not uncommon.

Learning about development was more difficult, however, for many individuals within SCOPE. Most, including those in leadership roles, were (and are) hard scientists, uncomfortable with policy questions at all, and with a weak understanding of development concerns. Attempts at dialogue often went poorly because, as one participant put it, "the Third World and the hard scientists from Europe and North America hadn't talked to each other and they didn't really understand each other's problems at all" (R.E. Munn, author's interview). That lack of understanding might have contributed to the limited influence of SCOPE in conference outcomes. Hence, although the Stockholm Plan of Action echoed many SCOPE proposals on research and education, the ecological approach was buried under the concern of developing countries for economic growth.

In official preparations and at the conference itself, science played a role mostly in technical matters, and this did help promote interest in environmental concerns. For example, a number of analysts have noted that the preparation of country reports on the environment had a positive influence on government interest in the environment and helped build domestic infrastructure, particularly in developing countries (Engfeldt 1973). However, participating scientists did not constitute an epistemic community as such. Neither did the conditions identified by the hypothesis, such as scientific consensus, appear to be important for the impact they did have. In many cases, national scientists were brought into the process by governments, not vice versa. As one Canadian scientist put it:

> The scientists in Canada I know took it as a big pain in the neck when [government] organized all these task forces and committees and it just meant that people couldn't get on with what they wanted to do. So they were drafted into these things. In fact they used to send petitions around that all political meetings like that should be canceled for 10 years (R.E. Munn, author's interview).

Scientists also influenced discussions on the nature of environmental hazards and on a number of specific recommendations in the action plan.[17] Scientists were most successful with specific proposals on their own turf, such as with a SCOPE report that led to the creation of the Global Environmental Monitoring System (GEMS), a component of the Earthwatch system that was one of the most significant concrete results of Stockholm (SCOPE 1971). These activities likely facilitated the strengthening of transnational environmental science research networks and the names of prominent scientists who participated in activities related to Stockholm often appear in later ICSU or other transnational research programs or activities.

However, as the conference preparations got under way, diplomats gradually took over from scientists in the formulation of policies and framing of global environmental problems. According to Lynton Caldwell (1990: 62), a long-time observer of and participant in UN environmental diplomacy, "There was conflict between science advisers and foreign policy advisers at Stockholm reflecting differing assumptions regarding the bases and priorities of international cooperation. These differences . . . were never wholly overcome."

The most active scientists thought Stockholm should promote and institutionalize a planetary conservation ethic that would transcend national allegiances. Such a position supported the creation of mandatory rules in international law that could be enforceable directly on individuals and transnational corporations (Caldwell 1990:42). However, the realities of international law and politics militated against this approach because it conflicted with sovereignty and rules that largely left the regulation of individuals and corporations to national laws, and governments were unwilling to relinquish freedom of action and control over domestic development. Scientists were particularly disappointed by the unwillingness of delegates to take a holistic approach. Instead, they displayed an overriding concern with maintaining sovereignty and what scientists perceived as short-term interests.

Perhaps that in part explains why IUCN downplayed the importance of the conference, even though it actively participated in the preparatory committees. The significance of the new perception of the position of developing countries in relation to environmental concerns was lost on IUCN, which in its annual review listed with enthusiasm the achievements of the Second World Conference on National Parks and adoption of the World Heritage Convention before its lukewarm appraisal of Stockholm.[18]

As a group, scientists felt disappointed with what they had achieved at Stockholm. The then executive director of the U.S. environmental committee of the National Academies of Sciences and Engineering summed up scientists' impact this way:

> Despite this promising start [in preparatory activity] science never emerged in Stockholm in the role of a recognized, let alone equal, partner in a common enterprise. Although there had probably never been an intergovernmental conference in which science was accorded a larger and more direct share in the preparation as well as the outcome, this was hardly apparent during the Stockholm proceedings (Kellermann 1973:485).

Scientific Ecology and International Environmental Governance: 1972–1987

The creation of UNEP and Maurice Strong's leadership of it meant the ideas and scientific proposals that came out of Stockholm would immediately find a comfortable home within the United Nations. For example, Strong immediately provided money for GEMS proposed by SCOPE, a program UNEP continues to fund.[19] Strong's leadership also assured that the overall norm-complex of Stockholm, which included the uneasy mix of development concerns and state sovereignty in addition to environmental protection, would also guide scientific research and the framing of environmental issues.

Perhaps the high point in the influence of scientific ecology on this agenda came with the attempt to entrench the idea of "ecodevelopment" within UNEP. This attempted "marriage" of ecology and economy did not come from the science of ecology, so it could not really be said to have arisen from an epistemic community in its purest form, but it did attempt to stay close to the ecological values of systems ecology. While ecological ideas clearly influenced ecodevelopment thinking, ecodevelopment did not translate into great success in shaping governing norms because it sold poorly in developing countries and could not forge a broad-based normative consensus.

Note also that Maurice Strong, who coined the term while head of UNEP, said the single biggest influence on his thinking about ecodevelopment was Ignacy Sachs, a French development economist, whom Strong credits with giving the concept intellectual content.[20] I do not mean to suggest that UNEP did not try to incorporate ecological principles in its programs. For example, its regional seas program certainly did (Haas 1990), but the focus on ecological principles tells only part of the story. The main orientation of efforts to shape how the international community would respond to environmental threats stemmed from the attempt to mix ecology and development, which did not come mainly from a scientific ecology community.

Furthermore, UNEP as an organization had only limited success selling ecodevelopment in the developing world, because it was perceived as laying too much emphasis on the ecological side of the agenda.[21] UNEP could not forge the necessary North-South consensus or alliances among key state or institutional actors to create a normative focal point for environmental governance.

The World Conservation Strategy attempted to use the term "sustainable development" to overcome these difficulties, but had only a small impact on overall governance structures, further demonstrating the limitations under which policy-oriented scientists and environmentalists worked. The final strategy focused on conservation of living resources, although some sections did mirror ecodevelopment thinking.[22] As chapter 2 indicated, at bottom WCS argued that development, since it alters the biosphere, must take conservation into account to be sustainable. The solution, then, is to give conservation a higher priority.

The WCS undoubtedly contained some ecological ideas, though it did not fully overcome its conservation orientation. That the final strategy focused as much on development as it did resulted from consultations and negotiation on a second draft among delegates from developing countries at the IUCN 1978 General Assembly in Ashkhabad, USSR. The revised draft then went through consultations with UNEP, WWF (its co-sponsors), FAO, and UNESCO (McCormick 1989:162–170). Despite the compromises and influences from these various constituencies, it never overcame its lack of attention to the main concerns of developing country governments, nor did it take into account the essentially political nature of development nor the social production of nature (Adams 1990:50–51). The problem stemmed in part from the difficulty IUCN leadership had with its own constituency in getting development on the table. As a result, rather than recasting the debate, it tacked development concerns onto the traditional conservation agenda. The greater the degree to which ecological ideas dominated that seemed insensitive to the above concerns, the more WCS seemed to reflect old environment and conservation thinking of the 1970s to many analysts (Adams 1990:50–51; McCormick 1989:165). The perceived ecological focus decreased the likelihood that WCS would make a long-term impact. Ecological ideas had to be substantially recast before they could provide a pillar for international environmental governance.

Science, Ozone, and Global Ecology

While environmental- and conservation-oriented international organizations struggled to find the proper meshing of environment and development concerns, some transnational scientific communities did achieve major successes in raising the profile of global environmentalism. In particular, the rise in prominence of atmospheric science research programs—although many were U.S.-based and not transnational until much later—helped raise the alarm over the threat of ozone depletion and climate change.[23] UNEP, WMO, and ad hoc intergovernmental bodies set up to study and report on ozone depletion also played important roles in promoting international action and raising the profile of global atmospheric science.

A number of analysts point to the ozone debate and eventual action as a defining moment in the shift to truly global environmental concerns and have commented on the advent of global ecology starting around this period (Sachs 1992, 1993; Hawkins 1993). At least one author (Finger 1993) attributes this trend in part to the influence of atmospheric chemists, geologists, oceanographers, and climatologists who helped define a new type of ecology called "global ecology." Whereas atmospheric scientists, climatologists, and other scientists involved in global environmental issues gained higher profiles and larger research budgets as attention turned to global environmental concerns, the question still remains whether they actually shaped how such problems would be addressed. The evidence shows that even in most-likely cases—such as international action to prevent ozone depletion—an epistemic community hypothesis achieves only mixed success.

Most observers consider the Vienna Convention and subsequent Montreal Protocol to combat the depletion of stratospheric ozone the result of the most successful diplomacy to date to solve a highly technical international environmental problem. The case is a most-likely candidate for an epistemic community explanation since knowledge about the problem, its sources, and detection depended wholly on advanced science. Yet scientific consensus did not appear to be a major factor in getting political action; scientists themselves did not seem to push for a clear set of ecological values; and scientists virtually ignored the North-South dimension of the problem which became crucial to the long-term success of the treaty and to the broader normative structure of environmental governance to which the ozone issue contributed. For example, scientists were not important actors in introducing provisions to allow longer phase-in times for developing countries or in the process that led to the 1990 London Amendments to the Montreal Protocol, which created the Multilateral Ozone Fund to help developing countries phase out ozone-depleting chemicals.

Only rarely does a community of experts first establish consensus and then mobilize public policy as the epistemic communities hypothesis suggest. More typically, research programs on an issue will go on independently until a handful or even one entrepreneurial piece of research (or individual researcher) manages to start putting theories together, garners media attention, or catches the ears of policymakers. The common pattern is then that the initial flood of public attention produces more money for research and only then may a scientific consensus begin to build (Kowalok 1993).

In this regard, the story of getting the attention of the U.S. government on ozone is not dissimilar to the acid rain story mentioned earlier, with the exception of its relative quickness—scientific research about human-induced ozone depletion only started around 1970.[24] At that time, large-scale research concentrated on the potential threat of supersonic transports (SSTs). Some scientists feared that the release of nitrogen oxides and water vapor might deplete ozone in the stratosphere, the protective layer of atmosphere 10–50 kilometers above the Earth's surface where most ozone is found. This ozone layer protects the Earth's surface from ultraviolet radiation. These concerns led the U.S. Senate to terminate funding for a planned fleet of 500 SSTs (a Boeing project), despite a prominent study that concluded that ozone depletion from SSTs would be insignificant.[25]

Large-scale transnational research also occurred, but concern quickly shifted to the ozone depleting potential of chlorine as SST programs were cut or scaled back. The new concern came from research by NASA scientists on possible effects of the space shuttle's rocket boosters, which would directly inject chlorine into the stratosphere. However, for political reasons, NASA scientists downplayed the threat from the shuttle and instead emphasized the threat from volcanoes, which left some scientists puzzled as to the concern since they did not see the danger for lack of a major source. Then, in 1974, F. Sherwood Rowland and Mario J. Molina's famous article in *Nature* identified chlorofluorocarbons (CFCs) as plentiful sources of atmospheric chlorine. Thus, a source of concern had been found, and an entirely synthetic one at that.

What got the ear of government, however, was not activity by scientists *per se*, but a front-page article in *The New York Times* following a meeting of the American Chemical Society where Molina and Rowland presented their data. Their paper warned that predicted ozone loss could lead to significant rises in the incidence of skin cancer and possible crop loss. The *Times* article was followed by extensive national television coverage that prompted more letters to Congress than any other issue since the start of the Vietnam War (Brodeur 1986:70). After the public outcry, the U.S. government funded further large-scale and coordinated research efforts. What happened next in terms of policy responses is well documented elsewhere. For the purpose here, the significant pattern in U.S. regulation (starting with bans on the use of CFCs in nonessential aerosols) is that it consistently outpaced scientific consensus on the extent of the problem until the late 1980s. Only then did the transnational scientific consensus on the causes of ozone depletion became more solidified.

However, it should also be noted that some government scientists did promote a precautionary stance despite uncertainty, at odds with the chemical industry. For example, Russell W. Peterson, chair of the President's Council on Environmental Quality in 1976, and formerly a chemist with DuPont for 26 years, argued that despite equivocal science, "chemicals are not innocent until proven guilty." He then argued for restrictions on CFCs (Brodeur 1986:74). However, the U.S. government acceptance of a precautionary stance waxed and waned depending on the leadership within key government bodies such as the EPA (Brodeur 1986; Litfin 1994:61–73).

At the international level, the ozone issue represented a defining moment in global environmental cooperation and seemed to demonstrate the ability of science to influence global governance more generally. The interaction of science and policy in reaching agreement on the Montreal Protocol has also been well documented elsewhere (Litfin 1994; Rowlands 1995a; Haas 1992b; Parson 1993; Benedick 1991). Here, I will only comment on those findings as they relate to the question of whether epistemic consensus was the main driver of policy in terms of both getting action and the type of action received. I will also discuss the broader implications of the community's activities for governing norms more generally.

No author questions that science played an important role in creating an international ozone agreement since expertise was a necessary condition for identifying a decrease in concentrations of invisible stratospheric ozone. In this sense, the epistemic community literature is certainly correct that knowledge of environmental problems is based on a scientific rationality view of the world. The ozone layer "is only available as an object of knowledge because of our scientific culture" (Yearley 1992). However, the influence of scientists on policy did not work in the way an epistemic community explanation would suggest. Neither did an epistemic community influence general governing norms, with one important exception: the Precautionary Principle. Even in the latter instance, the principle did not stem from cause-effect relationships inherent in the study of ozone depletion.[26]

Rowlands, for example, found some correlation between the level of consensual scientific knowledge on ozone depletion and international cooperation, but also noted a major anomaly in that the major international study credited with producing transnational consensus—the Ozone Trends Panel Report—did not appear until *after* agreement on the Montreal Protocol. The epistemic communities hypothesis suggests the former is a precondition for the latter (Rowlands 1995a:89). Parson (1993:60) is more blunt: "it was not science, but bargaining, that determined the decisions adopted in Montreal. The 50% cut that was agreed to had no particular scientific prominence. Indeed, the distribution of expert opinion at the time seemed strongly divided." Litfin (1994), in the most detailed test of an epistemic communities hypothesis on ozone to date, similarly argues that only after the treaty was signed did scientific consensus emerge, especially on the causes of the ozone "hole" over the Antarctic, which became known in May 1985. Litfin is convinced that the "hole" played a major role in framing subsequent negotiations, even though delegates agreed not to consider the evidence or its cause (1994:96–102). Prior to that point, scientific data had been open to wide interpretations in terms of policy implications.

However, scientists in the U.S. EPA at a June UNEP/EPA conference in Leesburg, Virginia 1986 successfully moved the "terms of the dominant discourse toward precautionary action." Although they did not promote any particular policy option, they moved the discourse by emphasizing the long atmospheric lifetimes of CFCs and long-term modeled predictions (Litfin 1994:91–92). Ironically, the discovery of the ozone hole, which atmospheric models had not predicted, rather than undermining scientists' legitimacy, strengthened the precautionary discourse because the models could no longer be relied upon. That uncertainty changed the framing of the scientific issue itself from one of ozone depletion to one of increasing concentrations of chlorine (which could be calculated without the use of atmospheric models, based on production data and atmospheric lifetime).

Once framed in terms of chlorine-loading, a phase-out no longer seemed like a drastic proposal.

Litfin thus concludes that the scientists were not the driving force. Rather, the framing of the issue by "knowledge brokers" such as EPA administrator Lee Thomas (a career bureaucrat trained in psychology) played more of a role. Thomas felt the risk and uncertainty of the ozone problem warranted a precautionary approach, a view driven by his orientation to risk, not science. Thomas successfully pushed this view over that of other officials with different orientations to risk, such as White House science adviser William Graham:

> Graham looked at it from a purely scientific perspective, whereas I looked at it from more of a policy perspective. Where there was uncertainty, he thought we needed more research, and I thought we needed to be cautious. We just looked at the same thing and came to two different conclusions.[27]

Hence, even in the case of the precautionary approach, which did find its way into broader international governance, the values could not be directly derived from the science itself. In addition, NGOs probably played an equally prominent role in promoting the precautionary approach to ozone (and more broadly). For instance, groups such as the U.S. Natural Resources Defense Council threw their support behind the chlorine-loading approach because it would lead to complete phase-outs.[28]

Nonetheless, prominent scientists did promote the precautionary principle here and elsewhere and should be credited in large part with giving legitimacy to this norm at UNCED. A prominent example of a highly respected scientist who easily crosses over to policymaking is Robert Watson, a NASA scientist who chaired the 1986 WMO/NASA ozone assessment and replaced Bert Bolin to head the Intergovernmental Panel on Climate Change in 1997. Speaking about the implications for policy of ozone depletion, Watson emphasized that his main policy concern was the long time frame to reverse effects, "which means you could not wait for cause and effect to be fully established."[29] He added that the same concern applies to the issues of biodiversity and climate change. However, these considerations clearly go beyond science, as they concern uncertainty itself and the implications of that uncertainty for economic conditions, security, and health. As Watson put it, "In all of these cases [ozone, biodiversity, climate change] it is an issue of how you make a policy judgement with significant scientific uncertainty. You simply can't wait for all the information to come in."[30] Even though this orientation to risk did not stem from scientific findings, it is fair to say that precautionary concerns did gain prominence as the result of the community of scientists involved in atmospheric issues; hence in this one respect there was an epistemic community that seemed to play a necessary leadership role.

Ironically, the success of an epistemic community on this norm came in a case where a lack of consensus on standards of validity is precisely the issue at stake: the view of science that underlies the precautionary principle remains highly controversial within the community of environmental and ecological scientists. While it is fair to say that the principle gained the status of a norm, as defined earlier, within environmental governance, the conflict within communities of environmental scientists makes it at least questionable whether the acceptance of the norm stems from consensus within an epistemic community rather than a combination of promotion by a wide variety of actors and organizations concerned about environmental impacts.

Precaution rests on a scientific basis only if one questions the adequacy of traditional statistical standards of significance to capture cumulative or anomalous, though potentially harmful, effects in the field (see endnote 26). Hull (1999) suggests, though, that the group of scientists who take this position is still in the minority. In a study that documents these two views of environmental science, she notes that experimental methods deriving from a formal philosophy of science still dominates the discipline, and practitioners from this perspective often consider non-laboratory methods "unscientific." These environmental scientists would be less sympathetic to the precautionary principle as a guide to policy because they believe that chemical causes of toxicity or harm can only be determined through laboratory tests of cause-effect relationships that adhere to the strict scientific standards required in formal positivist philosophies of science. Conversely, scientists who engage in epidemiological or "ecoepidemiological" research are more skeptical of laboratory results, noting that requirements such as linearity, replicability, statistical significance (with a focus on avoiding type 1 error, or false positives), and specificity are inappropriate for complex interactions of organisms and their environments in the field, where it matters most. In the field, relationships between chemicals (for example) and their consequences on organisms are just as likely to be nonlinear (with threshold effects, for example), nonreplicable (to the degree that ecosystems are not easily replicable under controlled circumstances), to lack specificity (individual causes may not be directly linked to precise effects), or to be missed by traditional statistical tests which may not capture rare or subtle though real effects, especially on individual organisms, owing to the variability and complex interactions of biological organisms and their environment.

The latter view clearly fits more with a scientific ecology ethos as defined by Haas, but current policy debates suggest that "science" as a legitimizer of norms is being used to support both positions. For example, the current debate about genetically modified organisms that played out in the negotiation of the Biosafety Protocol, and is likely to remain contentious within the WTO and other trade agreements, focused heavily on whether the Precautionary Principle or "scientific" proof should be the standard for limiting trade. Currently the WTO, under GATT article XX on exceptions for health and safety concerns, demands a standard of "sufficient scientific evidence" which appears to rest on a formal, deductive, and physicalist view of science where standards of proof of cause-effect relationships can rest only on laboratory experiments, although the WTO Appellate Body decision on the Beef Hormone dispute between the EU and Canada and the United States, discussed further in the concluding chapter, uneasily bridges the two positions.

As for ecological values more broadly, no one I interviewed in leadership positions in global environmental research or policy indicated that a set of ecological values *per se* was widely accepted in transnational scientific communities, and those that did hold such values may have also valued other goals, such as economic growth. For example, in Watson's view, the ozone issue is important because it potentially affects the quality of life, but his policy orientation does not stem from science itself. In an interview in 1996, he responded this way to a question about what values motivated him on the ozone issue:

> What we need to strive for is a high quality of life and within that it means good economic performance and a clean environment—I believe, just as our Vice President [Al Gore] believes, you can have good economic growth and environmental protection. You have to handle them very carefully and together and one cannot be the afterthought of the other.[31]

Watson expressed this view well after the signing of the Montreal Protocol and the Brundtland and Rio processes. If anything, it suggests a learning process might have occurred among scientists as a result of those events.

Science and the Brundtland Commission

Whereas atmospheric science contributed somewhat to raising concerns about global environmental issues, the Brundtland Commission told the world how to think about them. The Brundtland Commission process, however, only reinforced the limited influence of the ecological scientific community on governance. Had the project of preparing a report on international environmental action to the year 2000 not been taken out of the hands of UNEP, science might have played a more central role, as it appeared to in UNEP's parallel report (discussed in chapter 2).

As for the role of scientists in the WCED process itself, analyses and interviews suggest that scientists were neither the initiators nor the driving force behind most of the recommendations, perhaps due to a mandate that focused more on values than physical realities. In particular, the conclusion that economic growth is needed and will not damage the environment did not come from scientists nor was it based on a consensus on cause-effect relationships (Timberlake 1989).

But perhaps the most telling anomaly for an argument based on the influence of a scientific ecology epistemic community is the lack of influence of IIASA on global norms. This lack of influence provides another powerful counterfactual example of the weakness of the hypothesis in this case (that is, its positive influence on outcomes would have provided strong evidence for the hypothesis). The Austrian-based institute sponsored the epitome of Haas's version of a scientific ecology research program and contained nearly the ideal of a high-level transnational community of scientific experts. In 1982, IIASA undertook a well-developed research program called "Ecologically Sustainable Development of the Biosphere" which brought together historians, engineers, geographers, environmental scientists, economists, management experts, and policy people, to examine how to manage the interaction between development and environment.[32] The group involved was truly transnational owing to IIASA's stature as one of the only places where natural and social scientists from east and west interacted; it conducted policy relevant and cross-disciplinary work; and William Clark, who headed the program, saw IIASA as perfectly suited to take advantage of burgeoning research from many sources and countries around global environmental problems. Clark describes the potential he saw for the IIASA this way:

> The IGBP [now at the center of global change research] . . . was beginning to take form at that time and there were other ventures interna-

> tionally. It just seemed to me that there was a niche . . . for a couple of years in which IIASA might really be able to be a forum in which the relevant natural and social sciences and the relevant countries, at least of the developed world, could jointly participate in trying to sort out what the research agenda underlying this notion of what we today see as sustainable development might be (author's interview).

Clark, incidentally, is about as close an approximation to Haas's ideal-type "scientific ecologist" as one could get. He had training in ecology, his research experience ranged from detailed studies of rural development to regional ecosystems, and he had participated in a large-scale study with natural scientists on carbon dioxide, energy, and climate change. IIASA attracted him because of its systems approach. That meshed with his belief that the issue of climate change, and global change research more broadly, was "so coupled to other issues of human development and other environmental issues that the emerging notion of what is now called sustainable development" could be developed there. He felt IIASA's strengths made it an ideal setting "to do global environmental issues in an integrated way" (author's interview).

However, Clark objected to the word "ecology" as a description of the sustainable development of the biosphere program at IIASA, insisting that the word "ecologically" in the title of the program was a "bureaucratically imposed modifier" by IIASA management who represented sponsoring countries, not the work of the scholars involved. IIASA management feared the program branched too far into social issues, which they felt was "inappropriate." Clark did not share this concern since his entire project was meant to explore the "lovely ambiguity of the phrase sustainable development," which meant that the interaction of society and the environment could be explored from both the natural and social sciences. He did not see it as a project centered in the discipline of ecology, except in the broadest sense of looking at the problem of sustainable development in the context of an integrated social-environmental system. The specific influence of ecology came only from notions he borrowed from his thesis adviser, ecologist C.S. Holling, such as "surprise," "bounded stability," or "threshold effects." Such concepts are also linked to chaos theory, such as in its discussion of how ideas around small events can lead to large, unexpected changes.[33]

Given this focus, the IIASA program might seem a logical place to look for a broad scientific basis on which to frame the Brundtland Commission report. Similarly, one would expect that the team at IIASA might have sought out the Brundtland Commission if it acted in the way expected of an epistemic community representing a global change research program. However, neither occurred. Asked if IIASA had much interaction with the Brundtland Commission, Clark responded:

> I say with embarrassment, no, there was very little: We vaguely knew that the Brundtland Commission was working away. . . . In ways that it's hard for me to understand now how we could have been so unconnected. Not only was I not particularly aware of the details of what was going on, most of the scholars I engaged in the project weren't . . . at that time we were obviously invisible to them—hardly surprising—and they were pretty invisible to us (author's interview).

Despite the direct relevance of this research, the Brundtland Commission only spent one day consulting with the project and at least some people at IIASA had difficulty relating to the concept of sustainable development as framed by the Commission. In the words of Ted Munn, one of the project's lead researchers and one of the few natural scientists there who had been involved with UN processes previously and after: "We thought that as a North-South exercise it [WCED] didn't have much to do with us, I guess. And the Brundtland report was not a prescription for action, it was rather a mindset or a dream of what might be without telling anybody how to get there. So it didn't bother me or anybody I know at all. It sort of operated on a different plane" (author's interview).

Clark said that at the time there was very little communication between the community of researchers involved in burgeoning global change research programs and the more political activities that emerged from the UN system. Those who worked on the UN-sponsored Brundtland Commission were "a very different line of people" than the scientists who tried to move environmental research toward an integrated global change research program: "We were just different people. And I think it has been in large part due to the Brundtland group that those linkages are stronger today" (author's interview). Thus, Brundtland was the catalyst for bringing the work of scientists from organizations such as IIASA toward concerns of global environmental governance, not vice-versa as the epistemic community hypothesis argues.

The beginning of the major turn in environmental norms toward a more growth-oriented, Keynesian-style global management norm-complex seemed to occur largely independently of scientific ecology research programs. That is not to say that scientists did not provide technical information, nor that science or ecological ideas did not influence various recommendations of the Brundtland Commission. However, a unified scientific community did not appear to play a primary role in this turn of environmental governance and the weight of the Brundtland report did not draw its content, legitimacy, or underlying value-orientation from the findings of a scientific ecology epistemic community.

Finally, another development following the Brundtland Commission report suggests that it will be increasingly difficult for epistemic communities to fulfill the requirement of relative independence from government interests. After 1987 and the rebirth of environmentalism on the international agenda, this time as a truly global concern, governments appeared to make a more concerted effort to rein science in rather than allow "free wheeling" scientists to dictate the environmental agenda (James Bruce, author's interview). In looking at the lead-up to UNCED, it appears that as global environmental research picked up steam, the fitting of such research into palatable forms of global governance became a prime concern of major states from both the developed and developing worlds. One finds systematic attempts—most notably in climate change, but in other issue areas as well—to retake control of transnational research endeavors and the processes by which these projects feed into international policy formation.

Science and UNCED

Transnational environmental research by 1992 was better funded and better organized than in the years before Stockholm, yet a remarkably similar pattern of limited scientific influence characterized UNCED. Scientists did of course play some role. Transnational and international scientific organizations fed into many aspects of UNCED preparations and more than 160 countries submitted reports on the state of their environments, although experts other than scientists contributed to such reports on environment and development in each country or territory.[34] Individual scientists also played a role in UNCED, some serving as members of delegations and as participants in preparations of conference documents and agreements. Nonetheless, like Stockholm, formal scientific community involvement in UNCED was relatively small as professional diplomats and administrators dominated the UN negotiation process (Marton-Lefèvre 1994; Haas, Levy, and Parson 1992:33 fn. 11). Scientific knowledge was requested and supplied, but the process shaped how science would be used, not vice-versa. As the executive director of ICSU put it, Agenda 21, the blueprint for environmental action into the next century, "can be viewed as an instrument through which scientific knowledge was transformed into a uniquely UN frame of reference" (Marton-Lefèvre 1994:171).

The most direct input of the scientific community came from ICSU, who Maurice Strong invited to serve as the conference's official scientific adviser. Its main recommendations came from a November 1991 conference on An Agenda of Science for Environment and Development in the 21st Century in Vienna (ASCEND 21).[35] Although participants read like a who's who of transnational environmental science, the report came too late to have much influence in UNCED preparations, which were already well underway. In fact, ASCEND 21 took place two months *after* the original deadline for chapter 35 of Agenda 21 on "Science and Sustainable Development." The deadline was pushed back so the authors could revise the chapter based on ASCEND's recommendations. Other outcomes related to science included Agenda 21's chapter 31 on the "Science and Technology Community" and the Rio Declaration's Principle 9, which promotes cooperation to increase national scientific capacities and exchanges of scientific and technical knowledge "for sustainable development."

The ASCEND 21 report did express some policy positions, but mainly focused on recommendations related to implementation of research and observation programs; strengthening of interdisciplinary research and communication among the natural, engineering, health, and social sciences; building links between science and development agencies; and building scientific capacity in North and South (Dooge et al. 1992:5–11). Development concerns were not well integrated into the overall report and were largely ignored in the invited papers. One participant suggested that the problem in part stemmed from the difficulty most hard scientists at the conference had understanding how to relate development concerns to their work. Twenty years after Stockholm, ICSU still had little interaction with social scientists.[36] Thus, ASCEND 21 hardly represented a consensus on science and development except in the loosest sense of the word. The document suggests few participants thought deeply about this relationship.

Although ASCEND 21 recommendations stuck closely to the promotion of scientific activity, some policy-relevant themes stood out such as a focus on population and carrying capacity, consumption patterns, and a strong endorsement of the Precautionary Principle. The latter was seen as the proper response to the complexity of the Earth's systems and the uncertainty of the effects of human disturbances (Dooge et al. 1992:6–8). The issue of Northern consumption did receive attention during negotiations and developing countries successfully negotiated for the issue to be included in several chapters of Agenda 21. However, the final wording remained vague, with developed countries agreeing only to "take the lead in achieving sustainable consumption patterns" but not agreeing to specific proposals, targets, or mechanisms (energy efficiency guidelines, for example) to achieve the goal. Population had not been included in the original mandate of Rio and only made it onto the agenda at the behest of industrialized countries who wanted it paired with consumption issues.

In the end, neither North nor South seriously negotiated on bargains over population and consumption patterns, and the United States especially resisted any discussions on consumption. The debate that did occur (mostly on consumption patterns) was politically charged and produced little concrete action that drew from ASCEND's work. ASCEND could hardly be credited with having influenced this debate since the G-77 had long used the strategy of shifting international environmental negotiations from a focus on population growth, which the North emphasized, to a focus on consumption patterns in the North. The South had also explicitly made this strategy a part of its negotiating position for Rio from the start.[37] ASCENDS's support of the Precautionary Principle had much greater impact as the principle made major inroads, although it had already become prominent in the ozone and climate change negotiations.

More generally, UNCED experienced the same uneasy relationship between science and policy that pervaded Stockholm. The scientific community appeared either too unprepared, unwilling, or unable to communicate effectively within the diplomatic setting of the conference. As a result, its message often got watered down or else became one of a myriad of nongovernmental voices with no particular special status. Susskind (1994:66–81), for example, argues that these difficulties, among others, are typical of the impact of scientists on international environmental negotiations in most cases.

As in the case of Brundtland, the specific example of IIASA also deserves special mention since Strong had hoped it would play a major role (1992:22). But, according to Strong, that influence never materialized:

> IIASA had an opportunity to play a special part and they did not do it. They were a disappointment, to be perfectly honest. I've always been very convinced of the systemic nature of these issues and was trying to design in UNCED a framework in which the systemic nature of those issues could be clearly seen by policymakers, and also the points of effective intervention identified. . . . I thought that we had a great opportunity to demonstrate this. . . . They did not rise to the opportunity the way I'd hoped (author's interview).

This lack of influence by one of the few candidates for membership in a scientific ecology epistemic community suggests that such a community, if it existed at all, had a limited substantive impact at UNCED, and little influence on governance norms or the framing of issues more generally.

The discussion so far should not suggest that scientific evidence was unimportant to UNCED, but rather that it was not the driving force behind norm creation or the initiator of action. The broad shape of the norm-complex articulated in the Rio Declaration and Agenda 21 does not reflect the primacy of "scientific ecology" as the basis for agreement, and the Declaration in particular appears less concerned with environmental protection or ecological concerns than even the Stockholm Declaration. Agenda 21, while it incorporates insights and linkages identified by scientists, also reflects the environment and development mix of liberal environmentalism that did not come primarily from science. As at Stockholm, scientists achieved the most success on their own turf. However, much of the science used at UNCED came from governments and the secretariat-commissioned reports, not from the independent influence of an epistemic community. Scientists were not particularly active outside of those limited roles.

Science and Climate Change

To be fair, one cannot measure the influence of an epistemic community by looking at conference preparations alone. Indeed, the power of epistemic communities can occur in their ability to frame the issues for negotiations rather than changing negotiation outcomes *per se* (although the latter position is often taken in empirical tests of the literature, where the community is credited with forming specific focal points for agreement). Hence, below I examine in some detail the influence of the scientific community in the lead-up and follow-through of international action on climate change that led to the signing at UNCED of the Framework Convention on Climate Change (FCCC). Space limitations prevent me from undertaking similar detailed analyses for biodiversity and forests agreements.

Admittedly, the biodiversity case does provide some support for an epistemic communities hypothesis since the long-standing activities of scientists affiliated with organizations such as the IUCN and later UNEP did play an important role in promoting the biodiversity concept. Scientists also helped to define issues and propel bargaining to produce international action around the concept.[38] However, even on biodiversity, the area closest to traditional conservationist concerns, ideas that shaped the overall agreement included the range of norms discussed in chapter 3 that had little to do with the relevant science. Major debates in negotiations revolved around intellectual property rights and sovereign control—debates that shaped the core normative basis of the treaty, making it a good fit with the broad normative contours of liberal environmentalism.

Climate change is an appropriate focus for a number of reasons. First, most analysts agree that climate change, although not even officially a part of the UNCED process, became a main galvanizing issue for action, and mirrored many core debates (Imber 1994; Boehmer-Christiansen 1994c: 181). It was also a central outcome of the UNCED process. As such, many of the norms found in FCCC mirror those found in other UNCED documents negotiated at the same time. Second, climate change, especially as part of the broader agenda of global change, epitomizes a problem appropriately framed in ecological terms. Its very definition implies complexity, interaction of various environmental media (land, sea, and air and the chemical, physical, and biological cycles that link them) and their relationship to human activity (anthropogenic change), and for solutions to take account of those interactions. It also represents, perhaps more than any other problem except ozone depletion, a truly global issue. In fact, climate change is often used interchangeably in practice with the umbrella research program of "global change," which by the 1990s had become the most prominent global environmental research program. States, international organizations, and non-state actors have devoted an enormous amount of effort and resources to this issue, which, owing to its potential implications and a scope that encompasses a wide variety of other environmental and non-environmental concerns, has become the dominant environmental issue on the international agenda. Finally, although consensus appeared uncertain at times, the scientific community around climate change was well organized and mobilized in terms of promoting international action. Here I focus not only on how science worked in the negotiation and treaty process, but also on how much it influenced the form the treaty finally took, hence how it helped shape global environmental governance more broadly. The story is told more or less historically to show how scientific ideas about climate change made their way onto the international agenda.

The climate change case does show that scientists can affect international action on a highly technical issue. However, typical of other examples and the broad evolution of governance explored above, that influence did not work in the way the epistemic communities hypothesis suggests. There was no consensus on values of "scientific ecology," early success in getting international attention did not translate into control over how the problem would (or would not) be addressed, and science eventually got molded by the political process and normative structure as much as or more than it molded them.

Fears of human-induced climate change are nothing new. Since the mid-eighteenth century, scientists have arguably used this threat as one of the few effective instruments to persuade governments of the seriousness of environmental change. The so-called greenhouse effect (that naturally occurring carbon dioxide and water vapor keep the Earth's temperature about 33 degrees C higher than it would otherwise be) has been known since the nineteenth century. The two concerns became linked when in 1938 G.S. Callender found higher concentrations of carbon dioxide in the Earth's atmosphere than in the nineteenth century and that human burning of fossil fuels since the industrial revolution began could account for the differences. He also suggested that global warming might result, although his findings were greeted with much skepticism.[39]

It took another thirty years for a sustained transnational research program, which began with the Global Atmospheric Research Programme (GARP) in the mid-1960s, a collaborative effort of ICSU and the WMO. UNESCO's Man and Biosphere Programme also researched the issue, which often overlapped with ozone research as the problems are related. However, political activity around ozone, until at least the late 1980s, generally ignored the links present in major scientific studies. An epistemic community of sorts could be traced to these efforts as key personalities would later become involved in GARP's successor, the World Climate Research Programme (WCRP), ICSU's International Geosphere-Biosphere Programme (IGBP), and the Intergovernmental Panel on Climate Change (IPCC). A prominent example is Bert Bolin, who from 1988–1996 headed the IPCC, mandated since 1988 to advise governments, UNEP and WMO (its sponsors) and the United Nations system as a whole on climate policy. Governments also mandated IPCC to update, transmit and assess relevant scientific information and point out policy implications. The overlap in IPCC and global research bodies at senior levels is well documented.[40] In addition to climate scientists, the core group of researchers in these organizations and at IIASA included energy demand forecasters.[41]

Yet again, IIASA deserves special attention for its long-standing involvement in the issue, although its direct influence on climate change policy is difficult to discern. However, Boehmer-Christiansen (1994a:146) suggests that the potential growth of influence of IIASA, especially through nongovernmental groups, "possibly became threatening to some governments and governmental science, encouraging efforts to capture climate change research from the private sector," a task at which governments proved quite successful, as I explain below. IIASA was one of the few organizations that explicitly linked climate change to "sustainability," broadly defined, and key members of the climate research community had connections to IIASA. For example, William Clark, who headed up the sustainability project, also delivered a keynote address on policy at the Villach conference in 1985 that helped spur the scientific community into political action on the climate change issue.[42]

The meeting in Villach, Austria marked the turning point toward a sustained transnational scientific research program aimed at generating international political attention. Although the first World Climate Conference had addressed the issue in 1979, the Villach conference marked the real beginning of efforts to build a scientific consensus.[43] The relatively late start to consensus building was not owing to a lack of attention by scientists. On the contrary, since the mid-1970s, IIASA, the International Energy Agency, and even the OECD had held high-profile conferences and raised concerns about climate change. Around the time of the first climate conference, UNEP began to get involved and it was UNEP—not WMO, which had most of the governmental science expertise on the issue—that attempted to link climate change to development policy. UNEP also encouraged SCOPE to consider such linkages in its research on the carbon cycle. UNEP head Mustafa Tolba, a botanist by training, encouraged this trend, but his concerns appeared to stem primarily from his political convictions and concern for the developing world, not his scientific credentials or background. He had hard work ahead of him to bring scientific research around to this point of view (Boehmer-Christiansen 1994a:155).

Villach pulled together high-profile governmental and nongovernmental scientists and what might be called research brokers or science managers.[44] Jointly sponsored by UNEP, ICSU, and the WMO, this conference represented the core of an epistemic community on the climate change issue. It also explicitly aimed to influence policymakers, a position in line with the general philosophy of all three sponsoring organizations. Other high-profile transnational scientific organizations gave institutional support, notably IIASA and the Beijer Institute (which later became the Stockholm Environment Institute). Also present were high-profile U.S. environmental organizations such as the Environmental Defense Fund and World Resources Institute, and national research institutes such as two Max Planck Institutes in Germany.[45] James Bruce, an Environment Canada scientist and assistant deputy minister at the time, chaired the conference.

Villach came out with a strong, unified position that an unprecedented increase in global mean temperature could occur in the first half of this century. Current trends in greenhouse gas (GHG) emissions (including gasses other than CO_2) would lead to the equivalent of a doubling of CO_2 in the atmosphere during this time period. If left unchecked, GHG concentrations could lead to an unprecedented, and potentially catastrophic, increase in global mean temperature from 1.5 degrees C to 4.5 degrees C.[46] A follow-up workshop in Bellagio in November 1987 recommended that science-based targets should be designed to limit temperature increases to 0.1 degrees C per decade. However, there is arguably little "scientific" basis of this 0.1 percent limit, especially since what is socially tolerable or dangerous involves value judgments (Agrawala 1999:164). These early prescriptions came out much stronger than the IPCC report of 1990 on the policy side, although more cautious on the science. Thus their main emphasis was the high level of uncertainty and need for greater research. Apart from climate science, research and development was to focus on alternative energy technologies and policies. Significantly, the one nonconsensual document at Villach was Tolba's "agenda of action," which would have made UNEP the main center for policy, while others present thought organizations such as IIASA were better placed to give policy advice.

Participants at Villach set up the independent Advisory Group on Greenhouse Gases (AGGG) in July 1986, under the auspices of WMO, UNEP, and ICSU. F. Kenneth Hare, a respected climatologist from Canada, who had also helped organize the 1979 First World Climate Conference sponsored by WMO, chaired this small group of experts at the very top of their fields. All were prominent in various transnational research efforts or organizations and individually their work had laid much of the basis for current climate change research. Other group members are as follows, listed with just some of their credentials: Bert Bolin (later the first chair of the IPCC); Gilbert White and Mohammad Kassas (both worked on water resource management and land degradation issues, and were associated with ICSU and UNEP); Syukuro Manabe (a pioneer in general circulation models and the leading modeler at the Geophysics Fluid Dynamic Lab at Princeton), Gordon Goodman (head of Stockholm's Beijer Institute, who focused especially on energy policy issues at this time), and Gueorgui Golitsyn (who headed the Institute of Atmospheric Physics at the Soviet Academy of Sciences). Other younger researchers and "advocacy scientists" coalesced around the Beijer Institute under Goodman, and undertook activities sanctioned by AGGG.[47]

Organizing institutions mandated the AGGG to monitor climate research data, conduct assessments of increases in GHG concentrations and effects, advise governments on possible mitigation measures, and possibly to initiate consideration of a global climate convention. Along with other organizations, AGGG followed up on Villach with a second workshop there and another in Bellagio, Italy (mentioned above) in 1987. These conferences are notable for increasing calls for political action and greater participation by policymakers. AGGG clearly constituted an epistemic community concerned about the potential environmental impacts of human-induced climate change, although it also included energy-demand forecasters. Agrawala describes the early days of the AGGG as "an almost utopian era where a small network of experts, international organizations and environmental advocacy groups had a near monopoly both on the international science and policy agenda" (1999:158).

The AGGG marked the high water point of scientific leadership on the climate issue, but that "utopian" era was short lived, as the IPCC gradually overtook the AGGG's work and place in global climate policy. Its influence culminated with the June 1988 Toronto Conference on "The Changing Atmosphere: Implications for Global Security," which grew directly out of recommendations and findings of the Bellagio workshop.[48] The Toronto conference director, Howard Ferguson of Environment Canada, who had attended Bellagio, worked closely with Canadian Environment Minister Tom McMillan to marshal the Villach and Bellagio findings to produce a strong policy statement, which McMillan wanted. Ferguson also recruited AGGG members to be on the steering committee. Among other members, the committee also included Jim MacNeill, who had just finished his work on the Brundtland report, another impetus for the conference. The presence of Gro Harlem Brundtland, Canada's Prime Minister Brian Mulroney, and number of ministers from a G-7 Summit held earlier at the same venue, among the 300 scientists and policymakers present, gave climate change science its most influential audience to that point.

The main conference recommendation, that governments and industry should reduce CO_2 emissions by 20 percent from 1988 levels by 2005 "as an initial global goal" was the most powerful and specific policy recommendation to that date, and became a rallying point for global action in early climate change negotiations. The conference statement also recommended the development of "a comprehensive global convention as a framework for protocols on the protection of the Atmosphere."

These high-profile and credible policy conferences also happened to correspond with a series of external events and political and economic circumstances that, although largely unrelated to science, elevated public concern and galvanized government responses to climate change. Economic factors included falling fossil fuel prices and growing fuel and energy technology competition, which gave alternative energy suppliers (that is, not coal and oil) incentive to support climate change research and action. Political factors included the winding down of Law of the Sea and acid rain negotiations that had preoccupied countries interested in environmental negotiations. Most of all, high-profile environmental events such as the discovery of the ozone "hole," the Chernobyl disaster, and especially the unusually hot summer, and drought, in North America in 1988 raised public concern about global environmental problems.

Heightened media attention culminated with the famous U.S. Senate energy committee testimony of NASA scientist James Hansen, who said research supported a causal relationship between an increased greenhouse effect and observed higher temperatures over decadal time scales, which could cause increased heat waves and droughts. With temperatures reaching a record 101 degrees F., Hansen, chief of NASA's Goddard Institute of Space Studies and a climate modeller, told the hearing "the greenhouse effect has been detected and it is changing our climate now," and suggested that politicians should "stop waffling" and take action (Paterson 1996:33; Boyle 1999). It would, however, take another seven years for scientific consensus, when the 1995 IPCC Second Assessment Report found that, "The balance of evidence, from changes in global mean surface temperature and from changes in geographical, seasonal and vertical patterns of atmospheric temperature suggests a discernible human influence on global climate" (IPCC 1995:2.4).

The summer of 1988 marked the pinnacle of epistemic community influence. Until then, most Western governments viewed climate change as mostly a scientific and environmental problem (Bodansky 1994:50). The United States, however, from the start viewed the issue from a more economic perspective and through the lens of domestic policy. Thus, while other states primarily dealt with the issue through environment ministries, the U.S. administration set up its own committee of the White House Domestic Policy Council, having learned from ozone negotiations that the EPA and State Department might move more quickly than the White House desired. Although EPA had representation, the major players included the powerful departments of Energy, Commerce, and Interior, the Office of Management and Budget, and the Council of Economic Advisors (Bodansky 1994:50; Victor 1995:365). The result was a policy position that emphasized measuring economic costs and cost/benefit calculations of environmental risk. The combination of domestic interests including a powerful fossil fuel lobby and a conservative White House who viewed environmental problems primarily through an economic lens meant scientists were essentially outmuscled in U.S. policymaking.

Internationally, pressure from the United States especially, but also other industrialized countries, led to the marginalization of the work of the AGGG as climate change turned from a primarily scientific to a political issue. James Bruce, secretary of the WMO Executive Council when it decided to set up IPCC, described the shift from the AGGG to the IPCC this way:

> . . . after a couple of years of their [AGGG] work, there was an unease . . . that crept into some governments that this was an issue that was going to have enormous economic repercussions one way or another and they, in particular the United States, didn't like the idea of these free-wheeling scientists pronouncing on the subject. They preferred something with more governmental involvement (author's interview).

This unease about "the sort of influence that a semi-independent group of scientists might have," as Bruce put it, led the WMO and then UNEP to jointly sponsor the *Intergovernmental* Panel on Climate Change, which, as its name suggests, was more directly under the control of governments. The idea of an IPCC had percolated within WMO since at least May 1987, overlapping with the AGGG, and AGGG work continued until 1990, but for all intents and purposes the IPCC took over the policy role of AGGG upon its launch in November 1988 (Agrawala 1999). In addition, the original members of the AGGG had already begun to drop out of the group. Some expressed increasing discomfort with their policy role because they worried that advocating policy as science, would compromise their credibility (Agrawala 1999:164, referring to a published interview with Syukuro Manabe).

In December 1990, governments also took the actual negotiations toward a convention out of the hands of the WMO and UNEP (unlike ozone, for example, which was negotiated under the auspices of UNEP) and put them into the hands of the UN General Assembly. Under Resolution 45/212 on Protection of the Global Climate for Present and Future Generations the UNGA set up the Intergovernmental Negotiating Committee (INC), handing over negotiations fully to diplomats and out of the hands of the IPCC, which still operated independently of the negotiations themselves.

Since the FCCC came into force in 1994, new subsidiary bodies that institutionalized the role of scientists further entrenched government control (biodiversity and other agreements contain similar bodies), although the

IPCC remains as a source of advice "independent" of the convention. Governments approve the scientists who sit on subsidiary bodies responsible for processes such as periodic reviews, evaluations of triggers for further action, monitoring, and joint research activities. Despite the importance of such functions, no guarantee exists that parties will accept the findings of such groups or abide by their recommendations since political pressure at home, for example, might lead them to usher counter scientific evidence, or use political or economic arguments to suggest alternative actions, no action, or the need for more research (Susskind 1994:65). The increasing level of government control also means such bodies are less likely to embark on independent initiatives in terms of shaping the overall governance structures, which have essentially been set for them. Actual allocative decisions or objectives, then, are not likely to stem from these bodies, though their findings may be used to suggest the need for swifter action, for example.[49]

The combination of these events served to compromise the independence of the climate change research community, which suggests it can no longer be considered an epistemic community according to the definition given earlier since members now consist of government-approved scientists. Bruce, although he defends the IPCC's scientific integrity, largely confirms how the role of science changed as the political stakes grew. One such change involved the replacement of natural scientists largely by economists in formulating policy options.[50] Nonetheless, the scientific community sees the IPCC as its main voice in politics and policy.

The first two IPCC reports (1990 and 1995) also present a window through which to see the politics of scientific activity on climate change. The IPCC divided into three working groups. For the first assessment report, working group I provided the scientific assessment of climate change, working group II the impacts of climate change, and working group III response strategies. In the negotiations that led up to the climate change convention, the working group I report received the most attention, although its influence apart from warning about the problem was limited. It was also somewhat divided over its emission scenarios, the part of the report most likely to affect policy because projections on emissions are a necessary part of determining what kind of action would be required to prevent "dangerous" levels of increases in greenhouse gases (the objective of FCCC as found in article 2) (Boehmer-Christiansen 1994a:148–149).

Other significant recommendations came from working groups II and III on the need for more research into the sensitivity of "socioeconomic" systems to climate change. Working group III also noted the lack of cost-benefit analysis or research on technological or market impacts of proposed solutions. The findings had little impact on the treaty process, as working group III was largely ignored by policymakers, but findings proved significant for the future research agenda of IPCC.

As a result of the report, and pressure especially from the United States, which favored cost-benefit analysis of environmental problems and was moving to support market-based solutions, working group III was completely recast in 1992 to undertake the second assessment.[51] Its new mandate focused much more on economic modeling, specifically to conduct "technical assessments of the socioeconomics of impacts, adaptation, and mitigation of climate change" (IPCC 1995, Working Group III Summary for Policymakers:1). Whereas the original policy group consisted of a mix of scientists, engineers, and administrators, economists dominated the new group, reflecting also the growth in the academic field of the economics of climate change. It also represented a shift in emphasis from technical solutions and opportunities favored by countries such as Japan, to the economic costs and benefits of various responses and the policy instruments to best achieve them.[52] Other social scientists (political scientists, sociologists, geographers, and so on) were not generally selected by governments, hence the report has little to say about sociopolitical factors such as societal stresses, changes in government, institutional adaptation, and so on.

Although it has been the object of some controversy over subjects such as differential costing of "statistical" lives in developed and developing countries, the 1995 findings of working group III have generally received more attention by policymakers than did the 1990 report.[53] This increased impact seems likely a result of the report's more economistic rather than ecological approach to policy.

The motivation of lead researchers on climate change, a main component of the epistemic communities hypothesis, is difficult to determine in aggregate. Interviews and documentary evidence suggest that many shared causal beliefs about the nature of global change, but consensus on principled beliefs or a desire to "develop social laws from their understanding of the laws of nature" finds little support.

For example, Boehmer-Christiansen, in her extensive study of the climate change policy process, argues that the coordinated research community "acted primarily as a lobby for its own research agendas dedicated to the modeling of planet Earth and the development of alternative energy sources" (1994a:140; 1994b). Expensive technologies for modeling, the performance of which in large part drove the success of the climate research in the mid-1980s, perpetuated this need. Other research programs related to energy forecasting, for example, also used climate change to convince governments of policy relevance at a time when fossil fuel prices were dropping. Boehmer-Christiansen argues that scientific bureaucracies used climate change to convince governments of policy relevance and the need for further research at least in part as a way to gain public money (even at the expense of increased government control) to fund these programs beyond the means of the private sector. Most other literature takes a less cynical view of motives, but emphasizes only that scientists pushed for some policy response, not that they tried to develop policy norms based on "social laws" derived from nature.

Evidence also suggests that splits existed in the scientific community on what scientific findings meant in terms of policy implications. Moreover, many scientists, even the most active, such as former IPCC chair Bert Bolin (1994), have expressed reluctance to enter debates that speak too directly to actual policy choices. Admittedly, a coordinated research community certainly emerged after 1985 that shared a consensus on the nature of the problem and agreed that it ought to be brought to the attention of policymakers (although the group also acknowledged a high degree of uncertainty). However, there was and remains much less consensus on the principled beliefs about how to think about the threat from climate change in terms of resiliency of ecosystems and the like (James Bruce, author's interview). If one principle did arise, it was the notion of precaution in the face of uncertainty.

In terms of an ecological approach, even the most ardent supporters of the values of an "ecological scientific community" did not press hard for that approach in policy. For example, the initial approach, advocated by Canada, called for a framework agreement on a "law of the atmosphere," like the Law of the Sea. Agreement on a framework convention could then lead to separate protocols on specific issues such as acid rain, ozone depletion, and climate change. "The rationale for this approach was that it recognized the interdependence of global atmospheric problems" (Bodansky 1994:53). Similarly, the Toronto Conference Statement called for "a comprehensive global convention as a framework for protocols on the protection of the atmosphere," which fit well with a "scientific ecology" orientation. Ironically, a second approach later adopted—to focus simply on a convention on climate change—came from Tolba, whose stature was high because of his leadership role in producing an international agreement on the ozone issue. At a conference in Ottawa in 1989, Tolba strongly criticized the "law of the atmosphere" approach as politically unrealistic, and argued for a more narrowly focused convention. As a result, it never again achieved serious consideration (Bodansky 1994:53). So it would seem that when it came to policy, the scientists most actively engaged seemed as driven by political expediency as by drawing social laws based on the laws of nature.

The actual content of the climate change treaty, while it certainly does reflect some ecological ideas, also has embedded in it the core norm-complex of liberal environmentalism. For example, the convention's objectives include (in Article 2) stabilization of greenhouse gas concentrations "at a level that would prevent dangerous [not defined] anthropogenic interference with the climate system . . . achieved within a time frame sufficient to allow ecosystems to adapt naturally." However, it goes on to say that the level decided upon should also "enable economic development to proceed in a sustainable manner."[54] The principles and commitments reflect this balancing act, reproducing the same or similar wording to principles found in the Rio Declaration. Scientists certainly did not have consensus on this set of norms, nor did they clearly articulate a set of values that flowed from scientific research that would frame the convention differently.

In addition, virtually no discussion occurred prior to the Toronto meeting about North-South issues. The scientists who dominated atmospheric research primarily came from the North and this group, for the most part, did not address discussions about the effects of climate change on development. The only notable exception came out of the second (1987) Villach workshop where delegates recognized that aid might be necessary to pay for anticipatory adaptation to climate change in developing countries (Rowlands 1995a:189). Scientists simply did not deal with these issues, which they perceived as political.

Even after the treaty was signed in 1992, entrenching ideas inherent in the concept of sustainable development, scientists did not much discuss the concept, but simply accepted it as part of their mandate. The understanding of sustainable development within working group III of the second assessment report was a basic notion that severe environmental or economic damage would make development unsustainable. As Bruce, co-chair of the working group, put it, "If [the population is] going to be flooded, then that makes it difficult to sustain development for a small island state . . . or if [the state is] going to have economic losses of nine per cent GDP per year" (author's interview). He added that the ecological perspective did not dominate thinking about sustainable development among scientists. Moreover, they often viewed issues around sustainable development as political questions not fit for recommendations by the IPCC. For example, the first IPCC report largely left equity issues unexplored and the second report took a cautious approach, emphasizing that politicians should choose between such policies although the effects could often be determined scientifically.

It is not therefore surprising that the IPCC itself simply had little direct effect on the content of the FCCC signed at Rio. A commentary on the 1990 IPCC assessment report by authors affiliated with IIASA put it as follows:

> The first report [on the science of climate change] is easily the single most referenced document on the science of climate change, demonstrating its focal role in the public debate. IPCC "consensus" documents are paraded by both environmental non-governmental organizations (ENGOs) and governments in their domestic debates to "show" that the climate problem is real and deserves policy action—and by others to demonstrate the reverse—even though the IPCC documents are appropriately more cautious. . . . [However], in practice, it has been extremely difficult to integrate research on the effects of climate change and policy options into coherent and useful consensus documents (Victor and Salt 1994:27).

They note elsewhere that a reorganization for the second assessment report explicitly included more economics and expertise drawn from developing countries, two developments driven much more by governments than the scientific community or the science of climate change (Victor and Salt 1995). Rowlands (1995a:89) is more blunt, stating that the 1990 IPCC report was marginalized in the intergovernmental negotiating process and that "politicians regarded any consensual scientific knowledge as but one of many inputs."

The second assessment report (1995) responded to these concerns on the policy side, but its presentation of options (its mandate) does not demonstrate a consensus. Furthermore, the themes that do emerge draw much more on economics than they do on a vision typical of ecological science. This result is not surprising, given that social scientists who dominated working group III were primarily economists. Most of the economists approved by governments, although certainly prominent, were classical and/or environmental economists, not ecological economists. In fact, hard scientists and economists often disagreed on policy instruments and approaches (and there were debates on technical issues among economists as well) (James Bruce, author's interview). While the overall approach reflects the mandate given to the IPCC (and taken from the convention) to "place the socioeconomic perspectives in the context of sustainable development," it carved out its analysis more narrowly to reflect liberal environmentalism.

This disjuncture between research programs of natural scientists and social scientists persisted within research circles throughout the work of the IPCC. Natural scientists continued to approach the problem of climate change in terms of physical flows of matter and energy, while social scientists defined environmental problems in terms of human behavior. Cohen et al. (1998) illustrate the conflict between climate change framed as a scientific problem and the discourse of sustainable development, which they see reflected in the two most prominent transnational research programs, the IGBP and the International Human Dimensions Program (IDHP), dominated by social scientists. They note a "puzzling lack of interaction between two fields of research and activity, associated, respectively, with climate change and with sustainable development" (1998:342). Policymakers were relatively free to shape the social scientific analysis as well, along lines already suggested within a narrowly economistic framework. Although this made the work more policy relevant, it tended to be weak on broader sustainable development concerns. As Cohen et al. (1998:342) note, although the mandate of working group III of the second assessment report was to address sustainable development issues, "the actual discussion of SD [sustainable development] is almost non-existent." This limitation of the research might explain why policymakers could relatively easily treat scientific findings as less policy relevant, except in the minimalist sense (in policy terms) of identifying the environmental problem and biogeophysical impacts (rather than social impacts or policy responses).

Three conclusions on the climate change case raise difficulties for the epistemic communities approach to understanding international environmental governance. Recall, climate change should be an easy case, which suggests the challenges to the hypothesis ought to be taken seriously.

First, scientists could not or would not formulate a coherent set of policy ideas that states then adopted. Admittedly, a transnational scientific community did prompt an international policy response independently and ahead of public concern with the climate change issue. In that way, the epistemic community hypothesis finds some support in the climate change case. However, scientists did not have a large influence on policy formation. The height of influence probably came in 1988 with the Toronto conference target of 20 percent cuts. Although environmental NGOs and states likely to be most affected by climate change (for example, small island states) often raised this concrete recommendation prior to Kyoto, it never received serious consideration as a focal point for agreement. In addition, the ecological approach did not dominate proposals. Governing norms that stemmed from the scientific community, apart from the Precautionary

Principle, either did not appear in the agreement reached or did so in the context of other norms that supported liberal environmentalism. The one policy implication, apart from reducing emissions, that seemed to produce consensus in the scientific and technical communities involved in climate change research was the need for policy to focus on alternative energy technologies and policies. But that concern too gradually eroded as a focus for policy, even within the IPCC, which concentrated more on the economic efficiency of policy options.

Second, when scientists did gain public/political attention, they did so through encouraging concern on the issue itself, not its framing. In any case, governments quickly responded to such concern by taking control of transnational research and policymaking. Governments also took allocative decisions out of the hands of scientists, who proffered few unified recommendations anyway on such issues. Far greater consensus could be found among economists who seem to be increasingly important providers of policy advice.

Third, the linkage of scientific activity and ideas to sustainable development came late, and was largely pulled by individuals with existing links to the development community, such as UNEP head Mustafa Tolba. These links did not spring from science or the climate change research community. In Tolba's case, the concerns stemmed from his official position and his own convictions. In numerous published speeches he stressed the importance that should be placed on the effects of climate change, among other environmental problems, on the developing world. His speeches also emphasized the need to formulate problems in a way to address such concerns. In contrast, most of the relevant scientific community demonstrated either indifference to sustainable development as framed by UNEP or unease with UNEP playing a leading role (Gordon McBean, author's interview). The conclusions on climate change are indicative of the overall performance of the epistemic communities hypothesis in explaining the evolution of international environmental governance.

CONCLUSIONS

This chapter has questioned the proposition that science is a primary informer of policy direction on international environmental concerns. Thus it challenges a key conclusion of Peter Haas's, "that science is essential for the understanding of global environmental problems, thus shifting the determination of the scope of allocative decisions to the international institutions for science" (1996:1). To the contrary, scientists were largely excluded from allocative decisionmaking and often eschewed such roles. When they did have influence, such as in promoting the Precautionary Principle, it did not come as a direct outcome of their specialized knowledge of cause-effect relationships.

Second, consensus on cause-effect relationships within scientific communities did not seem to correlate well with action on major issues, although sometimes individuals or groups of scientists played an active role in promoting particular environmental concerns. On the central question of principled beliefs, consensus often seemed particularly weak on a number of dimensions, making the case difficult to sustain that such consensus was either necessary or sufficient for development of particular norms of environmental governance. Even on specific matters of policy choice, consensus was relatively uncommon. Thus the consensual knowledge which supposedly gave a potential epistemic community its political power is open to question.

Third, problems arise from the literature's emphasis on tracing the effects of single communities, based on the assumption that a particular group should be privileged because of its claim to authoritative knowledge in the particular issue area. This approach can easily miss the competition of expert groups who come at policies with different agendas. Environmental policy since 1972 has not been the sole concern of a community of scientific ecologists. As the development of norms in practice suggests, development and environmental economists also had an interest in environmental policy and actively sought, or were solicited, to influence the shape international environmental governance would take (a point taken up in the next chapter).

A related problem is the focus in the literature on an ideal-type scientific ecology epistemic community. This focus has led Haas to the erroneous conclusion that environmental governance now faces a backlash from rules and principles of trade regimes and market challenges at the domestic level (Haas 1996:43–44). This bias is built into epistemic community studies since they start with the ideas of a particular community and assume a backlash when those ideas do not dominate. The bias closes off a critical examination of how such ideas interacted with other forces or whether they are indeed the basis for the norms and institutional arrangements that finally result. Hence the focus on "scientific ecology" simply misses the compromise of liberal economic and environmental norms that was at the very center of bringing environmental norms into mainstream international governance. Although arguably the compromise embodies important contradictions, it has shaped the way environmental concerns are now institutionalized in international governance and arguably it has been the single most significant factor in shaping international environmental governance over the last thirty years. The so-called backlash does not exist; it is instead a logical outgrowth of the norm-complex developed over that time period.

A second set of conclusions concerns how science actually did work. The primary pattern revealed is that, contrary to the epistemic community hypothesis, scientists were reactive, not proactive in the major norm-articulating events identified, even learning themselves from their involvement in such activities.

The science policy literature often focuses on reasons for a lack of independent influence, including mutual distrust of scientists and policymakers/publics or simply a reluctance of scientists to enter into the policy process, even among those most active in communicating scientific ideas to policymakers.[55] Similarly, Lemons and Brown (1995:13) note the "fallacy of unfinished business"—the tendency for scientists to see problems as technical, therefore requiring only technical solutions. That view closes off for them social, ethical, or political solutions. Poor interdisciplinary communication may also limit the production of policy-relevant knowledge.[56] Such problems may also stem from the difficulties of effective communication between scientists and policymakers and the public, which leads to misunderstandings especially in the face of incomplete knowledge and scientific uncertainty (Rowland 1993). Often policymakers can then choose among competing knowledge claims within scientific communities.[57]

It should not be surprising, then, that scientists involved in international environmental policy exhibited the same kind of unease as their colleagues in other policy-relevant fields when they got too close to political processes. This situation remains largely unchanged since 1972. At that time, Lynton Caldwell (1990:115) noted that SCOPE "appears to accept an assumption widely shared among scientists who believe that their public mission is largely fulfilled when scientific studies are made available to governments and international organizations."

William Clark similarly looked back on the Brundtland Commission process and argued the small role science played was for the best:

> I think it's probably just as well. . . . The agenda on sustainable development moved ahead very rapidly in the Brundtland era into UNCED in ways that were largely successful in shaping a political re-framing of the terms of the debate, a political consensus on at least some directions we

> needed to be heading. . . . Frankly, they had about enough science to let that go forward and not so much that it got in the way. [Now it is time] for the science community to make a re-examination of the sustainability issue and see whether, given the political consensus that has shaped up . . . we're doing the right science . . . whether the necessary long-term science and monitoring legs for this venture fit . . . the development, the political, and economic legs. So I think [the science and politics of sustainable development] were out of phase, but whether that was done intentionally or not, [it was] probably done very effectively and would have been less effective had the scientists been running all over the Brundtland Commission (author's interview).

Such a conclusion suggests the causal arrow runs opposite to the way the epistemic communities hypothesis suggests. Even when groups of scientists attempted to maintain their independence, governments proved particularly adept at reining in science and setting parameters for their research and influence on policy. Interestingly, in one of the few detailed comparative studies of the science-policy nexus in major international environmental agreements, Andresen et al. (2000) find that scientific influence on policy, although weakly correlated with involvement in the policy process, may be *inversely* related to autonomy of scientists in the policy process (Underdal 2000b:196–199). While not completely contradictory to an epistemic communities emphasis on the need for scientists to infiltrate domestic bureaucracies and international institutions, it does suggest at least a healthy skepticism that the influence of epistemic communities will reflect their own "worldview" once involved closely with the policy process, since governments and other political factors influence the research process to their own ends. Underdal argues that this finding suggests that scientists may perform different functions at different stages of the policy process, or in different institutional settings. Also consistent with the findings here, Andresen et al. found that while scientific evidence "often serves to precipitate *some* kind of policy response" and can play a role in agenda-setting, "The *substance* of that response, however, is determined essentially by politics rather than science" (Underdal 2000b:184, emphasis in original).

Finally, a number of empirical anomalies suggest that the evidence simply does not hold up in defense of a narrow focus on science in explaining the evolution of environmental governance. Most obviously, there is a lack of fit between ideas generated by an epistemic community of natural scientists and observed normative shifts. The uninterest with which most scientists greeted the political/economic questions that seemed to guide much of norm creation suggests that a key condition for such an epistemic community's influence has not been met.

One broader normative concern about the epistemic community literature deserves mention before closing off this discussion—that the literature makes an assumption that if consensual knowledge exists on environmental management in an epistemic community, then institutions ought simply to be designed to better integrate such knowledge. That position ignores the possibility that contestation might come from outside that community, or that the community is not equipped to deal with the broader social and political implications to which that knowledge might be put to use. Conversely, that position might blind analysts into assuming that the epistemic community is being listened to when the problem it identifies is being addressed. Such an assumption makes it easy to ignore the real contestation over how the problem is being addressed, to what ends, and for whose benefit. Thus, epistemic community analysis either misses the boat on how ideas inform governance, or leads to an uncritically examined normative end point where the community's prescriptions are assumed to be in the best interest of humankind.