SCIENTISTS, KNOWLEDGE - FORMATION AND POWER

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SCIENTISTS, KNOWLEDGE-FORMATION AND POWER: A STUDY OF SCIENTIFIC AND POLITICAL DISCOURSE IN THE FORMATION OF INTERNATIONAL ENVIRONMENTAL POLICY

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LIST OF ORGANISATION AND TREATY ABBREVIATIONS

CCOL	(UNEP) Coordinating Committee on the Ozone Layer
CIAP	(US Department of Transport) Climate Impact Assessment Program
CLRTAP	Convention on Long-Range Transboundary Air Pollution
CMA	The International Manufacturing Chemists Association
CSCE	Conference on Security and Cooperation in Europe
DOE	(United States) Department of Energy
ECE	United Nations Economic Commission for Europe
EPA	(United States) Environmental Protection Agency
FPP	(CMA) Fluorocarbon Program Panel
GAO	(United States) General Accounting Office
IGBP	International Geosphere-Biosphere Program
IMOS	(Interagency Task Force on) Inadvertent Modification of the Stratosphere
IPCC	Intergovernmental Panel on Climate Change
NAPAP	(United States) National Acid Precipitation Assessment Program
NAS	(United States) National Academy of Sciences
NASA	(United States) National Aeronautic and Space Administration
NCAR	(United States) National Center for Atmospheric Research
NOAA	(United States) National Oceanic and Atmospheric Administration
NRC	(United States) National Research Council
NRDC	(United States) National Resources Defence Council
OECD	Organization for Economic Cooperation and Development
OES	(United States) Bureau of Oceans and International Environmental and Scientific Affairs
OSTP	(United States) Office of Science and Technology Policy
PCC	Policy Coordinating Committee of the US National Security Council
SPC	IPCC Special Committee on the Participation of Developing Countries
UNEP	United Nations Environment Program
USGCRP	Global Change Research Program
WHO	World Health Organization
WMO	World Meteorological Organization

SYNOPSIS

This thesis blends philosophy of science, sociology, political science, communications studies and various physical sciences in an attempt to understand how environmental policy is shaped out of the discourse within and between scientific and policy communities. The focus of the exercise is to show how patterns of communication generate communities *identified* by their discourse; that is to say, how issues and policies shape as well as reflect bodies of knowledge.

For all of the nineteenth and much of the twentieth century it was an article of faith in virtually all Western political thought that knowledge and power could be dichotomised, thereby (falsely) creating unrealistic expectations that science can induce political consensus. Yet in practice, knowledge is framed in light of specific interests, so that information begets counter-information. Most analyses of science in world politics suffer from the modern misreading of the relationship between knowledge and power. The literatures on post-industrial society, functionalism, and epistemic communities are all vulnerable to this criticism.

The availability of scientific knowledge to the relevant decision makers was a necessary condition for the negotiation of the Montreal Protocol on Substances that Deplete the Ozone Layer, but it was far from being a sufficient one. The power of science was a function of the political context in which it was debated, a context which was defined substantially by the discovery of the Antarctic ozone 'hole'. The prominence of knowledge-based power in at least some situations means that conventional materialist notions of power should be expanded to include a more discursive and productive conception of power. Environmental problems are not merely physical events, but informational phenomena. A case study methodology is used to develop an interactive conception of power and knowledge. A detailed study of the Montreal Protocol is offered.

The potential for science to be influential in a significant way in the evolution of the Montreal Protocol was itself a function of a constellation of factors, not the least of which was the political context in which knowledge was expressed, interpreted and debated. An understanding of the interaction of power and knowledge in specific situations cannot be derived through any *a priori* theoretical exercise, but requires contextual analysis of concrete cases. Theoretical inquiry into the mutual embeddedness of power and knowledge, however, does suggest that neither the scientific nor the political context can stand as an independent variable. This thesis supports such a judgment, analysing the events leading to the Montreal Protocol as a complex interplay between scientific and political contexts, without reducing one to the other. Science did not offer a supply of value-free facts from which a policy consensus could be formulated, for knowledge could be framed in light of specific interests so that questions of value were rendered as questions of fact. On the other hand, scientific knowledge was not completely malleable, for it was rooted in an intransitive ontological dimension of facts [Bhaskar 1986] recognised by most, if not all, participants.

The question of why some forms of knowledge are accepted and others rejected, as well as the questions of when, where, and by whom, requires an analysis of institutional considerations. The political context was defined by the following factors, among others. Domestic structures in the participating nations conditioned the extent to which scientific knowledge was available and appreciated. The nature of relations between industry and government, as well as the structure of the various national CFC industries, were also important factors in setting the political context. Another element was the strength of domestic environmental organisations.

One outstanding event that cannot reasonably be classified as either wholly scientific or political, but which had an overriding impact on all aspects of the problem, was the emergence of the Antarctic ozone 'hole'. Although the participants in the negotiations explicitly agreed that any agreement should disregard the hole, the causes of which were unknown at the time, it played a vital and catalytic role in shaping the outcome of the negotiations. Not only did it pervade the psychological and political milieu by heightening the status of scientific uncertainty, the hole also had a strong influence on the political acceptability of specific modes of *framing* and *interpreting* the consensual knowledge at hand.

The availability of a comprehensive international scientific assessment [WMO/NASA 1986] was a key factor in shaping the context of the debates and the eventual outcome. The WMO/NASA report was accepted as the most up-to-date and authoritative expression of scientific knowledge on stratospheric ozone, and hence as the scientific basis for the negotiations. From a policy perspective, the fact that this document represented an international consensus, rather than the work of scientists from any one country or region, was as significant as its actual scientific content. The process of assembling the study was an unmistakably political one. The three-volume assessment also contains a wealth of information that was barely mentioned during the negotiations, most notably on climate change, largely because the policy debate focused on the health effects of ultraviolet radiation. This mode of framing the debate had a major impact on the policy making process. Thus, while knowledge was indispensable, it was never apolitical.

Moreover, the predictions in the WMO/NASA assessment were fundamentally dependent upon reliable production statistics for the trace gases, especially for the CFCs. The accuracy of the economic data became a major source of contention. The greatest stumbling block, however, was in predicting future growth rates for CFC production. Industry had a vested interest in predicting low rates, whereas those who favoured strict regulation favoured high rate. At first glance, this appears to be an issue resolvable by applying 'objective' economic projections based upon growth of demand, but this was far from the case. On this issue, knowledge was nearly inseparable from interests, despite the fact that the participants had reached a consensus on many aspects of the atmospheric science.

To say that scientific knowledge was critical, however, does not entail that the scientists themselves were the driving force in the policy realm. Of course they were necessary, for without them there would be no science, but once the knowledge was produced, it became a potential tool for other actors with a stronger policy orientation. In this instance, a group of *knowledge brokers* was instrumental in both translating the available knowledge into terms understandable to decision makers and pushing forward specific policy proposals. This group, largely employed in the US Environmental Protection Agency (EPA), was more inclined than the scientists themselves to employ the available knowledge on behalf of far-reaching policy recommendations, both domestically and in the negotiations. To this end, the manner in which that knowledge was framed and communicated became a significant factor; the range of possible formulations was itself conditioned by the political context and external events like the discovery of the Antarctic ozone hole. Domestically, these EPA research brokers were able to develop close ties with other agencies and departments, most notably the State department and, to a lesser environmental groups. Likewise, they developed transnational alliances with individuals in foreign environmental bureaus which, at least in some instances, were helpful in swaying those agencies' governments in the direction of the US position.

The existence of a group of ecologically-minded research brokers armed with an international scientific consensus under crisis-like conditions provoked by the Antarctic ozone hole was not enough to bring about the Montreal Protocol. Another factor was a relatively neutral atmosphere that could only be provided by an organisation with no specific national ties. Through its Coordinating Committee on the Ozone Layer (CCOL), UNEP disseminated up-to-date scientific knowledge to countries that did not have strong research programs of their own. In effect, UNEP engaged in a sophisticated public relations program to recruit support through the dissemination of scientific information framed in a particular way, or according to certain perceived interests. Critically, the way in which such information was communicated played a major role in gathering political support for a certain perspective. The use of satellite images, computer enhancements and renderings of the ozone hole, statistical projections, graphics of a future world bathed in ozone, and so on, all played a vital part in framing the debate, in particular among economically weaker but nevertheless potentially politically important interlocutors. At a geo-political level, then, the medium had become the message.

The thesis also looks at two other international environmental problems, though in far less detail: acid rain and global warming. The main purpose of these cases is to provide some grounds for testing whether the findings regarding the Montreal Protocol process are exceptional or typical. Like the issue of ozone depletion, neither of these problems is immediately detectable by the senses; a great deal of inference is necessary, not only for making predictions about the future, but even for understanding the current extent of the damage. Consequently, scientific knowledge is an essential ingredient in formulating adequate policies to address these problems.

There are, however, important differences among the three issues, differences which provide an opportunity for studying how policy makers cope with scientific uncertainty under a range of conditions; but scientific uncertainty is not necessarily an independent variable in these cases, for it can influence how other factors are perceived. The most important differences among the issues are the distribution of costs and benefits for control measures, the diffusion of pollution sources, and the level of involvement of developing countries. Another significant difference is that, contrary to the negotiating process for the Montreal Protocol, the US has not assumed a leadership role in efforts to ameliorate acid rain and global climate change.

Each of the cases analysed provides strong testimony against the epistemic cooperation thesis. For acid rain, a scientific consensus was reached during the early 1970s that sulphur dioxide emissions were destroying Scandinavia's freshwater ecosystems. Yet genuine efforts to control these emissions in Europe were only undertaken in the mid-1980s. The precipitating event was the discovery of massive forest death in Central Europe, a phenomenon which received enormous publicity but had not been conclusively linked to acid rain. For global climate change, scientists generally agree that current levels of greenhouse gas emissions will take the planet into uncharted climatological territory. As a community, they have been outspoken in their support for major policy shifts to mitigate the changes, yet an effective global treaty in the near future is only a remote possibility.

To the extent that the power of social actors is rooted in how they frame and interpret information, then an approach that focuses on discursive practices may be most appropriate. As determinants of what can and cannot be thought, discourses define the range of policy options, thereby functioning as precursors to policy outcomes. I argue for such a discursive practices approach, one that would be highly sensitive to the interactive dynamics between knowledge and power. The power of competing knowledges -- likely to be decisive under conditions of scientific uncertainty -- is the critical issue in the studies analysed in Chapters Five and Six.

The major contribution of a discursive practices approach is its ability to conceptualise power and knowledge as intricately enmeshed. The overarching regulation of the political field by linguistic symbols "transcends the generative and critical capacities of any individual speaker or speech act" [Terdiman 1985: 39]. The power embodied in dominant discourses is most easily characterised in terms of Foucault's notion of "disciplinary power". Discursive power is decentralised, non-monolithic, and rooted in linguistic practices rather than overt control and material domination.

Environmental problems may be viewed primarily as informational phenomena, or as struggles among contested knowledge forms. The Montreal Protocol process, for instance, is essentially the story of how a dominant anti-regulatory discourse was supplanted by a new regulatory discourse. This discursive shift occurred both within the US and internationally during the Montreal Protocol negotiations. The 1986 WMO/NASA report defined the scientific parameters of discourse, yet that document was amenable to a wide range of policy stances. A discursive practices approach focuses on the contextual factors, such as the discovery of the Antarctic ozone hole, that empowered a subordinate discourse.

More generally, a discursive practices perspective on international regimes provides a valuable alternative to both liberal and neo-realist approaches. The former place inordinate emphasis on the communitarian and cooperative dimensions of regimes, whereas the latter, while being more cognisant of the dynamics of power, tend to reduce power to material factors in the possession of specific agents. Only a discursive practices approach offers an understanding of regimes as crystallised embodiments of power and knowledge.

If those scholars who discern a trend toward a "post-industrial" or "informational" world order are correct, then this argument has important implications not just for environmental issues, but more generally for the nature of power in the emergent global system. One trend may be the diffusion of the sovereign power of nation-states to non-state actors and the proliferation of disciplinary micro-powers. Consistent with this diffusion would be the displacement of power toward those actors most proficient at controlling and manipulating informational resources. There are good reasons to believe that, as environmental pressures become more severe and other international problems become increasingly technical, the terms of political discourse will become ever more scientific. Yet the prevalence of scientific discourse should not delude us into the common misconception that polities will therefore become more rational and less conflict-ridden, whether through functional cooperation or through epistemic communities. A profusion of information may, in fact, lead to greater confusion as the world becomes a ubiquitous market for discourses. The scientisation of politics may well devolve into the politicisation of science.

CHAPTER ONE
OVERVIEW OF THE ARGUMENT

1.1 Summary of the Argument

While most studies about science and world politics focus on military and economic issues, and some work has been done on science and domestic environmental politics, precious little is available on how scientific knowledge functions in international environmental negotiations. Yet the expanded temporal and spatial perspectives implicit in large-scale environmental problems make the analytical skills of scientists especially important in this issue area. Because environmental problems are plagued by uncertainty, a new class of environmental experts has arisen to advise policy makers.

Extending this analysis beyond environmental issues, scientific culture may be seen as a driving force in the politics of a "post-industrial" society [1]. Indeed, with the end of the Cold War and the emergence of new security challenges -- the very least of which is to review if not revise the very notion of 'national security' itself -- many theorists of world politics are seeking alternatives to the traditional ways of conceptualising political power [Rosenau 1990; Nye 1990]. A host of old and new issues, including pandemics, famine, the disintegration of many multicultural states, trade policies, state-sponsored terrorism, and environmental problems, require cooperative endeavours among states while simultaneously involving a diffusion of power away from states to non-state actors. Technical experts are frequently drawn into the policy process, becoming important political actors in their own right.

Conventional approaches to international relations tend to depict power as a material resource, a tool wielded by nation-states to further their own interests [Waltz 1979; Baldwin 1985; Gilpin 1975]. The kind of power most clearly relevant for many of the new issues, including international environmental problems, diverges from the conventional wisdom in three respects. First, because it is so deeply connected to technical knowledge, power is more than a function of material resources such as wealth and military capabilities. Second, because a nation's interests are often unclear under conditions of scientific uncertainty, scientific knowledge may become a significant source of power as it facilitates the clarification of a state's interests. Third, the determination of state interests entails various sub-national processes, so that structural approaches which 'black-box' the state are rendered woefully inadequate to the challenges presented by many of the new issues at hand. Thus, issues that highlight the role of technical expertise require a three-fold revision of neo-realist theoretical assumptions: power must be conceived in terms broad enough to encompass knowledge-based power; interests must be conceptualised as arenas of political struggle to be formulated in light of contending knowledge claims; and domestic political processes should be moved closer to centre-stage.

Because the problems commanding the attention of most students of international relations have been military or economic ones, the literature on the specific role of science in world politics is relatively undeveloped. Implicit in those few theoretical frameworks that are available for analysing the application of expert knowledge to diplomatic relations is the assumption that scientific consensus generates political consensus. The prevailing approaches, including variants of functionalism and the literature on epistemic communities, suffer from a naive view of science as standing outside of politics, of knowledge as divorced from power.

Contrary to the implications of these approaches, I argue, the cultural role of science as a key source of legitimisation means that political debates often are framed in scientific terms; questions of value become reframed as questions of fact, with each confrontation leading to the search for further scientific justification. Paradoxically, the demand for legitimisation results in a process of delegitimisation [Lyotard 1984].

Moreover, facts must be expressed in language and they require interpretation. Some facts may be emphasised and others ignored, depending upon the interests of the communicator and the relevant audience. This is where *knowledge brokers* enter -- as intermediaries between the original researchers, or the producers of knowledge, and the policy-makers who consume that knowledge but do not have the time or the training necessary to pore over the original research. The ability of knowledge brokers, who typically operate at low or mid-levels of governments or international organisations, to frame and interpret scientific knowledge is a substantial source of political power. Knowledge brokers can be particularly influential under the conditions of scientific uncertainty which characterise most so-called technical dilemmas -- that is to say, issues, the problematic natures of which are defined in terms of instrumental or scientific ignorance, uncertainty, containment or management -- of which environmental problems are examples *par excellence*.

The argument that knowledge and interests are mutually interactive is most effectively demonstrated through detailed contextual analysis, and an intensive case study approach is an effective way of exploring such complex sets of interactions. Of course, a single case study cannot prove that a given set of dynamics is the rule, but it can provide a heuristic device for exploring possible conjunctions [**2**]. The Montreal Protocol on Substances that Deplete the Ozone Layer provides an excellent case for contextual analysis because of the pivotal role played by science; in particular, the ways in which scientific knowledge was formed, construed and communicated.

Although the ozone case provides the focus of the thesis, two other transboundary environmental problems are examined in light of these questions about science in politics: acid rain and global warming. My purpose in introducing these other cases is to show that the interactive conception of power and knowledge developed from the ozone case is not peculiar to that issue alone, but in fact is pervasive even under relatively different conditions.

The Montreal Protocol, negotiated in 1987 -- and subsequently amended at Helsinki in 1989, London in 1990 and Copenhagen in 1992 -- under the auspices of the United Nations Environment Program (UNEP) as the first international treaty on a global ecological problem, was the result of close collaboration between scientists and policy makers. Superficially this landmark agreement appears to have been the result of a rigorous process of risk analysis, with sophisticated atmospheric models as the scientific basis of the negotiations. This, essentially, is the thesis of Richard Benedick, the chief negotiator for the US during the Montreal Protocol negotiations [Benedick 1991]. It would be a mistake, however, to believe that science provided a body of objective and value-free facts from which international cooperation emerged. Rather, knowledge was framed in light of specific interests so that questions of value were rendered as questions of fact, with exogenous factors shaping the political salience of various modes of interpreting that knowledge.

The Montreal Protocol process is a rich source of detail for analysing the complex sets of interactions between power and knowledge. Moreover, precisely because it looks like an example of "epistemic cooperation," or consensual knowledge generating political agreement, the ozone case provides an excellent vehicle for the study of other, more counter-intuitive possibilities. Finally, because that treaty is widely considered a prototype for future agreements, an understanding of its evolution is important for making any inferences about policy making under conditions of global ecological interdependence

1.2 Overview of the Chapters

Chapter Two, which builds upon the works of a wide range of thinkers in order to make a case for an interactive conception of power and knowledge, is the most abstractly theoretical chapter of the thesis. Using the widely accepted notions of power rooted in the works of Max Weber and Robert Dahl as a springboard, I argue that limiting power to intentional domination by specific social agents is not broad enough to encompass many of the instances in which knowledge-based power seems to come into play. The most prevalent conceptions of power downplay or ignore issues of legitimacy, consensus, and interpretation -- issues which are fundamental for knowledge-based power. The prominence of knowledge-based power in at least some situations means that conventional materialist notions of power should be expanded to include a more discursive and productive conception of power. Alternative conceptions of power, particularly those advanced by Talcott Parsons, Hannah Arendt and Jurgen Habermas, emphasise these neglected communicative dimensions of power. There is an unfortunate tendency, however, among generative and systemic conceptions of power to sidestep some of the more troubling conflictual dynamics that are central in the conventional account. As more recent work in the philosophy of science and the sociology of knowledge indicates, conflict does not magically disappear in the knowledge production process. In fact, the accumulation of scientific knowledge may be driven more by conflict than by consensus.

The works of some post-structuralists, particularly the later works of Michel Foucault, can help to bridge the gap between conventional and generative accounts of power. Not surprisingly, these works, which consciously link power and knowledge, provide valuable insights into the kind of knowledge-based power that I argue is central in international environmental politics. For Foucault, power is generative, but it is also inherently conflictual, and its dynamics tend to be much more subtle that simple domination or repression. The traditional notion of power as control by specific agents is described by Foucault as "sovereign" power, which he links to pre-modern forms of monarchical power. The kind of power characteristic of modernity, and certainly post-modernity, if such a period can be said to be upon us [Poster 1984], Foucault terms "disciplinary" power. Disciplinary power is a fundamentally linguistic phenomenon, and therefore not primarily material in nature, rooted in discursive practices [Terdiman 1985].

A troubling dimension of Foucault's work, and one that has vexed his critics, is the avowed expulsion of all notions of subjectivity and social agency from his theory of power. I argue, however, that despite his own rhetoric, Foucault's work is more concerned with "decentring" the subject than with eliminating subjectivity altogether. In fact, certain aspects of his theory, such as the notion of resistance and even the concept of discursive practices, are rendered incomprehensible in the absence of concrete subjects engaged in power relations. The decentring of the subject, furthermore, is a useful move in that it highlights the constitution of identity through practices, a key process in knowledge-based power.

The second half of Chapter Two provides a conceptual analysis of scientific knowledge in light of the preceding discussion, an analysis which seeks to avoid the twin epistemological perils of objectivism -- or foundationalism, as it is sometimes called -- and relativism. Much of twentieth century philosophy of science, particularly in the past three decades, has called into question the notion that there exists a solid foundation, whether in the intellectual or the physical realm, upon which objective knowledge can be based. The most extreme interpretations suggest that science is merely epiphenomenal to social forces. Drawing upon the works of Ludwig Fleck and Roy Bhaskar, I argue for a middle path rooted in a realist ontology and a pragmatic, yet hermeneutical, epistemology. While recognising that science is an inescapably social process, my approach also grounds scientific knowledge in the world of extant objects and structures. To that extent, I necessarily adopt a realist ontology. Although their emphases are different, a realist ontology is consistent with a social constructivist epistemology which explicitly links knowledge with power. For philosophers Joseph Rouse, Steven Woolgar and Bruno Latour, science is a deeply practical and profoundly powerful activity which transforms both the world and how it makes itself known.

Science is much more closely related to a Foucauldian conception of disciplinary power than it is to conventional notions of power as control by specific agents. As a cornerstone of modernity, scientific knowledge delimits the boundaries of legitimise discourse. "Regimes of truth" define not only what can be said, but what can be thought. And once the profound links between science and technology are acknowledged, then science can be seen as the enormous source of social power that it is. In recent years, as political problems have become increasingly entwined with questions of scientific evidence and proof, the ability to interpret reality itself has become a major productive source of political power. Thus, science is both a powerful activity in its own right, acting upon the world in many ways (some of which are reasonably well-understood by it practitioners, others not at all), and a source of power for those who would use it either to pursue their own goals or to describe the world.

Chapter Three takes up the more practical question of how science interacts with politics in light of the preceding theoretical discussion. Conventional accounts, I argue, are strongly coloured by an Enlightenment intellectual heritage which wrongly dichotomises knowledge and power. Once science is set apart from social life as a rarefied world of objective facts and unblemished rationality, the road is paved for unrealistic expectations that science can eradicate political conflict. The roots of such thinking run deep, spanning the four centuries from Francis Bacon to the present, and including such diverse political thinkers as the Marquis de Condorcet, Karl Marx and August Comte.

Yet if knowledge and power are conceptually related, as I argue they are in Chapter Two, then the links must be significantly stronger when scientific knowledge is brought to bear on political problems. Such problems, which "hang on answers to questions which can be asked of science, and yet which cannot be answered by science," have been termed "trans-scientific" [Weinberg 1972: 209]. The dynamics of expert advice dominate the power-knowledge nexus for trans-scientific problems, of which environmental problems are a subset. In many respects, policy makers and technical experts inhabit different worlds and speak different languages. Yet they are embedded in complex networks of power with one another, with the authority of each group being highly circumscribed by the authority of the other. At various turns, the fact-value distinction, which is commonly believed to divide these worlds, can be seen to break down.

The ability of science to defuse political conflict has two sources: the grounding of facts in an intransitive ontological dimension, and the *belief,* even if it is not a well-founded one, that science is apolitical. These two factors, however, are greatly diluted in the policy process because power frequently hinges on the ability to manipulate knowledge. In practice, knowledge is framed in light of specific interests, so that information begets counter-information. Interpretation and framing of knowledge become crucial political problems as information is mustered for various policy objectives.

Many of the prevailing contemporary analyses of science in politics suffer from the modern misreading of the relationship between knowledge and power. Since the 1960's, a good deal has been written about "postindustrial," or "knowledge-based," society, and most of it is overwhelmingly optimistic about the ability of science to infuse politics with an unprecedented degree of rationality. Yet, if the arguments developed in Chapter Two and the first half of Chapter Three are persuasive, then one should be wary of such enthusiasm. Turning to the more specific question of science in world politics, where the added dimension of international conflict is generally taken as the overarching systemic feature, a sceptical attitude should be even more appropriate. Yet the only three theoretical approaches to have specifically taken up the question -- functionalism, neo-functionalism, and the epistemic communities literature -- have all adopted a rationalistic stance, emphasising instead the global unifying potential of science.

One can only speculate as to why a field of study in which conflict is so conspicuous would so blithely take such a view **[3]**. There are three possible explanations, and the actual one is most likely some combination of the three. First is the ubiquity of an intellectual legacy which has seen science as representing salvation from politics. Second, the methodological leanings of much of international relations as a discipline are strongly influenced by a quasi-positivist notion of science. So it is not surprising that, when the question of science in politics is addressed, science would be seen as providing an objective and value-free basis for political consensus. Third, given that world politics is so intrinsically contentious, some theorists may unconsciously yearn for some basis for consensus, some common language to make relations more harmonious. **[4]**

Indeed, this has traditionally been a primary project of liberal international relations scholars, and the liberal convictions implicit in the functionalist, neo-functionalist, and epistemic communities literatures are all but transparent. **[5]**

The last section of Chapter Three shows how international environmental problems provide an ideal terrain for tracing the interactional dynamics of science and politics. Particularly as the temporal and spatial scales of these problems expand into global and inter-generational proportions, they are characterised by conditions of high risk in the face of scientific uncertainty. Scientists often become politically important actors because they are the first to discover the problem, and therefore are instrumental in defining both how the problem is conceptualised and what policy options should be addressed. Through such methods as risk analysis, policy makers have sought to sharply delineate science from politics with the goal of providing objective knowledge from which policy decisions can rationally be made. But such strategies rely upon a strict fact-value distinction, a distinction that I have already argued is conceptually suspect in the abstract and of dubious practical value for trans-scientific problems. Nonetheless, because science is a primary source of legitimisation, and because environmental problems are defined to a great extent by scientists, the language of international environmental policy debates can be expected to be flagrantly scientific.

While much of the material in Chapter Four represents a detour in the narrative, it is necessary as historical background for the case study on the Montreal Protocol key terms, such as 'ozone' and 'chlorofluorocarbons,' are defined, and the origins of the relevant scientific and political networks are described. Although the theoretical argument takes a back seat to these informational matters, the main thrust of the argument is not lost. Certain modes of framing the available scientific information had important political implications, even before the international negotiations for control measures got underway. For instance, although there was nothing scientifically improper in doing so, evading the implications of ozone depletion for global climate change in virtually all policy discussions greatly limited the dominant discourse on the problem. Likewise, framing the issue almost solely in terms of skin cancer made sense for an American audience, but it also had important repercussions in the later international debates.

Chapter Five is a detailed case study of how scientific knowledge and political processes interacted in the international negotiations leading up to the Montreal Protocol. The agreement, negotiated by sixty two nations through the United Nations Environment Program (UNEP) in 1987, called for roughly 50% reductions in chlorofluorocarbon and halon consumption by the year 2000. It was immediately hailed as "the first truly global treaty that seeks to anticipate and manage a world problem before it becomes an irreversible crisis" [UNEP Press statement, 22 September 1987].

Without the modelled predictions that CFC emissions would eventually cause ozone depletion, the Montreal Protocol would have been inconceivable. Clearly, scientific knowledge was a necessary precondition for the treaty's negotiation. However, it is one thing to focus on how knowledge facilitated cooperation, and another thing entirely to claim that scientists themselves were the precipitating force behind the agreement. Yet the dominant interpretations seem to dodge this distinction, attributing the political consensus to both a scientific consensus and to the atmospheric scientists themselves [Benedick 1991; P Haas 1990, 1991]. For Benedick, the Montreal Protocol is a prototype for a new form of diplomacy grounded in the international application of risk analysis techniques. For Peter Haas, the treaty was brought about by an epistemic community whose core members were atmospheric scientists.

Both these views conflict with the fact that most atmospheric scientists working on the ozone problem felt that their research did not warrant the virtual elimination of CFCs that the US advocated, a position that was instrumental in shaping the course of the negotiations host of them, in fact, steered away from making specific policy recommendations. In addition, the prevailing analyses fail to distinguish adequately between scientific knowledge and the scientists themselves, thereby downplaying the extent to which information, once produced, is available to a multitude of political actors. Scientists were important actors, but to say that the issue was science-driven is not to say that scientists themselves were the driving force behind the protocol.

The analysis presented in this thesis, based primarily upon original source documents, focuses on the contending discursive practices during the negotiations, and seeks to trace how specific discourses shifted from subordinate into dominant positions. The capacity of scientific knowledge to facilitate international cooperation on the ozone layer, I argue in Chapter Five, was mediated by two crucial factors. First, the science was framed and interpreted by a group of ecologically-minded knowledge brokers associated with UNEP and the US Environmental Protection Agency (EPA). Second, the context of the negotiations, defined largely by the discovery of huge ozone losses over Antarctica, determined the political acceptability of various modes of framing the scientific knowledge. For example, the negotiators explicitly agreed to ignore the ozone hole during their deliberations because its causes were enigmatic. The central fact, though, that the hole was not predicted by computer models, enhanced the credibility of alternative ways of framing the knowledge -- interpretations which advanced the environmentalists' agenda.

Science did not offer a set a objective facts from which a policy consensus evolved. The international scientific assessment that served as the informational basis for the negotiations (WMO/NASA 1986], the rationale for which was explicitly political, was itself amenable to a wide range of interpretations. This, despite the study's relatively narrow margins of uncertainty (of course, the confident tone of the report was due in large part to the fact that the ozone losses over Antarctica, discovered just prior to the report's publication, were not covered in it). Not surprisingly, what was accepted as 'knowledge' was tightly coupled to the political and economic interests of the principal antagonists, the US and the European Community (EC).

During the pre-negotiation workshops, which really set the stage for the later normal bargaining, the US proved to be far more adept than its adversaries at framing its perceived interests in scientific terms. Officials from the EPA skilfully emphasised the long atmospheric lifetimes of CFCs and the long-term modelled predictions in order to shift the context of the debate by extending the relevant time frame well into the next century. The EC, however, stated its position virtually without reference to scientific information. If, as I have argued, science is a primary source of legitimacy, then failing to bolster one's position with scientific language is likely to be an unsuccessful strategy. By the end of the workshops, it was apparent that the eventual outcome would be strongly influenced by the American line of reasoning, despite the fact that the opponents were more numerous and economically stronger.

US support for stringent controls on CFCs, however, was itself a result of a contentious domestic process of inter-agency wrangling. Again, the framing of scientific information was the crucial determinant of the EPA's ability to define the terms of the dominant discourse. The turning point in the inter-agency debates came when certain high-level officials in the Reagan administration publicly attempted to frame the issue narrowly in terms of skin cancer. Although there was nothing scientifically wrong with this position -- indeed, most research on the effects of ozone depletion focused on skin cancer -- environmentalists successfully subjected those officials to intense political embarrassment.

Thus, the international policy process that led to the Montreal Protocol was not characterised by linear movement from scientific consensus to policy coordination. Typical of trans-scientific problems, the process was a far more multi-dimensional one defined by an interactive relationship between knowledge and power, science and politics.

Chapter Six looks at this relationship for two other international environmental problems acid rain and global warming. My main purpose in analysing two other cases is to provide some grounds for testing whether the findings in Chapter Five are exceptional or typical. The level of detail is much lower. Like the problem of ozone depletion, neither of these problems is immediately detectable by the senses; a great deal of inference is necessary, not only for making predictions about the future, but even for understanding the current extent of the damage. Consequently, scientific knowledge is an essential ingredient in formulating adequate policies to address these problems.

There are, however, important differences among the three issues, differences which provide an opportunity for studying how policy makers cope with scientific uncertainty under a range of conditions; but scientific uncertainty is not necessarily an independent variable in these cases, for it can influence how other factors are perceived. The most important differences among the issues are the distribution of costs and benefits for control measures, the diffusion of pollution sources, and the level involvement of by developing countries. Another significant difference is that, contrary to the negotiating process for the Montreal Protocol, the US has not assumed a leadership role in efforts to ameliorate acid rain and global climate change.

Each of the cases analysed provides strong testimony against the epistemic cooperation thesis. For acid rain, a scientific consensus was reached during the early 1970's that sulphur dioxide emissions were destroying Scandinavia's freshwater ecosystems. Yet genuine efforts to control these emissions in Europe were only undertaken in the mid-1980's. The precipitating event was the discovery of massive forest death in Central Europe, a phenomenon which received enormous publicity but had not been conclusively linked to acid rain. For global climate change, scientists generally agree that current levels of greenhouse gas emissions will take the planet into uncharted climatological territory. As a community, they have been outspoken in their support for major policy shifts to mitigate the changes, yet an effective global treaty in the near future is only a remote possibility.

Among the three environmental problems examined in this thesis, climate change is the only one for which developing countries have been key players. The lop-sided distribution of resources that permeates all relations between North and South is, in this case, supplemented by another disparity:

the uneven distribution of informational resources. Almost all of the relevant science is concentrated in three countries the United States, the United Kingdom, and Russia The meagre scientific infrastructure in developing countries means that there is a shortage of local and regional impact studies, a shortage which makes it difficult for these countries to formulate a conception of their own interests on an informed basis. They tend, therefore, to use their lack of scientific information, to justify their reluctance to adopt major policy shifts.

Access to vast stores of scientific and technical knowledge, however, does not guarantee a willingness to support far-reaching action to mitigate global change -- a point epitomised by US policy on the issue. As in the Montreal Protocol negotiations, a group of knowledge brokers had forged the US position, but this time the group was not associated with EPA and the position was a strongly anti-regulatory one. Again, knowledge is an important political tool. But if the ozone case appears to indicate that scientific leadership begets political leadership, the case of global climate change, at least up until now (mid-1995), demonstrates that the two are not necessarily correlated. It also suggests that, when the stakes are very high, the ability of scientists to influence policy is likely to be very low.

The final chapter, Chapter Seven, examines the implications of an interactive conception of power and knowledge for international relations theory. In the first two sections, I argue that none of the conventional theories of regime formation, at either the systemic or the national level of analysis, can account for the processes and outcomes described in Chapters Five and Six. Structural theories, which take interests as given, are particularly unsatisfactory since the processes in question are specifically about the formulation of interests. Hegemonic stability theory, which links regime formation with the presence of a single dominant nation, cannot account for even the broadest outlines of what transpired in the three cases. The notion of a "scientific hegemon" is explored and found to be of limited utility. The various shortcomings of structural approaches accentuate the need for a strategy which emphasises domestic politics and the role of knowledge in the process of interest formation.

Yet the dominant theoretical models which take domestic processes as primary also miss the mark. The bureaucratic politics approach is helpful in accounting for the different policies of the US for ozone depletion and global warming, a difference which is at least partially due to the abilities of competing agencies to achieve dominance. The approach, however, is not helpful in explaining the different views adopted by individuals occupying the same bureaucratic position, or the divergence among agencies with analogous responsibilities in different countries. Interest group models would wrongly predict that industry, the most overtly powerful pressure group, would have prevailed in each of the three cases. The fundamental problem with both these domestic-level approaches, I argue, is that they fail to account for the central role of scientific knowledge in shaping the processes and outcomes in the three cases. More fundamentally, they overlook the importance of cognitive factors in shaping actors' conceptions of their interests and identities.

The third section of Chapter Seven looks first at the burgeoning literature on ideas in international relations, and then turns to the more specific question of how scientific and technical knowledge influence politics among nations. Recalling the discussion about knowledge and power in Chapter Three, I argue that the epistemic communities literature, which represents the most recent attempt to theorise about the place of scientific knowledge in world politics, is of limited service in explicating the three cases. This literature, with its implicit conception of knowledge as divorced from power, tends to assume that scientific consensus engenders more rational and cooperative political processes. Just as interesting, and perhaps even the norm under the conditions of uncertainty that often prevail in environmental decision making, may be *epistemic dissension*. Epistemic community approaches downplay -- almost to the point of neglect -- the ways in which scientific information simply rationalises or reinforces existing political conflicts. Questions of framing, interpretation and contingency are glossed over in an effort to explain politics as a function of consensual knowledge.

In particular, Peter Haas' claim that the Montreal Protocol was the work of an epistemic community is mistaken on three counts. First, he considers such a 'community' to have been composed largely of atmospheric scientists. But, as I argue in Chapter Five, these scientists were largely reluctant to commit themselves to concrete policy recommendations before the causes of the Antarctic ozone hole were understood. Almost none of them advocated the near phase-out of CFCs that was promoted by the knowledge brokers associated with EPA. Second, the epistemic communities approach downplays the framing and interpretation of knowledge. Although a body of consensual scientific knowledge existed [WMO/NASA 1986], the wide range of possible interpretations of that knowledge limited its influence. Third, and partly as a consequence of the second, this approach tends to disregard the contextual factors that condition the salience of various possible interpretations. Thus, in his analysis of the Montreal Protocol, Peter Haas says little about the Antarctic ozone hole, which, I argue, was a key factor in shaping the outcome. If my argument is right, then scientific *ignorance* was at least as important as *consensus* in determining the terms of the debate.

To the extent that the power of social actors is rooted in how they frame and interpret information, then an approach that focuses on discursive practices may be most appropriate. As determinants of what can and cannot be thought, discourses define the range of policy options, thereby functioning as precursors to policy outcomes. I argue for such a discursive practices approach, one that would be highly sensitive to the interactive dynamics between knowledge and power. The power of competing knowledges -- likely to be decisive under conditions of scientific uncertainty -- is the critical issue in the studies analysed in Chapters Five and Six.

While lacking in methodological parsimony, this approach is best able to characterise the complex dynamics of actual negotiating situations. An emphasis on discursive practices, rather than on states, bureaucracies or individuals, would interpret international regimes as loci of struggle among various networks of power/knowledge. Unlike the epistemic communities approach, issues of framing, interpretation and contingency are central to a discursive practices approach, and epistemic dissension is at least as likely an outcome as epistemic cooperation.

The major contribution of a discursive practices approach is its ability to conceptualise power and knowledge as intricately enmeshed. The overarching regulation of the political field by linguistic symbols "transcends the generative and critical capacities of any individual speaker or speech act" [Terdiman 1985: 39]. The power embodied in dominant discourses is most easily characterised in terms of Foucault's notion of "disciplinary power". Discursive power is decentralised, non-monolithic, and rooted in linguistic practices rather than overt control and material domination.

Environmental problems may be viewed primarily as informational phenomena, or as struggles among contested knowledge forms. The Montreal Protocol process, for instance, is essentially the story of how a dominant anti-regulatory discourse was supplanted by a new regulatory discourse. This discursive shift occurred both within the US and internationally during the Montreal Protocol negotiations. The 1986 WMO/NASA report defined the scientific parameters of discourse, yet that document was amenable to a wide range of policy stances. A discursive practices approach focuses on the contextual factors, such as the discovery of the Antarctic ozone hole, that empowered a subordinate discourse.

More generally, a discursive practices perspective on international regimes provides a valuable alternative to both liberal and neo-realist approaches. The former place inordinate emphasis on the communitarian and cooperative dimensions of regimes, whereas the latter, while being more cognisant of the dynamics of power, tend to reduce power to material factors in the possession of specific agents. Only a discursive practices approach offers an understanding of regimes as crystallised embodiments of power and knowledge.

If those scholars who discern a trend toward a "post-industrial" or "informational" world order are correct, then this argument has important implications not just for environmental issues, but more generally for the nature of power in the emergent global system. One trend may be the diffusion of the sovereign power of nation-states to non-state actors and the proliferation of disciplinary micro-powers. Consistent with this diffusion would be the displacement of power toward those actors most proficient at controlling and manipulating informational resources. There are good reasons to believe that, as environmental pressures become more severe and other international problems become increasingly technical, the terms of political discourse will become ever more scientific. Yet the prevalence of scientific discourse should not delude us into the common misconception that polities will therefore become more rational and less conflict-ridden, whether through functional cooperation or through epistemic communities. A profusion of information may, in fact, lead to greater confusion as the world becomes a ubiquitous market for discourses. The scientisation of politics may well devolve into the politicisation of science.

1.3 Methodology

The underlying methodological issue in this discussion is the type of explanation that I am attempting in the case studies. There is much debate among social scientists and historians about the kind of explanation that is desirable, or even possible, in the study of social phenomena. On the one hand, there are those who would seek for the social sciences the same kinds of deductive, causal laws that are purportedly found in the physical sciences. Carl Hempel, who advanced the "covering-law" model of explanation, is probably the most easily identifiable proponent of this school [Hempel 1965]. His work has come under attack, in varying degrees, from others who argue that explanation of social events can never be equivalent to explanation in physics, for instance, because human action is not deterministic and historical events are inherently unique. Some of Hempel's critics have merely softened the covering-law model, claiming that only probable laws or transitory regularities are attainable in the social sciences [White 1965; Joynt and Rescher 1960], or that the Hempelian model is rarely attained even in the physical sciences [Nagel 1952]. Others have gone further, maintaining that history is self-explanatory; knowing what happened is equivalent to knowing why it happened. On this view, historians explain not by making generalisations, but by providing ever greater detail [Oakeshott 1933]. Put more succinctly, "History is a species of the genus 'story'" [Gallie 1968: 105].

This debate underlies a methodological controversy in the social sciences, one which bears directly on the theoretical usefulness of case studies. According to the so-called "nomothetic-idiographic dilemma," or the "richness-rigour debate," as it is sometimes called, theoretical precision and generaliseability are sacrificed in intensive analyses; yet contextual richness is sacrificed in correlational studies of a large number of cases [Russett 1970: 426; Snyder 1984: 89-90]. The easiest and most straightforward response to this alleged dilemma is that such types of studies are necessary and that, in fact, the two are complimentary. This is essentially the tack taken by many of those who have addressed the issue [Eckstein 1974; George 1979; Russett 1970; Snyder 1984]. Of course, another response has already been outlined; a carefully chosen case, and specifically a most-likely case, can have immense theoretical relevance. Thus, a well-designed case study can have it both ways, yielding potentially generaliseable conclusions without forfeiting a rich narrative style.

Historical sociologists also believe that one can have it both ways -- and that, in fact, the best studies succeed in both illuminating the particular while inferring the general. The recent renaissance of historical sociology is, at least in part, a response to the extremes of grand deductive theory and abstracted empiricism that defined the mainstream of Anglo-American sociological research in the post-war era [Skocpol 1984: 2]. Echoing the concerns of Max Weber, historical sociology asks questions about social structures and large-scale processes, while taking temporal sequences and specific contexts seriously as bases for explanation [Weber 1949]. Historical sociological studies tend to emphasise the interplay of intentional action and social structures [Skocpol 1984: 1; Giddens 1984].

Theda Skocpol distinguishes three genres of historical sociology, all three of which lie somewhere between pure deductive theory and idiosyncratic storytelling. Analytic studies explain, typically through induction, well-defined historical outcomes or patterns. As an example of this genre, Skocpol cites her own work [1979]. Interpretive studies use concepts to develop meaningful explications of broader historical patterns. A classic work along these lines is Weber's *The Protestant Ethic and the Spirit of Capitalism.* The third genre, popular during the 1950's and 1960's among sociologists, seeks to apply a universally valid social theory to specific historical events. A recent example of general model application would be Immanuel Wallerstein's *The Modern World-System* [1976]. Table 1.1 locates these varieties of historical sociology in the middle of the spectrum running between idiographic history and abstract deductive theory.

Table 1.1: Some Categories of Qualitative social Science Methodology

			Historical Sociology		
Type:	Idiographic	Analytic Model	Interpretive Theory	Applied	Deductive
Focus:	History as story	Causal links	concepts as guides	Confirm theory	Abstract logic

The research methodology employed in this thesis is most akin to the interpretive genre of historical sociology. The purpose of the three case studies is to illustrate and explore the interaction of specific concepts: knowledge, power and interests. Those concepts are not tightly tied together into a general, overarching theory. The discursive practices approach that I propose in the end is far closer to a conceptual strategy than it is to any sort of universal model. Yet my results are intended to be generaliseable; a multi-dimensional concept of knowledge and power is likely to be useful in interpreting a wide range of cases beyond the particular ones analysed.

The two theoretical chapters, Chapters Two and Three, are largely deductive, proceeding from the question, "If knowledge were to be considered either a source or a kind of power, how would power need to be conceptualised in order to be broad enough to encompass it?" But the chapters' purpose is more to provide conceptual background than to erect anything resembling a universal theory or model. On the contrary, I argue that an understanding of how power operates requires concrete contextual analysis. Nonetheless, the argument in these chapters is capable of standing alone as a piece of analysis.

Properly speaking, the material in the empirical sections of this thesis is historical, although the events are very recent. The contemporary nature of the material carries with it distinct advantages and disadvantages, not the least of which is that each of the cases in question is still evolving, so that there is some sense of arbitrariness in stopping the studies at specific dates, even if it is a practical necessity. The problem of moving targets, however, is not a serious one. For the ozone and acid rain cases, the choices were fairly straightforward because major international treaties served as landmarks: the 1985 protocol to the Convention on Long-Range Transboundary Air Pollution and the 1987 Montreal Protocol on Substances that Deplete the Ozone Layer. Subsequent developments on both issues are covered only cursorily. Because there is no international treaty on the climate change problem, in a sense there is no case. Yet I have chosen to focus on the workings of the Intergovernmental Panel on Climate Change (IPCC) as a case study of the interaction between the realms of science and policy. My choice, I believe, is justified on the grounds of both the unprecedented level of involvement by scientists on a global environmental problem and the case's enormous policy relevance.

Not only does this thesis have it both ways on the richness-rigour debate, it also stands surprisingly firmly on both sides of the theory-policy divide. The agreement of the cases in Chapters Five and Six with the earlier conceptual argument has significant theoretical consequences, which are laid out towards the end of Chapter Seven. The strong case made both analytically and empirically for a discursive practices approach also has important implications for international environmental policy making. Because the Montreal Protocol has been widely upheld as a prototype for a global climate change treaty, the analysis offered here is particularly relevant for emerging policy problems. Policy makers and their observers should not expect science to offer an endless supply of objective, value-free facts on demand, from which a political consensus can be formulated, for information is typically framed in light of specific interests and perspectives.

Virtually all of the research material cited in this thesis was made available to me in 1992 while visiting Stanford University. At the time I accepted a windfall of valuable data with hardly an idea of how I would use it other than for enlightenment. It was only in the months following my return that I turned to the material and fairly extensive notes written in spurts of inspiration over the following year about environmental matters, in particular, the scientific background to the Montreal Protocol on stratospheric ozone depletion.

I began my thinking on this thesis with the tentative hypothesis that the Montreal Protocol in specific, and international cooperation on environmental problems in general, were the result of efforts by epistemic communities to forge a political consensus on the basis of scientific information. These problems, I believed, demanded collaboration among scientists and policy makers, and scientific knowledge could provide the common ground that might otherwise be lacking among competing nations. Knowledge could furnish the means for reorienting actors' conceptions of their own best interests. In short, like so many others, I was beguiled by the Enlightenment faith in the ability of science to make polities more rational and cooperative. Given the intuitive appeal of the epistemic cooperation theory and the superficial resemblance of the Montreal Protocol process to the procedures of risk analysis, my delusion is not surprising.

Had my original hypothesis been confirmed, I could have at best told an interesting story with little theoretical value. The Montreal Protocol is a "most likely case," meaning that it would be expected to conform to the epistemic cooperation hypothesis [Eckstein 1974]. So no reliable generalisations beyond this individual case could have been made. My study might have answered the question, "*How* did an epistemic community operate in this particular instance?" but it would have been of little service in ascertaining *whether* the application of science to political problems tends to precipitate more cooperative solutions.

Fortunately, during a period spent at Stanford University in mid-1992, in my capacity as Senior Research Scientist with the Faculty of Design, Architecture and Building at the University of Technology, Sydney, I met with Dr Anthony Surtees, an Australian Research Fellow at Stanford. Among other things, Tony and I were working on a joint research proposal regarding an international environmental database using Australian-developed artificial intelligence software. In the course of discussion I mentioned to Tony that I was interested in the events leading up to the Montreal Protocol, in particular the roles played by key scientists in the US and elsewhere in shaping political and public opinion. Needless to say, in this as well as every other matter I've ever raised with Tony he had something intelligent to say -- only more so. Tony was good friends with at least three of the key US scientists who had helped shape official US policy on stratospheric ozone depletion, *and* he had access to virtually all of the (available) seminal literature. One day after asking, I was given access to no less than a library of data on the subject, the services of an energetic research assistant, and a dedicated photocopier. Within a week I had harvested what would otherwise have taken years to collect. For this and so many other gems and kindnesses I dedicate this work to Tony's two baby daughters, Nadja and Cara.

As I read the source documents from the Montreal Protocol process I began to understand that more complicated dynamics were involved than I had originally assumed. It became increasingly evident to me that 'knowledge' in the environmental sphere is not simply a body of concrete and objective facts, but that accepted knowledge is deeply implicated in questions of framing and interpretation, and related to perceived interests. The disconfirmation of my initial hypothesis required some psychological adjustment, but in the end gave me a far more theoretically interesting research agenda.

Indeed, what was at first merely the product of personal and academic interest had become a *de facto* research program.

The evidence against the epistemic cooperation theory in the Montreal Protocol case study, supported by two other case analyses, is fairly damning. Had I stopped at that point, the following would have made a contribution by challenging what has become the dominant theoretical perspective on the problem of how knowledge influences international relations. But the project would have been very short. I chose to press on with the further question: If the epistemic cooperation thesis is deficient, then what other theoretical tools might be more useful? The cases cried out for a more interactive and multi-dimensional conception of knowledge-based power, one that I have begun to develop through a discursive practices approach. Thus, the purpose of the case studies shifted from testing the epistemic cooperation thesis to forging alternative theoretical approaches. In other words, the research began unintentionally as a crucial case study, with the Montreal Protocol being a most-likely case for an epistemic communities approach, and developed into a series of heuristic case studies to trace the outlines of a discursive practices approach. Heuristic case studies are "used deliberately to stimulate the imagination toward discerning important general problems and possible theoretical solutions" [Eckstein 1974: 104]. The kind of research design that might be generated to test a discursive practices approach, however, is inherently lacking in the kind of methodological rigour typically thought to be imperative in a well designed inquiry. By specifically conceptualising knowledge as being in an interactive relationship with interests and power, this approach refuses to accept the disciplinary canon that dependent and independent variables must be clearly distinguishable. Instead, it contributes a virtually seditious representation of *interdependent* variables.

Furthermore, a discursive practices approach, with its resistance to unidirectional causal explanations, offers little in the way of methodological tidiness. Sometimes scientists can shape policy, and sometimes they cannot; sometimes consensual knowledge engenders a policy consensus, and sometime it does not. Only a detailed contextual analysis can explain how a particular discourse comes to be persuasive. The requirement of contextual analysis dictates that the research methods be qualitative, not quantitative. A narrative style is most appropriate for the case studies found in Chapters Five and Six, with an exegesis of the interplay of knowledge, interests and power embedded in the accounts. The method of process tracing [George 1979: 56; Aggarwal 1985: 16-19] is ultimately a highly disciplined and conscientious way of telling a story.

The case of acid rain and global climate change are different from the ozone case in enough respects to allow for some worthwhile focused comparison among them [George 1979]. In only one case, for instance -- acid rain -- was a comprehensive international scientific study not available, yet nations were able to agree on tangible pollution control measures. In the case of global warming, such a study is available, but no treaty has been negotiated. Therefore, we can conclude that an international scientific consensus is of questionable value in generating a policy consensus.

A second area for focused comparison is the presence or absence of a widely publicised environmental crisis. In the two cases in which there was such a crisis, even when the events had not been conclusively linked by scientists to the problem at issue, a treaty was negotiated. In the one case for which there has been no such crisis, the global climate change problem, there has also been little progress toward an agreement. Moreover, the process traced in Chapters Five and six substantiates the view that the emergence of a perceived crisis opens up a space for the introduction of alternative discourses.

CHAPTER TWO
CONCEPTUALISING POWER AND KNOWLEDGE

2.1 Aspects of Power

Power is undoubtedly the central concept in the study of politics, yet there is no consensus on what it is, much less how to measure it [March 1966]. One often has an intuitive sense of power and when it is at work, but has trouble conceptualising the intuition. One way to proceed is to start with simple definitions, extract their essence, and gradually construct a consensual, compound model. For instance, we want to say that scientists can and do exercise power under some circumstances, as in the Montreal Protocol process, so we need a notion of power that includes access to knowledge, the importance of consensus, and the ability to persuade. We may also want to say that scientists exercise power even if they deny it, either sincerely or not, and claim to be totally apolitical, so we need a definition that can include both the covert and the unintentional exercise of power.

Such an eclectic approach does not entail tailoring the definition to suit the case; it merely ensures that the definition is comprehensive enough to cover the concept and its manifestations in general. This approach is reminiscent of Ludwig Wittgenstein's analysis of language games [1953]; it seeks to determine the overlapping sets of family resemblances that exist among different uses of the term 'power'. Such an abductive approach also has an advantage over deductive approaches in that it constantly checks its conjectures with concrete situations, rarely drifting too far from experience. Moreover, by building, rather than simply applying, a conceptualisation, we can locate it within the general literature on power.

Power may be defined generically, from the Latin *potere*, as the ability to act or produce an effect, a definition that includes everything from the power of a steam engine to a nation's ability to wage war. While this is an obvious starting point, it offers little insight into *political* power. Later, however, I will show how this rudimentary definition can shed light on the interaction of power and knowledge. For now, though, I want to clothe a naked definition by adding that not just any action counts; walking into a room, for example, devoid of any import, does not count. An action must occur in a political context -- that is to say, within the context of the discussion and practice of the distribution of resources in society -- which includes (but is not limited to) actions taken by and within states.

Perhaps the most common definition of power in contemporary political science is that proposed by Robert Dahl: A has power over B to the extent that he can get B to do something that B otherwise would not have done [1957: 203].

One problem with this definition, which identifies power with control over behaviour, is that one must know what B would have done otherwise and then somehow measure 'the extent' to which one behaviour differs from the other. Max Weber's earlier, influential notion of power is similar, but does not fall victim to this criticism. He defines power as:

> The chance of a man or a number of men to realise their own will even against the resistance of others who are participating in the action [1946: 180].

By looking at power in terms of the *capacity* to impose one's will on others, rather than its actual imposition, Weber avoids identifying power with outcome. Of course, if ability remained a mere potential and were never exercised, then one would be hard-pressed to verify the existence of a power relationship. Another advantage to Weber's conception is that it brings in the notion of resistance, a subject to be addressed later in the thesis.

Weber's definition, however, is problematic in that, by placing will at the centre, it covers only intentional actions. Weber is not alone in committing this error; Bertrand Russell defines power simply as "the production of intended effects" [1975: 251]. The issue of unintentional consequences of action is fundamental to structural notions of power [1]. Kenneth Waltz, for instance rejects Dahl's definition of power on the grounds that "to measure power by compliance rules unintended effects out of consideration, and that takes much of the politics out of politics" [1979: 192]. While Waltz is mistaken in claiming that unintended effects are incompatible with Dahl's definition (for compliance can be brought about unintentionally), he does make an important point. Whatever its other flaws, his *structural* realism is based on the valid observation that "the common relational definition of power omits consideration of how acts and relations are affected by the structure of action" [1979: 192]. Behavioural and individualistic notions of power ignore the fact that social structures themselves can exert power.

Although Waltz has in mind the structure of the international system, the notion of structural power also can contribute to our understanding of knowledge-based power. The ability of scientists and other advisers to influence policy decisions derives from their location within institutionalised structures of knowledge production. They may exercise power, despite their intentions and declarations to the contrary, simply by virtue of conveying information at critical junctures in the policy process. I am not making the foolish claim that scientists rarely consciously influence policy, for recent history is replete with evidence to the contrary, from Albert Einstein's famous letter to President Roosevelt to Edward Teller's vocal support for space-based defence. Rather, I maintain that experts often exert power without intending to, an important claim given the ostensibly apolitical stance of most scientists. In the context of the Montreal Protocol, for example, many of the atmospheric scientists involved claim that they had nothing to do with the political process. I argue, however, that whatever their intentions, they did exercise power if for no other reason than by virtue of their ability to interpret data and word their conclusions in certain ways. This kind of knowledge-based power points to the limitations of individualistic and behavioural notions of power offered by Dahl, Weber and others, for at least some of the outcomes were shaped by the institutional structures in which the process occurred.

Despite Waltz's emphasis on the structure of action, his notion of power ends up being not much different from that of other realists, with the addendum that it may be exercised unintentionally. Among realists, power is typically taken to entail domination and control, it is always measured in relative rather than absolute terms, and it is usually materially-based. Klaus Knorr, for instance, separates economic exchange from power and politics, thereby defining power as a zero-sum game [1975: 135]. Waltz declares that "an agent is powerful to the extent that he affects others more than they affect him" [1979: 192]. For Waltz, power is defined in terms of the distribution of capabilities rather than inferred from the outcomes one may or may not achieve.

There are obvious difficulties in applying these conceptions to knowledge-based power. First, if one is persuaded to act differently on the basis of new information, it is not clear that one has been dominated or controlled. Second, while one could claim that some experts have greater power relative to others, whether on the basis of their superior professional reputations or their stronger relationships to policy makers, the notion of relative power sheds little light on the concept of knowledge-based power. Third, while some states and individuals may have measurable access to more and better information, the intangible nature of ideas means that knowledge-based power is not easily quantified and does not fit easily into a distribution of capabilities framework. Fourth, a major consideration in the operation of knowledge-based power is not so much to assess *how much* actors are affected, but *how* they are affected. Identities and interests are themselves necessarily implicated in knowledge-based power [2].

To further contrast knowledge-based power with realist conceptions of power, it is useful to consider further how they relate to the issues of domination and interest. Hans Morgenthau offers a definition representative of realist thinking, essentially identifying power with domination:

> Power may comprise anything that establishes and maintains the control of man over man. Thus power covers all social relationships which serve that end, from physical violence to the most subtle psychological ties by which one mind controls another [1985: 11].

While this definition no doubt covers many power relationships, it does not so obviously apply to the knowledge-based power of persuasion. Yet Morgenthau allows that power may take many different forms, depending upon historical, political and cultural context. He states:

> Realism assumes that its key concept of *interest defined as power* is an objective category which is universally valid, but it does not endow that concept a meaning that is fixed once and for all [1985: 10, emphasis added].

In the abstract, this definition seems to be broad enough to cover knowledge-based power, but only if persuasion is considered a subtle psychological method of control. Perhaps a starting point for a revised realist conception of power might be E. H. Carr's notion of power as control over opinion [1964] or Arnold Wolfers' distinction between power and influence [1962]. Both of these, however, place knowledge-based power in a decidedly secondary position in relation to physical power, particularly military might, and remains an under-developed concept.

A greater problem, however, is with the identification of power as the capacity to realise interests. Realists who argue that states, by definition, act to enhance their national interests run the risk of *post hoc* reasoning [Morgenthau 1951; Krasner 1978]. On this view, interests are surmised by observing the behaviour of states [3]. But instrumental action implies goal-oriented behaviour, and only a contextual analysis can reveal what goals are being pursued and why. Realists neither explain adequately how interests are formulated, nor provide a method for determining when states pursue long-term rather than short-term interests. The dilemma is especially relevant for global environmental problems, where interests in the two time frames are typically pitted against one another.

The problem of how to determine interests independently of action and outcome is a central issue in the community power debates [4]. In his influential work, Steven Lukes [1974] distinguishes between the one-dimensional and the two-dimensional conceptions of the relationship between power and interests. In the former, interests are revealed preferences, manifest in an actor's ability to prevail over the contrary interests of others [Dahl 1957]. On this view, actors are thought to be conscious of their actual interests. Under the two-dimensional conception, advanced by Peter Bachrach and Morton Baratz [1970], power may also be exercised by controlling the agenda and the very terms of discourse. For them, interests are expressed covertly as well as through revealed preferences, requiring some degree of interpretation on the part of the social scientist. Joseph Nye, recognising the changing nature of power in post-industrial world politics, applies this second dimension to contemporary international relations. Nye contrasts the more traditional "command power," or the ability to influence other states' actions either through inducements or threats, with "co-optive power," which rests on the ability of a nation to set the political agenda based on the appeal of its ideas or culture [1990: 31].

Lukes goes further, distinguishing a three-dimensional view, under which power operates to determine people's interests in ways which conflict with their actual interests. For Lukes, power is only exerted *contrary* to the interests of those on the receiving end. On this view, the 'true' interests of those subject to the power of others are never expressed, so the analyst can only surmise what such interests might be.

The ability of power to shape interests is a key element of knowledge-based power. The views discussed above, however, all link power with domination and control. Such an association is problematic in situations where an actor is persuaded to revise her conception of her interests through convincing evidence or reasoning. One would want to say that power was at work in such a situation, yet one would also want to question the repressive, even malevolent, connotations implicit in most definitions or power [5]. Rather, power would be conceived as potentially generative and rooted in the self-understandings of the people involved in the relations of power. Brian Fay depicts power as dyadic, "arising out of the interaction of the powerful and the powerless" [1987: 117]. While one certainly would not want to say that the policy maker is 'powerless' relative to the experts advising him, Fay's dyadic conception of power can contribute to an understanding of this power relationship. Fay modifies Dahl's definition as follows: A exercises power with respect to B when A does x, a causal outcome of which is that B does y which B would not have done without the occurrence of x [120].

While there are problems with this definition, some of which have already been discussed, it offers a good starting point for classifying various forms of power. Fay distinguishes between *force*, where A removes from B the effective choice to act otherwise; *coercion*, where A threatens to deprive B; manipulation, where B does not know that A is doing x; and *leadership*, where B accepts A's right to require y because of A's authority. The last of these, leadership grounded in legitimacy, is most closely related to knowledge-based power and can include situations in which B is persuaded by A to do or believe y.

The question of political legitimacy crops up frequently in the study of political theory and domestic politics; it seems out of place, however, in a discussion of world politics. This is especially true in realist theories of international relations, which link power with domination and characterise the international system as one dominated by the principles of anarchy and self-help [Waltz 1979; Grieco 1988; Knorr 1975; Smith 1982]. On this view, the normative consensus prerequisite to the possibility of legitimise international institutions does not exist. The Grotian tradition, a minority position in recent years, holds that the international system functions according to at least some norms, beliefs and rules held in common by national actors [Bull 1977; Rosenau 1973]. But theorists rooted in the Grotian tradition typically focus on norms of diplomacy and international law, and do not address the issue of scientific knowledge as a source of legitimisation [6]. A key question, then, is the extent to which knowledge can serve as an instrument of legitimisation and springboard for consensus, in the international arena. If confidence in science is a hallmark of the modern era, as I believe it is, then scientific knowledge can be expected to facilitate cooperation. But if the production and interpretation of scientific knowledge is an unavoidably political process, as I also believe, then knowledge may feed into new or existing arenas of contestation.

Fay's category of leadership, more than other forms of power, is fundamentally consensual [7]. It is also furthest removed from the notions of power as force and coercion which dominate international relations theory. Although the notion of leadership is typically applied to political leaders, as in Weber's works, it also can contribute to an understanding of the role of experts in the policy arena.

Weber adds the notion of political power, or leadership, to his notion of power in general; political power is legitimised power, entailing the capacity to impose one's will on others because of certain normative beliefs they have [1946: 78-79]. I have already noted the problems involved in Weber's basing his definition of power on will or intentionality, but his concept of legitimacy adds to our understanding of knowledge-based power. In fact, while Weber refers to the "legitimisations of domination", the very legitimacy of this sort of power relation is what makes it difficult to fully accept it as domination. Moreover, the mechanisms of legitimisation that Weber proposes (tradition, charisma, and rational legality) do not explicitly include scientific legitimisation, although any of these mechanisms may apply to science in certain contexts. What is important, though, is that the power of scientists and other sorts of advisers, inasmuch as they do not directly decide policy, is rooted in the normative beliefs of policy makers. This observation holds not just for policy issues, but for any situation involving expert advice. A doctor, for instance, may be exercising a form of power when she prescribes a certain medicine for me, but it is hard to maintain that she is dominating me. In this sense, scientific legitimacy is similar to political legitimisation, and to this extent the two refer to a particular notion of power that has little to do with overt domination.

Another aspect of power that also has little in common with domination, and can even be interpreted as its antithesis, is empowerment. Here, a group comes together, gains an understanding of its own interests, and works together to achieve them [Fay 1987: 130] [**8**]. Such a collective and, in Fay's mind, positive conception clearly applies to some situations in which we want to say power is at work; social movements such as the trade union movement come readily to mind. Social empowerment may also involve knowledge-based power, for people can be educated as to a new conception of their best interests. Thus, scientific knowledge can inspire a wide range of collective action [**9**]. In his critique of power as defined by Weber and C. Wright Mills [1956], Talcott Parsons offers some related insights [1963: 1967]. He argues against their 'zero-sum' concept of power, whereby power is always exercised in the context of mutually exclusive interests. Rather, he defines power as:

> the generalised capacity to serve the performance of binding obligations by units in a system of collective organisation when the obligations are legitimised with reference to their bearing on collective goals [1963: 237].

Parsons suggests that power is "generated" by a social system, just as material wealth is generated within an economic system. He takes this analogy very seriously, arguing that both money and power are "circulating media," having value only insofar as they are recognised by members of a system as a mode of exchange. That is, power is a collective phenomenon rooted in the normative beliefs of actors in a social system. Parsons characterisation of power as generative, systemic, and authoritative has obvious appeal for our search for a notion of power comprehensive enough to include knowledge-based power.

Parsons, however, merely reiterates at the systemic level the teleological concept of power, eg., the capacity to realise goals, that Weber observes among individuals. What is lacking in Parsons' account according to Hannah Arendt [1969] and Jurgen Habermas [1977], is a communicative model of action rather than an interest-based model. While Arendt and Habermas affirm the importance of legitimisation, for them power is more the ability to act in concert through consensual communication than the ability to obtain goals by mobilising resources. This notion of communicative power aimed at agreement is relevant to a discussion of knowledge-based power. The production of knowledge is itself a fundamentally consensual activity, although Habermas and Arendt miss the pervasive element of struggle. In the context of international technical issues, the translation of knowledge into policy requires, among other things, substantial consensus.

The communicative notions of power proposed by Arendt and Habermas, however, differ in important respects. For Arendt,

> [p]ower corresponds to the human ability not just to act but to act in concert. Power is never the property of an individual; it belongs to a group and remains in existence only so long as the group keeps together [1970: 44].

Power here is an end in itself, both stemming from and ensuring the continuation of people's communicative action. This notion, like Parsons', emphasises the generative and legitimise aspects of power but, also like his, ignores the dimensions of power as domination and coercion. Recalling Brian Fay's typology, we may say that Arendt's definition can at most incorporate the fourth dimension of power as leadership, and even this is questionable since she claims that power is never an attribute of individuals. Habermas also puts forth a communicative notion of power, but without the Aristotelian underpinnings of Arendt's account. He agrees with her that political power, by which he presumably means governments, can survive only so long as they are recognised as legitimise. He disagrees, however, with Arendt's view of political structures as consensual. He adds the notion of structural violence which, like Lukes' third dimension of power, operates at the level of ideology and interests.

> [Structural violence] blocks those communications in which convictions effective for legitimisation are formed and passed on. . . . In systematically restricted communications, those involved form convictions subjectively free from constraint, convictions which are, however, illusionary. They thereby generate a power which, as soon as it is institutionalised, can also be used against them [1977: 20].

Much of Habermas' work is concerned with explicating these blocks to communication and exploring the conditions under which true communicative rationality might be possible [1978: 1981]. In the context of international relations, not to say any aspect of social life, an important question is whether such conditions can ever be attained. Within the context of science, the question is to what extent such "communication blocks" inhibit the rationality of scientific consensus, and how similar blocks might distort the translation of knowledge into policy decisions. Nonetheless, the conception of communicative power proposed by Habermas and Arendt might serve as an ideal toward which both scientific and political negotiations can strive.

The notion of power as generative and systemic is also found in the writings of Foucault, yet his conception is not nearly so optimistic [**10**]. For Foucault, power is omnipresent; one is never outside it [1980: 141]. He opposes what he sees as an Enlightenment belief in the possibility of emancipation from power relations, a belief he attributes to both liberal and Marxist thinkers. He speaks of the "productivity" of power, "networks" and "webs" of power, and a "microphysics" of power [1979; 1980a; 1980b; 1983]. Rejecting the conventional wisdom that the most visible expressions of power are the most important, he focuses more on the subtle workings of power than with power as repression or domination. He asks:

> If power were never anything but repressive, if it never did anything but say no, do you really think one would be brought to obey it? What makes power hold good is simply the fact that . . . *it traverses and produces things, it induces plurality, forms knowledge, produces discourse* [1980b: 119; emphasis added].

Yet he does not disregard the reality of domination, noting that,

> it is always exerted in a particular direction, with some people on one side and some people on the other [1977: 213].

Believing that power should be studied at its "extremities," Foucault examines those social relations to which the discourse of power traditionally has not been applied, eg., schools, hospitals, confessionals, etc. To these, I would add the social institution of science and all of the networks through which science is applied to political problems.

Foucault traces an historical shift from what he terms the "sovereign" form of power and display exercised under monarchy to a more discursive form of "disciplinary" power typical of modernity [1979]. He opposes the notion of power as exerted by autonomous agents imposing their sovereign wills, a view that he identifies as a throwback to pre-modernity. Foucault would include the definitions given by Weber, Dahl and Morgenthau as instances of sovereign power. At its most fundamental level, disciplinary power acts upon the body. This is the focus of Foucault's two major works of the 1970's, *Discipline and Punish* and *The History of Sexuality, Volume I.* He is concerned with how "disciplinary power" and other "technologies of power" operate on bodies, how discourse makes the body the object of knowledge and invests it with power [1980a: 92-3]. Because individuals are themselves the effects of power, becoming so entwined in networks of power that they emerge as both agent and victim of social control, there is no autonomous subjectivity for Foucault. Knowledge is intrinsic to these networks of power: "between techniques of knowledge and strategies of power, there is no exteriority. 'Truth' is a system of ordered procedures for the production, regulation and circulation of statements" [1980a: 133].

The Panopticon, Jeremy Bentham's design for a model prison, epitomises Foucault's notion of disciplinary power [Bentham 1791]. The cells of the Panopticon are arranged in a ring around a tower, which has wide windows that open onto the inner side of the ring. Each cell along the periphery has two windows, one facing the tower and the other facing outside, producing an effect of backlighting to make the prisoner visible within the cell. Each inmate is constantly visible from the tower, but isolated from other inmates. The effect is "to induce in the inmate a state of conscious and permanent visibility that assures the automatic functioning of power" [1979: 201]. Prisoner becomes jailer; subject becomes object.

Although the Panopticon may represent an extreme, the main point is important: that the effects of power may be internalised, thereby yielding a form of contingent subjectivity. As Foucault recognises, power need not embody only the negative qualities of the Panopticon; consistent with "the productivity of power" is the construction of knowledge and discourse. While the same dynamics of internalisation and normalisation apply to knowledge production and its application to policy decisions, the insidious overtones may be absent. In fact, while Foucault often speaks critically of the workings of power, an agnostic approach is actually more compatible with his general outlook. If power is genuinely productive, then the most appropriate attitude should be the suspension of judgment as to its value and significance until one has evaluated the particular situation. This would be more consistent with Foucault's denunciation of liberal and Marxist attempts to escape from power. If power is truly omnipresent then, unless one is prepared to adopt a stance of unmitigated pessimism regarding social relations, its effects cannot be universally negative, and observation consistent with Foucault's critique of power as repression. Thus, we may suspend judgement and speak of scientific knowledge as a product of power, a possibility to be explored in the next section.

Mark Poster argues convincingly that, as the "mode of information" displaces the "mode of production" in post-industrial society, Foucault's analysis of "knowledge/ power" is becoming more relevant than conventional materialist notions of power. He observes that

> knowledge and power are deeply connected and their configuration constitutes an imposing presence. . . . The form of domination characteristic of advanced industrial society is not exploitation, alienation, repression, etc., but a new pattern of social control that is embedded in practice at many points in the social field and constitutes a set of structures whose agency is at once everyone and no one [1984: 78].

Without affirming the emergence of 'post-industrial society,' one can at least acknowledge that Foucault's analysis offers insights into those social practices involving discursive power.

Poster's allusion to the problem of agency, however, points to what many cite as a basic flaw in Foucault's work: a total rejection of the subject is highly problematic [Habermas 1981b; Taylor 1984]. If the subject is wholly a product of power, then she has *no* clear interests, nor has she any basis upon which to confront power. In Foucault's words, "there is no position of exteriority for subjects" [1972: 121]. As one critic succinctly puts it, it seems to be that "Foucault gives us a hermetically sealed unit; a domination that cannot be escaped" [Philp 1983: 40]. This deep pessimism about the possibility of human emancipation, along with his view of power and resistance as local rather than universal, stem from Foucault's disappointment with Marxist-inspired social movements. While Foucault rejects the discourse of liberation, he frequently speaks of resistance, particularly in his later writings. The concept of resistance, it seems, is our only deliverance from "a hermetically sealed unit." Yet Foucault never offers a rationale for resistance, simply asserting its coexistence with power. Obliquely, he tells us that:

> [there] are no relations of power without resistances; the latter are all the more real and effective because they are formed right at the point where relations of power are exercised; resistance to power does not have to come from elsewhere to be real, nor is it inexorably frustrated through being the compatriot of power. It exists all the more by being in the same place as power; hence, like power, resistance is multiple and can be integrated into global strategies [1980b: 142].

In contrast to the Enlightenment dream of universal emancipation, Foucault's concept of resistance as ubiquitous yet local rings true, while its abstractness raises doubts about the ontological status of the subject. If people are wholly products of power relations, then who can resist? Power and resistance together take on the demeanour of a "hermetically sealed unit". Ultimately, with no theory of action Foucault falls into a deep structuralism. Without subjects or interests, there is no rationale for resistance, and certainly no reason to prefer one form of it over another. As one critic suggests, Foucault's support of resistance is so blind and undiscriminating as to seem politically irresponsible [Connolly 1983b: 332]. Beyond the failure to advance normative grounds for action seems to lie the larger failure to produce a theory of social agency. Anthony Giddens puts it bluntly: "Foucault's 'bodies' are not agents" [1984: 154].

Foucault's own rhetoric, however, is misleading; in fact, his own model of power *requires* human subjectivity. The Panopticon, for instance, only functions because of the prisoners' consciousness. Even language, probably the most all-encompassing model of power [1973], does not *determine* all of our thought and actions, though it may circumscribe them. Foucault's goal, I believe, is to *decentre* the subject, which he succeeds in doing, not to eliminate it. Agents exist, but they should be seen as the effects rather than the fountain of power. In his own conception of power as tied to agency, Giddens brings us back to our primitive notion of power as the ability to produce effects. He regards power as generative and ubiquitous, as do Parsons and Foucault, but spurns both Parsons' reduction of power to consensus and Foucault's denial of subjectivity. Giddens' theory of structuration can supplement Foucault's conception of power/knowledge, yielding a form of "contingent subjectivity," a phrase Foucault himself often invokes. While Giddens distances himself from all versions of voluntarism, since all social action is embedded in and emergent from social structures [1984: 3], his notion of agency nonetheless implies the capacity to "have done otherwise" [1977: 346] **[11]**. The inclusion of a distinct conception of agency allows Giddens to avoid a serious flaw in Foucault's works. The Montreal Protocol, for example, was defined neither by inflexible structures nor omnipresent power without subjects; certain people were absolutely central. Yet, following Foucault, I do not see these actors as autonomous agents wielding the power of discourse on behalf of transparent interests.

In his theory of structuration, Giddens defines power broadly as the "transformative *capacity* of human action; the capability of human beings to intervene in a series of events so as to alter their course" [1977: 348; emphasis in the original]. Intentionality may be associated with this kind of power, but it need not be. Likewise, domination may be entailed, but need not be. The effects of power may be either positive or negative, depending upon context and values. For all of these reasons, Giddens' conception can accommodate a broad spectrum of power relations, including knowledge-based power.

Moreover, it is embedded in a theory of structuration which avoids the twin perils of voluntarism and structuralism **[12]**. Social structures are historically contingent, being reproduced and shaped through the actions of social agents who are conditioned by structures in which they find themselves. Despite their mutual embeddedness, agents and structures do not constitute "a hermetically sealed unit," for they are distinct. This approach offers clear advantages over those granting ontological primacy to either structures or agents. For example, the emphasis on agency avoids the tendency of Realist and Marxist structuralists to reify structures; structures are *constraints* on actors, not actors themselves. On the other hand by locating actors, including their desires and beliefs, within structures, structurationists problematise interests. This is important if we want to avoid the assumption implicit in most of international relations theory that interests are straightforward keys to the concept of knowledge-based power is the possibility that actors can be led to revise their self-understandings.

Stewart Clegg has proposed a useful typology of theories of power along two axes. One is the dominant trajectory, which extends from Hobbes to Lukes and encompasses Marx, Weber and Dahl rooted in analogies drawn from classical mechanics, power here is exerted by a sovereign will over the will of others. At its most subtle, sovereign power defines the thoughts of others, as in Marxist conceptions of false consciousness [Lukacs 1971] and Lukes' third dimension of power. To the extent that Giddens defines power solely in terms of agency, his work may also be placed along this axis [Clegg 1989: Chapter Six] Clegg's second trajectory, running from Machiavelli to Foucault, sees power as facultative, strategic and contingent. Without abandoning the concept of agency altogether, as Foucault seems at times to do, I have found it useful to draw heavily on the second axis in formulating a discursive conception of power. knowledge structures the field of power relations through linguistic and interpretive practices, through organisational strategies, and through the contingencies of local contexts.

2.2 Conceptualising Knowledge

For two reasons I will not go into great depth in exploring the nature of knowledge. First, the complexity of the issues and the abundance of controversy is far beyond the scope of this thesis. Second, my primary interest is in the political dimensions of knowledge, not in epistemological questions per se. Consequently, I will allude to many arguments in the philosophy of science without developing them in their entirety.

I am interested in how and why the producers and communicators of knowledge are able to exercise power, and the answers to these questions have at least something to do with what knowledge is. Without addressing this issue explicitly, I might be perceived as implying that the authority of scientists is no different to the authority of either priests or dictators **[13]**. While these forms of authority may overlap at times, I believe that an explication of the nature of scientific knowledge can point to some important differences. Moreover, in terms of the overall cohesiveness of my approach, I hope to outline an image of knowledge that is consistent with and complementary to my analysis of power in the previous section, showing how the two may be interdependent rather than necessarily unrelated. Throughout most of the modern era, the validity of science has rested on its supposedly increasing access to objective and irrefutable truth. Richard Bernstein defines objectivism as:

> the basic conviction that there is or must be some permanent, ahistorical matrix or framework to which we can ultimately appeal in determining the nature of rationality, knowledge, truth, reality, goodness, or rightness [1985: 8].

The two primary traditions in Western philosophy, rationalism and empiricism, share a basic commitment to an Archimedean point from which objective knowledge is thought to be attainable, whether through the epistemological levers of 'universal' reason or 'unbiased' observation. The former is expressed in the Cartesian and Kantian traditions; the latter in the positivist tradition of the British empiricists and, later, August Comte. Probably the most sophisticated attempt to forge "a permanent, ahistorical matrix" has been logical positivism through its 'physicalist' language. The logical positivists claimed to have articulated "absolutely fixed points of contact between knowledge and reality" [Schlick 1959: 226]. But their physicalist language was fraught with confusion and impracticality. Responding to the inadequacies of logical positivism, Karl Popper [1972] defended "objective knowledge" through his doctrine of falsification, which has been abundantly criticised by Thomas Kuhn [1962], Imre Lakatos [1970], and Paul Feyerabend [1975], among others. The strongest basis of these criticisms, is the "theory ladenness of observation," which strikes at the heart of the entire positivist tradition. This, of course, opens the door to relativism since 'objective' empirical knowledge requires the possibility of unhindered observation. Despite the many contortions of objectivism to forge a "mirror of nature," the tradition has failed to establish any "permanent, ahistorical matrix," whether in the realm of observation, language or rationality." **[14]**

The publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962 precipitated a storm of controversy by explicitly looking at science as a fundamentally social activity **[15]**. For Kuhn, periods of scientific crisis are characterised by intense debate over conflicting paradigms, and are resolved through intersubjective consensus. The ultimate acceptance of one over another is compared to a "gestalt switch," and is achieved through such "unscientific" means as 'faith,' 'aesthetic grounds' and 'persuasion' [1970: 121, 158, 159]. Writing the first major attack on Kuhn's work, Dudley Shapere declared that, "Kuhn must relinquish the notion that science brings us closer to the truth" [1964: 392]. Most of Kuhn's critics admit that subjective factors enter into the context of *discovery*, but would relegate that domain to the historians of science. For them, philosophers of science should be concerned only with the context of *justification*, where subjective factors are absent. Kuhn's work thus blurs the disciplinary distinctions between the philosophy, the history, and the sociology of science.

Although Kuhn has adamantly, even if inconsistently, denied the accusations of relativism [1977], he clearly laid the basis for a mountain of work seeking to contextualise science as a social activity. While Kuhn's work is explicitly internalist, for he fails to locate scientific communities within the larger context of history and culture, later writers have shown how external social and political considerations affect not just the context of discovery, but also the context of justification **[16]**. Some have simply sought to uncover some influence; for instance, Paul Forman [1979] argues that the antirationalist climate in Weimar Germany nurtured the acceptance of an acausal quantum mechanics. Feminist philosophers of science take this perspective even further, arguing that the very categories of objectivity and rationality upon which science is based are themselves conditioned by the socialising effects of gender [Keller 1985; Lloyd 1984]. Others, such as Steven Shapin and Simon Schaffer, have wholeheartedly embraced relativism. In *Leviathan and the Air Pump*, they argue that the debate between Hobbes and Boyle over experimentalism was actually a debate about social order. They reject questions about truth, preferring instead to explore questions of "accepted vs. rejected knowledge" [1985: 13-14]. Their work may be read as an intellectualised version of "might makes right," conflating knowledge and power.

While accepting that science is shaped by social factors, I want to avoid such a wholesale relativism. For radical social constructivists, social factors are fundamental and science is epiphenomenal. Hence they tend to ignore science per se and merely study social conditions. I accept that science is an inescapably social process of negotiation and persuasion, a position which allows me to link science with power. At first glance, this may seem like a precarious balancing act, but in the end I believe my approach leads to a conception of science quite consistent with our intuitive understandings of it. If science actually operated according to the image of the lonely bespectacled scientist isolated in her laboratory, then there would be little justification for speaking of scientific progress. The growth of knowledge has been a long process of debating and overturning dominant theories or paradigms, a process involving power relations among other things. To speak of the scientific "community" or scientific "consensus" raises a host of issues involving power dynamics.

First, scientists do not independently verify most of what they accept as valid knowledge, nor do they debate it as a community: most of it is accepted on authority. Similarly, the scientist gives her allegiance to the "invisible college" of her field of specialisation, and the usual method of entry into an invisible college is achieved through patronage [Ziman 1968: 131; Crane 1972]. These are observations, not criticisms, for things could not realistically be otherwise.

Second, given the tremendous amount of competition and conflict in science, notions of community and consensus seem somewhat euphemistic. Stanley Aronowitz argues that the sociological conception of the "scientific community derives from the liberal doctrine of possessive individualism, in which there are no power structures transcending individual determination [1988: 33-34]. This liberal notion runs contrary to the argument that discourse is embedded in relations of power.

It may even be the case, as Francois Lyotard contends, that the goal of scientific debate is *dissension*, not consensus [1984: 60-66]. As one physicist puts it, The game is, you try to smash everybody else's theory" [Hagstrom 1965: 31]. Like other social institutions, the scientific community tends to ignore or repress "strong moves" if they disrupt the status quo [Lyotard 1984: 63; Kuhn 1970]. Thus it makes sense to apply Foucault's notion of power to science: theories, like other power centres, generate resistance **[17]**. It can even be argued that by erecting various locales of resistance, scientific discourse provides the context for its own delegitimisation. The denotative statements of science must ultimately be legitimated in terms of a second-level narrative discourse, which opens the door to struggle around principles of good theory construction. According to Lyotard,

> [w]hat we have here is a process of delegitimisation fuelled by the process of legitimisation itself. The "crisis" of scientific knowledge, signs of which have been accumulating since the end of the nineteenth century . . . represents an internal erosion of the legitimacy principle of knowledge. [1979: 39-40].

Lyotard points outs that this was Nietzsche's basic argument when he tried to show how European nihilism followed from the truth requirement of science being directed against itself **[18]**. This process of dissension and delegitimisation is especially evident in the policy arena, where claims are met with counter-claims and research seems to be self-propagating.

Third, publications in scientific journals, which are the major social mechanism for disseminating and producing scientific knowledge, must bear the stamp of authenticity from editors and referees [Ziman 1968: 111]. 'Contributing' to journals can be analysed anthropologically as a gift-giving practice, with social recognition as an expected consequence [Hagstrom 1965] **[19]**. Robert Merton demonstrates that the failure to credit previous work threatens the social system of incentives within science [1957]. Thus, it is evident that power dynamics are pervasive in science as a social institution.

However, I want to make the further claim that power is at work even in the internal workings of science, not just in its social dynamics. To this end, I raise the fundamental distinction between ontology and epistemology, for the failure to maintain this distinction has been a major source of misunderstanding between objectivists and relativists. As they talk past one another, the former seem to claim that knowledge faithfully reflects reality and the latter seem to say that all knowledge is arbitrary. A more balanced and realistic view is that objects and events actually exist and our knowledge has *something* to do with them. Thus, I argue for an ontological realism and a hermeneutical, yet pragmatic, epistemology.

In his analysis of the "genesis and development" of facts, Ludwig Fleck claims that every fact comprises both "active" and "passive" associations. Fleck's own relativist bent is rooted in the former, which derive from psychological, social and historical factors. But Fleck is not a Berkeleian anti-objectivist, for he does not question the reality of the second set of associations, which we normally think of as "objectively" given [1979: 95]. As an example, he cites the assignment of the atomic weight of 16 to oxygen -- an arbitrary convention, hence an active association. Once this arbitrary convention is established, the atomic weight of hydrogen is inevitably 1.008; this inevitability constitutes a passive association. Scientific progress is marked by an increase in both the active and passive aspects of facts. Fleck, however, is more interested in epistemological rather than ontological questions. Though he defines a fact as "a signal of resistance opposing free, arbitrary thinking," [1979: 201] his main concern is not with how empirical reality intervenes to constrain thinking. Rather, he looks at the constraints ensuing from history, politics and psychology. Like Fleck, Roy Bhaskar is concerned with these two aspects of facts, but his primary concern is with the philosophical explication of the ontological dimension. He argues convincingly that the neglect of this dimension has led to stagnation in the philosophy of science. For Bhaskar, recent philosophy of science is paradoxical. While the fundamental assumptions of positivism lie shattered, alternative accounts of science have been unable to sustain a coherent notion of the rationality of either scientific change or the non-deductive component of theory. He traces this difficulty, the continuance, alongside the new philosophy of science, of a philosophy of being materially incompatible with it, ie, the Humean ontology that analyses causality as constant conjunctions of events. Bhaskar shows how the Humean account rests upon a mistaken conflation of causal laws with their empirical grounds [1989: 11-17].

Bhaskar's alternative ontology, transcendental realism, tells us that in order for experimental activity to be intelligible, which it clearly is, the world must be structured and differentiated. Although he does not presume to say how the world is structured, for that is the scientists' task, he argues that science moves from knowledge of manifest phenomena to knowledge of the structures behind them

[1989: 20]. He opposes both the Humean identification of causal laws with patterns of events and the Kantian (or transcendental idealist) a priori conceptual framework [1986: 38-50]. Rather than being based on constant conjunctions, logical constructs or "unscientific" laws of gravitation, thermodynamics and electromagnetism, are rooted in the actual structures of nature.

Bhaskar's account of scientific knowledge, I believe, avoids the shortcomings of both objectivism and relativism, or in his own terms, "epistemic absolutism" and "epistemic irrationalism" [1989: 24]. While his major focus is not on the social dynamics of science, for he is trying to fill a critical gap in the philosophy of science by constructing a consistent theory of being, he nonetheless is clear that science is a social process. He refers to it as "a produced means of production," and "a practical labour in causal exchange with nature" [1989: 21, 22].

Bhaskar's comprehensive criticisms of Richard Rorty, whose work he sees as evincing "epistemic irrationalism," point to some social and political ramifications of transcendental realism. Against both the rationalist and empiricist species' of objectivism which aspire to devise a "mirror of nature," Rorty adopts a hermeneutical and discursive posture. Bhaskar argues that, in his "linguistic turn" which makes discourse man's essence, Rorty succumbs to an anthropocentric view of nature and cannot offer an adequate account of human agency. He points out that Rorty's theory is not only wrong, but "ecologically irresponsible," if it entails that there are no real world constraints on people's actions [1989: 153]. Indeed, Rorty claims there are "no non-human forces to which human beings should be responsible" [Rorty 1986: 10; quoted in Bhaskar 1989: 153]. Bhaskar's criticism has moral overtones, but it is pragmatically grounded: social theory and action do not exist in a vacuum.

Although Bhaskar doesn't make the connection, much of his criticism of Rorty applies equally to Foucault, for whom discourse sometimes seems to be all-encompassing. This, however, is partially excusable because Foucault did not apply his own ideas beyond the social sciences [1990: 106] **[20]**. It is also noteworthy, though not surprising, that Bhaskar's criticisms of Rorty are reminiscent of Giddens' criticisms of Foucault. Both Bhaskar and Giddens are proponents of structuration theory, which holds that there are real structures, both natural and social, which shape and constrain human action. Thus, besides representing an alternative to objectivism and relativism, Bhaskar's work, like that of Giddens, moves beyond the dichotomy of voluntarism and determinism by partly affirming both.

While Bhaskar emphasises ontology, transcendental realism can also be supplemented and enhanced by an epistemology which explicitly links knowledge with power. Like Bhaskar, philosopher of science Joseph Rouse is troubled by the post-Positivist focus on the theoretical dimensions of science, which tends to look at the epistemic success of science rather than why it has been so extensively applicable. Instead, Rouse seeks to understand the sciences "not just as self-subsistent intellectual activities but as powerful forces shaping us and our world" [1987: ix]. Not only does this approach avoid the mind/body dichotomy implicit in the theory/practice disjunction, it also encompasses most people's ordinary understandings of science. For Rouse, science is a deeply practical activity which transforms both the world and how the world is known; its power lies not so much in the representational accuracy of its theories as in the practical skills it deploys. "The face of the globe has been physically transformed" by developments originating in the natural sciences. Thus, he proposes

> [a] turn from representation to manipulation, from knowing that to knowing how, [which] does not reject the commonsense view that science helps disclose the world around us [1987: 25].

Rouse borrows insights from various schools of post-Positivism, but goes beyond each of them. He agrees with the pragmatists, who argue that science is a consensual activity in which the standards of validity are not objective, but themselves the product of contestation [Rorty 1979; Habermas 1977; Hacking 1983]. In contrast to them, however, he is more concerned with science as practical skills rather than linguistic practices. Rouse adopts a much broader Foucauldian conception of power than the pragmatists, who remain wedded to the idea that power is repressive and partisan, ie, an obstacle to consensus rather than a facilitator of it. This approach allows him to sidestep their search for a mode of inquiry unconstrained by the effects of power, although he recognises the negative effects on science of certain uses of juridical power.

Like the new empiricists, Rouse redirects the locus of knowledge from accurate representation to the ability to manipulate and control events. He goes a further, however, in linking this ability to power; the new empiricists never wrestle with the concept of power. Rouse also concurs with the new empiricists' anti-realist epistemology: the success of a theory need not depend upon the accuracy of its representation. Both Rouse and most of the new empiricists, however, adopt a realist ontology: the entities to which theories refer are real. Although Rouse does not explicitly deal with ontological questions, I believe his epistemology is logically consistent with and even complementary to Bhaskar's transcendental realist ontology. The ability of science to control natural events is largely a function of its ability to work with the structures that must exist if experimental activity is to be understood as intelligible. Thus, it is not surprising that laboratory experiments seem to reveal something about the world, for the scientist works hard to make them relevant to one another. Rouse regards the laboratory and the processes that go on within it as the most characteristic expression of how science works. Drawing upon Foucault's conception of disciplinary power, he argues that,

> [t]he construction of simplified and controlled "micro-worlds" within laboratories provides models and strategies for reconstructing the world around us. This reconstruction changes the political possibilities open to us and creates new issues we have to respond to [1987: xiv].

Rouse's laboratory is not necessarily a physical space bounded by four walls but "a context of equipment functioning together, which even incorporates nature among that equipment" [1987: 107] **[21]**. In the laboratory, scientists labour to create phenomena; the objects they study are not so much 'natural' events, but the products of artifice [Latour 1983: 159-169; quoted in Rouse 1987: 23]. He compares the laboratory to the school, the asylum, the factory, and the prison, all of which Foucault sees as "blocks" within which a "microphysics of power" is developed and reaches out to shape the surrounding world [1987: 107]. The ubiquity of science as disciplinary power has profound environmental implications, for "the 'natural' world itself is now to an astonishing extent the product of artifice" [Rouse 1987: 23]. And its political significance is evident in the plausibility of Bruno Latour's claim:

> In our modern societies, most of the really fresh power comes from science -- no matter which -- and not from the classical political process [1983: 168].

At first glance, the power that arises from science seems qualitatively different from more narrowly conceived notions of power; the former is rooted in power over natural phenomena whereas the latter entails power over people. I think, however, that this dichotomy is based on three misconceptions. First, it rests upon a rigid dichotomy between nature and human beings. Second, it ignores the productive and generative aspects of all types of power other than simple "juridical" power. Third, and more important for the following discussion of the political implications of knowledge-based power, such a dichotomy ignores the profound degree of interdependence between the two sorts of power. Latour does not claim that political power is subordinate to the power of science, but only that new forms that it takes have emerged largely from knowledge-based power. In plain terms, recent developments in military, aerospace, communications, and transportation technology, have reshaped virtually all patterns of social relations. Moreover, the power of scientists to interpret reality has itself become a productive (but not necessarily positive) source of political power, regardless of how knowledge gets translated into technology. Their power derives from their socially accepted competence as interpreters of reality. Thus, the adoption of comprehensive, contextual and process-oriented conceptions of power and knowledge can help generate insights into the nature of knowledge-based power.

CHAPTER THREE
SCIENCE IN POLITICS

3.1 Beyond the Enlightenment

The relationship between knowledge and power presented in Chapter Two is rather different from the conventional wisdom. Yet the received wisdom provides the backdrop for the standard accounts of science in politics, which tend to characterise the relationship as one between an independent and a dependent variable respectively. [1] Joseph Rouse contrasts the three dominant accounts, all of which see power and knowledge as conceptually separate, to his own theory [1987: 13]. First, knowledge can be applied to get power. Second, power can impede or distort the acquisition of knowledge. Third, knowledge can emancipate people from the repressive effects of power. Each of these views has its roots in Enlightenment thinking. Power and knowledge come closest to one another in the first variant, which was most influentially articulated by Francis Bacon [2].

Power here does not have direct social connotations, only as the ability to produce effects. In a sense, Bacon's view of science overlaps with Rouse's in that both view science as an instrument of control. Bacon's beliefs about the place of science in society [1974], however, are very different. Science, portrayed as harmonious and monolithic, is the organising principle of Bacon's utopia, which he depicts as hierarchical, meritocratic and conflict-free. If politics is inherently conflictual, then Bacon's work suggests that science promises the abolition of politics, evincing an early modern faith in science as the universal principle of legitimisation.

Perhaps the most well-known advocate of science during the eighteenth century Enlightenment was the Marquis de Condorcet who, like Bacon, believed that science would be the salvation of humanity. Unlike his predecessor, however, he held that science would flourish only when freed from the fetters of monarchy [Condorcet 1955]. He argued that science should function independently within a democratic state in order to ensure its integrity. Yet, in principle, science and politics were not incompatible, even if they should be kept structurally separate, since for Condorcet both science and democracy shared a common commitment to the power of reason to order human affairs. Moreover, science could lead to a greater understanding of politics and even, for that matter, all human endeavours, a belief that typified Enlightenment thought [Cassirer 1955]

In the nineteenth century, two of the most celebrated proponents of science were Henri Saint-Simon and his renegade student and father of positivism, August Comte. Despite the differences in their ideas, they both believed that science, both in its content and its structure, could provide the guiding principles for social harmony. Opposing liberal notions of equality, other thinkers proposed hierarchical social systems reminiscent of Bacon's utopia [Saint-Simon 1952; Comte 1853]. Both criticised scientists for not recognising their role as builders of the new society. Like Bacon, they also claimed that their proposed utopias would lead to the elimination of domination and conflict. For Saint-Simon, the unifying force would be a new "religion of science". For Comte, industrialism and science would replace the military character of earlier social orders. For both, reason and calculation would replace political discourse; science was thought to exist outside of power. Given the twentieth century's wars of unprecedented destruction, despite scientific progress, the predictions of both Condorcet and the positivists seem wildly optimistic. But the point is not so much to test the validity of various notions of power and knowledge, as to understand their place within the history of ideas.

There is a divergent tributary diverging from the mainstream which portrays science more dubiously as Frankenstein's creation or Pandora's box. This position, typified by the nineteenth century Romantics, has grown in popularity as the negative effects of science and technology have made themselves felt. For instance Hans Morgenthau, writing at the close of World War Two, argued that the rationalists' faith in science is both intellectually wrongheaded and politically dangerous [3]. Scepticism regarding the social benefits of science grew during the 1960's and 1970's, particularly in Europe and North America [Dickson 1984]. In many ways, the environmental movement is an expression of this scepticism [Nicholson 1970]. Despite the fact, however, that science may wear two faces, science remains a strong force in the world, if for no other reason than that it dominates the world of thought. Morgenthau may be right in declaring that "this belief in science is the one intellectual trait which sets our age apart from preceding periods of history" [1946: 4]. I do not mean to imply that the universal faith in science can eliminate conflict, a position which would be antithetical to my whole argument, but that science can be used as a tool for forging political consensus. This is due, in part, to the common belief dating back to the Enlightenment that science can make politics more rational. The following statement typifies this attitude:

> When science is divorced from policy, the result is not only that science is "set free" but also that policy is thereby thrown on its own resources - which is to say that it is left to be determined by tradition, prejudice, and the preponderance of power [Kaplan 1964: 24].

But if knowledge is inseparable from power, we may expect even stronger links when knowledge is applied to political conflicts. Most technical problems in the policy arena cannot neatly be divided into technical versus political elements. Alvin Weinberg states:

> Many of the issues which characterise the interaction of science or technology and society - eg, the deleterious side effects of technology, or the attempts to deal with social problems through the procedures of science - hang on answers to questions which can be asked of science and yet *which cannot be answered by science* [1972: 209; emphasis in original].

He proposes the term "trans-scientific" for these questions, a term which has become quite popular. Some examples of trans-science include the biological effects of low-level radiation exposure, the probability of extremely improbable events, and engineering judgement in design. Regulation of ozone-depleting chemicals is another example. Because trans-scientific questions are becoming increasingly prevalent, it becomes more important to understand the dynamics of expert advice on policy questions.

The importance of expert advice in shaping policy decisions is nothing new. In the past, advice came from such sources as oracles and prophets. But not all pre-modern political advice was "irrational"; rational calculation and the equivalent of cost-benefit analysis have existed since ancient times [Goldhamer 1968: 1291-31]. A great deal of expert advice has historically pertained to military matters, a trend that has continued up to the present [**4**].

Before discussing the contemporary context, I want to survey some general characteristics of expert advice in policy making. Though my focus is knowledge-based power, I do not want to exaggerate its importance; information is always relayed and exchanged in the larger political arena. In the words of one observer, "The reason experts can never be too influential is simple. They cannot replace the existing political process" [Benveniste 1977: 159]. Ultimately, the expert can be fired or, short of that, he can be ignored or his advice can be distorted to justify preconceived policies. Scientists can also be used as pawns. Their prestige can be used to legitimise policy objectives not even directly relevant to their areas of expertise, as when scientists were mobilised in support of the Atmospheric Test Ban Treaty during the Kennedy administration [Uyehara 1966]. Disagreement among some experts can also be used as an excuse to ignore their advice, as was the case for many years with the acid rain issue and is now occurring with respect to global climate change [Whetstone 1987].

Despite these limitations, experts can wield real power. The ability to interpret reality is an important source of power. Policy maker who ignore the experts or conceals the facts risk political embarrassment, particularly in pluralistic societies where scientists often speak out publicly. They are likely to find allies in the media, since both science and the media seem to have an innate distrust of a secret decision making by elites [Wood 1964: 59] [**5**].

The dynamics of expert advice are strongly coloured by the inherent differences between experts and decision makers. An obvious one is that they speak different languages, leading to the possibility of mutual misunderstanding and mistrust. Experts often deal with abstractions within their narrow specialty, whereas politicians must pay great attention to specific circumstances and how various interests will be affected by their decisions. Policy makers may be uncomfortable with the experts' neglect of the economic consequences of their recommendations, as often happens in environmental politics. Experts may be uneasy in the world of compromise and pork-barrelling, and they may be ignorant of their own political influence. Policy makers may be awestruck by technical language, leading them to place absolute faith in the experts and develop unrealistic expectations of what expert advice can accomplish. They may also resent the experts for their sometimes pedantic approach, or they may lose patience with the level of technical detail. Not surprisingly, although scientists are widely respected, the two faces of science also emerge in a policy context [6].

A related problem is the divergence in the time frames between experts and policy makers. Part of what experts have to offer is their ability to expand the time horizon to anticipate policy implications that might go unrecognised otherwise. Yet their analyses may be unwelcome or seen as irrelevant in political circles, where the ability to stay in power depends upon relatively short-term considerations [Benveniste 1977: 83]. This is a major concern for global environmental issues, where negative effects may not be felt for generations.

As with other forms of power, success breeds success for knowledge-based power [7]. Experts do not deal simply with facts; they must cultivate their reputations as sources of authoritative knowledge. In this matter, "it is not rationality per se that matters but *power or trust*" [Benveniste 1977: 32; emphasis added]. The ability of experts to reduce uncertainty is in part dependent upon whether they are perceived as powerful or trustworthy, yet they are trusted inasmuch as they succeed in reducing uncertainty. This phenomenon may be especially evident in crisis situations, in which experts save the day and consequently may modify the existing power structure by displacing others. Thus, power is generated in a circular fashion. A good example is the prestige that NASA obtained through its successful Apollo program. The obverse, however, is seen in how public trust in NASA plummeted after the Challenger disaster.

Because of their access to specialised knowledge, scientists are uniquely situated to place some issues on the public agenda that might otherwise go unnoticed. Scientists were the first to point out the potential to build atomic weapons, the unresolved problems of nuclear waste disposal, the dangers of recombinant DNA research, and the ecological threat of DDT. In most cases, however, they must rely on coalitions with public officials, interest groups and bureaucracies to see their policy proposals through to implementation. But unlike the search for knowledge, which is perceived as a legitimise practice, the search for allies must be disguised by other professional activities [Benveniste 1977: 149].

Ultimately, this necessity is rooted in the larger belief that science is, or can be, objective and value-free, while political life is ideological and value-laden. Typically, the attitude on the part of technicians is often one of "the government makes the decisions; we just make the missiles." But, of course, the fact-value distinction is blurred once we acknowledge that facts comprise what Fleck calls active and passive associations. But the perceived division of labour between science and politics, based upon the fact-value dichotomy, raises other problems in a political context.

First, if the dichotomy were a genuine one, then scientists should never call attention to a problem, for to do so betrays a commitment to certain values. Why did Einstein urge Roosevelt to develop atomic weapons? Or why should a researcher point out that a certain chemical might cause cancer? For the simple reason that they care; they are committed to specific values. At a minimum, they believe in the value of their own information. As Benveniste maintains, "It is not possible to be an expert and not be concerned with the way one's recommendation is received" [1977: 54].

Second, if science consisted of atomistic facts of equal value, then there would be no genuine criterion for choosing research topics. Both scientists and policy makers recognise that not all facts are of equal value, for they vary in their interest and productivity, as well as in their internal robustness [Ravetz 1986: 421].

The fact-value distinction is further eroded by the recognition that, through their access to knowledge, scientists often determine the range of possible policy objectives. For instance, it was until fairly recently generally held that civilian scientists doing military research should abide by goals set by the military. This belief was shattered as scientists uncovered knowledge of greater military value through their unrestricted research than under the guidance of policy makers. Thus, means can determine ends [Price 1964: 27-8]. Moreover, many policy problems only exist because of scientists acting as citizens, not as ciphers.

Further dividing the two worlds are the different modes of factuality involved. Kratochwil's notion of "three worlds of facts," is relevant here. For the most part, scientists operate in the world of observational facts, while policy makers deal primarily with intentional and institutional facts [Kratochwil 1990: 21-27]. Scientific facts are subject to very different validation tests than political facts. If the public believes something to be true, even if science has shown it to be false, that belief remains for policy makers [Caldwell 1990: 34]. Similarly, even if the fact-value distinction does not truly exist, the perception of its validity can influence the politics of technical advice. This undoubtedly was an important dynamic in the ozone negotiations.

Scientists who perceive their own work as by definition value-free in its approach and beneficent in its results can easily delude themselves about the nature of their own political involvement. Robert Wood remarks that,

> a better impetus for unfettered political activity can scarcely be imagined. Not responsible for past errors in public policy, carrying the keys which will unlock the doors to a better way of life for all, and capable of bringing order our of political chaos, the scientists can enter the decision-making process secure in the knowledge that their activities are not impelled by personal ambition and the thirst for power [1964: 63].

This observation about the potential for self-deception is consistent with the argument that power is not inherently intentional. Most scientists, even those who work in policy relevant areas, do not see themselves as seeking power. Nor do their more overtly political counterparts, for the belief in the objectivity of science is deeply entrenched and nearly universal.

Even if the logical distinction between fact and value were credible, the distinction cannot be sustained in practice. Every fact must be communicated somehow, and the choice of words is itself a value choice **[8]**. Ideological goals can also be pursued and legitimated through science. Technical questions often reduce to matters of non-scientific judgement when more than one solution is justifiable on scientific grounds **[9]**. Interpretations of scientific rules of inference show that "there is a significant area in which science and policy are not separable -- they organically interpenetrate one another" [Wynne 1987: 108-9]. While all of these rules are technically defensible, the outcome is a policy rooted in ideological beliefs.

Debates about values and norms are typically couched in empirical terms [Lindblom and Cohen 1979: 46]. Seldom does a person rest her case for a particular policy on mere subjective preference; rather, she buttresses her position with reference to a myriad of 'facts'. For this reason, no matter what political values underlie a controversy, the debates generally focus on technical questions; questions of value become framed as questions of fact. And since science is modernity's pre-eminent instrument of legitimisation, all participants can be expected to claim that their positions are "mandated by science," even if it is hard to see how science alone can ever mandate anything **[10]**.

Evidence, however, need not be cited in order to don the mantle of scientific objectivity. For instance, President Reagan's science adviser, George Keyworth, told a congressional committee in 1983 that the administration intended to "reduce the excessive burden of federal regulations by improving their rational basis" [US Congress, 1983; quoted in Dickson 1984: 263]. As David Dickson observes, this statement, expressing the Reagan administration's quest for "scientific" regulation, *assumes* that more rational regulation will be less costly for industry. Advocates of stringent health and environmental regulations were presumed to be irrational and unscientific.

There is no small irony in the fact that the increasing amount of reference to science seems to be accompanied by its decreasing credibility. Policy science is paradoxically a scarce resource, in spite of its exponential growth. Every debate must be framed in scientific terms, each confrontation undermining the credibility of various positions, leading back to the search for more scientific weapons [Wynne 1987: 99]. Expertise generates counter-expertise [Benveniste 1977: 147], particularly in pluralistic societies. Lyotard's epistemological argument that the demand for legitimisation results in a process of delegitimisation is mirrored in the world of policy.

Paradoxically, as our scientific knowledge increases exponentially, our relevant ignorance grows even more rapidly and is often politically contentious. For instance, our ancestors had no knowledge of how to dispose of radioactive wastes; nor did they need such knowledge. But now that we have learned how to generate them, we have created a new area of practical ignorance. As Jerome Ravetz observes, "we have conquered a former ignorance, in our knowledge of radioactivity, but in the process created a new ignorance, of how to manage it in all its dangerous manifestations" [Ravetz 1986: 423]. Environmental politics may be characterised not only by scientific uncertainty, but by what Ravetz calls "scientific *ignorance*."

The argument in this chapter may appear to imply that science is of no help in depoliticising issues because it is itself hopelessly mired in political questions. But this is not my intention; I simply want to claim that the waters are muddier than is generally appreciated. Just as I would want barristers to read their briefs, I would want policy makers to listen to scientific advice. Ultimately science holds out the possibility of depoliticising issues to some small extent for two reasons. First, facts have fixed and conventional dimensions, and the former are axiomatically apolitical. Either chlorofluorocarbons destroy stratospheric ozone, or they don't: the question is not answerable in polemical terms. Second, inasmuch as policy makers believe that science is apolitical, they may be willing to use it to build a policy consensus **[11]**. But that belief can only be sustained if scientists appear as apolitical elites, disavowing political objectives and behaving unlike other participants in the policy world. In some cases, the ability of scientists to be influential may require their power to be so covert as to be hidden even from themselves.

3.2 The Knowledge-based Society

The period since World War Two has seen a tremendous rise in the importance of science to public policy. In the early postwar period, this was generally welcomed as a wholly positive development. Vannevar Bush's historic report, *Science: The Endless Frontier* [1945], reflected the popular sentiment in Baconian terms: in science lies human salvation. Statistics show that the numbers of technical employees and the amount of government spending for research and development, particularly in the industrialised countries, have risen dramatically in the past four decades [Lipset 1964; Holzner and Marx 1979: 3-4; Dickson 1984: 7] **[12]**. This shift represents a major transformation of the social structure, one that places knowledge-based power at centre stage **[13]**. The new social genre has been referred to variously as cybernetic [Bell 1967], technocratic [Brzezinski 1968], programmed [Touraine 1972], post-industrial [Touraine 1972; Bell 1973], knowledgeable [Lane 1966], active [Etzioni 1967, 1968], and post-modern [Kavolis 1970, 1974; Bell 1976; Marx 1976]. Despite their differences, all of these thinkers emphasise the centrality of knowledge in the new social order, and most are optimistic about this development, applauding what they see as the unifying impact of science and information technology.

Rather than attempting to analyse the contributions of all of these writers, I will focus on Daniel Bell, who is probably the most influential of them **[14]**. Bell proposes three categories, which he views as ideal types rather than mirrors of reality, for conceptualising the dominant social structures historically: pre-industrial, industrial, and post-industrial. The first, characteristic of much of the

Third World today, is based upon a technology of raw materials. Pre-industrial society is "a game against nature." Industrial society relies on a technology of "a game against fabricated nature," by which Bell means economic classes, the urban environment, and political bureaucracies. Post-industrial society is based upon science and information technology. Unlike the previous two, post-industrial society is not a game against anything or anybody, but rather a game between persons [Bell 1987: 61-65]. The basis of political power is seen as shifting from economic position to functional capacity, with policy increasingly dictated by international events rather than parochial interests [Kleinberg 1973: 36-38].

Bell believes that the new social order will be -- or even that it already is -- essentially free of conflict. According to Bell, post-industrialism's reliance on abstract theory and objective knowledge is bringing about the "end of ideology" [1961]. "The only universal symbolism which remains is that of science" [Kleinberg 1983: 210]. At times he is simply observing a significant social development; at other times he speaks as an champion of post-industrialism. He rails against "contemporary populism," which opposes elitism not out Of a sense of justice but "*ressentiment*" [1987: 70]. Sounding much like Bacon or Saint-Simon, he argues that post-industrial society is not a "technocracy," but a "just meritocracy" [1987: 70-71]. His "professional class," which is the heart of the new social order, is allegedly less self-interested than other classes because of its intellectual training. He has no sympathy for those who are wary of post-industrialism out of a concern that democratic participation could be eroded [**15**].

Bell seems oblivious to the possibility that knowledge could be a source of conflict rather than a panacea, or that it could simply serve particular social interest. He treats knowledge as monolithic and uncontroversial; in fact, he devotes little analysis to his key theoretical categories of information and knowledge, apparently embracing them as unproblematic. His view of science recalls the Enlightenment notion of knowledge as divorced from power. Despite Bell's denunciation of ideology, there is some warrant in labelling him "an ideologue of technocracy" [Jameson 1984: xx] or an "end of ideologist" [Kleinberg 1973: 2].

Other analysts of post-industrial society are not as sanguine [**16**]. Alain Tourain, for instance, takes a more critical neo-Marxian stance [1972]. He argues that much of the contemporary decision-making apparatus has been monopolised by a technocratic elite, effectively replacing democratic participation with "dependent participation." His analysis is similar to Habermas' argument that technocratic societies rely on a "structurally depoliticised public realm" in which the legitimisation process elicits a diffuse mass loyalty while avoiding substantial participation [Habermas 1973: 36-7].

Ernest Mandel develops the Marxist theme further, arguing that Marx's categories of analysis for classical capitalism retain their validity today [Mandel 1975: 190-91]. He takes issue with Bell's contention that a new social order is emerging. Rather, he claims, we are in a stage of late capitalism, a culmination of industrial society in which all segments of the economy have been industrialised. Mandel's point is well taken; what Bell calls post-industrial society continues to be suffused with the products and effects of industrialism.

Mandel argues further that knowledge can be analysed in Marxist terms as a commodity. Timothy Luke [1989] sharpens this analysis, using semiotics and critical theory to develop a post-Marxist critique of commodity production and exchange in informational society. Indeed, much of what Marx says about commodities applies to information. Speaking of their "mystical character" and the "fetishism of commodities," he says that "the productions of the human brain appear as independent things endowed with life" [Marx 1978: 320-1]. Despite Marx's own tendency to isolate science from power, the application of his analysis of commodities to information brings them together. Mandel and Luke are explicit on this issue, pointing out the relations of domination involved in the production and application of knowledge. Their works provide an important antidote to the liberal notion of information as an unequivocal public good.

While the parallels are interesting, the correlation between knowledge and commodities should not be pushed too far. The conditions of scarcity that are operative for commodity exchange are not replicated in the realm of informational exchange. Once knowledge is produced, even if it is produced by and for elites, it differs from the means of production in that it may fall into the hands of the less privileged. This phenomenon has enabled many grassroots groups involved in technical issues to exert political power [Nelkin 1984]. Knowledge may not be a public good, but neither is it the sole property of a ruling class.

Neo-Marxian critics of post-industrialism evince a healthy scepticism toward theorists like Bell, for they recognise that production and exchange of information take place in a field of power relations. However, like other intellectual descendants of the Enlightenment, they often succumb to the subjectivist faith in the possibility of an escape from power. For them, however, that possibility cannot be fulfilled under contemporary post-industrialism, but rather it awaits the day when the alienated masses revolt against the technocratic elite and reclaim the instruments of knowledge production [Touraine 1972]. This perspective, like its more liberal antithesis, is oblivious of the mutual embeddedness of power and knowledge and naive about the unifying potential of expert advice.

Post-structuralism also adopts this critical edge, while rejecting the Marxist tendency to erect a hegemonic discourse out of its own critique of domination. Marxism is based upon the labouring subject, just as liberalism is premised upon the rational subject. Post-structuralism challenges the notion of subjectivity, which it places at the heart of modernity, claiming instead that subjects are constituted by discourses or constellations of knowledge/power **[17]**. Foucault's approach is particularly germane to a social field pervaded by the "mode of information" rather than the "mode of production" [Poster 1984]. Likewise, Baudrillard's analysis of the commodification of consciousness through representation and "hyperreal simulation" provides a far better description of post-industrial dynamics, ranging from advertising to computer modelling, than structural or materialist approaches [1975, 1981, 1983]. Environmental politics can offer some key nodes for the study of disciplinary power and surveillance, ie, "the flow of information from the object under scrutiny to the authorities" [Poster 1984: 163].

3.3 Conceptions of Science in International Relations: Functionalism, Neo-functionalism and Epistemic Communities

All of the problems inherent in the politics of technical advice repeat themselves in international policy decisions, only with the added complication of interstate rivalry. Earlier I argued that the power of technical experts is proportional to the trust that decision makers have in them. This problem arises with a vengeance in international relations, an arena characterised by inherent distrust. Governments are far more likely to pay attention to studies done within their own borders than those from other countries. International organisations and, in some cases, scientists themselves, have sought to alleviate this problem by conducting studies and evaluations through independent international panels, the United Nations specialised agencies, and regional organisations like the Organization for Economic cooperation and Development (OECD). Nevertheless governments often insist upon doing their own studies, and in the end they may not even listen to their own scientists. If the stakes are high, there is an added incentive to disregard the science.

On the one hand, there is some validity in the widespread belief that "science forms the most truly international culture in our divided world" [Brooks 1964: 79]. International conferences and journals are the main channels through which scientists communicate new ideas and discoveries. Even at the height of the Cold War, scientists on both sides of the Iron Curtain were calling for greater openness, not just for political reasons but to further their own work. In addition, to the extent that scientists are committed to universalism and communality [Merton 1973: 263-4], there is reason to see science as a potential unifying force in international conflicts over technical issues.

On the other hand, the political dynamics of technical advice indicate that an untempered optimism in the globalising potential of science advice may be misplaced [**18**]. The hope that science can harmonise international politics is understandable and not completely groundless. Unfortunately, however, it is often accompanied by a naive view of knowledge as separate from power, as well as a poor understanding of the complexities involved in translating scientific knowledge into policy. In this section, I look at three theoretical approaches to science in politics at the international level functionalism, neo-functionalism, and the literature on epistemic communities. To one degree or another, each of these approaches sees knowledge as divorced from power, and thus succumbs to some of the problems discussed above. Functionalism has its historical roots in nineteenth century thinkers as diverse as Saint-Simon [1925], Herbert Spencer [1896], and the Fabian Socialists [Woolf 1916]. Like post-industrialism, functionalism is sometimes a descriptive or predictive theory and sometimes a normative theory; it also tends to place considerable faith in technical rationality. Most representative of the recent literature is David Mitrany, who foresees the eventual reduction and control of sovereign national power by international functional organisations in areas of universal concern [1975]. As the nation-state's ability to protect the welfare of its citizens decreases due to the interdependence of nations and issues, power will be ceded to international organisations. Kenneth Thompson distinguishes several characteristics of functionalism: it is non-political, involving social and economic issues, addressed to urgent problems, undertaken in a problem-solving manner, and built upon the cooperation of professionals [1979: 96-99].

Technical experts are expected to steer the way to a functional world by virtue of their ability to fashion a consensus on means-ends relationships. Although Mitrany's focus is economic, environmental problems are also of universal concern and may even fit his theory better since knowledge in the natural sciences tends to be more consensual than in the human sciences. A major problem, however, is that Mitrany assumes both expert consensus and technical certainty, an assumption which contradicts much of what I have argued above regarding the conflictual nature of knowledge. Mitrany, like so many other theorists, portrays experts as above the fray of social and political conflict, putting their knowledge at the service of humanity.

And, like others, he has a purely negative conception of power, arguing that creative work, not power, is an appropriate human pursuit **[19]**. Another problem with functionalism is its deterministic and rationalistic assumption of historical efficiency. Mitrany, like Talcott Parsons, assumes the rationality of social processes **[20]**. He believes that a community-building consensus will *inevitably* develop in a context wider than the specific issue area that a functional organisation addresses. Functionalism provides no theory of how this development will come about; it has no theory of social change because it lacks a theory of agency.

In his first major work, Ernst Haas [1964] attempts to amend functionalism by proposing a more empirically relevant, less ideological neo-functionalism. In his two case studies on the World Health Organisation and arms control, Haas finds that the predictions of functionalism do not withstand scrutiny. In the former, he sees no attempt to clearly delineate political and technical concerns. In the latter, experts were important, but they frequently pursued nationalistic, rather than universalistic, objectives. These inconsistencies point to the need to reconceptualise important aspects of functionalist theory. Haas argues that functionalism's main shortcoming is that it has no theory of interests, and so must resort to the utopian notion of the common good as the motivation for action. Ignoring the role of interests in international cooperation, functionalists attribute cooperation to manipulative experts working for the common good [1964: 31-34]. For Haas,

> cooperation among groups is the result of convergence of separate perceptions of interests and is not a spontaneous surrender to the myth of the common good. Furthermore interest goes well beyond the liberal separation of *economic* interest from the restraining force of *society*. Any claim made upon the community on behalf of the values dear to some group represents an interest [1964: 34; emphasis in original].

For Haas *any* claim represents an interest, interests need not consciously be shared in order for integration to take place, and integration can occur under conditions of competition as well as cooperation. Unlike Mitrany, Haas provides an account of how authoritative decisions can be made even when experts disagree among themselves. Likewise, he can account for at least some unintended consequences of actions.

Unfortunately, Haas' conception of interest is truncated. If *any* claim constitutes an interest, then Haas cannot differentiate between interests and delusions. He overlooks the power of knowledge to change one's conception of one's interest, although this concern is the focus of his most recent work [1990]. Moreover, Haas does not adequately link his notion of interest to Mitrany's notion of function. In his concern to distinguish his own more 'scientific' work from the normative aspects of functionalism, he offers no basis for the performance of functions other than the growth of organisations. Functionalism, with its normative bias, at least grounds its prescriptions in the desirability of meeting human needs.

Haas also takes issue with the assumption of the inevitability of spillover effects, pointing out that functionalism offers no theory of how this development comes about **[21]**. His less deterministic neo-functionalism attempts to solve this problem by adding the process of "social learning" to account for international community building. Haas interprets functionalism as a development from 'Gesellschaft' to 'Gemeinschaft,' or from an elite community to a network of societal associations **[22]**. Haas' neo-functionalism, like its precursor, evades issues of power. He does not give sufficient credence to the possibility that scientists may constitute an elite class with a monopoly on information. Nor does he consider that expert advice may simply reinforce existing power relations among states, rather than move the international community "beyond the nation-state". Thus Haas, like Mitrany before him, succumbs to the Enlightenment fallacy: he drives a wedge between knowledge and power, removing expert advice from the realm of politics.

In his later works, Haas is more cautious in this respect, though I believe he continues to adhere to the same misconception at a more subtle level. He and his co-authors of *Scientists and World Order* [Haas 1977], Mary P. Williams and Donald Babai, specifically consider whether the introduction of science into international politics indicates progress toward a more rational and cohesive world order. On the one hand, they recognise that science has not brought about the end of ideology, nor is it likely to do so. They admit that scientists suffer from sociological ambivalence [Merton 1963].

They draw no rigid line between the technical and political, but rather propose a continuum from the "purely technical" to the "purely political".

On the other hand, much of their work seems to be rooted in contrary principles, particularly their depiction of knowledge as the independent variable and world order as the dependent variable. While claiming that their taxonomy of world orders in a two-by-two matrix is merely a heuristic device, their method belies an underlying commitment to this dichotomy. **Figure 3.1** shows their matrix depicting possible world order models as a function of how knowledge and politics are perceived, with the lower left quadrant representing the current world order. While the authors believe that a pragmatic world order represents our best hope, they clearly yearn for the rational world order. "Learning" for them occurs when knowledge becomes more consensual and political goals more comprehensive; movement toward a rational world order, "a technocrat's delight," represents progress [1977: 52].

Figure 3.1 (Adapted from Haas et al 1977: 51)

Goals versus Knowledge

		Political goals are: Specific	Expanding, Interconnected
Expert knowledge becomes	More consensual	*Pragmatic* world order	*Rational* world order
	Not more consensual	*No new* world order	*Sceptical* world order

Although the depiction of world order models is useful for purposes of identification, there are several problems with this approach. First, the characterisation of knowledge in terms of degrees of consensus is misleading, for their discussion is really about the extent to which interdisciplinary linkage is present. In fact all four world order models, even the most disorderly of them, presume that expert consensus exists at least on single issues. Thus, the underlying assumption is that expert advice is inherently consensual, an assumption which is at odds with our discussion hitherto. As a consequence, Haas *et al* ignore decisions made on single issues in the face of technical uncertainty, thereby leaving out a great deal of international policy involving scientific advice.

Second, the authors' method of seeking the potential for various world orders in the beliefs of scientists about their own role in international politics recalls the Enlightenment faith that science can engender a world free of conflict. It is as if the scientists' beliefs were determining what kind of world order actually comes about. In the end, in a tone reminiscent of Saint-Simon, they seem to chastise scientists for adhering to the sceptic's vision of world order, arguing that such a stance subverts the ability of science to bring about a more rational world order. They gloss over the sceptical view, and overlook the many possible avenues by which science might not lead to a more rational world order.

Haas' more recent work, *When Knowledge is Power* [1990], is an attempt to elaborate and improve upon his earlier ideas about how states employ new knowledge. Of all the analyses of science and international polities examined thus far, this work is probably most consistent with my argument. Nonetheless, Haas' thinking continues to evince a tendency to dichotomise knowledge and power. As in his earlier writings, Haas' primary concern in this work is with "social learning," or the process by which consensual knowledge is used to influence public policy [1990: 23]. Haas explicitly argues here that interests are problematic rather than straightforward, and that consensual knowledge can be instrumental in helping policy makers to define their interests. His reasoning thus echoes a predominant theme of this thesis **[23]**. For Haas, *epistemic communities* are the most significant agents of institutional innovation, or social learning. After Burkart Holzner and John Marx, he takes epistemic communities as:

> those knowledge-oriented work communities in which cultural standards and social arrangements interpenetrate around a primary commitment to epistemic criteria in knowledge production and application [Holzner and Marx 1979: 108; quoted in Haas 1990: 40].

He applies this definition to learning in international organisations, limiting epistemic communities to groups of typically interdisciplinary professionals who share a commitment to a common causal model and a common set of political values. They are also committed to truth tests based upon adversary procedures and a claim's ability to solve what has been deemed by the community as a problem. The success of an epistemic community in implementing its preferred policies depends on two factors: its persuasive ability and its ability to ally with the dominant political coalition [1990: 412]. Success is most likely during crises, when new knowledge is apt to be solicited. Superficially at least, the concept of epistemic communities seems consistent the mutual embeddedness of knowledge and power.

Although Haas warns against thinking of epistemic communities as "the white knight of expertise as being arrayed against the forces of darkness" [1990: 40], he disregards his own warning at times. Epistemic communities are portrayed as the "enemies of habit-driven institutions;" their inability to achieve their heroic mission is not due to any handicap of their own, but to the institutional tenacity of habit. By juxtaposing experts with habit-driven actors, Haas neglects the extent to which knowledge is itself institutionalised and routinised. By arguing that experts can contribute to the emergence of shared meanings in opposition to "the drag of institutions," Haas forgets that institutions themselves embody shared meanings. Moreover, Haas' belief that knowledge is the independent variable and politics the dependent variable is implicit in the fact that he only discusses how science influences politics, and never how politics affects science. Thus, flying in the face of his earlier caveat that "the line between consensual knowledge and political ideology is often barely visible," Haas concludes that "the language of modern science is creating a trans-ideological and transcultural signification system" [1990: 20, 46].

The notion of epistemic community proposed by Holzner and Marx is both promising and problematic. On the one hand, their concept is intended to illuminate the nature of post-industrialism [1979: 3-19]. In addition, it integrates the concepts of power and knowledge in a potentially useful and provocative way. Holzner and Marx argue that knowledge involves the social construction of reality, and that the ability to construct reality is a source of tremendous social power [1979: 92-3; see also Holzner 1972; Berger and Luckmann 1968]. Knowledge is defined as "a communicable mapping of experienced reality," and is inextricably linked to specific frames of reference [1979: 93]. Each epistemic community is characterised by particular reality tests to which it subscribes. Empirical reality tests are emblematic of scientific epistemic communities, but other reality tests may be pragmatic, authoritative, rational, magical and mystical. Thus, only a few epistemic communities are scientific; others could include astrologers, witches, beauticians, logicians, mechanics, and bureaucrats. Socialisation into an epistemic community requires the internalisation of its values and norms, a process which involves a form of disciplinary power. The relative autonomy of an epistemic community is guaranteed because the larger social system yields, on trust, a cognitive monopoly to it for a certain range of issues [1979: 110]. Interest groups differ from epistemic communities, yet may actively work to guarantee their autonomy; for example, the Australian Medical Association's support of the medical epistemic community has served this purpose.

The most troubling aspect of this model for understanding how science affects policy is its very broad conception of knowledge. Astonishingly enough, Holzner and Marx claim not to be relativists, arguing that "discovering lawfulness" in the various knowledge systems "will contribute to a more general theory of rationality, rather than strengthen a historicist withdrawal into relativism" [1979: 93]. They do not pursue this task, however, so their claim can be little more than an article of faith, particularly given the vast spectrum of phenomena that counts as knowledge for them. Because their notion of knowledge bears no relation to ontological reality, it is unclear how Holzner and Marx can skirt relativism. Likewise, their notion of epistemic community, like the more traditional "scientific community," ignores the ubiquity of dissension. Thus their view of the medical profession as an epistemic community, to say nothing of science, is problematic. Moreover, one wonders how a theory designed specifically to elucidate the application of knowledge in post-industrial society can also incorporate all other frames of reference. If almost any group can qualify as an epistemic community, then how is post-industrial society unique?

These general problems with Holzner and Marx's conception persist in the work of Peter Haas. Like his father, Peter Haas is interested in linking power and knowledge through the concept of epistemic communities, but he goes into greater detail on the nature of their political power. His work is specifically on environmental epistemic communities **[24]**. Like his father, Peter Haas defines epistemic communities in terms of shared knowledge common political perspective. But the bonding principle of epistemic communities remains vague. At times Peter Haas refers to their "common perspective," which could apply to bureaucracies and interest groups as easily as to epistemic communities [1989: 380] **[25]**. One wonders why consensual knowledge should be more persuasive in an international arena, where the potential for divisiveness is presumably much greater, than domestically, particularly given the tendency of governments to mistrust information from outside sources.

The same problem arises in Peter Haas' recent attempt to provide a more comprehensive conception of epistemic communities. Again, he defines them in terms of shared consensual knowledge and political values. But he acknowledges that consensual knowledge alone does not ensure that scientists will maintain group solidarity, citing various studies showing that "scientists as a group proved no less venal or subject to political temptation than their non-specialist counterparts." Hence he proposes that loyalty to an epistemic community derives from a commitment to shared political values [1990: 183]. If this is the case, however, then the distinction between epistemic communities and other sorts of more overtly political groups breaks down.

Peter Haas also fails to provide any guidelines for what counts as knowledge. In his work on the Med Plan, he explicitly dodges all epistemological issues [1989: 401], so that knowledge simply becomes whatever an epistemic community believes, raising the question of relativism. In his more recent piece, Peter Haas explores various epistemological approaches. He seems sympathetic to a hermeneutical stance, but he never commits himself to any position [1990: 20-24]. Nonetheless, Peter Haas clearly regards only a limited range of groups as epistemic communities: all of his examples are groups of scientists. He does not seem inclined to include astrologers and mechanics in his definition. Thus, he has in mind *a reality test as the basis of epistemic communities,* but fails to declare what it might be. Instead, he dismisses this question by announcing that his primary concern is "the political influence of such groups on collective policy making rather than the correctness of their policy advice" [1990: 22]. But if knowledge is the source of that influence, then the failure to examine epistemological questions leaves a big gap, even if such an examination would require an interdisciplinary move beyond the study of politics.

The epistemological question also arises from the observation that most of the literature on epistemic communities identifies these groups with particular schools of economic and political thought **[26]**. If the power and the unity of epistemic communities are derived from their shared causal views, it is strange that such groups should be so prominent in the disciplines with so little consensual knowledge. Hence, it is not surprising that epistemic communities in the social sciences tend to be coupled with dominant political institutions, whether they be nation-states, classes or interest groups. Their "knowledge" may be a form of ideology; their "facts" are not necessarily grounded in what Bhaskar calls the intransitive ontological dimension. The power of epistemic communities is rooted in Gramscian hegemony; ideas are linked to power but there is nothing distinctive about knowledge. Knowledge-based power is reduced to an epiphenomenal expression of conventional forms of political power. If one finds this reduction distasteful and wishes to understand how knowledge influences policy, then one must be willing to address epistemological questions, even if that means crossing disciplinary boundaries.

While political power and scientific knowledge are mutually embedded and complexly related, as I have argued, they are not identical; to conflate the two entails an undesirable commitment to relativism. On the other hand, to drive a wedge between them is to misunderstand both categories and tends to create unrealistic expectations of the ability of experts to force political consensus. Only a detailed contextual analysis of concrete political processes can uncover how knowledge is translated into power, how interests mask themselves as knowledge, and how the fixed and conventional dimensions of facts get expressed.

3.4 The Context of International Environmental Policy-Making

Environmental issues insinuate distinctive temporal and spatial understandings into international political processes, tendencies which are amplified as the problems take on planetary proportions [Ruggie 1986; Dahlberg 1983]. The long-term character of many environmental problems requires an inter-generational perspective typically lacking in other issue areas [27]. The spatial interdependence embodied in global ecology stands in counter-position to the fragmented character of the nation-state. Ecological interdependence gives rise to the transition from an 'open-spaced' to a 'closed-space' conception of the world. This change is evident in the movement from regimes based on principles of open access and free use for international commons (the high seas, the atmosphere, outer space and climate) to more restrictive regimes [Brown, Cornell, Fabian and Weiss 1977]. The Montreal Protocol, quite possibly the only successful treaty on a global environmental problem, is an important expression of this movement.

One factor which is both cause and effect of this spatial and temporal expansion is the emergence of new actors in global politics: specifically, a network of scientific and technical experts with access to consensual knowledge about the long causal chains involved in extended space and time frames [28]. In general, uncertainty increases as the causal chain of events moves further into the future. Not only does this empower a new class of environmental experts to advise policy makers, but it may inspire a different kind of decision making among the policy makers themselves. John Ruggie describes it as a "'bias shift' . . . away from a conventional problem-solving mode, wherein doing nothing could be favoured on burden-of-proof grounds, toward a risk-averting mode, wherein prudent contingency measures would be undertaken to avoid risks we would rather not face [1986: 231]. Many participants in the Montreal Protocol explicitly refer to such a shift in their accounts of the negotiations.

Particularly important is the emerging class of "risk professionals" [Dietz and Rycroft 1987]. Some see this group as part of the "new class" of post-industrial society [Cotgrove 1982]. Whatever the label, the implications for international relations are unclear. Do they constitute a transnational alliance, or do they merely rationalise their own countries' interests? Does the use of science as a guide for policy lead to more rational policy? Does the fact that risk analysis has been largely an American enterprise entail the US preponderance in environmental regimes, or does it engender scepticism toward such methods on the part of other participants?

Environmental policy-making is becoming increasingly linked to the relatively new field of risk analysis, which was developed largely in the US from economic models **[29]**. At least one participant in the Montreal Protocol process believes that the success of the negotiations can be traced to the implementation of risk analysis [Benedick 1990]. Proponents of risk analysis argue that the 'scientific' and 'objective' functions of risk assessment can and should be separated from the more overtly political process of risk management.

Several works have been published over the last decade criticising risk analysis variously on epistemological, ethical and political grounds **[30]**. Among other things, those works call risk analysis to task for reducing all values to economic values, for the assumption of linearity in calculating risks, and for failing to recognise the value-ladenness of certain techniques such as 'discounting' the future. At the risk of oversimplification, these criticisms largely reduce to the failure of risk analysis to recognise that the 'science' of risk assessment is more accurately classified as "trans-science" **[31]**. In terms of the argument here, if even the purest of the sciences cannot be wholly extricated from social and political considerations, then it is unwise to have such expectations for environmental problems **[32]**.

The assumption of human rationality is also undercut by the importance of the framing of decision problems and options. Different frames are analogous to varying visual perspectives on the same scene; the apparent size of an objective, for instance, varies with the observer's distance from it. Rationality and logical consistency require that changes of frame should not reverse one's preferences. While it is sensible to question the very existence of such an Archimedean reference point, the problem of framing suggests that even if did exist, it may be unattainable in the practical world of decision making **[33]**.

Due to the highly specialised nature of much of the relevant 'pure' science, an important intermediary category of actors often enters into the environmental policy-making process. These people are not themselves researchers, but have the skills needed to understand the work of academics and other researchers. Typically, they also have a flair for translating that work and framing it in language accessible to decision makers. James Sundquist calls them "research brokers," citing as an example the US President's Council of Economic Advisers [1978: 130]. I prefer the term "knowledge brokers" to highlight the broad range of information that is translated and interpreted. Implicit in the term is the recognition that "injecting science content into governance . . . is itself an act of policy and requires a strategy of information transfer" [Caldwell 1990: 23] **[34]**.

Conditions of high risk, however, do not inevitably lead to agreement, as is evident in the debate over regulating greenhouse gases. Despite a broad scientific consensus that the accumulation of certain trace gases in the atmosphere will lead to global warming, a conservative group of knowledge brokers in the Bush administration managed to block US and, consequently, concerted international action. The persistence of *any* scientific uncertainties can allow contending interests the freedom to interpret information in ways compatible with their own values [Mingst 1981: 164]. As the greenhouse debate confirms, such a process may prevail even under the threat of catastrophe **[35]**.

Moreover, knowledge-based power remains circumscribed within the bounds of state power. While national interests may be conditioned by expert knowledge, state power continues to exert an independent influence. Again, the Montreal Protocol process provides a good example. Once the US was persuaded to push for a strong agreement, US political and economic power were employed to that end; the threat of trade sanctions was at least one cause of the Japanese acceptance of a more stringent protocol. A challenge facing environmental experts and knowledge brokers, then, may be to convince states to employ their power in the service of broader, more long-term interests.

Science is entwined in state power in another respect: all scientists are also citizens. National governments are often reluctant to accept "foreign science," and this is no less true for environmental issues. A major reason that science was able to facilitate cooperation in Montreal was the fact that a genuinely international scientific assessment had been produced [WMO/NASA 1986].

Because of its seemingly unrivalled status as universal legitimator, science is capable of facilitating international consensus and cooperation, particularly on environmental issues. Knowledge can be vital in revealing, shaping and revising nations' conceptions of their own interests. It can also modify the behaviour of states through inculcating alternative sets of norms into the practice of world politics. But the path is much more circuitous than one proceeding directly from knowledge to enlightenment. Knowledge is embedded in structures of power: disciplinary, national and socioeconomic power. Knowledge is neither reducible to power, nor an independent variable. Rather, it is the complex web of interactions between knowledge and power that makes the study of science in international environmental politics a potentially fruitful one. This web cannot be unravelled solely through theoretical inquiry, but requires detailed contextual analysis. Because of the prominence of scientific knowledge in defining and framing the issue of stratospheric ozone depletion, the bargaining process which led to the Montreal Protocol provides an excellent opportunity for such contextual analysis.

CHAPTER FOUR
A BRIEF HISTORY OF THE OZONE CONTROVERSY

4.1 Background

Concern about the ozone layer first emerged in 1970, when the US, Britain, and France were planning to build a fleet of Supersonic Transport airplanes (SSTs). In that year Paul Crutzen, a German scientist, theorised that the oxides of nitrogen emitted in the exhaust of the high-flying planes would catalytically consume ozone [Crutzen 1970]. Harold Johnston, a Canadian chemist at the University of California at Berkeley, calculated that the planned fleet of five hundred SSTs would cause a 22% depletion of the earth's ozone layer [Johnston 1971]. Despite a (US) National Academy of Sciences study which concluded that ozone reductions from the SST would be insignificant, Congress voted in 1971 to terminate funding for the program. Initial fears about ozone depletion were unrelated to CFCs, the target of the Montreal Protocol. Later the "ozone scare" would be blamed for killing the SST program, but evidence suggests that economic considerations were foremost.

Several aspects of this controversy were important for later developments. First, this was one of the first times that scientists became involved in environmental policy. It is noteworthy that the original draft of Johnston's paper, which would be considered cautious by today's standards, was rejected by the *science* editors for having too many references to political questions [Dotto and Schiff 1968: 65]. Second, the SST controversy provided the first major opportunity for atmospheric chemists and meteorologists to spar with one another; the conflict has abated somewhat but was still evident even after the 1985 discovery of the Antarctic ozone hole. Third, after the American program was defended and the question of landing rights for British and French SSTs became primary, the Europeans accused American scientists and environmentalists of "environmental neocolonialism," an accusation which resounded during the Montreal Protocol process .

Perhaps the most important outcome of the SST controversy was the new interest it generated in atmospheric research. Until then, little was known about the upper atmosphere and communication between atmospheric chemists and dynamicists was poor. In 1971, the US Department of Transportation began the Climatic Impact Assessment Program (CIAP) to study the potential impact of the SST. The program's name reflected early concerns about climate, but those concerns later shifted to ozone loss once the skin cancer link gained publicity [1]. A parallel study was done by the National Academy of Sciences in order to ensure objectivity [National Research Council (NRC) 1975]. CIAP scientists revised previous estimates of the altitude at which nitric oxides switch from ozone destroyers to ozone producers, concluding that SSTs would not be as harmful as originally feared [CIAP 1973]. By 1973, a team of scientists working with Richard Stolarski and Ralph Cicerone had determined that emissions from the planned space shuttle posed more of a threat to stratospheric ozone than the SSTs. They found that chlorine in the shuttle's exhaust would destroy ozone far more efficiently than nitric oxides [Stolarski and Cicerone 1974].

Because the space shuttle was the first human-made source of chlorine to be studied, it may be regarded as the link between the SST and CFCs. Politically, it had important repercussions, for it sparked a great deal of concern within the US National Aeronautics and Space Administration (NASA). At the time, the shuttle comprised one third of NASA's budget. As the successor of the Apollo project and the only man-in-space program, it was NASA's "prestige project." Small wonder, then, that scientists were pressured by NASA to downplay the significance of the shuttle as a source of chlorine, and to instead emphasise volcanoes [Dotto and Schiff 1978: 127]. As a result of the shuttle controversy, NASA convinced Congress that it was the best lead agency to study ozone, and thereafter gave the shuttle-chlorine problem little publicity. Meanwhile, James Lovelock, an independent British scientist, had measured CFC-11 in the lower atmosphere. Emphasising that the compound posed "no conceivable environmental hazard," he proposed that it could be used as an indicator of the earth's wind and weather patterns. His estimates of atmospheric concentrations for CFC-11 were about the same as industry figures on cumulative global production, indicating that the chemical was apparently imperishable [Lovelock 1973: 94-96]

Two chemists at the University of California at Irvine, F. Sherwood Rowland and Mario Molina, began to wonder if CFCs might eventually be destroyed in the stratosphere. They argued that although CFCs are insoluble and chemically inert in the troposphere, "odd-chlorine" atoms in the stratosphere could initiate a catalytic chain reaction with ozone molecules [1974]. Their paper defined the major issues of the ensuing controversy, the most important of which, from a policy perspective, was the issue of time scale. Because the atmospheric lifetimes of CFCs are 40 to 150 years, a steady-state concentration would not be reached for decades, even if CFC production ceased immediately. Rowland and Molina were particularly disturbed by the rapid growth in CFC production, which had doubled roughly every five years since World War II. On the basis of 1973 growth rates, they predicted that ozone would be depleted between 7 and 13% before stabilising by the end of the next century. Later that year they called for an immediate ban on aerosol propellants, accounting for half of all CFC usage in the US.

While the public was shocked by the Rowland-Molina hypothesis, [2] the scientific infrastructure and the federal bureaucracy were well prepared to take up the issue as a result of CIAP and related studies. In the words of Carroll Bastian, who co-chaired the federal Interagency Task Force on Inadvertent Modification of the Stratosphere (IMOS) charged with coordinating the scientific assessment, the response to the new CFC-ozone problem was "an unusual (if not unique) example of federal interagency coordination on an environmental regulatory question" [1972: 167]. The IMOS study affirmed the Rowland-Molina hypothesis, but postponed its policy recommendations until the National Academy of Sciences completed its study [IMOS 1975]. Some American states, such as

Oregon and New York, were unwilling to wait, and passed legislation restricting the use of CFCs as aerosol propellants. Those states had access to the same science as did the federal government and other states, but they chose to err on the side of caution, possibly as a result of greater pressure from their constituents.

Industry's main response was to call for further scientific study. The International Manufacturing Chemists Association (now the CMA) increased funding for its Fluorocarbon Program Panel (FPP), established in 1972 shortly after Lovelock measured CFCs in the atmosphere. The technical director of Du Pont, maker of half of all CFCs in the US, proclaimed that if studies showed "that chlorofluorocarbons cannot be used without a threat to health, Du Pont will stop production of these compounds" [US Congress 1974]. As the scientific evidence mounted, Du Pont was frequently reminded of its pledge. By tying its policy to the effects of CFCs on human health, Du Pont helped frame the issue in a way that focused debate on the health effects of ozone depletion rather than climatic and environmental change. While the evidence suggests that this mode of framing the issue was unintentional, I will argue in the next chapter that the ramifications of it were felt throughout the debate.

Shortly before the Academy planned to release its study, Rowland and Molina discovered that the photochemistry was more complicated than they originally believed. They ascertained that chlorine and nitric oxides could combine in the stratosphere to create chlorine nitrate ($ClNO_3$), thereby forming "reservoirs" that would retard the ozone depletion rate by both chlorine and nitric oxides [Rowland and Molina 1976]. They concluded that previous estimates of ozone destruction were too high, leading some to accuse the scientists of alarmism [Dotto and Schiff: 255]. The new discovery prompted the Academy to postpone the release of its final report for several months. The report, reflecting the new data, cut its estimate of ozone depletion in half, from 14% in an early draft to 7%. The report's policy recommendations seemed flimsy in light of its conclusion that continued releases of CFCs at 1973 levels could reduce ozone in the upper stratosphere by as much as fifty percent. Deferral of a regulatory decision for a few of years, it is suggested, would cause only a fraction of a percent change in ozone depletion [NRC 1976]. Rowland later lamented that the report "established a debilitating precedent at a crucial time in the whole affair when [it] advocated a delay in regulation" [quoted in Brodeur 1986: 80]. Industry, of course, capitalised on this conclusion, claiming that the report substantiated its position that regulation was premature.

It is worthwhile to consider the scientific merit of the main arguments used by industry and other detractors of the ozone depletion theory in the early years, for these persisted throughout the controversy [3]. The least sophisticated argument was that because ozone varies greatly under natural conditions, a decrease of several percentage points globally would not be disastrous. The fallacy in this logic is seen clearly by analogy with temperature. While a ten degree change in one place on any given day is not of great concern, a decrease of ten degrees averaged globally would bring on an ice age.

Industry also seized upon reports that ozone had *increased* over the Northern Hemisphere during the 1960's to undermine the Rowland-Molina hypothesis. There are three problems with this line of reasoning. First, many scientists attributed the increase in ozone to the ban on atmospheric testing of nuclear weapons early in the decade. Second, the increases might have been greater without CFCs during that period. Third, and most notably, industry's reasoning ignored the fact that the effects of CFCs are delayed because of their long atmospheric lifetimes. This point later became immaterial as satellite data indicated declining ozone levels in the late 1970's and early 1980's.

A third fallacy is the exaggeration of the "self-healing effect" of ozone. As ozone is destroyed in the stratosphere, more ultraviolet light enters the lower atmosphere, where it is absorbed by diatomic oxygen molecules to make more ozone. This reasoning wrongly implies that the net result is no change in the total ozone column. Each ozone molecule destroyed in the upper stratosphere is not matched by a new one at lower elevations. Besides, the computer models had already incorporated the "self-healing" process. This is why much more depletion is expected at the poles; the self-healing effect drops off as the intensity of ultraviolet radiation decreases with latitude. Moreover, redistributing ozone would alter the stratospheric temperature profile, possibly precipitating dramatic climatic changes. With the discovery that CFCs are potent greenhouse gases, this possibility became increasingly salient [Ramanathan 1975].

Despite the Academy's indecisiveness in its 1976 report, Russell Petersen, chairman of the President's Council on Environmental Quality declared that "we cannot afford to give chemicals the same constitutional rights that we enjoy under the law; chemicals are not innocent until proven guilty" [Brodeur 1986: 74]. As a Du Pont chemist for over twenty years before becoming governor of Delaware, Paterson's request that federal agencies develop plans to regulate CFCs was particularly remarkable.

In its amendments to the Clean Air Act in 1977, Congress voted to relinquish authority to regulate CFCs to the executive branch. The legislation called upon the EPA to regulate any substance "which in his judgment may be reasonably anticipated to affect the stratosphere," thereby mandating *preventive* action based upon the best scientific knowledge available [**4**]. The fact that empirical evidence of ozone destruction need not proceed regulatory action later became an important factor in formulating the US position during the international negotiations. The 1977 legislation also required the US to try to convince other nations to adopt regulations mirroring its own. Only Canada, Sweden and Norway, all of which were heavily influenced by the 1976 Academy report, followed the US lead in implementing an aerosol ban [Stoel 1983: 59]. Despite pressure from the US, the European Economic Community (EEC}, as it then was -- thereafter, the European Community (EC), and since January 1994, the European Union (EU) -- refused to adopt an aerosol ban. The British and the French were most resistant, and remained so during the later negotiations [Jachtenfuchs 1980]. The lack of success abroad was one reason that Phase Two, EPA's proposed reductions in non-aerosol uses of CFCs, never went beyond its notice in the *Federal Register*. Other factors included the lack of readily available substitutes, and diminished public interest following the aerosol ban.

Despite industry's protests that no substitutes were available, the day after the aerosol phaseout was announced Robert Abplanalp, inventor of the original aerosol spray valve and a vocal critic of the Rowland-Molina hypothesis, unveiled a new aerosol spray mechanism that did not use CFCs [Roan 1989: 85]. Abplanalp's hydrocarbon substitutes were in use by the time the ban took effect at the end of 1978.

While the CFC-ozone controversy receded into the background politically during the 1980's, it continued to receive substantial scientific attention. Knowledge about the ozone issue parallelled the evolution of computerised atmospheric models. Over 192 chemical reactions and 48 photochemical processes are involved [Brasseur 1987: 8]. The first models in the mid-1970's were one-dimensional, averaging local effects of a single perturbation to yield a uniform global picture. The key analytical tool used in the 1976 NAS study was a model developed by the Lawrence Livermore Laboratory [NRC 1976: 323-331]. Later in the decade, two-dimensional models were developed to take into account latitudinal distributions. Because these models predicted large losses of ozone in the upper stratosphere, they called attention to the possibility of dramatic climate change due to ozone depletion. Most modelling since the late 1970's had used 2-D models. Three-dimensional models, developed in the early 1980's, added wind turbulence and divided the earth into grids. Although they are dynamically more accurate, 3-D models are very expensive to run and their chemistry is somewhat simplistic, precluding widespread reliance on them. The models have become increasingly refined and their results have tended to converge; disparities in their predictions are more often due to new data rather than differences among the models.

Not only did the models improve, but some of the reaction rates were revised. In 1977, two researchers at the National Centre for Atmospheric Research (NCAR) in Boulder, Colorado found that nitrogen oxides and hydroxides would react forty times faster than previously believed [Testimony by Dr. Rowland, Senate Hearings, 12 May 1987]. This implied that nitrogen would be inactivated more quickly, thereby decreasing the amount available to form reservoirs of chlorine nitrate. Thus, ozone could be depleted much faster than either Rowland and Molina or the National Academy of Sciences had predicted in 1976.

The 1979 NASA report, taking these findings into account, painted a bleak picture. This report predicted as much as 16.5% depletion by the end of the next century, and included much more information on climate change and the potential for increases in skin cancer, disruption of the aquatic food web, and crop damage. It concluded with a sense of urgency, declaring that failure to regulate CFCs beyond the aerosol ban already in place would not be prudent [NRC 1979]. In the same year, Britain's Department of the Environment released a much more equivocal study [Stratospheric Research Advisory Committee 1979]. While confirming the 16% depletion estimate, that report vacillated by emphasising that the ozone depletion theory was still a mere hypothesis. It concluded by calling for more research before taking action. (One British scientist later admitted that the US report, using better models, was probably more accurate [Roan 1989: 99].) Not surprisingly, industry on both sides of the Atlantic stressed the inconsistencies between the studies and endorsed the latter's call for further inquiry [Brodeur 1986: 76].

Industry also emphasised that ozone losses had not yet been measured. Du Pont's statement in response to the 1979 studies reflects industry's stance: "No ozone depletion has ever been detected despite the most sophisticated analysis . . . . All ozone depletion figures are computer projections based on a series of uncertain assumptions" [Brodeur 1986: 76]. A vocal minority of scientists, including Rowland and Molina, countered that verifying predictions of events in the distant future is impossible. They argued that once depletion could be confirmed empirically, it would be too late to reverse the damage.

Validating the ozone depletion hypothesis, however, did not require detecting actual ozone losses; most scientists believed that finding chlorine radicals (chlorine and chlorine oxide) would be sufficient. In 1976, James Anderson found them, but he found more than was predicted by the computer models [NRC 1977: 25-27]. Several explanations were given for this discrepancy, but it ultimately turned the scientists' attention to measurement of ozone itself.

Measurement of changes in ozone, in the parts per trillion, presents an array of problems. A few ground-based monitoring instruments, called Dobson stations, have been in place since the 1920's, with many more added after the International Geophysical Year in 1957. Their data, however, may be unreliable. There have been questions about how well the stations are calibrated with one another. They are not dispersed homogeneously, with only a few stations in the Southern Hemisphere and almost none in the equatorial regions. About one third of the existing stations do not report regularly to the World Ozone Data Centre in Toronto [UNEP/WG.69/3: 5]. The stations are also vulnerable to interference from aerosols spewed into the atmosphere by volcanic eruptions. Their data cannot distinguish CFC-caused ozone loss from natural fluctuations, especially those associated with the eleven-year solar cycle. (As sunspot activity decreases, less ultraviolet radiation is emitted, resulting in a drop in ozone.) Finally, while the Dobson stations measure the total ozone column overhead, they cannot decipher changes in distribution. Vertical profiles are measured by means of the indirect ground-based Umkehr method, which deduces levels from the differential absorption of light at various angles, and by balloon ozone-scondes, which take measurements at different altitudes.

The former method is highly inaccurate due to susceptibility to disruption from aerosol particles; the latter is more accurate but very expensive.

Ozone measurements are also gathered by the total ozone mapping spectrometer (TOMS) on board the Nimbus 7 satellite, launched in 1978. Satellite data have the advantage of being able to differentiate ozone levels at various altitudes. Because computer models all predict that the earliest and most severe depletion will occur in the upper stratosphere, this is important. But the satellite monitoring system is also vulnerable to confusion caused by aerosols like volcanic dust, and its instruments can be degraded with age [Cowen 1986]. Consequently, those who sought empirical proof on either side of the controversy were frustrated by the data itself. In 1981 Donald Heath of NASA's Goddard Space Flight Centre, the principal analyst of the satellite data, compared data from the two satellites and believed he found a 1% decrease in total ozone during the 1970's. This finding, the first evidence of global ozone depletion, concurred with predictions of computer models. However, Heath's paper was rejected by the editors of *Science* because of questions about the accuracy of comparing data from two different satellites. That same year, the CMA released a summary of data from the ground-based Dobson network. Their analysis indicated that ozone levels had actually increased during the 1970's. Yet a NOAA study found that ozone over North America had decreased by 1% between 1961 and 1980 [Brodeur: 78] [**5**].

While the scientists disputed the data, some diplomatic progress was made. The EEC Council of Ministers voted in March of 1980 that all members should freeze their production capacity and reduce their consumption of CFCs in aerosols by at least 30% compared with 1976 levels [EC 1980]. At an international meeting the following month in Oslo, delegates from all major CFC-producing nations agreed to voluntarily reduce emissions from non-aerosol applications. Representatives from the EPA took the additional step of offering to freeze annual CFC production in the US at 1979 levels. However, changes in the political environment made further CFC regulation in the US appear unlikely. In the last days of the Carter administration, the EPA honoured its pledge to publish an Advance Notice of Proposed Rulemaking in the *Federal Register* outlining plans for future CFC regulations [EPA 1980]. In response to this announcement, a coalition of 500 CFC users and producers formed the Alliance for a Responsible CFC Policy to ensure that regulations were not "based on unproven and unverified theory" [Alliance 1987: I-1]. As a result of the Alliance's intensive lobbying efforts, of the over 2,000 written comments received by the EPA in response to its notice, only four expressed support for the proposal [Brodeur 1986: 77]. The promise of "regulatory relief" under the Reagan administration made it increasingly doubtful that the rule-making would ensue. Anne Gorsuch, EPA's new administrator, made it clear that she did not take the threat to ozone seriously [US Senate 1981].

Seeing that further regulation was unlikely, Du Pont stopped its research on replacement compounds for CFCs. In the six years following the Rowland-Molina publication, Du Pont had spent over

US$15 million to develop commercially acceptable alternatives. Only a handful of compounds survived the tests, and no long-term toxicology studies had been done by 1980 [Du Pont 1980] [6]. Clearly, the "essential" CFCs would not be replaced as easily or as cheaply as the aerosols had been. In explaining Du Pont's suspension of its research program, Joe Steed, environmental manager for the FREON division, remarked, "There wasn't scientific or economic justification to proceed. How do you trade a possible [environmental] risk for a [business] risk that is real?" [Weiskopf 1988].

In 1981, an international ozone trends study was released which found no clear evidence of actual ozone loss, but predicted that ozone would be depleted between five and nine percent by the second half of the next century [NASA/WMO 1981]. The new figures were based on refined chemical reaction rates and a better data on the interactive effects of CFCs, carbon dioxide, nitrogen oxides and methane. On the biological and human health effects, the NASA/WMO analysis painted a more ominous picture than had past reports. The National Academy of Sciences published its third report a year later, echoing much of the NASA/WMO analysis. Despite the bleak forecast, newspapers optimistically proclaimed that the danger was not as great as was previously believed. This rosy picture only made sense in contrast to the 1979 prediction of 16.5% depletion [Brodeur 1986, 78].

A fourth Academy report in 1984 seemed to offer even better grounds for complacency. Lowering its estimates for eventual ozone loss to two to four percent, the study relied on questionable economic data and some new chemical considerations. Ignoring the fact that CFC production was already rising as the world recovered from a recession, the report assumed that CFC output would remain stable. Methane, increasing by one percent annually, would also slow ozone depletion; oxidation of methane increases ozone in the troposphere, compensating for some ozone lost in the stratosphere. Increasing concentrations of carbon dioxide and nitrous oxide might also counteract some of the negative effects of CFCs [NRC 1984]. However, though total ozone loss was expected to be low, the report substantiated earlier forecasts that the vertical distribution of ozone would be greatly perturbed. According to Rowland, this is important because "looking at the total ozone loss minimises the importance of the issue. No one has yet succeeded in developing a scenario in which the increase of CFCs doesn't decrease upper stratospheric ozone" [Quoted in Roan, 111].

While superficially heartening on ozone depletion, the report contained some disturbing news regarding climate change. The 1984 report claimed that the atmospheric concentration of CFCs was growing ten times as fast as carbon dioxide, the chief greenhouse gas. This information was alarming. Each CFC molecule contributes as much to greenhouse warming as 15,000 carbon dioxide molecules [Rowland 1987]. Combined with other trace gases, primarily methane and nitrous oxide, CFCs could contribute as much to global warming as carbon dioxide. Yet ozone depletion from CFCs is mitigated primarily by rising methane levels, which also increase the risk of global warming. Thus, the 1984 NAS report demonstrated the inseparability of ozone and climate issues, though this was not the message that reached policy makers and the public [*Nature* 1985; *Science Digest* 1984].

Critically, from a communications perspective, conflicting messages from a number of sources -- depending on interests and commitments -- flooded the popular and popular scientific press and media. The ozone hole itself became a jumble of metaphors of doom and perplexion. Simply, there was no media consensus about what line to take. Unlike issues such as racism, sexism, foreign aid, and so on, the environment was not presented as a coherent set of inter-related matters of public interest. More often than not, problems such as water pollution, deforestation, atmospheric pollution, poor agricultural methods and so on were treated as distinct, at best partially-integrated issues, not as part of a broader consensus about the environment. This was not for want of trying on the part of many environmentalists, scientific and political. But for all the commentary and pamphlets, for all the university courses and television programs on related issues, the "consensus manufacturers" had themselves yet to achieve a policy on how environmental issues were to be conceptualised, modelled and communicated. What the entire Montreal process reveals is a vast, pluralistic discourse among informed and ignorant, powerful and weak interlocutors all struggling to understand the issues at hand, and *then* reconcile them with their perceived interests.

Despite a general lack of interest in the issue between 1982 and 1984, a few developments hinted at the prospect of stricter CFC controls within the US. Anne G. Burford's resignation as EPA head amid a storm of controversy in March of 1983 provided an opening for a small group of EPA staff who backed additional regulation. Interest among environmental groups was all but dead. With little support from his colleagues at the National Resources Defence Council (NRDC), Alan Miller decided to sue the EPA for neglecting to follow up on its Phase Two regulations. He was especially troubled by the fact that CFC output was increasing as the global recession subsided. After giving William Ruckelshaus, incoming EPA Administrator, a grace period at his new post, Miller filed his suit in mid-1984. The agency persuaded Miller that a lawsuit compelling unilaterally might undermine the sensitive international negotiations already underway. Miller agreed to delay the suit. But when it became clear that international regulatory measures were unlikely to be adopted at the diplomatic conference scheduled for March 1985 in Vienna, he filed the suit.

Two things happened at the EPA which would have an impact on the international negotiations. Ruckelshaus resolved a longstanding dispute between two EPA offices by transferring authority over stratospheric pollution from the Office of Toxic Substances, where it had been since the mid-seventies, to the Office of Air and Radiation. The Toxics Office supported Burford's position that international regulation of CFCs was premature, but EPA's Office on International Activities felt that the US should at least back a worldwide aerosol ban. The conflict between the two offices was resolved by the switch. Officials in the Air Office viewed CFCs as part of the greenhouse problem and supported international controls. Despite some objections from Burford allies within the US State Department, the EPA succeeded in making its plan the formal US policy.

The other important development was the appointment of Lee Thomas as EPA Administrator in January 1985. Thomas, head of the agency's highly visible Superfund/Resource Conservation and Recovery Act programs under Ruckelshaus, was hand-picked for the job by his predecessor. Initially, observers doubted that a non-lawyer could operate effectively in such heavily legalistic position. Others questioned the ability of a career bureaucrat to exercise political leverage within an administration whose ideological leanings ran counter to environmental regulation [Environmental Forum 1985, 23-26]. But Thomas, with an educational background in psychology, was also known for his management skills and his ability to work well with politicians. Soon after assuming his duties, Thomas was briefed on the ozone issue by scientists from NASA and NOAA. In his words, "I just took a black-and-white view when I saw the data. I knew we had to get [CFCs] out of process. It didn't appear that even a little bit of them was going to be safe" [Lee Thomas 1986].

4.2 The Road to the Vienna Convention

The United Nations Environment Program (UNEP), responding to a statement prepared by the World Meteorological Organisation [WMO 1976, 59], convened a meeting of scientists in 1977 to draft the World Plan of Action on the Ozone Layer [UNEP 1978: 190]. Despite its grandiose title, the plan simply called for research on ozone depletion and on the consequences for human welfare. The Plan identified various UN organisations as lead agencies with respect to specific research efforts. The primary international organisations working with UNEP on the World Plan of action were WMO and the World Health Organisation (WHO). The Coordinating Committee on the Ozone Layer (CCOL) was created to make periodic scientific assessments of the problem [UNEP/WG 7/25/Rev.1]. According to R.S. Mikhail, a meteorologist and deputy director of the UNEP's Environmental Assessment Division which oversees ozone layer activities, UNEP did not intend to push for regelation of ozone-depleting substances. Rather, it saw its role as facilitating an international scientific consensus [Stoel, et al 1980: 276].

UNEP's role as scientific coordinator was surprising at the time, for its staff was small, geographically isolated, and not highly specialised in the relevant sciences. Moreover, its work up until that point was primarily in developing countries, whereas the ozone problem was centred in the developed world, both economically and scientifically. In fact, some observers believed at first that the OECD was the logical candidate for international leadership, since the major CFC producers were all OECD members [Stoel 1983]. In 1982, at the request of several Scandinavian countries, UNEP convened the first meeting of the Ad Hoc Working Group of Legal and Technical Experts to negotiate a framework convention on stratospheric ozone depletion. The UNEP secretariat prepared a paper in November 1982 for the working group in which it outlined alternative structures for protocols or annexes to a draft convention [UNEP/WG.78/3]. The paper examined fifteen different conventions and protocols from its regional seas program. Following these examples, the group agreed to draw up a "framework convention" to be supplemented by protocols and annexes calling for specific control measures.

But the negotiations became polarised when Finland and Sweden submitted a draft protocol, known as the Nordic Annex, calling for an aerosol ban and limits on other uses of CFCs. The EC, Japan and the Soviet Union strongly opposed the Proposal. arguing that the science did not mandate such measures [7]. The Europeans claimed that their aerosol reductions and production cap were sufficient to stem destruction of the ozone layer. The US refused to back the Nordic Annex, arguing on procedural grounds that provisions for specific controls should not precede a framework convention. American supporters of the Nordic Annex maintained that such a stance sent the message that the US aerosol ban had been a mistake. With Burford's resignation, the US joined the Nordics and Canada in what became known as the Toronto Group after the group's first meeting in that city. Australia was sympathetic to the Toronto Group's position [Sand 1975: 42].

The working group met seven times between 1982 and 1985, and it often seemed that no agreement would be reached among the fifty participating countries. The debate about CFC controls eclipsed other issues, at times threatening to derail efforts toward a framework convention. The EEC, bowing to the wishes of Britain and France, supported a cap on production capacity for CFCs 11 and 12, and a 30% reduction of non-essential aerosol uses. The Toronto group viewed this proposal as self-serving since the EEC had already adopted a 30% aerosol reduction and its producers were only operating at about 65% capacity. So the EEC proposal would not require them to modify their behaviour at all. Although any of the Toronto group's four proposals would have reduced CFC emissions more than the EEC proposal, they were also seen as self-serving because they focused on reductions in aerosol uses. The first two proposals were a ban or an 80% cutback on non-essential aerosol uses and exports. A third option was a 20% reduction of all CFCs within four years. A fourth was a 70% reduction of aerosol uses, accompanied by a production cap. The EEC also argued that the Toronto Group's proposals did not take into account long-term growth of "essential" uses of CFCs, a conviction that the US negotiators later came to share. Through several years of talks, neither side was willing to accept new constraints on its own industries [Sand 1985: 22].

Two legal issues impeded the negotiations: the procedure for settling interstate disputes and the voting status of the EEC. After the 1984 International Court of Justice verdict against the US for mining Nicaragua's harbours, the US insisted on a clause allowing the option of third-party mediation [Article 11, *Vienna Convention*]. On the second issue, the EEC and other regional economic integration organisations would be permitted to vote on behalf of their states if those states chose not to vote [Article 15].

In the end, a global framework convention was adopted in March 1985 without any control provisions. UNEP officials proudly proclaimed that the Vienna Convention was the first legal instrument to protect the global atmosphere; even more important, they declared it to be a sign of "political maturity" in that it dealt with a "distant threat," expressly recognising an "intergenerational responsibility" [Mostafa Tolba, quoted in Sand 1985: 20]. The Vienna Convention, consisting of 21 articles and two technical annexes, establishes a *norm*, both in terms of state behaviour and the environment itself. It mandates that states have a general obligation to refrain from activities which are *likely* to modify the ozone layer. *No* change in the ozone layer is acceptable; the environmental norm is an unmodified ozone layer, and the international norm is behaviour which sustains the environmental norm. Moreover, empirical evidence of ozone depletion is not required before states must modify the behaviour of CFC producers within their borders. Rather, the *probability* that certain actions will be detrimental to stratospheric ozone should be enough, although the responses of states remain purely voluntary. Since this probability can only be known through the use of scientific models, policy makers must look to the scientists to identify the actions they should take. Consensual scientific knowledge becomes the *sine qua non* of state conduct.

The Vienna Convention negotiated under UNEP's auspices, is careful to avoid the implication that the environmental norm it seeks to establish interferes with the principle of state sovereignty in any way. The second sentence of the preamble cites principle 21 of the 1992 Declaration of the United Nations Conference on the Human Environment, which provides that "states have . . . the sovereign right to exploit their own resources pursuant to their own environmental policies, and the responsibility to ensure that activities within their jurisdiction or control do not damage the environment of other states."

In addition to establishing a general responsibility of states to protect the ozone layer, the Vienna Convention calls for various forms of scientific and technical cooperation among its participants. Despite the vagueness of some of these provisions, they were crucial for negotiating the Montreal Protocol. As early as 1982, it was recognised that, "the primary and paramount need [was] for global production statistics for all substances which, directly or indirectly, may be involved in stratospheric ozone modification processes" [UNEP/WG.78J6: 3]. Without good production data, reliable predictions of ozone depletion cannot be made, regardless of the computer models' sophistication. Western Europe, the US and Japan report their production data for CFCs 11 and 12 to the CMA. Communist countries, however, did not, and they refused to divulge them until after the Vienna Convention was adopted. In many countries there is no existing legislation under which the government can compel firms to provide production statistics, especially when a company can show that disclosure would be prejudicial [UNEP/WG.78J6: 5]. Additionally, the CMA does not compile data on CFC-113 or the halons, so those data were subject to greater uncertainty. The major contribution of the Vienna Convention, it can be argued, was that it prompted the gathering and disclosure of critical economic and technical information. In order to ensure some degree of commercial and national secrecy, the convention contains safeguards such as aggregation of data. The Convention also calls for scientific cooperation in developing computer models, monitoring the stratosphere, and developing alternatives to CFCs. The most immediate tangible result of these provisions were greater efforts to ensure that instruments were consistently calibrated.

The greatest significance of the Vienna Convention was that it represented the first global consensus that there was indeed a problem. The failure to adopt any control provisions at Vienna was fortuitous, since any protocol adopted at that time would not have been as comprehensive as the Montreal Protocol that was adopted two years later.

CHAPTER FIVE
THE EMPLOYMENT OF KNOWLEDGE IN THE GLOBAL CONTROL OF CHLOROFLUOROCARBONS

5.1 The International Scientific Consensus

The Montreal Protocol represents a transition from a regime based on the principle of free access to the upper atmosphere to one based on limited access. This regime shift was made possible by the extended time frames embodied in sophisticated computer models, time frames which inevitably clashed with the shorter time frames typical of political decision making. Without the existence of a scientific consensus that continued CFC emissions would cause ozone loss, the Montreal Protocol would have been inconceivable. Although some scientists saw themselves as totally apolitical, as a group they were nonetheless powerful political actors simply by virtue of their authority as interpreters of reality. Their power had little to do with control or domination, but was a function of the perceived legitimacy of their knowledge.

In July 1986, just over a year after the Vienna Convention was signed, the most comprehensive report on atmospheric ozone to date was published [WMO/NASA 1986]. This document "established a common understanding of the fundamental scientific issues among all participating nations" [Statement by A. James Barnes, Deputy EPA Administrator, Congressional Hearing, 29 October 1987] **[1]**. Building upon the 1982 assessment, which was co-sponsored by three US scientific agencies and one international scientific agency, the 1986 report was co-sponsored by the same three US agencies, three international organisations, and a West German scientific agency **[2]**. It was generated through a series of thirty focused workshops held in 1984 and 1985, with participation from approximately 150 scientists from eleven countries. The purpose of the report was:

> to provide governments around the world with the best scientific information currently available on whether human activities represent a substantial threat to the ozone layer. [WMO/NASA 1986: 4]

It was an explicit response to the Vienna Convention, which acknowledged the existence of a problem and called for coordinated research and monitoring but had failed to adopt control measures. Dr. Robert Watson, an atmospheric scientist with NASA and "a master at blending the roles of bureaucrat and scientist," [Roan 1989: 159] coordinated the assessment. He had played a more minor part in the 1982 assessment, during which he and a few others perceived the need for more international participation. Watson states:

> Before 1980, there were several assessments being done periodically in different countries. This must meant that the policy makers spent more time looking at the differences between them rather than at the similarities, even when they said basically the same thing. With one document, even if there was a range of views in it, then the international policy community had a constant base [Watson 1986: 183].

In particular, some of the scientists were concerned with "what the British government was putting out" [Cicerone 1986] [3]. The reasons for including broad representation were political and not merely scientific. Watson made a special effort to include British scientists in the workshops, but feels that he should have made a greater effort to include scientists from the developing countries and the Eastern bloc. He attracted scientists to the workshops by emphasising their professional value, stating that "the world's best atmospheric scientists would be there" and that "a document would come out of them that we could all be proud of" [Cicerone 1986].

Watson and the other scientists who saw the need for a highly international report "wanted to break down the false scepticism that wasn't based on fact, but rather on things like, 'This is only American research'" [Watson 1986]. Thus, the rationale for the structure of assessment was inherently political: to mitigate nationalistic biases. And the timing of its publication -- just before the international negotiations began -- could hardly have been more fortuitous [4].

The assessment, more than any previous one, concluded that "to really understand the processes which control atmospheric ozone and to predict perturbations we are drawn into a study of the complete Earth system" [WMO/NASA 1986: 2]. Consequently, it delved more deeply than past reports into trends that might modify the impact of the halogenated carbons on ozone, including reactions involving other trace gases as well as tropospheric, solar and dynamic processes. More than past assessments, the 1986 volumes emphasised the issue of climate change more than simply ozone depletion and increased ultraviolet radiation [UNEP/WG.151/Background 4]. It devoted the bulk of the chapters to analysing trends of trace gases and aerosols, tropospheric processes and changes in the vertical distribution of ozone, all of which are climate issues.

Nonetheless, the chapter modelling predictions of ozone changes received by far the most attention for its policy implications [Chapter 13]. On the basis of continued release of halocarbons [5] at 1980 levels, one-dimensional models predicted an eventual total ozone loss by the end of the next century of between 5 and 8%. Two-dimensional models predicted an average of 9% total ozone loss, with as much as 14% loss in the polar regions [WMO/NASA 1986: 18]. All models predicted that continued release of CFCs 11 and 12 at the 1980 rate would reduce ozone by 40% or more at an altitude of forty kilometres [WMO/NASA 1986: 18]. The report stressed that these predictions were strongly dependent upon emission trends for other trace gases, most notably methane, nitrogen oxides and carbon dioxide, and that these constituted the greatest source of uncertainty for the modelled predictions. The 1986 predictions are a bit gloomier than those in the 1982 WMO/NASA assessment and the 1984 report by the National Academy of sciences, but not nearly as extreme as the ones made at the end of the 1970's [NRC 1982; NRC 1984].

Observational data were also reported. Total column ozone measurements using the Dobson network indicated no significant trend between 1970 and 1984, although there was some evidence of losses in the upper stratosphere. These agreed with the modelled predictions. Measurements based upon the Umkehr method showed negative trends which were consistent with one-dimensional models, but that data was suspect due to interference from aerosol particles [WMO/NASA 1986: 21].

Despite the uncertainties, especially with respect to the emissions report is one of confidence. No major new chemical reactions had been discovered since the last assessment; many of the reaction rate coefficients had been refined; and the measured observations concurred with the models. In general, the reader had the sense that knowledge about atmospheric ozone had reached a plateau where no new major breakthroughs or controversies were expected. Quite possibly the only indication to the contrary is a brief reference to "a considerable decrease in Antarctic total ozone during the spring period since about 1968." Little is said about this phenomenon except that is "presently the subject of further analysis" [WMO/NASA 1986: 20].

From a political perspective, the most interesting thing about the 1986 international assessment is that it offered something for everybody. Those who did not perceive the problem as a serious one could argue that the predictions were not dire and would not come about for quite some time, that no total ozone losses had been detected with any certainty, and that the impact of CFCs would be tempered by rising levels of methane and other trace gases. Those who believed that the problem was a grave one could point to the fact that the models predicted more ozone loss than they had two years before and, consistent with the models, ozone losses had already been measured at certain altitudes and were predicted to become very large by the middle of the next century. They could also point to the potential folly inherent in relying on increased levels of greenhouse gases to mitigate the impact of CFCs. Finally, the scientists themselves benefited. The need to clarify the remaining uncertainties entailed a growing research budget for atmospheric scientists; by 1987 the NASA budget for ozone research had risen to about US$100 million. Whatever their interpretations of the science, all participants agreed that the WMO/NASA study represented an international consensus which constituted the scientific basis of the ensuing negotiations.

Those who took a pessimistic view of the WMO/NASA study brought to light an issue that would eventually become hotly debated and even shift the context of the negotiations. The atmospheric scientists, for all their sophisticated modelling, were ultimately dependent upon economic projections of CFC growth rates for their own predictions of ozone depletion. These scientists, having little knowledge of economic trends, had arbitrarily chosen 1980 as a baseline date from which to draw their conclusions. A few individuals, particularly a group of analysts at EPA, realised that projections premised upon 1980 emissions were misleading for three reasons. First, the world had been in a recession at the time, and demand for CFCs is strongly correlated with economic growth. Second, the US has recently enacted its aerosol ban and other countries did not seem likely to emulate its example, so the 1980 figures were artificially low. The 1980 date did not take account of economic growth, nor did it consider the potential for mushrooming demand for CFCs in the developing countries [Seidel 1983]. Third, the figures did not consider the burgeoning demand for CFC-113 in the electronics industry. Since predicted ozone depletion varied greatly with the quantity of CFCs emitted, this group of knowledge brokers [6] took up the task of demonstrating the inadequacy of the 1980 baseline date, and showing that the modelled predictions were much more reliable economic data were applied.

The political reception of the WMO/NASA assessment evinces much of what has been discussed above regarding the inter-relatedness of power and knowledge. First, the very existence of such an international assessment was political, as was the method of assembling it. Second, the atmospheric science itself could not stand alone, for it was fundamentally tied to economic projections, and these were in turn intertwined with interests and values. Third, the complexity and uncertainty inherent in the science allowed the contending political factions to interpret the report's conclusions in ways that bolstered their own preconceptions -- in spite of the fact that the science was more refined than at any previous time. All of this, plus the fact that the purpose of the document was to inform political leaders, suggests that the WMO/NASA assessment was not merely scientific but trans-scientific. The issues addressed, and the manner in which they are addressed, straddle the line between what are ordinarily considered science and politics.

5.2 Meetings Preceding the Negotiations

After the international community failed to adopt control measures as part of the Vienna Convention, it was clear that a consensus was needed on critical scientific and economic issues before a political agreement could evolve. UNEP decided that the best vehicle for forging such a consensus would be a two part workshop focusing on economic issues and a separate conference on health and environmental issues, both of which were held outside the actual negotiating context. The workshops were also part of the settlement of the 1984 lawsuit brought against EPA by the National Resources Defence Council (NRDC). In January of 1986, EPA unveiled its "Stratospheric Ozone Protection Plan," to be coordinated by John Hoffman, which called for new scientific and policy assessments and for a series of domestic and international workshops. During the Vienna Convention negotiations, according to Ambassador Benedick, those who had advocated adopting control provisions "may have put the cart before the horse: [they were] trying, in effect, to make a risk management decision before conducting a risk assessment" [quoted in Roan 1989: 154]. Though I take issue with Benedick's dichotomy between science and politics, for as I argue below the workshop was fraught with politics, there is little doubt that the atmosphere was *less* political than a negotiating session simply because there was no bargaining. Moreover, while science did not provide a set of objective, value-free facts from which a political consensus could be forged, the ontological dimension of the facts was undeniably present. For instance, the existence of the 1986 WMO/NASA assessment made it very difficult to question whether ozone could be destroyed by CFCs.

The first part of the economic workshop, held in Rome during May of 1986, was "a grave disappointment, characterised by bad temper and disagreements" [UNEP 1989: 8]. However, at the second session, held in Leesburg, Virginia that September, "trust was built up, and for the first time an obvious international will to forge a successful protocol emerged" [UNEP 1989: 9]. The divergent outcomes are surprising given that the first session addressed production and emission issues, whereas the second took on the more contentious problem of alternative regulation strategies. Because the modelled predictions of the atmospheric scientists were so tightly coupled to economics, a common understanding had to be reached in this domain before any strategy could be adopted. Moreover, if substitutes and alternatives for CFCs were not feasible in the foreseeable future, then adoption of a protocol might be blocked simply for pragmatic reasons. Thus, the workshop addressed current production data, CFC growth projections, the costs and benefits of regulatory scenarios, and technical control options. it addressed these issues not simply as a prelude to international regulation, but also because an accurate scientific assessment was impossible without good economic data [UNEP/WG.151/Background 1]. Thus, the workshop's explicit purposes were both political and scientific.

Given the lack of concordance at the Rome meeting, it is remarkable that the Montreal Protocol was negotiated in just over a year. There was not even agreement on figures for *current* production, use emissions and trade of CFCs, much less than on future trends. UNEP had sent out 170 requests for data and received only 18 responses, which led the most sceptical participants to question whether anything could be done about production. Most participants agreed that data provided by the CMA accounted for 85% of all production for CFCs 11 and 12, although the fact that some countries, most significantly China, had not even begun compiling data, added fuel to the sceptics' fire [UNEP/WG.151/Background 1]. Those who favoured stringent controls pointed out that the CMA figures did not include CFC-113, which was the fastest growing market. Given the lack of consensus on current production, the level of uncertainty was forseeably much greater for future projections. Industry argued forcefully that it had no plans to expand. And while the CMA had recorded 7% yearly increases in CFC production for 1983 and 1984, those who had an interest in predicting slow growth, and thus low ozone perturbation, pointed out that two years were not enough to establish a trend [UNEP/ WG.148/2 Annex I: 1].

Regarding costs and effects of CFC controls, the discussion focused on aerosol controls. A study done by ICF and commissioned by the EPA was summarised as having found that switching from CFCs to hydrocarbons in the US resulted in savings to both industry and consumers [UNEP/WG.148/2 Annex II: 2]. Those findings were strongly attacked by industry and the British representative, who argued that European consumers prefer the finer mist produced by CFC aerosols [UNEP/WG.148/2 Annex III: 3]. This section of the workshop concluded that "studies provide no comprehensive basis for estimates of the cost and effects of CFC controls" [UNEP/ WG.148/2 Annex III: 41].

What is interesting from an analytical perspective (though it is not surprising) is that the positions adopted on these "facts" generally corresponded to the interests of the actors. What was accepted as knowledge was tightly linked to the political and economic interests of the principal antagonists, the US and the European Communities (EC). Representatives from the EPA argued most forcefully that trends could be predicted from existing production statistics and that growth would accelerate. The EC's CFC industry was operating at only about 65% capacity and was the world's top exporter, while American industry was operating at nearly full capacity and was threatened with further domestic regulation. Not surprisingly, then, the EC opposed strict controls and favoured a cap on production capacity. Within the EC was a range of positions, with the British and French emphasising the uncertainties and the Dutch coming closest to the American position. Because of its dependence on CFCs for its microelectronics industry, Japan tended to side with the EC. China, with its plans to massively expand its refrigeration industry, predictably raised the same kinds of doubts as the British and the French.

Just three weeks after the Rome meeting, UNEP and the EPA co-sponsored and International Conference on the Health and Environmental Effects of Ozone Modification and Climate Change. The UNEP/EPA conference, which was attended by scientists and officials from approximately twenty countries, resulted in a four-volume publication [Titus 1986]. Dozens of paper were delivered but, unlike the workshop, there was little time for discussion and debate. The topics included atmospheric modelling; the effects of ultraviolet radiation on human health, aquiculture and marine systems; and the impact of global climate change. Although none of the science was new, this conference was important for providing the most comprehensive compilation to date on a range of issues related to ozone depletion, especially health and biological effects. One striking feature of the conference is the extent to which the issue of climate change dominated the agenda; nearly twice as many papers were delivered on this topic as on ozone modification and ultraviolet radiation. According to Lee Thomas,

> [Atmospheric] science tells us that ozone depletion and global warming are inexorably interconnected. However, the domestic and international politics surrounding each issue are separate and unique. Combining the two in one conference had the potential to confuse and compound the political controversy surrounding each issue. Separating the issues would fail to address their physical interdependence. In the end the choice was clear: we resolved this issue by recognising that this conference is first and foremost a scientific meeting, not a political one, and therefore it should be organised around the science [Thomas 1986: 27].

For some environmentalists at UNEP and EPA, the ozone issue was nested within the larger and more complex climate issue, and an agreement on the former could be used as a springboard for dealing with the latter. Although the issues were linked during the scientific conference, as well as at meetings of UNEP's Coordinating Committee on the Ozone Layer (CCOL), they rarely were coupled in the policy debates. Brief mention was often made of the fact that CFCs would account for almost one quarter of the anticipated rise in global temperature in the next century [UNEP 1987: 25], so that regulating these compounds would be a partial solution to the problem of greenhouse warming. But otherwise, from a policy perspective, the problem of ozone depletion was strictly one of increased ultraviolet radiation. This is particularly remarkable given that the major scientific studies since 1984 had framed the issue substantially in terms of climate [National Research Council 1984: WMO/NASA 1986]. The issue of ozone depletion, defined narrowly, was perceived as politically manageable, whereas the climate issue was seen as a much greater challenge.

The second session of the economic workshop, held in Leesburg near Washington DC, about four months later, was characterised by a very strong US presence. At least in part because of the meeting's location, the US had fourteen official participants. Britain, with the next largest number, had four. Thirty-one papers were presented, nearly half of which were from the US. While some of these were from industry, eleven papers were presented by EPA and its contractors, Rand and ICF. It is clear from the proceedings that the EPA was extremely well-prepared; if it did not dominate the meeting, it was certainly the strongest and most well-organised delegation in attendance. As in the June UNEP/EPA conference, little new scientific knowledge was presented, and the few strictly scientific papers that were presented agreed on their modelled predictions of ozone depletion [Papers 8,9 and 11, UNEP/WG.151/Background 2: 6]. The scientific papers presented by industry emphasised the uncertainties, and in particular that future increases in CFC growth rates would be possible with large increases in methane emission rates.

More important than the actual scientific knowledge that was communicated was how the various policy proposals were justified through the science. The papers presented by EPA officials and contractors are particularly interesting; many of them are well-crafted interpretations of science designed to advance the cause of a strong regulatory protocol. As a consequence of how they framed the issues, the knowledge brokers from EPA were able to highlight certain issues and increase the sense of urgency among the participants. Two papers by John Hoffman, Chairman of EPA's Stratospheric Protection Task Force, are especially noteworthy. In one, he focused on the long atmospheric lifetimes of CFCs, citing such statistics of the CFC-11 emitted in 1987 will still be in the atmosphere by 2000, and 56% by 2030. He also broached the "chlorine loading" issue, stating that in order to stabilise concentrations as present levels, the presence of past emissions in the atmosphere would require an immediate 85% cutback in CFC emissions [UNEP/WG.148/3, Annex I: 5]. I will argue later that this mode of framing the science was helpful in shifting the flow of the debate in the direction of a stronger protocol, and that this formulation gained much of its salience from discovery of the Antarctic ozone hole. In his second paper Hoffman applied Ivar Isaksen's two-dimensional model to show that global CFC emissions would need to be lower than 1980 levels in order to limit total depletion to 2% and depletion at 50 degrees latitude to 5% [UNEP/WC.148/3, Paper 13]. Without explicitly addressing it, Hoffman's analysis demonstrates the danger inherent in the EC's proposed production capacity ceiling, which would have permitted far greater ozone depletion.

Another EPA paper by Stephen Seidel directly addresses the EC's proposal for a production capacity cap in terms of its impact on ozone depletion [UNEP/WG.148/3, Annex I: 31]. Seidel shows that 7% total ozone depletion would occur by 2050 and 14% by 2075, with far worse depletion at the northern latitudes. Never do Seidel and Hoffman refute the EC position on *political* grounds, although it was patently unfair to US industry, which was operating at nearly full capacity while EC industry could have expanded by as much as 40%. Rather, they simply state in *scientific* terms the modelled predictions of the EC position.

In their papers, two EPA contractors focus on the costs of postponing regulation [UNEP/WG.148/3 Annex I, Papers 5 and 12] [**7**]. The first, by James Hammitt of Rand, concludes that immediate controls would be the most cost effective option if the likelihood that further emission reductions would be required exceeds 0.3 to 0.5. Although the Antarctic ozone hole was not discussed, the implication is that a greater than 50% chance that it was CFC-induced should entail prompt regulation. A paper by Michael Gibbs of ICF surveys various control strategies and argues that since only a limited chlorine burden is tolerable, the most prudent policy would be to control the worst ozone depleters first. Gibbs argued that fully-halogenated CFCs should be regulated, but that eventually compounds like HCFC-22, methyl chloroform and carbon tetrachloride might have to be considered.

The EC, unlike the US, presented its position with little analysis in terms of the atmospheric models, despite the fact that two of the four purely scientific papers were from the Commission of the European Communities (CEC). If the CFC-ozone issue was truly science-driven, as I believe it was, and science is a key source of legitimacy in modern society, then focusing on economic issues with little scientific support was unlikely to advance the EC position.

The EPA papers, on the other hand, did not promote any particular policy position [**8**]; rather, they sought to demonstrate the inadequacy of weak proposals. In his opening remarks to the Leesburg meeting Fitzhugh Green, EPA's Associate Administrator, urged the participants to focus on the concepts of "inevitability and timeliness" [UNEP/WG.148/3: 2]. In other words, some ozone depletion was inevitable, and the effects of CFCs would occur long after their emission. Indeed, in one way or another, all of the EPA papers shared one underlying objective: *to shift the context of debate by extending the relevant time-frame well into the next century*. To this end, EPA emphasised the long atmospheric lifetimes of CFCs and the long-term modelled predictions. An overview of the science was presented at the end of the workshop by Robert Watson, coordinator of the WMO/NASA assessment. One of his main points was that ozone responds to the total burden of stratospheric chlorine; it does not matter whether the source is CFC-11 or CFC-12, aerosol sprays or refrigerants.

This fact strengthened the case of those who favoured a comprehensive approach over controls on specific uses.

Watson also advised the participants not to allow their awareness of the "Antarctic ozone phenomenon" to influence their approach to the protocol. He suggested that scientists should first complete an intensive one-to-two year investigation, at which time the policy makers should re-examine their regulatory policies in light of the evidence [UNEP/WG.148/3: 15]. Watson's counsel was accepted by the workshop participants, and later was adopted as a premise of the official negotiations. By the end of the economic workshop, a consensus had emerged that CFC emissions should be controlled, although the degree and timing of regulation were far from clear. Industry seemed to recognise the imminence of controls, and was beginning to ask that governments "provide clearer signals to the marketplace" [UNEP/WG.148/3, Annex II: 3]. For the first time, the Soviet Union and some smaller nations divulged their production data, furthering the mood of openness [UNEP/WG.148/3: 14]. In his concluding remarks as the session's Chairman, Ambassador Benedick confidently proclaimed that "the ingenuity, good will, and sense of responsibility" that had characterised the meeting would infuse the upcoming negotiations with "the spirit of Leesburg" [UNEP/WG.148/3, Annex II: 3].

5.3 US Industry Shifts Its Position

Within days of the workshop's conclusion, it was apparent that US industry was not immune to the "spirit of Leesburg". Both Du Pont, the world's number one CFC producer, and the Alliance for Responsible CFC Policy, the lobbying group which represented over 500 US CFC producers and users, announced their support for an international protocol that would limit global emissions [Alliance 1987: 11]. Du Pont also announced that alternatives to CFCs could be available in about five years. Although their announcements were cautiously worded and short of endorsing the 50% reductions that were eventually negotiated, industry's change was perceived by many of the participants as a major breakthrough. The shift, however, was greeted with scepticism by Europeans and Japanese in both industry and government. They feared that their American competitors were backing controls because they already had CFC substitutes available to fill the market.

A principal goal of the Alliance, which was formed in 1980 in reaction to the EPA's notice of rulemaking, was to ensure that US industry not be placed at a disadvantage relative to the world market . US industry resented the fact that the US had banned CFCs in aerosol propellants without the rest of the world following its lead. With the threat of further unilateral controls as a result of the NRDC suit and the surge of pro-regulatory thinking within EPA, US industry feared that EPA would "let the US go its own way and commit industrial suicide" [Fay 1987]. Only an international agreement could prevent this. Since US industry supported an international approach, and since it had become clear on the heels of the Leesburg meeting that some kind of regulatory protocol would be adopted, then the change of heart seems neither risky nor drastic. In fact, if controls were perceived as inevitable, then supporting "a limit on worldwide CFC emissions" was a strategically wise move. Although neither Du Pont nor the Alliance specified what that limit might be, their position sounds very much like the EC position, the weakest one advanced at the time. Nonetheless, the shift was perceived as a significant one in that it indicated both an acceptance that CFCs posed a potential problem and support for the upcoming negotiations.

Du Pont's announcement that some CFC substitutes would be available within five years was seen as removing one impediment to a regulatory protocol. The controversy surrounding the issue of substitutes provides a good example of how an apparently factual question can actually be a question of interpretation and politics. As the largest CFC producer and the leading researcher for replacement compounds, Du Pont was a major force in shaping the tone of the policy debates: for many players, the absence of alternatives counselled against a strict protocol. As late as March of 1986, Du Pont claimed that there were no foreseeable alternatives available. RAND's report on market trends, published in May of 1986, assumes the existence of "no remotely feasible alternatives," and its authors claim to have obtained their data from industry [Hammitt 1986]. Yet within less than six months, Du Pont declared that alternatives could be available in five years. No new knowledge had been uncovered in the interim; Du Pont's research program for CFC alternatives had lain dormant since 1980. What had changed were *perceptions* and these had changed on all sides of the debate.

Joseph Glas, Manager of Du Pont's FREON Division, claims that Du Pont never changed its position and never misled EPA. He points out that Du Pont had announced in 1980 the conclusions of its six-year research program: that "seven to ten years may be necessary to reach commercial production for most alternatives, assuming all technical and toxicological programs yield favourable results" ["Fluorocarbon/Ozone Update" June 1980: 2]. Glas argues further that "chemistry was never the issue." Du Pont officials had testified before House and Senate hearings that without incentives it couldn't make these chemicals, the implication being that a regulatory decision would spur the development of alternatives. The unavailability of substitutes was simply a function of the absence of a market. One factor that was linked to Du Pont's apparent shift on the availability issue, although whether it was a cause or an effect of the policy shift remains a puzzle, was the replacement in July 1986 of Dr Donald Strobach, a thoroughly committed sceptic of the Rowland-Molina hypothesis, with Dr Joseph Steed as Environmental Manager for Du Pont's FREON Division. Until his departure, Strobach claimed that his company was so confident in the inadequacy of the Rowland-Molina hypothesis that it had abandoned its research on alternatives [Roan 1989: 147]. Steed was perceived as more open-minded by those at EPA who were working on the ozone problem.

The issue of substitute availability, which appears to be a straightforward matter of "fact," actually hinged on perceptions about market trends, and this in turn hinged on the political question of regulatory policy. Again, knowledge and interests are closely related -- in this case, barely distinguishable. The issue resembles a chicken-and-egg situation: without regulation there could be no substitutes but, at least in the minds of many, without the promise of substitutes there could be no regulation. However, it was obvious at the Leesburg meeting that an influential minority was intent on pursuing a protocol, even in the absence of substitutes. Industry's recognition that CFC emissions should be limited was important in that it helped the negotiators to focus on the issues necessary to gain a consensus [Glas 1989: 148].

While the debate over substitute availability can easily be recast as a conflict over perceptions, the controversy surrounding predictions of future growth rates seems to be a more manifest case of deception on the part of industry. Many participants from the scientific and policy communities believe that they were intentionally misled by industry. As discussed above, the modelled predictions for ozone loss were extremely sensitive; 1980 levels would not have had disastrous consequences, but a 5% growth rate would [WMO/NASA 1986]. At an EPA-sponsored domestic workshop with industry in March of 1986, Strobach and other industry representatives argued that low growth rates should be assumed. They claimed that the refrigeration and automobile markets were saturated, while Rand analysts for EPA countered that this was only true in the industrialised countries. Industry also claimed that it was facing a potential shortage of fluorospar, the mineral from which CFCs are derived. Rand and ICF countered with studies showing that "there was so much fluorospar in the mine" that we could never figure out what to do with it." **[9]**

Working with data provided by industry and making their own economic predictions, the Rand authors predicted annual growth rates for CFCs averaging 3% [Hammitt et al 1986]. Industry claimed that these predictions were "wildly optimistic" [Fay]. Glas maintained that the EPA-sponsored reports predicted 5-10% annual growth, which would have caused enormous ozone depletion. But the predicted growth rates were actually a little less than 3%, and these have been borne out -- even after the Montreal Protocol was negotiated. The Rand figure was in the middle range of those discussed at the Rome workshop, and though a consensus was never explicit on future growth rates, Rand's estimates were cited frequently during the negotiations.

Notwithstanding industry's decision to support the negotiations, it continued to frame the available knowledge in terms most favourable to a weak protocol. Several statements were repeated and emphasised throughout the negotiations: there was enormous scientific uncertainty; additional research was essential; fears were based on "unproven theory;" there was "no imminent danger"; and no total ozone loss had been measured. While these statements were valid, they also disregarded other important facts. While a great deal of uncertainty existed, much was known and uncertainty could cut in both directions; reality could end up being *worse* than predicted, a possibility highlighted by the emergence of the Antarctic ozone hole. While it was true that the threat was not "imminent," it was nonetheless real and would be felt by future generations. And while no total ozone loss had been conclusively measured, losses were being measured in the upper stratosphere, and these were consistent with the models. In essence, all of industry's arguments reflected a short-term perspective, in contrast to EPA's defence of a long-term outlook. This was not surprising: scientists, environmentalists and knowledge brokers are probably more likely than businessmen to think in inter-generational terms.

5.4 The Antarctic Ozone Hole

In May of 1985, just two months after the Vienna Convention was adopted and during the final stages of preparation of the WMO/NASA assessment, a scientific paper was published that would "transform both scientific and political perceptions of the problem" [UNEP 1989: 8]. Dr. Joseph Farman and his colleagues from the British Antarctic Survey reported that for three consecutive years since October 1982, major losses of stratospheric ozone had occurred over Halley Bay [Farman et al 1985]. These losses had not been predicted by any of the models. Though Farman did not seek to explain the "hole" **[10]**, he stated that "chemical causes must be considered" and included a graph showing a strong correlation between atmospheric concentrations of CFCs, which his group had also been measuring, and ozone losses.

Because the Montreal Protocol negotiators explicitly decided to ignore the Antarctic hole, the causes of which had not been determined, it is difficult to argue that it played a major role in the negotiations. Nonetheless, I believe that it did. Despite the fact that the hole had not been predicted by any of the models, it did not underline the power of the scientists or the knowledge brokers. Instead it changed the political context in which the negotiations occurred, and made certain ways of framing the available knowledge more salient than others. The ozone hole created a *sense of crisis* which was conducive to the precautionary approach eventually sanctioned in the Montreal Protocol.

'Crisis' here refers to a different sort of phenomenon than is typically found in the international relations literature. There crises entail "the perception of a dangerously high probability of war" [Snyder and Diesing 1977: 6], and the perceived danger is caused by the actions of nation-states or their agents, rather than by external or natural events. In the minds of many participants, the ozone losses over Antarctica represented a sudden change in the course of events, a change which indicated *a dangerously high probability of ecological disaster*. The existence of scientific uncertainty continued to function as a justification for caution, but the meaning of 'caution' changed dramatically. Farman initially found large ozone losses of more than 20% over Halley Bay in the austral spring of 1982 but, suspicious of the peculiar data, he repeated the measurements in 1983, and again in 1984 with new equipment. During the summer months, the ozone layer recovered almost completely. Farman's measurements went back to 1957, when the Dobson station was established through the International Geophysical Year, so he had a good historical record for comparison. After recording ozone losses over 30% in 1984, Farman decided to publish his data. The discovery quickly sparked commotion within scientific circles, although it received little immediate attention either in the press or within government or industry circles [Brodeur 1986: 84; UNEP 1989: 8]. One scientist describes the initial reaction of those who had been immersed in the issue:

> it was totally unexpected. We scientists are professional sceptics. We looked at it in an almost perverse sense as filled with joy about something new, something we could learn about. If it was predicted, we wouldn't have learned anything [Stolarski 1986: 56].

Many scientists had reservations about the paper, not so much on the merit of the data, but because they had never heard of Farman and his group [**11**]. As Ralph Cicerone puts it,

> The British Antarctic Survey is not exactly a household word. At the time, most of us had never heard of it, [and] had no idea whether these people did good work. You couldn't automatically give credence to the work [Quoted in Roan 1989: 129].

The implications here regarding "peer review" are obvious; basically your work isn't any good, or of any consequence, if it you are "not known" in the right circles. The implications for the import of science communications is enormous. Aside from the ethical issues entailed by such folkloric perceptions of merit, exactly where and by whom need one be known? Does science communications, then, largely boil down to a sophisticated form of public relations?

The first scientists to take Farman's work seriously were those on the NASA team responsible for satellite measurements. As it turned out, they had programmed their computers to reject any anomalous measurements below 180 Dobson units since nothing in that range had ever been recorded. Fortunately the original data had been saved, and when re-examined, it confirmed the Farman findings [Stolarski et al 1986]. This anecdote illustrates an important point: by the early 1980's atmospheric scientists were so confident in their grasp of the ozone issue that they trusted their models implicitly and decided in advance to ignore any findings that contradicted them [Gribbin 1988: 112]. The story substantiates the argument that the theory-ladenness of observation calls into question the possibility of a fully objective science. On a more practical level, this incident led to a deeper sense of humility on the part of the scientists, which translated into a greater willingness, at least in private if not publicly, to support a more robust regulatory policy than they had previously.

Once the Farman findings were accepted, the race was on to explain them. Although most scientists suspected that chlorine chemistry was involved. Three major sets of hypotheses sprang up [*Science*, 14 November 1986] [**12**]. Of the three, only the first had significant implications for the international negotiations. The first were based on chlorine chemistry [Solomon et al 1986; Hamill et al 1986; McElroy et al 1986]. The papers by the Solomon and Hamill teams argued that the Polar Stratospheric Clouds formed in the extreme Antarctic cold could provide surfaces for heterogenous reactions [**13**]. They maintained that chlorine could be sequestered on the clouds, and then released as the clouds dissipated in the spring. Other theorists sought a dynamical explanation [Mahlman et al 1986]. And a third set suggested a link to the eleven-year solar cycle [Callis et al 1986]. The journal *Geophysical Research Letters* published a special supplement in November 1986 devoted to theoretical accounts of the Antarctic ozone losses.

Amid the theoretical debates, NASA's Robert Watson organised the first National Ozone Expedition (NOZE I) to study Antarctic ozone between August and October 1986. Balloon and ground-based measurements were taken of ozone, chlorine and other chemicals. Susan Solomon, leader of NOZE I, broadcast from Antarctica that chlorine chemistry was the culprit, unleashing a storm of controversy among the dynamicists who felt their theories had been overlooked or misunderstood [*New York Times*, 21 October 1986]. The evidence, however, was against the solar cycle hypothesis. The consensus was that only a second expedition, one that included measurements taken from aircraft, could dispel the uncertainties. Thus, preparations were being made for the Airborne Antarctic Ozone Experiment throughout the Montreal Protocol negotiations, but the data from that expedition were not available until after the treaty was signed.

One central fact about the Antarctic phenomenon stood out in stark relief for both scientists and policy makers: *it was not predicted by any atmospheric modeller*. Among the scientists, this was translated into a heightened sense of humility and a frantic investigative effort. In policy circles, it was translated into a belief that the problem might be much worse than was previously thought, and that stricter regulation could be necessary. For most atmospheric scientists the hole generated a sense of crisis. Industry predictably emphasised that the causes were unknown, but the softening of their position just as the scientific debates were heating up was probably no mere coincidence **[14]**. Although the hole represented an enormous scientific anomaly, I have found no evidence that it overtly undermined the authority of the scientists. One participant argues that the hole prompted a major change in the scientists' attitudes, and when policy makers saw that the scientists were visibly disturbed, they in turn became more concerned. If anything, the hole's discovery increased the prestige of those few scientists who, like F. S. Rowland, had advocated strong regulatory measures from the beginning.

However, in a more subtle sense, the Antarctic phenomenon generated suspicion about the validity of the atmospheric models and opened the door to an alternative way of framing the scientific knowledge, one with far more radical policy implications. If it did not undermine the authority of the scientists, it did raise doubts about the science. The models predicted approximately 2% total ozone depletion with constant 1980 CFC emissions [WMO/NASA 1986]. As John Hoffman argued at the Leesburg meeting, an 85% reduction in CFC emissions would be necessary just to keep atmospheric chlorine levels constant [UNEP/WG.148/3, Paper 2]. His calculations required no modelling, only knowledge of production data and the compounds atmospheric lifetimes.

Hoffman's "chlorine-loading" argument gained salience from the ozone hole for another reason. Due to the earth's weather patterns, most chemicals penetrate the stratosphere over the tropics. Ozone, however, is much more sensitive to chlorine at the higher latitudes, where at least some of the CFCs decompose because of their long atmospheric lifetimes. Thus, the latitude at which CFCs break apart makes a crucial difference, but there is no clear sense in the models of when CFCs release their chlorine. The extreme losses over Antarctica suggested that much of the chlorine could be released in the polar regions, which would mean that the models had underestimated the threat. The truth was between the chlorine-loading perspective and the calculations based on ozone depletion potential, but the ozone hole gave credence to the chlorine-loading scheme. Hoffman's simple calculation received enormous publicity in Congressional hearings and in the press [Senate Hearings, 28 January 1987: 61; *Science,* 12 November 1986: 928]. When the issue was framed in these terms, suddenly a phaseout did not seem like such a drastic proposal. The decision to shift the debate from ozone depletion to concentrations was "a strategic one," according to Michael Gibbs [1990]. He recalls the decision as follows:

> There was no new information here, just a different way of framing it. We thought: since the hole may be linked to concentrations, let's shift the debate. This also shifts the focus to the warming issue, and in general to the responsibility to the future. It would not have worked one year before; it only worked because of the Antarctic hole.

Thus, this mode of framing the science had explicitly political purposes: to promote an environmentalist agenda.

Framing the issue in terms of chlorine loading, as Hoffman did, rather than in terms of ozone depletion potential (ODP), as did the atmospheric models, is somewhat misleading because it implies that all forms of chlorine are equally menacing to ozone. But, as the discovery in Antarctica demonstrated, the models were also misleading. As one scientist put it:

> Chlorine doesn't affect us; ozone does. ODP is more sophisticated, more complicated. Two years ago, I would have said chlorine loading was a good measure; now think we should use the state-of-the-art models. At the time the models couldn't account for the Antarctic hole, and now they can. [Dak 1990].

In other words, the hole enhanced the status of a particular mode of scientific framing, one which tended to justify a strong control measures. Groups like the NRDC used the chlorine-loading analysis to promote sweeping controls; 85% became, as it were, the line in the sand for environmentalists. The hole had a more direct impact on one aspect of the protocol that was eventually adopted. Because its causes were not known, and because it illustrated clearly how quickly both natural systems and scientific knowledge about them could change, EPA Administrator Lee Thomas argued successfully for a provision in the text for a periodic scientific update, followed by additional control measures if necessary.

There is little doubt that some kind of agreement would have been reached even without the Antarctic ozone hole. The combination of the 1986 WMO/NASA assessment and the realisation that CFC production was increasing were probably enough to have led to an agreement. However, the resulting protocol would have been less stringent than a 50% cutback, and certain countries might not have been party to it. The large amount of press attention devoted to the issue of 'the' hole permeated the political milieu during the negotiations. A colourful time-lapse videotape assembled by NASA from satellite data dramatically depicted the hole emerging over Antarctica. That segment, which was shown on international television and in US Congressional hearings, had a powerful effect on its audiences. The environmental movement certainly received more media attention, and possible greater deference, as a result of the Antarctic hole, particularly in the US where the ozone issue was politically popular.

The timing of the Antarctic discovery could not have been much better for it to have a major political impact. The Farman paper was published just after the Vienna Convention had been signed, before negotiations resumed, and too late to contribute meaningfully to the 1986 WMO/NASA report. It also coincided with the growing realisation that, with new economic trends, CFC emissions rates would be increasing. Because the hole was expressly ignored at Vienna, it is something of a wild card in developing an explanation of political events. However, few people can ignore a hole the size of Australia, and very few politicians can ignore massive publicity. Despite the lack of a scientific consensus on its causes or, ironically, maybe partly because of that lack, the hole dramatically altered the political context of the negotiations, and it altered the acceptability of various modes of framing scientific debates, lending support to those who believed that the consequences of under-reacting were worse than the consequences of overreacting. In the face of widespread scientific uncertainty and enormous risk, this mode of framing the debate was a force to reckoned with.

5.5 Evolution of the US Position

In November 1986, the US Department of State sent a draft position paper to its embassies around the world to get feedback from foreign governments [Department of State, 3 November 1986]. During the previous summer, EPA and the State Department's Bureau of Oceans and International Environmental and Scientific Affairs (OES), had convened interagency meetings to develop the US position, but there was little interest from other agencies **[15]**. This essentially gave EPA and OES free reign to devise a position. Their draft paper called for a near-term freeze on the consumption **[16]** of CFCs 11, 12 and 113, as well as Halons 1211 and 1301; a scheduled phaseout of these compounds; and periodic policy reviews based upon new scientific knowledge [Department of State, 3 November 1986].

The US negotiating position grew out of an interesting set of interrelated political and scientific considerations. The EPA was under some pressure to promote stringent controls because of the pending NRDC suit, but the proposed phaseout went beyond what the NRDC had expected, and probably went further than would have been legally necessary [**17**]. According to EPA, the NRDC suit was only a secondary consideration. More important was the belief on the part of EPA and OES that, despite the scientific uncertainties, the risks demanded intervention. During the debates, both domestic and international, they argued for "a prudent insurance policy," even without the Antarctic ozone hole [Department of state, 23 February 1987]. But the hole clearly and dramatically drew attention to those risks. The State Department's framework protocol, like most draft positions, did not specify numbers and dates; it simply provided the general authority to negotiate for a scheduled phaseout. The decision to call for reductions of 95% by 2000 was initially made by two EPA staff members who "just decided to fill in the blanks and the brackets." But the casual revisions of a few EPA staff could hardly have become the official US position without the commitment of EPA Administrator Lee Thomas. From the beginning of his tenure, Thomas had taken a considerable interest in the ozone issue. One of his first actions was to order a major regulatory impact analysis, which was released in late 1986 [EPA 1986]. The extent of Thomas' commitment is evident in his address to the EPA's workshop of March 1986, in which he announced EPA's new perspective on the risk of ozone depletion:

> In the face of all this scientific uncertainty, one might ask why has the EPA embarked on programs to assess the risk and to decide whether additional CFC regulations are necessary? Why not simply adopt a 'wait-and-see' attitude and hold off a decision until depletion is actually confirmed? Let me address this question squarely. EPA does not accept, as a precondition for decision, empirical verification that ozone depletion is occurring. . . . *[We] may need to act in the near term to avoid letting today's risk become tomorrow's crisis.* [Quoted in Brodeur 1986: 86; emphasis added]

Thomas' decision to press for a virtual phaseout was based on both scientific and political factors, though he emphasised the former. The political impetus behind Thomas's decision is evident in his additional remarks to the above: "It is clear to me that we have to get rid of these chemicals domestically, that either the NRDC lawsuit is going to drive us in that direction, or Congress." When he looked at the data, he concluded that CFCs had to be banned. Although Thomas believed that regulation was required despite the scientific uncertainties, he also thought that periodic policy reviews should be held as the uncertainties were resolved, and he made sure that the final protocol included this provision.

Thomas' perception that scientists were the driving force behind the US position is somewhat surprising in light of the fact that *very few scientists offered any policy recommendations*, and that *most of those who did thought a 50% cut would be enough.* Watson, for instance, testified before Congress that "the science doesn't justify a 95% cut," and expressed concern that the rush could promote unsafe alternatives [Congressional Hearings, 9 March 1987: 901]. Daniel Albritton of NOAA, the other major US scientist coordinating work on the ozone layer, continued to harbour doubts about the CFC-ozone link, maintaining that "it was just a theory, and as a theory it was worth about a 'B-'" [Congressional Hearings, 9 March 1987: 722]. Since Watson and Albritton were the two top scientists advising policy makers on the ozone layer, it is difficult to see how they could have been the "driving force" behind the US position. Rather than the science itself, it was Thomas' *interpretation* of the science and his own philosophical orientation to the problem of risk that drove his decision. Thus, there was no clear distinction between science and politics and, as I have argued above, all the interlocutors in the ozone hole debate attempted to legitimise their policy positions scientifically. That is to say, science itself had become a metaphor for truth, and those who ultimately were perceived to have won the scientific debate would at least be counted among the righteous and truthful -- if not necessarily the successful. In other words, a critical part of the entire rhetoric of environmentalism in general, not just this particular debate, was -- and is -- to win the scientific high ground, so to speak; for this alone carries enormous moral weight. If scientists say, for example, that chewing gum is carcinogenic, then, providing you know what carcinogenci means (a crucial point) it becomes morally impossible to avoid the implications of trading in chewing gum. Consider the rhetoric surrounding tobacco. Everything hinges on clearly establishing a scientific basis for what everybody already "knows."

When representatives from industry learned of the US position, they registered their discontent with the Departments of Commerce and Energy, sparking a series of intense interagency debates which persisted throughout most of the international negotiations. Recognising the pervasiveness of CFCs in consumer goods, particularly in the import sector, the Department of Commerce feared that hasty regulation could disrupt the US economic infrastructure. The Department of Energy was primarily concerned that CFC regulation would endanger the insulation industry, which had grown rapidly as a result of the oil crises during the 1970's. The Pentagon entered into the debates to defend its access to halons, chemicals used to extinguish fires wherever water might damage equipment or pose a health risk. The Department of the Interior also became involved in the issue, ostensibly because of its role as the largest manager of US lands. The one point of agreement among all the bureaucracies was that the original US position had been developed at too low of a level, and that it should be resolved in the Domestic Policy Council, or perhaps at the Cabinet level.

Under the auspices of the Office of Management and Budget, David Gibbons of the EPA convened a series of weekly or biweekly interagency meetings that spanned a period of several months. The meetings were designed to educate political appointees and senior career officers on the scientific and economic aspects of the ozone problem. However, because the fractious interagency meetings paralleled the international negotiations and spilled out into the press, the US negotiating position was significantly weakened [Doniger 1988: 89].

Throughout the debates, the risk was framed largely as one of increased rates of skin cancer. Yet this narrow mode of framing the issues ultimately undermined the policy position of those who adopted it. In her presentations at the interagency meetings, cancer specialist Dr. Margaret Kripke emphasised that although skin cancer receives a great deal of media attention in the press, it was a mistake both scientifically and politically to concentrate on it. Instead, she argued that from a scientific perspective, the three most serious issues were the impact of increased ultraviolet radiation on the human immune system, on the world's food supplies, and on aquatic ecosystems. She also argued that since skin cancer mostly affects Caucasians who spend a lot of time in the sun, international cooperation would depend on framing the issue differently [Kripke 1989: 7].

The turning point in the interagency dispute came with Interior Secretary Donald Hodel's imprudent statement that the administration should consider a policy of "personal protection" instead of international regulation. He suggested that a public relations campaign should be launched to encourage the use of sunglasses and skin lotion, without violating the administration's philosophy of minimal government regulation [*Washington Post*, 29 May 1987]. The public outcry was swift and intense. A *New York Times* editorial lamented that Hodel's "meddling" threatened to undermine the international talks and " force the US from a widely admired position of leadership into humiliating retreat" [31 May 1987]. Environmentalists responded to Hodel's proposal by wearing hats and sunglasses at a press conference and calling for his resignation [*Los Angeles Times* and *Washington Post*, 30 May 1987]. David Doniger's statement that "fish don't wear sunglasses" was cited throughout the press accounts, indicating the folly inherent in defining the issue narrowly in terms of skin cancer.

Secretary of State George Shultz temporarily resolved the issue by instructing his negotiators to continue working for an international agreement until the issue could be resolved at the Cabinet level [*Washington Post*, 5 June 1987: A13]. The original 95% position was not revoked, primarily because "it had already been put out on the street". But the US delegation received instructions to press only for a 50% reduction in CFCs on halons [Doniger 1988: 90]. Those who opposed stringent controls received one important concession, a concession viewed by environmentalists as undercutting US support for an agreement. At the final meeting in Montreal, the US delegation was instructed to propose that the treaty could not take effect until countries representing 90% of all consumption had signed it. The purpose of this stipulation was to ensure that the US would not have to reduce CFCs faster than a non-signatory country. It was also intended to put public pressure on all countries to sign. Environmentalists complained that the concession effectively gave either Japan or the Soviet Union veto power over the treaty's entry into force [Doniger 1988: 90].

While the final US position was somewhat weaker, it was also stronger in one important respect. Because of the contentious nature of the American policy process, the US position was personally approved by President Reagan, whereas no other country's position was approved at the cabinet level. Lee Thomas, who led the US delegation in Montreal, claims that this gave him the strongest position of anyone going into the final negotiations. Despite the interagency squabbles, there was one point on which all sides, including US industry, agreed: a global problem required a global solution. No Americans wanted to repeat the experience of the late 1970's, when the US banned CFCs in aerosols while most of the world sat on the sidelines. Because further domestic regulation was likely, spurred on by both the NRDC lawsuit and the scientific predictions, there was a consensus that a treaty was necessary so that US industry would not be put at a disadvantage. Just before the second round of negotiations, two US senators sent such a message to the rest of world, introducing legislation that would have cut CFC use domestically by 95% and blocked all imports containing or manufactured with the chemicals [S. 570; S. 571]. The bills were intended to help the EPA and the State Department to negotiate a strong protocol; they gave Ambassador Benedick, leader of the US delegation prior to the Montreal meeting, an important bargaining tool.

Thus, the US bargaining position grew out of a complex set of scientific and political considerations. Predictably, since science was the predominant legitimating force, the negotiators emphasise the former in relating their stories. As with the Antarctic ozone hole, the mode of framing the science had important political implications. Those who defined the issue solely in terms of skin cancer became the objects of political embarrassment when they publicised their views, despite the fact that most of EPA's research on the human effects of ozone depletion dealt with skin cancer. Beyond the domestic consensus in favour of a global treaty, the nature of the US national interests was not initially obvious. Rather, it evolved through a process of internal debate that blended both science and politics. As that interest was clarified and formulated into a negotiating position, the US was able to use its economic leverage as a major imported to influence the international negotiations.

5.6 The Negotiations

As the first round of negotiations opened in December 1986, the two principal adversaries were the EC and what had been called the Toronto group prior to the Vienna Convention. The US was the largest and most outspoken of the latter, which included Canada, Norway, Sweden, Switzerland, and New Zealand. A third group, including Australia, was initially neutral but later moved closer to the US position [Benedick 1989: 48].

The EC's position was strongly influenced by industry; in fact, industry representatives sat on the delegations of most EC countries. Britain, France and Italy, endorsed a cap on production capacity, with Japan and the (then) USSR sympathising with this position. They argued that significant ozone depletion would not occur for decades, thereby allowing time for further study before cutting production. These countries distrusted the motives behind the US position, suspecting that such a drastic regulatory proposal coming out of the Reagan administration could only mean that US industry had secretly developed CFC substitutes. Within the EC, however, was a diversity of opinion. (The then) West Germany, the Netherlands, Denmark and Belgium favoured stricter controls, but only Germany was a major producer. Other EC countries were not active participants. The internal structure of the EC influenced both the pace of the talks and the content of its negotiating positions. The Commission itself cannot make binding decisions; only the Council of Ministers can. The Environmental Ministers, however, only met twice yearly, whereas the negotiations were proceeding at a much faster pace. With no good common denominator among the various positions of its member states, the EC position was vague; the sole area of consensus was on the need for further research. Complicating the matter was the fact that each country had a representative at the meetings, but there was an internal agreement that the EC would speak with a single voice. From the US perspective, the EC's requirement of unanimity made it "a difficult and inflexible negotiating partner" [Benedick 1989: 48].

At the December meeting in Geneva, there was overall agreement dating from the Leesburg workshop that some limits on CFCs were necessary, but the EC's proposed production capacity cap was very far from the virtual phaseout proposed by the US. The EC was willing to discuss a freeze, but would not move beyond that [UNEP/WG.151/L.4]. Both sides acknowledged the need to develop safe alternatives to CFCs, and both justified their positions in terms of economic "knowledge," arguing that their proposals would "exploit the law of supply and demand"' by raising the prices of CFCs and forcing producers to seek safe substitutes [*Wall Street Journal*, 2 December: 1986l. The US and its supporters argued that a freeze or a production cap would not accomplish this goal quickly enough and that, if drastic reductions turned out to be required in the future, the social cost would be much higher. Most importantly, the US argued that, because of the long atmospheric lifetimes of CFCs, a delay in reductions would allow unacceptable levels of chlorine to accumulate. Hence the debate between the US and the EC was really over the appropriate time frame to employ in formulating an international regulatory policy.

The US showed some flexibility in its position at the first negotiating session, but this was interpreted by congressional advocates of a strong protocol as a "backing off." During Senate hearings held in January 1987, Senator Chafee chastised Ambassadors John Negroponte, head of OES, and Richard Benedick, the lead negotiator, for vacillating at the first negotiating session. He stated that,

> [When] we got to Geneva, the Government of the United States had changed its position. It was no longer a near-term freeze, but it was a meaningful near-term first step to reduce significantly. Step two was no longer a long-term strategy and goals for coping with the problem. Frankly, I think that we have to push you folks and, if this fails, go it alone [Senate Hearings, 28 January 1987: 49].

Benedick shrewdly used the congressional criticism, along with the legislation introduced by Senators Chafee and Baucus which threatened to ban imports made with CFCs, to his advantage at the next meeting. When the delegates convened in Vienna that February, Benedick was very critical of "other nations which were more concerned with short-term economic gains instead of the well-being of future generations" [UNEP/WG.167/2: 6]. He adroitly depicted himself as a victim of domestic pressure, informing the delegates of the pending legislation designed "to protect our industry from imports from countries which continue to ignore the threat to the global environment" [Department of State 1987: 17].

Benedick's use of scientific knowledge to support his position is particularly interesting. In addition to mentioning the predictions of computer models, he claims that "both satellite and land-based measurements suggest that the process of ozone destruction may already be underway" [Department of State 1987, 17]. Since the group had decided to ignore the Antarctic ozone hole, the reference here must be to Donald Heath's satellite measurements and the data from certain Dobson stations, both of which were considered highly unreliable. Benedick's allusion to measured ozone loss also contradicts his later assertion that the Montreal Protocol was a *preventive* action, "based at the time not on measurable evidence of ozone depletion or increased radiation but rather on scientific hypotheses" [1989: 43]. In his address to the Vienna meeting, Benedick framed the issue in terms of chlorine-loading, citing Hoffman's calculation that an immediate 85% reduction in CFCs would be necessary to keep atmospheric concentrations stable. As discussed above, this mode of framing, which does not depend upon computer models, became more salient with the discovery of the Antarctic ozone hole and lent support to the US proposal to phase out CFCs. Benedick's statement illustrates the political character of knowledge: using partial truths and how the available information is framed can lend persuasive power to one's position.

The strength of the US position at the second session bore fruit as the EC reluctantly began to consider reductions of 20%. The EC's new flexibility, however, arose not merely from the persuasiveness of the US position, but from growing dissent within its own membership. West Germany in particular was moving in the direction of stringent controls. The German government, to a greater extent than most other European governments, was supportive of and receptive to the atmospheric science. The 1986 WMO/NASA assessment was co-sponsored by a German agency, and several major scientific meetings had been held there. More important, though, was the growing political influence of the West German environmental movement. After the Green Party's impressive electoral showing, the government succumbed to pressure to back an aerosol ban and a long-term phaseout [Doniger 1988: 90]. By the second negotiating session, Germany was planning unilateral cuts of 50% and urging other EC members to support deep reductions [Benedick 1989: 49]. France and the U.K., however, remained steadfast in their opposition to deep cuts.

At the February meeting, some countries which until then had shown little concern began to move toward the US position. The Australian representative spoke of the high domestic incidence of skin cancer and the consequent interest in protecting the ozone layer. The delegate from Argentina noted that the ozone losses over Antarctica extended near his country's southern borders, and registered his support for control measures. Yet, along with representatives from Thailand and Egypt, the

Argentine delegate insisted upon a protocol based on the principle that "the polluter must pay" [UNEP/WG. 167/2].

There was unanimous agreement among the participants at the second session that CFCs 11 and 12 should be subject to regulation, but little agreement beyond that. Many permutations were suggested from a list that included five CFCs, carbon tetrachloride, methyl chloroform, and Halons 1211 and 1301. The Japanese delegate maintained that CFC-113 should not be regulated because no viable substitutes were available and it would contribute only 10% to ozone depletion. While the Japanese information was correct, others pointed out that CFC-113 production was growing faster than any of the other compounds.

Another contentious issue was whether production or consumption should be controlled. The EC sought to control production on the grounds of simplicity, since there were innumerable points of consumption and only a handful of producers. The US, Canada, and others countered that production controls would mean that the EC, which exported about one third of its CFC output and was virtually the only exporter, would have a near monopoly. This arrangement would be especially unfair to developing countries, who would be prohibited from increasing their own production while the EC could reduce exports to compensate for growth in domestic consumption. This would motivate developing countries to circumvent the treaty and build their own CFC plants. The US devised a compromise based on "adjusted production" which was intended to satisfy the EC's desire for simplicity and yet give all countries an incentive to enter into the agreement. Controls would be placed on production plus imports minus exports to other treaty signatories [UNEP/WG.167/2: 11]. This would allow producing countries to increase exports to protocol parties without having to cut domestic consumption; moreover, importing countries would not be totally dependent upon one source of CFCs [Benedick 1989: 49].

Throughout the first two rounds of negotiations, opponents of stringent controls emphasised the scientific uncertainties. They tended to focus on the points of disagreement among the predictions of the atmospheric models rather than on the areas of agreement. This became a source of increasing frustration for those from the EPA, State Department, UNEP, and the scientific community who saw the need for a strong protocol. Consequently, members of the US delegation sought to sway other governments through informal conversations with officials, bilateral meetings, scientific exchanges, and satellite conferences. EPA staff worked extensively with their counterparts in other environmental bureaucracies, and were quite influential in several European ministries. Watson and Benedick conferred with journalists, officials and scientists from dozens of countries via the US Information Agency's "Worldnet" satellite hook-up. A team of scientists and diplomats from the US travelled throughout the world, including Japan, the Soviet Union, India, Egypt, and to most European countries. The most important of these meetings were in Japan, the Soviet Union and Egypt, where there was "a real exchange of ideas." More to the point, there was a genuine attempt to understand each other's interests and modes of communication. The Japanese grew more receptive to including CFC-113 in the protocol, largely because they became convinced that skin cancer was not the only important issue. The Soviets concurred on the US assessment of the science and agreed to joint scientific endeavours with the US. The head of the Egyptian Environment Directorate met personally with the delegation and used his influence to garner support from other Arab and developing countries. Many of these meetings dealt only with scientific issues, with the goal of establishing a scientific consensus. Yet, in typical fashion, science and politics were intertwined, even if one was not reducible to the other.

Despite the increasing degree of consensus, both scientifically and politically, the British and the French continued to emphasise the uncertainties and the discrepancies among the various computer models. Mostafa Tolba, Executive Director of UNEP, was convinced that the differences were largely illusory. In early April, he convened a meeting of five modelling teams from different countries in Wurzburg, West Germany, asking them to use exactly the same data. As Tolba had anticipated, the models produced roughly similar results. The scientists unanimously agreed upon the need to control those compounds with the greatest ozone depletion potential: CFCs 11, 12, 113, 114, 115, and Halons 1301 and 1211 [UNEP/WG. 167/INF: 1]. This represented a turning point for UNEP in the negotiations. For the first time "it was no longer possible to oppose action to regulate CFC releases on the grounds of scientific dissent" [UNEP/WG.172/2: 2]. When the negotiations reconvened in Geneva on April 27, the US position had been both weakened and strengthened: weakened due to domestic interagency dissension and strengthened by the flurry of bilateral interactions, as well as by the Wurzburg meeting. At this point, UNEP took on a greater leadership role. Dr. Tolba, a respected scientist, personally addressed the delegates, recounting the Wurzburg findings and tabling an ambitious proposal for a freeze on CFC and halon production in 1990 at 1986 levels, followed by a 20% cutback in production even until they were eliminated by the year 2000 [UNEP/WG.172/2: 2]. Tolba met individually with heads of key delegations to press his case [Benedick 1989: 49].

The EC announced that it would agree to a freeze, followed by a 20% reduction in CFC production and imports, but maintained that "it would be pointless to go further if the possible benefits of doing so were negated by the refusal of significant CFC producers and consumers to sign the protocol." Japan, which was not invited to the scientific meeting at Wurzburg, continued to emphasise the scientific uncertainties and to oppose controls on CFC-113, although its opposition had softened somewhat. The US reiterated its original negotiating position, but was decidedly less vocal than at previous meetings [UNEP/WG.172/2].

By the third session, it was apparent that the primary issues requiring resolution were not scientific, but political. Once the discrepancies in the models could not be used to justify inaction, and once the need for significant reductions was generally recognised, the process came to be dominated by the usual political dynamics of compromise and concession until the treaty was finally signed in September.

The EC's shift may have been partly due to the change in the Community presidency. US negotiators observed that progress in the negotiations was made only after a Belgian replaced a Briton as EC President in January 1987. Britain, however, was in the 'troika' (composed of past, present, and future presidents), which held closed meetings with key delegation heads throughout the negotiations. When the presidency rotated again in July, the troika consisted of Belgium, Denmark, and Germany, all of which supported strong control measures [Benedick 1989: 48]. This fortuitous political event may have helped to erode the EC's opposition to significant reductions. Other obstacles were surmounted through a process of political bargaining and compromise. Japanese objections to including CFC-113 were finally answered at the Montreal meeting through a concession permitting countries to shift consumption between the various CFCs, so long as their total ozone depletion potential was not exceeded [*Montreal Protocol*]. The conclusions of the Wurzburg meeting made it relatively easy for UNEP to persuade countries to include the halons in the list of controlled substances. At the last minute, it was learned that the USSR was using a halon that no other country produced. The Soviets agreed to include it on the list of controlled chemicals; in exchange, they were allowed to include two CFC plants already under construction in the 1986 base line.

The US proposal that an enforceable treaty could only come about when a formal protocol had gained ratification by countries representing 90% of production -- a concession to those who wanted to protect US industry from unfair competition -- was widely criticised [Doniger 1988: 90]. Agreement was reached that the treaty would be in force when countries representing two thirds of 1986 consumption had ratified.

One last stumbling block emerged in Montreal. The problem involved how the EC was to be defined, and how production was to be rationalised among its member countries. Tolba convened a meeting of the twelve major producer countries to resolve the issue, a meeting which did not adjourn until after midnight. Lee Thomas and Laurens Brinkhorst, the EC representative, met until 3:00 am. In the end, it was agreed that the EC would be treated as a single entity, thereby allowing industry to redistribute production among plants in different countries to guarantee maximum efficiency. The Treaty, signed on 16 September 1987, by 24 of the 62 nations at Montreal, was regarded by UNEP as unprecedented. Tolba called it "the first truly global treaty that offers protection to every single human being on this planet," and "unique because it seeks to anticipate and manage a world problem before it becomes an irreversible crisis" [UNEP Press Statement, 22 September 1987].

5.7 The Immediate Aftermath

Even as the Montreal Protocol was being signed, scientists were uncovering new data that would dispel many of the uncertainties and revise their conception of the ozone problem. Scientists on the second Antarctic expedition concluded from their detailed measurements of the atmosphere that CFCs were the culprit: announcing their preliminary results just two weeks after the protocol was signed [*Washington Post*, 1 October 1987; *Nature*, 8 October 1987]. Most scientists involved in the issue believed that if CFCs were shown to be linked to the hole, then they should be banned. Immediately, calls were heard for strengthening the Montreal Protocol [*Nature*, 8 October 1987; *Washington Post*, 28 October 1987].

Several months later, in March 1988, NASA's Ozone Trends Panel, which had been established to critically reanalyse nearly all satellite and ground-based measurements of ozone, released its report. The panel was primarily a response to controversial satellite data which had shown annual global ozone losses of 1% since 1979. These findings provoked Du Pont to reassess its CFC policy. Less than a week after the report's release, Du Pont announced that it would halt all production of fully-halogenated CFCs as soon as possible [Glas 1989: 150].

Within a year, the US and the EC decided to surpass the Montreal protocol's requirements and phase out CFCs by 2000. Most remarkable was the reversal in the British position. Prime Minister Margaret Thatcher, who was deeply distrustful of the NASA Ozone Trends Panel report, requested that the British Stratospheric Ozone Research Group (SORG) assess the issue. SORG published a summary of its findings in June 1988, and the full report in October, corroborating the NASA report [*London Observer*, 2 October 1988]. At that point, Thatcher became personally committed to protecting the ozone layer, convening a major international conference in London the following year to promote a global reduction of CFCs by 85% [*Washington Post*, 8 March 1989] [**18**].

Once most of the scientific uncertainties about the CFC-ozone link had been resolved, the debates was longer formulated in terms of science. The emerging scientific consensus was mirrored in the new symmetry between the positions of the US and the EC. The major source of contention soon became the issue of assisting developing nations to obtain CFC substitutes, chemicals which would cost three to five times as much as the ozone-depleting CFCs. The developing countries, however, did not frame the issue in scientific terms, but rather in terms of economic equity and national sovereignty.

After several international conferences and extensive bilateral negotiations, these issues were finally resolved at a London meeting in June 1990, convened under the Montreal Protocol's provisions for periodic reassessment.

5.8 Conclusion

International decision making in the face of scientific uncertainty involves a rich and complex set of interactions among facts and values, knowledge and interests. The existence of uncertainty can be a source of scientific legitimisation for a wide array of contending interests, thereby furnishing an obstacle to political consensus. On the other hand, scientific consensus can facilitate international cooperation, although it does not make it inevitable. In the case of the Montreal Protocol, industry and the EC emphasised the uncertainties and framed what was known in terms most favourable to its own interest in averting strict regulations. The US, stressing both what was known and the need to mitigate the risks, also framed the science in terms of its own interests. In both cases, 'interests' were not independent variables, but were themselves a function of accepted knowledge.

Scientists were important actors in the process, but to say that the issue was science-driven is not to say that the scientists themselves were the driving force. First, they rarely made policy recommendations. Second, and more importantly, once the science became enmeshed in the policy debates, other contextual factors determined how it would influence policy. Foremost among these was the Antarctic ozone hole which, although it was only in the background of the policy debates, provided a strong case for those who would err on the side of caution. Another major factor was the existence of an international consensus, set out in the 1986 WMO/NASA assessment and fortified by the Wurzburg meeting, that uncontrolled CFC emissions would lead to devastating consequences. The position of the US delegation, most articulately expressed by EPA officials, was that a long-term perspective was required and that the high stakes mandated strict controls. That position was reinforced both by the predictions of the atmospheric models and by the discovery of the Antarctic ozone hole. In general, uncertainty increases as the causal chain of events moves further into the future. Not only does this empower experts to advise policy makers but, when combined with .he perception of great risk, it can require a different kind of decision making among policy makers. John Ruggie [1986: 201] describes it as a

> 'bias shift' . . . away from a conventional problem-solving mode, wherein doing nothing would be favoured on burden-of-proof grounds, toward a risk-averting mode, wherein prudent contingency measures would be undertaken to avoid risks we would rather not face.

The kind of power those who successfully translated their information into viable policy proposals was not a function of control and domination, but rather entailed *persuading* delegates to re-evaluate their own conceptions of their nations' interests. Their ability to persuade was a function both of external contextual factors, and their own skilful employment of knowledge in support of their position. *Perceptions* were central -- most importantly, how great the risks were perceived to be and how authoritative the scientists' knowledge was perceived to be. These perceptions were heavily influenced by the persuasive power of specific experts and knowledge brokers, who were able to expand the time frames of most of the delegates and heighten the sense of urgency.

CHAPTER SIX

SCIENCE AND INTERNATIONAL POLITICS: ACID RAIN AND GLOBAL CLIMATE CHANGE

Despite important differences among the various international environmental problems, many of the dynamics I have traced above with respect to the Montreal Protocol are reproduced in other contexts. Scientific knowledge, rather than automatically inspiring political consensus, is often used as a source of legitimisation by parties on behalf of their own perceived interests, yet it can also stimulate revision of perceived interests. Certain fundamental questions arise whenever environmental policy must be formulated under conditions of uncertainty: how much must be known before regulatory action is taken? What counts as good evidence? Whose interpretation of the science is considered authoritative? What are the political conditions under which certain facts are highlighted and others down played? This chapter takes these questions as the backdrop against which to examine efforts to address two other threats to the stability of the earth's atmosphere: acid rain and global warming. These also happen to be the two most serious global environmental problems associated with the use of fossil fuels. **[1]**

The problems of ozone depletion, acid rain and climate change were each discovered by scientists, and international negotiations in each case have involved close collaboration between scientists and policymakers. None of these problems is immediately detectable by the senses; a great deal of inference is necessary not only for making predictions about the future, but even for understanding with any confidence current trends. While scientists played a central role in defining each problem, the persistence of substantial uncertainties has opened the door to a range of interpretations. Moreover, all three issues are characterised by potentially irreversible environmental damage caused by chemical accumulation, thus requiring a kind of long-range planning that is uncommon for most policy making. Thus, in many ways the scientific dimensions of the three sets of problems are similar. Yet the real differences among the issues provide an opportunity to see how scientific uncertainty is handled under a range of conditions. While all three problems are serious, clearly the climate issue dwarfs the other two, both in terms of its likely ecological consequences and the social impact of proposed responses. In terms of control measures, the ozone issue is most amenable to resolution because both the cost of controls and the number of polluting sources are relatively low compared to acid rain and global warming. However, the expected distribution of costs and benefits is fairly even for the Montreal Protocol; industrialised countries in the Northern Hemisphere will make the greatest adjustments, and their citizens will also benefit the most. For acid rain and global warming, the distribution of the costs and benefits of control is quite skewed, with some countries emerging as clear victims of the industrial activities of others. **[2]** While the primary actors involved in deliberations on ozone and acid rain have been from Europe and North America, some key actors in the global warming debate are industrialising countries. Finally, a dramatic and visible crisis helped spur action on both ozone and acid rain, but no such crisis has yet emerged regarding climate change. An indicative summary of these similarities and differences is provided in **Table 6.1**. The table is neither authoritative nor drawn from any single source.

Scientific uncertainty is not only one dimension of these environmental problems but casts a shadow of doubt upon all the other categories. Under conditions of uncertainty, the gravity of the problem is a matter of despute; actors cannot easily infer what they stand to gain or lose from regulation; proposed control measures may fail to alleviate the problem; and a dramatic event may or may not be causally linked to the issue at hand. What constitutes evidence, therefore, becomes a relative matter, a tendency which is exacerbated as contending national interests are overlaid onto controversies about assessment and proof. International environmental cooperation then becomes an exercise in the "management of ignorance" [Van Lier 1980: 45]. The need for additional research can be used to justify inaction, yet the potential for major irreversible damages means that action may be too late by the time unassailable scientific proof is at hand.

Table 6.1: Comparison of International Environmental Problems

	Ozone	Acid Rain	Climate
Scientific Uncertainty	High	High	High
Predicted Damages	High	Moderate	Very High
Distribution of Costs and Benefits	Fairly Even	Uneven	Uneven
Cost of Controls	Moderate	High	Very High
Sources of Pollution	Few	Many	Very Many
Major Actors	North	North	North/South
Visible Crisis	Yes	Yes	Yes
Human Health Issue	Yes	No	Yes

The issues of acid rain and climate change are important for another reason. In contrast to the ozone problem, the U.S. has not assumed a primary leadership role in international efforts to implement control measures for either of these issues. The successful negotiation of the 1979 Convention on the Long-range Transport of Air Pollution, along with the subsequent control measures adopted, shows that international environmental cooperation is possible without U.S. leadership. On the other hand, the lack of a treaty on global warming -- despite strong support from most OECD countries suggests that strong opposition from the U.S. can be sufficient to block international action.

6.2 Acid Rain in Europe and North America

The chemical precursors of acid rain derive from the burning of fossil fuels. Acid rain results from the transformation within clouds of sulphur dioxide (SO_2) and nitrogen oxides (NO_x) into the water soluble sulphuric and nitric acids. The basic reactions can be summarised as follows:

(1) $SO_2 + 1/2\ O_2 >> 2H^+ + SO_4-$

(2) $NO + NO_2 + O_2 >> 2H^+ + 2NO_3-$

Both scientific research and policy discussions have focused on the sulphur compounds. Sulphur dioxide, with twice as many hydrogen ions as nitric acid, can remain in the atmosphere for days and travel thousands of miles before being deposited, whereas oxides of nitrogen usually fall to the ground within hours [Sitwell 1984: 42]. Coal, which is high in sulphur, is thought to be a major culprit.

Acidity is measured by the number of hydrogen ions (H^+) in suspension. A pH of 7 is neutral; anything lower is acidic and anything higher is alkaline. Since the pH scale is logarithmic, a pH of 6 is ten times more acidic than a pH of 7 [Mohnen 1988: 30-32]. Rainwater is naturally acidic due to the reaction of carbon dioxide with water to form carbonic acid. Traditionally a pH of 5.6 has been taken as the natural acidity of precipitation, although a wide range of values have been found under what are believed to be natural conditions [Beilke and Elshout 1983: 7]. Uncertainty about what constitutes natural acidity has allowed industry to claim that rain with a pH of 4.5, which is common in the Northeast U.S. and Central Europe, is normal [Katzenstein 1981: 6-12]. Nonetheless, there is a broad scientific consensus that the acidic rainfall in these regions, sometimes with a pH as low as 3.0, has industrial origins [Beilke and Elshout 1983: 7; Sitwell 1984: 41].

The acid rain problem emerges from the following chain of causal links, each of which entails some uncertainties. Local emissions are transported by winds and weather systems, reacting with water vapour to form sulphuric and nitric acids. Emissions data, the quantities of acids produced, and the distances travelled are all matters of uncertainty. The acids fall to the ground either as droplets of moisture from fog or precipitation or as dry particles. Much more is known about "wet deposition" than "dry deposition," although the latter is thought to account for about half of the total acidification in Europe in North America [Beilke and Elshout 1983: 11-13]. The primary factors controlling the degree to which particular aquatic ecosystems are acidified are the buffering capacities of the soil and bedrock in the surrounding watershed, capacities which cannot be measured in any straightforward way [Glass *et al.* 1982: 164]. While there is general agreement that Scandinavia's fish populations have been seriously damaged, the evidence is not as strong elsewhere [Schmandt and Roderick 1985: 52] and the economic consequences of this decline are obscure [Crocker 1984: 66].

The link between acid rain and forest decline is even less certain, particularly due to the fertilising effects of nitric acids [Katzenstein 1981: 27]. The causes of forest dieback, observed in both Europe and North America, may be either natural or anthropogenic, ranging from drought to ozone pollution to nutrient leaching by heavy metals [Mohnen 1988: 34]. Since there are no pollution-free trees in the affected areas, this "experiment" has no good controls [MacKenzie and El-Ashry 1988: 15]. The lack of long-term research into each of these causal links, from emissions to effects, has provided political actors with the scientific tools to legitimise a range of positions.

Public awareness of acid rain developed only within the last two decades, causing some to trivialise the issue "an overnight sensation" [Bender 1986: 243]. Yet the term was first coined in 1872 to account for many of the same observations that have been made more recently [Smith 1872]. Thus, scientists who are deeply involved in the issue can legitimately ask, "Why did it take so long for acid precipitation to be recognised as an important environmental problem?" [Cowling 1982: 110]. Despite the fact that the chemical mechanisms themselves are relatively straightforward, particularly compared with the reactions between CFCs and stratospheric ozone, the problem received almost no attention until the late 1960's.

Several factors explain the delay. First, the problem simply was not so serious in the past. The postwar industrial boom created unprecedented quantities of chemical emissions, and some methods used to reduce ambient pollution levels actually exacerbated the acid rain problem. In the 1950's and 1960's, major steps were taken to reduce the alkaline fly ash in the air, thereby removing a primary neutralising agent from emissions [Hubert Vogelmann, quoted in Sitwell 1984: 46]. Industry and utilities also built tall smokestacks in order to decrease local pollution levels, thus shifting the problem from the immediate vicinity to a much larger geographic region [MacKenzie and El-Ashry 1988: 54]. Second, the science of acid rain and its effects is complex and highly interdisciplinary, involving chemists, meteorologists, botanists, foresters, limnologists, and statisticians. The path from one discipline to another involves devising new networks of communication among scientists, and also creates additional uncertainties as the causal links are expanded. Third, reliable quantification of existing and potential damage is difficult because historical records of regional precipitation chemistry are largely nonexistent, particularly outside Europe [Schmandt and Roderick 1985: 26].

In 1968 Svante Oden, a Swedish soil scientist, published his research about the inadvertent "chemical war" being waged among European nations [Oden 1968; Cowling 1982: 114]. Drawing from many scientific disciplines, Oden predicted that acid precipitation would kill fish populations, leach toxic metals into soils and aquatic ecosystems, reduce forest productivity, and damage urban materials. He foresaw that Scandinavia's sensitive lakes would be particularly at risk.

Oden's hypotheses sparked scientific and political debates in Europe which spread by the late 1970's to North America. Although Sweden would later be the world's leading advocate of acid rain controls, the Swedish government was initially unimpressed by Oden's research and discontinued his funding. The media, however, found his work more convincing, so that by 1971 the government responded to public pressure by commissioning a case study on the environmental effects of atmospheric sulphur. This study was Sweden's primary contribution to the historic U.N. Conference on the Human Environment, held in Stockholm the following year. Of all the conference participants, only Norway was prepared to accept the results of the Swedish report [Bolin *et al.* 1972]. Hoping to make progress toward international cooperation, Sweden and Norway brought their case to the Organisation for Economic Cooperation and Development (OECD), which initiated a program to monitor transboundary air pollution over Europe. The OECD study, published in 1977, confirmed that large quantities of sulphur emissions were being transported to downwind countries. In five of the eleven countries studied, most of the sulphur appeared to be imported. Britain emerged as the worst offender, with much of its sulphur being deposited in Scandinavia. West Germany was also shown to be a major source of acidification in Scandinavia [OECD 1977].

The uncertainties surrounding both emissions data and atmospheric transport models, however, gave these findings a 50% margin of error, thereby allowing the British and German governments to minimise the study's conclusions [Regens and Rycroft 1988: 41]. Britain finally conceded that its emissions may have been transported across the North Sea, but remained sceptical about the negative consequences. Other net exporters, including France, Italy and Belgium, showed no concern for Scandinavia's lakes. Austria and Switzerland came out as net importers of acid rain, yet these countries refused to take the problem seriously. Only Finland, Denmark, and the Netherlands were swayed by the OECD study to support Sweden and Norway, but this appears to have been more for the sake of Nordic unity than out of concern for their own ecologies [Whetstone 1987: 169].

The impetus for international cooperation came not from any scientific findings, but from a speech by Soviet President Brezhnev at the 1975 Helsinki meeting of the Conference on Security and Cooperation in Europe (CSCE). At that meeting, Brezhnev challenged the parties to cooperate on three problems facing the European continent: energy, transportation and pollution. Sweden and Norway, sensing an opening, decided to push for an agreement on acid rain through the United Nations Economic Commission for Europe (ECE), which could provide an arena for East-West cooperation. Because Canada and the U.S. are among the ECE's 34 member states, these two countries were also brought into the debate. One of the first actions of the ECE Secretariat was to promote international coordination of research through the newly established Cooperative Programme for Monitoring and Evaluating the Long-Range Transmission of Air Pollutants in Europe [Cowling 1982: 116].

The negotiations leading up to the Convention on Long-Range Transboundary Air Pollution (CLRTAP) in 1979 show some striking parallels to the Montreal Protocol process. Sweden and Norway, in what was called the Nordic Proposal, argued for a freeze and eventual reductions of sulphur dioxide emissions. As in the ozone talks, the U.K. and the FRG were dominant within the E.C., so that this single most important actor opposed all control measures. Eventually, Britain relaxed its outright opposition to any agreement under the assumption that its plans to expand nuclear power generation would lead to reduced sulphur emissions. West Germany insisted that the phrase "economically feasible" be appended to the Nordic provision that the "best available technology" be employed to reduce sulphur emissions. In contrast to the Montreal Protocol process, however, the U.S. did not assume a leadership position. The U.S. proposed an ambient air quality standard for sulphur dioxide, much like the one it had legislated domestically, but the E.C. opposed it because scientists disagreed on the relationship between local and long-range pollution [Whetstone 1987: 179].

As in the debates that preceded the Montreal Protocol, all parties were able to claim the science as their own, using it to justify their own perceived interests. The major polluting countries used the scientific uncertainties to argue against reductions, thereby delaying action by calling for further research. Because of the lack of good historical data, as well as the discrepancies among methods of measurement, these countries could claim that there might be no acidification. This argument was most credible in North America, where there was little consistent data prior to 1970 [Cowling 1982: 117]. Britain also stressed that scientists disagreed about whether the relationship between emissions and acid rain was linear, a line of argument that was later adopted by opponents of controls in the U.S. [Harrington 1989: 8-11]. If it turned out to be non-linear, then the benefits of reducing sulphur emissions would be dubious, despite the high costs. Perhaps the strongest scientific justification for inaction was that damages were not traceable to their specific origins. The lack of data about source-receptor relationships allowed Britain, and later the U.S., to argue that more research was needed before emissions reductions could be optimally imposed [Regens and Rycroft 1988: 52].

Despite the uncertainties, Sweden and Norway had the weight of circumstantial evidence on their side. Scientists agreed that industrial emissions were increasing the levels of atmospheric nitrates and sulphates and that these were likely to acidify precipitation, even if the observational data were lacking. They also agreed that Scandinavia's surface waters and fish populations were being harmed, even if they could not quantify the damage or predict levels of future damage [Schmandt and Roderick 1985: 45-52]. The Convention that resulted from these debates was more significant for its symbolic value than for any tangible contribution it made to reducing air pollution. First, it was the first major environmental agreement that brought together nations from the East and West blocs. Second, like the Vienna Convention for the Protection of the Ozone Layer, the acid rain convention recognised the existence of an environmental problem and established international mechanisms for scientific monitoring and joint research. Third, like the Vienna Convention, the ECE Convention spelled out the obligation, at least in principle, of countries to refrain from exporting their pollution, thus paving the way for explicit and more stringent control measures in the future.

Sweden and Norway continued to push for controls, but it was not until the dramatic discovery of extensive forest damage in the FRG that progress was made. Bernd Ulrich's initial research [1979] on the devastation of West Germany's forests was virtually ignored until it was publicised in *Der Spiegel* two years later [Whetstone 1987:189]. Like the initial response of the Swedish government to new information, the official German response was subdued until media attention and public pressure forced it to centre stage. In 1981, the government convened a working group of scientists, which published its report in November 1982. That study concluded that eight percent of West Germany's forests were afflicted with the "crown dieback" syndrome originally observed by Ulrich and his colleagues. The 1982 report attributed the dieback to the cumulative effects of air pollution, drought, frost, and poor forestry practices, with sulphur dioxide and its products emerging as the primary culprit [Roberts 1983: 302]. The 1983 government survey found that 34% of the nation's trees had been damaged by air pollution -- four times the 1982 estimate. By the following year, the damage was put at over 50% [FRG Ministry of Food, Agriculture and Forestry 1983; 1984]. Some scientists feared that the damage was irreversible and that the German forests could not be saved [Bradley 1983]. The term "Waldsterben" (literally, forest death) became a household word among West German citizens, for whom the forests were not only an important economic resource but a cherished national heritage [Postel 1984: 7].

Both domestically and internationally, FRG policies shifted quickly and dramatically. New federal regulations were applied to older power plants, the worst polluters, with a goal of reducing their sulphur dioxide emissions by 50 percent before 1993 [Whetstone 1987: 190]. These reductions were intended to bring about a 30 percent cut in the FRG's aggregate emissions. Internationally, Germany became a strong and vocal advocate of controls in the EEC and the OECD [Roberts 1983: 305]. This shift was most evident at the 1982 Stockholm Conference of the Human Environment, where the FRG abandoned its earlier position and joined with the Nordic countries and Canada in promoting a control annex to the ECE Convention. Although no specific controls were adopted at Stockholm, sufficient support was generated to obtain the final ratifications needed to bring the 1979 Convention into force. In terms of developing an international scientific consensus on transboundary sulphur pollution, the Stockholm Conference was "a model of success" [Whetstone 1987: 185]. Both the scientific information and the non-binding Conference statement, which was adopted unanimously, supported the Scandinavian perception that the problem was a serious one requiring specific control policies [Swedish Ministry of Agriculture 1982].

By June 1983, the Convention's Executive Body had endorsed as a draft decision a 30 percent decrease in sulphur dioxide emissions within ten years [Regens and Rycroft 1988: 150]. Abstaining from the emerging consensus were the United States, the United Kingdom, Belgium and Poland. In March 1984, environmental ministers from Canada, West Germany, and eight other Western European nations met in Ottawa and agreed to reduce their sulphur dioxide emissions by at least 30 percent from 1980 levels by 1993. The sulphur dioxide protocol was formalised in 1985 and entered into force in 1987 [U.S. Department of State/EPA 1990: 38]. Members of what came to be known as the "30 Percent Club" intended to pressure other countries, especially the major polluters like Britain and the U.S. into making comparable reductions [Regens and Rycroft: 151].

West Germany also assumed a central role in coordinating international scientific conferences on acid rain, hosting an E.C. workshop in September 1982 where Ulrich's work on the ecological effects of acid deposition was discussed. In June 1984, the FRG co-sponsored an ECE conference on aquatic and forest damages from air pollution. The tacit purpose of that conference, held in Munich, was to build support for the "30 Percent Club" [Regens and Rycroft: 151]. As part of the ripple effect across the continent caused by the German forest studies, Switzerland, Austria, France, the Netherlands, Czechoslovakia, Poland and the GDR all commissioned national studies, documenting serious damage in their own forests. The dramatic dieback of forests in Central Europe received a great deal of attention at the Munich conference so that, later that year, most Central European countries agreed to decrease at least their transboundary flows, if not their emissions, of sulphur dioxide by 30 percent [Whetstone 1987: 191]. Although the decline of the forests was the primary impetus for European acid rain controls, it is important to note that there was far less certainty around that issue than on the problem of damaged aquatic ecosystems. Scientists agreed unanimously that the damage to Scandinavia's lakes was caused by acid rain but, while they agreed that Central Europe's forests were dying, they did not agree that sulphur emissions were the primary cause [Beilke 1983: 4]. Rather, scientists pointed to several causes, acting either alone or in combination: ozone pollution, nutrient leaching by toxic metals, drought, and frost [Mohnen 1988: 33-34; Ember 1988: 20]. While the link between forest damage and sulphur compounds was unclear, many scientists believed that only air pollution, which includes more than just sulphur dioxide, could account for the number of species affected, the rapid onset of symptoms, and the large geographical areas afflicted [Mackenzie and El-Ashry 1988: 15; Tomlinson 1983]. Yet the *context* of the policy discussions had already been established by the Nordic countries, so that proposals for the alleviation of forest damage revolved around the perceived need to reduce sulphur dioxide emissions. If ozone were the primary culprit, as some scientists believed, then universal membership in the "30 Percent Club" might do little to help the forest.

West Germany's turnaround, which precipitated substantial international support for the "30 Percent Club," left Britain and the U.S. politically isolated in their opposition to a control annex to the 1979 ECE Convention. In 1986, the U.K. made a surprise move at the annual meeting of the EEC environment ministers, endorsing a 30 percent decrease in sulphur dioxide emissions by 1993 and calling for a total decrease of 45 percent by 2005 [Dickson 1986: 315]. Britain's shift represented a real sacrifice; with 67 percent of its electricity generated from coal-fired plants, the U.K. was far more coal-dependent than other Western European countries [Rosecranz 1980: 15]. Because there were no major new scientific findings and no change of government during the two years preceding the shift, the British reversal appears to have been the result of international political pressure. According to some observers, the shift was engendered by a combination of "old-fashioned morality and shame" [*The Economist*, June 16, 1990]. The British decision left the U.S. as the only major Western industrial nation opposing acid rain controls.

American intransigence can be explained by a combination of factors. First, to the extent that the problem was an international one, the U.S. was a net pollution exporter with respect to Canada. As long as the U.S. perceived regulation to be more costly than inaction, it was unlikely to back specific controls. Second, on the domestic front, the primary obstacle involved deciding who should bear the burden of the costs: the polluting Midwestern states or the recipient states. Third, the Europeans were proposing control levels similar to U.S. standards already implemented under the 1977 Clean Air Act, so that Americans could argue that further reductions were unnecessary. U.S. sulphur dioxide emissions had declined by 29 percent between the peak year of 1973 and 1983 [U.S. Department of State/EPA 1990: 38]. Moreover, existing U.S. pollution laws were intended to control primary pollutants considered harmful to human health, not secondary chemicals that might damage ecosystems. Since acid rain was not framed as a human health problem, there was no existing legal framework for legislating controls [Schmandt and Roderick 1985: 120]. [3] Fourth, the U.S. lagged behind much of Europe both in terms of its theoretical knowledge about acid deposition and its historical record of observations [Cowling 1982: 113]. The greater range of uncertainty in the U.S. offered more leverage to those who preferred to delay action. Fifth, acid rain emerged as a policy problem during the Reagan administration, which generally took an anti-regulatory stance. These factors combined to constitute a powerful obstacle to U.S. action.

Despite a warning in Sweden's 1972 case study that acid rain could pose a threat to ecosystems in the Northeast of Canada and the U.S., in addition to some early research confirming that warning [Beamish and Harvey 1972], the problem received scant attention until nearly 1980. Significant public awareness first emerged in Canada. In 1977, Canadian Environment Minister Romeo LaBlanc labelled acid rain an "environmental time bomb," triggering public interest in the problem [Howard and Perley 1980, quoted in Whetstone 1987: 171]. The Canadian Coalition on Acid Rain (CCAR), a grassroots coalition formed in 1980 by some fifty organisations, was instrumental in educating the Canadian public and even lobbied the U.S. Congress [Schmandt and Roderick 1985: 143]. The U.S. initiated intergovernmental discussions with Canada specifically out of concern over the impact of emissions from a Canadian power plant on a nearby recreation area in northern Minnesota. Canada was able to revise the agenda to include all major transboundary pollution problems. A joint research endeavour was initiated in 1978, and its first study was published the following year. As mentioned above, U.S. officials regarded air pollution wholly as a human health matter, and so were quite dubious about Canada's ecological concerns. The scientific consensus that emerged from the joint research effort, however, tended to support the Canadian perspective [Whetstone 1987: 171].

In August 1980 a Memorandum of Intent (MOI) was signed, committing both governments to negotiate a bilateral agreement and implement interim controls. Various joint working groups were established to summarise the science, but in the summer of 1981 the U.S. withdrew its support for them, declaring that each nation should perform the assessments unilaterally. Although scientists from the two countries agreed on the summary of the data, science and politics overlapped in their *interpretations* of the data. U.S. scientists felt that the Canadians overstepped the data, whereas the Canadians perceived the Americans' wariness as rooted in their own political values [Schmandt and Roderick 1985: 12-13]. The major area of dispute was the threshold level of pollution, above which unacceptable damage might occur. At first, both countries agreed to a threshold limit of 20 kilograms of wet sulphate per hectare per year. Canada maintained its support for this figure but the U.S., under the new Reagan administration, held that further research was needed before a threshold level could be set [Whetstone 1987: 174].

The dramatic shift in U.S. acid rain policy under the Reagan administration was part of a larger ideological shift, and is particularly interesting because it was justified on scientific grounds. **[4]** For the Carter administration, further research was needed but enough was known to develop a policy response. In contrast, according to the Reagan administration, regulations based on "bad science" had precipitated "economic tragedies" [Regens and Rycroft 1988: 119]. George Keyworth, Reagan's science adviser, declared that the administration's goal was to "reduce the excessive burden of federal regulations by improving the rational basis on which those regulations are made" [quoted in Dickson 1984: 264]. This position *assumes* that more rational regulation will be less costly. Because of the scientific uncertainties discussed above and the high cost of pollution abatement, **[5]** the acid rain issue was well-suited to the administration's anti-regulatory philosophy.

Domestic contingencies may also have hardened the Reagan administration's position on acid rain. In response to pressures to allocate resources to highly visible hazardous waste sites during the 1980'8, the EPA shifted funds from its air pollution control programs to the Superfund and Resource Conservation and Recovery Act (RC) efforts. Abatement Control and Compliance Funds for the stationary source air quality program, the bureaucratic niche for acid rain controls, shrank from US$40 million to $17 million annually, a cut of over 75 percent in real dollars [MacDonald 1989: 13].

The combination of the scientific uncertainties and the Reagan administration's position also provided fertile ground for widespread scepticism among the American public, an attitude that was uncommon in most other OECD countries. Some articles suggested that acid rain was a myth [*Science Digest* 1984]. Others produced far-fetched explanations, such as the "Smokey the Bear" hypothesis that emerged from the Hudson Institute, suggesting that fighting forest fires was responsible for acid rain [Brown *et al.*, 1984]. Everybody, regardless of their position, justified their beliefs primarily in scientific terms. As the primary author of the Hudson Institute study stated, "We need to start thinking of acid rain as a scientific rather than as a political issue," the implication being that those who disagree with him have considered the issue only in political, not scientific, terms [Brown 1984]. Nonetheless, mounting scientific evidence, not only in Canada and Europe but also in the U.S., challenged the Reagan administration's strategy. A 1981 study by the National Academy of Sciences (NAS) argued that, while no direct causal link between specific sources and receptors could be established, the circumstantial evidence linking power plant emissions to acid rain was overwhelming [NAS 1981]. In its 1982 annual report, the U.S. National Acid Precipitation Assessment Program (NAPAP) concluded that manmade pollutants were probably the major source of acid deposition in North America [NAPAP 1982]. A second NAS report was released in 1983 confirmed the "linearity hypothesis." That study undercut the argument that reductions in sulphur dioxide emissions would not reduce acid rain proportionately [NAS 1983].

Perhaps the most damning evidence came from the U.S. peer review committee appointed by the White House Office of Science and Technology Policy (OSTP) to evaluate the joint U.S.-Canadian research projects. That committee corroborated the conclusions of the joint working groups and the Royal Scientific Society of Canada, calling upon the U.S. to recognise acid rain as a real problem requiring significant reductions in sulphur emissions [Nierenberg *et al.*, 1984]. Ironically, the scientists selected by the White House were probably less critical than the original scientists on the joint review committee would have been, simply because they were less familiar with the documents and their flaws [Whetstone 1987: 175]. This incident demonstrates that science is not necessarily the handmaiden of politics, even when the scientists are hand picked by politicians. However, at least in the short run, the accumulation of scientific evidence against the Reagan administration's position did little to modify U.S. policy. William Ruckelshaus, EPA Administrator from March of 1983 until January of 1985, sought to impose limited controls on sulphur emissions, but his proposals were blocked by the Department Of Energy and the Office of Management and Budget [Whetstone 1987: 176]. Lee Thomas, his successor, opposed controls in the belief that the cost of reducing acid rain precursors outweighed the likely benefits.

While the Reagan administration continued to oppose controls, the mounting scientific evidence does seem to have inspired a change in rhetoric. In March 1986, President Reagan and Canadian Prime Minister Brian Mulroney formally accepted *The Joint Report of the Special Envoys* [1986], which recognised acid rain as a serious environmental problem and called for a major U.S. research program on clean coal technologies [Stanfield 1986]. While this represented a symbolic victory for Canada, it also lent support to U.S. efforts to limit responses to research rather than actual control measures. It was not until Congress passed the Clean Air Act of 1990 that the U.S. joined the rest of the Western industrialised world in enacting acid rain controls. Although there was substantial international pressure for the U.S. to join the "30 Percent Club," particularly from Canada, the recent legislation calling for sulphur emissions reductions of 40 percent was more clearly the result of domestic pressures. In fact, the U.S. never formally joined the "30 Percent Club." The acid rain issue gave rise to a powerful coalition of environmental, health care and outdoor recreation interests, most notably under the National Clean Air Coalition [Schmandt and Roderick 1985: 144]. This coalition, along with Canada and the victim states, constituted a powerful congressional lobby.

There are several observations that can be made about the employment of scientific knowledge in the acid rain issue. First, the persuasive influence of scientific studies was related to their institutional origins. Major breakthroughs, largely arising from the independent research of individual scientists, received little credence from policy makers until they were corroborated in government reports. Strong public pressure was needed to make governments take seriously the data of independent researchers documenting environmental damage. Studies sponsored by government agencies were influential within their countries of origin, but had little international impact [Whetstone 1987: 192]. Acid rain science did not stand on its own terms; rather, both its production and its reception entailed political dynamics.

Second, it would be a mistake to conclude from the experience of the Montreal Protocol that U.S. leadership, either scientific of political, is necessary for international agreement on environmental controls. The U.S. lagged behind many western countries, in terms of both knowledge and concern about acid rain. In the end, U.S. action to limit sulphur dioxide emissions was the result of the dynamics of domestic compromise, rather than international pressure. The British example, however, suggests that international pressure can sway some countries.

One might be tempted, though, to draw a third conclusion: that the absence of an international scientific assessment on acid rain (like the WMO/NASA ozone report) may have hindered international cooperation on this issue. While this inference rests on counterfactual evidence, it is a plausible one. A mere two years elapsed between the Vienna Convention and the Montreal Protocol, whereas there were eleven years between the signing of the ECE Convention on acid rain and the agreement by all key countries specific control measures. The absence of an international acid rain assessment gave some countries greater leverage to exploit scientific uncertainty to their advantage.

A fourth observation, however, echoes one lesson of the ozone negotiations: a dramatic and visible crisis can facilitate international cooperation, even if scientists disagree on its causes. The important factor seems to be public perception that the problem is a serious one. The "Waldsterben" in Central Europe, like the Antarctic ozone hole, demonstrated concretely that devastating and potentially irreversible environmental deterioration could occur rapidly. Although plausible explanations linked these crises to acid rain and CFCs respectively, large gaps in the science remained. In both cases, concerted international action occurred only after the sensational discoveries were made public.

In the end, however, political decisions can be taken which seem to fly in the face of the scientific evidence. The Reagan administration's acid rain policy, for instance, ran contrary to the recommendations of the U.S. National Academy of Sciences and the committee established by the White House itself. The rationale for U.S. policy may have been ideological, or it may have been rooted in the high cost of further sulphur dioxide emissions. In any case, this highlights the fact that while science plays a crucial role, environmental policy making is essentially a political, and not a scientific, matter. While the Reagan administration may have sacrificed some political capital in obstinately maintaining its policy of inaction, for eight years it successfully resisted both international and domestic pressures to adopt control measures.

6.3 Science, Politics and Global Climate Change

The problem of human-induced climate change dramatically poses the dilemma of formulating policy under conditions of scientific uncertainty when the stakes are very high. As in the case of ozone depletion, the twin problems of accumulation and irreversibility may compel preventive action before environmental damage is measured conclusively. But, unlike the ozone case, costly changes on many fronts will be required to mitigate global warming. Despite the potential for even greater harm from the greenhouse effect than from ozone depletion, the complexities of the former -- scientific, technical and political -- are more likely to hinder effective international cooperation.

The politically differences between the two problems are striking. First, the U.S. has not assumed a leadership role in promoting international efforts to control greenhouse gases. While two cases are insufficient to infer any significant generalisations, the contrast between the success of the Montreal Protocol and the sluggish pace of climate negotiations suggests that a major power can at least block progress. Moreover, the fact that U.S. scientists pioneered discoveries in both fields of research suggests that scientific proficiency does not correlate with political leadership. Likewise, one should not conclude from the acid rain case, where the U.S. lagged behind some European countries both scientifically and politically, that political belatedness is a function of scientific inferiority. In fact, once a policy decision is made to resist environmental controls, a country's access to abundant scientific information can help bolster that decision. As in the ozone case, knowledge can serve as a political tool and not necessarily as the foundation for a policy consensus; for global warming, science has been used to justify a U.S. policy of inaction.

Second, in contrast to the two environmental problems already examined, developing countries are major players in the global climate change debates -- both as contributors to the problem and, more importantly, as key participants in any adequate solution. Thus, a host of equity issues that were largely absent in past debates on international air pollution problems has arisen. **[6]** In this sense, climate change can be seen as the first truly global environmental problem. The entrance of developing countries into the global ecology debates highlights a crucial question regarding the role of scientific knowledge in international environmental politics: If scientific knowledge is an important political tool, how can an equitable policy consensus be reached between North and South when the distribution of informational resources is so radically skewed? This troubling question underlies much of the existing political tension between industrialised and developing countries on the global warming issue.

The level of political involvement by scientists in the climate change issue is unprecedented in global environmental politics. While scientists were responsible for putting the problems of ozone depletion, acid rain, and climate change on the political agenda, only on the last issue have they been outspoken regarding policy options. For the first two issues, scientists were key actors in framing and defining the problems, but for the most part were quite cautious when it came to making policy recommendations. **[7]** In contrast, scientific conferences for the past several years have generated a plethora of declarations in favour of serious, concrete efforts to control greenhouse gases. If the issue is truly shrouded in scientific uncertainty, as it is commonly taken to be, then this high level of political involvement by scientists is rather puzzling. I argue that this puzzle can be clarified, at least partially, by considering the following. First, most of the scientific uncertainties revolve around the *timing* and the *degree* of expected climate change, not on *whether* major changes will occur. Most climate experts avoid pinpointing the timing, preferring to avoid "the predictable embarrassment" of explaining the inevitable cold years [Usher 1989: 25]. But there is a broad consensus that humanity is "moving the climate into uncharted territory" [*Los Angeles Times Magazine* 1989: 16] Given a consensus that unprecedented climatic change is imminent, it is not surprising that scientists have made strong policy recommendations. The values implicit in their proposals typically go unstated, and perhaps unexamined, but large numbers of scientists seem to believe that it is unwise to tamper with the global climate system in ways that induce conditions with which the human species has no experience.

Indeed, the structure of the Intergovernmental Panel on Climate Change (IPCC), the primary global mechanism for addressing both the science and the politics of the problem, is conducive to involving scientists in the policy process. At first glance, this would appear not to be the case since the panel is broken down into three separate working groups: science, environmental and social impacts, and policy response strategies. While each of the three working groups has a separate membership composed of different sorts of experts, the fact that the groups' deliberations have proceeded simultaneously tends to blur the distinction between science and politics. **[8]** Thus, the IPCC has been aptly described as "operating at the cutting edge of science and policy" [*New York Times*, November 5, 1990].

Finally, it seems that some scientists were encouraged by the Montreal Protocol experience to be more outspoken on policy issues. Because many of the scientific issues are similar, there is some overlap in the ozone and climate change scientific communities. (Recall that CFCs are powerful greenhouse gases, and the stratospheric ozone is an important determinant of climate.) Certainly, by the time the global warming issue was on the international agenda, a fair number of the relevant scientists were accustomed to being in the political limelight. More importantly, after the Montreal Protocol was signed in 1987, not only environmentalists, but scientists as well, turned their attention to the climate problem [*Nature*, September 24, 1987].

6.4 Greenhouse Science and Uncertainties

Although the theory of the "greenhouse effect," caused by the radiation-trapping properties of certain gases, is one of the oldest and most widely accepted theories in the atmospheric sciences [Schneider, 1989: 771], global warming has only very recently become an international policy problem. Greenhouse gases, principally carbon dioxide, act as one-way filters, allowing the sun's visible radiation to pass through to Earth while absorbing most of the infrared radiation [UNEP 1987: 8-11]. The greenhouse effect is responsible for the earth's mildly warm climate, one which has been conducive to the evolution of life. Planetary climatologists refer to this as the Goldilocks Phenomenon: without it, our planet would be cold and lifeless like Mars; with too much of it, Earth would be unbearably hot like Venus [Schneider 1988: 2930].

As early as 1896, the Swedish scientist Svante Arrhenius, recognising that large-scale industrialisation could alter the global climate, calculated that a doubling of carbon dioxide would increase the Earth's temperature by 6°C [Arrhenius 1896]. His estimate was remarkably close to those of today's computer models. Until recently, however, most scientists believed that the oceans would absorb virtually all human-produced carbon dioxide. In 1957 two researchers at Scripps Institution of Oceanography, Roger Revelle and Hans Suess, demonstrated that only half of the carbon dioxide produced by humans is absorbed by the oceans. In a phrase that has since been repeated frequently, they declared that "mankind is conducting a great one-time geophysical experiment" [Revelle and Suess 1957: 27]. More recently, scientific interest in the greenhouse effect has been stimulated by the discovery that other gases may double the warming trend expected from carbon dioxide alone [Lacis *et al.*, 1981; Ramanathan *et al.*, 1985; Cicerone and Dickson 1986]. The primary greenhouse gases, besides carbon dioxide, are methane, nitrous oxide and the CFCs. **Table 6.2**, below, shows their atmospheric lifetimes, current concentrations, annual rates of growth, and their contributions to global warming.

Table 6. 2

Major Greenhouse Gases

Name	Atmospheric Life (in years)	Contribution to Radiative Forcing	Annual Growth Rate
Carbon Dioxide	120	55%	0.5%
Methane	10	15%	1%
CFCs	65-150	24%	4%
Nitrous Oxide	150	6%	0.25%

Compiled from data in WMO/UNEP, *IPCC First Assessment Report* (August 1990), pp 6-12 of the Policymakers Summary.

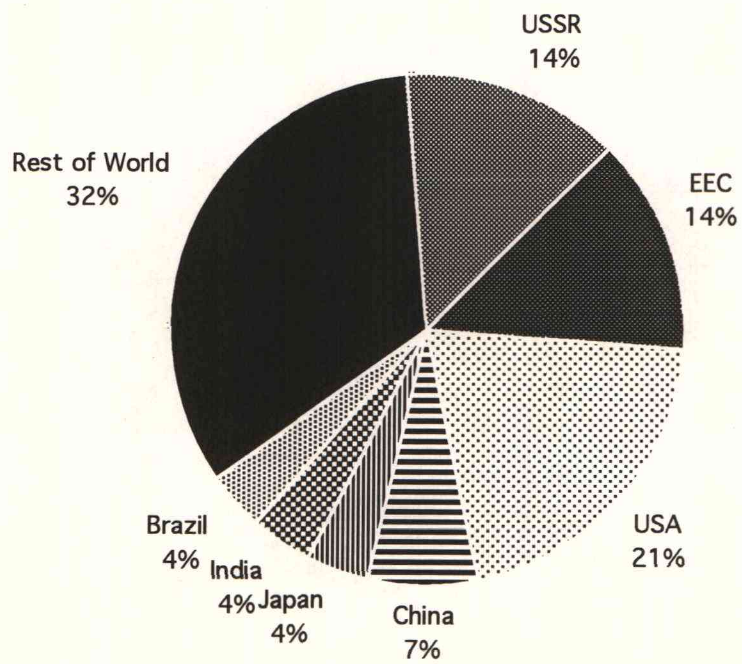
Although the climatic impact of CFCs was a relatively minor consideration during the Montreal Protocol negotiations, this chart shows the magnitude of the problem: CFCs are powerful greenhouse gases, their atmospheric lifetimes are very long, and their recent growth rates have been high. Taking these factors into account, the IPCC has calculated the twenty-year global warming potentials for CFC-ll and CFC-12 to be 4500 and 7100 respectively, with carbon dioxide having a value of one [IPCC 1990, Table 2.8]. In other words, a one kilogram reduction of CFCs will have the same effect in twenty years as reducing carbon dioxide emissions by several thousand kilograms. Eliminating CFCs would be a fairly painless way to delay the onset of global warming by several decades, except that many of the substitutes that are filling the CFC market have nearly the same heat-trapping potential as their precursors [*Atmosphere*, February 1991: 2].

Like the CFCs, nitrous oxide has a very long atmospheric lifetime, but it has received little attention because its yearly growth rate is so low. Its anthropogenic sources include mineral fertilisers and, indirectly through reactions involving carbon monoxide, automobile emissions. On the other hand, methane, with its rapid growth rate and short lifetime, has received a great deal of attention. Its sources include livestock, coal mining, ventilation of natural gas, and rice cultivation.

It is helpful to break down the cluster of issues known as the greenhouse effect into the following series of steps, each with its own set of inherent uncertainties: projecting emissions, projecting future concentrations of greenhouse gases, predicting climate, and likely environmental and social consequences. Not only is this a logical way of considering the problem, it also sets the stage for an argument I will make later in greater detail: that the *nested complexity* of the global warming problem is such that there can be no experts on it *per se*, but only for specific subsets of the problem. For this reason, policy makers may base their beliefs more on the authority of the information's sources than on the persuasiveness of the arguments at hand simply because consideration of all the relevant issues would be too time consuming and would require expertise in too many fields. Thus, the global climate problem should be characterised in terms of *complexity*, and not just uncertainty, though the two are closely related since large numbers of causal factors and feedback mechanisms open the door to greater uncertainty. Moreover, since scientific authority is not straightforward, the policy process may be involved not only in recognising it but in conferring it for some participants.

To a degree even greater than in the ozone controversy, the natural and the social sciences are intertwined. Projecting future emissions of greenhouse gases requires a range of behavioural assumptions, any one of which is contestable. Fossil fuel consumption, the main source of carbon dioxide, will vary according to population growth. Per capita consumption will depend upon such factors as national energy policies, oil and coal prices, and the availability of alternative energy sources. Carbon dioxide levels will also be a function of deforestation, currently thought to account for about 20 percent of total carbon dioxide emissions. Methane emissions will vary with agricultural practices, as well as levels of coal and natural gas use. Future CFC emissions will be greatly altered by the Montreal Protocol which, along with recent revisions, could not have been foreseen as recently as five years ago. Thus, the work of the climate modellers is heavily dependent upon data from the social sciences. Recognising the uncertainties involved, modellers typically make predictions for a range of scenarios, with one reflecting "business-as-usual" assumptions [Mintzer 1987: 7-12; IPCC 1990: 1]. Although there is some uncertainty about present emissions, the proportions shown below in **Figure 6.1** represent a broad consensus.

Figure 6.1: Relative contributions to greenhouse gases



Source: EPA; GAO/NSIAD-90-63, p. 12.

Even if future emissions of greenhouse gases were known, there would be fierce debates on the framing of the numbers. Poor countries, which prefer to emphasise per capita emissions, have serious misgivings about making any cuts at all [Ahuja 1990]. They point out that the average American consumes five times as much carbon dioxide as the average person on the planet, using as much wood in the form of paper as the average resident of a Third World country burns as cooking fuel [Duke 1988: 29]. Wealthy countries, being more efficient in their energy usage, focus on emissions per unit of GNP. Japan, with its energy efficient industries, tends to use these figures [Yokobori 1990]. To illustrate, Japan and Brazil each account for 4 percent of the world's greenhouse gas emissions, but Brazil's emissions per unit of GNP are about fifteen times as large as Japan's [from data in Brown and Flavin 1988: 16]. None of these modes of framing the information is more valid scientifically than the others; each, however, appeals to a particular set of social interests.

Once greenhouse gas emissions are projected, the next task is to calculate their future atmospheric concentrations. Because of the complex biogeochemical processes governing the global carbon cycle, emissions do not automatically translate into concentrations. Roughly half of humanity's carbon emissions are thought to be absorbed by plants, soils, and the oceans [UNEP 1987: 13-15]. Complex feedback mechanisms spurred by global warming could affect some of these sinks. Forest death and increased microbial decomposition in the soils brought on by a warmer climate could amplify the greenhouse effect. Large quantities of methane trapped below the frozen tundra could be released with significant Arctic warming, thereby adding to the accumulation of greenhouse gases [Schneider 1989: 773-74].

Emissions are not equivalent to concentrations for another reason: gases with long atmospheric lifetimes accumulate over time. This fact was one of the key arguments during the Montreal Protocol talks for strictly regulating CFCs. In the current climate change debates, methane is considered by some to be "an ideal candidate for control" because of its short atmospheric lifetime [Gibbs and Hogan 1990: 23]. Reductions of only ten percent would be sufficient to stabilise methane concentrations, so that the full value of the cutbacks would be experienced in the very near future. Interestingly, the opposite argument was used to promote an immediate phaseout of CFCs. Recall that their *long* atmospheric lifetime meant that an immediate 85 percent reduction was necessary to simply stabilise their concentrations. The argument for methane controls appeals to the desire for immediate, benefits; the argument for CFC controls appeals to a concern for long-term environmental stability. Both arguments are modes of framing scientific information which are intended to promote regulation of specific chemicals.

An evolving consensus in several spheres has enabled atmospheric scientists to project greenhouse gas concentrations with some confidence. Scientists agree that industrialisation has increased carbon dioxide concentrations by 25 percent [IPCC WGI 1990: 6]. Since the mid-1980's, there has been a consensus that other gases will roughly double the greenhouse effect of carbon dioxide [Bolin *et al.*, 1986]. Computing the global warming potentials of the various greenhouse gases requires accurate assessments of their absorption spectra, *i.e.*, the wavelengths of radiation absorbed by the gases. These values have been refined in the past decade and there is a broad scientific consensus on them [see Ramanathan *et al.*, 1985; Lashof and Ahuja 1990]. Based upon a range of plausible economic data, scientists expect a doubling of equivalent carbon dioxide concentrations sometime in the next century. The four scenarios adopted by the IPCC science working group anticipate a doubling sometime between 2025 and 2050 [IPCC WGI 1990: 1].

Once greenhouse gas concentrations are estimated, their impact on the global climate can be projected by inserting the numbers into the computerised general circulation models (GCM's). [9] These models attempt to reproduce Earth's climate system by superimposing a grid of typically 400-by-400 kilometres boxes on the earth's surface, overlaid with approximately twenty layers up to an altitude of twenty kilometres, and solving equations representing the basic laws of physics for each box. The accuracy of the models is tested by its ability to model the temperature differences between summer and winter, as well as the climates of Mars and Venus [Schneider 1988: 32]. Recent GCM's estimate that an equivalent carbon dioxide doubling would cause an average global warming between 3.5 and 5°C. (For a comparison of results from different models, see Schlesinger and Mitchell 1987). Changes, however, in the mean global temperature may be less serious than changes in the temperature *differential*: an expected increase of roughly 1°C at the equator, compared to 8°C or more at the poles. To put these figures in perspective, consider that the average global temperature during the last Ice Age (18,000 years ago) was only 5°C colder than today's [UNEP 1987: 4]. Remember also that the models only generate averages, but people die from extremes -- cold spells, heat waves, droughts and storms [MacDonald 1989: 20].

The largest uncertainty in estimating future climate stems from the problem of measurement. Clearly, major variables, like clouds, occur on a much smaller scale than the GCM's grid, but parameters describing the average values of these variables are used. For instance, average cloudiness can be expressed as a combination of mean relative humidity and average altitude of the moisture. But no matter how high the resolution, all models must translate dynamic local processes into large-scale averages, a process that can at best provide only a rough approximation of actual weather patterns [Schneider 1989: 774]. Moreover, most models neglect oceanic currents, instead treating the oceans like a large swamp [UNEP 1987: 21].

Another major source of uncertainty in climate predictions comes from attempting to account for interactive feedback processes. For instance, a warmer planet will have less snow cover. Snow reflects radiation, so a darker planet will also be a warmer planet -- a positive feedback mechanism. A more complex feedback mechanism involves clouds. A warmer Earth will lead to higher rates of evaporation, thereby producing more clouds. Because water is an important greenhouse gas, many scientists believe that increased cloud cover would enhance the warming effect [UNEP 1987: 13]. Others suggest that clouds will reflect more incoming radiation, leading to a cooling effect [Lindzen 1990: 47], although only a small minority of climatologists feel that the net effect is likely to be negative [Cess *et al.*, 1989]. There is a broad consensus that the cumulative impact of these and other feedback mechanisms will be to augment the warming trend [IPCC 1990: 3].

Two other sources of uncertainty in the models are noteworthy. First, the data in the models necessarily come from incomplete climate records, and the monitoring stations reflect a land-based bias [Bryson 1989: 98]. Second, many models treat the atmosphere as a pure gas, ignoring the cooling effect of aerosols other than clouds [Bryson 1989: 99]. This topic, however, is receiving increased attention [Advisory Group on Greenhouse Gases 1988].

While the models do not produce identical results, they generally foresee a warming trend in the next century. Since scientists agree that we are presently in a relatively warm interglacial period, this means that the anticipated global warming will be steering the planet into uncharted waters. The IPCC science assessment, which drew on the work of over three hundred scientists, predicts that the warming will be between 1.5° and 5.0°C [IPCC 1990: 3]. There is a solid consensus that, even if all greenhouse gas emissions ceased today, the build-up in the atmosphere and the thermal lag of the oceans have already committed Earth to a warming of about 0.8°C above the pre-industrial period [Ramanathan *et al.* 1985]. The models also agree that overall precipitation will increase by between 7 and 11 percent, with wide regional variations and some areas becoming much drier [UNEP 1987: 22]. While the models depict a gradual warming, experts agree that the actual changes will occur sporadically in "jump events" [Postel 1986: 7]. Consequently, they anticipate major surprises, such as shifts in the oceans' circulation patterns, [Broecker 1989: 202-204]. Given that only a little over a decade ago scientists believed that an Ice Age was as likely in the next century as global warming [Tickell 1977], this degree of consensus is remarkable.

A major source of controversy, however, is whether a warming has already occurred. At first glance, if warming is inevitable and irreversible, actual detection of a warming signal may seem superfluous. Yet ultimately the theory remains unproven until its predictions are experienced. Detection, therefore, is a scientific problem with major political ramifications, for it would increase the credibility of scientists' warnings and make action more politically feasible. The summer of 1988, when the Mississippi River dropped twenty feet, thwarting all barge traffic, brought the global warming problem to the attention of Americans. Climatologist James Hansen became internationally prominent when he testified before Congress that there was a 99% probability that the hot years of the 1980's were a result of the greenhouse effect [Hansen 1989: 40]. According to another climate scientist,

> the irony is that a solid case of good physics had been given vastly too little credibility for 15 years, whereas one, essentially random, hot event in 1988 has perhaps given us climatologists too much credibility [Schneider 1988: 35].

Whether or not the summer of 1988 was the result of the greenhouse effect, it did reveal society's limited ability to cope with even small changes [Usher 1989: 27]. Occurring just before the U.S. Presidential election, it also delivered the climate change issue from political obscurity. Abnormal weather has raised concerns in other countries. As much as anything else, several storms of historic proportions during the late 1980's sparked British interest, both scientific and political, in the climate problem [*London Times*, 21 October 1989; 15 December 1989]. Recent snowfall in the Alps was extremely scant in the late 1980's, causing concern among the Europeans [*Petroleum Economist*, January 1989]. Prime Minister Bikenibau Paeniu of Tuvalu told the Second World Climate Conference in November of 1990 that global change had already manifested itself in his country in the form of increased tropical cyclones and a decade of lower rainfall [Atmosphere, February 1991: 6].

The evidence of warming, however, is stronger than a few "random" events. Data collected at NASA's Goddard Institute shows that the seven warmest years since the 1880's have occurred since 1980, and the warmest year on record was 1990 [Atmosphere, February 1991]. Decreasing snow cover in the Northern Hemisphere [*New York Times*, October 30, 1990] and rising sea levels around the world [Schneider 1988: 33] provide strong circumstantial evidence for a global warming signal. The observed global warming of 0.3 to 0.6°C in the past century is consistent with the modelled predictions, but it also falls within the range of natural variability. An unequivocal signal of greenhouse warming is unlikely for another decade [IPCC 1990: 3].

Since the effects of global warming will be local and regional in nature, but the models' generate global averages, there is considerable uncertainty surrounding predictions of environmental impacts. For this reason, the report of the IPCC's working group on environmental consequences is extremely vague in contrast to the science assessment. **[10]** Much of it is devoted to the question of sea level rise, which can be calculated easily from a given increase in the global mean temperature and is probably the best understood environmental consequence of global warming. Since the 1985 scientific conference in Villach, Austria, there has been general agreement that a 20 to 140 centimetre rise in the world's seas can be expected as thermal expansion [Conference Statement in Bolin *et al.*, 1986]. Although it is itself a global average, this figure can be used as the basis for estimates of specific environmental impacts. A rise of one metre, for instance, would inundate low-lying coastal areas [UNEP 1987: 32].

Less is known about other potential environmental effects. Whether and to what degree the Antarctic ice sheets will melt is a matter of controversy [Schneider 1989: 777]. Although more precipitation is expected globally, higher rates of evaporation mean that soils in many places will be drier, particularly in semi-arid regions [IPCC 1990: 6]. Crop yield and forest productivity will be affected not just by changes in precipitation patterns [Postel 1986: 22], but by the altered composition of the atmosphere. Carbon dioxide acts as a fertiliser, increasing plant growth both for crops and weeds, but also decreasing food quality [UNEP 1987: 26-27]. Warmer climates will also increase the likelihood of pest infestations. A few areas, like Soviet Europe, can expect agricultural benefits from global warming, but most stand to lose from it [UNEP 1987: 29]. Changes in the timing and severity of the monsoon may threaten already marginal tropical areas [Pittock 1987: 27]. Because climates will probably change more quickly than species can travel or adjust, large losses of plant and animal biodiversity are expected [U.S. Interagency Task Force 1989]. Oddly enough, it is simpler to predict what will happen to the planet, which is essentially a closed system, than to make forecasts for specific regions [*Scientific American* 1989: 20]. Only a handful of countries, including Canada, the U.S. and the Netherlands, have commissioned studies on the effects of global climate change within their own borders. The U.S. study, mandated by Congress and performed by the EPA, is the most comprehensive [Smith and Tirpak 1989]. The EPA study predicts the destruction of most of the nation's wetlands and the disappearance of millions of acres of forests. Adaptation, the study concludes, will be possible, but the price will be high.

Such national studies have a built-in bias -- a bias with important political implications: only those wealthy countries with a large scientific infrastructure can afford to do them. **[11]** If a nation's leaders are unaware of the potential impact on their own country of global climate change, then they are unlikely to believe that action is necessary. This bias is deepened by the fact that even allegedly global studies tend to focus on environmental effects in wealthier regions. Since most climate scientists are citizens of those countries, the bias is not surprising. It is even evident in the work of the IPCC, in spite of its special task force on involving developing countries. The science working group looked at climate changes in five areas of the globe. Two of those areas were, predictably, Europe and North America. In the Southern hemisphere, only Australia was examined [IPCC 1990: 17].

Scientists stress that a critical factor will be the *rate* of climate change. Rapid changes will be far more cataclysmic than a gradual warming because there will be insufficient time for adaptation. An equivalent doubling of carbon dioxide is typically taken as the danger point. If greenhouse gases were to continue to increase at 1973 rates, a doubling would occur by 2025. Holding growth rates to one percent, however, would delay the doubling by a century [IPCC 1990: 17]. Because of the nested complexity of the problem, it is difficult, if not impossible, for the layperson to consider all the relevant evidence and arguments. The interdisciplinary nature of the greenhouse problem means that even most scientists cannot fully grasp the interconnected fields of knowledge that must be considered in order to arrive at an overall picture. The non-expert, therefore, is likely to seek areas of scientific consensus and then believe what the majority of the experts seem to believe, not because they have been *persuaded* but out of a *faith in authority*. There may be good reasons for those beliefs, but the non-expert need not -- and cannot -- understand them all. Policymakers are non-experts, so the same holds for them. Thus, the authority of the scientific community is increased simply by the nature of the problem, though paradoxically this does not necessarily endow it with greater influence.

Scientists as a whole have been more outspoken on this issue than on any major environmental problem in the past, but their recommendations have not been adopted. The turning point came in 1985 with the discovery by Ramanathan, Cicerone and others that other trace gases would double the greenhouse warming of carbon dioxide [Pomerance 1989: 262]. Prior to then, most studies adopted the wait-and-see approach condoned by the U.S. National Academy of Sciences in its 1983 assessment [NAS 1983]. Even policy-oriented assessments tended to argue that global warming could not be meaningfully mitigated by actions taken in the near-term [Seidel and Keyes 1983]. However, unlike the scientific conferences on ozone depletion, recent international climate conferences have resulted in clear declarations by scientists in support of urgent action. Conferences in Villach and Bellagio, sponsored by UNEP, WMO and the International Council of Scientific Unions (ICSU), have called for a rapid reduction in the use of fossil fuels, increasing energy efficiency by 50 percent, cessation of deforestation, and massive reforestation programs [Woodwell and Ramakrishna 1989: 289]. The intended goal is to limit the rate of global warming to 0.1°C per decade in order to stabilise the world's climate [Jaeger 1988].

No doubt, scientists themselves stand to gain from the burgeoning political interest in climate change. Huge increases in research and monitoring budgets are needed to resolve existing scientific uncertainties [MacDonald 1989: 23]. Indeed, quite a few scientists are "building careers on the concept" [*Los Angeles Times*, 1989: 12]. The fact that scientists might benefit from the high degree of political concern, however, need not taint their policy recommendations. They would benefit more by claiming that further research is necessary before specific control measures should be adopted. But they have not taken this route; instead, they have called for radical policy shifts *before* the uncertainties are resolved. *Scientific American*, for instance, has placed paid advertisements in *The New York Times* [January 17, 1990] calling for specific cuts in carbon dioxide emissions, programs for reforestation, and the subsidised transfer of technology from the wealthy countries to the Third World.

There is also a trend toward mixing conference diplomacy with scientific meetings. The World Conference on the Changing Atmosphere, held in Toronto in June 1988, began as the first in a series of international scientific gatherings on air pollution, but was elevated to a high level political gathering when the Canadian and Norwegian prime ministers attended. Over 300 scientists and policymakers produced a statement calling for reducing carbon dioxide emissions by 20 percent of 1988 levels by 2005. The statement also recommended that work proceed toward a comprehensive "Law of the Air," with a framework convention modelled on the Vienna Convention on the ozone layer and a World Atmosphere Fund to facilitate technology transfer to developing countries [Environment Canada, 1988]. Although the declaration was considered quite sweeping, scientists recognised that 20 percent cuts would fall far short of stabilising the atmosphere. The U.S. EPA, for instance, estimates that to stabilise atmospheric concentrations of carbon dioxide at the current level, emissions must be reduced by 50 to 80 percent, which would put them at 1950's levels [Flavin 1990: 20]. Rather, participants in the Toronto conference saw their target as a pragmatic estimate of what was politically attainable [Usher 1989: 26].

Two categories of policy responses are available: limitation of damages and adaptation to the consequences [Jaeger 1989: 99]. Limitation strategies include energy conservation, alternative energy sources, emissions recovery systems, and reforestation [UNEP 1987: 33-38]. These are controversial because they require immediate and often costly action to prevent indeterminate environmental changes. Adaptation strategies, generally favoured by economists, include building sea dykes and planting different crop varieties. These are often advocated because of uncertainty regarding future environmental conditions and because of high discount rates [Schneider 1989: 777]. Strong support exists for "tie-in strategies" having clear benefits independent of any future climate changes. These measures include energy conservation, adoption of renewable energy sources, reforestation, and, for some, increased use of nuclear energy [Kellogg and Schware 1982: 1104].

As in CFC substitutes, "availability" of technologies that could mitigate global warming often turns out to be a political and economic problem, and not so much a technical matter. Consider, for instance, the fuel efficiency of cars, which account for nearly 25 percent of all carbon dioxide emissions. Numerous high-efficiency cars were developed in the early 1980's in response to the 1979-80 oil crisis; most of their highway fuel economies exceed 75 miles per gallon. But they have never been commercially available because there has not been a perceived market for them [Bleviss 1990: 26]. A whole different set of concerns arises in weighing strategies for reducing Third World greenhouse gas emissions. Globally, 25 percent of carbon dioxide from fossil fuels is emitted in developing countries, although these countries account for nearly 80 percent of the world's population [US AID 1990: 2-4]. While per capita energy use is a small fraction of that in industrialised countries, production and use of energy in developing countries is very inefficient so that the potential for applying "tie-in strategies" is great [Prickett *et al.*, 1989]. On the other hand, even with intensive efficiency programs, total energy usage is expected to grow for decades, eventually surpassing that of industrialised countries [US AID 1990: 1-3].

Many developing countries have adamantly opposed strategies that go beyond their immediate economic interests. Policies that cross the "pain threshold," they argue, should be paid for by wealthier nations [Munasinghe 1990]. Some Third World countries have entered the global warming debates only because they see opportunities for enhancing their economic development. Since temperatures near the equator are expected to rise only slightly, and precipitation may increase substantially in some regions, some developing countries believe they will benefit from global climate change [Woodwell and Ramakrishna 1989: 290]. Not surprisingly, the oil producing countries, led by Saudi Arabia, have been particularly resistant to any reduction in fossil fuel consumption [*The Economist*, June 16, 1990: 18]. For all of these reasons, efforts to stabilise the global climate have encountered considerable scepticism in much of the Third World. Some scholars in developing countries point out that global climate change will simply mean that countries in the temperate zones will have to contend with some of the same environmental disadvantages that have always afflicted tropical countries. Tropical weather, they argue, has been a major impediment to industrialisation. One Indian political scientists suggests that the emphasis should be on ameliorating tropical conditions through deliberate climate change, rather than on preventing future warming in the higher latitudes. He states:

> From a global point of view, the permanent disadvantages imposed on the South by the tropical macroclimate are of infinitely greater environmental significance than the local and regional issues faced by the countries of the North and popularised by them through vigorous international propaganda [Bandyopadhyaya 1983].

Even a report by the U.S. National Academy of Sciences notes that "the present North-South dichotomy can be described as a climatological division" [NAS 1978: 22-23]. A major factor contributing to the developing countries' scepticism is the lack of their own base of scientific knowledge. Nearly all information on global change is generated by a small "club of climate scientists," based principally in the U.S., the U.K. and the Soviet Union [Testimony by William Nitze, U.S. Congress, October 26, 1989: 24]. Notably, the IPCC's three working groups are chaired by representatives from these three countries. Special efforts have been made to disseminate expertise to the developing countries through roving seminars held on a regional basis and sponsored by scientific institutions in the North such as The Woods Hole Research Centre. Despite such efforts, official delegates to some IPCC meetings appear to have been technically uninformed. According to scientists who attended one such meeting,

> Representatives from the less-developed countries complained at being excluded from discussions, but they were not sufficiently well-versed in the topic either to advance arguments that were specific to their interests or to advance proposals for solutions [Woodwell and Ramakrishna 1989: 291].

The situation may be improving due to the large number of information exchange seminars being held by governments and regional organisations for developing countries in 1990 and 1991 [IPCC SPC Policymakers Summary: 7].

As mentioned earlier, the lack of scientific infrastructure and technical resources means that there are no climate impact studies for developing countries similar to the one published in the U.S. by the EPA. Without this knowledge, it is impossible for developing countries to adequately formulate a conception of their own interests. Thus, many climate scientists believe that a truly global policy consensus will require comparable studies in the Third World, and have urged their own governments to funnel resources there for such studies [MacDonald 1989: 18]. The U.S. Global Change Research Program (USGCRP), for instance, has included in its 1992 budget funds for a new initiative to create global change research institutes in developing countries [U.S. Committee on Earth and Environmental Sciences 1991: 81-82].

The strong position taken by the "Group of Small Island Nations," however, casts doubt upon the notion that comprehensive national impact studies must precede political action. Despite the fact that most of them lack the resources to invest in national studies, this group, led by the Maldives and Trinidad and Tobago, has been extremely visible and vocal in its demands for immediate major reductions of greenhouse gas emissions [*London Times*, 3 November 1989; *Atmosphere*, February 1991: 6]. They base their position not on specific environmental impact studies, but on the general prediction that mean sea level will rise. Interestingly, this is the only environmental effect that can be predicted on a global basis with any degree of certainty [IPCC Volume I, WGI: 22-23]. Although the "Group of Small Island Nations" comprises some of the world's weakest states, there are many of them; in the U.N. General Assembly, they may be able to exert substantial influence. Their position echoes an earlier message from the Montreal Protocol experience: accepted knowledge can be a function of perceived interests. The low-lying nations have readily accepted the scientists' predictions on sea level rise as a basis for action because their very survival is at stake.

In addition to its three working groups, the IPCC has established a Special Committee on the Participation of Developing Countries (SPC). That committee made several recommendations in the IPCC's first assessment report of 1990, virtually all of which dealt with the shortage of scientific and technical resources in the less-developed countries. The kinds of information found to be insufficient included: scientific data; techniques for designing computer models; analytical tools for performing impact analyses; state-of-the-art methods of energy production; and tools for determining possible policy options [IPCC SPC: 8]. Many developing countries are unable to monitor even their own weather in accordance with international requirements, much less predict the impact of future climate changes on their own populations [IPCC SPC: 4].

This lack of resources has had a major effect on the functioning of the IPCC. In its report, the SPC devotes a good deal of attention to the problem of travel to IPCC meetings, the majority of which are held in the Northern hemisphere. A special IPCC Trust Fund was established to help boost attendance from developing countries. The number of developing countries represented at the first IPCC plenary in 1988 was 11, rising to 17 at the second plenary in 1989, and to 33 at the third in 1990. At the 1990 meeting, the number of developing countries exceeded that of industrialised countries for the first time. Yet these numbers are deceptive. The shortage of experts has often meant that these countries are represented by embassy officials lacking the technical background to effectively participate in the meetings [IPCC SPC: 5-7].

The poor quality of scientific infrastructure in many developing countries also means that data about greenhouse gas emissions is sketchy. This is particularly true for data on tropical deforestation, which is thought to account for anywhere from 5 to 33 percent of global carbon dioxide and between 10 and 20 percent of methane emissions [Prickett *et al.* 1989: 2; US AID 1990: 2-6]. Until recently, estimates of emissions from deforestation were based on 1980 rates of land clearing. A recent study by the Food and Agriculture Organisation (FAO), however, estimates that current rates are now about 50 percent higher [FAO 1990]. Satellite data suggest that deforestation may be occurring at even higher rates [Fearnside 1986; World Resources Institute 1991]. International scientific research projects have been launched to fill in the gaps in the data. In 1990, the world's top space agencies began a remote sensing program under Brazil's leadership to ascertain the status of Earth's tropical forests [dos Prado 1990: 36].

The lopsided distribution of knowledge resources has manifested itself in an international political struggle over the institutional setting of negotiations for a climate change convention. The U.N. General Assembly voted at the end of 1989 to have a draft convention ready for the 1992 Conference on the Environment and Development in Brazil [*New York Times*, January 3, 1990]. Since preliminary talks occurred during meetings of the IPCC, sponsored by UNEP and WMO, many observers presumed that the U.N. General Assembly would give negotiating authority to those agencies. Industrialised countries preferred such an arrangement, which in essence would have entrusted preparation of the treaty to technical experts. Developing countries, however, felt unable to fully participate in the IPCC process because it was dominated by experts from industrialised countries. Initially, a compromise was reached in Resolution 44/207 whereby UNEP and WMO would take charge of drafting the treaty, but the General Assembly would give instructions to the negotiators. However, in December 1990, the General Assembly Resolution 45/212 established an International Negotiating Committee under the General Secretary's office, supported by UNEP and WMO, for preparation of a framework convention on climate change [US Department of State and EPA 1991: 63, hereafter State/EPA]. The rationale was that the issue involved the very foundation of national economies, and was no longer a technical matter, like CFCs and ozone, that could be dealt with by an agency like UNEP.

Some non-scientist environmentalists and scientists alike worry that progress toward a treaty will be hindered because the new arrangement requires educating a new group of diplomats, and because the General Assembly might shift the emphasis to economic development [*Atmosphere*, February 1991: 3]. Another possibility, however, is that greater power could shift to the low-lying nations, which have been most supportive of stringent measures and are well represented in the General Assembly. Whatever the outcome, this decision clearly represents an attempt by the developing countries to counterbalance the power of the wealthy nations, related to their superior access to scientific knowledge, with their own political power in the U.N. General Assembly.

This series of events also reinforces a point made earlier with respect to the ozone negotiations: scientific knowledge can be an important source of power, but not necessarily so. Knowledge only influences one's actions when one has been persuaded of its validity, and one may not be persuaded if one perceives the interests of those possessing the informational resources to be antithetical to one's own. Thus, the *source* of the information becomes a crucial political issue, so that those with superior knowledge resources may need to finance the acquisition of that knowledge by those who with scarce resources. Developing countries will not make major efforts to control greenhouse gas emissions until they perceive such efforts as being in their interest, and this is unlikely until they have been convinced by evidence from trusted sources. The scope and complexity of the climate change issue has triggered an interesting plethora of joint research and information exchange programs between U.S. executive branch agencies and their counterparts in developing countries. Just to name a few, the National Oceanic and Atmospheric Administration (NOAA) joined with India in 1988 to study climate variability and the monsoon, and with Brazil to establish a climate prediction facility. EPA is involved in bilateral research projects with Brazil, China, Korea, Sierra Leone, India, Indonesia, and many other developing countries [State/EPA 1991: 49-52].

U.S. support for international research efforts has been strong, and is increasing. The U.S. has been a key participant in the World Climate Program, initiated in 1979 by WMO, UNEP, and ICSU, and the International Geosphere-Biosphere Programme (IGBP), launched in 1986 by the ICSU. Several of its national research programs are linked to these efforts, most notably the Global Change Research Program (USGCRP), initiated in 1989 by Congress in support of the IGBP and operated under direction of the White House Office of Science and Technology. The USGCRP, which interfaces with several international programs and incorporates projects of many federal agencies, is charged with developing the scientific basis for a sound national and international climate policy [Committee on Earth and Environmental Sciences 1991]. It was at the time (and remains) the world's largest research program on global change, having a 1991 budget of nearly one billion dollars, up by 45 percent from 1990 levels [State/EPA 1991: 48]. **[12]**

While the U.S. is an undisputed leader in the scientific dimensions of climate change, its political leadership has been more wavering. Congress passed the Global Climate Protection Act of 1987 (P.L. 100-204) requiring the Secretary of State and the EPA Administrator to submit a joint report detailing U.S. efforts to further international cooperation to limit global climate change. According to a General Accounting Office report released in January 1990, the Bush administration appeared to be applying the Reagan administration's acid rain strategy to global warming by emphasising the need for further research in lieu of committing itself to specific policies [U.S. GAO 1990: 26]. In the early part of his presidency, George Bush made several statements supporting the IPCC process and efforts to negotiate a framework convention on the global climate. At the July 1989 "Group of Seven" Summit of the most industrialised nations, Bush joined in a statement advocating efforts to limit greenhouse gas emissions. The 1990 Economic Summit reiterated that statement. At the 1989 Malta summit meeting with Soviet President Gorbachev, Bush stressed the commitment of the U.S.

by offering to host the first negotiating conference after the IPCC completed its interim report, a meeting which occurred in February 1991.

Despite these statements and pledges, U.S. leadership has not been forthcoming. At key international meetings, the U.S. has isolated itself as it blocked initiatives by other nations. At a meeting of environment ministers convened by the Netherlands and France in November 1989, the U.S., along with Japan, blocked movement toward cuts in carbon dioxide emissions, effectively leading to a watered down Noordwijk Declaration [*New York Times*, November 7, 1989]. President Bush's decision at Malta the following month to host the first IPCC negotiating conference was largely a response to criticism of the U.S. coming out of the Noordwijk conference. Again, at the Second World Climate Conference in November of 1990, the U.S. delegation, led by NOAA under the Commerce Department, stood aside as twenty nations set specific targets for reducing carbon dioxide emissions. Strong criticism from both scientists and its European allies, including a statement by Prime Minister Thatcher, did not sway the U.S. [*New York Times*, November 8-9, 1990].

The administration's ambivalence is linked to the interagency wranglings that began before the Montreal Protocol and intensified around the climate issue. Because of the complexity of the issue, nine U.S. agencies are involved in research on climate change. In contrast, federally supported ozone research was concentrated in NASA and EPA. While NASA absorbs the lion's share of the USGCRP budget, EPA has been a relatively minor scientific player. Nonetheless, EPA officials believe that the agency's experience in dealing with global environmental problems, particularly its role in the Montreal Protocol process, should qualify it to take the lead in formulating a national climate change policy [GAO 1990: 16]. Instead, the Department of Energy (DOE), whose share of the USGCRP budget is triple the EPA share, has been the dominant agency politically [CEES 1991: 86-87]. DOE has claimed that there is too much scientific uncertainty to justify action [Schneider 1988: 30]. Emphasising the uncertainties, Secretary of Energy James D. Watkins supports reforestation and efforts to reduce CFCs, but has opposed specific cutbacks in carbon dioxide emissions [State/EPA 1991: 26].

The U.S. delegation to the IPCC includes many of the same staff members who tried to weaken the U.S. negotiating stance on CFC controls, particularly from DOE and the Commerce Department. Although Secretary of State James Baker stated in a speech to the IPCC's Response Strategies Working Group that action could not wait until all of the scientific uncertainties were resolved, the U.S. delegation displayed considerable confusion at the early IPCC meetings [Stanfield 1989]. Unlike the question of CFC controls, nearly every U.S. agency has strong opinions on global change policies. Moreover, because the problem is both international and domestic, policy has been formulated in both the Policy Coordinating Committee of the National Security Council (PCC) and a special Domestic Policy Council working group, chaired by the President's science adviser [U.S.

Congress, October 26, 1989: 15]. **[13]** The large number of agencies with strong views and the lack of hierarchical clarity in the decision making structure have hampered the ability of the U.S. to take the lead politically, despite its strong scientific leadership.

The Montreal Protocol process has cast its shadow on the IPCC process, and in particular has served to rationalise the U.S. approach. U.S. officials often refer to the Vienna Convention as the model for a climate convention, advocating a "sequential approach" of first negotiating a general framework convention, deferring specific obligations until a later date [Testimony of Daniel Lashof, Congressional Hearing, October 26, 1989]. Likewise, Richard Benedick claims that the IPCC is "analogous to the fact-gathering phase of the ozone history," implying that the science and the politics can be neatly compartmentalised [Benedick 1990: 43]. But, as Chapters Four and Five show, these officials have misinterpreted the precedent; control measures were debated prior to the Vienna Convention, which was adopted only after agreement could not be reached. The entire process, including the scientific workshops preceding the formal negotiations, was a political process. Moreover, the IPCC process is structured such that scientific assessments and response strategies are being formulated simultaneously.

A U.S. strategy was finally announced in February 1991 at the first negotiating session for a climate change convention. That strategy, dubbed "the comprehensive approach," is strongly influenced by the Montreal Protocol and, in fact, would be unthinkable without it. The strategy was developed by a group of knowledge brokers from an interagency task force with representatives from most cabinet level departments and several presidential advisory offices. The proposed "action agenda" would stabilise overall greenhouse gas emissions at 1987 levels by the year 2000, although U.S. carbon dioxide emissions would continue to rise at approximately their present rate [Office of the President 1991: 2-3]. In essence, the plan merely renames the CFC reductions mandated by the Montreal Protocol as a climate stabilisation strategy. Although the "comprehensive approach" boils down to a reformulation of the Bush administration's earlier wait-and-see approach, not surprisingly it is justified in scientific terms. The U.S. position rests on the extremely high global warming potentials (GWP) of CFCs due to their heat trapping ability and their long atmospheric residence times. **Table 6.3** summarises the scientific basis of the U.S. strategy.

Table 6.3: Global Warming Potentials of Greenhouse Gases

Gas	Relative GWP over 20 years
Carbon Dioxide	1
Methane	63
Nitrous Oxide	270
CFC-11	4500
CFC-12	7100
HCFC-22	4100
HCFC-134a	3200

(Adapted from *Report of the Task Force on the Comprehensive Approach to Climate Change* (1991), p. 4: Source: IPCC 1990, Tables 2.3, 2.8.)

Although the volume of CFC emissions is far less than the volume of carbon dioxide, their enormous GWPs mean that their phaseout will effectively stabilise overall greenhouse gas emissions. Of course, this will only occur for countries with substantial CFC production in 1987, *i.e.*, the wealthiest nations. Under the "comprehensive strategy," other countries would presumably have to limit some combination of carbon dioxide and methane emissions.

Proponents cite declarations of the IPCC and the Second World Climate Conference calling for a "comprehensive strategy" which considers all greenhouse gases and their sinks [U.S. Task Force 1991, 1]. Clearly, there is widespread support in the scientific community for a genuinely comprehensive strategy that would stabilise Earth's climate. But the authors of the U.S. strategy, while focusing on global warming potentials, gloss over the fact that major *reductions* of greenhouse gases will be necessary to stabilise the global climate. Implicit in their argument is the notion that stabilising overall emissions will be sufficient. Yet, at least in their initial report, the impact of the proposed policies on future climate remains unexamined. In essence, the authors of the U.S. climate change strategy have taken the argument of the EPA knowledge brokers who forged the U.S. negotiating stance for the Montreal Protocol, and stood it on its head. The long atmospheric lifetimes of CFCs, which necessitated immediate reductions in order to simply stabilise the ozone layer, have been used by a new set of knowledge brokers to say that carbon dioxide emissions should not be reduced. The scientific information cited in support of the "comprehensive approach" is not *wrong*, it is simply *incomplete*. Like the knowledge brokers who formulated the U.S. position for the Montreal Protocol, these knowledge brokers have found a scientific rationale which does not rely on computer models. The former group, however, downplayed the modelled predictions because the Antarctic ozone hole had cast doubt upon the models. In the more recent case, there is no apparent reason to ignore the models. Consequently, the "comprehensive approach" is likely to remain unappealing to scientists and others interested in stabilising the global climate.

Indeed, the U.S. proposal has encountered strong opposition from many corners. Domestically, the Bush administration has come under fire from Democrats and environmentalists, who assert that the "so-called 'action plan' is in reality an 'inaction plan'" [*Los Angeles Times*, February 6, 1991]. Shortly after the plan was announced, the U.S. National Academy of Sciences indirectly registered its own criticisms in a report citing gross inefficiency in U.S. energy usage. That report concluded that the U.S. should act promptly to stabilise the global climate, and could cut greenhouse gas emissions by 40 percent with little economic cost [*Los Angeles Times*, April 11, 1991]. The western European countries and, more recently, Japan have strongly opposed the U.S. strategy. The Japanese were persuaded to abandon the U.S. position not so much by the scientific evidence as by their growing awareness of the potential economic opportunities associated with the issue. A consensus between government and industry has emerged that, because climate change is at least perceived as a real problem, international markets for Japanese pollution control technologies could begin to open [Oppenheimer and Boyle 1990: 170-173]. The E.C. has voted to stabilise carbon dioxide emissions by 2000, but many of its member nations plan more stringent reductions of 20 percent by then [Clark 1990, 74]. These European countries, most notably Germany, Denmark and the Netherlands, have sought to take on a leadership role by setting an example to other countries through their own unilateral cutbacks [*Los Angeles Times*, February 17, 1989]. The Dutch have also hosted two major international climate conferences, one jointly with France. Particularly in comparison to its position during the ozone and acid rain deliberations, Britain has emerged as a significant leader. The IPCC science working group is chaired by Britain and, during her tenure in office, Prime Minister Thatcher took a personal interest in the climate problem. While some sceptics suggested that her interest derived from her "nuclear obsession," she nonetheless guided her government in exceeding the E.C.'s pledge on carbon dioxide reductions [*New York Times*, May 10, 1989]. **[14]** Thus, the leadership roles of the U.S. and Western Europe on global warming are reversed from what they were for the ozone problem.

During 1988 and 1989, when the climate issue was in the political limelight, there were signs that a competition might emerge for who international leadership on global climate change policies. At that point, the U.S., the U.K., and the Soviet Union all seemed to be positioning themselves through their leaders' statements [Stanfield 1989: 511]. The sense of optimism that accompanied the end of the Cold War gave rise to the hope that the U.S. and the Soviet Union, the two largest greenhouse gas emitters, might jointly assume a leadership role. However, as the issue receded from public consciousness, the budding competition for world leadership also ebbed. Whether such leadership is a necessary condition for greenhouse gas controls to be implemented is a matter open to debate. What is clear, however, is that progress can be effectively blocked, at least for a time, through the actions of one country whose emissions are high and whose access to scientific information is great. If the Montreal Protocol experience seems to suggest that the primary polluter is likely to function as an international leader if it also happens to be a scientific leader, the global warming experience provides contrary evidence. Rather, strong opposition from domestic agencies and the existence of scientific uncertainties can provide a rationale for inaction. In any case, those actors with access to scientific information will often use it to bolter their political position. In the case of global warming, those actors with little access to scientific information also used that lack to justify their hesitancy. To a greater degree than in the ozone case, the global warming problem, perhaps because the stakes are so much higher, supports the view that scientific knowledge is a political tool. This case also suggests that if the issue does not force itself onto the public agenda by virtue of extreme weather conditions, the chances of international leadership emerging are remote in the extreme. In the case of global warming, there has not been a dramatic event like the Antarctic ozone hole or the "Waldsterben" in Central Europe to spur international action. The summer of 1988, at least in parts of North America and Europe, heightened the political salience of the issue, but it was a transitory event that did not provoke an enduring sense of crisis.

One should not infer from this that science has played an unimportant role in this issue. To the contrary: scientists have been more outspoken in recommending far-reaching responses to the greenhouse effect than for any other major international environmental problem. There is a popular misconception that the reason that there has been no concerted international action on global warming is that "the scientific consensus does not match that achieved in the ozone cases" [Stanfield 1989: 513]. In reality, among scientists there is "remarkable unanimity that it's a very, very real problem" [*Los Angeles Times,* May 21, 1989]. Since 1985, scientists have been calling for efforts to stabilise the global climate, efforts which would require as much as a 50 percent equivalent reduction in carbon dioxide. Such sweeping recommendations stand in marked contrast to the virtual silence of scientists on CFC reductions prior to 1987. Hence, it is evident that the international inertia is much more a political phenomenon than a symptom of the status of the science.

However, while scientists may be confident in both their *Global* predictions and their understanding of what measures will be needed for climate stabilisation, their ability to make local and regional forecasts is far more limited. Thus, the *specific* consequences of global warming remain unknown and are perhaps unknowable, just as the benefits of action are uncertain and remote in time. The specific costs of climate stabilisation are less uncertain, and they will certainly be high. Yet people only experience local weather conditions, not the global climate, and there is no way to say with confidence how a 3°C warming would affect an individual or a country. In a sense, scientists are working at a global level, while the consciousness of both the political leadership and the public remains local. Still, even if they could predict reliably at the local level, scientists would not have the last word.

6.5 Conclusion

In the cases of acid rain and global warming, as in the ozone case, scientific knowledge and political interests are mutually intertwined. Knowledge is used as a political tool to further the interests of various actors in each case, yet knowledge can also change their perceived interests. In all cases, the existence of scientific uncertainties provides some actors with a justification for their preferred policies of inaction. In each case, scientists have opened the policy debates by uncovering the problem and publicising their findings. And in every case, a first step taken by the international community to address the problem has been to establish mechanisms for scientific coordination and to generate an international scientific assessment as the authoritative body of knowledge. But unless accompanied by dramatic exogenous events -- the Antarctic ozone hole, massive forest dieback, and the summer of 1988 -- these studies have been largely ignored. Such events need not be conclusively linked to the problem at hand (in fact, none of them were); they need only call attention to it. Individual scientists may be particularly outspoken: Rowland on ozone, Ulrich on acid rain, and Hansen on global warming. Only on the climate change issue, however, has the scientific community as a whole publicly declared its support for far-reaching pollution control policies. Time will tell whether this new level of activism represents a larger shift toward greater involvement by scientists in international environmental politics.

There are some important differences, both scientific and political, between the three problems which suggest that concerted international action on the greenhouse effect will be far more elusive than it was for the other two. Scientifically, the consensus on *Global* climate change is actually somewhat stronger than it was for the other problems. (Recall that the Antarctic ozone losses had not been linked to CFCs when the Montreal Protocol was negotiated; nor was the German Waldsterben linked to acid rain.) Predictions of local and regional climate and environmental effects, however, are on very shaky ground. This means that nations and other social groups have a great deal more difficulty defining their own interests with respect to this issue, particularly because, unlike acid rain and ozone depletion, climate change may benefit some groups. The enormous complexity of the climate change problem opens the door to many more uncertainties, both scientific and political, than have plagued previous environmental problems.

The political hurdles themselves are daunting. While the primary actors in the ozone and acid rain agreements were the industrialised countries, developing countries have emerged as major actors in the global warming debates. This raises a host of equity issues that pose potential obstacles to agreement, not the least of which is the uneven distribution of knowledge between North and South. The disparity in access to scientific information has already proven to be a great source of conflict within the IPCC process and in debates over the proper arena for international negotiations. Technology transfers will be an indispensable part of any global change agreement, and these will entail efforts to balance the distribution of knowledge: all crucial science communications issues.

If current predictions are correct, climate stabilisation will require far greater sacrifices than either acid rain abatement or preservation of the ozone layer. Even if adopted globally, the 20 percent reductions of carbon dioxide emissions being taken unilaterally by several European nations would be woefully inadequate. The drastic character of the required measures is perhaps *the* defining political feature of the global warming issue, for those measures are perceived as colliding with innumerable interests, most notably those involving fossil fuel consumption. This quandary, in fact, underlies the lack of political leadership by the U.S. on global warming, a lack which stands in sharp contrast to the early U.S. support for strong CFC controls. Unlike the ozone case, where the EPA and the State Department took the lead, virtually every federal department has strong views on climate change policies. In particular, a group of knowledge brokers with a strong interest in unrestricted American fossil fuel consumption has been able to formulate the U.S. "comprehensive strategy" on the basis of valid, but partial, scientific information. Not surprisingly, scientific knowledge has again been used as a tool for political legitimisation, although this time not to promote an environmentalist agenda.

CHAPTER SEVEN
IMPLICATIONS FOR THEORIES OF WORLD POLITICS

As if in reaction to past materialist excesses, a good deal of attention has recently been given to ideas and information as sources of foreign policy and international collaboration. The literature harkens back to the recurring debate in history and the social sciences between materialism and idealism, and it appears as if the pendulum of international relations theory may be swinging, after several generations, in the direction of the latter. While much of the literature focuses on political and economic ideologies, some of it looks at scientific knowledge as a foundation for international cooperation.

The literature on epistemic communities, which explains international regimes as resulting from the consensual knowledge claims of a group of experts, seems well-suited to the highly technical arena of environmental policy. The Montreal Protocol, characterised by a high level of involvement by atmospheric scientists, seems at first glance to be an ideal case for applying the epistemic communities approach. However international environmental policy making cannot be explained by the notion of epistemic communities alone. Implicit in that approach is a conception of knowledge as divorced from power, generating a more rational politics. Questions of framing, interpretation and contingency are glossed over in an effort to explain politics as a function of knowledge. In contrast the discursive practices approach which sees knowledge and power as mutually interactive. The power of competing knowledges -- likely to be decisive under conditions of scientific uncertainty -- is the critical issue. While lacking in methodological tidiness, this approach is best able to reveal the complex dynamics of actual negotiating situations. An emphasis on discursive practices, rather than on groups or individuals, allows the interpretation of international regimes as loci of struggle among various networks of power and/or knowledge. Unlike the epistemic communities approach, issues of framing, interpretation and contingency are central to a discursive practices approach, and epistemic dissension is at least as likely an outcome as epistemic cooperation.

If those scholars who discern a trend toward a "post-industrial" or "informational" world order are correct, then this argument has important implications not just for environmental issues, but more generally for the nature of power in the emergent global system. One trend may be the diffusion of the sovereign power of nation-states to non-state actors and the proliferation of disciplinary micro-powers. Consistent with this discussion would be the displacement of power toward those actors most proficient at controlling and manipulating informational resources. As is already evident in the global warming debates, the implications of this trend for relations between information-rich and information-poor nations are both intriguing and unsettling. In any case, it is not clear that politics in a "post-industrial" world is likely to be any less conflictual than in the past.

7.1 A structural Approach to Regime Formation

Both the Montreal Protocol and the European Community (EC) acid rain agreement are examples of new pollution control regimes; the IPCC process represents a global effort to forge a climate change regime. Using Stephen Krasner's oft-quoted definition, regimes are "sets of implicit or explicit principles, norms, rules, and decision-making procedures around which actors' expectations converge" [Krasner 1983: 2]. In each of the above, that there is little doubt as to whether or not a regime exists.

Structural realism, or neo-realism as it is sometimes called, has relatively little to say about regime creation in general, and even less about the environmental regimes in question here. According to Kenneth Waltz [1979], the structure of the international system shapes both the most significant aspects of nations' interests, as well as the outcomes of their interactions. The distribution of capabilities -- in particular, military capabilities -- is the key element of structure. Yet military force seems quite irrelevant to problems like preserving the ozone layer and stabilising the global climate. The presumed bipolarity of the international system, which in itself is a questionable assumption tor recent years, tells us little national interests or bargaining outcomes for the problems of ozone depletion, acid rain, and global warming. Beyond Leonid Brezhnev's rhetorical challenge to the EEC, which led to the 1979 acid rain convention, the Soviet Union played only a minor role in any of the three issues.

Perhaps the structure of the international system has been hegemonic rather than bipolar. There are two classes of hegemonic structural explanation for international regime construction which dominate the literature, neither of which does a good job of explaining either why specific regimes were negotiated for CFCs (or acid rain), or why no agreement has been reached to control greenhouse gases. The first sees regimes as provided by a hegemonic power acting altruistically to supply a public good, and the second maintains that regimes are imposed upon reflecting the dominant power's narrowly defined interests [1]. Both versions of hegemonic stability theory associate regime creation with the existence of one dominant nation. Yet the recent period of intensive environmental regime-building, beginning in the early 1970's, coincides with what most observers regard as a period of dwindling US hegemony and a diffusion of power [Kennedy 1987, 1992; Rosencrance 1986; Keohane 1984]. Admittedly, the theory does not maintain that great powers are sufficient for international collaboration, yet the timing of the recent increase in environmental regime formation suggests that they are not even necessary.

Keohane and Nye have proposed a more disaggregated "issue-structural" model of hegemonic stability theory that applies under conditions of "complex interdependence" [1977: 50-52]. For certain issue areas, they argue, military force is beside the point; the line between international and domestic politics is blurred, and trans-national actors are as important as nation-states. The distribution of capabilities, or the structure of a sub-system, can only be determined for a specific issue area, and this may differ from the overall structure of the international system. For instance, although Brazil is a minor power in the international system as a whole, it is a dominant actor in the issue-area of deforestation. Because international environmental problems are often characterised by the dynamics of complex interdependence, it make sense to apply Keohane and Nye's model. Yet one is immediately confronted with a mix of theoretical and empirical obstacles. Even within a narrow issue-area, there is no unequivocal method of gauging the distribution of power. Taking the case of the Montreal Protocol, one could argue that, since the US and EC each account for about 30% of the CFC market, there was no single dominant power. With a rough balance of power, hegemonic stability theory would correctly predict that no significant agreement would have been reached. Turning to the global warming problem, the theory fares a little better since it would suggest that, as the single largest emitter of greenhouse gases, the US could successfully veto efforts to adopt global control measures. But the theory cannot explain *why* the US would pursue such a policy.

A different tack would be to argue that because most research on stratospheric ozone was generated in the US, there was indeed a "scientific hegemon." This would seem to account for American predominance in the technical debates. Clearly, US dominance shaped the outcome of the negotiations; without the US pushing for a virtual phase-out of CFCs, there would have been no reason to move beyond the freeze or the cap on production capacity favoured by the EC. The two other major CFC producers, Japan and the Soviet Union, were at least as reluctant as the EC to adopt stringent controls. With only the Scandinavians backing a strong treaty, little if any progress would have been made.

The "scientific hegemon" argument, however, falls apart for the global warming issue. While US predominance on climate change research is not as overwhelming as it is in the ozone sciences, it is still quite pronounced. Yet the US policy positions on the two issues have been dramatically different, despite unprecedented unanimity among scientists for reducing greenhouse gas emissions. At the core of these difficulties lies the failure of structural approaches to examine the nature and constitution of state interests. Once a nation's interests are determined, of course it will use its resources, technical and otherwise, to further its perceived interests. The truly engaging question, however, is how those interests are constituted -- a question about which structural theories are essentially mute. As Haggard and Simmons observe, "structural theories must continually revert in an ad hoc way to domestic political variables" [1987: 501]. And since information and ideas generally enter into the policy making process via domestic politics, this means that structural theories tend to neglect these factors as well.

The failure of structural approaches to consider the nature and origins of national interests is rooted in their ties to a broader "choice-theoretic" tradition. Once the state is accepted as a rational, unitary actor, many of the most significant political questions are swept under the carpet. Cooperation in the anarchical international system is thought to be possible only in a world of positive sum interactions

[Nye 1985], but the preconditions for such structures are never examined. As Wenat and Duvall argue,

> though neo-realism's choice-theoretic approach permits an abstract analysis of the potential for cooperation in various kinds of strategic situations, it provides little insight into the determinants of those situations, and therefore into the question of how often or under what conditions international cooperation will obtain. [1989: 56]

Yet the evidence on environmental bargaining suggests that actors' interests, and thus the structure of the games they play, are shaped by the knowledge they accept. Because of the flaws in their underlying assumptions, choice-theoretic approaches routinely disregard the impact of knowledge on behaviour. The assumption that preferences are stable and can be neatly ordered conflicts with the many cases where preferences, not to mention the real interests underlying those preferences, are at issue. And the empirical prevalence of uncertainty in many instances subverts the assumption of perfect information. Not only can uncertainty confound efforts to assign probabilities to alternative ends and means, but it can even hinder agreement on what the proper ends and means should be [Rothstein 1984: 734-735].

The shortcomings of structural approaches have spawned a great deal of research on domestic sources in international politics [Gourevitch 1986; Katzenstein 1978, 1985; Haggard and Simmons 1987]. At the core of these analyses is a question asked by Arnold Wolfers three decades ago: "When in the name of analytic parsimony is it justifiable to disregard the impact of the internal workings of the state on foreign policy behavior?" [1962: 4; quoted in Potter 1980: 410]. Zimmerman, elaborating upon Wolfers' own conclusions, claims that only two types of issue are likely to be amenable to a structural approach which treats the state as a rational unitary actor: those which threaten vital national interests and those which have little impact on domestic interest groups and bureaucracies [Zimmerman 1967]. Theodore Lowi [1967] labels these the "poles of power and indifference."

As it turns out, however, relatively few issues fall into either of these categories; the vast majority fall somewhere in between the two poles. International crises, particularly those involving military threats, are most likely to fall under the "pole of power," yet empirical research indicates that even these "most likely" decision making situations often are not amenable to unitary rational actor analysis. Putnam observes that of the many crises studied by Snyder and Diesing [1977], in fully half of them top decision-makers were not unified, so that an analysis of internal bargaining would be necessary for understanding even those national decisions at "the pole of power" [Putnam 1988: 435]. Thus, the value of a structural choice-theoretic approach is questionable even in those cases where it would be most likely considered to apply.

7.2 Domestic sources of Foreign Environmental Policy

Taking the state as a 'black box,' however, is far less likely to be helpful in the analysis of international environmental problems, which are among the vast majority of issues lying somewhere between the poles of power and indifference. Moreover, because international environmental agreements entail new regulations by the national governments implementing them, they inevitably affect actors at the domestic level. Putnam characterises diplomatic negotiations as "two-level games," with bargaining among the negotiators at Level I to reach a tentative agreement, and bargaining within each delegation and among its constituents at Level II about whether to ratify and implement the agreement [1988: 436]. As the negotiations on ozone, acid rain, and global warming reveal, bargaining at the two levels occurs simultaneously, not sequentially.

If the blurring of the distinction between domestic and international politics is a key feature of complex interdependence, then global environmental problems not only partake of "complex interdependence" but they foster it. As Harold and Margaret Sprout argued at the dawn of the environmental movement, several years before Keohane and Nye coined the term, a principal ramification of environmental problems is the "progressive convergence of domestic and external politics" [1971: 10]. The two traditional ways of looking at the impact of domestic actors on foreign policy are the bureaucratic politics and interest group approaches, the first being state-centred and the second society-centred. Clearly, both have something to contribute to an understanding of international environmental politics. Among the primary actors in every case are national agencies, various industries, and environmental pressure groups.

Graham Allison, the chief architect of the bureaucratic politics model, claims that actors' policy stances are largely determined by their organisational post; or, as the truism goes, "where you stand depends upon where you sit" [Allison 1971: 176]. Rather than being the result of rational analysis by a unitary governmental actor, national decisions are the outcome of internal wrangling among competing agencies with conflicting interests. The detailed discussions above of the US policy making process on the ozone issue confirm the importance of inter-governmental debates based upon bureaucratic interests -- and are just as relevant to Australia as they are to the US, or, indeed, any other geopolitical context.. Intense struggles in most cases find their way up to the national cabinet level, with many of the competing agencies taking fairly predictable positions.

EPA, for instance, advocated strong control measures in both ozone and global warming cases, measures which would have enhanced its own power as a regulatory agency. The State Department's OES office took a similar position, which can be interpreted as an attempt to raise its status from an obscure outpost to a potent diplomatic force. Because regulations are often expensive in the short run, OMB's opposition to them in both cases was consistent with its role as guardian of the government's purse strings. The Department of Energy opposed CFC reductions because insulating foams made with CFCs had become an important component of the agency's energy conservation program, a program which had been institutionalised since the mid-1970's. The Pentagon opposed regulation of the halons because they were necessary for a certain type of fire extinguishing equipment. As the primary governmental advocate of US industry, the Commerce Department predictably opposed a stringent Montreal Protocol.

Important dimensions of the decision-making process in most countries are also anticipated by a bureaucratic politics approach. As such an approach would predict, the principal ministries advocating a strong Montreal Protocol were the environmental agencies, while those associated with the interests of industry were most likely to oppose strong measures. In those countries where the industry-related agencies were most powerful -- Britain, France, Japan -- the governments were slowest to back stringent reductions of CFC emissions. Not only are the positions of most national bureaucracies largely predictable , but often so are those of the international organisation involved in the process. The global status of UNEP, not to mention its budget, would be expected to increase with a strong Protocol. Moreover, many of these same patterns of bureaucratic interest established during the Montreal negotiations have carried over to climate change debates [Hiskes 1986].

While the overt power of an agency, which is largely a function of budget and other material resources, is an important indicator of its influence in the policy process, another source of influence is its access to and ability to manipulate information. The bureaucratic politics model also suggests that bureaucracies can shape foreign policy by virtue of their ability to select the information that is given to top political leaders [Art 1973: 467]. Indeed, this was an important dynamic in the case of the Montreal Protocol, where a group of "knowledge brokers" associated with EPA was able to control, frame and interpret much of the information disseminated both to domestic and foreign decision makers. This dynamic may also have been at work in the Department of Energy's ability to shape US "comprehensive approach" to global warming. In this latter case, DOE's dominance over EPA also appears to be related to its sheer size and its superior access to resources.

But the bureaucratic politics approach leaves some important questions unanswered, each of which points to the distinctive role of knowledge and beliefs in formulating policy. it cannot explain why the influence of particular agencies waxes and wanes over time. Why, for instance, was EPA, a relatively small US agency, able to dominate the ozone policy process, over regulating greenhouse gases? Moreover, as an exercise in comparative statics, the bureaucratic politics model has little to say about why the positions adopted by a particular agency vary over time. The model cannot account for the EPA's dramatic shift regarding international regulation of CFCs from the early 1980's to what it became by 1986, a shift which occurred primarily because of the different attitudes toward risk-taking of EPA Administrators Anne Gorsuch and Lee Thomas. Likewise, if where one stands is determined by where one sits, then environmental ministries in disparate countries would be expected to adopt roughly similar positions. That this was not the case can only be explained in terms of the different beliefs espoused by officials in the various agencies.

As Robert Art argues, "mind-sets" are at least as important as bureaucratic position in determining actors' policy stances [Art 1973: 470]. The differences between, say, a Gorsuch and a Thomas, cannot be attributed to bureaucratic position. Along the same lines, in many cases the preferences of certain officials could not be determined by their positions. Why should Donald Hodel, Secretary of the Interior, have been so vehemently opposed to regulating CFCs? Or why should the US Energy Department have resisted CFC reductions on energy conservation grounds, yet later have rejected conservation measures recommended by scientists to mitigate global warming? Nor is there an obvious bureaucratic explanation for why, for instance, French environmental officials differed so widely from their Scandinavian counterparts. To answer these questions, one must consider cognitive factors.

The bureaucratic politics model ignores the distinctive role of knowledge; persuasive ability is not determined simply by institutional position, but also by the inherent power of alternative discursive practices. EPA's ability to prevail during the interagency review process on ozone depletion was undeniably due more to its discursive competence than to any conventional measure of bureaucratic prowess. In those instances in which scientists injected themselves into the policy process by making specific recommendations, their influence was more a function of their persuasive ability and their status as authoritative experts than of their bureaucratic position. For instance, as the bureaucratic politics approach would anticipate, of the two principal American scientific advisers during the Montreal Protocol negotiations, Daniel Albritton of NOAA, an agency of the Department of Commerce, was marginally more sceptical of the CFC-ozone hypothesis than was Robert Watson of NASA. Yet, in the end, their positions were comparable; neither scientist made explicit recommendations to phase out CFCs. Moreover, some scientists affiliated with NOAA, like Brasseur, did back CFC reductions, despite the position taken by NOAA's parent department. Those few scientists who publicly advocated a phase-out of CFCs, such as Rowland and Molina, were academic scientists and had no bureaucratic axe to grind.

Similar patterns are observable in the acid rain and global warming controversies. The German scientists who sounded the alarm on forest dieback, and the scientists who predicted severe societal disruptions from global climate change, had no obvious bureaucratic interest in those positions [Regens and Rycroft 1988; Postel 1984; Ulrich 1979]. Thus, the insights generated by a bureaucratic politics approach to such issues are overshadowed by the model's inability to consider the distinctive role of knowledge in shaping behaviour and outcomes.

Suffering from the same shortcomings, an interest group approach has even less to offer. In the Montreal case study, interest groups were important actors, but at no time were the results determined simply by bargaining among those groups. Rather, expert knowledge and specific modes of framing that knowledge were critical mediators of the ultimate outcomes. The main antagonists in every case were industry and environmental pressure groups, with industry being the more overtly powerful of the two. Yet the agreements regulating CFCs and sulphur dioxide emissions were antithetical to industry's interests, and were actively opposed by industry. The power of scientific knowledge is evident in Du Pont's decision to move beyond the Montreal Protocol after the release of NASA's Ozone Trends report, unilaterally embracing a full phaseout of CFCs, a decision that cannot be explained solely on the basis of self-interest.

Because of the prominence of interest groups in the acid rain controversy, one might be tempted to portray it as a contest between industry and environmentalists. Industry, particularly the coal industry, was a strong and well-organised force during the acid rain debates, and for a time successfully drew upon the scientific uncertainties to frame knowledge in support of its own interests. That strategy, however, was less successful as new scientific information became available. In Europe, the turning point seems to have been Berend Ulrich's findings in 1979, and their later confirmation by the West German government, of massive forest decline. In North America, the shift came more gradually and several years later, beginning with the National Academy of Sciences report in 1983. As a corollary, citizens' groups like the Canadian Coalition on Acid Rain and the Clean Air Coalition in the US eventually became powerful interest groups, but their influence was tied more to the emergence of specific scientific information than to their ability to mobilise prodigious material resources. Clearly, by conventional measures of power, their influence should have been minimal in comparison to that of industry; their power was greatly magnified by the growing acceptance of particular scientific discourses.

Environmentalists were not particularly vocal in either North America or Europe during the Montreal Protocol negotiations, so that agreement cannot be explained as the result of their efforts **[2]**. The two instances where environmentalists seem to have made a difference were the NRDC lawsuit in the US and the rash of victories by the German Greens in the mid 1980's. In the latter case, while environmentalists were not overtly involved in bargaining for CFC reductions, they may have been instrumental in shifting the political climate in West Germany, and hence in the EC. The former case is probably the strongest instance of an environmental organisation having a direct impact on national behaviour, for without the threat of legal action the EPA may have been less likely to push for strong controls internationally.

Yet, the NRDC's influence was substantially related to its ability to frame and manipulate information. There were two important instances of this phenomenon. The first was its proposal, submitted at the UNEP/EPA economic workshop in Leesburg, Virginia, for an 85% reduction of CFCs, a stance which was based upon an idiosyncratic, but convincing, method of framing the available knowledge. The second instance, the press conference called to ridicule Donald Holdel's "personal protection plan," pointed out the pitfalls of framing the issue narrowly in terms of ozone depletion and skin cancer. These events are not unique; environmental groups frequently rest their claims to legitimacy on the technical rationality of their arguments, rather than on the appeal of their ideals or their ability to mobilise resources [Lowe and Goydzr 1983: 127]. Thus, without taking into account the political implications of scientific knowledge, an interest group approach alone is not particularly helpful in understanding the Montreal Protocol process.

The most vocal group in promoting a global climate change agreement has been scientists, yet the unique role of knowledge in determining their views makes it difficult to characterise scientists as an interest group in any conventional sense of the term. Scientists as opposed to other groups, have nothing special to gain from an agreement to limit greenhouse gas emissions. They would, of course, benefit from increases in funding for research programs, but such advantages would accrue even if they were to argue successfully that regulation should be postponed until after the uncertainties are resolved. The widespread support among scientists for an international climate change agreement is a consequence of a combination of their expert knowledge and their attitudes toward risk-taking. As a whole, atmospheric and environmental scientists involved in the issue have concluded that current economic activities will drastically alter the planet's climate systems, and that it is unwise to follow such a path. Their conclusions do not reflect their membership in any specific interest group; in fact, their persuasive ability is a function of the common perception that they are 'objective' and do not belong to an interest group.

While the two traditional approaches to domestic sources of foreign policy, rooted in bureaucratic and interest group politics, generate some insights and explain some aspects of environmental regulatory negotiation, their advantages are offset by their inability to incorporate the distinctive dynamics of knowledge-based power. These shortcomings suggest that, because of the complex interplay between knowledge and interests, conventional pluralist models of the policy process should be amended for environmental decision making and other issue-areas in which knowledge is a key factor.

7.3 The Role of Ideas in International Policy Collaboration

The argument that knowledge can be an integral part of the policy process is a subset of the broader claim that ideas and other cognitive factors are important determinants of politics and history. Until recently, the dominant strains of thought in international relations scholarship have downplayed the role of cognitive factors, focusing instead on structural and material explanations of state behaviour. In general, ideas have been viewed both by realists and Marxists as epiphenomenal expressions of material interests [Carr 1964; Cox 1987]. A new trend in the literature, however, is emphasising the independent influence of ideas on foreign policy. In particular, a good deal of attention has been given to the impact of certain liberal ideas to US foreign economic policy. Judith Goldstein's work, for instance, examines the role of ideas in shaping US trade policy and the embeddedness of those ideas in national institutions [1988; 1989]. John Odell looks at the political influence of ideas on US monetary policy [1982]. Peter Hall has taken a more cross-national approach in his investigation into the broad political appeal of Keynesian economics [1989]. These works have generated important insights and have performed the invaluable service of reopening an important avenue of research.

Yet methodological problems inevitably arise when ideology and ideas are taken as independent variables in explaining policy outcomes. If, as the environmental policy cases examined above indicate, knowledge and interests can merge even when the natural sciences are involved, how much more pronounced is this entanglement likely to become when economic and political knowledge is implicated? Because the emphasis of these works is on "the political influence of the content of an idea, not the cognitive processes" [Goldstein 1988: 182], the political origins of ideas, which are frequently tied up in material interests, are ignored.

Indeed, implicit within any explication of the role of knowledge in international environmental policy making is a more general methodological argument. Any attempt to treat knowledge and political power as either a purely independent or a purely dependent variable would ultimately generate a false picture. Knowledge and power are best understood as *interdependent* variables. This is not to say that they are entirely indistinguishable, but only that the causal arrows should not be assumed to be neatly drawable in either direction. Thus, for instance, Stephen Krasner's observation that structuralist approaches to regime formation should be modified to include a possible role for knowledge as an intervening variable must itself be modified [1983: 195]. If accepted knowledge is often related to perceived interests, then the arrows must point both ways and the model gets much messier. There is nothing novel in these observations; they are consistent with a long tradition in the social sciences which explains events in terms of the interaction of material interests and ideas. Weber's *The Protestant Ethic and the Spirit of Capitalism*, which traces the influence of religious ideas on political and economic institutions, is a classic work in this tradition. Weber, despite his disagreements with Marx, did not believe that ideas were reducible to an independent variable. Rather, his position was that,

> Not ideas, but material and ideal interests, directly govern men's conduct. Yet frequently the "world images" that have been created by ideas, have, like switchmen, determined the tracks along which action has been pushed by the dynamic of interest [Weber 1958: 280].

Even Marx did not wholly deny the impact of ideas on historical processes. In *The German Ideology*, he discusses "the reciprocal action of intellectual and cultural forces with productive forces" [1976: 28]. Among the great social theorists, it would be difficult to find one who is either a pure materialist or a pure idealist. The weight of historical evidence runs contrary to either extreme, but points toward a more interactive approach to the question of ideas and interests. In as much as institutions "reflect a set of dominant ideas translated through legal mechanisms into formal governmental organisations" [Goldstein 1988: 181], the literature on ideas as an impetus for foreign policy is closely associated with recent institutional approaches. Consistent with the findings in the three cases examined above, "critical junctures" are "unanticipated and exogenous "events that drive institution-building" [Ikenberry 1988: 233]. The most commonly studied crises are depression and war, which tend to become "watershed events in states' institutional development" [Skowronek 1982: 10] by challenging the capacity of the old institutions to cope with the new situation. Crises essentially clear a space for new ideas to be considered [Moore 1950: 419].

Environmental crises like the discovery of the Antarctic ozone hole and the *Waldsterben* of Central Europe have spurred new national and international institution-building. By challenging old institutions, and hence old patterns of thinking, these crises stimulated a search for new explanations of and solutions to problems. A moment's reflection on the history of international environmental institutions suggests that disasters are frequently the prelude to new regimes. The 1986 Chernobyl disaster and the ensuing negotiations to update the nuclear accident regime under the International Atomic Energy Agency are a case in point [Young 1986]. Similarly, two giant oil spills in the late 1960's, one from the grounded Torrey Canyon off the British coast and the other from an oil well run amok near Santa Barbara, California, led to a complete revision of the international regimes governing marine pollution [McGonigle and Zacher 1979: 17].

The apparent importance of crises for catalysing environmental regime formation bodes poorly for those problems that are inherently more gradual and cumulative. This observation is particularly alarming in light of the fact that the damage associated with these problems is often irreversible. Consider the increasingly evident loss of global biodiversity. With a total of anywhere from four to ten million species in existence, the extinction of a few hundred species each year may be virtually unnoticeable [Myers 1979]. A related problem is the disappearance of the tropical rainforests, home to the bulk of the planet's vertebrate species [Hecht and Cockburn 1989]. Global warming is another creeping problem, one which is unlikely to produce a dramatic crisis until it is too late. A few hot summers may temporarily sound the alarm bells, but the return of apparently normal weather is likely to put the brakes on international regime construction. Moreover, large-scale events such as the volcanic eruption of Mt. Pinotubo may mask signals of global warming for the near future. The absence of a perceived crisis, therefore, may hinder the pursuit and acceptance of new ideas, as well as the process of institution building.

Ideas about environmental problems are rooted in two different categories of beliefs which are at least analytically separate: causal and normative beliefs. The former would include the belief that CFCs deplete the ozone layer, whereas the latter would include the belief that, in the face of scientific uncertainty, one should err on the side of caution [3]. *Scientific knowledge* about environmental problems is concerned with testable beliefs about causal relations, and the authority of scientists derives from their presumed expertise about causal relations in their specialised area of study. While most of the literature on the impact of economic ideas on foreign policy is about a mix of both categories of beliefs, there is also a developing literature on the impact of causal beliefs on international policy coordination. The notion of epistemic communities, or networks of experts who share knowledge about causal relations as well as a common set of normative beliefs about preferred policies, has been developed to explain international cooperation on technical issues. Under conditions of uncertainty, it is argued, and especially during crisis episodes, epistemic communities may become influential either through persuasion or by usurping decision-making channels [Peter Haas 1992b: 50]. The notion of epistemic communities is saturated with an Enlightenment faith that science transcends politics, that knowledge is divorced from political power. While proponents of epistemic community approaches are more careful than their functionalist predecessors to qualify their conclusions about the ability of science to make politics more rational and less conflict-ridden, ultimately the differences between them on this core issue are minimal.

Epistemic community approaches downplay -- almost to the point of neglect -- the ways in which scientific information simply rationalises or reinforces existing political conflicts. Just as interesting, and perhaps even the norm under the conditions of uncertainty so prevalent in environmental decision-making, may be *epistemic dissension*. While epistemic community approaches make an extremely important contribution in their claim that state interests are not given, but are formulated in light of prevailing knowledge claims, they provide only a partial, and therefore a distorted, picture of the policy process. Actors may revise their conceptions of their interests when presented with new information, but their openness to new information is itself a function of their perceived interests. Thus, knowledge and interests are mutually interactive, a possibility that epistemic community approaches miss in their depiction of a unidirectional relationship between them. **Figure 7.1** below contrasts the dynamics of epistemic cooperation with those of epistemic dissension.

Figure 7.1

Knowledge >> Interests >> Epistemic Cooperation

Knowledge > < Interests <> Epistemic Dissension

The power of epistemic communities allegedly rests on their privileged consensual knowledge, but both the nature and the role of consensual knowledge are ambiguous. It seems almost tautological to argue that a group of policy makers who agree on the facts are more likely to come to agreement on policies than a group who does not. More interesting -- because they are unexpected -- are those instances when consensual knowledge disintegrates under political pressure [Miles 1987: 37], or when ignorance, not knowledge, increases the chances of cooperation [Rothstein 1984: 750]. A body of consensual scientific knowledge existed in the ozone case, yet, almost until the end, the wide range of possible interpretations of that knowledge limited its influence.

Because of the prominent role of science and scientists in the ozone negotiations, as well as the existence of an authoritative document representing an international scientific consensus [WMO/NASA 1986], the Montreal Protocol process is a "most likely case" for an epistemic community approach. In fact, Peter Haas attempts to provide such an explanation [1992b] and, while paying less attention to the political dynamics of knowledge recognition than does Haas, Richard Benedick [1991] offers a very similar explanation. In both accounts, science and scientists are the key ingredients. Peter Haas claims that atmospheric scientists were key players in the epistemic community responsible for the Montreal Protocol. According to Benedick,

> First and foremost was the indispensable *role of science* in the ozone negotiations. . . . Scientists were drawn out of their laboratories and into the negotiation process [1991: 5; original emphasis].

Both accounts downplay the roles of interpretation and contingent events in determining the knowledge that was accepted by various actors. In emphasising the involvement of scientists, both accounts clash with the fact that most of the relevant atmospheric scientists felt that the evidence did *not* warrant a near phase-out of CFCs, a position which was advocated by the US and which shaped the course of the negotiations. Most of the scientists, in fact, steered away from making specific policy recommendations. Thus, beyond putting the issue on the agenda in the first place, scientists were not directly responsible for the ozone agreement. The "power of problem definition" is extremely unlikely to remain with the scientists throughout the policy process [Weingart 1982: 80].

Scientific knowledge, rather than the scientists themselves, was a key factor. First, the science was framed and interpreted by a group of ecologically-minded knowledge brokers associated with EPA and to a lesser extent, UNEP. Second, the context of the negotiations, defined in large part by the discovery of the Antarctic ozone hole, determined the political acceptability of various modes of framing the scientific knowledge. Both Benedick and Haas wrongly downplay the impact of the hole on the negotiations. No body of consensual knowledge, either from the computer models or from empirical observations, indicated at the time that major cuts in CFC emissions were necessary. Only one distinctive mode of framing the available knowledge supported such a conclusion, and that mode gained its credibility largely from the heightened sense of risk that accompanied the discoveries over Antarctica. The argument that CFC reductions of 85% would be required in order to simply stabilise atmospheric chlorine concentrations at 1985 levels did not rely on the atmospheric models. But since the ozone hole had proven the models to be faulty, alternative modes of framing the information were more readily accepted -- despite the negotiators' explicit agreement to ignore the hole in their deliberations. No significant new scientific knowledge was generated during the two years following the signing of the Vienna Convention in 1985, yet the CFC reductions agreed to in Montreal would have been inconceivable two years earlier. The only real difference between the two historical contexts was determined by the ozone hole. Scientists have been far more outspoken on policy choices for the global warming problem than they were for the ozone problem, yet prospects for an effective international regime in the near future are dim. Despite considerable scientific certainty on the broad outlines of climate prediction, many of the contending political actors, particularly oil-producing and developing countries, have seized on the uncertainties to bolster their perceived interests. As in the ozone case, knowledge is framed in light of interests, with exogenous factors shaping the political salience of various modes of interpreting that knowledge.

A key problem with the epistemic communities approach is in specifying how they exercise their power. If they influence policy by "usurping bureaucratic decision-making channels," as Peter Haas suggests, then this approach may be reducible to a bureaucratic politics approach. In that case, the actors' power has little or nothing to do with their persuasive ability or their privileged access to knowledge, but is explainable in terms of more conventional understandings of power. To the extent that their power is related to their ability to mobilise resources on behalf of their shared normative beliefs, epistemic communities may be indistinguishable from interest groups. Yet the purpose of introducing the concept was to explicate the unique functioning of knowledge-based sources of power. Finally, to the extent that the power of epistemic communities is rooted in the power of their modes of framing and interpreting sources of knowledge, then an approach that focused on discursive practices may be most appropriate. Such an approach would need to be highly sensitive to the interactive dynamics between knowledge and power.

7.4 Discursive Practices and International Regimes

The critique of the epistemic communities approach suggests that the relationship between science (and scientists) and policy (and policy-makers) is *multi-dimensional.* Scientists may come together in an epistemic community to influence the course of policy, but their power is circumscribed by a host of contextual factors. Policy-makers may coopt or manipulate the scientists, or they may simply ignore what the scientists have to say. Whether or not the voices of scientists are audible may depend upon seemingly extraneous contingencies beyond the control of either scientists or policy-makers. Furthermore, the scientists may deliberately refrain from addressing the policy implications of their research.

In problematising interests, the epistemic communities approach moves beyond the constraints of structural and choice theoretic approaches. And by bringing knowledge into the equation, it opens research vistas beyond the bureaucratic politics and interest group approaches. Yet ultimately, the epistemic communities approach overly simplifies the relationship between knowledge and power, or science and politics, reducing it to a uni-directional process of enlightenment leading to cooperation. An approach which recognises that knowledge and power are not neatly distinguishable -- either conceptually or in practice -- is more useful. An alternative approach might be to take *competing discourses*, rather than groups or individuals, as a starting point. As determinants of what can and cannot be thought, discourses delimit the range of policy options, thereby functioning as precursors to policy outcomes. In the Montreal Protocol process, the power of groups and individuals emanated from their ability to set the terms of discourse. It was not supremely important who these individuals were; their authority was often more a function of their ability to manipulate information than of their professional or bureaucratic credentials.

The post-structuralist emphasis on discourses calls into question the epistemic communities approach's focus on *agents*, without reverting to *structure* as the ultimate explanatory factor [Wendt 1987; Dessler 1989]. It recalls the notion that subjects are constituted by discourses. As Michael Shapiro et al argue, what is at issue here is an epistemological shift away from the standard rigid separation of subject and object (decision-maker and decision situation) toward a recognition that subjects are constituted by the discursive practices and historical contexts in which they are embedded [Shapiro et al 1988: 398].

It is important not to read into this epistemological shift the wholesale *elimination* of the subject, despite the language of some post-structuralists. Rather, what is entailed is the *decentring* of the subject, in Foucault's words, engendered by a refocussing of one's methodological lens onto the study of discursive practices rather than on the agents implicated in them. Just as power necessarily entails some degree of subjectivity, even if not in any form resembling a Kantian identity, so too do discursive practices require subjectivity. Discourses could not exist without individuals and groups promoting them, identifying with them, and even struggling with them.

The major contribution of a discursive practices approach is its ability to conceptualise power and knowledge as intricately related. The overarching regulation of the political field by codes, specifically linguistic codes, "transcends the generative and critical capacities of any individual speaker or speech act" [Terdiman 1985: 39]. The subject surrenders to the "law of the signifier" [Descombes 1980: 97]. The supreme power is the power to delineate the boundaries of thought -- an attribute not so much of specific agents as it is of discursive practices. What becomes important, then, is how certain discourses come to dominate the field and how other, more marginal "counter-discourses" [Terdiman 1985] establish networks of resistance within particular "power/knowledges" of "regimes of truth" [Foucault 1972: 223]. The power embodied in dominant discourses is more easily characterised in terms of Foucault's notion of "disciplinary power" than by more conventional notions of power. Discursive power is decentralised, non-monolithic, and rooted in linguistic practices rather than overt violence. All discourses, including hegemonic ones, are, in Mikhail Bakhtin's words, "heteroglot." They represent

> the co-existence of the socio-ideological contradictions between the present and the past, between differing epochs of the past, between different socio-groups in the present, between tendencies, schools, circles and so on. [Bakhtin 1981: 291; quoted in Terdiman 1985: 18-19]

Networks of resistance operate perpetually between the dominant discourses and subjugated knowledges. Yet because counter-discourses are always intertwined with the hegemony they oppose, the two stand in a necessary relation of "conflicted intimacy" [Terdiman 1985: 16].

Consider, for instance, how the discourse on ozone depletion was shaped, which is essentially the story of how a dominant anti-regulatory discourse was supplanted by a new regulatory discourse.

This discursive shift occurred both domestically within the US and internationally during the Montreal Protocol negotiations. The 1986 WMO/NASA report defined the scientific parameters of discourse. Yet the "heteroglot" nature of that document is evident in the wide range of policy stances claiming to be derived from it. EPA Administrator Lee Thomas described this phenomenon in noting the divergence between himself and Science Adviser William Graham: "We just looked at the same science and saw things very differently" [Thomas 1986: 62].

One of the key properties of discourses is their capacity to define how issues are connected within an issue area [Keeley 1990: 94]. During the ozone talks, the politically messy climate issue was essentially excluded from policy discourse, even though it was at the core of scientific discourse. Skin cancer was highlighted in the early years, but was downplayed later as it became necessary to attract the support of countries not concerned about skin cancer. Even the discourse of cost-benefit analysis, intended to bolster an anti-regulatory approach in the US, was used by the pro-regulatory forces to strengthen their own position. As an emerging counter-discourse came to dominate the field, the definition of permissible issue-linkages shifted. As argued earlier, although specific individuals and groups were associated with various positions, there is no easy method of correlating bureaucratic or professional identity with policy stance. Rather, what was important was the structure and content of discursive practices, a subject that is easily ignored with an overemphasis on the roles of specific agents. The fact that, for instance, John Hoffman and Michael Gibbs authored the particular discursive mode associated with the EPA knowledge brokers is interesting and noteworthy, but it says little about how that particular mode came gained its authoritative status. An approach, however, which takes discursive practices as central can say something meaningful about the legitimisation process.

More importantly, discourses define the menu of possible policy options. The rise of a particular counter-discourse to displace the discourse which dominated until 1986 was primarily shaped by the discovery of the Antarctic ozone hole -- despite the negotiators' explicit agreement to ignore it. By presenting the real possibility of unprecedented catastrophe, the hole shifted the terms of the debate in favour of those who wanted to err on the side of caution. Indeed, as I suggest, the hole changed the *meaning* of 'caution' altogether; the vulnerability of ecosystems suddenly became more salient than the vulnerability of CFC industries. More than any approach examined thus far, a discursive practices approach takes historical context seriously. Richard Terdiman's criticism of mainstream thinking about culture also holds for the dominant schools of thought in international relations theory:

> Context is thus marginalised. Treated as something like a background, it is refused status as a ground at all. Once seemingly acknowledged, it becomes increasingly diffuse, transparent, until -- like the Cheshire cat in *Alice* -- it simply disappears [Terdiman 1985: 16].

Once discursive practices, which constitute a complex web of disciplinary power and social relations, are brought to centre stage, the pervasive nature of context becomes evident. Translating Terdiman's observations about literary texts into a more useful form for political analysis, the policy process becomes marginalised by the context we had taken to frame it; "the frame comes unexpectedly to define the centre" [1985: 17]. Thus, a discursive practices approach would be very sceptical of an analysis of the Montreal Protocol process which downplayed the discovery of the Antarctic ozone hole on the grounds that the negotiators had agreed not to consider it. This is precisely the sort of decontextualised analysis offered by Richard Benedick and, to a lesser extent, Peter Haas. Yet even the newspapers of that period confirms that the Antarctic hole was at the forefront of public consciousness on the ozone issue. And a more in-depth investigation into the perceptions of key scientists and officials indicates that it also dominated their consciousness. Under such conditions, it would have been impossible for the hole's discovery not to influence the dominant discursive practices. Similarly, a decontextualised analysis which attempted to explain acid rain and global warming policy dynamics solely in terms of conventional political factors, even bringing in consensual knowledge, would be woefully inadequate. The turning point in international acid rain policy was Bernd Ulrich's discovery of massive forest death in Central Europe -- even though his observations were not conclusively linked to acid precipitation. Likewise, the recent flurry of activity on the global climate change problem was inspired by unusually severe weather conditions in Europe and North America. Particularly given the enormous scope of any policies to either mitigate global warming or adapt to it, progress is unlikely in the absence of severe weather conditions.

Initially, it might seem as if the argument that crises empower counter-discourses is little different from the institutionalist approach which holds that crises generate new ideas and, hence, institutional change. Clearly, there are significant areas of overlap. The key difference, though, is that, unlike the new institutionalism, the discursive approach goes beyond the institutional expressions of ideas and gets into their deeper *structure* and *content*, and how these are conditioned by context. This approach puts the focus on discursive practices, rather than on individuals or organisations, embedded in discourse, as opposed to the institutional approach, which tends to take ideas and power as separate independent variables. For Foucault, ideas and discourses are not free-floating entities [**4**];

> discursive practices are not purely and simply ways of producing discourse, they are embodied in technical processes, in institutions, in patterns for general behaviour, in forms for transmission and diffusion, and in pedagogical forms which, at once, impose and maintain them [1977: 200].

Such an approach provides an alternative to materialistic notions of power. A discursive practices approach offers insights into political process which go beyond those generated by a purely materialistic approach. Environmental crises, for instance, are not just physical phenomena, but they are *informational phenomena*. As one commentator puts it, problems like ozone depletion and global warming are "rumours" [Anderson 1990]. As soon as information appears, people make it into stories in order to render it meaningful. For instance, information about global warming has supported their deeply held beliefs [*In Context* 1989]. New information is co-opted by existing discursive practices, yet under certain conditions, counter-discourses can become predominant.

Putting the spotlight on discourse suggests it enriches these key concepts, just as it yields a more complex picture of power and social order. As the earlier discussion of hegemonic stability indicates, realists tend to define power narrowly in material terms and then explain regime formation in terms of the interests of the preponderant power. Much regime analysis, however, is rooted in more liberal assumptions about the international system; regimes are assumed to be communitarian, benevolent, voluntary, and cooperative. The conventional notion of regimes as "principles, norms, rules, and decision-making procedures around which actors' expectations converge" [Krasner 1983: 2] connotes ideas of freely shared judgments freely converging to a consensus [Keeley 1990: 83-85]. A post-structural approach to regimes, however, would offer a more variegated conception of power than that of the realists, and it would adopt a more sceptical stance to the problem of community and order than do those of a more liberal persuasion. A discursive practices approach regards individual international regimes as "localised power-knowledges," each providing an arena for contestation among contending discourses. In his defence of such an approach, James Keeley argues that,

> Adding knowledge to power and treating a regime as an implementation of both, this analysis goes *inside the regime* rather than treating it as a mere dependent variable. It is, therefore, better able to ask how regimes work and what they do and to incorporate cognitive issues into both its questions and its answers [Keeley 1990: 100; emphasis added].

Such an approach is particularly germane to the study of regimes in which the framing of information is decisive, as is the case in environmental regimes. It also would anticipate that regimes such as those instituted by the Montreal Protocol would become arenas for debate among competing "power/knowledges," as has been the case during the subsequent meetings to update the protocol.

In stating the advantages of a discursive approach, it is only fair to acknowledge its methodological shortcomings. As Keeley notes, a discursive approach can be good at answering "how" and "what" questions. It is not, however, good at providing parsimonious explanations [5]. And because of the central role it gives contingency and context, nor does a discursive approach make sweeping generalisations or offer precise predictions. What, then, is it good for besides telling a story well? First, many would see inherent value in a well-told story, particularly if the topic were related to planetary sustainability. But I would also argue that a well-told story can offer important insights into the policy process in general, and perhaps into future events as well. It can alert the analyst to certain misconceptions that might arise, and it can also alert the practitioner to alternative modes of discursive action. In the specific stories I have told, the insights generated suggest a multi-dimensional approach to the relationship between power and knowledge.

7.5 Implications for the International System and Planetary Politics

There are good reasons to anticipate that discursive power, particularly that associated with scientific knowledge, will become increasingly important in the future. As environmental problems become more serious and more global, which is virtually inevitable if present trends continue [World

Commission of Environment and Development 1988], then scientific knowledge can be expected to play an expanded international institution-building. Other developments in the international system as a whole are likely to reproduce and reinforce this pattern.

Commentaries in the scholarly literature about the declining utility of a related factor, the diminished fungibility of power, have become almost commonplace [1986; Baldwin 1985; Nye 1990; Rosenau 1990] [**6**]. National security, traditionally viewed in terms of military power, has become more complicated as the nature of the threats has shifted toward economic and environmental dangers [Nye 1990: 179] [**7**]. These developments are both causes and consequences of increased interdependence in the international system. Unprecedented complexity, the functional nature of many of the most significant contemporary issues, and the blurring of the lines between domestic and international politics have all enhanced the authority of non-state and transnational actors relative to the state [Keohane and Nye 1977]. The simultaneous decrease in the usefulness of military power as an instrument of foreign policy and the new prominence of non-state actors has cleared a space for knowledge-based power. The proliferation of information which has accompanied the recent "post-industrial" revolutions in computers and communications technology has rushed in to fill that space.

James Rosenau has aptly applied the term *"turbulence"*, defined as extraordinarily high degrees of complexity and dynamism among the system's actors, to these conditions [Rosenau 1990: 65]. He suggests that international politics under post-industrial conditions, or "post-international politics," is increasingly bifurcated, with traditional state actors on the one side and a proliferation of non-state actors, including corporations, social movements, experts and international organisations, on the other. Not only are the actors changing, but so too is the nature of power. Rosenau believes that, because "scientific proof" is becoming elevated as a major political tool, "the tendency to contest issues with alternative proofs seems likely to grow as a central feature of world politics" [Rosenau 1990: 203]. He explains this development as a consequence of the growing analytical competence of citizens everywhere; the enormous expansion of available information; and its relative cheapness in terms of resources expended. Control techniques based upon scientific proof are less costly than more coercive techniques because less material and personpower are required for their effectiveness. Rosenau notes that,

> When proof evokes compliance, moreover, those whose behaviour has been modified are unlikely to carry resentments forward to the next situation, whereas those coerced are likely to approach the next set of control relations with animosity and an inclination to resist [Rosenau 1990: 185].

Joseph Nye's analysis echoes many of Rosenau's observations. He suggests that post-industrial world politics are distinguished by a "diffusion of power" away from state actors to non-state actors [Nye 1990: 20]. Nye also sees a shift away from traditional notions of power, which he labels "hard" or "command" power to "softer" forms of "cooptive" power. The former entails influencing other states' behaviour either through inducements or threats, whereas the latter rests on the attractiveness of one's ideas or on the ability to set the political agenda [Nye 1990: 31-32]. While Nye is primarily interested in cultural and ideological expressions of cooptive power, clearly his conception is broad enough to include power based upon scientific proof. John Ruggie has argued that during periods of systemic transformation, the key question about power shifts from how power is distributed to the question of, "Who has the right to act as a power?" [Ruggie 1975: 558]. I would add to Ruggie's question that systemic transformation may also entail a shift in the *nature of power*, e.g., from sovereign to disciplinary power.

If the evidence is convincing that, indeed, the contemporary system is moving toward a diffusion of power to non-state actors, then we may be witnessing a systemic transformation. Yet that transformation need not include the demise or transcendence of the nation-state, advocated by visionaries from the post-war world government movement to the functionalism of the 1970's [Wooley 1988]. Rather, the evidence points to an emergent trend along the lines of Rosenau's bifurcationist model multi-centric actors, a trend that stems from the growing realisation that the nation-state has become "both indispensable and inadequate" [Rosenau 1990: 249].

Recent international institution-building to cope with environmental problems supports Rosenau's argument. On the one hand, global ecological interdependence could usher in a new era of governmental activism around a new set of welfare concerns, thereby reversing the recent trend in many places toward reducing the role of government in people's lives. Since major environmental problems will not be resolved by the invisible hand of the market, it can be argued, the state will find itself engaged in "an unprecedented degree of domestic intervention and international cooperation" [Maynes 1988]. Only the state has the human and financial resources to mount the large-scale scientific and technical projects for detecting, monitoring and preserving the global environment. Only the state, standing at the intersection of domestic and international politics, has sufficient authority, political legitimacy, and territorial control to influence the myriad causal agents of environmental deterioration. Thus, global ecological problems might be expected to bolster the power and legitimacy of the state.

On the other hand, this argument suggests that states and markets are the only institutional mechanisms available. It ignores the fact that the knowledge-based nature of environmental problems has opened up the playing field to a whole array of non-state actors. In particular, scientists and social movements have instigated virtually all existing international environmental agreements, and in many cases were key actors in their negotiation, implementation and monitoring. Moreover, these non-state actors are infusing new rules, processes and norms into both new and existing social structures. Ultimately, these developments may express a shift in fundamental social values and worldviews away from the dominant industrial paradigm toward a more holistic, long-term and global ecological paradigm [Pirages 1991]. This paradigm shift would arise from the structural contradiction between Earth, an integrated system, and the nation-state system, based upon the principles of sovereignty and territorial exclusivity. The implications of such a shift for international relations would be dramatic, generating new identities, roles and interests to challenge the nation-state system.

The three cases examined above, which represent some of the most innovative and comprehensive attempts at building international environmental regimes, suggest that both arguments are plausible, which in turn lends credence to Rosenau's bifurcated image of "the two worlds of world politics." Clearly, both the dominant actors in the debates and the actual parties to the treaties are nation-states. Yet the debates themselves would have been unthinkable in the absence of particular non-state actors, including industries, scientists and environmental organisations. States seem to be caught in the midst of a tug-of-war as non-state actors spar over how state interests will be determined, and how states will regulate other non-state actors. Whatever the actual content of the debates, however, the case study examined above suggests that *the terms of the debate will be based on current scientific knowledge.*

Yet the prevalence of scientific discourse should not delude us into the common misconception that politics will therefore become more rational and less conflict-ridden, whether through functional cooperation or epistemic communities. The Enlightenment faith in science dies hard, however, even for those who foresee turbulence as the dominant mode of the future. Consider how Rosenau's observation that "the enormous increase in the availability of information has accentuated the practice of seeking knowledge before making decisions" [Zuckerman 1986: 341; quoted on Rosenau 1990: 31-33] moves toward the optimistic conclusion that "the science of muddling through may well give way to the science of *modelling* through" [Rosenau 1990: 324]. Rosenau discerns within the rise of scientific proof in international politics a nascent "global culture" [1990: 121-22]. Yet the three cases examined in this thesis suggest that, in as much as the language of science permeates political debates, as often as not it may serve to verbalise or rationalise existing interests and conflicts. One must wonder how great the difference really is between muddling and modelling.

A profusion of information may, in fact, lead to greater confusion as the world becomes a ubiquitous market for discourses. Frederick Jameson's observation rings disturbingly true: "No society has ever been quite so mystified as our own, saturated as it is with messages and information" [quoted in Terdiman 1985: 46]. Ironically, the gigantic volume of environmental data generated in the past two decades by a host of monitoring and research programs can hinder, rather than facilitate, the process of environmental management [Gardner 1972]. Both politically and epistemologically, the process of delegitimisation is fuelled by the demand for legitimisation itself [Lyotard 1984: 39]. Yet as the global warming debates have revealed so starkly, the distribution of knowledge is uneven. The proliferation of information characteristic of post-industrial society is occurring far more rapidly in the North than in the South. For example, about 95% of the world's production of chemical knowledge in 1980 was available in only six languages, and two of these, English and Russian, accounted for over 82% [Laponce 1987: 198, quoted in Rosenau 1990: 427-428]. As one analyst of trends in environmental information argues, "The information rich will become richer, and the information-poor will not even suspect what they have missed" [Davis 1974: 27].

The equation, however, is not so simple: the information-poor *are* beginning to suspect and they are likely to compensate for their deficiencies in surprising ways. Most obviously, while scientific knowledge can be an important political resource, it is quite different from standard material resources. Access to knowledge does not necessarily bestow political influence merely because *persuasion* is an essential ingredient in the process. Knowledge is only a useful tool when others are convinced of its validity or, more precisely, of the validity of one's own interpretations of it. Such cognitive processes are not easily predictable, and are shaped significantly by seemingly serendipitous events like the weather or the Antarctic ozone hole. All of this suggests a distinctive "power of the weak" phenomenon: those without access to informational resources may simply refuse to be persuaded to act.

A core issue, then, is whether the apparent "scientisation of politics" observed by Rosenau and proponents of the epistemic communities approach, among others, is not really the "politicisation of science" [Weingart 1982: 73]. The observation that "the language of science is becoming a world view that penetrates politics everywhere" [Ernst Haas 1989: 46], while intended to support the former, is actually consistent with either trend; old cleavages may simply be recloaked in new scientific garb. Indeed, the cases above suggest that epistemic dissension is at least as likely an outcome as epistemic cooperation. In this sense, there are both hopeful signs and causes for concern in each of the three cases discussed above. The celebratory mood that has surrounded the Montreal Protocol must be tempered with the recognition that it took thirteen years for this "sudden global emergency" [Roan 1989] to be addressed. Mostafa Tolba and Richard Benedick are only partially right in upholding the agreement as a "preventive" one, "unique because it seeks to anticipate and manage a world problem before it becomes an irreversible crisis" [Benedick 1991: 1-2: UNEP Press Statement, 22 September 1987]. The ozone protocol was too late to prevent the Antarctic ozone hole, and probably would not have contained the provisions that it did without the hole. Even with the existence of a single document representing an international scientific consensus [WMO/NASA 1986], epistemic dissension was the rule throughout most of the bargaining process. Even in this "most likely case", there was no clear path leading from enlightenment to cooperation.

The acid rain case is also a hopeful case in that there has been international cooperation to regulate sulphur dioxide emissions, despite uncertainty about the chemical's links to the decline of central Europe's forests. As in the ozone case, action was taken to prevent irreversible damage, despite scientific uncertainty. But in this case as well, action was only taken fifteen years after the initial Swedish discovery linking acid rain to dying aquatic ecosystems, and even then the resulting policy was not linked closely to the scientific consensus that emerged from the Swedish research. Again, there was no clear passage from knowledge to action and, without a perceived crisis, international cooperation would have been unlikely. The global climate change debates provide the strongest case against an epistemic cooperation approach. Despite virtual unanimity among scientists that drastic policy changes are in order, and despite an unprecedented degree of activism by scientists as a whole on an international environmental problem, international action to significantly regulate greenhouse gases has not been forthcoming.

These cases raise the larger question of what role scientific knowledge can be expected to play in developing environmentally responsible policies. Conventional wisdom among environmentalists has been that scientific knowledge communicated to people and their political leaders will lead to ecologically sustainable societies; education engenders action. Faith in the power of science runs deep.

There are, however, two arguments -- based upon two different views of science -- against this position. The first, put forward by Lynton Caldwell, is that because science deals with the world of (overt) 'facts', not (overt) 'values', and values are ultimately what inform our actions, we cannot expect science to save us. Rather, what is required is a reorientation of fundamental values regarding human relationships with the biosphere, whether through political, ethical or religious movements [Caldwell 1985]. The second argument rejects the premise that science and politics, facts and values, are wholly divorced from one another. On this view, science can promote sustainable policies when it is used as a political tool and framed in ways that enhance environmental preservation, as was the case when a group of knowledge brokers from EPA and UNEP deftly defined the terms of the CFC-ozone debate. Paradoxically, the more sceptical view of science gives it more power; the key questions on this view then become *how* it is framed, by *whom*, and on behalf of which *interests*. While Caldwell may be correct in claiming that values motivate action, the truly interesting issue is how scientific knowledge and values interact to inspire action.

The political impact of scientific knowledge is determined far more by its incorporation into various discursive practices than by either its validity or the degree to which it is accepted by scientists. Scientific knowledge, then, is not likely to save us from environmental ruin. But, as Caldwell observes, politics might.

ENDNOTES

CHAPTER ONE NOTES

1 While recognising the ambiguities of this term, I use it both for want of a better one and as a way of placing this thesis in the context of an existing literature. In my view, "post-industrialism" does not entail the end of industrial society; indeed, the two exist side by side and in varying proportions around the globe. Rather, the term refers to a postwar shift in the economic structures of advanced industrialised countries towards a greater prominence of informational, as opposed to industrial, modes of exchange. My argument regarding the political implications of this shift is spelled out in Chapter Three.

2 The question of case study methodology is taken up at greater length in the last section of this chapter.

3 Some realists have taken a much more sceptical view regarding the ability of science to make politics more rational. A classic statement to this effect is Morganthau's *Scientific Man versus Power Politics* [1946]. The argument presented here is similar to Morgenthau's, with one important distinction. In Morgenthau's account, knowledge and power are so thoroughly divorced that science cannot penetrate the world of politics. In this thesis it is argued that knowledge and power are interactive, so that the pure, objective facts that allegedly serve as the grounds for political consensus in some accounts simply do not exist.

4 Most-likely case studies, a special instance of crucial case studies, are especially tailored to invalidating a hypothesis. The logic is that if a case is most likely to fit a theory and does not, then the theory is probably wrong. Conversely, least-likely case studies, the other form of crucial-case study, are tailored to confirmation. Crucial-case studies are the most apt to produce generaliseable conclusions. At the other extreme, configurative-idiographic case studies are descriptive explanations of unique cases couched in idiosyncratic terms [Eckstein 1974; Holt 1962; Verba 1976; George 1979].

5 A further factor which compels the use of qualitative rather than quantitative methods is the conspicuous scarcity of available cases in international environmental policy coordination.

CHAPTER TWO NOTES

1 I reject those teleological and Neo-Darwinist approaches which hold that structures have intentions, for only agents act intentionally. However, structures can exert power by causing effects, as do natural forces like gravity, but this does not require intentionality. And, while agents act intentionally, the consequences of their actions are frequently unintended. We may speak of collective intentionality, as in "The US decided to ban CFCs," but this is really a shorthand description of decisions made by individual agents [Giddens 1984: 213-221].

2 In a sense, the dichotomy between knowledge-based power and other forms of power is an artificial one. If power is taken as the production of intended effects, then agents must know how to produce those effects. Even producing unintended effects usually entails some knowledge of social conventions. Nonetheless, some forms of power are based less on material factors and are more thoroughly dependent on manipulation of information. The view of power as the production of effects by autonomous agents is eroded with the recognition that subjectivity, identity and interests are themselves generated through discursive practices; this is particularly true in "post-industrial" society.

3 The behaviourists' belief that interest can be discerned from behaviour has been roundly criticised for both its mechanistic conception of causality and its neglect of intentionality. See Ball 1975; 1976; 1978; Clegg 1979; and Isaac 1987.

4 Some major works in the debate include Dahl 1957; Bachrach and Baratz 1970; Lukes 1974; Gaventa 1980; Stone 1980; and Waste 1989.

5 I do not mean to imply a dichotomy between malevolent power based upon force versus the benevolent power of persuasion. The belief that force can be used for self-defence or liberation underpins the 'just war' tradition. Likewise, one can be persuaded deceitfully and perniciously to revise one's conception of one's interests, as was Othello by Iago. Nonetheless, one can also be edified about the nature of one's interests. A comprehensive conception of power should encompass all of these situations.

6 Important exceptions to this are certain strands of functionalism and neo-functionalism.

7 Legitimacy, however, should not be confused with consent. Marxists fault the social contract tradition for this error, arguing that workers do not consent to capitalism, though they may, out of false consciousness, consider it legitimise.

8 Because I have rejected the notion that structures have intentions, I should clarify that, properly speaking, only people have intentions, but those intentions, as well as interests, are shaped by their membership of certain groups. See fn. **1**.

9 Along these lines, knowledge-based power has enabled a host of non-state actors to participate meaningfully in international environmental politics.

10 Foucault's work is greatly influenced by the nihilism of Friedrich Nietzsche, who saw behind the scientific"will to truth" a broader "will to power". Explicitly linking power and knowledge, Nietzsche argued that "we gain knowledge about the power and effects [of nature] so as to . . . make us masters and proprietors of nature" [1979: 135].

11 Structuration theory occupies a middle ground between voluntarism and determinism through its analysis of the mutual embeddedness of social agents and structures. In a related debate, quantum theory is invoked to refute mechanistic views of social life, with *indeterminacy* rather than human choice opposite determinism. The important role of chance in human affairs has been recognised from the ancient Greeks to Machiavelli. For one philosopher's attempt to outline for the social sciences something intermediate between randomness and determinism, see Popper 1972. For an application of Popper's thoughts to political science, see Almond and Genco 1976.

12 Other writings on structuration theory include Bhaskar 1979; Layder 1981; and Bourdieu 1977. For critiques of Giddens, see Barbalet 1987; Clegg 1989: 135-148; and the Spring 1982 issue of *Theory, Culture and Society*, which contains the proceedings of a symposium on Giddens' work.

13 The glossing of epistemological issues in many analyses of science in politics leads to exactly this conflation of different forms of authority.

14 Throughout the philosophical debates, scientists have stayed largely on the periphery, apparently accepting neither the logical positivists nor their critics as guides to their own work.

15 It is remarkable that Ludwig Fleck's *Genesis and Development of a Scientific Fact* [1935], raising almost identical issues, was published twenty seven years earlier, but was ignored by academics. For whatever reasons, the time was ripe for Kuhn to function as an agent of a new approach.

16 A good survey of articles on the social construction of science can be found in Brooks and Cooper [1987].

17 Foucault's analysis of power also applies to what Robert Merton calls the system of norms and counter-norms that governs science as a social institution. Instances of the sociological ambivalence of science that can be viewed as locales of power and resistance include: universality vs. nationalism; the quest for innovation vs. the rejection of fads; apprenticeship vs. autonomy -- and teaching vs. research [Merton 1967: 113].

18 Weber makes a similar point about the "disenchantment" of the world when he argues that science is meaningless because it cannot address questions of value [Weber 1946].

19 Hagstrom's discussion of the dynamics of scientists' search for recognition provides an interesting example of what Foucault calls disciplinary power. A scientist who openly admits to seeking recognition is considered deviant, but this does not mean that such desires are not prevalent. As Hagstrom argues, "Whenever strong commitments to values are expected, the rational calculation of punishment and rewards is considered improper" [1965: 21]. Thus, the scientist internalises the community's values and is disciplined by a process of normalisation.

20 When asked in an interview whether his analysis of knowledge and power applies to the exact sciences, Foucault replied that it does not. However, his reply suggests that this is the case because he simply has not tried to make the connections, rather than because there is an unbridgeable gap between the physical and the human sciences. Further on, he states that "science, in Europe, has become institutionalised as a power," and that "science exercises, literally, a power that forces you to say certain things if you are not to be disqualified not only as being wrong, but, more seriously, as being a charlatan" [1990: 106-7]. While Foucault never applies his own understanding of power to the internal workings of science, one can do so with interesting results.

21 Thus, the notion of laboratory is broad enough to cover the main techniques of monitoring the global environment, computer modelling and satellite surveillance.

CHAPTER THREE NOTES

1 The term 'science' covers too much ground to be defined concisely . John Ziman traces the expanse of the territory:

> [Science] is indeed the product of research; it does employ characteristic methods; it is a body of organised knowledge; it is a means of solving problems. It is also a social institutions; it needs material facilities; it is an education theme; it is a cultural resource; it requires to be managed; it is a major factor in human affairs [1984: 12].

I employ the term in all of these senses.

2 Though not a scientist himself, he took it upon himself in the early seventeenth century to envision a positive social climate conducive to the growth of science. Bacon argued that science should be pursued not as an end in itself, but for its usefulness for society. In his classic formulation, he wrote: "Knowledge and human power come together as one. . . . For where the cause is not known, the effect cannot be produced" [1985: 39].

3 He opens his *Scientific Man versus Power Politics* with the following statement:

> Two moods determine the attitude of our civilisation to the social world: confidence in the power of reason, represented by modern science, to solve the social problems of our age and despair at the ever-renewed failure of scientific reason to solve them [1946: 1].

4 In fact, most of the books and articles about the interaction of science and politics published in the two decades following World War II deal almost exclusively with military matters. See, for instance, Cox 1964; Lapp 1965; Wiesner 1965. More generally, advisers have been associated with turbulent political conditions. For example, Machiavelli sought to counsel the prince in a context where numerous city-states existed in close proximity to one another under conditions of constant rivalry and warfare [Goldhamer 1978: 22]. Turbulent political conditions are characterised by complexity and uncertainty, inducing decision makers to seek greater clarity and predictability. Contemporary turbulence is characterised by an increase in international interdependence and a greater connectedness among policy issues [Rosenau 1989; Keohane and Nye 1977]. Thus, the recent growth in the number and importance of scientific advisers is not surprising.

5 Political leaders also risk embarrassment when their ignorance of the dominant scientific discourse is exposed, as happened more than once during the Montreal Protocol process. But the most important source of power for experts, even when they disagree, is the fact that without them, policy makers are more likely to make bad decisions. In the words of one author:

> To assume that technical inputs are unimportant because both sides of a controversy commonly present technical analyses purporting to prove their own side of the issue is a little like a judge deciding the facts of a case are unimportant because the lawyers on each side always present briefs purporting to show how the facts support their own client [Margolis 1973: 51].

6 Non-scientist participants in policy processes have been known to complain of "the cult of doctor worship" [Wood 1964: 43]. Decision makers are also liable to ignore significant aspects of advice, suffering from the general human proclivity to believe that parts of a problem one does not understand are not really that important [Margolis 1973: 50].

7 Some experts view their power in more conventional terms as a given and finite resource. Hence they resist giving policy advice, preferring to withhold their limited influence for a more important issue sometime in the future.

8 At first this might seem insignificant, but in practice it becomes important. One example is the choice of whether risk is stated in absolute or relative terms. The same exposure to a theoretical toxic chemical may be expressed either as a one in a million risk per year, or as a 5% increase over normal background rates [Wynne 1987]. Clearly the second mode makes the risk seem graver.

9 A good example is how the Reagan administration's political goal of deregulation was implemented in the US EPA. For instance, new decision rules regarding carcinogens included the following: animal studies are not necessarily relevant; benign tumours may not be considered damaging; and merely looking at the chemical structure of some substances can suffice to determine their carcinogenicity.

10 As Dorothy Nelkin argues,

> [t]his is tactically effective, for in all disputes broad areas of uncertainty are open to conflicting scientific interpretation. Thus *power hinges on the ability to manipulate knowledge*, to challenge the evidence presented to support particular policies, and technical expertise becomes a resource exploited by all parties to justify their political and economic views. In the process, political values and scientific facts become difficult to distinguish [Nelkin 1984: 16: emphasis added].

11 Kenneth Boulding makes a similar point in promoting "the virtues of hypocrisy" as a way of helping nations to behave according to their professed ideals [Boulding 1983: cited in Brooks 1987: 152].

12 While military projects account for much of the change, especially in the US and the former Soviet Union, major increases have also occurred in the health, transportation, agricultural and, more recently, environmental sectors. Data on the number of scientists, the 'service' economy, and the computer 'revolution' document the shift to an informational as opposed to a resource-based mode of production in the affluent countries.

13 Daniel Bell, perhaps the most renowned of these, argues that the new social structure does not necessarily entail changes in the other two dimensions of society: politics and culture [1987: 61]. This argument is ostensibly meant to mollify critics who claim that such a transformation should be visible in other arenas. Bell's case, however, falters on the practical difficulty of maintaining such analytical distinctions. Other writers, however do find the new post-industrialism reflected in politics and culture [Inglehart 1979: Milbrath 1984].

14 David Apter's work, though not as popular as Bell's, paints a post-industrial vision in the starkest way. Apter foresees and advocates a society divided into a "scientifically literate" leadership and the functionally superfluous, scientifically illiterate masses [1964: 31-38].

15 The conflict between knowledge-based power and democratic values can be construed as a competition between the two fundamental legacies of the Enlightenment: faith in science versus commitment to democratic participation.

16 Still other theorists offer a positive vision of post-industrial society which is utterly divergent from Bell's, eschewing technical rationality and transforming the mechanistic paradigm of science. See Griffin 1988; and Merchant 1989.

17 The problems posed by an absolute denial of subjectivity are addressed in Chapter Two. I believe that they can be overcome by recognising that discursive conceptions of power blur the line between subject and object, but that some notion of contingent subjectivity is needed to make sense of social agency.

18 Echoes of Condorcet resound in statements like the following:

> If [international scientific associations] increasingly become the medium for infusing scientific knowledge and technology -- from a *global humanist perspective* -- into the political and economic decision-making process at the international level, there is a chance that some of the worldwide problems may yet be solved in a rational as well as humane manner [Evan 1981: 7; emphasis in original].

19 Morgenthau suggests an alternative breed of functionalism, one that is more realistic about the ubiquity of power [1985: 513-251. Like Mitrany he believes that human survival may require a functionalist world order. Unlike Mitrany, he believes that functional agencies can only change the context in which power operates, but cannot eradicate power itself. Thus, Morgenthau's position is more consistent with my own. Unfortunately, it is not spelled out and there is almost no reference to the role of experts. Moreover, Morgenthau maintains a fundamentally negative conception of power, though it differs from Mitrany's at least in its inescapability.

20 For a detailed criticism of functionalism on these grounds, see Giddens 1984: 293-97.

21 Ultimately, this problem is rooted in the larger problem that functionalism cannot provide a theory of social change because it lacks a theory of agency. In Chapter Two, I criticise Parsons' functionalist notion of power on this basis.

22 Durkheim, however, believes that *Gemeinschaft* is a prerequisite for *Gesellschaft*. Like Weber, he argues that the latter may occur at the expense of the former, such that a highly specialised elite community may be a disintegrative force rather than an integrative one. For a discussion of this, see Konig 1968. This line of reasoning is consistent with the critique of post-industrialism offered above.

23 Consistent with my argument, Haas declares:

> it is as unnecessary as it is misleading to juxtapose as rival explanations the following: science to polities, knowledge to power or interest, consensual knowledge to common interests. We do ourselves no good by pretending that scientists have the key to giving us peace and plenty; but we do no better in holding that politicians and capitalists, in defending their immediate interests with superior power, stop creative innovation dead in its tracks [1990: 11].

24 He argues that an ecological epistemic community was responsible for negotiating and implementing the Mediterranean Action Plan (Med Plan), a regime for controlling pollution in the Mediterranean Sea. Peter Haas' conception of epistemic communities suffers from two defects. First, it does not explain adequately the source of power for epistemic communities. Second, it provides no coherent conception of knowledge. Since knowledge is allegedly the source of power for epistemic communities, their power is thereby rendered mysterious.

25 He argues that social learning regarding the Med Plan took place on two fronts. In terms of foreign policy, marine scientists were able to persuade decision makers of the validity of their consensual knowledge. Domestically, the epistemic community "usurped" decision-making authority through "bureaucratic preemption" [1989: 398]. It is difficult to see how the usurping of authority, which connotes an illegitimate power move, is compatible with the notion of social learning.

26 In his recent work, Ernst Haas examines epistemic communities in the World Bank and the International Monetary Fund [1990]. Peter Cowhey [1990] looks at the economic theories of the dominant epistemic community of the international telecommunications regime. See also Putnam and Henning 1989; Babai 1992; Ikenberry 1992; Hopkins 1992; and Adler 1992.

27 This is not strictly true for many pollution regimes, but it does hold for conservation regimes and some pollution regimes.

28 In the decade following 1972 the number of national environmental agencies rose from 10 to 106 [Kim 1984: 287]. These bureaucracies are largely staffed with scientists or technically-trained laypersons.

29 Soon after his appointment as EPA Administrator in 1983, William Ruckleshaus gave his first policy address. He emphasised the central role of risk analysis, based on the clear distinction he saw between risk assessment and risk management. For an outline of his position, see *Environmental Forum* 1984.

30 See Johnson and Covello 1987; Hiskes and Hiskes 1985; Shrader-Frechette 1985; Fischoff et al 1981.

31 Some observers have carried this perspective to its extreme, reducing all environmental concern to social and psychological drives [Douglas and Wildavsky 1982]. My view, elaborated in Chapter Two, is that scientific facts are rooted in both intransitive natural structures and transitive social processes; reductionism in either direction leads to distortions.

32 Recent research on risk perception and decision making under uncertainty point to major flaws in the rational actor models that have been imported from economics to political science, suggesting some of the difficulties inherent in environmental policy making. Anthony Downs' pathbreaking *An Economic Theory of Democracy* [1957] was followed by a plethora of works applying rational models to voting behaviour, interest group politics, bureaucracies and international relations. Recognising that the assumption of pure rationality was unrealistic, various authors have proposed models of "bounded rationality," including Simon [1957]; Elster [1979]; and Steinbrunner [1974].. People tend to employ a number of general inferential rules, known as heuristics, in evaluating risks [Tversky and Kahneman 1973]. One such judgmental bias, called 'availability', predisposes individuals to overestimate risks resembling ones they have encountered recently, and to ignore risks that they have never experienced. Thus, the likelihood of unprecedented environmental change, such as extreme changes in local climate or severe ozone depletion, may be discounted in spite of dire scientific predictions. A related heuristic is the "out of sight, out of mind" bias [Fischoff, Slovic and Lichtenstein 1978]. People tend to be complacent in the belief that whatever data is in front of them represents all possibilities. These heuristics are particularly pernicious because people are generally overconfident in decisions based upon them, a tendency just as prevalent among experts as among laypersons [Slovic, Fischhoff and Lichtenstein 1985: 473-475]

33 Tversky and Kahneman uncover some fascinating instances of the framing problem. They find, for instance, that choices involving gains are often risk averse and choices involving losses are often risk taking, even though the same problem can be framed both ways [1981: 453]. Other research reveals that people prefer insurance that covers specific harmfully over policies covering a wide range of harms conditionally, even if the latter would be a more 'rational' choice [Kunreuther 1978]. Apparently, insurance is "bought against worry, not only against risk, and worry can be manipulated by the labelling of outcomes and the framing of contingencies" [Tversky and Kahneman 1981: 456]. An action increasing one's annual risk of death from 1 in 10,000 to 1.3 in 10,000 is perceived as far more hazardous when framed as a 30% increase in mortality risk [Slovic, Fischhoff, and Lichtenstein 1985: 479]. These findings have important implications for environmental policy, which is essentially probabilistic. In fact, participants in the Montreal Protocol process explicitly referred to an ozone accord as an insurance policy [US Department of State 1986].

34 Knowledge brokers can exist at lower levels of government, as did the small group of EPA policy analysts who kept the CFC issue alive for several years. Non-governmental actors, including social movements, can also function as knowledge brokers, framing and translating information not only for decision makers but for the public, thereby creating an atmosphere conducive to their own favoured policies. To look to scientists as the impetus for the Montreal Protocol as is common [Benedick 1990; Haas 1992], is a gross oversimplification of the process The notion of knowledge brokers refines the concept of epistemic communities, which generally overemphasises the role of scientists. It underlines the political flavour of epistemic communities, showing their embeddedness in the policy process, thereby mitigating the temptation to adopt a naive enlightenment perspective. It also problematises "knowledge," stressing the social process of translation and interpretation..

While scientific information was an important source of power, scientists were not the only actors with access to that information.

35 Another perspective on the same phenomenon is Ruggie's observation that "scientific and technological images and roles inform patterns of institutionalisation as long as a given issue remains at a relatively low level of political concern" [1975: 570]. The global warming debate indicates what can happen when an environmental issue does rise to the top of the political agenda. Thus, one would be mistaken in assuming that consensual knowledge necessarily leads to progressive political change. Scientists as a rule are very cautious; virtually every article published in peer-reviewed journals emphasises remaining uncertainties and the need for further research The ingrained caution of scientists may be partly responsible for the power exerted by knowledge brokers in some contexts, for the latter are often more likely to "go out on a limb" and make a policy recommendation before all the data is in.. These caveats can then be used in the policy arena to defend inaction, as many participants in ozone debate did right up until the end.

CHAPTER FOUR NOTES

1 In an important sense, the politics drove the science on this issue by defining the salient areas of research. While climate change was perceived as a distant threat, skin cancer from ozone depletion was perceived by the public as immediate, direct and universal [see Ohi 1985].

2 Public reaction was so fervent in the US that Congress received more letters on the issue than on any other since the Vietnam War [Brodeur 1986: 71].

3 These arguments are examined in Dotto and Schiff: 208-214.

4 For a summary of EPA's regulatory authority under the Clean Air Act, see "Protection of Stratospheric Ozone: Proposed Rule," 40 CFC Part 82, US Environmental Protection Agency, 1987.

5 By the mid-1980's, the consensus was that ozone levels had remained essentially stable during the 1970's. See "An Interview with Robert Watson," *Science Impact* September 1987.

6 Of all the producers, Du Pont had the only major research program for CFC substitutes.

7 National attitudes were key factors. Those countries which had already decided to regulate -- the US, Canada, Sweden and the Netherlands -- had high levels of environmental consciousness. The Strongest opponents of CFC controls, including Britain, France and Japan, lagged behind other industrial countries on issues like toxic substances, leaded gasoline, and nuclear proliferation [Stoel 1983: 68-69].

CHAPTER FIVE NOTES

1 The group included, but was not limited to, the following: John Hoffman, head of EPA's Stratospheric Ozone Protection Plan; Stephen Seidel, policy analyst with EPA's Air and Radiation Office; James Losey, staff officer with EPA's International Activities Office; Stephen Anderson, an economist with EPA; and Michael J. Gibbs, policy analyst with ICF Incorporated; James Hammitt, statistical analyst with Rand: and Frank Lamm a Rand economist.

2 The sponsors of the 1986 report were NASA, the World Meteorological Organisation (WMO), the Federal Aviation Administration, the National Oceanic and Atmospheric Administration (NOAA), UNEP, the Commission of the European Communities, and the (then) West German Federal Ministry for Research and technology.

3 A good example are the reports written by the Stratospheric Advisory Research Group for the British government [UK Department of the Environment 1979].

4 The value of the 1986 WMO/NASA report is evident in the fact that some participants in recent international talks on global climate change believe that the report was the main inspiration for the Montreal Protocol.

5 Halocarbons include not only CFCs, but methyl chloroform, carbon tetrachloride, and the bromiated compounds called halons.

6 Rand and ICF both contracted with EPA to do detailed analyses of market and production trends. These studies were presented and discussed in detail at the second session of the international economic workshop [Hammitt, et al 1986; UNEP/ WG.151/Background 2 Paper 1].

7 Many of EPA's arguments for stringent CFC controls were couched in terms of cost effectiveness. This was not merely a ploy to convince industry, but was mandated by President Reagan's Executive Order 12291, which he signed shortly after taking office. This order, stipulating that all major new regulations must undergo benefit-cost analysis, was part of the administration's overall policy of "regulatory relief." The order specifies that economic efficiency should be the basis for evaluating regulations [Smith 1984: 4]. Hence, EPA studied such seemingly peripheral matters as the price of replacing plastics damaged by increased ultraviolet radiation [Titus 1986: 71].

8 The US position was still being developed. While there was a mounting consensus to move beyond the aerosol ban proposed by the Toronto Group before the Vienna Convention, the US bargaining stance was not finalised until November 1986. A proposal tabled by the National Resources Defence Council (NRDC) at Leesburg called for a full phase-out of CFCs over ten years [UNEP/WG.148/3, Paper 6]. That proposal is remarkably similar to the US position that was eventually adopted.

9 According to Joseph Glas, the real issue concerned fluorospar's potential lack of availability for political reasons. Du Pont had been granted an exception to US trade restrictions in order to import South Africa's substantial fluorspar deposits, but South Africa is a risky trading partner. However, Glas admits that any fluorospar scarcity would have engendered only "short-term limits to the rate of growth" [Glas 1989]. If industry's predictions of a de facto freeze in CFC growth were valid, then there should have been no shortage. The suspicion arises, then, that those who deemed the fluorospar issue a "red herring" were correct.

10 Because the depletion was never total, the term 'ozone hole' is not fully accurate. Some industry representatives saw the psychological connotations inherent in the term and objected to it, preferring to speak of "temporary ozone losses" [Fay 1987]. Names that include repeated sounds, like the three 'o's' in 'ozone hole,' have "an advantage in the marketplace of ideas"' [*Los Angeles Times*, 28 November 1986]. Since the term has gained widespread acceptance, even in the scientific journals, I will use it.

11 This reluctance to accept Farman's work supports the argument for the social construction of knowledge. Science is deeply involved in such matters as reputation and recognition.

12 Other, more offbeat hypotheses were advanced. Two NASA scientists proposed that meteoric particles could be trapped in the winter polar vortex and break apart later to destroy ozone [*Washington Post*, 19 May 1987]. Researchers at Los Alamos Laboratory blamed the ozone hole on electrons spiralling down the earth's magnetic field lines [*Los Angeles Times*, 24 May 1987]. The plethora of theories lent support to those who hoped to block strong CFC controls by emphasising the uncertainties.

13 Heterogeneous reactions occur between chemicals in different states, eg, between a liquid and a gas. The models used during the Montreal Protocol negotiations considered only homogeneous reactions among atmospheric gases. F. S. Rowland had already done some laboratory research on heterogeneous reactions. When his data were included in the computer model used for the 1984 National Academy of Sciences report, the predicted total ozone loss skyrocketed from the 2-4% range to 20-30% [Rowland et al 1984]. This information was available at the time of the 1986 WMO/NASA assessment, but received little attention.

14 A team of scientists affiliated with industry published a paper in the special supplement of *Geophysical Research Letters* which favoured a chlorine chemistry explanation [Rodriguez et al 1986]. Robert Watson, a key person in both the scientific and political circles, believes that the ozone hole was responsible for industry's "philosophical reorientation".

15 The issue of whether or not other agencies were allowed sufficient input into the initial process later became hotly debated. Officials from the Commerce, Interior and Energy Departments were excluded from the meetings. EPA and State Department officials argued that these departments simply disregarded the issue because they didn't realise its political and economic significance. The fact that all of the above departments signed the original position paper through the Circular 175 Process lends support to the second interpretation.

16 Consumption was defined as production, minus exports, plus imports, minus the amount destroyed [Department of State, 3 November 1986]. This issue was controversial during the negotiations because the exporting countries, primarily in the EC, objected to having CFC consumption and production equated.

17 No specific numbers were mentioned either in the suit or in the Clean Air Act under which the suit was filed. David Doniger, the NRDC's lead attorney on the ozone issue, was "pleasantly surprised" by the US position [Roan 1989: 195]. The position was also much more comprehensive than the proposals of some environmental organisations. World Resources Institute, for instance, backed cuts in specific uses ranging from 25% for refrigeration and air conditioning to 90% for aerosol uses, averaging roughly 50% overall [Miller and Mintzer 1986: 19].

18 It is noteworthy that an 85% cutback, which was necessary in order to stabilise CFC concentrations was originally proposed by John Hoffman. Once the Antarctic ozone hole had been linked to CFCs, the goal of stabilising concentrations took on new meaning.

CHAPTER SIX NOTES

1 Properly speaking, acid rain is a regional problem of global consequence. Acid rain has been measured not only in the industrialised countries, but in Brazil, Africa, Southeast Asia, and the Arctic [Sitwell 1984: 39]. For the distinction between regional and global environmental problems, see section four of Chapter Three.

2 Of course, when scientific uncertainty is high, the distribution of costs and benefits cannot be known with great confidence. Hence, the Rawlsian veil of ignorance might apply and could facilitate efforts at international cooperation [see Young 1989: 362]. This, however, has not been the case for the climate change problem.

3 The data linking acid rain to human health problems is scarce, although sulphur is known to damage the skin, the eyes and the respiratory tract [Sitwell 1984: 47].

4 Even President Reagan's grossly mistaken claim that the Mount St. Helens volcano spewed more sulphur dioxide than America's automobiles and industrial sources was an attempt to frame the issue scientifically, rather than politically. To the contrary, the pollution caused by even such a major natural disaster was minuscule next to that produced by human activities [Lewis and Davis, "Joint Report of the Special Envoys on Acid Rain," January 1986].

5 The environmental consulting firm ICF put the cost to the U.S. of membership in the "30 Percent Club" at about US$4 billion per year in 1982 dollars [Harrington 1989: 14]. One scrubber for an electrical power plant costs approximately $100 million, and decreases the plant's efficiency by 3 to 6 percent [Katzenstein 1981: 35].

6 Equity issues came to the fore in the aftermath of the Montreal Protocol and the subsequent establishment of a trust fund for the transfer of CFC-substitute technology to developing countries. These same issues have dominated North-South discussions on global warming, and are discussed below.

7 Recall that the U.S. proposal for a near phaseout of CFCs, formulated by a few knowledge brokers affiliated with the EPA, was not endorsed by the nation's top ozone scientists. Similarly, a few pioneer researchers on acid rain in Europe advocated sweeping controls, but their proposals were met with scepticism by the wider scientific community. Later collaborative reports substantiated the earlier findings but, in general, stopped short of making concrete recommendations.

8 Richard Benedick [1990] argues that the IPCC is "analogous to the fact-gathering phase of ozone history," comparing their work to the series of scientific and economic workshops that preceded negotiations for the Montreal Protocol. As I argued in Chapter Five, his neat depiction of those workshops as a first, apolitical step in a risk assessment process is problematic. Yet even if his analysis reflected the ozone case, it cannot be sustained for the IPCC process. At least the "fact-gathering phase" preceded the more overtly political ozone talks in time, but that is not the case for the IPCC, where the two are occurring simultaneously.

9 Until recently, one-dimensional radiative/convective models of Earth's atmospheric system were used. These provide only global averages, failing to simulate geographic or altitudinal variations in gas distribution [Mintzer 1987: 42].

10 The sketchy quality of the second working group's report may also be related to the fact that the then Soviet Union, which chaired the group, was unable to exert leadership due to its own internal problems between 1988 and 1990. Australia took on some responsibilities, but could not fill the vacuum.

11 The EPA study took two years to complete at a cost of US$2.5 million [*Scientific American* 1989: 20].

12 Of course, a large budget may be poorly managed. One geochemist told a congressional hearing that, "Most scientists would agree with me that the handling of research on greenhouse gases by DOE . . . has been a disaster." He stated that money is misspent, and that scientists have trouble finding research funds to work on related issues where the agency does not see a clear interest [Broecker 1989: 206].

13 The dualism of global environmental politics highlights the Janus-faced nature of the nation-state as it functions at the hinge of domestic and international processes. The following chapter examines this point at greater length.

14 The U.K.'s target date of 2005, however, falls short of the target date of 2000 announced by several other E.C. countries [*Economist*, June 16, 1990]. Since Thatcher left office, British leadership on the issue has lagged somewhat.

CHAPTER SEVEN NOTES

1 The most widely cited proponent of the first viewpoint is economist Charles Kindleberger, who sees hegemonic status as tied to an ethical responsibility to lead [1973, 1986]. The second perspective, however, is more common among international relations theorists as an explanation for the formation of economic regimes [Krasner 1976; Keohane 1980; Gilpin 1981].

2 The environmental community's disinterest in the ozone issue is evident in the non-participation by any environmental group in the Vienna Convention meetings [Benedick 1991: 44].

3 I specify that these two categories of beliefs are only analytically separate because in practice they tend to merge. Much of the critical literature on risk analysis is rooted in this observation.

4 Foucault's notion of discourse has evolved since his first treatment of it in The *Order of Things* [1970]. He moves from an early conception of discourse as static -- a conception which at times seems to verge on idealism -- to a more dynamic conception of discursive *practice* which is embedded in social institutions [Terdiman 1985: 55-56].

5 Of course, the virtues of those approaches purporting to offer parsimonious explanations have already been called into question in the first two sections of this chapter.

6 While much of the new literature on the declining utility of force draws its cogency from the end of the Cold War, that literature is also continuing a school of thought that was influential during the 1970's. Events such as Tien An Men Square and the Persian Gulf War seen to cast doubt on the "declining utility of force" thesis. Yet a deeper analysis reveals the striking extent to which even these events, principally because of their direct coverage by the electronic media, were about ideas, persuasion and the ability to sway world opinion.

7 On some of the pitfalls involved in applying the language of national security to environmental problems, see Dan Deudney 1990.

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