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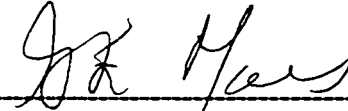
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CONSTRUCTIONS OF CLIMATE SCIENCE IN THE  
AGE OF ENVIRONMENTALISM

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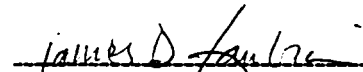
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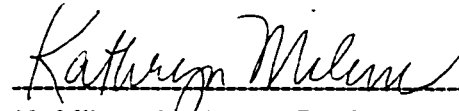
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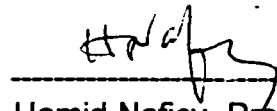
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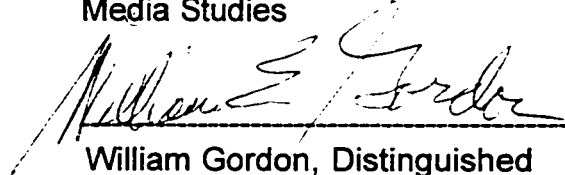
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## **Abstract**

This doctoral thesis analyzes the scientific and political debate about human-induced climate change, popularly known as “global warming,” and describes the shaping influence of larger U.S. social dynamics and political entities (Congress, President, federal policies, NGOs, the right-wing, and industry groups) on the climate debate.

Environmental concern and new science funding practices have profoundly altered the social dynamics and distribution of resources and recognition within meteorology. At a time where funding for scientific research is more difficult to come by, new levels of status and resources are available to climate modelers. This conflicts with a traditional hierarchy within the sciences which granted theoretical mathematicians and physicists the greatest levels of prestige. The older hierarchy often ranked climate modelers below higher-status scientists, labeling them as “engineers,” “technicians,” or “computer-operators.” While their new status is contested, climate modelers presently enjoy increased levels of access to status, funding, and influence, because they respond to needs of policy makers and the environmentally concerned public. At the same time, empirical meteorologists and scientists in other fields doing less policy-relevant science have found their access to resources reduced -- resulting in resentment among some scientists, particularly when the climate projections are known to be more uncertain and problematic than sometimes suggested.

The thesis suggests that status competition among scientists, a tightening national funding situation, and environmental concern, can encourage favorable public claims concerning the reliability of computer-based climate projections. The strongest scientific critics of climate projections tend to be empirical meteorologists and theoretical or defense-related physicists. They object to projections of significant human-induced climate change by pointing to large uncertainties in the science. In addition to resistance to recent changes inside and outside scientific circles, the arguments of "contrarian" scientists -- a small subgroup of vociferous critics -- reflect competition for access to funding, status, and political influence, and staunch political convictions which converge with the far Right. An older elite of highly influential physicists forms one contrarian subgroup. The thesis discusses manifest differences in historical consciousness, values, and subcultural styles, between this old scientific elite of physicists and emergent scientific elites of environmental scientists.

## **Acknowledgments**

I owe a lot of my intellectual development to the stimulating and unusual atmosphere in the anthropology department at Rice University. I am grateful for having been in a department which brings together such vital energy and creativity, and which allowed me to follow uncharted paths. I wish to thank Sharon Traweek and Michael Fischer for helping me conceptualize this research during its early stages, and George E. Marcus, James D. Faubion, and Kathryn Milun, for their many insightful and inspiring comments and suggestions as it was in progress. I thank them all, in addition to Hamid Naficy and William E. Gordon, for their willingness to read this dissertation, the size of which is not negligible! In particular, I want to thank William E. Gordon and James D. Faubion for their close, critical readings and prompt responses and support at all times during the writing of this dissertation. Without their warmth, thoughtful feedback, and meticulous care, writing this dissertation would have been a most frustrating affair.

In terms of institutions, special thanks to the National Center for Atmospheric Research (NCAR) in Boulder, Colorado, which -- besides financial support for part of 1994 -- has provided invaluable infrastructural support and allowed me to be around atmospheric scientists on a regular basis during the years of research and writing. I am infinitely grateful to all scientists, scholars, and administrators, at NCAR and elsewhere, who graciously offered pieces of their valuable time to help me and to share their perceptions and experiences

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## INTRODUCTION

"Global warming...has become a powerful symbol of the larger [environmental] crisis and a focus for the public debate about whether there really is a crisis at all"

Al Gore

"Global warming is the mother of all environmental scares"

Aaron Wildavsky

"Nature has become the favourite looking-glass of modern society. We look in the mirror of nature and reflect on the merits of society: how well are we doing?"

Maarten Hajer

Global warming has thrust itself to the forefront of issues of environmental concern in many countries around the world during the last decade, in large part because it powerfully symbolizes and reinforces the perception of a larger crisis of postindustrial society.<sup>1</sup> As a symbol of this perception, and as an issue with profound economic implications for a fossil-fuel-dependent world, global warming has become intensely politicized both within the United States and internationally, a politicization that reflects, provokes, and intensifies conflict among climate scientists. This dissertation describes and analyzes conflicts around the science of climate change, with a scope that includes analysis of the production, legitimization, and contestation of the science on which projections of human-induced climate change are most centrally based. It explores competing scientific theories and analyzes the various groups and actors, scientific and

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<sup>1</sup> Global warming is more properly referred to as "human-induced (or "anthropogenic") climate change," a term which emphasizes its human origin, by contrast to climate changes due to natural variability. "Global warming" also evokes the misleading understanding of the phenomenon as involving uniform warming around the globe, while scientists widely believe that the globe will warm in some areas and remain the same, or even cool, in others.

non-scientific, within broad socio-cultural, historical and political contexts, with a particular focus on the United States.

Just a few months ago (December 1997) representatives from about 160 nations met in Kyoto, Japan, to negotiate controls on human emissions of carbon dioxide and other "greenhouse gases" widely believed to be altering the global climate by trapping heat in the atmosphere. The meeting -- "COP3," the Third Session of the Conference of Parties -- was part of the United Nations Framework Convention on Climate Change (FCCC) established in 1992 at the Earth Summit in Brazil. Discussions at this meeting as at other meetings under the FCCC were highly technical and complex, and the political conflict intense as the thousands of attendees -- diplomats, scientists, environmentalists, journalists and industry lobbyists -- sought to understand the issue and to impose their understanding of this new environmental issue of concern. Meetings under the FCCC take place against a confusing background of scientific claims and counterclaims and of personal attacks and counter-attacks. Though the Intergovernmental Panel on Climate Change (IPCC) -- the central scientific authority on climate change operating under the auspices of the United Nations - - recently concluded that the "balance of evidence suggests a discernible human influence on global climate," there is great uncertainty and contestation associated with the science supporting this conclusion. Uncertainty in the science of climate change allows for different conclusions concerning the reliability of the science -- and conflicting judgments of the reality and potential

severity of human-induced climate change -- and of what constitutes appropriate response in face of it.

In November 1997, immediately before the Kyoto meeting, many attendees at the meeting received a book in the mail titled *Hot Talk, Cold Science: Global Warming's Unfinished Debate* by Fred Singer (Singer 1997). The book was also listed in *The New York Times* among the books to consult for further information about the science of human-induced climate change (Fernandez 1997). *Hot Talk, Cold Science* challenges the view that human-induced climate change has been detected in the climate record. In it, Singer advances many of the observations, theories, and criticisms frequently voiced by scientists skeptical of the theory of global warming, and asserts confidently that "[e]ven if a moderate warming were to materialize, its consequences would be largely benign" (Singer 1997:2). Fred Singer is presented as "one of the preeminent authorities on energy and environmental issues," an "atmospheric and space physicist" with "unassailable scientific credentials," including a Ph.D. in physics from Princeton University. The foreword to the book legitimizing the scientific credibility of Fred Singer and his arguments is written by Frederick Seitz, himself an extremely accomplished physicist whose past posts include presidency of Rockefeller University, of the American Physical Society, and of the National Academy of Sciences. The back cover includes blurbs from other important figures similarly endorsing the book, including Robert C. Balling, Jr., Director of the Office of Climatology at Arizona State University, William Happer, former Director of

Energy Research within the U.S. Department of Energy, and Richard Lindzen, a highly respected Professor in theoretical meteorology at the Massachusetts Institute of Technology, and a member of the U.S. National Academy of Sciences.

The broad range of scientific and non-scientific actors centrally shaping understandings of the science underpinning this environmental threat of supposedly potentially grave social, ecological and economic consequences has received little systematic description and analysis. Who are the scientists who wrote or endorsed this book? Who are the scientists most supportive of the theory of human-induced climate change? What scientific factions and perspectives are represented by the IPCC? By what processes and based on what considerations -- social, scientific and political -- did the IPCC come to its consensus statement that the "balance of evidence suggests a discernible human influences"? In short, how are non-scientists to make sense of the scientific disagreement between the competing scientific statements concerning this new environmental issue, and what shapes this disagreement? Informed understanding of the competing expert claims concerning global warming requires a level of historically, sociologically and ethnographically informed contextual knowledge currently non-existent in any coherent, comprehensive, and accessible form. To list more crucial questions: How are the "facts" supporting the theory of human-induced climate change produced? How is this science perceived by the producers themselves? Who supports and who contests the credibility of this science and its producers? Why do they support or

contest it, and how do they do it? How does differential access to various types of power factor in, including access to funding, to political decision-makers, and to the media? How do actors (scientific and nonscientific) on each side define the science and how do they manage uncertainties and contradictory evidence? How do they understand their own role in the "climate debate"? And, importantly, what role does differences in social and scientific backgrounds, practices, interests, historical consciousness, worldviews, and values play in the formation and appropriation of different expert opinions on global warming?

All of these questions are largely unanswered. Decision-makers and scientists at meetings such as that in Kyoto -- which by some accounts is "one of the most important environmental meetings of the 20th century" (Glantz 1997) -- are expected to navigate through the sea of competing scientific claims without a "map" providing this sort of crucial, contextual knowledge. The insights gained by scholars of science into the shaping role of social settings and contingencies in the development of science are largely ignored. To the extent that sociological or historical arguments are evoked, this is typically done so crudely and self-servingly as to be unilluminating, if not destructive of meaningful dialogue. Actors on both sides of the debate point to social, psychological, and political factors underlying (and discrediting) opponents' positions. For example, proponents of the warming theory suggest that those skeptical of it are loud but wrong-headed and few, funded by industry groups interested in protecting status quo (Gelbspan 1995; Kellogg 1987; Kellogg 1991; Nature 1996; Schneider 1990 (1989)). Critics of the theory, on the other hand, argue that there are more critics of the theory of

human-induced climate change than the IPCC consensus statement suggests.

They claim that the threat has been wildly exaggerated by single-minded environmentalists and sympathetic scientists (led by a core group), and that the theory of climate warming has gained its current prominence because some scientists secure funding for their research by producing "doomsday scenarios" (Balling 1992; Seitz 1997; Singer 1991; Limbaugh 1994; Wildavsky 1992:ixvii-xviii). The conflict among scientists and their supporters in the debate about global warming, and the vilifying or derogatory accounts of opponents' scientific abilities, motivations and interests, only indulges the tendency for nonscientific social and political groups to believe the scientific evidence fitting their assumptions and serving their needs.

The absence of perspectives informed by Science and Technology Studies (STS) in the above-mentioned arenas is regrettable. As Sheila Jasanoff has described, the constructivist perspective underpinning most work in STS has much to offer for policymaking (Jasanoff 1996) . "Constructivist" refers to a perspective according to which science is not an unmediated or unproblematic representation of the natural world. Research integrating this perspective has demonstrated that scientific claims are certified as true in part through the mediation of social processes, challenging the notion that scientific "facts" are established exclusively through objective processes. Understanding of the deep structures that shape their own and opposing positions on any given issue concerning a scientific controversy can help those familiar with constructivist ways of thinking to reframe their own questions so as to maximize the return

from scientific inquiry and to minimize antagonism and misunderstanding. Rich contextual knowledge of important technical, historical and socio-political factors structuring the differences between experts on the issue of climate change can aid the democratic process by informing voting and policymaking nonexperts of the underlying social and scientific assumptions, values and considerations variously shaping the constructions of the science on which they depend for their understanding of this environmental threat. There is at present a profound lack of such in-depth, historical and ethnographic study analyzing and contextualizing the "construction" -- that is, the development and characterization -- of the science supporting human-induced climate change, of those producing and those using the resulting information. It is this void that my research is designed to fill.

### ***Influences and Contributions***

#### **Influences from STS: boundary work and climate rhetoric.**

"Science" in Science and Technology Studies (STS) refers to a range of distinct though interrelated things, including (1) a set of characteristic methods by which knowledge is created ("certified") (2) a body of accumulated knowledge which has resulted from the application of these methods, (3) a set of cultural values and mores governing the activities termed scientific; and (4) any combination of the above (Merton 1973 (1942):268). Anthropological studies of science form part of the field of STS, sharing with it the key finding that social as



well as scientific facts are restrained by physical reality, but that they are in important ways socially constructed. Focusing on a wide variety of different scientific fields and practices, STS and the anthropology of science challenge the notion that scientific facts are tested and established with exclusive reference to objective criteria of validity, and explores the strategies by which scientists nevertheless succeed in acquiring and maintaining cognitive authority in a distrustful world (Jasanoff 1990 (1994)). Social studies of science have demonstrated how scientific facts at any given historical moment both reflect and impact the wider social contexts in which they were produced e.g., (Hollinger 1996; Latour 1987; Latour and Woolgar 1986; Rosenberg 1996; Martin 1989; Shapin 1979; Shapin and Schaffer 1985; Wajcman 1991). Such work has shown that "'science' is no single thing" and that the boundaries between "good" and "bad" science, between science and "pseudo-science," are ambiguous, flexible, historically changing, contextually dependent and variable, and sometimes disputed (Gieryn 1983; Gieryn and Figert 1990; Hess 1993:145).

Work in STS has rendered evident that evaluations of science -- what Gieryn has termed "boundary-work" -- are connected to the workings of interests, values and social conflicts within scientific institutions and between science and other social institutions. Through boundary-work, actors validate some kinds of science over other kinds, and, in the process, construct boundaries not only between different types of science and scientists, but also between themselves and other non-scientific social groups. Given the involvement of both scientific and nonscientific actors in defining and legitimizing scientific knowledge, the

challenge is to show how scientists and nonscientists with a stake in the scientific pronouncement of human-induced climate change engage in such boundary-work.

Whether or not human-induced climate change proves to be a significant threat, my dissertation research provides insight into the intersection of power and knowledge in late twentieth century societies, especially, but not exclusively, in the context of environmental issues. A central aspect of my work is to map the competing claims and arguments -- including their sources, contestation, and trajectories -- drawing on a wide range of material, including interview material and media reports. Given the fact that belief and value systems are reflected in scientists' rhetoric, especially when communicating to broader audiences (Mukerji 1989; Primack and von Hippel 1974), I have, in addition to interviews I myself have collected, also studied archived interviews with climate scientists (an important collection of which can be obtained through the American Institute of Physics and at the National Center for Atmospheric Research) and congressional testimonies, as well as popular books and articles by and about climate scientists concerning the threat of climate change and the political debate.

My study concerns what I, influenced by John Lyne's (1990) notion of "bio-rhetorics," call "climate rhetorics" -- the strategies by which people engaged in the climate change debate make their discourses about climate mesh with discourses of social, politico-economic, or moral life. Such analyses have shown that languages of purpose are an integral dimension of science discourses; while

science's domain is description, rhetorical devices that are used in personal and public deliberations, in processes of inquiry and advocacy, work to make facts -- or probabilities -- support judgments and actions. This move from description to prescription is particularly obvious and forceful in climate science, where scientists play prominent public roles as scientific advocates and policy-advisors, and where the connection between the science and social, political and economic issues is so strong.

**The study of the intersection of science and environmentalism, including the environmental opposition**

In addition to the production of contextual knowledge aiding nonexperts to calibrate competing expert claims, my work contributes to anthropology and Science and Technology Studies through its focus on the role of science in debates over 'green' issues, a focus that has not figured prominently in either field. "Science studies" (within anthropology or STS) have much to offer by way of analysis of environmentalism and environmental policy concerns due to the fundamental insight of science studies that scientific knowledge is disputable, and assembled and framed in a way dependent on social and practical considerations, assumptions and restraints. Yet 'science and the environmental movement' and the 'greening of science' still do not form well established focal points of research interest in science studies (Yearley 1995:457).

A second contribution of my research in this area is its analysis of the groups and actors critical of the scientific and environmental focus on human-

induced climate change. The dissident views among scientists and their extra-scientific supporters tend to be ignored in academic studies concerning the science and politics of human-induced climate change. Critical treatment of these dimensions tends to be done by non-scholarly popularizers, usually journalists. Such studies tend to be one-sided, positioned either for or against environmental concern about climate change. Work of this type tends to involve superficial analysis and relatively uncritical valorization of either the dissenters or of the mainstream scientific community and the IPCC.<sup>2</sup>

Scholarly studies within STS that do seek to analyze in a critical and even-handed way the politics of climate science tend to focus centrally on scientific constructions of human-induced climate change (Boehmer-Christiansen 1994a; Boehmer-Christiansen 1994b; Edwards 1997; Hart and Victor 1993; Kwa 1993; Shackley 1995; Shackley and Wynne 1995). Such studies thus side-line, if they don't entirely exclude, more marginal views that diverge from those of the environmentally concerned scientific mainstream. I know of no scholarly work specifically analyzing the forces -- the practices and orientations, including the environmental, political, and economic values and beliefs -- that prevail among the opponents of the theory and concern about climate change. It seems that the situation noted by Samuel Hays in 1991 has not significantly changed; that while the environmental impulses associated with scientific issues such as climate change have given rise to "formidable and persistent opposition," that opposition

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<sup>2</sup> For examples of the former, see Bailey 1993; Brookes 1989; Easterbrook 1995. For examples of the latter, see Gelbspan 1995; Gelbspan 1997; Helvarg 1994; Rowell 1996.

has received "little systematic treatment as a major element of environmental history. [Yet t]hey deserve analysis as careful and complete as the environmental drive itself" (Hays 1991:42).

### **Contribution to social scientific work concerning climate change.**

Research on climate change is dominated by the physical sciences, and while there appears to be an emergent interest in social scientific work of relevance to climate among physical scientists, administrators in scientific institutions and policy-makers, the body of social scientific research satisfying or responding to this interest is limited both in quantity and scope. The majority of social scientific research concerning climate change focuses on how humans contribute to, and are affected by, the environmental changes (CRU/ERL 1992; Glantz 1988; Gleick 1994; Lahsen and Jamieson 1996; National Academy of Sciences 1994), and climate scientists tend to exclusively associate the "human dimensions of climate change" with such ("impact") research (see for example (Schneider 1997)). My project differs significantly from such studies, as it focuses on how climate change is constructed as a problem in the first place.

Though still sparse, there is an emergent body of work concerned with understanding the social and scientific processes shaping the science and concern about human-induced climate change (Boehmer-Christiansen 1994a; Boehmer-Christiansen 1994b; Hart and Victor 1993; Edwards 1997; Herrick 1991; Jamieson 1988; Jamieson 1991; Jamieson 1992; Lave and Dowlatabadi

1993; Morgan and Keith 1997; Paterson 1996; Rowlands 1995; Shackley 1995; Shackley and Wynne 1995; Weart 1992; Weart 1994; Weart 1997). My own historical and ethnographic examination of the development of the scientific focus on human-induced climate change, and my analysis of the actors and issues shaping this focus, is centrally informed by such work. Also valuable to this research are studies into the demographic and psychological dimensions of environmental attitudes (Freudenburg and Pastor 1992; Hammond, et al. 1984; Kempton, Boster and Hartley 1995; Krinsky and Golding 1992; Milbrath 1984; Otway 1992; Pawlik 1991; Sjöberg 1989; Stewart 1991). Finally, I have found the work by Karen Litfin (1994) and Marteen Hajer (1995) helpful due to their focus on discourses in the context of environmental issues which, like the problem of human-induced climate change, involve great complexity in terms of the actors, institutions, processes and discourses they involve.

My research differs from all of the above-mentioned emergent social scientific research concerning climate change in its in-depth ethnographic approach and cultural perspective, and in its attempt to understand not only the production of the science but also its dissemination and contestation. My research also differs from that of Litfin and Hajer in its cultural perspective and its focus on people; while battling with the difficulty of defining and identifying “communities” -- as discussed in the following chapter -- I am concerned to understand individuals and groups of people, thus seeking to get beyond or below the relatively more abstract level of discourses.

### ***My own interpretive biases***

In my research into the debate about human-induced climate change I have attempted to approach each side both critically and with a sincere attempt to understand the particularities of perspectives and concern. I unavoidably bring my own personal biases to this study -- indeed, I intend my work to constitute critique, which by implication involves perspective and critical examination. Also, I should perhaps note that I consider the inevitability of bias in my work and assertions to also apply to those of scientists involved in debate about human-induced climate change, contrary to the many pervasive claims to objectivity found among them. In this debate, I have encountered many who are undecided, but no one who was scientifically and ideologically neutral. Actors in this debate often seek to promote themselves and their particular interpretations of the science of human-induced climate change. In the process of debating, persons involved also promote their understandings of the state and nature of society, the environment, and the economy, as well as the interrelations between these. In my view, what is needed -- and realistically possible -- is not more objectivity; as my research shows, claims to objectivity are generally instances of boundary-work. Rather, I argue that persons involved in the debate -- scientists, policy makers, and analysts of the debate such as myself, as well as others -- need to be more forthcoming about the values and assumptions we bring to whatever issues at hand. To the extent that such self-reflexive accounts are offered,

persons less familiar with the issues, and with those seeking to define the issues, have contextual information by which to calibrate the “expert” claims we make.

On this note, let me briefly state my own interpretive framework. In the early stages of designing this research, before I began my fieldwork among scientists, I considered the threat of human-induced climate change more certain than I do now. A closer examination of the science involved has impressed me with the profound uncertainties that characterize our current understanding of the reality and of the potential severity and impacts of human-induced climate change. However, my values have remained very similar throughout, which has placed me in a position more immediately at a distance from that of most actors within the environmental opposition. In my view, ecological disasters and the threat of global environmental change epitomize the reality of global interdependence in late twentieth century, and in my opinion it requires a new kind of awareness and consideration of the potential consequences of our actions, individually and collectively. While I recognize the difficulty in identifying the threats we live with -- and the role of bias and politics in identifying and defining these threats -- I am sympathetic with the argument that humans need to develop greater awareness of the effects of their activities on the global environment, and to seek to develop more equitable and sustainable ways of living with each other and on the planet. This is particularly the case for those of us who are introducing the greatest amount of actual and potential stresses into the ecological systems on the planet.



This orientation on my part appears to have deeper roots than I initially realized; I was well into this project before I realized that it probably wasn't pure incident and academic reasoning which led me to this research into a trans-boundary environmental problem. When I was still a little girl, I regularly felt anger and injustice when looking across the water from Denmark to Sweden's nuclear plant, Barsebæk. The outlines of the nuclear plant were visible on clear days from where I lived on the coast in the Northern suburbs of Copenhagen. I was impressed by a sense that something was very wrong with the fact that Danes had no say in Sweden's unilateral decision to build a nuclear plant in the South of their country, as close to Denmark's capital and most populated area as at all possible. By contrast to Sweden, Denmark had decided not to build nuclear plants, at least not until nuclear fission had proven a safe source of energy, and until the problem of how to dispose the radioactive waste had been effectively solved. However, Danes' attempt to minimize the risks with which we have to live was compromised by Sweden's right to sovereignty, and this struck me as profoundly unjust.

Barsebæk, as well as the threat of human-induced climate change as widely understood at this point in time, exemplifies how global security and the protection of the environment -- in addition to economic management, important areas of social policy, the protection of human rights, and the general creation of more just societies -- involve issues, and require actions, that transcend local and national frameworks. With the decline of distance in the world since the beginning of industrialization, the arena for political action has become global.

The previously dominant belief that the welfare of peoples depends primarily on the actions of their own governments is giving way to the realization that it also depends not only on their own choices of life-style and behavior as consumers, but also on actions and decisions taking place beyond the frontiers of their own state, by other governments and groups and by transnational corporations. It is therefore also necessary to develop means of counter-acting social injustice and environmental degradation which similarly both transcend and impact the local.

At some level, my experience of living next to Barsebæk has motivated my concern to study and strengthen the development of a way of thinking which recognizes the interconnections between humans, actions and processes (socio-economic, cultural, political, and environmental). I believe that it is important to stimulate a new type of sensibility and morality. As Stuart Hall has pointed out, the "enormous impact" of this ecological interdependence forms an important part of the process of globalization, and requires the development of "some form of an ecological consciousness which has to have, as its subject, something larger than the freeborn Englishman" (Hall 1991:25); in other words, such necessary ecological consciousness must transcend narrowly individual, local, and nationalistic frames of reference and concerns.

While my views are thus clearly environmentalist in orientation, I want to stress Maarten Hajer's point that environmentalism takes many shapes, and that, in some of its shapes and instances, environmental impulses can be oppressive,

ethnocentric, self-serving and intolerant.<sup>3</sup> Thus, while my views clearly conflict in important ways with those of the contrarians as a whole, I am sympathetic to some of their points, myself in some respects critical of the actions and power exercised by certain mainstream scientists and “gate-keepers,” whether those actions are or were rooted in self-promotion and insecurity, misunderstanding, arrogance or personal convictions about what is right and wrong. The chapter titled “Spirals of Silence and Dissent” expresses some of these criticisms, and shows the basis on which they are founded.

### ***Constructivism and environmentalism***

The constructivist framework presents certain difficulties in my study of how human-induced climate change is constructed -- materially and rhetorically by various actors, scientific and nonscientific. First of all, I disagree with radical constructivism at a theoretical level. I emphasize with the intentions and inclinations underlying claims to the effect that modern science is an “ethnoscience” of the West, with “no more global purchase than any other culturally specific, local knowledge system” (Sandra Harding, quoted in Nanda 1997), and that “scientific knowledge is only a communal belief system with a dubious grip on reality” (Edinburgh School quote, in Gottfried & Wilson 1997). However, I do not subscribe to the radical constructivist position that there is no physical reality. Distrustful of some radical strands of constructivism, my own

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<sup>3</sup> For literature illustrating this, see Sachs et al. 1993 and Agarwal and Narain 1990

theoretical framework resonates with that outlined by Chalmers under the description of “unrepresentative realism” (Chalmers 1994:163). Unrepresentative realism does not incorporate a correspondence theory of truth, meaning that it does not hold that any account of reality can describe the world as it really is, because we do not have access to it in an unmediated way, independent of our theories and available tools and methods of investigation. Unrepresentative realism is realist in its assumption that the physical world is the way it is, independently of our knowledge of it. According to this paradigm, we can attempt (with an inescapable element of subjective judgment) to appraise theories from the point of view of how successfully they come to grips with some aspect of the world, but we cannot claim to know in any unmediated way the world as it really is.

While I care to point this out, my position is not different from that of most scholars of science, in spite of suggestions to the contrary in the recent “Science Wars” about the nature, worth and validity of science and of sociological studies of science; I argue that in spite of recent suggestions to the contrary, radical constructivism does not dominate in STS. While the Strong Programme of the Edinburgh School has been attacked for suggestions of such radical constructivism and labeled “anti-science” by some outside of STS (Gottfried and Wilson 1997; Gross and Levitt 1994), figures of the school dismiss this as a misunderstanding of its philosophy, describing its stance as anti-rationalist, not anti-science (Dickson 1997). Suggestive of this, a recent article in *Newsweek* (Begley 1997) noted that of twenty or so STS scholars contacted, no one

contested that science, with the standard of replication, is a particularly powerful mode of inquiry. Modern science, with its rigorous methods of inquiry, is a demonstrably fallible but nevertheless an undeniably potent tool by which to attempt to gain understanding of physical phenomena.

Secondly, radical constructivism appears to be a potentially high-priced academic indulgence, at least when issues of social and environmental justice and health are involved. I consider myself an environmentalist in the sense that I am concerned that “the environment should be protected, particularly from the harmful effects of human activities” (Milton 1996:27).<sup>4</sup> In this regard, I am obviously committed to the view that there is a reality “out there” (and that it matters), independent of individual and conflicting constructions of it. Moreover, I am to some extent dependent on scientific knowledge, positioned vis a vis science in the ambiguous way characteristic of environmentalism: on the one hand, science is a threat to the environment, traditionally involving an instrumentalist attitude to nature, and involving and enabling the exploitation and pollution of nature through its technological products and material waste; on the other hand, (besides the fact that it appears impossible to prevent scientific innovation) science can be a source of knowledge and a vehicle for innovation resulting in less polluting practices and technology. Science is generally necessary for knowledge of environmental problems, particularly new

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<sup>4</sup> Environmentalism as a term covers an immensely heterogeneous mass of actors, concerns and interests, to the point that it can lose its meaning and use as a concept. As environmentalism has become a dominant rather an oppositional force in many industrialized societies, including the United States, most people consider themselves environmentalists. What is required, then, is a detailed, empirically-based

environmental problems such as acid rain, stratospheric ozone depletion, and human-induced climate change. These new environmental issues are creeping, cumulative and only known through the mediation of sophisticated instruments and scientific experts.

### ***Structure of the Dissertation***

My dissertation is structured as follows: The first chapter following this introduction (Chapter 1) concerns theoretical, methodological and practical dimensions, and difficulties, of my study. Chapter 2 introduces readers to important actors and issues in my research. It does so through an analysis of a controversy which took place around the 1995 report by the Intergovernmental Panel on Climate Change, the United Nations' international scientific authority on the science underpinning concern about human-induced climate change. Chapter 3 describes how the scientific focus on human-induced climate change developed historically. A subsequent chapter (Chapter 4) describes attempts by fossil fuel industry groups, among others, to shape public understandings of human-induced climate change. This is followed by a second historical chapter (Chapter 5), which describes how the field of atmospheric science developed with the rise of numerical modeling. The two historical chapters (Chapters 3 and 5) emphasize how environmental concern and new technological developments have meshed with a variety of other factors, all of which have profoundly shaped scientific practices and socio-political dynamics within the atmospheric sciences.

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examination of conflicting definitions of the state of the environment, of what environmentalism is, and of

The historical chapter on the rise of numerical modeling is followed by a chapter (Chapter 6) which analyzes the numerical climate simulations, the science which most centrally supports concern about human-induced climate change. This chapter also discusses the apparent effect of the simulation practices on the climate modelers performing them, and the role of federal funding practices in what work is performed and how it is presented. Chapter 7, titled "Spirals of Silence and Dissent," discusses the role of environmentalism and "gate-keeping" behavior among proponents of concern about human-induced climate change. As such, chapters 6 and 7 combined provide insight into key factors shaping how the science of human-induced climate change is presented to the public by mainstream scientists. These chapters are then followed by a chapter which provides an historical analysis of an older generation of physicists -- among them, Frederick Seitz -- which has been particularly influential on the "contrarian" side of the issue of human-induced climate change.

**Chapter 1.**  
**THEORY AND METHODS –**  
**FRAMEWORK, DIFFICULTIES, AND STRATEGIES OF THIS STUDY**

***New environmental problems and the study of scientists***

During the last few decades, critical examination of scientists as a knowledge producing “elite” has become an important project for scholars. The increased centrality of science in understandings of society and nature, and in the exercise of power, has empowered a new “class” of scientists and other professionals, providing them access to considerable power and influence through the production, mobilization and contestation of knowledge. This power aspect of science was not addressed by the Mertonian sociology of science because it accepted scientists' positivist self-conception and -representations. As a reflection of this, this approach focused on scientists' behavior, but not on the content of their work, thus failing to recognize the social and political aspects of scientific knowledge itself. This short-coming has been criticized and compensated for by the dominant “new vision” in Science and Technology Studies, which was inspired by the work of Thomas Kuhn (Kuhn 1970). This new vision in STS also grew out of awareness of, and discomfort about, the heightened authority of experts in an age in which science often is the most central source of power. Usually accompanying this anxiety about the a priori trust placed in scientific experts was an appreciation of democratic participation, an appreciation partly shaped by the counter-culture of the 1960s and 1970s, in



the United States and elsewhere (Edge 1995). My research is informed by similar concerns. As I describe in this chapter, however, a critical study of experts, such as the one I have undertaken, presents considerable difficulties, for a variety of reasons.

### **The politicization of science**

The need for critical examination of science as a social institution is heightened by the increased contestation of scientific knowledge over the last three decades, a development partly rooted in the same protest politics that informed the new sociology of science (the effect of protest politics on the social institution of science is described in the chapter on “New and Old Elites”). As David Dickson has noted, the more a science is connected to economic structures, the more the boundaries between the two dissolve and the science is politicized (Dickson 1989). In the environmental sciences, politicization of science is particularly apt to occur when scientific findings might be interpreted or presented as justification for the regulation of socio-economic activities on behalf of the environment. The issue of human-induced climate change is a case in point. The scientific disagreement that characterizes areas of science which have become strongly politicized is of great consequence for policy makers and the general public, and has led to calls for greater knowledge about the cultural, social and political differences and processes underlying conflicting expert claims competing for authority.

Calls for social scientific research into the socio-cultural and political dimensions of production of scientific knowledge also derive from awareness of new types of risk in late-industrial society. The process of industrialization has produced risks the nature of which escape direct perception and which -- to the extent that they are knowable at all -- can be known only through the mediation of scientific formulae, instrumentation, and interpretations. The "new environmental" problems of acid rain, ozone depletion and human-induced climate change are examples of the new nature of certain risks. New environmental problems often have far-reaching consequences in time and space, representing risks that are inherently uncertain and unpredictable. As Ulrich Beck points out, these risks have become a central structuring feature of late twentieth century "risk society" (Beck 1992). An important feature of the risk society is that science becomes "demonopolized" because it cannot adequately predict and account for new types of risks that are inherently unpredictable, yet of great potential danger. In the face of such risks, science is simultaneously increasingly important and increasingly ineffectual or insufficient. As Beck writes, "science becomes more and more *necessary*, but at the same time, *less and less sufficient* for the socially binding definition of truth (Beck 1992:156).

Confirming Beck's theory, even in scientific areas which do not involve new environmental problems, STS and policy studies show that advances in scientific knowledge and reduction of scientific uncertainty do not necessarily

translate into reduced conflict over policy.<sup>5</sup> In the process of doing fieldwork, I gradually learned about the larger political context into which climate science conducted at NCAR and other scientific institutions nationally and internationally is absorbed, in the process coming to see how the production of science involves a feedback loop by which larger social and political contexts, themselves partly shaped by science, in turn impact the production of science.

This understanding of science and its role in society contrasts traditional -- and still prevalent -- assumptions about the relationship between science and society. These assumptions were the basis of Vannevar Bush's classic 1945 report, *Science: The Endless Frontier*, which centrally shaped post-war U.S. science policy. This model of science, which some call "the linear model," involves the assumption that basic research leads to applied research and technological developments of benefit to society (Pielke 1997; Price and Bass 1969). The linear model describes science as shaping society through the practical implementation of the pool of knowledge gained through scientific exploration and innovation. As Roger Pielke and others have pointed out, what is lacking from this model is a recognition of the other side of the feedback loop, namely the influence of society on science (Pielke 1997). Political scientists and new sociologists of science have pointed out that the linear model serves to isolate science from critical examination of its actual benefit to society, and science does not necessarily lead to real and sustainable advances in the quality

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<sup>5</sup> For instance, this was the conclusion of a study of safety standards for the suspected carcinogenic

of human life for a majority of people (Pielke 1997).<sup>6</sup> In addition, the linear model fails to recognize the difficulties and choices imposed by the existence of conflicting scientific evidence, and how decision makers and other groups outside the scientific community choose to lean on the expertise of particular experts.

Given that scientific results are only one factor in definitions of truth -- in science as in policy making and lay understandings -- it is particularly important to study the ways in which scientific arguments are infused with political interests and with moral and ethical values and judgments. Science involving great uncertainties is particularly rich ground for such infusion, rendering important parts of the scientific debate essentially political, moral and ethical. Because of the uncertainty in the science concerning human-induced climate change, scientists are asked to make judgments and to provide "expert opinions" of aid to policy makers who need to consider the threat and possible preventive measures in the face of uncertainty. This is what triggers much of the tension around the science of human-induced climate change among scientists, and where scientists' biases surface in their discursive constructions of human-induced climate change.

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chemicals, formaldehyde and benzene (Graham, Green and Roberts 1988).

<sup>6</sup> As Dr. William Gordon has noted regarding this point, the top one billion in the world may be better off, in some regards, while the bottom billion people in the world, as well as probably three billion of the middle, are unaffected, at best (personal communication).

### **Influences on concern about human-induced climate change**

Sociologist Stephen Klineberg's survey of environmental attitudes found factors such as age, education and gender only weak determinants of environmental attitudes. His surveys of Texans' environmental values and beliefs over a period of years found the single most significant predictor of a person's environmental values to be political ideology. Particularly indicative of environmental values in his research of was whether a person had a favorable or unfavorable perception of Rush Limbaugh (Klineberg 1997). Klineberg's survey found that the twenty-three percent of Texans who said they had a favorable impression of Rush Limbaugh evidenced less concern about environmental issues across virtually all of the different measures in the survey, when compared to the thirty-seven percent who had an unfavorable impression, and eleven percent who held mixed views. Limbaugh's supporters were more resistant to any proposed sacrifice of short-term economic interests for the sake of environmental protection, and they were "opposed across the board" to any new environmental initiatives. Moreover, they were more likely to dismiss the importance of local, statewide, or global environmental problems.

Other studies not similarly limited to a particular region support Klineberg's conclusion that demographic indicators such as age, education and gender are only rather weak determinants of environmental concern. For example, Jaeger et al. have argued that the factors influencing environmental understandings and action related to climate are tied less to the above demographic profiles than to

socio-cultural variables, such as interpersonal rules and social networks than to socio-democratic characteristics (Jaeger, et al. 1992) .

Political (social, cultural, environmental) values and beliefs appear to constitute important shaping factors in differences of position on the theory of human-induced climate change. I have found considerable correlation between experts' differing scientific positions on the seriousness of the threat of human-induced climate change and their values and beliefs, particularly their beliefs concerning the state of the environment, the economy and the future, and the interrelationships between these. So have scientists engaged in the debate, even as they find this realization upsetting to their conceptions of science as independent of subjectivity and bias:

MARTIN HOFFERT: It seems very interesting to me that most of the scientific opponents of global warming tend to have politically conservative positions. They are politically conservative, as well as scientifically conservative with regard to the global warming issue. And the opposite is also true: many of the proponents of global warming happen to be politically liberal. Now, as a scientist, I find that highly upsetting! I mean, it would be as if the liberals of the time of Newton laws of motion thought that force equals mass times acceleration, and the conservatives thought force equals mass times acceleration-squared, or something! I mean, science should be independent. In science it is presumed that there is an objective reality and that we can determine that objective reality by applying the scientific method to observations, and that we can rule out hypotheses that don't square with observations. [...] Given that, it seems suspicious that the opponents of the global warming theory tend to subscribe to a conservative position and the proponents to a liberal position. Now, I haven't *proven* this, I haven't given out questionnaires. But I know most of the people in this business personally. And I have had discussions with them. And I believe this to be the case. [...] And now, if that is the case --*if* that is the case; I am not sure, but I think there is a good probability that it is -- then I think that the differences might be coming from the implications of global warming rather than the science. And those implications go into ideology as much as they go into science.

[...] Because the impacts are very important. Just because you believe in global warming doesn't mean that global warming is going to be "bad". And you have to define what "bad" means.

Richard Lindzen, who is on the skeptical side of the issue of human-induced climate change has acknowledged that he votes Republican, pointing out how rare that is among atmospheric scientists (Stevens 1996b).

Hoffert's suspicion that at least an important part of the disagreement between the scientists on different sides of the issue is rooted in different perceptions of the (socio-economic and environmental) implications of global warming, more than in the science itself, was confirmed by climate modeler Jerry Mahlman, Director of the Princeton affiliated General Fluid Dynamics Laboratory (GFDL). In an interview with me,<sup>7</sup> Mahlman explained that he and Lindzen agree on most of the scientific points, and that where they disagree is on "what this means." Mahlman described Lindzen's position as being that 'if you don't know everything, then you don't know anything and can't do anything.' By contrast, Mahlman supported the precautionary principle, suggesting that enough is known with enough probability to justify some action.

An interview I had with Richard Lindzen<sup>8</sup> also confirmed Hoffert's theory. In the interview, Lindzen emphasized the uncertainties in the projections of human-induced climate change, as well as his personal view that 'given a choice between freedom and government, he tends to think that freedom is better.' He explained that he doesn't agree with the view that regulations are the best way to

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<sup>7</sup> November 28, 1995.

improve the environment. Rather, he believes that increasing wealth leads to a cleaner environment. This simple example of the differences between two high-profile scientists in the debate suggests the existence and complexity of issues involved in the deeper dialogue shaping the conflicting scientific pronouncements on the issue of human-induced climate change. The difference also reflects the general difference between “high proof” and “low proof” scientists in environmental debates, as identified by Samuel Hays (Hays 1987).

An anthropological study of the discourses involved in a scientific controversy can provide information by which to calibrate scientific disagreement in scientific disputes with a bearing on environmental protection and socio-economic restructuring. Such a study can “map” the various groups (scientists, politicians, industry groups, grass-roots movements etc.) who are shaping the construction of environmental threats, and show how these groups seek to promote their particular construction of the threat. However, actually doing an ethnography of a scientific controversy is not an easy task, for reasons having to do with (1) the difficulty of framing the study of an often highly heterogeneous and geographically dispersed “field” of conflicting actors, processes, and discourses, (2) problems associated with “studying up,” and (3) the difficulty of actually accessing different actors’ “worldviews.” Below, I explore each of these difficulties and describe how I have chosen to address them.

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<sup>8</sup> December 5, 1995.



***A dispersed “community”?***  
***Reconceptualizing social formations with globalization.***

Traditional sociology has been skeptical of the power of disembodied communication, believing that social structures require frequent face-to-face interactions among members. According to *The Concise Oxford Dictionary of Sociology* (Marshall 1994:72), the term “community” concerns a particularly constituted set of social relationships based on something which the participants have in common -- usually a common sense of identity. The concept of community is frequently used to denote a wide-ranging relationship of solidarity over a rather undefined area of life and interest. The concept has been most widely used in the social scientific literature to refer directly to types of population settlements such as villages or urban neighborhoods, that is, to physically bounded social units. Ethnography as a social science genre has always depended upon the understanding of communities as bounded within relatively small geographical regions. This construction applies poorly to many new social formations, to the extent that some consider this problem a “crisis” in the social sciences (Marcus and Fischer 1986). While concern within STS to understand the emergence of new social formations with globalization is not new, their actual nature and consequences for scientists, science, and societies have received little systematic analysis (Restivo 1994:104). The concept of community needs to be expanded to also account for (sometimes highly heterogeneous) social formations which increasingly form across geographical distance with the means

of new communications systems and the increased ability to physically move across the globe, provided the material means.

Certain symbolisms of community dominant in the nineteenth century have been prevalent during the twentieth century, symbolisms associating "community" with a high degree of personal intimacy, emotional depth, moral commitment, social cohesion, and continuity in time. Many new social formations do not fit this definition of community -- for that matter, nor do many more traditional social units; this idealized conception of community has been widely disputed within the social sciences. As Benedict Anderson has persuasively argued, community based on shared nationality and other types of bonds above face-to-face interaction are all imagined because "the members of even the smallest nation will never have known most of their fellow members, meet them, or even hear of them, yet in the minds of each lives the image of their communion" (Anderson 1991:5-6). Thus, not even people sharing geographical boundaries and national identity fit the idealized notion of community which implies a high degree of personal intimacy; members of even the smallest nation will never have known or even heard of most of their fellow nationals. Nevertheless, such idealized notions are still often associated with the concept of community in general usage.

In "Disjunction and Difference in the Global Cultural Economy," Arjun Appaduray provides an initial framework for reconceptualizing cultural shapes in a globalized world in which configurations of people, place and heritage gradually

lose semblance of isomorphism. He proposes that we begin to think of the configuration of cultural forms in today's world as fundamentally fractal, as possessing no "Euclidian boundaries, structures, or regularities" (Appadurai 1990). But how is such a study to be carried out; how is it to be framed?

It is exceedingly difficult to shape a study such as mine, involving such heterogeneity of actors and processes. Developments within science and concern about global environmental change have brought about an unprecedented level of multi- and inter-disciplinary research within the earth sciences, a development which has caused profound change to this field, and which has introduced a great complexity of scientific actors and approaches. Of all scientific disciplines, earth and space sciences now involve the most international collaboration (Luukkonen, Persson and Sivertsen 1992). The scientific "community" contributing to the production and rhetorical construction of knowledge concerning climate change thus includes a heterogeneous group of practitioners of different geographic backgrounds, and of different disciplines and specialties. It includes meteorologists, mathematicians, physicists, chemists, oceanographers, and biologists, with each field subdividing into a host of subdisciplines, cross-disciplines, and specializations. Yet the amount of social scientific and historical studies of the different "communities" involved in climate change research is quite limited, and as Jasanoff has noted, "a great deal more work remains to be done along these lines" (Jasanoff 1996).

Given the dispersed and often amorphous nature of scientific communities, is the designation “community” even appropriate? The scientists I have studied collaborate across time and space, and often never even meet their collaborators in person. Informants do not form a neatly knit and bounded social unit, as defined by geography, social function and position, ethnicity, race or sex (although the overwhelming majority of interviewees have been white males of Euro-American culture, given the U.S. focus of my dissertation research) -- and even if they did, the findings of Klineberg and Jaeger et al. mentioned above suggest that these differences wouldn’t necessarily be important shapers of attitudes towards human-induced climate change.

Analyzing the presentation of certainty by scientists in different fields in scientific discussions about the sun, Trevor Pinch has noted the inherent “messiness” of his sociological research due to meshing of different fields and specialties; it is not clear how one is to choose one’s sample group, and it is difficult to find equal representation of different approaches and positions among the scientists, because such equal distribution doesn’t exist -- and unequal distribution is not easily accounted for, especially when the size of different groups is unclear (Pinch 1981). Estelle Smith has remarked that “there is no single scientific world, even within a field of study, let alone across disciplines;” individuals in groups inevitably coalesce into subgroups, invalidating the sense of homogeneity evoked by the generic references to “scientists” or the “scientific community” (Smith 1996:201). Smith’s statements resonate with the work of

Peter Galison, who has demonstrated how even a supposedly single scientific discipline such as physics -- as well *as each specialty within it*, is inherently disunified -- to the extent that practitioners of the discipline or sub-discipline "cannot be considered homogenous communities" (Galison 1997:782). How is one to constitute a research site out of such heterogeneity? How is one to justify the boundaries drawn?

### ***Dividing up the camps***

#### **Skeptics, contrarians, and the scientific mainstream defined**

Skepticism is a valued trait in science and skepticism is particularly prevalent in scientific areas involving considerable uncertainties. Hence, I make a distinction between "skeptical" and "contrarian" scientists by the latter group's tendency to be skeptical not only with regards to human-induced climate change but also a list of other issues of widespread environmental concern. Contrarians are marked by their tendency to be on the opposing side of environmentalists with regards to ozone depletion, acid rain, the pesticide DDT, and nuclear power, among other things. Contrarians contrast skeptical scientists by their high public profile, that is, by their frequent appearances in the media and other non-scientific forums where they express their views. Contrarians are also marked by

the extent of their affiliations -- material or ideological -- with industrial and politically conservative groups.<sup>9</sup>

Before I go on here, let me clarify that for the sake of convenience, I will refer to two, opposing sides around the issue of human-induced climate change. However, in actuality there is a wide range of scientific and political positions in this debate -- numerous "sides," so to speak. I will try to describe some of this range, but for convenience's sake, I will often group the differences of positions within two over-all conflicting positions as to the reality and severity of future impacts of human-induced climate change, with "proponents" of the theory of human-induced climate change on the one side, and contrarians and their supporters on the other.

There is a certain danger in doing such simplistic lumping into two opposing camps because (1) scientists might support or oppose the theory of (significant) human-induced climate change for different reasons, such that each camp in fact includes very varying assumptions and values concerning the state of the environment, the state of the economy, and the relation between these; they may find themselves in the same camp for very different reasons. Secondly (2), there is a danger in describing the different positions in a binary, oppositional manner in that differences in expert positions already tend to be represented as more diametrically opposed than they prove to be upon further scrutiny. As I will discuss in the concluding chapter, deeper scrutiny shows the important

similarities and agreements among scientists on different sides of this debate.

Finally, (3), such simplistic lumping leaves it unclear where the undecided fit in -- and the undecided on the issue appear to form the large majority, as I will describe below. This division of the range of positions into two groups leaves it unclear where “skeptics” fit in, those who share some or all of contrarians’ skepticism, but who differ from contrarians in the degree of (and, often, in the reasons for) their opposition -- and who, unlike “contrarians,” aren’t as public about their disagreement nor as categorically skeptical about most recent issues of widespread environmental concern in general.

The same kind of “lumping” occurs with my use of “the mainstream community,” a term I often use and which can appear interchangeable with the above-mentioned group of “adherents” of the theory of human-induced climate change. Messiness is inherent in any attempt to capture in a simplified form a field characterized more by its heterogeneity than its coherence. In the name of simplicity, and at a loss of having better alternatives, I frequently use the term “mainstream scientific community” to denote the dominant paradigm, as represented by the IPCC. This is problematic for a number of reasons; while the IPCC represents a consensus, this consensus is in fact forged out of great heterogeneity of views on the reality, significance and impacts of human-induced climate change. Besides the key summarizing consensus statement itself (in the latest report, that “the balance of evidence suggests a discernible human

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<sup>9</sup> Simon Shackley, in his study of the scientific debate about climate change, has similarly stressed the

influence on the global climate”), as consensus documents, the reports in fact accommodate a wide range of conflicting interpretations of the threat of climate change. Each caveat and uncertainty allows for a different view of the scientific foundation of the theory of human-induced climate change. To some extent, the IPCC process includes “contrarian” scientists, which theoretically renders any distinction between the IPCC and contrarians impossible.

Nevertheless, I use the term the “mainstream community” to refer to scientists who, by contrast to contrarian scientists, both (1) work in “mainstream” scientific institutions (as opposed to political think-tanks and other more directly political organizations) and who (2) operate under the dominant paradigm. The dominant paradigm involves a set of premises, including that human-induced climate change is at least a possibility; that human-induced climate change may have been detected in the climate record, though it hasn’t been detected with certainty; and that such human altering of the climate system is not desirable, in principle, and is likely to have deleterious effects. Finally (3), I use the term “mainstream community” to refer to scientists who publish exclusively in scientific, peer-reviewed (“mainstream”) journals. While some among the contrarians fit the first criteria -- i.e., they work in mainstream scientific institutions -- they do not fit the other two. As I will show, the contrarians are marked by their greater level of affiliation with external industrial and political groups, including a greater tendency to publish their work in non-mainstream and



non-scientific journals. This is in marked contrast to the vast majority of “mainstream” climate scientists who publish exclusively in scientific, peer-reviewed (“mainstream”) journals. Many scientists fit the first and the third criteria but not the second, because they take issue with elements of the dominant paradigm. These are nevertheless mainstream scientists insofar as many mainstream scientists in fact are skeptical of the dominant paradigm as a whole or of aspects of it.

When I refer to “the mainstream community,” then, I do this more out of convenience than to imply that the wide range of scientific disciplines and actors within mainstream scientific research labs and university departments involved into the debate form a community in the sense of being tightly interwoven and sharing of similar “world views.” I will use the terms “scientific community” (which researches climate change) and “mainstream scientific community” for the name of simplicity, even though these don’t form easily defined entities, nor are they experientially single communities. At a loss for alternatives, I will use the term “scientific community” to denote this larger constellation of actors in the parts of the larger scientific community which contribute to knowledge constructions of climate change. If I use the term to refer to scientists of all scientific fields, I will add “larger scientific community” to specify this more inclusive meaning.

### **Hawks, owls and doves**

In his analysis of the scientific debate about climate change, Mickey Glantz distinguishes between “hawks,” “doves” and “owls” within the scientific

debate about climate change (Glantz 1988). "Hawks" are those scientists who believe that "the evidence of a CO<sub>2</sub>/trace-gas warming is very convincing and that the warming is already underway;" "doves" are those who feel that the greenhouse warming scenario is "yet another doomsday scenario that will most likely fail to materialize;" and "owls" the ones who "have yet to make up their minds on the issue."

While helpful in some regards, I largely avoid Glantz's labels. There are two reasons for this. One is that while the division into three groups is better than none, it fails to make certain distinctions which I consider important. For example, it describes as "doves" those who feel that the greenhouse warming scenario is "yet another doomsday scenario that will most likely fail to materialize." It thus fails to distinguish between scientists who are critical of the global warming theory *yet* who are environmentally concerned. While contrarians ("doves") tend to discount environmental threats across the board, other scientists are critical of the strong focus on and concern about climate change in large part because they consider other environmental and social problems, such as population growth, poverty, and species extinction, for example, much more pressing issues compared to climate change. Of course, many will point out the difficulty of separating these latter issues from that of human-induced climate change.

The other aspect of Glantz's' distinctions with which I am uncomfortable is the meaning his symbols evoke through their references to "hawks" and "doves"

in particular. Hawks are associated with predatory behavior while doves are associated with peace. As I see the actors on both “extremes” in the scientific debate about climate change, they are all equally predatory. The characteristics associated with “dove-ness” apply poorly to the high-profile scientists of mainstream or contrarian views.

This said, let me briefly use Glantz’ terms to identify a number of key figures among hawks and doves. The debate, as carried out in the media and in the current Congress tends to involve the same handful of players on both extremes, often pitting them against each other. The prominent contrarians, or “doves,” who appear again and again in these arenas are: Fred Singer, Patrick Michaels, Robert Balling, Sherwood Idso, Hugh Ellsaesser, Richard Lindzen, plus a few others, such as George C. Marshall Institute affiliated scientists, Frederick Seitz and William Nierenberg. These scientists are not equally respected by mainstream scientists, though Lindzen and the Marshall Institute affiliated scientists have significant status and impressive resumes, even as their expertise in the field of climate change is contested to various degree.

Prominent hawks are scientists such as Stephen Schneider (formerly at NCAR, now enjoying an endowed chair in the biology department at Stanford), the late astrophysicist Carl Sagan, and Thomas Wigley (Australian scientist, now a senior scientist at NCAR and lead author of the IPCC), along with administrators and directors of major labs and programs, such as James Hansen (NASA-GISS), and IPCC leaders Bert Bolin and Sir John Houghton. Many

mainstream scientists also fit the hawk description to the extent that they find the evidence for human-induced climate change very strong. Unlike most hawks, however, most mainstream scientists are unwilling to make assertions to the effect that the signal of human-induced climate change has been detected out of the noise of naturally occurring climate variability.

Stephen Schneider, James Hansen and Carl Sagan were particularly prominent and outspoken in the years immediately following 1988, while (gauging from their representation in the media), Bert Bolin, and Sir John Houghton and Tom Wigley -- the prominent IPCC leaders -- seem to have dominated "hawk ranks" since the early to mid-90s. They are currently the ones willing to make the strongest claims to the effect that human-induced climate change has been detected. They also tend to emphasize the potential destructive ecological and socio-economic consequences unless humans change their ways.

Most scientists appear to be owls.<sup>10</sup> As one scientist put it when I asked what he thinks of the prevailing opinion about human-induced climate change among scientists in the field:

LEITH: Well, most people, I suppose, are in the middle, in any case. The other aspect of this, which has been noted is: most people involved in climate research who are taking any particular stand on the matter, one way or another, recognize the weaknesses of the models and they are trying to do whatever they can to find out where they are and where to fix them, and often don't want to get embroiled in political arguments, in any case, but would rather just continue to work on their own research. I

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<sup>10</sup> This is also the perception of Michael Glantz, an observer and commentator on the debate who has been in the field for decades.

think, and it is a little hard to change, I think most people recognize that there is a large range of uncertainty. They also recognize, and I think that it is rather clear, that it is the people who happen to have views on the extreme ends, one way or another, who make the statements which are the most interesting to read in the newspapers. Frankly, if a reporter goes to a scientist and asks "What do you think about all this climate stuff?" and he says, "Well, it's a very complicated business and there are large uncertainties," the reporter walks away because there is no easy way of writing anything very interesting about the fact that it's a very big and complicated matter with nobody knowing exactly what the truth is. So the people who make extreme statements are the ones that get quoted. And of course this leads to the controversy and so on, which makes it look a lot more exciting, so far as the popular press is concerned.

***Contested culture, complicated field-sites:  
anthropological studies of science***

To complicate ethnographic study yet further, my research isn't limited to scientists; I have chosen to focus on how science is constructed not only inside scientific circles but in society at large, such as to render evident how knowledge and the symbols of science are employed to serve social purposes.

Conceptualizing culture as multivocal, fragmented, and contested, I have designed my field-site in the broader way characteristic of anthropological studies within the field of Science and Technology Studies (Hess 1992; Hess 1993; Toumey 1996; Zabusky 1995). Thus, my research within the U.S. has included study of not only scientists but also politicians, environmental activists, fossil fuel industry representatives, and figures of the U.S. right-wing and libertarian movement, all of whom have variously sought to support, challenge, redefine and reconstruct the scientific basis for concern about human-induced global warming. The conceptualization of culture as porous and often

amorphous -- as involving an array of not always coherent patterns and intersections of distinct processes crisscrossing from within and beyond its borders -- is associated with a recent recognition in anthropological work that processes of globalization have brought about an unprecedented level of interconnectedness between different societies, cultures, subcultures and socio-economic and political processes (Rosaldo 1989). Given this general framework, I consider it inappropriate for me to use Bourdieu's term "scientific field" (Bourdieu 1975:22), even though this term appears useful to the extent that it avoids the discussion on community that I am engaged in here; the term does not apply well when the actors studied are not limited to scientists.<sup>11</sup>

Due to the recurrence of the same names of individuals and groups in the public debate about climate change, I will argue that there is a kind of field, or even "community," that has developed with the focus on human-induced climate change --albeit not a community in the above-described idealized sense of the term. In the U.S. debate about human-induced climate change, the same names appear again and again in data collected both from interviewees and from media articles and other literature concerning climate change.<sup>12</sup>

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<sup>11</sup> I am also reluctant to use this term because I think that some readers may mistake "the scientific field" for scientific fields in the sense specialized areas of scientific investigation within the larger "scientific field".

<sup>12</sup> During my interviews, I typically asked interviewees for suggestions about who to talk to and what kinds of questions to ask, what issues to probe. The responses from such questioning, in addition to media articles, have led me to be confident that I have included interviewees espousing view points that span the range of differences in perspective; while it was not possible to talk to every single actor or influential party in this debate, it soon became obvious who the important actors were, and I could ascertain that I had included many of these subjects, or at least individuals of the most prominent groups constructing the threat of human-induced climate change in conflicting ways. I feel that I have additional research to do to map more precisely the smaller divisions and differences within the mainstream scientific community, e.g., radiation transfer specialists, different kinds of biologists, meteorologists, etc. However, my study is built on

STS literature labels the set of scientists who contribute to a controversy “the core set.” The “core set” of scientists and their supporters (e.g., environmental activist organizations and politicians) in my study have come to know each other as allies or opponents -- albeit in some cases circumstantial and tentative allies and opponents. They continually engage with each other and shape each other’s discourses, through the mediation of journalists and other actors, or directly at public forums or Email correspondence (see for example examples of this in my chapter on the controversy over Chapter 8 of the 1995 IPCC report).

To repeat, then, I do not mean to evoke an understanding of community à la Emile Durkheim -- i.e., as implying unity and order -- when occasionally referring to the “scientific community” or to the “larger community” involved in constructing and contesting theory and evidence of human-induced climate change. Rather, one should understand my use of the notion of community as somewhat ironic, similar to Herzfeld’s use of the term “Western” in his study of the symbolic roots of “Western Bureaucracy” (Herzfeld 1992). Herzfeld uses the term in an intentionally ironic way, aware that it enshrines a stereotype and that it lumps together diverse countries under a shared identity. As a term, “community” -- like the term “Western” -- belies the connotation of coherence, and it acquires different meanings in the hands of different actors and in different situations.

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the implicit argument that such differences are of less importance than those between the clearly dominant actors in the larger debate, whose names appear again and again.

### ***Problems and Dynamics of “Studying up”***

#### **From “primitive tribes” to elite scientists: Why anthropology now studies scientists**

The increasing use of anthropological approaches in social studies of scientific knowledge production has converged with changes within anthropology towards the study of complex societies as well as simpler societies, of “Western” societies as well as non-Western societies, and of elites as well as the less powerful within societies.<sup>13</sup> Especially recently, some anthropologists have shifted their attention from “natives” abroad and in “less developed” countries to “natives” in the more developed parts of the world, including elites such as scientists. An important reason for this shift was a general recognition of the problematic power aspects of traditional anthropological studies. These studies were typically conducted by Euro-Americans and focused on non-Western ethnic populations, and they tended to reinforce existing power structures; they usually assumed Western superiority, integrating an ethnocentric, if not an even more problematic evolutionary, perspective. These studies often ignored that Westerners themselves are shaped by particularities of environment and social institutions.

Critiques of this tradition within anthropology are generally associated with “New Ethnography” (Hess 1992; Clifford and Marcus 1986). The critiques, stimulated by the process of globalization, reoriented more anthropological



studies towards study of “Western,” complex societies; in an increasingly globalized and stratified world, the “other” is no longer necessarily geographically distant, nor necessarily of different ethnic or national origin. At the same time, anthropologists started to recognize and problematize their own tendency to focus on the less powerful groups in the world and within particular societies (Nader 1988). With the New Ethnography, awareness also grew that knowledge of the culture and dynamics of powerful elites in the developed world often is lacking, and that such knowledge is important to understand the world and to examine the power dimensions of prevailing understandings of the world (Rosaldo 1989).

### **The dynamics of a social scientist graduate student studying physical scientists**

#### ***Intimidation***

The project of studying “up” as a graduate student is quite intimidating. With my academic background in literature and cultural anthropology, I began fieldwork with minimal understanding of the scientific issues involved. I found it difficult to break into the research, unsure what questions to ask and afraid of revealing my profound lack of knowledge about what I claimed to be studying. The most difficult part for me was to know what I wanted to ask about, what the

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<sup>13</sup> With the post-Mertonian focus on the social nature of scientific knowledge itself, traditional anthropological methods of in-depth interviewing and participant-observation have grown to be particularly important to social studies of science (Pinch 1992).

important issues were to my informants and to myself. Intimidation about the insufficiency of my scientific knowledge concerning climate change and NCAR scientists' scientific practices also contributed to my lack of participant observation among them. I was afraid that I might lose credibility among the scientists if I revealed the extent of my scientific illiteracy -- the public's scientific illiteracy being a frequently identified social problem in scientists' discourses already in my initial interviews.

My initial conversations with climate scientists -- and with climate modelers in particular -- made me aware that their work didn't consist in what I had initially assumed, furthering my intimidation. I originally expected scientists using the GCMs to be of similar mind-sets to the scientists performing the global model-based ecological studies associated with the Limits to Growth, who gained significant impact through the Club of Rome (Meadows 1972). The Limits to Growth researchers had employed global simulation models to consider future consequences of current unsustainable global trends in terms of population growth, land use practices and consumption patterns, among other things. I had read the work of the Limits to Growth authors and expected atmosphere scientists' simulation of future consequences of current trends in greenhouse gas emissions to involve similar consideration and critique of the current global socio-political and environmental situation. Some initial conversations with modelers gave me a sense that my assumptions about what modeling involved did not entirely fit the descriptions I got from scientists with intimate knowledge of

what this modeling involved. While I didn't have an understanding of what it was I had wrong, I was sufficiently aware of my ignorance to give in to a certain intimidation, which led me to delay seeking out direct contact with modelers for quite a while. For many months, my direct contact with NCAR climate modelers was mostly limited to occasional fragmented conversations during lunches when I happened to sit at tables which included modelers and they heard that I was at NCAR intending to study them.

*Coached self-representation, delicate positioning*

During the very initial phases of my research, before having even identified a place to do fieldwork, I contacted by phone Bob Ginsburg, a scientist who had done some work with a Los Angeles think-tank involved with environmental and social justice issues. Having obtained his name through personal networks, Ginsburg was very helpful and informative, providing candid recommendations about how to position and represent myself among scientists such as to secure their respect and cooperation. His recommendations highlight some general differences between traditional fieldwork and repatriated fieldwork, and between studying "down" and studying "up."

First of all, I had to seem unthreatening. Thus, in our phone conversation, Bob Ginsburg suggested how I might phrase my study in ways that would make it sound less threatening to the scientists. He suggested that I not say (as I said to him) that I wanted to "compare the competing theories" concerning climate change, as this easily could be understood as involving a "better or worse

framework." He thought this might sound threatening to the scientists involved, besides begging the question of who I was to be evaluating this. Instead, he suggested that I present myself as doing "a logical analysis" of the different models, of how they are developed, of their constituting elements, and of their dissemination, including how funding agencies prioritize among them, and between them and other scientific enterprises. The point of my work, Ginsburg suggested I say, would be to place the different models on a level playing field, with view of developing evaluation criteria and, with that, funding criteria for the models; it could help develop a framework for technically literate groups to evaluate global warming theories.

Secondly, I needed to make clear to scientists that my work was targeted for a technically literate, policymaking audience, and to present myself as doing "good science." Ginsburg explained that saying that I'm doing my study to educate the general public won't work, that "few people care about the masses; It is when technically literate people decide that something is a problem that something is done about it; it is the policy people who have to be convinced." Thus, rather than saying that I wanted to educate the general public with my work, I should say that the intended audience were policy people who are technically literate but not experts on global warming issues; I should present my study as aimed at describing the essential elements necessary to understand the limitations of the models for a technically literate group review. Framing my study as considering the theories and models as subject to available data, and as

developing a comparative framework for evaluating the data, would be perceived as “good science” and hence increase my chances of being met with acceptance and cooperation by scientists.

Thirdly, I had to find a physical location that didn't position me at either end of the spectrum of positions on human-induced climate change. When I asked Ginsburg about a good place to do my research, he recommended that I not officially associate myself with Greenpeace or other environmental groups deemed “beyond normal” by many, “extreme” by some; being associated with these organizations would not position me well among scientists. Rather, I should find groups placed “in the middle of the pack” to be with; groups which are recognized but which have a low profile. The same was the case if I chose to be in a scientific organization rather than an environmental organization; if I wanted to be among scientists, I should find a lab that is recognized but “middle,” a good academic institution considered “fairly uncontroversial.”

Finally, while needing to appear non-threatening, I also had to secure some level of credibility and prestige to obtain scientists' cooperation. Ginsburg expressed that getting the scientists to talk with me could be difficult; I needed some level of status, he explained. At the time, I had not obtained funding for my research nor other emblems reflecting my academic and scientific competence. What I needed, Ginsburg explained, was official affiliation with institutions scientists knew and respected; if I were funded by the National Science Foundation, “they'd all have to talk with [me]” because the NSF is their major

funder. "Money talks," he commented, adding that I needed to appear to be one of them, at least to some extent.

*My quest for respect, cooperation and inclusion*

All of this gave me something to work on. Where should I locate myself during my fieldwork? An atmospheric scientist at Rice University had suggested The National Center for Atmospheric Research (NCAR) in Boulder, Colorado, as an ideal location for me. The primary reason was that NCAR, a research organization under primary sponsorship by the National Science Foundation, has a federal mandate to provide out-reach, community services. With an explicit mandate to educate the public and to share resources with universities, NCAR was more likely to be willing to be my host than were other federal research labs. I visited NCAR in the latter part of 1993 and talked to people there about the possibility of obtaining a research position as well as financial support at NCAR. At the time, they had graduate fellowships (this program was terminated the following year). Based on a review of my resume and a meeting we had, the director in charge of NCAR graduate and post-doctoral fellowships expressed that I qualified for a graduate fellowship at NCAR. However, when calling the same director from Texas a couple of months later to obtain details about the process of applying, he had changed his mind. After talking with other people at NCAR, he had decided that it was not possible for me to receive a fellowship from NCAR. He explained that this was because I was from a field too unconnected to the atmospheric scientists; the NCAR graduate fellowships were

structured such that for a student to be accepted at NCAR, they had to find a scientist who would “sponsor” them and agree to work with them. During my visit to NCAR, I had found someone in NCAR’s social scientific Environmental and Societal Impacts Group (ESIG) who reluctantly agreed to sponsor me, but I sensed that that support wasn’t secure, and it appears to have eroded after my visit.

Besides finding an internal sponsor, the NCAR graduate fellowships required that a student’s advisors at their home institution already have ties to NCAR scientists. This was explained to me as a measure by which to prevent student’s advisors from feeling that NCAR was “stealing” their students away from them. This was the reason the director gave for later changing his mind about my eligibility to apply for a fellowship at NCAR. Informally I learned of other possible reasons. One was that a graduate student social scientist affiliated with NCAR’s Environmental and Societal Impacts Group recently had stirred up some problems for NCAR by writing his dissertation on the U.S. Global Change Research Program (USGCRP), the federal program set up under the George Bush administration to oversee U.S. research into global change (of which human-induced climate change is a sub-set). Hearing that this dissertation was in progress and suspecting that it was critical of aspects of how the USGCRP was being directed, scientific administrators in the highest echelons of the national scientific hierarchy threatened NCAR’s directors that federal funding of NCAR might be affected if they did not intervene. This incident has likely alerted

certain persons within NCAR of the dangers of research such as mine, intended to study the socio-political dimensions of climate change research.<sup>14</sup> Finally, by some accounts another factor counting against me in obtaining support from NCAR was that natural scientists also tend to be unwilling to give “turf” to social scientists, concerned that this might take away from their influence and resources. Whether intentional or not, and despite expressed support for interdisciplinarity, the structure of NCAR’s graduate fellowships discouraged newcomers from fields outside of the established disciplines at NCAR.

Having been told that NCAR is an open institution, I nevertheless moved to Boulder, Colorado in April 1994 to be near NCAR, in spite of the lack of financial and institutional support. Besides writing grant proposals, I spent time in NCAR’s library and talking to people around NCAR, using some of the pointers I obtained from Ginsburg in terms of how to present myself and my research. I was received with curiosity and interest -- but as an unmarried female at a traditionally male-dominated institution, it was sometimes difficult to discern the nature of the curiosity and interest.<sup>15</sup>

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<sup>14</sup> The outcome of these power pulls from high up within U.S. atmospheric science administration was that the student was asked to come to Washington to interview among the important administrators who had become concerned about his dissertation; the student was not kept from finishing his dissertation as he pleased. However, it is clear that this type of incident intimidates, and also induces caution, among those studying sensitive issues of importance for powerful science administrators, and among those supervising or serving as host for persons carrying out such research.

<sup>15</sup> At NCAR, female scientists are, by far, outnumbered by men, especially as one moves up in the scientific hierarchy.

<sup>16</sup> The notion of various types of “capital” is based on the work of Pierre Bourdieu. Bourdieu divides capital (goods and resources) into four categories: economic (i.e., financial), social (i.e., deriving from valued relations with significant others), cultural (pertaining to legitimate knowledge), and symbolic (i.e., related to prestige and social honor) (Bourdieu 1991).



Though management initially received me with ambivalence, I managed to obtain financial support from NCAR during the Fall of 1994, a period during which I did research and spent time writing grant applications. This, however, I owed above all to the Old Boys' Network: my informal advisor, Dr. William Gordon, put in a strong endorsement on my behalf among NCAR's management, and a request that NCAR support me while my grant application was pending. Bill Gordon is a space physicist and engineer and former Dean and Provost of Rice University, and a trustee of NCAR. He is a scientist with very high credentials within the larger scientific community, honored with memberships in *both* the National Academy of Sciences and the National Academy of Engineering. In addition, he served as foreign secretary of the National Academy of Sciences, and as vice president of the International Council of Scientific Unions.

In January 1995, I secured the much needed grant through the Ethics and Values Studies program within the National Science Foundation, with part of the grant money supplied by the NSF Atmospheric Sciences division. The NSF grant was needed not just to pay my bills and research expenses: it was needed to secure my status and credibility among scientists. Had I not obtained this grant, I would have had to leave NCAR, and it would have humiliated me among the leadership and scientists at NCAR who knew that I was in the process of applying for this support. The fact of obtaining the NSF grant immediately granted me access to a new level of resources at NCAR; when I learned that I

had obtained the NSF grant, I was now told by Bill Gordon that I deserved an office and a telephone at NCAR, and that I should request one. I was subsequently granted an office (albeit closet-size) at NCAR, along with phone and library privileges, among other things.

As Ginsburg had foreseen, the NSF grant (plus a three-year doctoral fellowship which I obtained shortly thereafter from the Environmental Protection Agency -- a fellowship known among physical scientists who have graduate students) helped establish my credibility and enhanced my access to scientists -- if nothing else in the sense that it enabled me to remain at NCAR. In cases where I tried to set up interviews long-distance, and prior to having met the interviewees in person, I found it useful to send them, usually via Email, my resume and a brief description of my research and research funding. This seemed to put my interviewees more at ease, and to render them more likely to give me a time slot in their usually very busy working days. Sending them my resume also helped them place me within their cultural status system, in spite of the fact that I was an anthropologist, and that I focused on the (subjective, cultural and political) aspects of their world they tend to deny, ignore, or keep at the level of corridor talk.

My official affiliation with NCAR as a "Visiting Scientist" in their Advanced Study Program was particularly helpful; this affiliation provided me with a level of legitimacy. NCAR is a known institution among climate scientists, and it's Advanced Study Program has helped build the careers of many now renowned

scientists. Moreover, the name of the program evokes the prestigious Institute of Advanced Studies affiliated with Princeton. The fact that Rice University was my home institution also helped me secure cooperation: Rice is known and respected among atmospheric scientists, and, perhaps especially so at NCAR and other affiliates of UCAR (The University Corporation for Atmospheric Research). This is because Rice University is one of the member universities of UCAR, a not-for-profit corporation which operates NCAR and which facilitates collaboration between over sixty North American universities. Some administrators and assistants within the federal program overseeing climate research, the U.S. Global Change Research Program, are financially supported by UCAR, which perhaps also played a role in their willingness to talk to me when I conducted research in the Washington, D.C. area. For all of the above reasons, I escaped being mistaken for a journalist, with whom scientists tend to be more circumspect; albeit strange and different, I was, at least in some tenuous sense, “one of them.”

All of this reflects on the changed circumstances of fieldwork when “studying up.” Informants’ respect and cooperation have to be earned in a different way than is usual when “studying down,” with structural affiliations and academic credentials -- social and symbolic capital earned through dominant structures in the researcher’s own society -- taking on primary importance.<sup>16</sup>

### **Methodological problems and implications of studying up**

“Studying up” sometimes requires different methods of anthropological investigation. Pondering the reasons for the preponderance of anthropological work focused on the less dominant in societies, Laura Nader has questioned the argument that the powerful are necessarily less accessible for ethnographic study than are other, less powerful social groups, and for that reason less studied. She attributed the imbalance to (1) anthropologists’ inclination to study “what they like and liking what they study,” generally ‘preferring the underdog,’ and (2) to the fact that elites often can best be studied in nonresidential settings, thus rendering participant observation in its traditional shape impractical, if not impossible (Nader 1988:479-81). Pointing out the importance of developing knowledge of the cultural and power-laden activities in such nonresidential settings as banks, industries and government agencies, Nader argued that though the self-image of anthropologists seems to depend on participant observation, study of powerful groups in such nonresidential settings might require that we “shuffle around the value placed on participant observation that leads us to forget that there are other methods more useful for some of the problems and situations we might like to investigate” (Nader 1988:481). Such methods include interviews of various sorts (formal and informal, conducted face-to-face and via telephone) as well as the use of public relations literature, personal documents, and memoirs.

In the nonresidential setting of scientific research institutions, I have carried out participant observation when attending scientific lectures and other formal or informal discussions among scientists about science, and about the intersection of science and politics. However, participant observation has been less central to my work than have the above mentioned alternative methods, for a variety of reasons.

First of all, given my dispersed research site -- that is, my interest in understanding the production, mobilization and contestation of the science supporting concern about human-induced climate change within a larger, national framework -- studying any one research institution didn't meet my goal (and time and resources did not permit me to perform detailed study of all institutions representing the wide range of actors I wanted to study). Moreover, even to the extent that I wanted to study the scientists at work in their laboratories, I found it difficult to relate the microlevel practice of measuring carbon dioxide emissions from leaves, for example, to the broader political conflict I was concerned to study. This was in part because low- and mid-level scientists in a mainstream scientific lab such as NCAR tend to know little about the larger debate about human-induced climate change; I was initially surprised to find how often scientists themselves weren't aware of who the prominent people in the larger climate debate are; When I talk with mainstream scientists at NCAR and elsewhere, they often ask *me* who the key players are and what they say, do and believe. More often than not, they know of Richard Lindzen, the MIT

Professor who is a widely respected meteorologist, but in terms of the contrarians and the larger political context of their work, including the politics at the higher level of federal funding institutions, their knowledge tends to be limited.<sup>17</sup> Suggestive of how different actors differently positioned in the scientific community and hierarchy (and in relation to the theory of human-induced climate change) have different mental maps of their “community,” I have found that the ones most aware of the larger political debate about human-induced climate change tend to be either higher up (manager positions) in the scientific hierarchy, or to themselves be more involved in the political debate than in scientific research on the issue (usually, such scientists are not part of what I have defined as the mainstream scientific community; some of the contrarians fit this category, as do certain scientists affiliated with environmental organizations).

A second reason why my research relied less on participant observation compared to most anthropological work in residential settings is that the types of scientific practice I studied weren’t very conducive to observation. While it is possible to follow a climate modeler around, for example, it is difficult to closely observe them as they produce their experiments; climate modelers do much of the work in the privacy of their closed offices or even at home. And even if you can observe them in action, all there is to see is a wealth of equations on their computer screens; the deliberations involved in the material production of

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<sup>17</sup> Scientists I have met often pride themselves on not following the broader politics surrounding their science, in the process validating their work by reference to the traditional scientific values of objectivity and disinterestedness. This suggests what Toumey has called “Geertz’ observation” that the scientific research ethos disguises the cultural turmoil that surrounds their science in society (Toumey 1996:161, 164)

human-induced climate change projections are not easily accessed nor observed, just as it wouldn't be easy to derive material of ethnographic richness out of watching academics type away on their computers when writing professional articles etc. In fact, controversies within the scientific community debating climate change (e.g., the chapter on the controversy over the 1995 IPCC report) increasingly center around editorial decisions, the influences on which, like the production of climate models and academic papers, are not easily established.

The issue of access is relevant here too, in that I didn't find any climate modeler whom I was comfortable asking permission to study in great detail and to ask about their experiments, step by step, as they worked. My initial timid inquisition in this regard was not met with open invitations, constituting a third reason why participant observation became a secondary rather than a primary method in my research. In her article on studying up, Laura Nader questioned the extent to which access is a greater problem when "studying up," calling this a proposition that has not "been adequately tested" (Nader 1988:478). Certainly, the anthropological cannon contains many accounts of ethnographers' difficulties in obtaining the collaboration with the people they intended to study; the introduction to *The Nuer* by Evans-Pritchard is only one example of this (Evans-Pritchard 1969 (1940)). However it compares to studying peoples with less institutionalized power than oneself, studying "up" can involve considerable difficulties in terms of access; I certainly ran into problems of exclusion from

important forums of potentially great ethnographic value. I will describe some of the problems in the chapter titled “Spirals of Silence and Dissent,” which discusses the ways in which scientists and institutions avert scrutiny.

Besides exclusion from certain forums, and self-imposed distance due to intimidation of not having the requisite knowledge (i.e., cultural capital) for inclusion and acceptance within the social unit under study, studying an “elite” dispersed “community” presents problems of access in the sense of limitations in terms of time (mine and that of the people I study) and monetary resources to travel to, and have time to “hang out” with, the people studied. Focused formal interviews become a primary method in such circumstances, in addition to indirect study of the written materials listed by Nader. In this regard, there are benefits to studying persons of high public profile: if lack of time or other factors hinder direct contact with persons intended for study, it is in some cases possible to study them through their publications and public appearances. Many scientists have published articles and books and given Congressional testimonies geared to a broader audience about climate change, materials which reveal personal values and beliefs along with perceptions of the climate change debate and the persons, institutions and processes shaping it. As mentioned earlier, these materials are a rich source of data as scientists' discourses are particularly revealing of belief- and value-systems when communicating to broader audiences (Mukerji 1989). A certain imbalance might result from this resort to public records, however, as is to some extent the case with my study: I have



found a preponderance of materials in which contrarian scientists reveal their values and beliefs, compared to scientists who are part of the mainstream, apart from the most prominent cases -- particularly the “hawks” identified above.

### **The threat of libel suits**

When studying particularly powerful persons, anthropologists run a greater risk of being sued for libel. Libel suits are an important weapon among participants in the climate debate whose status, power and influence depend on their public and scientific reputations.<sup>18</sup> As a scientist has noted, ‘the most precious intangible a scientist ever owns is his or her reputation’ (Schneider 1990 (1989):201).

Prior to recording interviews, I always asked for permission to do so. With a couple of exceptions, interviewees did not require any conditions for my doing so, nor did they put any restrictions on how I might use the interview data. In November 1995, I met in the Boston area with Ross Gelbspan who taught me the journalist ethics code for dealing with interviewees and the resulting material: journalists divide persons into two groups, public figures and non-public figures.

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<sup>18</sup> When I interviewed Richard Lindzen the same month a *Harper's* article by journalist Ross Gelbspan was issued, Lindzen said he was considering suing Gelbspan (he eventually decided not to sue, however). In the article, Gelbspan mentions by name various contrarians, including Lindzen, and calls the contrarians “interchangeable ornaments on the hood of a high-powered engine of disinformation” run by fossil fuel industry groups, among other interested parties (Gelbspan 1995). Fred Singer has also sued for libel in the past, as in the case where a scientist suggested that Singer was wrongfully using the name of the highly regarded scientist, Roger Revelle, after the latter had died. After Revelle's death, Singer published an article which listed Revelle as co-author with Singer but contained claims that Revelle's colleagues and family didn't recognize as those of the late Revelle. The contested claims concerned the estimated severity of future temperature changes due to increases in greenhouse gases. Revelle's colleagues and family

Gelbspan is a Pulitzer Prize winning journalist who worked for the Boston Globe for decades. He has published an article in *Harper's Magazine* and, recently, a book on the status and politics of climate science (Gelbspan 1995; 1997). Gelbspan explained to me that public figures are persons who appear frequently in the public eye, and who are accustomed to speaking to the public record and hence also aware of the consequences of doing so. Non-public figures are persons of low-profile with little or no experience of being in the public eye, and who thus are less likely to be aware of the potential consequences of speaking too freely. Gelbspan considered persons such as the high-profile contrarians to be public figures. I will assume that the highest positioned leaders of the federal science agencies and programs would fit into the category of public figure as well.

This distinction between public and non-public figures has guided me to some extent, but in most instances, I decided to send out the interviews I transcribed in full to the respective interviewees for comments as well as input as to what segments, if any, they preferred to contribute anonymously. In many cases, I received no reply, in others, the interviewees took great care to add corrections and comments, including what they wanted to contribute anonymously. When interviewees aren't identified by name in my dissertation, this is either because they have requested not to be identified (this only happened in a few instances, in fact), or because I haven't wanted to personalize

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expressed disbelief that Revelle would have agreed with the level of skepticism regarding serious climate

any critique I may have of the point made. In some cases, it is also because I haven't had the time to have the interviews proof-read by the interviewees and thus formally accepted retrospectively by them. In a few instances I have taken the liberty to quote persons without having had the materials "checked" by interviewees. This decision was usually guided by two criteria: either I judged the quoted passages non-sensitive material, or I relied on the journalistic ethics code outlined by Ross Gelbspan. Never have I identified by name someone I quoted if this person had expressed the wish to remain anonymous or to be quoted only with their approval. With only a few exceptions, people granted me interviews and expressed no conditions for how I use the material. As much as possible, however, I have tried to include names, to allow people to use personal knowledge by which to calibrate the statements and arguments made.

Because of the threat of libel suits, anthropologists may be comfortable following the traditional anthropological standard of 'protecting their subjects at all cost,' as urged by the official code of ethics of the American Anthropological Association -- albeit now at least as much for anthropologists' own sake as for the protection of the persons studied. But this option has its downsides as well. For one, when you are dealing with public figures, it is not easy to conceal their identity, especially not to the persons familiar with the key actors in the debate. Secondly, a study such as mine would lose much of its potency and use if I didn't include the names of key figures who appear again and again; after all, an

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change expressed in the article.

overarching aim of my research here is to provide a context of knowledge with which to calibrate competing expert claims -- which would be difficult if the identity of the different experts was entirely concealed. While I identify certain structural differences as shaping factors in the competing expert positions (see for example the chapter on the older generation of defense-related physicists) there is a considerable component of individualism and idiosyncrasy in which scientists take what positions around the issue of human-induced climate change.

### ***The merits of a discursive approach***

Social scientists at NCAR warned me upon my arrival at NCAR that I might get the cooperation of some physical scientists, but 'only if it didn't seem as though I was trying to psychoanalyze them.' What I did realize was that I wasn't likely to get great cooperation by undertaking any obvious examination of scientists values and beliefs in areas they themselves consider private and unrelated to their scientific work and views. I quickly realized that atmospheric scientists generally didn't appreciate studies attempting to probe what they consider their personal values -- for reasons I presume to be rooted in large part in the ideal of disinterestedness.<sup>19</sup> For instance, one MIT scientist referred with disdain to a questionnaire he had just tossed in his wastebasket because it

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<sup>19</sup> The value attached by scientists to objectivity in research was described by Robert Merton (Merton 1973 (1942)). Later scholars of science have pointed out that rather than constituting a "norm," as Merton suggested, disinterestedness in research is an ideal. As Michael Mulkay has pointed out, It constitutes a description of the legitimate ideology of science which scientists employ in cases of controversy and boundary-work (Mulkay 1991)

probed his position on topics such as abortion. As a result of this resistance on the part of scientists, and to the time constraints on interview situations and on my dissertation research as a whole, I did not probe values and beliefs as directly as I initially intended to.<sup>20</sup>

Another problem of such line of inquiry is that values and beliefs are elusive objects of study. This problem is avoided by adopting the theoretical framework of Michel Foucault, as is, to some extent, the problem of framing a study involving a dispersed and heterogeneous set of actors (Foucault 1972). Besides avoiding simplistic interest explanations, Foucault's framework recognizes the impossibility of accessing perceptions and "worldviews" in any unmediated way, focusing instead on the constitutive function of discourses.

In what Foucault calls "new history," the theme and the possibility of *total history* gives way to something Foucault calls *general history*. *Total history* is marked by the attempt to describe history in terms of continuity between events, to get rid of discontinuity and difference:

The project of a total history is one that seeks to reconstitute the overall form of a civilization, the principle -- material or spiritual -- of a society, the significance common to all the phenomena of a period, the law that accounts for their cohesion... (1972:9)

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<sup>20</sup> I had originally intended to probe scientists' socio-political views in a more systematic way than proved practically feasible, by asking questions such as: Which are the most critical problems facing the world now and in the future? How do you envision the world one hundred years from now? What are the institutions or processes of change? -- in addition to even more detailed and personal questions into fears, assumptions and values. This originally planned line of questioning was informed by studies suggesting that future images reveal sources of fear as well as assumptions and values concerning such things as humans' ability and right to influence and control the natural environment and the possibility, direction and desirability of social change (Bell, et al. 1971; Jones 1980; McKeown 1990).

As such, total history is based on the hypothesis that it must be able to establish between all the events of a well-defined spatio-temporal area a central core which they all express: "A total description draws all phenomena around a single center -- a principle, a meaning, a spirit, a world-view, an overall shape" (1972:10). This approach is challenged by new history/general history which assumes no such center and unifying principle behind disparate events, institutions or structures, socio-cultural, economic, political or technological etc. General history pays attention to plays of difference and speaks more of divisions, limits, differences, shifts and specificities, among other terms denoting discontinuity and difference.

As Foucault notes, the problem presented by doing general history is "to determine what form of relation may be legitimately described" between such disparate actors, processes and events; the field which appears when one suspends the focus on continuity is vast but it can nevertheless be defined:

this field is made up of the totality of all effective statements (whether spoken or written), in their dispersion as events and in the occurrence that is proper to them. [...] [T]he material with which one is dealing is, in its raw, neutral state, a population of events in the space of discourse in general. (1972:27)

What is required, therefore, when seeking out the totality (of all effective statements) is description of all the discursive events bearing on the larger field, a description which asks how each particular statement appeared rather than another. As such, an analysis of the discursive field "must grasp each statement in the exact specificity of its occurrence" and it must "determine its conditions of

existence, fix at least its limits, establish its correlations with other statements that may be connected with it, and show what other forms of statement it excludes" (1972:28).

The impossibility of knowing the intentions and motivations of various actors has led social theorists to turn their attention to discourses, aware of the level of construction involved in utterances, and of the impossibility of comparing such utterances with some unmediated reality.<sup>21</sup> As Foucault writes, when delineating his theory of discourses:

It is also clear that this [i.e., Foucault's own] description of discourses is in opposition to the history of thought. There too a system of thought can be reconstituted only on the basis of a definite discursive totality. But this totality is treated in such a way that one tries to rediscover beyond the statements themselves the intention of the speaking subject, his conscious activity, what he meant, or again, the unconscious activity that took place (Foucault 1972:27).

In the "history of thought," analyses have sought towards "total description:"

A total description draws on all phenomena around a single center -- a principle, a meaning, a spirit, a world-view, and overall shape... (1972:10).

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<sup>21</sup> An analysis of the relationship between positions on climate change and values and beliefs is difficult and potentially problematic insofar as it is impossible to know these in any unmediated form, and to know the cause and effect relationships between different expressed views. For example, if a group of scientists consider the science on which the theory of human-induced climate change is based to be "good" or "bad science," it is virtually impossible to know whether their position on the issue was preset, perhaps due to its policy implications, which in turn influenced their definition of good or bad science, or, inversely, whether their views regarding what constitutes 'good' or "bad" science in fact caused their skepticism concerning the theory of human-induced climate change. Sheila Jasanoff has noted that the distinction between research science and "regulatory science," introduced by Alvin Weinberg, opened the way for "politically loaded boundary work." Weinberg suggested that the branch of science involving prediction should be treated as a "new branch of science" called "regulatory science," and he identified regulatory science as involving norms of proof that are less demanding than are the norms in ordinary science. Jasanoff writes that scientists and other actors dissatisfied with policy outcomes gained easy political leverage by labeling regulatory science as "bad science" (Jasanoff 1990 (1994):78), thus suggesting that the label of "bad science" was a function of extrascientific values and interests.

Foucault points out the inherent difficulties and contradictions of efforts to identity and describe totalities such as “worldviews,” contrasting frameworks undertaking such attempts with his own study of the “discursive field” under the rubric of “general history:”

The analysis of the discursive field is oriented in a quite different way; we must grasp the statement in the exact specificity of its occurrence; determine its conditions of existence, fix at least its limits, establish its correlations with other statements that may be connected with it, and show what other forms of statements it excludes. We do not seek below what is manifest the half silent murmur of another discourse... (Foucault 1972)

As such, general history seeks to explain not a whole but the “space” or “system” of a dispersion of varying and often conflicting uses of a statement (or an argument, an idea, a theory etc.)

In a study such as mine, the description of a totality defined by spatial or temporal limits is hardly possible. Thus, whereas Bourdieu’s notion of “scientific field” proved too narrow to apply to my field site, Foucault’s notion of “totality of effective statements” is potentially too broad; a clear, fixed boundary around the general field of scientific and non-scientific actors partaking in the debate about human-induced climate change is not possible, as the field may change -- and, indeed, the configuration of actors in each instance of contention around human-induced climate change differs to some extent.

Nevertheless, due to the dispersed location and heterogeneous character of the various actors and influences in the field of discourses bearing on human-induced climate change, Foucault’s framework is useful for studies particularly



resistant to total description, as is the case with my study of actors engaged in climate rhetoric. Foucault's framework seeks to discover and describe the relations between discourses and events which manifest themselves in a discontinuous way but which nevertheless bear a relation to each other.

Foucault's framework thus allows for an exploration of relations between what can appear to be disparate processes, events, and institutions. For this reason, social scientists studying environmental phenomena have applied Foucault's theoretical framework to study the heterogeneous playing fields characteristic of controversies around new environmental problems (Litfin 1994; Hajer 1993; Hajer 1995).

### ***Discourse coalitions***

Based on Foucault's framework, among others, Maarten Hajer has developed the discourse coalition approach to new environmental problems, an approach I find useful for my purposes because it is able to account for simultaneous convergence and divergence within heterogeneous coalitions of actors in the controversy over human-induced climate change. Also of great value to my work is the fact that in his research on the acid rain controversy in Britain and the Netherlands, Hajer identifies two major discourse coalitions which in important ways resemble the two major conflicting discourse coalitions around the debate about human-induced climate change.

A discourse coalition is the ensemble of actors sharing a particular “story line”, and the practices that conform to such a story line. The discourse coalition approach provides a way of analyzing strategic action within a larger socio-cultural, historical and political context while transcending reductionistic interest explanations and the problem of what constitutes a community (even though that discussion appears necessary for anthropologists). This framework places actors within a larger context of discourses and organizational practices, showing how they help to perpetuate or contest a given bias -- something which they may do without necessarily being conscious of this fact, and without necessarily sharing deep values.

As Hajer persuasively argues, in their search for consistency, analysts looking for internally consistent paradigms or deeply held beliefs that motivate the political actions of specific actors overlook the many contradictions and ambiguities of environmental discourse and discourse coalitions (Hajer 1995:78). In this context, Hajer also points out that verbal expressions are shaped by an intractable complexity of contextual factors and intersubjectivity:

The reconstruction of paradigms or belief systems excludes the intersubjective element in the creation of discourse. It overlooks that in concrete political situations actors often make certain utterances to position themselves *vis-à-vis* other actors in that specific situation, emphasize certain elements and play down others, or avoid certain topics and agree on others (Hajer 1995:78-9)

In environmental politics, problems are often complex, comprising many different elements, with the consequence that the political arguments of actors typically rest on more than one discourse at a time (Hajer 1995). Thus, a

persuasive argument for or against remedial action on behalf of acid rain as well as human-induced climate change must combine scientific and engineering arguments as well as economic and political considerations, thus drawing from many different discourses. According to Hajer's framework, when groups share a combination of discourses around the same social construct, they share a "narrative" or a "story line" (Hajer 1993). Actors from widely diverse backgrounds may thus "cohere" in the sense that they share a particular social construct, presenting it within a similar "story line." The discourses of such diverse actors can hold together through their "discursive affinity"; while different in origin, such actors can share important ways of conceptualizing the world. Hajer provides an example from pollution politics where discursive affinity exists between the moral argument that nature should be respected, the scientific argument that nature is to be seen as a complex ecosystem which is insufficiently understood, and the economic idea that pollution prevention is actually the most efficient mode of production (a central tenet of the discourse of sustainable development) (Hajer 1993:47). Hajer associates the discourse coalition subscribing to this series of arguments with what he calls "ecological modernization." The same arguments are found among the scientific mainstream and the within the IPCC, which aligns actors on this side of the debate about human-induced climate change with ecological modernization.

In this dissertation -- albeit without consistently applying Hajer's terminology and framework -- I seek to identify and analyze the discourse coalitions that have formed around the issue of climate change.



**Chapter 2.**  
**THE DETECTION AND ATTRIBUTION OF CONSPIRACIES:**  
**THE CONTROVERSY AROUND THE 1995 IPCC REPORT**

This chapter analyzes the formal and informal institutions established to variously contest and support concern about human-induced climate change, popularly known as “global warming.” 1988 was the year global warming hit the headlines in the U.S., when atmospheric scientist Jim Hansen of NASA's Goddard Institute of Space Sciences testified before Congress asserting “99%” certainty that human emissions of greenhouse gases already were causing severe changes in the global climate. Other scientists were soon heard supporting his concern about potential future consequences, with statements to the effect that “the problems unaddressed have the potential of turning the world into a chaos not greatly different from that produced by global war” (Lawson 1990). Encouraged by the example of the 1987 Montreal Protocol to reduce global emissions of stratospheric ozone depleting CFCs, many concerned about global warming hoped to gain the same level of public and diplomatic support of action to reduce global emissions of greenhouse gases. The success of influential mainstream scientists in creating concern about the issue soon created a backlash among some scientists, politicians and socio-economic forces in U.S. society. The backlash comprises accusations concerning the motives of scientists and environmentalists stirring up public concern, and both

sides of the debate involves new types of institutions, new approaches, and new alliances between entities respectively concerned to promote and reduce public concern about human-induced climate change. The backlash has, in turn, provoked a wealth of equally acrimonious counter-accusations, rendering the scientific controversy -- the subject of this chapter -- bitter and rife with conspiracy theories.

As a result of concern about humans' possible "dangerous interference with the climate system," the United Nations' Environmental Program and the World Meteorological Organization set up the International Panel on Climate Change (IPCC). Designed to assess the science of climate change, the IPCC provides the scientific reports informing international negotiations under the United Nations Framework Convention on Climate Change (FCCC) to reduce global emissions of greenhouse gases. The first (1990) IPCC report expressed concern but also uncertainty regarding the reality of human-induced climate change. It was thus a significant new development when the IPCC released their "1995" report in June 1996, concluding that although observed temperature changes could be due to natural variability, "the balance of evidence suggests a discernible human influence on global climate" (Houghton, et al. 1995). This conclusion led many environmental groups and political leaders, nationally and internationally, to call for controls on fossil fuel consumption in favor of renewable energy sources. The 1995 report was released one month before the first meeting in Geneva of the Conference of the Parties (CoP) under the FCCC

to discuss international reductions in greenhouse gas emissions. Key to the report was Chapter 8 which assessed the science seeking to detect changes in the climate record and to establish whether observed changes can be attributed to humans. The chapter concluded that a “human signal” in the climate record seemed to emerge with recent improvements in the understanding and simulation modeling of the climate system.

Then, on June 12, 1996, the month the IPCC report was issued, an op ed appeared in *The Wall Street Journal* by Frederick Seitz, a now retired scientist with an impressive resume including past posts as president of the American Physical Society, president of Rockefeller University and president of the National Academy of Sciences.

### “A Major Deception on Global Warming,”

stated the large title of the letter. The letter concerned revisions to Chapter 8. Suggestive of his social concerns and values, Seitz feared that policy-makers would act to reduce greenhouse gas emissions based on the IPCC report, something he assumed to have “an enormous impact on U.S. oil and gas prices” and an “almost certainly destructive impact on the economies of the world.” He wrote that like other IPCC reports, this latest report is held in high regard because it has been peer-reviewed and approved by an international body of experts. But, he warns, things are not what they seem; there is a

deception involved on the part of scientists who made final editing decisions of Chapter 8:

[T]his report is not what it appears to be - it is not the version that was approved by the contributing scientists listed on the title page. In my more than 60 years as a member of the American scientific community, including service as president of both the National Academy of Sciences and the American Physical Society, I have never witnessed a more disturbing corruption of the peer-review process than the events that led to this IPCC report (Seitz 1996).

The IPCC is divided into a steering committee, and three working groups dealing, respectively, with the science of climate change; impacts, adaptation, and mitigation of climate change; and economic and social implications and policy responses. Within each working group are "Lead Authors" of individual chapters, usually consisting of three to four scientists, plus a dozen or so contributing authors. The chapters are drafted, then sent out to be peer-reviewed, a process involving scientists of varied persuasions and affiliations, including national research labs, industrial and environmental groups, governments, and universities. An estimated 2500 scientists worldwide were involved in the peer review of the 1995 report. The chapters and summaries constituting the report have to be approved at a final plenary meeting where all participants in the process - government organizations, industry groups, and non-governmental organizations - interact with the scientists to forge the language by which to state current scientific knowledge of climate change. This is where the wording and general presentation of chapters and conclusions are decided upon.



Governments participating in the IPCC process had accepted a draft of the chapter at a meeting in Madrid in November 1995, and all participants in the process had accepted the draft at the full plenary meeting in Rome the following month. Seitz wrote in his op ed that the version agreed upon in Madrid 'kept the participating scientists and the IPCC honest,' but that this version was changed afterwards, without proper authority; 15 sections had been changed or deleted, he charged, with the effect "to deceive policy makers and the public into believing that the scientific evidence shows human activities are causing global warming." Seitz singled out the Chapter's Convening Lead Author, atmospheric scientist Benjamin Santer, as most likely responsible for the changes.

Seitz's charges of deception and conscious plotting for political gain provide a quick introduction to the style of argument that characterizes scientific controversy about climate change - a controversy taking place through the media and involving scientists but also environmental groups, politicians, conservative think tanks, fossil fuel related industries, and PR firms, among others. A majority of climate scientists express some degree of concern about the possibility of human-induced climate change and no one disputes evidence that industrialization processes have increased atmospheric concentrations of the heat-trapping greenhouse gases. However, as mentioned earlier, significant scientific disagreement exists concerning the consequences; to the extent that there's agreement that the net effect will be increases in global average temperatures, the size, timing and impacts are subjects of debate. Seitz is

among the scientific faction of contrarians, which I characterized above as particularly staunch in their opposition to concern about human-induced climate change as well as a list of other issues of environmental concern. The contrarians have been unrelentingly active, vocal and high-profile in their attacks of mainstream scientific pronouncements of increasing scientific certainty and concern about the possibility of human-induced climate change. As a result, contrarians have been extremely influential in the U.S., despite the fact that they constitute a group of less than ten in the U.S..

Most broadly, in this chapter, I will (1) show the pervasiveness of conspiracy theories in the debate about human-induced climate change, and (2) render evident the disconnect between such theories - with their suggestions of tightly organized, deliberate and sinister acts of deception - and the actual complexity of actors and decisions that resulted in the revisions. I will show that the drafting of the report and editorial changes were made in a context of much less clarity and coherence, and with no clear acts of deliberate deception; it was, rather, a context characterized by imprecise and indeterminate knowledge, meaning and rules, and involving inherently “messy” processes of negotiation of different possible representations, each involving different sets of interests and values. This study identifies among scientists and groups on both sides of this controversy a tendency noted by Richard Hofstadter in his writing on conspiracy theorizing and paranoia in American society:

The typical procedure of the higher paranoid scholarship is to start with...defensible assumptions and with a careful accumulation of facts, or

at least of what appear to be facts, and to marshal these facts toward an overwhelming 'proof' of the particular conspiracy that is to be established. It is nothing if not coherent - in fact the paranoid mentality is far more coherent than the real world since it leaves no room for mistakes, failures, or ambiguities. (Hofstadter 1967:36)

In addition to showing the assumptions of conspiracy, this case study shows the ease with which unverified claims and suggestions of conspiracy are disseminated among sympathetic audiences. I suggest some reasons why and how this happens and illustrate how claims or suggestions of wrong-doing are changed and even further exaggerated in the process of their dissemination.

### ***Newness of the Process and Imprecise Rules***

Climate change is one of the new frontiers of science that environmentalism has pressed forward, and the ensuing debates to which these new frontiers generally have given rise has put intense pressures on the traditional methods of fashioning agreement - methods consisting in give and take of open discussion in journals and meetings, and in more private interchanges in 'peer reviews' of research proposals and results. Traditional conceptions of the function and role of science as objective, establishing "truth", and able to bring order to political emotion and factionalism, were shattered in the "environmental era" by scientific controversies revealing disagreement within the scientific community (Hays 1987).

This general context gave rise to the IPCC. The IPCC represents an effort of cooperation and consensus-formation unprecedented within the scientific

community in terms of its size and scope, and pressures to form consensus-position within the community are new to the climate change community as a whole; prior to the recent surge in concern about climate change, climate research was not associated with such urgency and policy-relevance, and the impact of this change within the atmospheric sciences has been profound. An apparent result of the newness, large scope and complexity of the IPCC process, the rules guiding the formation of the 1995 IPCC report were imprecise, sufficiently ambiguous to enable different persons to draw markedly divergent conclusions based on the same formulations. Seitz' charge that the editorial changes were unauthorized was not based on precise knowledge of the IPCC rules; the editorial revisions were neither in clear conformity with, nor in violation of, the rules, which simply weren't precise with regards to the point in the process after which authors must not make more editorial changes. A certain confusion reigns among participants and critics of the IPCC process due to partial knowledge of existing rules, to the imprecision of the actual rules and procedures guiding the process, and due to the almost overwhelming complexity of the processes, procedures, and competing claims involved.

Confusion is only heightened by the involvement of strong vested interests in the climate debate. For example, some fossil fuel industry groups have created organizations and "front groups" with green sounding names and, sometimes, the appearance of being grassroots organizations, obscuring the fact that they are in fact established to counter environmental concern. Oil and coal

companies have spent millions of dollars to hire public relations groups to orchestrate such efforts as well as aggressive media campaigns seeking to undermine public concern about global warming (Gelbspan 1995; Gelbspan 1997). This will be further described in a subsequent chapter concerning the attempts of industry groups to shape public perceptions of human-induced climate change.

When writing and disseminating his criticisms, Seitz went straight to politicians and the media without consulting IPCC leaders and rules. Without specifying the source of his understanding of the IPCC rules, Seitz confidently stated in the op ed in *The Wall Street Journal* that “Nothing in the IPCC Rules permits anyone to change a scientific report after it has been accepted by the panel of scientific contributors and the full IPCC.” He concluded that

If the IPCC is incapable of following its most basic procedures, it would be best to abandon the entire IPCC process, or at least that part that is concerned with the scientific evidence on climate change, and look for more reliable sources of advice to governments on this important question.

To Seitz, a more reliable source would be the conservative think-tank, the George C. Marshall Institute, of which he is chairman. The Marshall Institute will be discussed further in a subsequent chapter (see “New and Old Scientific Elites”).

***The Global Climate Coalition report***

In his op ed, Frederick Seitz suggested that he had “witnessed” what he considered a “corruption” of the IPCC peer-review process. Though this wasn’t clear from his letter, Seitz was not part of the IPCC process; he is not a climate scientist but a now retired physicist whose scientific contribution has been mainly in the field of solid state physics. Seitz himself learned of the charges from elsewhere; they had circulated in informal networks between scientists, industry groups and politicians before their debut in the mainstream media with Seitz’ op-ed letter in *The Wall Street Journal*.

I first learned of the charges two weeks prior to Seitz’ letter when interviewing another Marshall Institute affiliated scientist, physicist William Nierenberg, Director Emeritus of Scripps Institute of Oceanography in California. Nierenberg was co-author with Seitz and Robert Jastrow of a 1989 Marshall Institute report (Jastrow, Nierenberg and Seitz 1989) that centrally informed the Bush Administration’s position on human-induced climate change (Rowlands 1995:80). In his interview with me, Nierenberg referred to a document about the changes to the IPCC report that he had received through two different channels. He had not yet carefully read the document, Nierenberg said, but he relayed with confidence what had happened: the changes had been done by people in the highest echelons within the IPCC, without the knowledge and consultation of the lead authors of the chapter.

**NIERENBERG:** What it is, there is a chapter of which Tim Barnett, Wigley, Santer and four or five others were authors, and about twelve people were

advisors. It is a standard procedure. And what they were writing about was the detectability of - I forget the exact details about that chapter. And they finished it, and they sent it in. It had been reviewed, and so on. And the editors that finally put it together - they have done it before, but not in this way - they went ahead and edited it, to take out just about - I don't know how to put it; it just altered the *whole meaning* of the document. *Without* permission of the authors. In fact there is an editorial in *The Washington Times* about this. [He finds the document, leafs through it] They call it scientific cleansing now. It's got a new name. [...]

LAHSEN: and the changes, you feel, really change [the meaning]?

NIERENBERG: Well *you* can decide. It's uh [scoffing laugh]. People think that it is just *outrageous*! What is more, it was done without - they are not English changes, you know, grammatical changes, it was done - they never consulted the authors [...] Anything that would imply that the current status of knowledge is so poor that you can't do anything is struck out.

Nierenberg's rendition contradicted that of Seitz, who speculated that the convening lead author likely was responsible for the changes; by shifting the responsibility for the changes from the lead authors to unnamed and unknown people with no official editorial authority, Nierenberg added a new, more conspiratorial twist to the story. Nierenberg's rendition suggests the role of misunderstanding and misinformation in the dissemination of conspiracy theories surrounding Chapter 8. It also suggests the role of trust in the sources of information; how such charges of conspiracy can be accepted as valid and based on clear facts without being verified, as long as the source is trusted. When I asked who the report was by, Nierenberg suggested that that was irrelevant: "Oh - it just - it doesn't matter by whom! All they did was they took the final report and compared it with what was sent in for publication."

The source of the charges, I later learned, was the Global Climate Coalition (GCC), a Washington DC-based lobby group formed by about sixty companies and trade associations from energy and manufacturing sectors, including ARCO, Amoco, Texaco, Phillips Petroleum, British Petroleum America, Shell Oil, the National Coal Association, and the American Petroleum Institute. A GCC document, entitled "The IPCC: Institutionalized 'Scientific Cleansing,'" was sent to reporters, congressional representatives, the White House, and certain scientists. The document outlined the revisions and compared the draft of Chapter 8 that had been accepted in Madrid to the final, published version. Like Seitz, the GCC identified not unnamed higher-ranked officials but the Lead Authors of the chapter as the ones most likely responsible for changes, similarly alleging that the changes were unauthorized and "politically motivated," intended to suppress scientific uncertainties and to thereby increase scientific support for attribution of changes in climate to human activities. As a lobby group for fossil fuel- and related industries, the GCC does not hide its resistance to greenhouse gas emission reductions. Prior to the meeting of the Conference of the Parties (CoP) under the FCCC in Geneva, officials associated with the GCC publicly expressed concern that the 1995 IPCC report would result in regulations to reduce greenhouse gas emissions from fossil fuel combustion (Feder 1996).

Reflective of the role of trust and of convergence of views in the dissemination of conspiracy theories, the charges by the GCC traveled unchanged and unverified through politically conservative channels receptive to



the GCC's anti-regulatory, pro-business point of view, appearing in publications such as *Energy Daily* and *The Washington Times* (Wamsted 1996). The charges were given credibility without unverification and without consultation of alternative sources for perspective, defense, or deeper knowledge about what happened and about the actual IPCC rules and procedures. Within a short period, other articles appeared in the fossil fuel industry underwritten *World Climate Report* (World Climate Report 1996), in *The Financial Times*, *Energy* and *The Economist* (Feder 1996). Articles about the controversy that did present alternative perspectives (e.g., IPCC spokespersons) appeared in other publications, including *Science* (Weiss 1996), *Nature* (Masood 1996), *Physics Today* (Feder 1996) and *The New York Times* (Stevens 1996a).

### ***Mobilization of the defense: Consequent letters, Emails and exchanges***

The evening of June 12, the day Seitz's op-ed appeared in *The Wall Street Journal*, Convening Lead Author Benjamin Santer (of Lawrence Livermore National Laboratory's Program for Climate Model Diagnosis and Intercomparison) sent out an Email appraising some eighty-two colleagues around the world of the developing controversy and urging everyone to write letters of protest to the *Wall Street Journal* and the *Energy Daily*. The defense machinery set into motion. The day following Seitz's op ed, the Union of Concerned Scientists (UCS) distributed through their "Sound Science Initiative" (SSI) internet listserve a message with the subject heading: "SSI Alert: IPCC

under attack!” Describing the charges as an “extremely serious challenge to the integrity and credibility of the IPCC,” the UCS message, like Chapter 8 Convening Lead Author, Ben Santer, urged all recipients to write letters to *The Wall Street Journal*. They called upon everyone to monitor their local newspapers for other attacks on the IPCC and to respond to them in defense of the IPCC. The Email asserted that Seitz’s allegations were categorically false and that “There has been no politically-motivated doctoring of the IPCC report, and the [IPCC’s] own procedural rules have not been violated.” The UCS did not describe how it could ascertain the motives of the Lead Authors responsible for the changes. Moreover, again reflective of the role of social networks of trust in the circulation of information and of the potential for unchecked information to be widely distributed (this time on the side of “the defense”), the UCS message did not specify the source of its rendition of the IPCC rules. Yet it provoked a stream of letter writing.

Several scientists who wrote letters in defense of Ben Santer conceded, upon my questioning, that they had not checked the draft version against the published version to verify the nature of the changes, nor were they entirely clear as to the actual IPCC rules and procedures. Similar to Nierenberg on the side of the IPCC critics, many scientists supportive of the IPCC took their position in the controversy - and acted in the form of letter writing - primarily based on personal knowledge of Ben Santer. Actors on both sides are often influenced by mutual dislike and distrust that has built up since 1988 between the two opposing

“camps” of scientists, organizations, and political actors and groups around the issue of human-induced climate change. Also shaping responses in support of Santer was the knowledge among scientists involved in drafting Chapter 8 that Ben Santer had fought during drafting sessions to retain passages emphasizing difficulties and uncertainties associated with detection and attribution of a human influence on observed temperature changes.

Official responses by IPCC scientists to the charges were soon published in *Energy Daily* (June 3rd) and in *The Wall Street Journal* (June 25). The June 25 issue of *The Wall Street Journal* included letters of response by Santer and thirty-nine other IPCC Lead Authors, plus a letter expressing full IPCC support of Santer’s revisions by the top ranking leaders of the IPCC: Bert Bolin, Sir John Houghton, and Luiz Gylvan Meira Filho. “No one could have been more thorough and honest in undertaking that task,” they write, emphasizing that

As the responsible officers of the IPCC, we are completely satisfied that the changes incorporated in the revised version were made with the sole purpose of producing the best possible and most clearly explained assessment of the science and were not in any way motivated by any political or other considerations. It is, of course, easy to take isolated sentences from the earlier version that have been deleted or replaced to bolster arguments or suspicions such as those presented by Dr. Seitz. But that is to misunderstand the nature of the science with which we are dealing and the very open IPCC scientific assessment process.

The officials do not specify what the nature of the science is, perhaps a way for them to suggest that the science is sound *and* to suggest - without having to expand on this - that the IPCC process also requires IPCC scientists to operate

in untraditional and, in the words of an IPCC scientist quoted below, in a “slightly non-scientific mode.”

According to the IPCC and associated scientists, the changes were authorized, the rules observed; the authors of Chapter 8 only acted as required by making changes in response to written comments from scientists, governments and non-governmental organizations (NGOs) before, during and after the plenary meeting in Madrid. They also referred to the official demand of the United States in a letter from the U.S. Department of State (dated November 15 1995) that IPCC chapters *not* be finalized prior to Madrid. The letter stressed that it is “essential,” and “in keeping with past practice” “that chapters not be finalized prior to the completion of discussions” at the IPCC plenary meeting in Madrid, and that “chapter authors be prevailed upon to modify their text in an appropriate manner following discussion in Madrid.” It is unclear, however, that a government can unilaterally dictate IPCC procedures.

The defense by Santer and other lead Authors pointed out (correctly) that the changes did not alter the conclusion of the report that “the balance of evidence suggests that there is a discernible human influence on global climate,” nor the conclusion of Chapter 8 that “Taken together, these results point towards a human influence on climate”; both conclusions in the final report were entirely consistent with those in the draft and were unanimously approved at the Madrid meeting by delegates from nearly 100 countries. Santer et al. stressed that uncertainties were not deleted, which my analysis below confirms. They also

pointed out that some of the deletions were made in response to criticisms of "overlap" between Chapter 8 and other chapters in the same report, an issue that was raised often during the three drafting sessions prior to Madrid. The authors wrote that about half of the information in the concluding summary was integrated with material in another section within the chapter (section 8.6); the section containing the passages described by the GCC, Seitz et al. as deleted did not disappear completely. I will return to this below, showing that the statements Seitz highlighted as deleted indeed can be read into the last section of Chapter 8. However, I will also point out that certain wordings in the chapter did subtly change the presentation of the state of scientific knowledge concerning climate change. Rather than conspiracy, this highlights the unavoidable role of language, judgment and representation in assessment reports of this nature, and the insensitivity to this on both sides of the controversy, at least in their official rhetoric. The question of bias aside, the difficulty of accounting for editorial changes in this kind of scientific assessment, involving fluid processes of judgment and negotiation of meaning, is already apparent.

### ***Analysis of the revisions***

Seitz claims that widespread skepticism among scientists about the theory of human-induced climate change is reduced to mere "hints" already in the draft, with the final version deleting even these - in Seitz's view - too faint expressions of uncertainty. Yet, analysis of the revisions shows that the changes are not as

dramatic as claimed by Seitz and the GCC, and that uncertainties are given substantial treatment throughout the chapter. Allegations that uncertainties were downplayed have to be considered in light of the chapter's actual detailed description of current limitations of the science of detection and attribution of climate change. Simply checking the table of contents of the chapter shows that mention and treatment of uncertainties were not deleted, nor even reduced to "hints"; two entire sections out of the six sections that make up the chapter address uncertainties. One is titled "Uncertainties in Model Projections of Anthropogenic Change," with the following subsections: "Errors in Simulating Current Climate in Uncoupled and Coupled Models," "Inadequate Representation of Feedbacks," "Flux Correction Problems," "Signal Estimation Problems," and "'Missing Forcing' and Uncertainties in Space-Time Evolution of Forcing." The other section devoted to uncertainties is titled "Uncertainties in Estimating Natural Variability" and covers the difficulties of estimating natural variability based on instrumental data, paleoclimate records, and numerical computer models.

Analysis of the statements Seitz listed as deleted shows that the deletions were not as clear-cut or complete as he and others suggested. All three examples provided by Seitz to suggest deletions and deception can be found in, or inferred from, different sections in the final version. Whether the sentences in the final version form a satisfactory equivalent is open to interpretation, however.

Seitz offered the following examples of deleted sentences to make his strong allegations of wrong-doing and deception:

- \* None of the studies cited above has shown clear evidence that we can attribute the observed [climate] changes to the specific cause of increases in greenhouse gases.
- \* No study to date has positively attributed all or part [of the climate change observed to date] to anthropogenic [man-made] causes.
- \* Any claims of positive detection of significant climate change are likely to remain controversial until uncertainties in the total natural variability of the climate system are reduced.

Though Seitz provides no examples of sentences possibly replacing the deleted sentences, such examples can be found in the final version, including the following statement:

Finally, we come to the difficult question of when the detection and attribution of human-induced climate change is likely to occur. The answer to this question must be subjective, particularly in the light of the large signal and noise uncertainties discussed in this chapter.

The recognition that there are only “subjective” answers to “the difficult question of when the detection and attribution of human-induced climate change is likely to occur” implies that there is no conclusive evidence linking observed climate changes to human activities. The references in Seitz’s examples of deleted sentences to lacking conclusive evidence (“None of the studies cited above has shown clear evidence...;” “No study to date has positively attributed all or part...” and “Any claims of positive detection of significant climate change are likely to remain controversial until...” are also arguably summed up in a sentence in the final version not quoted by Seitz that “few would be willing to argue that

*completely unambiguous* attribution of (all or part of) this change has already occurred” (emphasis in original).”

The following example as well show that substantive changes to the chapter are not demonstrated through Seitz’ examples of deleted sentences.

The final version included this segment:

Some scientists maintain that these uncertainties currently preclude any answer to the questions posed above. Other scientists would and have claimed, on the basis of the statistical results presented in Section 8.4, that confident detection of a significant anthropogenic climate change has already occurred (Houghton, et al. 1995:439).

Ending the quote here, one could arguably consider the division of views represented (i.e., into those held by “some” versus “other scientists”) to be potentially manipulative, failing to specify the relative representativeness of the different views. It would have shifted the emphasis from the strong statements listed as deleted by Seitz (“No study to date has positively attributed all or part...” etc.) to sentences that might give the false impression that as many scientists maintain that confident detection of human-induced climate change has been made as maintain that it hasn’t. This would have been problematic, since very few scientists consider human-induced climate change to have been detected unambiguously; the only claim to that effect I know of was made by Thomas Wigley, in a quote in *Nature* described further below (Masood 1995). Even the studies that have come out with the greatest statements of confidence about having detected a human influence on the climate have not been unequivocal; for example, the statistical study by Hasselman et al. (Hasselmann, et al. 1995)



found the human signal in the climate record with 95% certainty - which still leaves a possibility of being wrong. But the chapter doesn't end on the 'some vs. others' argument; it goes on to acknowledge precisely this - that "few would be willing to argue that *completely unambiguous* attribution of (all or part of) this change has already occurred, or was likely to happen in the next few years."

There are, however, some subtle shifts in meaning between the draft and the final version. The word "*completely*" in the above sentence tilts the interpretation towards attribution rather than away from it. Had it been left out, leaving only "few would be willing to argue that unambiguous attribution of (all or part of) this change has already occurred...", it would have incorporated the opinions of more atmospheric scientists.

The editorial changes arguably resulted in other subtle shifts in meaning. For example, the change from "we do not know" when unambiguous detection and attribution might occur to "the answer must be subjective" changes the assertion of unanimous recognition of uncertainty to simply describing this point as "debatable." That can be taken to suggest that some scientists do claim to know. In Email correspondence responding to the charges of wrong-doing, one Lead Author explained the replacement of "we do not know" with "the answer must be subjective" to have been made in response to criticism during the plenary meetings of the "we do not know" statement. Once again, it is very difficult for outsiders to know whether this is true. The importance of personal judgment in deciding whether these editorial changes are satisfactory should be

clear to readers, along with the difficulty for outsiders to know whether the changes were or were not justified by the input received at the plenary meetings.

Of importance for this analysis is recognition of the subtlety of the changes in meaning resulting from the revisions - whether or not the revisions are perceived to be justified, which remains open to interpretation. The subtle changes between the draft and final version of Chapter 8 - at least some of which might have been made in response to the criticism and peer review built into the IPCC process - constitute the foundation from which the critics derived their strident claims of “scientific cleansing”<sup>22</sup> and “major deception” on the part of IPCC affiliated scientists and bureaucrats. As Hofstadter has written:

[I]f for every error ... one can substitute an act of treason, we can see how many points of fascinating interpretation are open to the paranoid imagination: treason in high places can be found at almost every turning ... (Hofstadter 1967:25)

“Error” in Hofstadter’s quote might in this case be replaced by “subtle editorial change.”

### ***Industry involvement: IPCC reports’ multiple influences***

Critics tend to represent the IPCC reports as one-sided documents with an environmentalist activist point of view excluding contesting perspectives and interests, but the reports involve significant ambiguity; in her study of the IPCC, political scientist Sonja Boehmer-Christiansen refers to the reports’ summaries

as “skillful exercises in scientific ambiguity” using “language which simultaneously allowed Greenpeace to call for a target of reducing emissions by 60 percent, and the UK Treasury to conclude that no action was needed until more scientific certainty was available - each citing the same source” (Boehmer-Christiansen 1994a). Also left out of contrarian renditions in the Chapter 8 controversy is the significant role of fossil fuel industries among other anti-greenhouse interests in the drafting of the reports and the fact that concessions also are made by groups who would like the wording to be more forceful.

Ben Santer commented on the role of such interests in drafting of Chapter 8:

We tried to represent the science in an accurate and balanced way. We did not shout "Eureka! We have found the answer!" It became evident during the course of our work on Chapter 8 that powerful interests were intent on skewing the "balance" of the Chapter, and on accentuating the uncertainties rather than what we had learned in the past five years. Such interests would have preferred us to attach three or four caveats to each statement documenting progress in our field. An extreme case of this was the view expounded at Madrid that there were no scientific basis for any statement made in Chapter 8, and that the entire chapter should have been deleted ...<sup>23</sup>

The important role of fossil fuel interests in shaping the report is also described in a *Nature* article about how Kuwait and Saudi Arabia, with clear interests against curbing fossil fuel consumption, held up the three day IPCC meeting in Madrid. They insisted on modification of the report's conclusion that "the balance of evidence suggests that there is a discernible human influence on

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<sup>22</sup> The charges were advanced during the height of the “ethnic cleansing” in former Yugoslavia, creating a discursive link between those atrocities and the revisions to chapter 8.

the global climate," and that the evidence of detection and attribution of a human influence on climate be described as more uncertain than suggested by the draft. Suggestive of the importance of words and representation in the IPCC process, they based their argument in part on the word "preliminary" used in Chapter 8 to describe new climate model-based evidence important for the chapter's conclusions, interpreting this word as suggesting more uncertainty than reflected in the concluding statements. Thomas Wigley, a Lead Author of the chapter, later objected to the two countries' take on that word: "This word," he is quoted saying in *Nature*, "implies that evidence for a human effect on climate change is initial, but clear and unambiguous. It does not mean that evidence of human influence on global climate is uncertain. We did not realize how this word could be misinterpreted." Due to loss of time resulting from such resistance at the plenary meeting, entire sections of the report, though published, remained unapproved and hence less authoritative (Masood 1995).

In a document distributed through Email, Greenpeace complained that these same delegates worked to "weaken" or "over-qualify" many of the IPCC conclusions. One of the examples it provides concerns the overall IPCC 1995 conclusion:

Industry attacked [Chapter 8] all the way through the review process and then at the IPCC Plenary. Without those attacks the conclusion would have been stronger, not weaker, as industry allege. The conclusion that "The balance of evidence suggests a discernible human influence on global climate" was adopted under extreme duress at the Madrid IPCC Plenary, with Saudi Arabia threatening to block the meeting and with only

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<sup>23</sup> Email communication with colleagues.

a few hours time left to complete the adoption of the IPCC report. The Lead Authors present wanted words that were significantly stronger: "The weight of evidence [strongly] indicates a significant human influence in global climate." At least one wanted the word "strongly" inserted. Lead Authors were increasingly side-lined during the final stage of the negotiation over the Summary. Their preferred version clearly conveys more force than the finally agreed upon text, which was very much a compromise pulled together at the last minute.

***The charges are repeated and added to***

The IPCC responses in the *Wall Street Journal* provoked another set of letters on July 11 by Seitz and two other contrarian scientists, Fred Singer and Hugh Ellsaesser, repeating the charges and furthering the theme of secret, self-interested plotting and mystery. Singer is a solar physicist who designed the first satellite instrument for measuring atmospheric ozone. Besides several professorships, Singer has held positions within the U.S. Department of Transportation and the Environmental Protection Agency. He has since abandoned such positions to establish - with mainly "private donations" - the Science and Environmental Policy Project (SEPP), over which he presides. Concerned to disseminate his views, Singer, like other contrarians, has established ties with groups of the political and religious radical right in U.S. society. Singer has received material support from conservative, religious group led by Reverend Moon, which since the early 1980s has worked to build organizations promoting far right-wing politics. He has become a key organizer for scientists skeptical of the theory of human-induced climate change, often orchestrating letter-writing and signature collection campaigns against remedial

action on behalf of human-induced climate change. Hugh Ellsaesser is a now retired meteorologist and Guest Scientist at California's Lawrence Livermore National Laboratory (LLNL). He is isolated at LLNL, where he occupies an office in a trailer all by himself, immediately behind the trailer shared by Ben Santer and dozens of other atmospheric scientists. Communication 'between the two trailers' have broken down and now takes place mainly through letter writing in major newspapers and journals such as *The Wall Street Journal*, *The New York Times*, *Science* and *Nature*.

In this second letter, Seitz repeats his allegation that the changes were unauthorized and suggests again that the IPCC is not to be trusted by the government. He writes:

Of course [IPCC procedures require changes in response to comments], but not after the governments have accepted the final draft. The fact is that someone connected with the presentation of the published version presumably Dr. Santer and others rewrote basic technical material in Chapter 8 with the result that scientific doubts about man made global warming were suppressed.

With the vague "someone connected with," Seitz evokes conspiracy through the theme of not knowing precisely who made the changes. And he repeats his pitch for alternative scientific authorities on the issue:

Clearly, governments will have to look elsewhere than the IPCC for sound science on climate change.

Singer's letter contains even stronger suggestions of secretive plotting and corruption, with the words "revealed" (i.e. something was hidden) and "tampered with for political purposes" in the introducing sentence, simultaneously

bolstering Seitz' scientific authority: "Dr. Seitz, former president of the U.S. National Academy of Sciences, has revealed that a UN-sponsored scientific report promoting global warming has been tampered with for political purposes." Evoking detective imagery, Singer delineates the time frame within which the act of changing the chapter had taken place: "A crucial chapter of the IPCC's report was altered between the time of its formal acceptance and its printing." The theme of not knowing is evoked again when Singer makes a point of the fact that IPCC officials mentioned in a *Nature* article about the controversy (Nature 1996) were "quoted (but not named)." At the time of his letter, official responses by *named* IPCC officials had already appeared in *The Wall Street Journal* and elsewhere, rendering unnecessary his reference to "unnamed" IPCC officials, which works to further envelop the IPCC in an aura of secrecy and unaccountability. A similar suggestion of inherent untrustworthiness of the IPCC is made when Singer calls IPCC officials' denial of wrong-doing "predictable," implying that the IPCC can be expected to put up a front of denial in the face of charges of wrong-doing:

Predictably, there have been protests from officials of the IPCC, claiming that the revisions in their report, prior to its publication, did nothing to change its emphasis. They also claim that such unannounced changes of an approved draft do not violate their rules of transparency and open review.

The words "claim" and "unannounced" similarly cast doubt on the reliability and transparency of IPCC officials' statements and actions. Yet the rules do not specify that all editorial changes have to be "announced."

Both Seitz and Singer emphasize the point that the chapter was altered. Seitz calls the changes “unauthorized,” considering his initial editorial letter to have presented “facts” indicating that Ben Santer “and possibly others” made “major unauthorized changes” to the chapter. Importantly, and by contrast, Singer acknowledges that it is unclear whether or not the changes were or were not in accord with IPCC rules, but then quotes a *Nature* article about the controversy which wrote that “there is some evidence that the revision process did result in a subtle shift ... that ... tended to favor arguments that aligned with the report’s broad conclusions.” Singer’s recognition that the changes might be in accordance with the rules is likely lost to most readers, however, surrounded as it is by suggestions of ill-doing. Thus, immediately following this recognition, Singer proceeds with more of the same subtle but pervasive accusatory rhetoric, suggesting secrecy, conspiracy and repression; he calls the IPCC Summary a “political document” that is “economical with the truth” and that “has problems with selective presentation of facts.”

Singer’s criticism of the IPCC’s “selective presentation of facts” suggests that *unselective* presentation of facts is possible; in reality it is the nature of representations to select and highlight certain things over others, and particularly so when what is requested in this interface between science and policy-making is an *assessment* of the science, an evaluation, that is, which by nature involves judgment and “selection.” The following excerpt from an interview with one of Chapter 8’s Lead Authors reveals the considerations and difficulties that are part



of drafting the IPCC reports. The Lead Author conceded the difficulty of the science-policy interface:

[T]his is meant to be not a review of the science but an assessment of the science, and the audience is the policy-making audience, so it is a case where an international group of scientists is operating in a slightly non-scientific mode. And most of the people are not experts in communicating scientific knowledge or scientific uncertainty in a way that can be understood by policy-makers or policy-advisors. So, boy, the number of reiterations that individual sentences can go through in order to express a particular concept in a way that doesn't overstate but doesn't totally diffuse the issue by stressing the uncertainty. It is a very difficult road to tread, I think. [...] What policy-makers want to know is: 'have we detected the greenhouse effect or haven't we?!' Black or white. But it is not black and white. So it is very difficult to word an assessment of the problem in a way that will provide useful information to the people who need the information, without completely discrediting the scientific approach to the problem.

IPCC official's defense above that the final version of Chapter 8 represented "the best possible" and "most clearly explained" assessment of the science, and that the changes "were not in any way motivated by any political or other considerations," is not directly contradicted by this Lead Author's articulation. Their defense does, however, reveal a lack of self-reflexivity and recognition of the role of interpretation, judgment, and of extrascientific considerations in representations of climate change research. Recognition of the role of such factors in this fierce, political debate is a rarity, and individual scientists who have acknowledged the role of values and beliefs in their scientific positions have subsequently suffered attacks on their scientific authority by opponents wanting to subsequently discredit them as too biased to offer credible scientific input on the subject.

### ***Suggestions of political repression connect with the U.S. Right-Wing***

Themes of political repression and, even, of totalitarianism are evoked by the above critics of the IPCC through references to “unauthorized” deletions, “suppression of dissent,” “political manipulation,” “scientific cleansing” and the like, furthering suggestions of deliberate and organized plotting, intimidation and abuse of power. These themes also constitute a shared discourse between contrarians and right-wing groups in U.S. society.

The theme of political repression is evoked repeatedly in Singer’s letter, as in his reference to the IPCC’s “selective representation of facts” and his claim that “politicians and activists striving for international controls on energy use (to be discussed in Geneva in July when the parties to the Global Climate Treaty convene) are ... trying to marginalize the growing number of scientific critics.” It is not clear that the number of critics is either growing or diminishing, but describing the momentum behind opponent groups as diminishing in size - and implying the force and numbers behind their own position to be growing - has obvious rhetorical gain. This strategy is found on both sides, as are suggestions of marginalization and even persecution; in the quote included above, Greenpeace described the Chapter 8 Lead Authors as “sidelined” at the Madrid meeting by industry lobbyists bent on weakening the IPCC report’s conclusions. The large number of scientists involved in the IPCC, and the increasing confidence of the conclusions of their reports concerning detection of a human influence on

climate, is often presented as illustrative of a growing consensus among scientists. Thus, a June 1996 editorial in *Nature* characterized the contrarians as “a dwindling band of skeptics,” asserting “growing support within the scientific community” for the IPCC view that “the balance of evidence suggests a discernible human influence on global climate” (Nature 1996). Of course, consensus knowledge is not immune to error, and it can discourage or deemphasize articulations and knowledge of alternative views.<sup>24</sup> Even so, it requires perceptions of organized and sinister plotting on the part of a coherent group to perceive such processes as due to conspiracies rather than the diffuse working of multiple, ad hoc social processes and interests; as Hofstadter writes, conspiracy theories exceed the real world in their coherence, leaving little room for mistakes, failures, or ambiguities (Hofstadter 1967:36).

Each side in this controversy has its martyrs. Santer is described among IPCC scientists and supporters as victimized by the critics' charges of professional irresponsibility and violation of procedural rules, at great personal and possibly professional cost. In contrarian renditions, the alleged political repression by the mainstream “establishment” is painted as a defensiveness growing increasingly oppressive as the opposition to its ‘regime’ supposedly mounts. Discourses by contrarians and their supporters frequently describe dissenters of the dominant view as righteous victims persecuted by powerful,

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<sup>24</sup> See Fuller (Fuller 1988:214) about the “spiral of silence” possible in “suboptimal essential consensus” when those who either disagree with a standing belief or have no strong views simply remain silent. The IPCC forms a suboptimal essential consensus insofar as each member of the group does not know the justificatory standards and current beliefs of all other experts and members of the IPCC.

repressive forces. For examples of this pervasive tendency, see *Wall Street Journal* editorial (Jenkins Jr. 1993) as well as Singer's July 11 letter described here, both of which also manifest anti-Communist rhetoric. With the decline of the Cold War, actors of the U.S. right-wing have shifted their focus to environmentalist activists, identifying the former "reds" in the "greens." Thus the late *Forbes'* writer, Warren Brookes, whose 1989 article was part of launching the backlash to scientific and public concern about climate change, wrote that "just as Marxism is giving way to markets, the political 'greens' seem determined to put the world economy back into the red, using the greenhouse effect to stop unfettered market-based economic expansion' (Brookes 1989:97). Singer's suggestions of conspiracy graduate into theories in other writings, where his strong anti-regulatory views are expressed with anti-Communist rhetoric. For example, in a piece called "Global Warming: do we know enough to act?" Singer writes on the "Hidden-Agenda Problem" asking:

Why do so many different groups focus on greenhouse warming? Because the issue provides a wonderful excuse for doing things that they already want to do, under the guise of saving the planet. [...] More dangerous are those who have a hidden political agenda, most often oriented against business, the free market, and the capitalistic system. Of course, after the collapse of socialism in Eastern Europe it is no longer fashionable to argue for state ownership of industrial concerns. The alternative is to control private firms by regulating every step of every manufacturing process.

Singer then mentions those using global warming as a vehicle for international action, "preferably with lots of treaties and protocols to control CO<sub>2</sub> or perhaps even methane," or who view the issue as "a launch platform for an ambitious

foreign aid program (Singer 1991:45-46). Singer sees the IPCC as an institution aiding such efforts, and he has suggested elsewhere that climate change is a plot by 'Third World kleptocrats' to find new excuses to demand money from the West (Singer 1992). More details outlining how such leaders of less developed countries have managed to enlist the international community of scientists are not provided.

The letter by Hugh Ellsaesser similarly exhibits the tendency of assuming great orchestrating powers on the part of opponents. Adding his own twist to the story, Ellsaesser considers the whole controversy around Chapter 8 unfortunate and attributes it to the manipulating powers of the opponents; Ellsaesser suggests that the controversy was masterminded by IPCC scientists and supporters through conscious plotting by which to divert attention from the weak basis for their conclusions regarding climate change:

By concentrating on IPCC rules and procedures, IPCC writers and supporters *have managed* to avoid the more important scientific debate as to whether 'the balance of evidence suggests that there is a discernible human influence on global climate.' [Italics added].

Ellsaesser does not specify how IPCC writers and supporters masterminded the GCC's charges.

Like other contrarians, Ellsaesser has established ties with right wing political groups; for example, Ellsaesser has associated with *21st Century*, a magazine by followers of Lyndon LaRouche, his name appearing on the list of fifteen people comprising the Scientific Advisory Board of *21st Century*. *21st*

*Century* has published a number of articles by Ellsaesser,<sup>25</sup> including at least one specifically criticizing the IPCC (Ellsaesser 1995). As Chris Toumey has described, LaRouche's ideology is strongly conspiratorial, believing the world threatened by evil orchestrated by, among others, the London Financial Center, the Swiss and Venetian insurance cartels, the Soviet and U.S. governments, the Anti-Defamation League, Jesuits, European royalty, Socialist International, and communism generally (Toumey 1996:85). In their publications, including a report titled *The Greenhouse Effect Hoax: A World Federalist Plot*, LaRouchites express their belief that the greenhouse theory is a hoax, a plot by the above groups, centrally orchestrated by British royalty and communist forces, who, by means of the UN and environmentalist dogma, infiltrate and undermine the U.S. (Executive Intelligence Review 1989).

While there are significant discursive convergences between contrarians and political groups such as the ones described here, it is important to not simplistically lump scientists like Singer and Ellsaesser with such groups. For example, in a November 1996 interview with me, Ellsaesser himself expressed that although he 'occasionally contacts them and sends them his papers when he comes up with them,' he is "not too sympathetic with a lot of the ideas LaRouche has." There are also instances where scientists do not realize or care to probe the points of views of particular groups soliciting their expert advice, sometimes simply reflective of the tendency for scientists to blind themselves to

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<sup>25</sup> Source: Hugh Ellsaesser, November 1996 interview.

the “hermeneutic larceny” and cultural turmoil surrounding science in society (Toumey 1996:161, 164). For the purposes of this chapter, the point of noting these connections is to show how contrarian suggestions of deception and conspiracy are amplified by political groups with even greater inclinations towards conspiracy theorizing.

Contrarians often explain their associations with extrascientific groups by means of their marginalization by the mainstream scientific community, and by what they portray as suppression of dissenting views. Exhibiting worldviews largely challenged by emergent worldviews shaped by the protest politics of the 1960s and 1970s, contrarians find themselves increasingly alienated from society, an alienation manifest in their opposition to main tenets of present-day environmentalist beliefs, which contrarians consider economically dangerous and rooted in incomplete scientific understanding and irrational emotionalism (see for example Seitz 1997 and contributors to Lehr 1992) . According to Hofstadter, perceptions of marginalization heighten tendencies to perceive social and political processes as conspiracies:

The situation becomes worse when the representatives of a particular interest - perhaps because of the very unrealistic and unrealizable nature of their demands - cannot make themselves felt in the political process. Feeling that they have no access to political bargaining or the making of decisions, they find their original conception of the world of power as omnipotent, sinister, and malicious fully confirmed. They see only the consequences of power - and this through distorting lenses - and have little chance to observe its actual machinery" (Hofstadter 1967:3940)

Seitz' charges of deception might thus in part reflect the fact that he was not part of the IPCC meetings, 'witnessing' the process only indirectly and through the mediation of accounts by the GCC and other interested parties. Despite their strong influence outside the scientific community, signs of disempowerment pervade contrarian discourses, a reflection of their marginalization by mainstream scientists, many of whom deny contrarians scientific authority on the issue of climate change and care little to engage with them in discussion.

Suggestions of conspiracy is also occasionally found among IPCC affiliated or sympathetic scientists, as in the following Email circulated at a U.S. climate research lab:

Ironically, the people who are conducting these attacks and accuse the scientific community of belonging to some sort of 'global conspiracy' are themselves part of a conspiracy, funded by the oil and coal industry, to discredit any scientist or piece of evidence that supports the hypothesis that humans are causing a detectable change to global climate.

Conspiracy theorizing on the IPCC side of the issue is perhaps most obvious in the discourses of environmental activists. See for example the following statement by Greenpeace. Greenpeace suggests that the critics actually believe that scientific evidence proves the reality of dangerous human-induced climate change; this is implied by the words "deliberate and blunt attempt to distort":

Greenpeace believes that this is a deliberate and blunt attempt to distort the nature of the climate threat. The evident intention is to force policy makers to ignore the science, and disagree over CO2 emissions reductions.



By contrast, my research suggests that critics of the theory of human-induced climate change sincerely believe that there is no demonstrated scientific basis for current concern about the issue.

Finally, with regards to several of the assertions of conspiracy by scientists on both sides listed above, including that by Singer concerning other political agendas at work (since environmentalists undeniably would like to see pollution reduced, regardless of global warming) and the one immediately above by Greenpeace (given my earlier description of industry creation of green sounding “front groups” and questionable ad campaigns<sup>26</sup>), one might mark Hofstadter’s words that

Paranoid reasoning begins with certain defensible judgments, and nothing entirely prevents a sound program or a sound issue from being advocated in the paranoid style [...] What distinguishes the paranoid style is not, then, the absence of verifiable facts (though it is occasionally true that in his extravagant passion for facts the paranoid occasionally manufactures them), but rather the curious leap in imagination that is always made at some critical point in the recital of events” (Hofstadter 1967:37).

Ellsaesser’s argument that IPCC scientists and supporters orchestrated this controversy to divert attention from uncertainties in the science is the mirror opposite of that found by on this side of the controversy; here accusations suggest that the GCC and Seitz et al. raised this entire controversy to “divert

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<sup>26</sup> For example, the above mentioned ICE campaign included ads asserting that the theory of human-induced climate change was proven wrong by local instances of colder than usual temperatures. The big print of one ad said: “Some scientists say the earth’s temperature is rising. They say that catastrophic global warming will take place in the years ahead; Yet, average temperature records show Minneapolis has actually gotten colder over the past 50 years... Facts like these simply don’t jibe with the theory that catastrophic global warming is taking place.” Yet, as also explained by the above-mentioned *Science* article on the subject, scientific arguments supportive of the theory of human-induced climate change hold

attention” from the strong conclusion of this new report that the evidence suggests that humans are changing the climate. These arguments by both sides can be read as conspiratorial, with their attributions of deliberate plotting for political gain on the part of their opponents. From another vantage point, these arguments are little more than legitimate political differences expressed through conspiratorial rhetoric - easy point scoring, based on apparent heart felt disagreement and dislike.

### ***Conclusions***

Whether meant seriously or simply used as means of making political gain, attributions of conspiracy are unhelpful for constructive discussion about the state of the scientific knowledge about climate change and about possible “no-regrets” policy action (i.e., policy responses related to energy consumption that have environmental and economic benefits aside from reducing the potential risk of global warming). The vilification inherent in such attributions of sinister motives rarely resonates with the self-perceptions and intents of the accused, and hence further polarizes the groups involved in this already frequently antagonistic debate. One lesson to draw from this case study is the care with which charges of conspiracy must be received and their factual basis examined for assumptions of sinister plotting applied to a reality of much more complexity and much less coherence. Charges and suggestions of conspiracy spread with

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that temperatures in any one place have little bearing on the global warming question, rises in the average global temperature believed likely to simultaneously involve local instances of average cooling.

little resistance among sympathetic audiences, in a social and scientific context characterized by uncertainty, fragmentation, complexity and competing interests; who was who, and who said or provoked what and with what authority and expertise was not always easily established. As a result, most controversies around human-induced climate change - including this one concerning Chapter 8 - remain unresolved, competing claims rarely verified but allowed to fester, with the general effect of reinforcing preconceived suspicions and positions.

The influence of the conflicting claims around this and other controversies concerning human-induced climate change are not easily measured. The GCC originally called for an investigation into the propriety of the revisions, as did Singer in an August 9 letter addressed to all recipients of Santer's informal Emails concerning the controversy. Dana Rohrabacher, the Republican Chairman of the House Subcommittee on Energy and Environment, initiated an investigation into the extent to which U.S. scientists within the Department of Energy have spent time and resources on the IPCC process. The controversy fit his view of climate change research as a "liberal claptrap" and his anti-regulatory values. However, none of the above were followed up by significant action, and the charges against the IPCC did not surface significantly outside of the U.S., where the report heightened concern about climate change. Several Global Climate Coalition member companies, including British Petroleum left the Global Climate Coalition immediately after the Chapter 8 Controversy, apparently because they disagreed with the GCC actions around this controversy. But the

GCC claims that other industries joined as a result, so the net effect in terms of the organization, force, and strategies of fossil fuel industry lobby efforts is unclear.

The charges did not appear to influence the Clinton Administration, at least not in an immediately obvious way. The Clinton Administration strongly supported the 1995 IPCC report and now considers global warming “no longer a theory” but a “fact” (U.S. Government 1997), a sufficient basis for policy action. However, Clinton’s Climate Action Plan presented at the FCCC meeting in Kyoto, Japan in December 1997 was sharply criticized as too weak by European leaders (Stevens 1997), and the relative modesty of the U.S. commitment to reduce its greenhouse gas emissions are often attributed to the existence of strong industry pressure in the U.S. against such reductions.<sup>27</sup>

As we have seen, conspiracy theories in this study span a full range of uses. Moving away from the political fringes of the U.S. political landscape occupied by groups such as the followers of Lyndon LaRouche, conspiracy theories amount to little more than rhetorical means by which to cast suspicion on scientific and political opponents; they constitute one tactic among many in a play between conflicting interests and views concerning what kind of society and future is wanted, a simple strategy by which to advance interests, including environmentalism, unregulated capitalism, and partisan politics.

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<sup>27</sup> Both President Clinton and Vice President Gore have modified their public statements of commitment to strong preventive action on the issue. See for example media analyses (e.g. Birnbaum 1997) of Al Gore’s attempt to appease both environmentalists and industrialists around the Kyoto meeting and his more moderate statements at the Kyoto meeting compared with statements in his book (Gore 1992).

**Chapter 3.**  
**SOME IMPORTANT POINTS IN THE HISTORY OF THE**  
**DEVELOPMENT OF CLIMATE CHANGE RESEARCH AND CONCERN**

***Early scientific work on CO<sub>2</sub> and its effect on climate***

The French Baron, Jean Baptiste Fourier, was the first to theorize, in an 1827 publication, that the temperature of the earth's surface is a function of heat-trapping gases in the atmosphere (Fourier 1827:585; Paterson 1996:17). Other scientists later in the century developed further knowledge of the gases, discovering the important role of atmospheric water vapor and carbon dioxide in the absorption of (infra red) heat radiation emanating from the earth, which itself had absorbed its heat from the sun's ultraviolet rays (Tyndall 1863).

The first calculation of the effect of doubled atmospheric concentrations of CO<sub>2</sub> on the climate, now the standard benchmark in calculations of human-induced climate change, is attributed to Swedish scientist Svante Arrhenius, whose 1896 paper calculated that such doubling would result in a global temperature increase of somewhere between 5 and 6 degrees Celsius (for Fahrenheit, multiply by 1.8) (Arrhenius 1896:268), [in Paterson 1996]. Twelve years later, Arrhenius presented the first known argument that human activity might be provoking such warming by changing the chemical composition of the atmosphere with carbonic acid (carbon dioxide) released into the atmosphere with the burning of coal. Writing from his habitat in a cold region of the world,

Arrhenius rejoiced in the prospect of a warmer climate, hoping that it might result in “ages with more equable and better climates, especially as regards the colder regions of the Earth, ages when the Earth will bring forth much more abundant crops than at present, for the benefit of rapidly propagating humankind” (Arrhenius 1906) [quoted in Somerville 1996: 35-6]. At the time, the dominant belief was that the oceans absorbed the increased levels of CO<sub>2</sub> emitted through human activity. As a result, Arrhenius’ research was ignored for half a century by scientists, with few exceptions. The most noted exception was the British scientist G. D. Callender, who in a presentation to the Royal Society in 1938 presented new evidence from temperature records of some 200 meteorological stations supporting Arrhenius theory of a link between CO<sub>2</sub> and temperature (Paterson 1996:21-22).

### ***New findings and the IGY advance climate studies***

**New research findings: Humans are conducting a grand geophysical experiment on the global climate**

In the 1950s, oceanographers started to explore the trajectory of carbon dioxide emitted by the sharply increasing combustion of fossil-fuels. Their findings disproved the long dominant belief that the oceans absorbed the increased levels of CO<sub>2</sub> emitted through human activity. The important finding that human-emitted CO<sub>2</sub> was not absorbed entirely by the oceans was made when oceanographer Hans Suess traced the circulation of carbon from fossil fuels by studying the radioactive carbon emitted into the atmosphere by nuclear

bombs (Edwards 1997). Using improved techniques for carbon dating, Suess noticed that there was a growing surplus of ordinary carbon in the atmosphere.

Nuclear bombs fueled environmental concern and awareness that humans had developed the means to damage the global environment, and it even provoked concern and conviction among the public that the bomb explosions were affecting the climate (Hart and Victor 1993:647-8). The Atomic Energy Commission (AEC) investigated the widely held belief that nuclear blasts and fallout from atomic tests had changed local weather patterns.<sup>28</sup> This was not the first instance of speculation that human actions were affecting the climate. In their extensive survey of environmental values, Willett Kempton et al. have found an historical tendency in American culture to believe that the climate is changing and that humans are the ones changing it (Kempton, Boster and Hartley 1995). However, as I will further describe in greater detail below, the scientific findings related to CO<sub>2</sub> and climate change in the 1950s came to be accompanied by a general change away from a “geoengineering” paradigm according to which humans had interests in controlling the climate and “improving” on it to better fit human comfort, agricultural productivity, and geopolitical interests. The geoengineering paradigm was slowly undermined by the environmental paradigm according to which humans ought to preserve the perceived “natural”

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<sup>28</sup> Military related research, more precisely the making and use of hydrogen and nuclear bombs, formed a link in the development of a focus on climate in several ways. Research agendas which developed along with nuclear bombs led to a focus on CO<sub>2</sub>, as the nuclear bomb tests stimulated research on the trajectory of CO<sub>2</sub> in the climate system. Bombs and climate research are also conceptually linked in that the simulation of both involves non-linear problems of fluid dynamics. Some of the scientists who helped conceptualize and build the simulations of hydrogen bombs also later played a part in developing the

(and presumed fragile) state of the climate system, be that for moral or utilitarian reasons, or both.

Hans Suess wrote up his findings along with his colleague, Roger Revelle. In their 1957 article, they made the now famous statement that by returning carbon stored in fossil deposits (coal and oil) back into the atmosphere through combustion, humans are carrying out “a large scale geophysical experiment” on the global climate. Countering -- and soon helping to change -- the dominant belief of the time, Revelle and Suess estimated that about half of the CO<sub>2</sub> resulting from combustion of fossil fuels remained in the atmosphere and was bound to affect it in some way (Revelle and Suess 1957:19) [in Paterson 1996]. A 1956 paper by Gilbert Plass is also attributed with having effected renewed interest in the influence of carbon dioxide on climate (Edwards 1997).

At the time of the Revelle and Suess article, the environmental paradigm had not yet become prevalent, and as noted by many scholars, Revelle and Suess’ tone reflected more scientific curiosity than alarm. The article suggested their excitement and hope that this “experiment” could yield important insight into the processes shaping weather and climate (Firor 1990:48; Paterson 1996; Somerville 1996).



### **The IGY: The development of an infrastructure for improved climate study**

Revelle and Suess' findings set the stage for many discussions at the international meetings of the already planned and UN-sponsored International Geophysical Year (IGY) which took place between July and December in 1957. Revelle and Suess' findings enabled them to persuade others to join the effort of exploring further the dynamics of human-emitted carbon in the atmosphere.

The IGY was planned through the internationally linked network of scientific societies and academies of the sixty-seven participating nations. The IGY's objectives were broad, but the meetings were deliberately scheduled to coincide with the first launchings of satellites, markers of the beginning of the "space age" (Sputnik I, the first artificial satellite, was launched into Earth orbit by the USSR in 1957, and the United States sent up their Explorer I the following year). The recognized potential of this new technology played an important part in the development of the IGY. The IGY logo showed a satellite circling the earth, even though none had been sent into orbit by the time of the IGY meetings (Baker 1990:11-12). At the time, global data sets were largely lacking and in great demand, and the IGY meetings focused on securing new and better observations of poorly understood geophysical phenomena through the use of observational satellites.

Although the IGY is known as the first global-scale research program and experiment in studying the Earth (Baker 1990:11), international cooperation around data collection was not new to the field of meteorology (cf. (Paterson

1996)). In the seventeenth century, there had been temporarily successful attempts to set up international networks of atmospheric monitoring stations. According to Paterson (Paterson 1996), enduring cooperation began at the initiative of a U.S. naval officer with the First International Meteorological Conference in Belgium in 1853. This conference established standardized meteorological observations to be taken from ships and a standard method of recording them. Two decades later, meteorological cooperation was institutionalized with the establishment at Leipzig in 1872 of the International Meteorological Organization (IMO). There was a general recognition that meteorology could not improve without international cooperation and standardization. The IMO coordinated such international efforts at collaboration and standardization of measurements and also organized a system of international weather information transfer (Paterson 1996:18-19). With time, technological innovations such as radio and aviation facilitated collection of meteorological data and improved weather forecasts. Governments' awareness of the economic and military value of meteorological data grew and the IMO became an intergovernmental body involving governmental representatives.

During W.W.II, military agencies -- acutely aware of the use of meteorological data for military purposes -- supported meteorological research. They also initiated efforts to improve weather forecasts with the use of the newly developed digital computers. After W.W.II, international coordination around meteorology was further formalized and in 1951 the IMO was renamed the World

Meteorological Organization (WMO). The newly formed United Nations furthered international scientific cooperation, as did the continued advancement of technology in the areas of aviation, satellite communications and computers.

However, the IGY moved international cooperation on meteorology to a new level (Soroos 1991:201), mobilizing international cooperation around the effort to better understand the earth system. Although the IGY took place against the backdrop of the Cold War, the IGY's scientific leaders were, according to Fleagle, motivated to a significant degree by the hope that international cooperation through the IGY might lead to peaceful cooperation in other, non-militaristic areas (Fleagle 1994:169).

In 1961, President Kennedy decided to further stimulate international atmospheric science efforts. Fleagle explains that Kennedy saw such efforts as a "desirable alternative to the vague goals and fruits of space science, and as part of his interest to limit nuclear weapons and to advance international peace and cooperation" (Fleagle 1994:91). Kennedy proposed to the U.N. General Assembly that all nations undertake cooperative efforts in weather prediction and eventually in weather control, and his proposal was adopted in December 1961 with the U.N.'s Resolution 1721.<sup>29</sup>

The first public recognition by the U.S. government that climate change could be caused by human activities, with important consequences for the world, came in 1965 when the President's Science Advisory Committee (PSAC)

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<sup>29</sup> See chapter 8 in Fleagle 1995 for more of the results of this resolution.

published a report titled *Restoring the Quality of Our Environment*. At this time, scientists “on both sides of the Atlantic” were only beginning to develop a theory to explain the behavior of the complex system that determines climate, a system which so far had been thought to be impossibly chaotic and unpredictable. Weather prediction continued to be the primary focus until the late 1970s, and it was not until then that the daunting task of forecasting climate, which had appeared impossible, started to appear less so. Chaos had been the focus in the 1970s, and it had imposed a limit of up to ten days or two weeks, roughly, on how far you could predict. This limit appeared to be an insurmountable obstacle until the end of the 1970s, when new lines of research began to appear promising.<sup>30</sup>

The IGY helped build the World Data Centers, through which meteorological data were made more widely available to scientists internationally. In the aftermath of the IGY, and at the suggestion of the UN General Assembly (UNGA), the World Meteorological Organization (WMO) and the International Council of Scientific Unions (ICSU) established the World Weather Watch in 1968 as an extension and expansion of existing cooperative arrangements between countries to collect and distribute weather information. The World Weather Watch coordinates the systematic observation, processing, and exchange, of meteorological data between countries, a coordination which made modern weather forecasting possible. This coordination was instrumental

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<sup>30</sup> Mike Hall, interview [date].

in the development and improvement of General Circulation Models (GCMs), which are heavily dependent on global data sets.

The sum of numerous technical, institutional and organizational developments -- largely initiated through the World Meteorological Organization (WMO) and the International Council of Scientific Unions (ICSU) -- laid the foundation for the scientific and environmental focus on climate change which started to emerge in the 1970s. One important change was the development of supercomputers. The first successful numerical weather prediction was done using the digital computer, which had been developed during W.W.II. by a group of meteorologists and mathematicians at the Institute for Advanced Study in Princeton. By the 1950s, weather prediction through the use of numerical models became routine, and soon the same technique was applied to modeling climate. Modeling the climate required more computer power and only began to appear possible by the latter half of the 1950s when an emerging commercial computer industry effected great improvement in computers' capabilities, simultaneously decreasing their cost. In 1967, the WMO and ICSU developed the Global Atmospheric Research Programme (GARP), an international collaborative research effort to understand the global weather system and to increase scientific knowledge of the weather, with a view to improve the services rendered by the World Weather Watch as well as the current understanding of climate (Cain 1983:81).

At the IGY, Roger Revelle proposed that a monitoring station be built to measure the levels of atmospheric CO<sub>2</sub>, of which only sporadic measurements existed at the time. The station was built on Mauna Loa, Hawaii, and the measurements were carried out by Revelle's graduate student, Charles Keeling, starting in 1958. These measurements have resulted in what is now the most central fact, and one of the few undisputed pieces of evidence, in the scientific debate about human-induced climate change: "Keeling's curve." Keeling's curve shows irrefutable evidence that the atmospheric concentrations of greenhouse gases have increased from about 285 parts per million to almost 400 parts per million since the beginning of the industrial revolution until the present.

### **Climate - weather competition**

Total annual U.S. federal expenditures on research and development grew from about \$1 billion in 1950 to \$17 billion in 1967, that is, from less than 1 percent of the gross national product to 2.8 percent (Fleagle 1994:3). Most of this increase occurred in development, but support for science also increased steadily. Scientists refer to it as the Golden Age of Science. This was the time in science when funding was readily available as long as a scientist had a convincing proposal, came from a reputable institution and had a sound reputation.

However, in spite of the favorable funding situation, climate simulation was not a federal priority during this time (1960s and 70s), and climate scientists had to struggle to obtain financial support for their research. The two scientists representing and pushing respectively weather simulation and climate simulation, Cressman and Smagorinsky, engaged in fierce competition for recognition and funds, and are said to not even have been on speaking terms with each other (Leith interview).<sup>31</sup> Climate modelers had to compete against other groups of scientists for access to the developing supercomputer technology. This is still a reality as nuclear physicists employ the supercomputers for simulating bomb detonations and other scientists, of training similar to those doing climate simulation, use them for other types of research, including the modeling of weather. These different types of simulations all involve the same fundamental numerical techniques as those used by climate modelers, and it is possible for scientists to cross over from one type of simulation to another, though it requires a period of some years to become knowledgeable about the particularities of the climate or weather vs. nuclear and atomic bombs.<sup>32</sup> Until the development of concern about the short-term stability of the climate system, there was less demand for climate simulations compared with supercomputer

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<sup>31</sup> While weather researchers and climate researchers thus diverged in the past, in recent years they have started to come back together to interact more, in part a result of the general effect of the IPCC in terms of encouraging cross-disciplinary collaboration; in the words of one interviewee, the two fields now involve "pollination from one place to another, fomented by need to create IPCC reports" (Somerville interview).

<sup>32</sup> Numerous scientists I have interviewed have crossed over from simulating nuclear reactions to climate simulations. They cited perceptions of declining resources and federal need for nuclear simulations and/or personal environmental values (i.e., a desire to do scientific work of benefit to the environment) when asked why they did the cross-over.

declining demand for simulation was a declining field

simulations of relevance for military purposes and weather forecasting. This shaped access to these computers, which were, and still are, extremely expensive, both to obtain and to run. Climate simulation was not encouraged by federal funding practices, as priority was given to research of interest to the military.

### **Keeling's curve coincides with first GCM study of CO<sub>2</sub> x 2**

Keeling's measurements, begun in 1958, quickly yielded evidence that concentrations of atmospheric carbon dioxide were increasing. This intensified scientific interest in climate change in the 1960s, and attention shifted from changes in CO<sub>2</sub> as an explanation of climate changes of the past to possible future implications of increasing atmospheric CO<sub>2</sub> concentrations. The release of Keeling's results coincided with a study on the effects of a doubling of carbon dioxide by GCM modelers Manabe and Wetherald. From an interview with Wetherald, November 1995:

Wetherald: "Now, two important things happened, and they happened almost simultaneously, which I think really truly sparked off the greenhouse warming research and controversy. One was our 1975 paper. The second one was an observational study by Charles Keeling that proved that the concentration of atmospheric carbon dioxide had gone up. Within about a month of our publication he came out with results showing that CO<sub>2</sub> was going up at a very constant rate in three different locations. One was Antarctica, one was Hawaii, and there was a third place. Those two together [gave the message that] this was really happening. This wasn't just an interesting scientific problem, this was really happening, and here was the model result showing what the result of that doubling might be. So those two things together sparked a report by the NRC [National Research Council], and that report, I feel, really sparked the



investigation into the greenhouse theory. And then of course, the research went on from there. It went on to more complicated models for us, and other people started getting into the game. So then it became clear that there might be an impact on many things -- agriculture, water resources, economics and so on -- because when we started looking into what the models were telling us, we quickly realized that there was more at issue than just increases in temperature.

### **Mid-70s: CIA and global cooling**

Stewart and Glantz (1985) write that around the mid-1970s, a new political perspective on American food exports was formed as food came to be seen as an important tool for American diplomacy efforts. In 1975, Daniel Moynihan said: "Food *is* a weapon and we should use it," recognizing that the U.S. and Canada together supply 85% of the world's internationally traded grain. The CIA started to become interested in climate as a result, and started to come to research institutions such as The National Center for Atmospheric Research for information. At this time, however, the primary focus was not global warming. Rather, concern centered around possible global cooling, a cooling either naturally occurring (given the regular return of ice ages in the past) or human-caused (through pollution in the atmosphere). Some of the same scientists who once believed that pollution was causing global cooling have since changed to believe that it causes global warming. For example, in a 1971 *Nature* article, atmospheric scientists Iqbal Rasool and Stephen Schneider were arguing that in spite of the

obvious uncertainties in estimating and predicting the effects of carbon dioxide and aerosols in the atmosphere, it seems that, on the balance,

man's continued pollution is likely to lead to a reduction rather than an increase in global temperature. Thus, far from there being a melting of the ice caps it is our view that the triggering of an ice age is more likely (quoted in (Rowlands 1995:69))<sup>33</sup>

The 1976 book by Lowell Ponte titled *The Cooling* (Ponte 1976) illustrates certain concerns of the time, including prevalent assumptions of climate determinism, and the widespread acceptance of (and even deliberate attempts at) human interference with the natural climate system. This was before the environmental paradigm had become as dominant as it is today, a time in which it was widely held that technology could, would, and should "improve" on nature (Ponte 1976).<sup>34</sup> Ponte writes that "The cooling is a fact, to be taken as you wish," and points to data showing that the planet has been cooling for the past three decades. He writes that "...many scientists take the possibility of an Ice Age beginning within the next 100 years very seriously," and that scientists interviewed by British science writer Nigel Calder in 1974 said they set the odds against this happening at only ten to one, "what climatologist Stephen Schneider of the National Center for Atmospheric Research (NCAR) calls 'Russian roulette odds'" (Ponte 1976:4). Ponte refers to a 1975 report by the National Academy of Sciences which said that if the then current cooling trend continued, there was a "finite" chance that an Ice Age would start within the next 100 years -- with the odds estimated to be one in 10,000. Ponte:

The NAS report was shocking, for it represented a warning by some of the world's most conservative, prestigious, cautious scientists that an Ice Age

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<sup>33</sup> For a justification and defense of Schneider with regards to this, see Ehrlich & Ehrlich 1996, pp. 137-9.

<sup>34</sup> The history of climate determinism has been described by Stehr and Storch (1995)

beginning in the near future -- perhaps even emerging from the cooling trend begun during the 1940s -- was not impossible. More than that, the tone of the report was one of repressed alarm. When compared to climate of the past million years or so, said the report, this century's warm climate is 'highly abnormal.' Moreover, 'as we approach the full utilization of the water, land, and air, which supply our food and receive our wastes, we are becoming increasingly dependent on the stability of the present....[abnormally warm] climate.' The NAS Committee on Climate Variation urged an immediate near-quadrupling of funds for climatic research. Why? Because, said the NAS report, 'we simply cannot afford to be unprepared for either a natural or man-made climatic catastrophe.' (1976:4)

But rather than want to "protect the climate" (i.e., prevent human-induced climate change) as is the dominant inclination today among scientists and policy-makers, the assumption then was that humans should alter the climate for their own purposes.

Ponte is unabashed about his position that "climate can and should be modified to best serve the three great powers sharing the top of the world" (Ponte 1976:135). At the time, these three world powers -- the United States, Canada and the USSR -- were all exploring techniques to modify the weather. Ponte notes that both the U.S. and the USSR have territories on the arctic ocean and hence interest in melting the ice year round, since it would save "lots of money and time if ships could use Arctic Ocean routes." During the 1970s, the U.S.A., Russia, and Canada, collaborated in POLEX, the Polar Experiment of the Global Atmospheric Research Program (GARP), mentioned above. Ponte writes: "POLEX studies changes in arctic ice with the seasons and how they affect weather. Such studies will find out how climate in the Arctic can and

should be modified to best serve the three great powers sharing the top of the world." Soviet climate scientist M.I. Bodyko's proposal at the time was to speed up melting of Arctic Ocean Ice; once melted the arctic would not refreeze in winter, it was thought, since the sunlight absorbed in summer by a dark ocean surface would warm the region enough to prevent new ice from forming. Also under consideration at the time was dusting glaciers with coal to attract heat and melt the glaciers. If the whole arctic ice pack were to be blackened, this project would have required 150 million flights by cargo airplanes carrying 10,000 pounds of carbon black each.

Another type of weather modification effort which received significant financial support by these governments during this period was the attempt to seed clouds with silver iodide and other chemicals. Inside the clouds, water condenses on the chemical crystals, which some believe to cause rain. In the 1990s, cloud seeding receives relatively little research money and effort, and it is disputed that cloud seeding has actual effects. By some estimates, there are perhaps fifty different cloud seeding projects going on right now in the U.S.. But almost none of them are deemed "scientific" by scientists. They are done at the request of small farmers and supported by industry groups who get public relations benefits from it. The attempt is to seed clouds to increase rain fall. They use some type of chlorine, and it is said not to have deleterious environmental Research into cloud seeding is currently underwritten almost exclusively by private industry rather than the federal government, and cloud seeding is no

longer part of the mission of the government sponsored research laboratory, the National Center for Atmospheric Research.<sup>35</sup> The reasons aren't clear, but they appear to be connected to the unpopularity of such interference with the natural system and also to skepticism about the success of cloud seeding efforts.

***Paradigm shift: From weather modification and geoengineering to environmental protection***

In the early 1970s, as the environmental movement gained force in the United States, scientists started making public claims to the effect that human activities were likely to alter the climate. Climate modeling was influenced in part by Forrester's World Dynamics models known as the "Limits to Growth" (associated with the Club of Rome). During the 1960s, Forrester was among the first to show how computer models could be used to simulate the ecological impact and interconnected dynamics of human activities and trends in terms of population, pollution, and consumption (Edwards 1997).

Climate modeling also developed greatly during the 1960s. In the beginning, climate scientists used very simple atmospheric models which either neglected the ocean or integrated highly simplified ocean dynamics. These models also simplified and largely left out social trends related to population size, agriculture, consumption, and pollution. By comparison, Forrester's simulation

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<sup>35</sup> William Cooper, Director of the Advanced Study Program at The National Center for Atmospheric Research, personal conversation, May 6 1997.

models that formed the basis of the Limits to Growth work were highly interdisciplinary, integrating both physical and social dynamics (and their interaction). Forrester et al.'s work thus foreshadowed the rise in the late 1980s of Earth systems models, General Circulation Models (GCMs) designed to simulate not only atmosphere, oceans, agriculture, and ecology, but also dynamics more directly related to economics, energy, and human social systems (Edwards 1997).

The Limits to Growth authors predicted impending collapse of human societies due to overpopulation and overexploitation of limited resources. Through the widespread dissemination of their findings by the Club of Rome, the Limits to Growth concept managed to impress the public and politicians. Its two basic findings were that current trends in population, pollution, and consumption levels could not continue to grow indefinitely, and that the issues were global in nature and required global-wide focus and action. As Paul Edwards writes:

The Limits to Growth are important because they mark the moment in the history of environmentalism when global issues first became salient not only to scientists, but also to the general public. Before this point, virtually the only issue discussed as global was population. The Club of Rome played a major part in building awareness of the integrated character of world systems, and especially of natural resources with human economies. From this point on, a growing minority of scientists, environmentalists, economists, and concerned citizens moved beyond the "pollution paradigm" to conceive of some environmental problems as global in scope.

The change from the pollution paradigm to a conception that many environmental problems are global in nature underlies much of the conflict

between mainstream scientists and their critics in the debate about human-induced climate change. Together with acid rain and ozone depletion, climate change is part of a new era in the politics of pollution. Before the early 1980s, pollution problems were considered within a local framework of impact and prevention. The acid rain issue was the first of a series of new environmental issues, and it marked a new era in the politics of pollution by shifting the focus from a local, isolated level to a global level, highlighting the interconnectedness of peoples and processes across traditional local and national boundaries. The problem of acid rain was followed by other similarly transboundary environmental issues which also provoked widespread environmental concern, including ozone depletion and human-induced climate change (Hajer 1993).

As Hajer notes in his analysis of the politics of acid rain in Britain, the systemic source and impact of new environmental issues led to calls for structural responses by groups which interpreted these threats within the context of what they perceived to be a crisis of industrial society (Hajer 1993:43). Hajer demonstrates the importance of this deeper level of meaning to the acid rain controversy. Similarly, my study of the U.S. climate debate shows that concern about human-induced climate change relates to an ongoing socio-political discussion about whether or not there is environmental change and whether or not this reflects a general crisis of industrial society. This discussion involves conflicting values and claims as to what kind of future society and "natural" environments are possible and preferable.

## **SCEP and SMIC**

Two scientific workshops during the 1970s were also important for the rise in environmental concern. The workshops were organized by Carroll L. Wilson, an MIT Professor of Management. In July 1970, he organized a conference on the Study of Critical Environmental Problems (SCEP), the main objective of which was “to raise the level of informed public and scientific discussion and action on global environmental problems” (SCEP report quoted by (Kellogg 1987:119)). The participants included about forty scientists and professionals drawn from over a dozen different disciplines. The SCEP report of the conference stated: “Although we must conclude that the probability of direct climate change in this century resulting from CO<sub>2</sub> is small, we stress that the long-term potential consequences of CO<sub>2</sub> effects on the climate or of social reactions to such threats are so serious that much more must be learned about future trends of climate change. Only through these measures can societies hope to have time to adjust to changes that may ultimately be necessary” (p.12 of the report).

In July of the following year, Wilson organized “The Study of Man’s Impact on Climate” (SMIC) conference, a follow-up to SCEP. The most consequential finding which had come out of SCEP the year earlier concerned the atmospheric effects of Supersonic Transport (SST), and SMIC therefore focused more sharply on climate change. SCEP and SMIC were important shaping events in



the development of concern about climate warming among some scientists,  
as confirmed by one interviewee:

FIROR: I saw [the focus on CO<sub>2</sub>] develop with the United Nations Meeting in Stockholm, in '72, which led to the creation of UNEP [the United Nations Environmental Program], and it had some chapters in its report about the human impact on the climate. And the only calculations in those days about the human impact was CO<sub>2</sub>, climate change. The preliminary work which led up to that were these books which came out of MIT -- "Human Impact on the Environment" type of reports. Will Kellogg was a major author in those, and they took a broader view.

I interviewed William Kellogg, an NCAR scientist, and he confirmed Firor's statement, expressing the importance of SCEP and SMIC for the development of his realization and concern about the possibility of human-induced climate change. Initially, the concern centered around the effect of Supersonic Transport (SST):

LAHSEN: When did you become concerned about the human impact on the atmosphere? What was your own personal development?

KELLOGG: Well, even when I was just a graduate student, I came to work for the RAND corporation, which is a think tank involved in disciplinary research, not just warfare. [It was concerned about] how we get information about the world, more generally. I was in the climate [section]. I first started there in 1947. We wrote a report on the uses of satellites, in 1949 or so. It was classified, under a highly classified project, a reconnaissance project. It was later declassified so that I and my coworkers could get a reward in our work on weather reconnaissance. So it is available now. In those days I worked with a young fellow, Carl Sagan. He and I wrote a fat book on the atmosphere on other planets.

LAHSEN: What influenced your opinion on human impact on the atmosphere?

KELLOGG: I guess I always earlier sort of scoffed at the idea that human beings crawling around the surface of the earth could have any impact on the global climate; that seemed incredible. The more I learned about it, the more I realized that we could. I guess the turning point for me was in 1970; a study in Williamstown, organized by MIT, called "The Study of Critical Environmental Problems," it came out as a MIT book called the SCEP report. I was a chairman of the working group that had to do with the environment, and it changed my ideas: I realized that indeed we could influence the atmosphere, the climate. Well, we knew so little about it; we couldn't really pin it down. The big thing that came out of the SCEP report was that we could change the stratosphere with Supersonic Transport (SST). That was a very sexy subject back then. The more we learned about it, we learned that we could in fact change the stratosphere. Though back then, the theory wasn't very well founded. The result of the SCEP report was that we ended up having to testify twice before congress about the Supersonic Transport. And I think that part of the reason Supersonic Transport was turned down was the thought that we could change the stratosphere and that we didn't want to mess with *that*. -- Although, that wasn't the real reason: the real reason was economics. The chairman of American Airlines testified saying his company wouldn't touch those supersonic aircrafts with a ten foot pole [laughs] -- because they were too expensive. The other airlines probably would have agreed with him.

LAHSEN: But the French and the British went ahead?

KELLOGG: Yeah, and they are still losing money, so, what's the use. SST was the first time I had to grapple with the idea that we could change the atmosphere. I was part of organizing the SMIC meeting, which took place outside Stockholm. It was an international meeting. Carroll [Wilson] who was a renaissance man, business man, was instrumental. He was fascinated by the problem, and he knew how to pull the resources together. SMIC was a one-month-long seminar about man's impact on climate. At this meeting, we never came to an agreement on whether we would have a cooling or a warming. Some very vociferous people said it would go the other way, that it would cool.

During the early 1980s, some of the scientists who later became strong proponents of the theory of human-induced climate change drew public attention with the concept of "nuclear winter." The theory was based on computer models

showing that the climate would grow cold and the planet lose much of its fertility if a nuclear war broke out. The theory was advanced by a small group of scientists, first in a paper referred to as the TAPPS paper (Turco, et al. 1984). While Stephen Schneider and an NCAR colleague, Starley Thompson, modified the conclusions of the TTAPS authors, Schneider's involvement, along with that of Carl Sagan, their involvement in the nuclear winter debate has brought upon them considerable criticism from certain mainstream skeptics and contrarians. It has also supported resistance to the claims by Schneider and Sagan, among others, a decade or so later on behalf of global warming. This is suggested by the following excerpt from an interview with Bill Gray, the Colorado State University hurricane expert and self-proclaimed "great skeptic." The excerpt suggests how some scientists distance themselves from what they consider to be manipulative scare tactics, a distancing which is particularly likely to occur when a scientist has seen several competing theories being advanced, accompanied by significant public concern, only soon to be abandoned:

GRAY: I have this nuclear article here somewhere. [He looks around.] Here it is. See, you have to have been around for twenty years and have seen the scare of the month from NCAR's chemistry department -- well, maybe ozone depletion [a phenomenon detected by chemists] is a real problem, I am not saying it isn't, but it is probably exaggerated. Here is this article -- I guess it is older than I thought: '83, you were quite young then. But anyway, this is a special report by Carl Sagan. [Gray reads from the report:] "Would Nuclear War be the End of the World?" "Major change, more than a billion of people would die, but the longer term consequences could be much worse". And they talk about this thing, he even says in here, where the idea is, they performed this very simple model and this is known as the TTAPS article from the

authors involved: Turko, Toon, Ackerman, Pollack and Sagan. Meteorologists know about this.

This is another [article]: 1985, within a year and a half they worked one side of the fence, namely nuclear winter. With something you can call a political agenda, Sagan -- probably for good humanitarian reasons -- feels that we need to reduce the nuclear weapons, so they come out and say a nuclear exchange would do all these terrible things, [that] is probably their strategy. [...] Then they researched it a bit, and the nuclear winter became the nuclear autumn, and even Schneider was writing "autumn," because the effects were not as cold [they found], once they did research on it for a year or two. Then after that accommodation was made, there was little money to be made there, so they needed something else, and boom, they jumped on this idea of CO<sub>2</sub>, which has been around for so long, but the hot summer of 1988 showed up and they thought here is our chance, the cold war has wound down, and they jumped in on this issue. As time went by, with the nuclear winter thing, they probably realized that things weren't as simple [...] So, [on to] global warming. And in comes Sagan again. And the world is getting warmer and the oceans are going up [he said]. And why? Because we are burning coal etc. It is just appalling.

Gray's reference to the model on which the TAPPS authors based their proclamations of nuclear winter as "very simple" points to another criticism which developed among some non-modelers as computer simulations grew in importance as a scientific tool by which to explore possible future ecological consequences of current socio-environmental trends in population, consumption and pollution. All scientists, modelers and non-modelers alike, admit that the GCM simulations are faulty, a science still in a stage of development, but while some still consider them useful, others focus on the limitations and criticize their use in policymaking. Such criticism of the models is strengthened when conclusions based on simulations are widely disseminated through the media with the result of heightening public environmental concern. Criticism of GCMs

limits modelers' ability to gain support for their simulations and projections of future climate changes. The following chapters will map the shaping structural, disciplinary and historical factors which underpin this criticism and which limit the acceptance of the projections of future human-induced climate change. I will propose that vocal scientific critics both inside and outside the mainstream lend crucial force to greenhouse skepticism rooted in vested interests and discourses in conflict with the environmental paradigm.

### **Calls for action**

The 1985 meeting in Villach, Austria, was important in attracting attention to human-induced climate warming. This meeting, organized by the World Meteorological Organization (WMO), the United Nations Environment Programme (UNEP), and the International Council of Scientific Unions (ICSU), led to two other important meetings, the 1987 Villach and Bellagio meetings in Austria and Italy respectively. As at the 1985 Villach meeting, the participants at the two 1987 meetings agreed that global warming could occur, but they also went a step further by forwarding proposals for policies of abatement (Rowlands 1995:xix-xx). People involved in these meetings came to form overlapping memberships and involvement in other efforts to initiate programs and build organizations around global change issues broadly, one of which is the issue of human-induced climate change. The pronounced overlapping of membership in general in the early organizational efforts around global change and climate

change has been noted by scholars (e.g. (Edelson 1988; Jäger and O'Riordan 1996)), and study of the various committees and reports initiating large-scale scientific and policy emphasis on these issues quickly show the recurrence of the same clusters of names on the lists of contributors, organizers and participants. For example, a large proportion of the participants in the Villach and Bellagio workshops became involved in the organization of the Toronto conference in mid-1988, an international conference hosted by the Canadian government around the theme of "The Changing Atmosphere." The Villach meetings also spurred committees and reports in various U.S. agencies and other institutions, including NASA's "Bretherton report," and several influential reports by the National Academy of Sciences.

Environmental concern blended with the funding needs of scientists who developed interests in propelling the focus on human-induced climate warming, and on global change more broadly. Few deny that a certain opportunism on the part of the scientific community has shaped the climate change issue. One officer in the National Academy of Sciences has referred to the focus on global change, of which climate change is a part, as the result of a "benign conspiracy" (Edelson 1988). He has described how the focus on global environmental change, now a 1.6 billion dollar annual program, was started by a core group of mid-level managers in federal agencies who "worked their way up the chain" to make it the major focus it has become since 1989, when the U.S. Global Research Program was started (Edelson 1988). In an interview with me in 1995,

the same officer noted that the first efforts probably came from the mid-levels because they most feel the competitive processes within the government and generally are the ones within federal institutions most concerned about their programs' survival. Blending with the opportunism was real environmental concern, stimulated in part by the Club of Rome's Limits to Growth: "There was a concern in the air about the future of the world," he explained.

As environmental concern rose, leaders (program directors, administrators, and prominent scientists) in climate-related fields learned to use that concern to secure funding. This has proven to be a double-edged sword, however, because it reinforces a trend in which science increasingly has to justify itself as "useful to society" to obtain funding. This is a sharp change from the "Golden Age" when, by comparison, funding was much more readily available and unconditional. The rise in federal demand for science with tangible social benefits is profoundly shaping what scientists study, do and say; in some scientists' words, they have had to learn to become good "used car salesmen." This change has also provoked or reinforced resistance to the theory of human-induced climate change among some non-modeling climate scientists; as I will describe in the following chapters, this change strikes at a core scientific value of freedom and independence to pursue 'interesting ideas', a value captured in the notion of curiosity-driven science.

The following derives from an interview with a research lab administrator who prefers to remain anonymous. It describes some of the motivations and

technological developments which shaped the current focus on human-induced climate change:

X: Maybe a place to start [this interview] is that after W.W.II there was the Vanavar Bush paradigm for science. From the early 1950s until the 1980s [the paradigm] was that science was good for society and that somehow the payback was significant so that if science was supported, good things would happen. This was the case for basic as well as [applied science]. And the NSF was formed in those days as a support for basic research. Scientists were pretty much free to do whatever they wanted, and science flourished under that model. All scientists had to do was to convince their peers that they were doing good and original science, and it was just a quality judgment, peer reviewed papers was a quantitative measure of success of a scientist, so social relevance wasn't much of a factor in the basic sciences -- although people did of course at least give some kind of lip service to how their work could be used in the future, but I don't think that was a primary criterion for judging what good science to support. The atmospheric sciences, which I know most about, were pretty much the same way. However, because of the benefit of atmospheric science in terms of improved weather forecasts -- more accurate warnings of hurricanes and those kinds of things -- it was easy to say that almost any aspect of atmospheric science has at least a downstream application towards better weather forecasts. But, even today, very little has been done to prove what's a better forecast worth, and is it being used, and how is it being used. It is -- the atmospheric sciences have a very good track record, because during those periods of the 1950s up until the present, the atmospheric sciences have had federal support, the forecasts effort -- it has been gradual, but there is no doubt that basic research has paid off and found its way to improve national weather forecasts.

But then, beginning in the -- probably in the beginning of 1980s -- this paradigm of giving the sciences a lot of support began shifting to more 'show how your science is going to be socially relevant.' And the scientific agenda became set more at the top, with named programs, with the U.S. Global Change Research [USGCRP] program being the best example of that.

One of the reasons for that was to sell big science research programs, but NASA in particular was trying to sell their very expensive satellite programs, probably already in the early 80s, maybe even in the 1970s. And they couldn't sell that as easily as they had in the past. They now had



other things to compete with, like the military and social programs in particular. It was just not as easy as it used to be in the 1950s and 1960s to mount huge scientific programs

LAHSEN: What interests were driving NASA in this?

X: Well, I think that scientists and engineers always want to do exciting programs that are on the cutting edge of science, people who are capable of building some wonderful instruments that can do terrific observations, and it is just in the nature of scientists and engineers to want to try those ideas out. I think it was curiosity-driven and genuine scientific interest, scientific and technological interest, the kind of things that drove NASA to put a man on the moon. So, but the programs became so expensive, one had to justify them more than just on the basis of, 'well, this is going to be great new stuff and somehow it is going to be useful.' So this gets us into NASA's early days of global change, and they had this word "global habitability." They wanted a program on global habitability. So they were actually trying to relate studies of the earth system to habitability for humans.

LAHSEN: That was a new concept?

X: I don't know if it was new, but maybe the organization of some of NASA's science around this notion of habitability was new. And then, that pretty good phrase didn't catch on. I think it led to the concept of global change, and Earth system science, which -- this integrating concept didn't develop until the mid 1980s. The federal agencies got together and said for the first time: 'we are going to have a national program called the U.S. Global Change Research Program (USGCRP).' And I think the reason for that is to sell this science to the OMB [Office of management and Budget], to get funds for the agency science programs. The scientists of course supported this, the scientists wanted to do the research, and the agencies got together and supported it and were quite successful at raising funds for earth system sciences.

The U.S. Global Change Research Program (USGCRP) was established by the federal government during the Bush Administration after a rise in scientific and public concern about human-induced climate change -- concern stimulated in part by a series of very hot summers and high-profile congressional hearings

in which prominent scientists supported the theory that humans were changing the global climate. The USGCRP is responsible for investigating global warming and other possible consequences of climate change, serving to reduce scientific uncertainties on the issue by heightening understanding of atmospheric, oceanic, and earth processes. As a program, it is expected to provide information that policy makers can use when deciding how to respond to the threat of human-induced climate change. As such, the USGCRP coordinates and supports what Sarewitz calls a “staggering range of research questions and problems” in fundamental research seeking a predictive understanding of Earth-system behavior on time scales of primary human interest. It includes, according to one list, “153 projects in thirty-five major areas of research under seven areas of ‘scientific priorities’” (Sarewitz 1996:84-85).

The creation of the USGCRP earmarked an annual sum of more than one and a half billion dollars in federal funds for climate-change-related research, thus guaranteeing support for many scientific projects which either were developed or repackaged to fit into this category to thereby gain access to this large pool of money. While this of course was beneficial to many scientists, the above lab director described how it also was achieved at a cost to scientists in terms of their freedom to direct their research agendas:

But the scientists paid a price for that, too, because it became more of a top-down type of thing, with agencies designing very specific programs on tropospheric chemistry, upper atmosphere physics, and so things became labeled in their accounting requirements.

And scientists had to adjust their proposals to fit the mission as defined in Washington, you know, they couldn't just write a proposal on some good idea, they had to at least make an effort to relate it to the national programs that were being developed in Washington. So scientists to some extent lost control of the scientific agenda. But not completely, because scientists were heavily involved in some of the planning committees and in defining the USGCRP, for example, so they certainly didn't lose complete control. There was certainly less independence, more emphasis on big coordinated science, less emphasis on individual, small science.

LAHSEN: So what is shaping the science?

X: Well, 1.6 billion dollars are labeled global change research. But the buzzword was until recently "policy relevant science." The USGCRP was going to give information that was relevant to policy makers. It is a good idea, you can't argue with the idea. The idea of providing sound science to guide the lawmakers to pass better laws is a good idea, and also to provide industry with good science upon which to base decisions. But -- my feeling is that now with the Republican agenda [interview date: June 1995], policy relevant science is not wanted, because, to be cynical, they would get information that would imply decisions that they wouldn't want to make anyway, like the regulation of carbon dioxide emissions, or regulations to induce greater energy efficiencies, legislation that would somehow restrict free enterprise, preventing industry from doing whatever it wants. So that is why they have this emphasis on basic research, research that may somewhere down the line provide something useful, but it won't provide embarrassing information. [...] And it certainly affects the way we [scientists, research lab directors] advertise our research.

This change in federal funding is also important for understanding the role and representation of climate change science, combined with particular developments within climate change related sciences. Scientific developments -- particularly the coordination of data collection and exchange, the development of new and faster supercomputers, and the emergent scientific ability to push beyond the ten-day limit imposed on weather prediction -- paved the way for organized study of global environmental problems. Moreover, federal agencies

were coming to a crossroads as GARP (the international research effort focused on weather dynamics) was ending and a new, clear focus was needed for programs to sustain themselves. They managed to put global change high on the list of national priorities through an unprecedented level of cooperation and cohesion among leaders of three U.S. agencies, NASA, NSF, and NOAA.

### ***The backlash***

Research institutions are under significant pressure to appear to be producing worthwhile science, and the marketers of the science sometimes oversell it and promote it in ways--aided by the media--that are overblown. This has been a point of much emphasis in the "backlash" to the initial rise and wave of concern about global warming started during the hot summer of 1988. 1988 was the year global warming hit the headlines when atmospheric scientist Jim Hansen of the NASA-Goddard Institute of Space Sciences, testifying before Congress, asserted 99% certainty that he had detected the human-caused greenhouse warming in the climate record. Another scientist at the subsequent international meeting of scientists and policy makers in Toronto seconded the alarm by asserting that "the problems unaddressed have the potential of turning the world into a chaos not greatly different from that produced by global war" (Lawson 1990).

The high-profile congressional hearings during the summer of 1988 had been planned months in advance and were deliberately scheduled to take place during the hottest time of the year. Those hoping to see remedial action on

behalf of protecting the global climate were encouraged by the example of the Montreal Protocol, signed in 1987. They hoped that it might be possible to gain the same level of public and diplomatic support on this issue as had been achieved around the issue of ozone depletion. The Brundtland Report, released in 1987, was also both a manifestation and a stimulator of general concern about global environmental problems under the rubric of “sustainable development” (WCED 1987).

The beginning of a backlash to concern about human-induced climate change is often traced back to an article by journalist Warren Brookes in *Forbes* magazine in 1989 (Brookes 1989). Stephen Klineberg (Klineberg 1997) has identified the height of the backlash period to be between the early and middle 1990s, noting that with this development, there was, for the first time in the history of the United States, a strong anti-environmental movement. In an interview, a spokesperson for the Global Climate Coalition acknowledged that industry decided to become involved in the climate debate after witnessing the success of environmentalists in achieving the Montreal Protocol, the international agreement to phase out the use of CFCs, the chemicals that deplete stratospheric ozone. The Global Climate Coalition is an umbrella group formed within the National Association of Manufacturers by a range of fossil-fuel-producing or heavily fossil fuel-dependent industry groups. The following is an excerpt from my interview with a spokesperson for the Global Climate Coalition:

The GCC was formed in 1989 to coordinate industry's involvement with this issue. Industry realized, especially coming out of the clean air debate

here in the U.S. and the Montreal protocol discussions and debate, that they really needed to coordinate in advance on an issue like this because industry realized that they didn't really coordinate actively in advance of the Montreal protocol discussions and felt that they could really have a more active and a more viable input by getting together on this issue. If you will, this issue really didn't break until about 1988, becoming a public interest issue. So we were formed in 1989 to get onto the crux of the issue before it got too far ahead of industry...

## Chapter 4. SHAPING PERCEPTIONS: INDUSTRY PUBLIC RELATIONS CAMPAIGNS AND CLIMATE SCIENTISTS

Without access to expertise (or counter-expertise), an interest group can scarcely participate today in the policy process, certainly not effectively.

Frank Fischer (Fischer 1991:348)

Those who find themselves in the public pillory as risk producers refute the charges as well as they can, with the aid of a 'counter-science' gradually becoming institutionalized in industry, and attempt to bring in other causes and thus other originators. The picture reproduces itself. Access to the media becomes crucial. The insecurity within industry intensifies: no one knows who will be struck next by the anathema of ecological morality. Good arguments, or at least arguments capable of convincing the public, become a condition of business success. Publicity people, the 'argumentation craftsmen', get their opportunity in the organization.

Ulrich Beck *Risk Society* (Beck 1992:32)

It is quite common on the scientific side of industry to believe that there aren't any real environmental problems, that there are only public relations problems.

Sherwood Rowland, atmospheric scientist and Nobel Laureate  
(1995)

We don't compete well with the fax machines of interest groups.

Stephen Schneider<sup>36</sup>

The Greenpeace Guide to Anti-Environmental Organizations (Deal 1993) outlines the orientations, methods and sources of funding of "anti-environmental" groups in the U.S., most of which have appeared over the last five to ten years as part of the backlash to the environmental movement's success in recent decades. These groups include public relations firms, corporate front groups,

think tanks, legal foundations, endowments and charities and Wise Use and Share groups.

The notion of "front group" runs throughout the *Greenpeace Guide*, and points to the perception on the pro-active side that industry is conspiring, deceiving and manipulating. Introducing the anti-environmental organizations, Carl Deal, the *Guide's* author writes: "Some [anti-environmental groups] are explicit about their mission, others cloak their agenda behind green rhetoric and still others are highly secretive about their activities" (15). Among the definitions of "front" in Webster's Third New International Dictionary are: "the outward, visible or feigned bearing or behavior of a person as contrasted with his true or essential character, feelings, or conditions" and "a person, group, or thing that is used to cover up or mislead concerning the identity or the use, usually illegal, harmful, or self-serving true character, purpose, or activity of the actual controlling or directing agent" (Webster's Third New International Dictionary 1981).

Looking at the anti-greenhouse side does indeed show numerous examples of manipulations on the part of powerful interests. To start, the names and functioning of umbrella groups set up to counter environmentalist views and desired actions suggest the public relations manipulation of some such groups which form what environmental activists call the "anti-environmental"

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<sup>36</sup> Woodlands conference, Houston, Texas, April 3, 1998.



movement.<sup>37</sup> A "Guide" is indeed necessary, or one easily draws the opposite conclusions about the orientation of organizations with names such as "Citizens for the Environment" (CFE), "Information Council for the Environment" (ICE) and "The Global Climate Coalition" (GCC). In the chapter on the controversy over the 1995 report, we learned that the "Global Climate Coalition" is an umbrella group founded by a long list of corporations and trade associations representing major sectors of U.S. industry, including multinational oil and coal companies. As was also obvious in that controversy, the GCC actively seeks to undermine efforts under the UN FCCC to reduce CO<sub>2</sub> emissions.

With the *Greenpeace Guide to Anti-Environmental Organizations*, one learns of many other similarly green-sounding groups whose names don't reflect their primary political goals: "Citizens for the Environment" (CFE), founded in 1990, describes itself as a "grassroots environmental group" which promotes market-based methods for protecting our environment. In fact, it has no citizen memberships, according to Deal's *Guide*; it is a think-tank and lobby group that advocates strong deregulation of corporations. Both its lobbying and its rhetoric places it in opposition to the objectives of the environmental establishment, as represented by the Clean Air Act and California's Proposition 128 ("Big Green") aimed to improve state regulations of toxins.

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<sup>37</sup> The members of such groups may define themselves in opposition to the environmental movement, but, as I describe in a later chapter, contrarians generally do not consider themselves "anti-environmental." However, contrarians do tend to require high-proof or absolute proof that there is an environmental problem

ICE, the Information Council for the Environment, Deal's *Guide* tells us, was established in 1991 "as a front group" for twenty-four coal companies, mining associations and public-utility corporations, and is run by the Washington, D.C., public relations firm, Bracy Williams and Co. The aim of this group is to persuade the government and the public that global warming is a myth, and to thereby undermine conversion to less-polluting alternative energy sources, like solar, wind and hydroelectric power.

I will describe ICE in further detail below. Before proceeding, however, I should note that I have not encountered industry groups orchestrating public relations campaigns to promote the theory of human-induced climate change (there are, of course, plenty of examples of industries seeking to build their public image through appearing "Green" and concerned about human-induced climate change). Certain industry groups may have interests in the theory; green technologies and alternative, non-fossil fuel energy producers for example clearly stand to gain from its acceptance and associated remedial action (Moore 1994), but despite persistent inquiry among scientists who are close to lobbying activities on the Hill and part of the IPCC negotiation process -- in which fossil fuel industries feature prominently -- I have found no such involvement by other industry groups. Political scientist Sonja Boehmer-Christiansen (Boehmer-Christiansen 1994a; Boehmer-Christiansen 1994b) has argued that nuclear interests along with scientific interests have been important in propelling the

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before taking action; in the absence of absolute proof, they prefer precaution to weigh in favor economic

current concern about human-induced climate change. She offered no evidence for the claim of nuclear industry involvement, however, and besides the possibility that such industries use the theory in local pitches and advertisements for their product (which I suspect they probably do), my repeated questioning suggests that nuclear or other alternative energy industries are not engaged in the larger debate about human-induced climate change.

***Contrarians -- non-scientific critics' important link to scientific authority***

To gain an idea of the importance of contrarians to right-wing and industry groups in U.S. society as a necessary link through which to legitimize their political views, consider these segments from a May 1995 hearing in Minnesota to determine the environmental cost of coal burning by state power plants. Michaels, Balling, Idso and Lindzen were paid by fossil fuel industries to testify as expert witnesses. The excerpts suggest at once (1) the role of the political Right and of industrial interests in promoting contrarians' view points, (2) how scientists' authority is appropriated, and (3) how these scientists don't feel compelled to understand the larger context which in this case financially benefits them -- or, even if they do understand this larger context, their scientific ethos allows them to remain indifferent to it.

**Robert Balling is questioned:**

Q: [by Mr. Glaser] Okay. I'd just like to go through briefly who the funding sources are and if you could tell us who they are and, if it isn't obvious, what their interest might be in global warming. Starting with, say, Cypress Minerals Company, what do they manufacture?

Balling: I have no idea. [...].

[They go through a number of other names of fossil-fuel related funders on this contrarian's resume, including Cypress Minerals, the British Coal Corporation and the German Coal Mining Association.]

Q: And the last one on this page is from the Kuwait Foundation for the Advancement of Sciences, can you suggest why Kuwait, or rather the Kuwait Foundation might have any interest in global warming?

BALLING: No, I can't. I know a Kuwaiti who did a Ph.D. with our group and the Kuwaiti said that he is well connected in Kuwait to funding sources. You have to understand, I am the director of a research laboratory and there's never ending pressure to find research money. And when you discover that the Kuwaiti government has a research pool that is being allocated to scientists in my field, you would be crazy not to run out and make some attempt to put a proposal in to obtain funding from that source.

Q: Okay, with respect to your book, *The Heated Debate*, isn't it true, [Dr. Balling], that the Pacific Research Institute for Public Policy [which published the book] was in fact founded to oppose environmental regulations?

BALLING: I know nothing of their history. I'm aware that they have been a conservative public policy group. But I did not investigate who these people were that asked me to prepare a book for them.

Q [asked after it was established that the Pacific Research Institute for Public Policy also published a middle eastern version of the book]: Is [the Middle Eastern version] published by this organization?

BALLING: yes.

Q: Was it funded by the Kuwaiti government?

BALLING: The Kuwait Foundation for Advancement in Science gave Dr. Nasrallah a grant and money from that grant went toward the publication of the book.

(Balling 1995)

**Patrick Michaels is questioned:**

Patrick Michaels is meteorologist and Professor in the Environmental Sciences department at the University of Virginia. He is also State Climatologist for his state.

Q: Dr. Michaels, [...] Page fifteen of your resume. You list the sources of your financial support over \$10,000. Could you identify which of those research projects involved global warming?

MICHAELS: [Michaels mentions: Commonwealth of Virginia (140 K), Cypress Minerals, 40,000, anonymous (50 K), Edison Electric, Western Fuels and 'the German project']

Q: Would you translate the German for us?

MICHAELS: I can't. Don't ask me. I don't know, can you?

Q: Is it the German Coal Trade Association?

MICHAELS: Beats me. Peter, do you speak German?

Q: Who did you get this money from?

MICHAELS: It was work through a scientist by the name of Gerth Vaber (phonetic).

Q: And you don't know the nature of the organization?

MICHAELS: It's a, I mean, it is a German energy-related company. But please don't ask me for a direct translation.

A subsequent cross-examiner, a representative of the Environmental Coalition, focusses on the publication that Patrick Michaels edits. It is funded by Western Fuels, and the cross-examiner notes that she didn't see any official mention in these publications of the fact that Western Fuels pays for them (Michaels 1995).<sup>38</sup>

**Sherwood Idso, May 24 1995, MN hearing**

Sherwood Idso is questioned. He is famous for the "greening of the planet" theory according to which rising levels of CO<sub>2</sub> will lead to a greener, more fertile and agriculturally productive planet. According to his theory, even burning all of the earth's fossil fuel reserves, estimated to raise atmospheric carbon dioxide levels tenfold compared to preindustrial times, would be beneficial because carbon dioxide induces plant growth. His theory does not consider the locally polluting effects this might have. Idso's theory was also in the magazine *New American*, a fact raised during the Minnesota hearing:

Q: [by Mr. Wirtschafter]: Mr. Idso, do you know if the *New American* is published by an advocacy group or a research institute?

Idso: I know it's not a scientific magazine, it's something in the popular press.

Q: Is it published by an advocacy group of some sort?

Idso: I don't know if it's advocacy, I know it's some political type [of] organization.

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<sup>38</sup> Early on the publications didn't include any reference to the fact that Western Fuels paid for the newsletter. That has since been changed.

Q: What organization is that?

Idso: I can't remember, some kind of society, I think.

Q: Was it the John Birch Society?

Idso: It sounds like it.

(Idso 1995)

It is difficult to believe that these scientists being questioned are that unaware of the names and political orientations of groups with which they have associated. However, scientists' culture does in some respects allow, if not encourage, such lack of concern about the sources of their funding and the socio-cultural and political contexts of their work (Toumey 1996). While critical of their opponents' sources of funding, climate scientists generally say that where they obtain research funding from has (or, when speaking hypothetically, would have) no bearing on the research they do. Scientists also often pride themselves on not being influenced by, nor even attending to, the politics that surrounds them in their labs and in society more generally. Such self-presentation reflect the traditional scientific objectivist framework which has only recently begun to crumble, as the rise of protest politics and the era of environmentalism have shattered traditional conceptions of the function and role of science as objective and able to provide "truth" and order to political emotion and factionalism (Hays 1987).

***Vested interests and Public Relations firms:  
strategies to shape public opinion***

Straightforward deception is not unusual in public relations campaigns. A recent article in *The New York Times* subtitled: "Lobbyists use 'grass roots' to hide backers" (The New York Times 1996) tells about Neal M. Cohen, a specialist in "grass-roots" lobbying from Apco Associates, a Washington firm, giving a talk to other lobbyists which was taped without his knowledge. In that talk, he "regaled the group with tales of hapless reporters and public relations coups, but he underscored a serious theme: the importance of keeping the public in the dark about who the clients really are." "Grass-roots" lobbying is a term for a Washington technique often used to camouflage an unpopular or unsympathetic client. Typically the client, often a large business, hires a Washington firm to organize a coalition of small businesses, nonprofit groups and individuals across the nation. The coalition draws public sympathy for the legislation sought by the original client, who recedes into the background. Using advertisements ("we used every campaign tactic we had in order to bring in as many people," "we made sure that it was typical people mixed in with large employers and political contributors"), and making it free and as easy as signing a list for people to join as members, they recruited members to thus use a grass roots front as a cover for the client initiating and funding the effort. One effect of this tactic is that those it targets or those who oppose it cannot easily identify who is behind the ad campaign attacks. In an example given by Cohen, his firm had helped powerful companies launch a campaign for a bill to impose the first



nationwide limits on liability lawsuits, scheduled for a Senate vote in March 1996. One of the targets of the campaigns, lawyers in Mississippi, were taken completely by surprise: "They didn't really know who was at the heart of everything, and there were no reporting requirements." The lawyers could not figure out who was at the root of the attacks because, in the end, "we have 1,500 Mississippians mixed in with who our clients were," Cohen explained. He would not say who his clients were, other than the coalition, their camouflage.

Similar campaigns have been launched to attack the climate change theory. "Can PR Cool the Greenhouse?" asks a 1991 piece in *Science* on an ad campaign by the Information Council for the Environment (ICE), funded by a group of electric utilities, coal companies, and manufacturers; "If a slick ad campaign can cool Americans' enthusiasm for controls on greenhouse warming, stand by for a big chill." The campaign effort started at a time of proposed legislation that would impose energy taxes and regulations on these industries in order to lower emissions of greenhouse gases. "Some scientists say the earth's temperature is rising. They say that catastrophic global warming will take place in the years ahead," says the big print of one of the ads. "Yet, average temperature records show Minneapolis has actually gotten colder over the past 50 years...Facts like these simply don't jibe with the theory that catastrophic global warming is taking place." Different versions of the same argument appear in ads about Albany and the state of Kentucky.

The *Science* article explains that the temperature in these various places has "little or no bearing at all" on the global warming question; the combined record does show that the globe as a whole has warmed during this century, but that that conclusion might not have been affected even if Minneapolis had cooled -- which it hadn't. The *Science* article quotes a scientist, Thomas Karl, of the National Climatic Data Center: "What they [the ads] say [about temperature trends in specific regions] may be true, but it is selective information; it's a bit of disinformation" (Science 1991).

Internal ICE documents, written when the organization was at first tentatively titled "Informed Citizens for the Environment," outlined the strategies for the campaign, which included: "Reposition global warming as theory (not fact);" "Target print and radio media for maximum effectiveness;" "Achieve broad participation across the entire electric utility," among others, the last one being: "Use a spokesman from the scientific community." ICE enlisted Robert Balling, Sherwood Idso, and Patrick Michaels, described by the *Science* article as "three of a half-dozen or so outspoken greenhouse dissidents among United States scientists" (Science 1991).

"The ICE test marketing sought people most receptive to their message, such as "older, less-educated males from larger households, who are not typically active information-seekers" and "younger, lower-income women." Among other places, they placed ads during Rush Limbaugh's radio shows. Their pre-test telephone interviews with 500 adults in Flagstaff Arizona found that

80% considered the problem of global warming "somewhat serious" while 45% considered it "very serious." "With this high level of awareness and concern in Flagstaff it will be interesting to see how the science approach sells," notes Bill Brier, Vice-president of Communications at Edison Electric Institute, which was part of the effort. "My concern is that the absence in the messages of reasonable approaches to solving the problems of global warming may reduce their effectiveness," he writes in a letter prior to the planned nation-wide media campaign.

According to *The Greenpeace Guide*, ECO (the Environmental Conservation Organization) is the result of similar organizing. Thus, while Fred Singer's organization describes ECO as a "coalition of some 250 organizations representing seven million U.S. households," presenting this as a sign that "the general public is becoming involved," *The Greenpeace Guide* describes ECO as "a front group for real estate developers and other businesses opposed to wetlands regulations" (Deal 1993:51-2). The promotional literature for Fred Singer's organization, SEPP (the Science and Environmental Policy Project), proudly states that Singer served at ECO's "first annual Congress" as "ECO's only scientific member."

Another dominant player on the industry side is Western Fuel Association, a \$400 million consortium of coal suppliers and coal-fired utilities, whose annual report openly admits having sought and found scientists espousing a skeptical point of view about climate change. Like the ICE documents, and in spite of their

claim to 'balance the argument over global climate change,' nowhere in any of the documents promulgated by Western Fuel could I find an analysis of the science considering all of the important theories, both for and against human-induced climate change: the focus, rather, is squarely on skeptical points of view and on the strategies to promote these views -- "facts" -- supporting their interests.

Western Fuels' annual report for 1993 (Western Fuels Association 1993) explains their rationale for attacking the global warming issue: "We experienced a loss of \$23,820 in 1993. [...] But for the climate change debate and the Midwestern flooding that disrupted coal deliveries, we would have operated in the black" (1993:5). "With the exception of the National Coal Association," continues the report, "there has been a close to universal impulse in the trade association community here in Washington to concede the scientific premise of global warming (or as the lawyers put it -- concede liability) while arguing over policy prescriptions that would be the least disruptive to our economy (or as the lawyers put it -- arguing damages). We have disagreed, and do disagree, with this strategy." Thus, in a section titled "Balancing the Argument Over Global Climate Change" (Western Fuels Association 1993:13), they describe their "alternative vision" when it comes to the future of national energy and environmental policy, a vision which is "positive and pro-people" (when thus painting environmentalists as doomsayers and anti-humanists, they evoke a key story-line of the environmental opposition). The annual report goes on to note

that as part of the first step in its decision to "take a stand," "scientists were found who are skeptical about much of what seemed generally accepted about the potential for climate change. Among them were Michaels, Balling, and Singer.

Finding that scientists working on research involving the effect of rising CO<sub>2</sub> concentrations on plant life saw their work "all but ignored," Western Fuels decided to finance the *Greening of Planet Earth* videotape production based on Idso's theory, at an estimated cost of \$250,000. The video premiered in Washington, D.C., in October 1991. The production, which features appearances by eleven scientists and researchers, was said to be influential in the Bush White House and also appreciated within the governments of OPEC (Gelbspan 1995:34). By Western Fuel's own assertion, the video 'made the rounds in policy circles and found its way overseas' to such extent that 6000 copies were in circulation by mid-1992, and had been broadcast 362 times in 61 media markets within a matter of months of its release. Advertisements in the *New Republic*, *National Review*, and *American Spectator* magazines further boosted worldwide circulation of *The Greening of the Planet* to 15,000 copies within the first year alone (Western Fuels Association 1993:14).

### ***The circulation of (mis)information***

Limbaugh and other public figures are highly receptive to criticism of the theory of human-induced climate change, as are their audiences, and right wing media figures such as Limbaugh and George Will provide important mechanisms

by which skepticism about the theory of human-induced climate change is disseminated.

"I have been proved right about so many things since my first book was published," Rush Limbaugh begins the chapter on the environment in his latest book, *See, I Told You So*, "but," he continues, "on no issue has the evidence of my foresight and keen political instincts been more compelling than that of the environment" (Limbaugh 1994). Limbaugh then lists the contents of his foresight and instincts, number one of which is: "Despite the hysterics of a few pseudo-scientists, there is no reason to believe in global warming." Later in the chapter, he returns to expand on this: "Perhaps the biggest environmental frauds perpetuated on us in recent years are the notions that the Earth is heating up and that the ozone layer is disappearing because of man's abuse of the environment. I've been telling you for years that there is little scientific evidence behind either of these theories, and what I have been saying is being validated by virtually every new study being done, with the exception of those using solely computer models" (1994:197). "[T]here is nothing resembling a consensus on this issue among the scientists who have expertise in this area," Rush continues. "In fact, a majority clearly does not believe global warming has occurred" (1994:199).

As noted earlier, Stephen Klineberg's survey of environmental opinions in Texas over a number of years has shown that above age, gender and other sociological factors, ideology, particularly whether a person thinks favorably or

unfavorably of Rush Limbaugh, is *the* most important indicator of environmental values and beliefs (Klineberg 1997). One may suspect that this is because people who are favorably disposed towards Rush Limbaugh also listen to him and are thus influenced by his pronouncements on climate change -- which I will describe below. However, such people are likely to already hold politically conservative values and beliefs, such that their environmental beliefs and their favorable opinion of Limbaugh are manifestations of the same key factor, their pre-established political values and beliefs, rather than there necessarily being a cause and effect relationship between their opinions regarding the environment and regarding Limbaugh. Whatever is the case, and despite his lack of expertise in the area, Limbaugh reaches millions of people with his anti-greenhouse rhetoric. With an audience in the millions on a weekly basis, it is possible that more people receive their information about climate change from Rush Limbaugh than from any mainstream climate scientist or institution.

Without peer review or other mechanisms of verification of claims, unverified accusations travel particularly easily in the public domain. Limbaugh is not one to use extensive footnoting to account for his sources, so there is no way of knowing what scientific evidence he has relied on and what methodology he uses to arrive at the strident conclusions described above. The only reference Limbaugh makes in this context is to a Gallup Poll of scientists involved in global climate research, a poll which he describes as having shown “that 53 percent do

not believe that global warming has occurred, 30 percent say they don't know, and only 17 percent are devotees of this dubious theory" (1994:180).

Rush may have his information from a column by George Will, in which Will attacks Al Gore's *Earth in the Balance* (Gore 1992), reflective of the partisan bent to the climate change debate. In his book, Al Gore places the burden of proof on the critics of climate change, claiming that "there's no supporting evidence" of the various skeptical arguments. In television appearances he has referred to the threat of climate change (which he called "the highest risk environmental problem the world faces today" on *Nightline*, February 1994) as based on a consensus within the "world scientific community." Will calls Gore "cavalier about the truth" and suggests that Gore must have known about -- but willfully overlooked -- the Gallup Poll of scientists concerned with global climate research which, according to Will, shows that "fifty-three percent do not believe warming has occurred, and another thirty percent are uncertain." Out of the whole poll, he cites only those two numbers, as does Limbaugh, suggesting that Will is Limbaugh's source, or vice versa.

The source matters because these numbers have no relation to the actual numbers of the poll. So we need to do a little detective work. A look at the complete results of the Gallup poll reveals that, in contradiction with Will's assertion, the *majority* of the four hundred scientists surveyed in fact *believe* that warming is occurring: to the question "In your opinion is human-induced greenhouse warming now occurring?," sixty-six percent give an unequivocal



"yes," ten percent say "no," with the remaining twenty-four percent undecided. Only two percent of scientists believe there is no possibility temperatures will rise two degrees Celsius (about four degrees Fahrenheit) or more. Seventy-seven percent believe there is a greater than ten percent chance of the above scenario occurring, with forty-seven percent believing that there is a greater than fifty percent chance. (The four hundred experts were randomly selected from "the leading associations of atmospheric, climate and oceanographic scientists in the United States," including the American Meteorological Society and the American Geophysical Union.)<sup>39</sup>

Certainly, George Will himself may have obtained his information from a secondary source that thus misrepresented the results. As may be clear from the description of APCO and ICE public relations campaigns, and further described below, there are numerous groups which may create such distortion in order to shape public opinion on the issue of human-induced climate change.

### ***Financial gain or political affinities?***

Ross Gelbspan, a Pulitzer-prize winning journalist, published an article in the December 1995 article of *Harper's Magazine* based on research into organizations' annual reports, past court testimonies, and tax reports of key players on the skeptic or industry side in the climate change debate. He found that, in the previous year and a half, the Global Climate Coalition had spent more

than a million dollars to downplay the threat of climate change, and expected to pay another \$850,000 on the issue in 1996. The National Coal Association spent more than \$700,000 on the global climate issue in 1992 and 1993. For the year 1993 alone, the American Petroleum Institute (API) -- just one of the more than fifty members of the Global Climate Coalition -- paid \$1.8 million to the public relations firm Burson-Marsteller, partly to defeat a proposed tax on fossil fuels. Gelbspan calculates that the spending of API alone, a single company, spent "only slightly less than the combined yearly expenditures on global warming of the five major environmental groups that focus on climate issues -- about \$2.1 million, according to officials of the Environmental Defense Fund, the Natural Resources Defense Council, the Sierra Club, the Union of Concerned scientists, and the World Wildlife Fund" (Gelbspan 1995:34).

Studying tax reports, testimonies, and other documents, Ross Gelbspan found that Patrick Michaels has received "more than \$115,000" over the last four years from coal and energy interests. Western Fuels finances publication and distribution of *World Climate Review*, which Patrick Michaels edits. The first issue was mailed to about 7,000 individuals in the fall of 1992. By the second edition in the winter of 1992, circulation climbed over 8,000 [including some 800 environmental journalists]. As of the third edition in the spring of 1993, it was "close to 12,000 and growing" (Western Fuels Association 1993:14). Concerning the other main skeptics, Gelbspan found that over the last six years, either alone

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<sup>39</sup> I base my assertions on my own reading of the relevant parts of the Gallup Poll.

or with colleagues, Balling has received more than \$200,000 from coal and oil interests in Great Britain, Germany and elsewhere; that Lindzen charges oil and coal interests \$2,500 a day for his consulting services, that his trip to testify before a Senate committee was paid for by Western Fuels, and that a speech he wrote calling into question the scientific consensus on climate change was underwritten by OPEC. Gelbspan also found that S. Fred Singer has received consulting fees from Exxon, Shell, UNOCAL, ARCO, and Sun Oil.

Liberal and left-wing environmental advocacy groups and individuals tend to portray contrarian industry funding as prime evidence that the credibility and authority of these scientists is problematic, if not entirely void. Mainstream scientists tend to be suspicious of such funding, for themselves or others. As one manager in a science bureaucracy put it, mainstream scientists are “cynical” about the feasibility of collaborating with industry. When I asked him to expand, he explained that mainstream scientists “are the ones who are the least friendly to interaction with industry. They have the academic attitude that they aren’t doing what they do so that they can make money at it. [...] You might find a few people that would be willing to do this kind of thing a little bit, but no one is going to really dedicate themselves to it”. He went on:

SCIENCE MANAGER: You can’t get people to take the money to do the job unless you entice them into it.

LAHSEN: Doesn’t money alone entice them into it?

SCIENCE MANAGER: No. Computer time will. [...]

LAHSEN: This is surprising to me, because what we hear all the time is that where the money is is where the scientists will go.

SCIENCE MANAGER: They will, if it is government money, but they won't if it is "BAD" -- "bad guy" industry, you know. It is not that they won't, but it is difficult to convince them to it

Their argument is mirrored by contrarians' reduction of government-funded scientists as corrupted because of their source of funding; Contrarian scientists criticize, if not vilify, mainstream scientists for their dependence on government funding, pointing to -- and in the process, exaggerating the role of -- manipulations by mainstream scientists to obtain and retain their funding.

My research suggests that instances of mainstream scientists receiving funds from industry groups and wealthy, private foundations are very few, and that mainstream scientists generally are mistrustful of industry funding. By contrast, contrarians tend to defend the involvement of industry, if not even valorize it (cf. Seitz 1993). However, the patterns of industry versus government funding of science are not easily identified, nor are their implications easily established. For example, complicating simplistic explanations is the fact that while mainstream scientists tend to distrust and avoid industry funding, I know of at least a few mainstream scientists who do receive part of their research money from fossil fuel related industries. For example, EPRI, a consortium of electrical companies, pays for the project ACACIA carried out under Thomas Wigley -- the NCAR scientist whom I earlier identified as a "hawk" in the debate about human-induced climate change! By contrast, according to an article in a local Virginia

newspaper, Patrick Michaels' research grants as of December 1997 were all from the state and federal governments (Stradling 1997).

Scientists have access to various sources of funding, and it appears that, in the vast majority of instances, they choose to accept funding from sources with whom they sympathize. This was also the conclusion Samuel Hays drew from his study of environmental science and politics:

There is much truth to the observation that scientific views depend heavily on the sources of funds that sponsored the work... However, the close relationship between sponsors of research and the views of those whose research they funded was rooted in shared personal values that transcended mere contractual relationships between employer and employee. They came together through reinforcement of sympathetic and symbiotic perspectives (Hays 1987:356).

My point here is not to judge the scientists who receive money from industry but, rather, to illustrate the economic interests and socio-political networks that seek to support contrarians' activities. An additional point is to show the need for greater public disclosure on the part of scientists concerning their financial backing.

It should be noted that at least some prominent "hawks" clearly have benefited financially from their activities related to the climate debate. An article in *Nature* and *Science* -- which I have not yet managed to find -- is said to have investigated the amount of money a number of high-profile scientists on both side of the debate had earned from public engagements related to the climate debate. According to two sources, the study listed Stephen Schneider as the

scientist who had benefited the most financially.<sup>40</sup> As the following quote suggests, monetary gains hardly form the most convincing explanation in what motivates scientists;

BILL GRAY: There are some industries that fund critics of it. See, because they don't want government putting restrictions on them. And all these people, like Lindzen, and Pat Michaels, they all get accused of getting money as payoff for this, where as the global warming people like Manabe, he has won big prizes from Europe and what not, worth \$150,000, right. I mean, I don't know how much Pat Michaels is making, but I read in the Wall Street Journal that in one year he was given \$27,000 to give speeches around the country. Which, I mean, that is not much. He is not making a lot of money. One time, some coal company invited Lindzen down to Australia to give some talks, which he did, and the idea was that he was being paid off by the industrial groups. Well, he may have had his way paid and made a few thousand dollars, but he is not making a lot of money. Not as much as Carl Sagan is making -- he used to get ten-to-fifteen thousand dollars to give a lecture.<sup>41</sup>

Money patterns, however, do reflect the powers supporting the various scientific points of view.

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<sup>40</sup> I will need to verify this, of course. I also need to check how long a period the study covered, and what criteria was used.

<sup>41</sup> Interview, October 10, 1994.

## Chapter 5. THE DEVELOPMENT OF COMPUTER MODELS AND THE GROWTH OF CLIMATE SCIENCE

To understand how knowledge and power operate and interrelate in the field of atmospheric science, one must understand the origin and growth of climate science and, importantly, how computer-based scientific inquiry developed and has come to shape the entire scientific field. Climate science owes its current status to the development of computerized numerical weather forecasting and to environmental concerns, which together have propelled it from being a relatively obscure field in the 1940s, of interest mostly to its practitioners, to a position of prominence in the scientific, public and political worlds.

My argument in this chapter is that some of the criticism of models -- and, by extension, of the model-based projections of human-induced climate change - - among non-modeling meteorologists may be rooted in a certain historical competition between the different branches within meteorology. Before the middle of the twentieth century, the “synoptic” approach dominated in forecasting. It “lost” to numerical modeling, however, which is a physics-based method considered less (if not “un-”) subjective and thus more “scientific” than the synoptic approach was. The rise of numerical modeling oriented synoptic forecasters toward other activities of central use in modeling, particularly analysis of data sets.<sup>42</sup>

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<sup>42</sup> Such analyses enable detection of patterns in the atmospheric system and help “reanalysis,” a technique of central importance to GCMs.

With the rise in prestige of the climate research due to the development of a theoretical (physics-based) dimension and predictive capability, scientists from other scientific fields were attracted to meteorology. To the extent that such newcomers came from more established and prestigious disciplines, the prestige of meteorology grew. One consequence of the rise of numerical modeling, of the involvement of scientists from other fields than meteorology in atmospheric research, and of the development of concern about human-induced climate change was that the older generation of meteorologists at times found themselves somewhat marginalized, even at an institution such as the National Center for Atmospheric Research, which was originally established by a group of meteorologists concerned to improve recognition and prestige of the field of meteorology.

As such, to some degree criticisms of models voiced by traditional meteorologists reflect “turf war” -- competition and conflict of interests. I will define “traditional meteorologists” as non-modeling, “empirical” meteorologists involved in weather forecasting prior to the development of public concern about human-induced climate change. An important faction of “traditional meteorologists” were synoptic forecasters -- that is, practitioners of the method of weather forecasting used prior to the development of numerical weather forecasting. However, as the chapter on modeling shows, the objections of such “traditional meteorologists” are also rooted in their awareness, as empiricists, of the short-comings of the models in terms of how they integrate data and relate to



observations. With public concern about human-induced climate change -- and the consequent (and general) pressure for science to be useful and to deliver answers of relevance to policy decisionmaking -- the important step of validating the models (i.e., checking them against observations) is sometimes short-changed; modelers can feel pressured to provide answers while under restraints in terms of time, knowledge and funding.<sup>43</sup> The net result of this, besides less reliable "answers," is that the contribution of empiricists is deemphasized, skipped, cut short and undervalued. Thus, some meteorologists who worked in the field prior to the development of concern about human-induced climate change perceive their field to have been taken over by a new set of concerns, practices, and scientists, while their own contributions are relatively sidelined by comparison. While such perceptions persist, there are signs of change in this regard, at least in some institutions. For example, an empirical meteorologist has noted that during the last year or two (i.e. 1997 and 1998), there have been more effort to include empiricists in modeling efforts and to provide resources for their work.

***The rise of meteorology and numerical weather prediction:  
three different and competing approaches***

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<sup>43</sup> The expense of running multiple runs of GCMs rises exponentially as the models grow increasingly complex and comprehensive in terms of the phenomena modeled. This has an important bearing on the validation of models to the extent that the expense of additional runs by which to examine the sensitivity and accuracy of a model becomes prohibitive.

Meteorology encompasses natural philosophy, weather observation, and forecasting, and as such dates back to Babylonian recordings of weather observations and the methods and theory of meteorology developed by the Greeks in the fourth and third centuries B.C. The development of the thermometer and barometer in the 17th century (AD) initiated the new era of meteorology in modern, Western societies. From its beginning in modern science, meteorology involved three different traditions which competed but developed separately until the middle of the twentieth century, the traditions of experimental (or “physical” or “empirical”), theoretical (or “dynamical”), and forecasting (or “practical”) meteorology (Nebeker 1995:1). Despite the data provided by thermometer and barometer, and despite the existence of relevant mathematical knowledge based on Newton’s work, meteorological theories remained almost entirely qualitative until the mid nineteenth century.

From the mid nineteenth century, some experimental (or “physical”) meteorologists made climatology (that is, climate, the “average” of weather) their specialty, but this remained a descriptive and statistical science until the middle of the twentieth century. As a result, until the 1930s, if they were offered at all, university courses on climate were taught in geology and geography departments. Geology and geography are disciplines with a traditionally practical orientation, shaped by their role in the U.S. Geological Survey (USGS). The USGS was established in the second half of the nineteenth century to explore and measure the territories. Before the middle of the twentieth century,

meteorology was oriented toward the study of weather, but it has since come to include study and forecasting of climate as well. Today, experimental (or “physical”) meteorologists are the ones who design and build instruments and new ways of collecting data, and who also collect and analyze them.

Many meteorologists working in the theoretical tradition made the laws of physics central to their approach, establishing dynamical meteorology. Initially unapplied in weather forecasting, physics and mathematics grew to assume a central role in forecasting of weather and climate with the development of the numerical computer, as will be described below. Weather forecasting developed with the beginning of daily releases of weather forecasts by national meteorological services in the 1870s. Weather forecasting was dominated by the synoptic approach until the second half of the twentieth century, at which point the numerical, computer-based approach came to dominate, as will be described below.

Three different approaches in meteorology developed separately and in competition until the mid-twentieth century at which point they were brought into closer relationship with the introduction of electronic computers. In physics and sometimes in climate research, these three are reduced to two traditions, theory and observation, with forecasters and experimentalists lumped together under the general heading of “observationalists” or “empiricists.” The difference between the two is that while synopticians (forecasters) and data analysts and experimentalists are similarly inductive in approach -- by contrast to theorists

who are deductivists -- experimentalists (or 'physical meteorologists') actually collect data to be analyzed by themselves or others, by contrast to synopticians who work with data sets from experiments they have not themselves been a part of setting up (i.e., the work of experimentalists). Since the development of computer based forecasting, those who formerly performed synoptic forecasting (i.e., without the use of computers and numerical methods) have shifted from weather forecasting to data analysis, also called diagnostics. As such, the better over-all label for meteorologists using the data-based (inductive) approach is "empirical scientists" or "empiricists."

It should be noted that many of the above distinctions are not easily made, as any one scientist may be a combination of theoretician, empiricist, and experimentalist. There is no such thing as a pure theoretician: all theoreticians work with data, just as all experimentalists and data analysts (synopticians and/or forecasters) integrate theory. Moreover, synopticians subdivide into those who are more experimentalist or more theoretical in orientation.

"Dynamicists" include all scientists studying the movement of air and ocean masses in the earth system; the label applies to synopticians, experimentalists, theoretical physicists as well as oceanographers (note: oceanographers are not considered meteorologists, but they are important in the study of climate, because of the central role of the oceans in regulating climate). Dynamicists study the movement of large masses (air, oceans), in contrast to chemists, who study the chemical composition of things. Tension can surface

between dynamicists and chemists because chemists can tend to assume in their studies of the atmosphere that the air is a fixed entity rather than a system in constant movement and change.

In the decades of the 1860s and 1870s, with the development of the telegraph, the first national weather centers were established in Europe and the United States and started to issue daily weather maps. This development enabled the “synoptic method” of weather forecasting, a method of mapping recorded weather data onto weather maps which allowed estimation of impending weather in surrounding areas. The premise of this method was that knowledge of the present weather over a large area can be used to produce foreknowledge of the weather within that area.

The considerable achievements of theoretical meteorologists played little part in the synoptic method which relied almost exclusively on recorded data and weather maps. The synoptic method of forecasting was not an exact science using calculations and physics-based theory. Synoptic forecasts were imprecise in nature, integrating no theoretical knowledge from physics for their forecasts and relying on experience and intuition. For example, in 1872 the Danish weather service cautiously restricted itself to three types of weather forecasts: “fine weather,” “unstable weather,” and “bad weather” (Nebeker 1995:39). Defenders of the synoptic approach argued that the importance of experience in this type of forecasting -- the only method of forecasting at the time --

established the authority of synopticians in meteorological forecasting. Thus, Pemter at the Austrian weather service wrote in 1903 that:

Since we have to do only with theorems founded entirely upon experience, the persons best qualified to make the predictions are those who through long years of practice have collected the most theorems as to the variations in the forms of pressure distribution, and have also learned by practice the many modifications to which these theorems are subject (Nebeker 1995:38). (Quoting Pemter 1903: 161).

Thus, the use of the weather map alienated experimental and theoretical physicists from the study of weather because weather forecasting from the beginning of state weather services in the 1860s to the middle of the twentieth century involved subjective processes and little theory-based calculation (Nebeker 1995:39).

But the “alienated” included scientific groups of high status in the exact sciences whose knowledge fit better with certain influential notions of “good science.” The modest success of the (synoptic) forecasters and their lack of theoretical framework attracted criticism of the synoptic approach from these meteorologists of competing persuasions, who described the synoptic forecasting technique as an “unsystematic, judgmental process that was not based on scientific knowledge” (Nebeker 1995:39). The criticism was so great that Nebeker considers it a contributing factor in the suicide of the first director of the British meteorological Office in 1865, the year before the British Royal Society recommended that daily forecasting be stopped for the reason that it

was not based on scientific knowledge (it was resumed seven years later) (Nebeker 1995:39).

In the meantime, other meteorologists were attempting to turn meteorology into an exact science by using the laws of physics to forecast the weather. The Norwegian physicist-turned-meteorologist, Vilhelm Bjerkness, was important in this development, because he advocated a calculational approach to weather forecasting in 1903. Bjerkness optimistically thought that the problem of weather had already been solved, in principle, by physics. The challenge was to bring together the full range of observation and theory to predict weather, something for which Bjerkness didn't consider a numerical approach; he did not believe that the physical equations defining the atmosphere could be solved analytically. Instead, he developed a graphics-based approach to forecasting, "graphical calculus." This method involves the drafting of meteorological dynamics onto maps, supplemented by use of graphical differentiation and algebra, to calculate the forced motions of the weather system. Graphical calculus gave some promise of being a suitable tool for calculating (forecasting) the weather (Nebeker 1995:51-56).

Bjerkness' idea of a calculus-based weather forecasting approach integrating observations and the laws of hydro- and thermo-dynamics was slow to catch on, and, according to Nebeker, it struck most meteorologists as "utterly impractical" (Nebeker 1995:85). For personal and practical reasons, Bjerkness himself switched from theoretical meteorology to practical forecasting in the

years following 1917, developing the method of “air-mass analysis” which was not based on physics but on “higher-level” rules specific to meteorology based on formal extrapolation of weather patterns.

Bjerkness’ “air-mass” approach to weather forecasting, integrating dynamical and synoptic methodologies, is associated with the Bergen School of Meteorology, established by Bjerkness in 1917. By the end of the 1930s, the approach was generally viewed as having improved weather forecasting substantially, and it had been adopted by national weather services worldwide. The American meteorologist Jerome Namias embraced the Bergen approach as a “practical method that the forecaster could use in his daily work,” and his 1940 enlarged edition of a 1935 monograph for the American meteorological society included contributions from members of the Bergen school (Namias, et al. 1940), [quoted and referenced in Maisel 1995].

The success of air-mass analysis did not eliminate the push for a physics-based meteorology, however. The following comment made in 1952 by English dynamical meteorologist, C. K. M. Douglas, reflects the rejection of Bjerkness’ practical forecasting by those continuing to insist on a physics-based approach:

The Norwegian work was often referred to as the “Bjerkness theory” but the word “theory” is unsuitable for anything in synoptic meteorology. It is really a technique based on simplified models of atmospheric structure and movement, and its successes have been based on its empirical rather than its theoretical aspects (Nebeker 1995:86)



Because there was even greater variability from forecaster to forecaster using Bjerkness' approach, Douglas argued that it was even more subjective than earlier techniques.

### ***Towards a numerical approach to meteorology***

The role of calculation in meteorology grew during the 1920s and 1930s. The amount of data processing increased sharply, though this was less because of more sophisticated processing than because of the intensification of data gathering. More people than ever before studied dynamical meteorology and sought to connect data and theory through the use of calculations.

After Bjerkness, the next big step towards a physics-based and arithmetic approach to weather forecasting was the discovery by Lewis Fry Richardson -- an English scientist (born 1881) with training in physics, chemistry, zoology, botany, and geology -- of a method by which to solve partial differential equations arithmetically, a method he applied to the prediction of weather in the period around World War I. Richardson attempted to use the basic equations of atmospheric motions to do a six-hour weather forecast, using only a mechanical calculator. It took him six weeks. As was well recognized at the time, the equations were highly complex and did not lend themselves to simple solution, such that the only way to solve the equations was to use numerical approximations. Unsure of the relative importance of the different factors in the atmospheric system, and thus unsure how to "weigh" them in his simplified representations, Richardson included a great deal of atmospheric physics, which

complicated the calculation process and resulted in highly unrealistic results (Washington and Parkinson 1986:4)

While the results of Richardson's forecast based on the numerical approach were highly unrealistic, his approach was very important to later developments of computer-based numerical modeling to simulate and predict the weather (and, later, climate). Simplifications as well as analog devices such as slide rules and graphical procedures are not adequate when long chains of calculations are performed, since errors can accumulate.

Academic mathematicians disdained Richardson's approach (Nebeker 1995), perhaps because it took recourse to approximations and because the arithmetical approach to solving differential equations rendered their scientific contribution, exact analytic solutions of theorems unnecessary. Whereas academic mathematicians sought ways to reduce to their simplest form the complex differential equations, the numerical method simply ground through the differential equations by means of algebra and calculus. The numerical approach required a huge amount of computation but could proceed on an incomplete understanding of the structure of the atmosphere (Nebeker 1995:76) .

Carl-Gustaf Rossby, a Swedish mathematical physicist (born 1898) was important in changing the perception among forecasters that theoretical, physics-based meteorological work had little relevance to their work. Rossby worked to render the Bergen techniques useful for forecasters, with important consequences in military maneuvering in the Second World War. Like

Richardson, Rossby was determined to get numerical answers out of theory, and he managed to produce equations for atmospheric dynamics which fit observations and could be solved by use of an ordinary slide rule. Through his calculations connecting theory and data, Rossby stimulated the interest of other prominent theoreticians in the field of dynamic meteorology.

Rossby was also instrumental in the institutionalization of meteorology as an academic discipline in Sweden as well as the United States, where he received a fellowship to study at the U.S. Weather Bureau in Washington. The field of meteorology grew as a result of the two world wars, as weather information was of great value in the increasingly sophisticated technologies of warfare, especially aviation. After each war, the field enjoyed increases in federal spending because of the recognized strategic value of meteorological forecasts. This led to remarkable growth in the field, especially during the period between the two world wars.

The field also owed a great deal of this growth to the gradual establishment of meteorology as an academic discipline. At the turn of the century, only Germany, Austria, and Scandinavia had professorships in meteorology. At this time -- and for the next several decades -- meteorology continued to suffer from the lack of recognition in colleges and universities. It was often considered little more than a small branch of geography or geology. Around 1920, the first professorship in meteorology was established in England at Imperial College. In 1928, the Massachusetts Institute of Technology

established the first professional-training program in meteorology in the United States, followed about a decade later by four other American universities: UCLA, Chicago, New York University, and Cal Tech. By 1951, eleven universities had departments of meteorology, four others offered graduate training in meteorology, and 82 others offered undergraduate training. The membership of the American Meteorological Society increased more than threefold between 1939-1949, rising from 1189 members to 3718 members (Nebeker 1995:120).

Rossby was instrumental in all of these developments, and he helped secure the dominance of the synoptic approach in meteorology. As a leading figure in the meteorological training program of the Army Air Forces during World War II, he helped train some 7000 meteorological officers in synoptic meteorology, using the 1940 text book by Namias et al. The same book based on the Bergen approach was taught as part of the training programs in the universities (Maisel 1995:15) [she cites as source an interview with Chester Newton dated August 25, 1993].

### ***The beginning of the computer era – and theory – in meteorology***

The role of theory in meteorology grew from the beginning of World War II onwards, as the expanded observational networks and the new technologies for collecting data (e.g., the airplane, which brought upper-air data sets) rendered physics more relevant to forecasting. In 1939 and 1940, Rossby introduced two important physics-based algorithms that proved important for weather forecasting during World War II. An important legacy of the war was the large-

scale government-sponsored research program. In the period after the war, the National Science Foundation declared meteorology a “field of special interest,” and it supported efforts to improve forecasting. The hope was to develop an objective style of forecasting based on algorithm. By the late 1940s, articles on algorithmic forecasting were common, but continuing well into the 1950s, subjective forecasting produced better forecasts than those based on algorithms. As Nebeker writes, there was, overall, little improvement in the time range and accuracy of forecasts between the late nineteenth century and the mid-twentieth century when computer forecasting started (Nebeker 1995:120-132).

The increasing amount of data collected required new methods of processing and analysis. For example, while an English forecaster in 1913 could deal with all relevant information, gathered through means of the telegraph from about thirty stations around the world, his counterpart in 1947 had to process reports from more than a thousand stations, and each report was much more detailed than those issued thirty-four years earlier. The data deluge meant that forecasting started to become a group activity, and various technological means came to assist the process, including punched-card machines, tabulation machines, and electronic calculators. Even so, these existing techniques were far from powerful enough to allow a comprehensive and effective numerical approach to forecasting.

In the early 1940s, the scientist John Maunchly studied geophysical data seeking to establish through statistics a connection between solar phenomena and terrestrial weather. This endeavor required extensive calculation which not even intensive human-power could effectively carry out -- the same problem Richardson had two decades earlier. This problem in meteorology inspired Maunchly to abandon his study in favor of the attempt to build a digital calculating device and, with a colleague, Maunchly came to be principal designer of four famous computers: the ENIAC, the EDVAC, the BINAC, and the UNIVAC (Nebeker 1995:97-99).

Also involved in the process of building the ENIAC at the Moore School of Engineering at the University of Pennsylvania in 1944 was John von Neumann, a mathematician who had earned fame for his axiomatization of quantum mechanics, and the principal founder of a new branch of mathematics, game theory. Von Neuman was an expert on theories of shock and detonation and worked for the Manhattan Project at Los Alamos, New Mexico, from 1943 to 1945. He became interested in computers while working as a consultant to the Army. His specific interest in using computers was to advance mathematical and scientific theory. In order to this, he needed to find a scientific problem which so far had resisted solution but which would yield to computation, and -- to secure funding and recognition -- he wanted it to be a problem of practical import. In 1946, he decided that the weather forecasting problem in meteorology --

complex, interactive, and highly nonlinear in nature -- would serve his goals (Nebeker 1995).

Von Neumann managed to secure financial support for his "Meteorology Project" and in 1949 obtained access to the ENIAC at the military's Aberdeen Proving Ground. Gathering people for the project was difficult because there weren't many meteorologists with the training in physics that he needed, since the non-physics-based synoptic approach was the norm. But von Neumann eventually managed to attract several scientists with backgrounds in meteorology and mathematics or physics, including Jules Charney and Philip Thompson, as well as the Norwegian synoptician Arnt Eliassen (for one year). This group performed the first successful numerical weather prediction using a digital computer. By the 1950s, weather prediction through the use of numerical models became routine, and models were used to simulate and project climate.

Von Neuman's efforts marked the beginning of a new style of meteorology which came to dominate from the 1950s and 1960s onwards, decades which brought fundamental change to meteorological research and weather forecasting because of computers. Reminiscent of Richardson's approach thirty-five years earlier, the computer enabled the new style of meteorology which was based on calculation. The new method in meteorology required new skills and collaboration: the production of implementable algorithms to produce useful information about the atmosphere couldn't be devised without a combination of the skills of a numerical analyst, a mathematical physicist, and a synoptic

forecaster. This style encouraged focus on quantitative data only -- data with the accuracy and geographic distribution required for calculation -- and an interest in only quantitative theories, rendering mathematics and physics as indispensable in meteorology as meteorological knowledge of weather dynamics. Because of the sudden prominence of numerical meteorology, universities broadened their programs to include training in this new approach to the study of weather on top of the three traditional approaches, experimental (or “physical” or “empirical”) meteorology, theoretical (or “dynamical”) meteorology, and forecasting (or “practical,” sometimes also called “empirical”)) meteorology (Nebeker 1995:152-155).

The limited memory of the early computers rendered forecasts beyond a few days impossible. By 1955, the first numerical model able to reproduce the (longer-term) main motions of the earth’s atmosphere was designed by Normal Philips. Increases in computer power gradually enabled such modeling, called “general circulation modeling.” While the large computer systems used for operational forecasting remained hugely expensive, their cost remaining more or less constant from 1955 to 1967, their computational power greatly increased. The cost of five million computations fell from 42.00 dollars to 20 cents during that same period. The development of high-level programming languages such as Fortran also facilitated computing by rendering the computers user-friendly for meteorologists. The increased speed of the computers enabled tremendous development of models in meteorology, which progressed from the “hand-



computed" 1-dimensional models based on vorticity conservation in 1949, to the 2-dimensional barotropic model which was run on the ENIAC in 1950, to the 2 1/2-dimensional baroclinic model run on the IAS Computer in 1952. Yet another momentous development occurred in the late 1950s to mid-1960s when models based on so-called primitive equations (the basic equations of physics describing the atmosphere) replaced the earlier baroclinic models, further securing the role of physics at the center of meteorology, sidelining the comparatively less precise synoptic approach to forecasting which also was less objective (Nebeker 1995:162-170).

The meteorological profession continued to enjoy steady growth during the 1960s. In 1960, The National Center for Atmospheric Research (NCAR) was built in Boulder, Colorado, under sponsorship of the National Science Foundation. Its mission was to plan, organize, and conduct atmospheric (and atmosphere related) research programs in collaboration with universities. It was designed to explore a broad range of investigations into the basic processes that drive the weather (and, later, climate), and to provide research tools and facilities to the entire atmospheric sciences community in an age where research was becoming prohibitively expensive for individual scientists at universities and smaller, diversified research institutions. Thus, NCAR was created in part in response to the growth of atmospheric research and the developing need for expensive equipment to carry out the research. Also important in the establishment of NCAR was the fact that meteorology suffered in terms of

prestige within the sciences; it was thought of as a field for scientists who had flunked out of physics.<sup>44</sup> NCAR was the brainchild of a group of meteorologists who wanted to change this perception, concerned to raise the influence and prestige of their field within the larger scientific community. They established NCAR as a means to change the negative perceptions of meteorology, in part by setting national standards of research quality and significance. The choice of the words “atmospheric research” instead of “meteorology” in the name reflected the increasing diversity of fields contributing to atmospheric research at the time; as mentioned above, with the development of numerical weather and climate models, scientists from other fields became involved in weather research, including theoretical mathematicians, the scientists endowed with the greatest prestige within the scientific community; it was no longer just meteorologists or geologists -- scientists relatively lower on the scientific totem pole -- who studied weather (and, increasingly, climate). No doubt, the new name was also strategic in the sense that it undermined conceptions which equated meteorological research with (synoptic) weather forecasting, thus deflecting residual evaluations of meteorology as a less prestigious science.

With time, numerical modeling grew to become the dominant approach to forecasting, and the central enterprise in meteorology. The interest in applied meteorology continued, as meteorological knowledge was implemented to aid

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<sup>44</sup> John Firor, interview, January 1998.

decision-making about factory locations, vehicle design, and choice of construction materials and techniques, and as commercial aviation, interest in weather modification techniques, and environmental concern about air pollution grew. Scientific interest in global circulation models grew rapidly in the 1960s for several reasons: the continual improvement of computers, the increasing ability to apply basic theoretical concepts (i.e., “primitive equations”), and the improvement in quantity and quality of data because of advances in international coordination and in observational techniques, particularly satellites. Computers also came to be used in the production and handling of data sets, with the goal of increasing efficiency while reducing costs and human errors. In 1953 the U.S. weather Bureau recommended automatic data handling, saying that “human intervention should be avoided whenever possible” because computers were more effective in detecting “bogus data” (Nebeker 1995:176).

As computer modeling became a dominant methodology in meteorology, more and more meteorological research was oriented towards the improvement of models, including that of non-modelers, efforts to improve the modeling thus increasingly directing efforts in both research and observation. Theoreticians, who had always worked to explain the results of observations, now often worked to explain the results of simulations, and observationalists increasingly oriented their endeavors towards gathering information of use as input to the models. Many methods in meteorology (e.g., statistical analysis) lent themselves to the growing orientation towards computers. This was less the case for other

methods, particularly the Bergen School synoptic approach, which thus benefited little from the growth of computer technology. Synopticians sought to redirect their activities toward areas least affected by the computer, or where their knowledge was necessary as a supplement to computers. Thus grew what is now known as diagnostics and “synoptic analysis.” The latter is the practice of deriving from irregularly-spaced observations the values of meteorological variables at the regularly spaced grid points used by the numerical models. This process of calculating the values between measured data points around the globe produces global data sets without the gaps that characterize surface measurements. Satellite-derived data sets tend to be more uniform in coverage, but even these are limited in precision and, sometimes, in coverage, thus similarly requiring synoptic analysis also known as “reanalysis;” ruptures in communication between satellites and land-based stations lead to gaps in the records, gaps which are all the more prevalent in land surface-based measurements because of the uneven geographical distribution of weather stations.

From the mid 1940s to the 1980s, there was almost a ten-fold increase in the number of American universities awarding advanced degrees in meteorology (Nebeker 1995:173), and by 1997 sixtyseven universities in the U.S. offered graduate degrees in the atmospheric sciences (Stull and Businger 1997). Along with this increase, meteorological research has grown to become a group enterprise and a Big Science -- and the field has continued to broaden to include

scientists from a wide range of disciplines producing knowledge of relevance to atmospheric studies. The resources required to run the increasingly complex climate models simulating the interactions between the atmosphere, oceans, and land-masses are so extensive and expensive that few institutions can carry them out. As a result, climate modeling efforts are concentrated in a few centers around the U.S., and the few other countries in the world who undertake climate modeling (primarily England, Germany, France, Canada and Australia) have only a single, national center. In the U.S., the development of GCMs is supported through a coordinated effort of the U.S. Global Change Research Program (USGCRP) and five Federal agencies (DOE, NASA, NSF, NOAA, and EPA); during the Fiscal year 1994 funding of modeling efforts totaled about \$45.3 million (U.S. Government 1996). Climate modeling in the U.S. is largely concentrated in a handful of centers. Von Neumann's group, which in 1955 consisted of five persons, grew steadily over the following decades, a harbinger of the new, highly collaborative style of research which now characterizes most meteorological research. In 1959, von Neumann's group was renamed the General Circulation Research Laboratory. In 1968, the laboratory was renamed the Geophysical Fluid Dynamics Laboratory, five years after which it was moved to Princeton, where it has been located ever since.

NCAR is another center and leader in weather and climate modeling. NCAR exemplified the new, collaborative and technology-intensive style of research. Throughout the years, NCAR's staff has been gradually expanded to

include the earth sciences more broadly, reflective of the development of the technology and ability to study at the global scale, of concern about global environmental change, and of general recognition of the interconnectedness of the earth system's different components. Thus, today, NCAR's staff includes meteorologists as well as solar physicists, atmospheric chemists, cloud and aerosol physicists, engineers, oceanographers, biologists, geographers, solid earth geophysicists, economists and political scientists, among others.

Other U.S. centers for climate modeling are the California-based Lawrence Livermore National Laboratory (LLNL) under sponsorship by the Department of Energy, NASA's Goddard Institute for Space Studies (GISS) in New York City, and two smaller, lower-profile groups at the University of Illinois at Urbana-Champaign, and at Oregon State University in Corvallis. Internationally, these U.S. centers share prominence with other modeling centers around the world, particularly the Hadley Center, the United Kingdom Meteorological Center and the European Center for Medium-Range Weather Forecasts (ECMWF) in England, the Max Planck Institute in Germany, the Australian Numerical Meteorology Research Center (ANMRC), and the Canadian Climate Center (CCC). France, Japan and the Netherlands also have modeling centers in the process of establishing themselves internationally.

Compared to other nations, modeling efforts in the U.S. are more dispersed, with numerous, simultaneous and competing modeling efforts even within institutions, each involving only a handful of people or less. By contrast,

European countries have tended to establish a single national center and to impose a pooling of effort of all scientists in these institutions, and this has worked to press the field as a whole towards ever Bigger Science. The scientific advances of such national centers as the English Hadley Center and the German Max Planck Institute have forced greater centralization and coordination of modeling efforts both within and between the U.S. modeling centers. This has brought about a new level of streamlining within an institution such as NCAR, which earlier had a more laissez-faire managerial approach, allowing multiple different modeling efforts to develop independently of each other. NCAR was therefore the target of considerable attack during 1994 and 1995 by people who thought the four or so independent modeling efforts at NCAR ought to be streamlined and coordinated into one large effort. Since 1996, NCAR has responded to such criticism by starting one large integrating earth system modeling effort, the Climate System Model (CSM). Management considered such integration of modeling at NCAR necessary if NCAR was to be competitive in the field in this age of Big Science, finding that in order to be a leader in the field of modeling it was necessary to expand beyond small efforts, each involving only a few people, to big-group efforts involving the combination ("coupling") of the various smaller models into one big model, the CSM.

This development has also served to secure ever more funding for climate modeling at an institution such as NCAR, at the expense of other scientific projects. Currently, the costs of acquiring and operating the supercomputers at

NCAR costs between 11-12 million dollars per year. Different scientific groups write proposals to do experiments on the computers, and “computer time” is a hot commodity for NCAR scientists who use the computers for a variety of purposes. However, due to the concern about human-induced climate change as well as intensified international competition in climate modeling and improved ability to build increasingly more comprehensive and complex models, ever more computer time is granted to climate modeling through top-down management. The Climate System Model takes up about half of the total computer time at NCAR, a huge increase compared to earlier, less complex models.

Thus, as the field of meteorology has grown, numerical modeling has come to be the dominant methodology and goal, and climate modeling has come to be prioritized over weather forecasting at the national atmospheric sciences super-computing research centers, while weather forecasting has come to be centered at the National Weather Service.<sup>45</sup> Within federal atmospheric research centers, the scientific practices of non-numerical modeling meteorologists have been reshaped as the computers have taken over important parts of their functions. Such scientists have found their roles redefined such that they increasingly serve the further development of the models, this development tending to be prioritized by funding practices at the national agencies and within the research centers. In short, it is not surprising that some meteorologists are less enthusiastic about the recent turn towards a focus on climate models. The



role of these developments in criticisms of the GCM-based projections of human-induced climate change will be further described in the following chapter on the producers, consumers, and critics of the climate models.

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<sup>45</sup> Weather forecasts emanate from the National Weather Service to media channels, after some processing by commercial weather companies.



VOLUME II

CLIMATE RHETORIC:  
CONSTRUCTIONS OF CLIMATE SCIENCE IN THE  
AGE OF ENVIRONMENTALISM

by

MYANNA H. LAHSEN

**Chapter 6.**  
**FROM MODELS OF REALITY TO THE REALITY OF THE MODELS: CLIMATE  
 SIMULATIONS AS CONTESTED CONSTRUCTIONS OF REALITY**

In the debates about predicting the climate, however, it appears that the shortcomings and uncertainties of both the global circulation models and of global observations are being understated by their proponents. (This is a natural response to a competitive funding environment.) As the late Nobel-laureate, Richard Feynman would probably say, the scientists are fooling themselves.

(Georges 1992)

Understanding is not having the number. Understanding is deeper and more cognitive. Putting a meaning to it. The number by itself, in a sense, has no meaning.

Empirical meteorologist, speaking about climate modeling

And there's a tendency to say that you are not doing things properly unless you can construct a complete mathematical theory of every event that you have observed.

Empirical meteorologist

This chapter describes the climate models themselves as well as computer modeling as a scientific practice and explores certain tensions between modelers and empiricists. "Empiricist" here denotes scientists within the atmospheric sciences and related fields who gather and/or analyze climatological data. Within meteorology, empiricists tend to be more critical of these projections, in part because they are well-positioned to identify discrepancies between the models and the data. However, I suggest that the tension between such empiricists and modelers -- and the greater level of skepticism concerning the theory of human-induced climate change among empiricists -- also in important respects reflects a discontentment due to changes

within the field of meteorology with the rise of climate modeling, changes in turn rooted in broader social changes in American society since the 1960s.

Traditionally, climate scientists have been divided into three broad groups: theoreticians, observationalists, and weather forecasters/synopticians. But with the development of computer modeling, a new hybrid form of scientific inquiry (and of scientific practitioners) has developed that, more than previous practices, bridges the divides not only between theory and empiricism, but also between different disciplines. Computer modeling is so different that some refer to it as a new mode of science. As a mode of science, modeling has become an important tool by which to explore environmental problems involving complex, interlinked processes requiring multi- and inter-disciplinary approaches. Customary ways of producing scientific knowledge tend to be narrowly disciplinary in scope, and are thus less than ideally suited for exploration of ecological problems. Unlike most customary research methodologies, modeling is conducive to the study of ecological problems in all of their dimensions, and modeling tends towards ever greater integration of both physical and social processes. This underlies much of modelers' current success in obtaining funding and broad-based recognition of their scientific products. Modeling is inherently non-disciplinary in the sense that it is not limited to any one dimension of the system being modeled; if the variables are commensurate, any number of disciplines can be integrated into a model. Key limiting factors in how much is included in any particular climate (or "Earth system") model are - besides the interest and goal of the modeler (i.e., what is she or he intending to explore): (1)

how well the phenomenon considered for integration into the model is known (phenomena not well known and not easily quantified is not easily integrated) and (2) the amount of computer resources available; the more comprehensive a model, the more computer power and time is needed, and both are very costly.

The interdisciplinary nature of models also underlies some of the resistance to them. Interdisciplinary work tends to be suspected by those who value and work within more narrow, disciplinary paradigms. Scientific status is usually obtained through the in-depth and precise knowledge more easily obtained through disciplinary and highly specialized scientific focus and practice. Francis Bretherton, one of the scientists involved in the development of interdisciplinary approaches to the study of climate change in the late 1980s, recognized the tension between doing such research and obtaining scientific status. As one of the builders of the International Geosphere-Biosphere Program, the aim of which was to transcend disciplinary divisions to foster better understanding of the Earth system, Bretherton recognized early on that

There are few individuals who are thinking hard about putting the whole thing together. It will take guts. It has to be someone who is not too concerned about his professional reputation, because he will have to simplify to a ridiculous extent to get it within the compass of a Vax at most. (Edelson 1988)

As a relatively new scientific practice, climate modeling -- like many new scientific simulation fields -- can seem problematic from the perspective of traditional scientific procedures because of the complexities of integrating data and verifying and replicating projection experiments. Many simulation fields struggle with the need to separate out the "noise" or artifactual results of the

modeling techniques themselves from the “real world” signals the techniques are meant to model. Though the reason for modeling is a lack of full access, either in time or in space, to the phenomena of interest (i.e., the inability to fully observe), acceptance of the validity of the climate simulations, and their efficacy in decision making, depends on their perceived correspondence with reality, the real climate system. Since it is impossible to check projections about future climates against observation, a model's reliability is tested by how well it simulates the past and the present, which can be checked against actual (if incomplete) observations. However, the “real” is not readily accessible and knowable, as I will show. As a result, though models are valued precisely for their ability to probe possible future consequences of current actions, thus enabling decisions about preventive action, there is likely to be resistance to model-based projections until they have been proven empirically.

The models are only as good as the scientists' understanding of how the climate works and they depend on the availability and quality of data; the quality of the output depends on the quality of the input. Among scientists, this is often put more casually as “garbage in, garbage out.” Climate models suffer in particular from the difficulties of performing long-term projections integrating incomplete, faulty, and biased global data sets. These problems, along with the fact that many key relationships between different climate factors remain poorly understood, render the act of modeling problematic even without factoring in projections of human activities and their “forcing” effects on the “natural” system. As such, it is a

science in which the relationship between the simulation and the real world in important respects is unclear and unknowable.

To some among the older generations of scientists who experienced the period when the postwar social contract meant abundant research funding with little accountability, climate modeling also represents many developments in science that they regret. These developments include the increasing demand from government for “useful” science, a demand which has led to a stronger top-down approach in management, and more conditional and stringent federal funding practices. Models have benefited from this general shift in federal funding policy since the decades immediately after W.W.II. The use of models has brought about important changes within meteorology. To scientists who value science for the sake of science more than immediate applications of scientific knowledge, modeling is an unfortunate development to the extent that the models are used not to advance basic knowledge but to produce policy-relevant numbers (“answers”), the accuracy and meaning of which are disputed.

Though most acknowledge the limitations of the models, sympathetic or otherwise insufficiently critical groups outside the scientific community sometimes credit the output (results) of models with greater certainty than may be warranted. This inclination is only strengthened by the tendency for modelers themselves to be seduced by their models. Climate modeling tends to seduce its practitioners into believing their models to be accurate representations of “reality” -- and, even if modelers themselves are not thus seduced, a competitive national funding situation in a social context of environmental concern encourages them to publicly



associate greater certainty with their modeled projections than many climate scientists consider warranted. All of this fuels criticism of the modelers and resistance to their simulation-based projections of human-induced climate change.

### ***Brief introduction to GCMs***

Supercomputer climate models<sup>46</sup> are the most central basis for current projections and concern about human-induced climate change. General Circulation Models (GCMs) are based on physical laws represented by mathematical equations. Through innumerable numerical computations, models simulate the complex interactions between the components of the earth system, the time-dependent three-dimensional flow of mass, heat, and other fluid properties. As such, GCMs are very similar to numerical weather prediction models, out of which they also developed.<sup>47</sup>

Modeling of climate began to appear possible by the latter half of the 1950s and went through great improvements during the 1960s. Numerous factors caused these improvements, including, importantly, a new, emergent commercial computer industry which effected great improvement in computer capability and simultaneously decreased their cost. A necessary development for modeling was the large, global data sets data assembled through satellite

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<sup>46</sup> For the sake of ease, I will simply use the term “models” and “climate models” rather than specify in each instance that I am referring to “GCM modeling” in particular. It should be made clear, however, that scientists of all disciplines and practices use models, and that models therefore can constitute a wide range of things. For example, a mouse may serve as a “model” in medical research.

<sup>47</sup> Climate is the “average” of weather, that is, the average of temperature, pressure, humidity, wind velocity, cloudiness, trace gases, aerosols, among other elements, including statistics relating to variations in weather elements. The time period of weather averaging which comes to be defined as climate is not fixed. It can be chosen to be as short as a season (and even shorter than that), but usually the period used is a decade or more.

technology and resulting from international scientific efforts at coordination and data gathering. While the situation has improved greatly since the 1950s, lacking or biased data sets continue to be a limiting factor. The first successful numerical weather prediction model was developed in the [late 1950s or early 1960s?] on one of the earliest computers by a team of meteorologists and mathematicians at the Institute for Advanced Study in Princeton, New Jersey (Somerville 1996:61).

The complexity of GCMs, and the intensive resources required to run them, means that only industrialized countries currently fund research centers that develop and run them. NOAA, NASA and the NSF now provide research funding for the three main modeling centers in the U.S. Internationally, the three U.S. centers share prominence with two other modeling centers in particular, one in England and one in Germany. In the U.S., the development of GCMs is supported through a coordinated effort of the U.S. Global Change Research Program (USGCRP) and five Federal agencies (DOE, NASA, NSF, NOAA, and EPA); Fiscal year 1994 funding of modeling efforts totaled about \$45.3 million, though the overall effort of modeling, including data collection and processing, brings the total up significantly higher (U.S. Government 1996). The costs and efforts involved in doing even a single model-run also limit rigorous testing of their output. The more runs one can do with slight variations in the equations and assumptions, the more can be learned about the error bars and problems in the models and the projections; if only a few runs can be afforded, little can be learned.

A variety of climate models are used to project potential global climate change, but the GCMs -- General Circulation Models -- are the most complex and the most influential models supporting concern about human-induced climate change. GCMs which simulate the greatest complexity of the earth system's components are called "Earth system models," and those that deal exclusively with climate are called "climate models" or, if they are more comprehensive, "climate system models." Earth system models differ from the other models most centrally in their inclusion of a vegetation model, an aspect of the earth system which is now being integrated in some modeling efforts. Initially, models exploring the effects of greenhouse gases on future climates included only atmospheric dynamics, as if these were separate from the dynamics of oceans and land surfaces (and of the vegetation which is both in oceans and on land). These "atmospheric models" were then expanded to include simplified ocean dynamics, and, later, to include even more complex ocean dynamics. Coupled atmosphere-ocean (only) models are still the norm, though most major modeling centers now are attempting to include vegetation models and land-surface models (again, these are not entirely the same thing because vegetation exists both in the oceans and on land).

I will allow myself here to use the terms "climate models" and "GCMs" loosely, that is, without implying that the models in question do or do not include more of the earth system, as this isn't always clear to outsiders, nor even to modelers (except to the model's developer). Suffice it for the purposes of my analysis to note that the move through the history of model development has

been from more simple models of the atmosphere only, towards models which simulate atmosphere, oceans, and land dynamics; currently, GCMs typically consist of coupled ocean-atmosphere models integrating basic fluid dynamical equations. Increasingly these models attempt to include not just ocean and atmosphere dynamics but also those related to land-masses and vegetation, and the interactions between all of these components of the earth system.

The models calculate the extent to which the basic physical laws governing the sun, atmosphere, oceans, land and other elements of the climate system dictate variations in things such as temperature and precipitation over time and under different conditions. Once a model is considered sufficiently realistic, it can then be used to explore reactions of the simulated climate system to the “forcing” introduced by human emissions of carbon dioxide and other greenhouse gases.

One big limitation of the models in predicting future climates is that despite the great advances in computer technology, their “resolution” remains coarse. Unable to calculate climatic changes everywhere in the atmosphere or climate system, they make the calculations only for units of three-dimensional grids that typically are about 150 miles horizontally and less than half a mile vertically. The global models divide atmosphere and oceans into large three-dimensional grid boxes and thus fail to include important climatological factors of sub-grid scale, such as cloud formations. Modelers approximate such factors as best they can, but they are limited by incomplete understanding of clouds as well

as many other key processes that may work to either further warm or cool the system as a whole. Oceans are important in the climate system as they absorb both carbon dioxide and heat, and because they transport heat from one part of the planet to another, thus influencing the heat and moisture balance in the atmosphere. Yet models that include oceans usually do so only in a limited way, e.g., with a horizontal grid covering only a certain amount of the oceans' depth. It is only within the last few years that some models have started to include some of the deep-ocean processes that are so critical to the climate system's functioning.

### ***Models and the scientific method***

Scholars have described the "scientific method" as involving the following steps:

- (1) postulation of a model based on existing experimental observations or measurements;
- (2) verification of predictions based on this model against further observations or measurements;
- (3) adjustment or replacement of the model as required by the new observations or measurements. (Walker 1963:5)

Climate models seek to do all of the above, but global modeling presents considerable problems with point (2) and (3) because model results aren't easily checked against observations.

Models have severe data problems because they are used for predicting the distant future of a global climate system. Data sets are limited in terms of the climate variables they include and the geographical areas covered, and they can also involve considerable bias, the nature of which is difficult to know and compensate for. Data records of land and sea surface temperature exist for the last hundred years for large regions of the world. But changes over time in thermometer quality, location, number, and measurement method introduce bias and uncertainty. For example, a majority of thermometers are based on land and tend to cluster in and near urban regions, which on the average are warmer than surrounding, non-urban and non-asphalted areas. Sea surface measurements are taken primarily in shipping lanes, thus leaving out less frequented ocean regions. Scientists know that such biases exist and seek to compensate for them.

Global data sets gathered by means of orbiting weather satellites began to provide the first truly global pictures of the atmosphere during the 1960s. Satellite data are highly valued and extensively used because of their global coverage, in contrast to the point-specific and uneven nature of data sets gathered through land and sea surface thermometers. Yet satellite measurements present their own problems, because they look down from above: their accuracy is limited for phenomena at low altitudes, the detection and satellite representation of which may be distorted by optical effects created by invisible (to the human eye and the satellite) water in the atmosphere between the satellite and the point being measured. Moreover, the lifetime of satellites is

typically two-to-five years, their instruments gradually -- or sometimes suddenly -- lose their calibration, and loss of communication between satellites and computers on the ground can cause “holes” in the data set.

The best solution scientists have found in face of the problem of biased and lacking data is “data reanalysis,” a theoretically curious process by which one set of computer models are used to make up missing data, or to correct presumptively biased data, for use in another set of computer models. To deal with these problems in both surface and satellite data records, actual observations are entered into computers programmed to “smooth” the data by filtering out abnormalities. In this way, data is made to conform with statistically calculated “normal” average means of the climate variables covered by the data. Besides thus “correcting” data to compensate for bias and for abnormalities (*assumed* to result from errors in measuring and instruments), these intermediary models “interpolate” (calculate) data for the regions not covered by surface thermometers and for lost data in the satellite records. This is done by using known data points to estimate the points in between for which measurements are lacking. This process of “data reanalysis” is sometimes performed by humans, but in most cases, and now automatically in the case of satellite data, computer models are set up to do this reworking of data sets. As Paul Edwards has written about this practice: “[i]n this seemingly paradoxical mirror world, data used to validate one class of models are themselves the product of other models” (Edwards 1997).

While this is considered necessary intermediary step to enable climate modeling, the practice of “correcting” data and of filtering out data which falls out of the range of what is thought to be “normal” in the climate system might work to erase the anthropogenic signals in the data (i.e., possible signals suggesting that the climate is being forced by human activities). The case of ozone depletion serves as an example of how this can happen, because two important events related to the use of computers in the process worked against the detection of the ozone depletion. The discovery of stratospheric ozone depletion was made by a team of British scientists in 1985, based on review of land-based measurements. They found that ozone levels recorded during the Antarctic springtime had fallen by about fifty percent since the 1960s. This huge change in ozone levels was soon confirmed by Japanese and American scientists when they rechecked their measurements. The ozone depletion had gone undetected by U.S. measuring satellites because the computers through which the satellite data were filtered had been programmed to automatically reject ozone losses of this magnitude as anomalies far beyond the error range of existing predictive models (Benedick 1991). These observations were contradicted by the existing model projections of ozone loss due to CFCs, the models which underpinned the control provisions of the Montreal Protocol. These models had assumed a probable global average ozone loss of around 2 percent by the middle of the twenty-first century, a level of ozone loss which the 1985 discovery showed to have already been exceeded. Thus, both in their predictions for the chlorine-induced Antarctic phenomenon and for the extent of ozone depletion over the



rest of the planet, the models had underestimated the level of ozone losses (Benedick 1991:111).

In this “mirror world,” models are not easily validated through comparison to observations, and a certain “incestuous” nature of the process of validation has been noted by modelers and model analysts alike. Unlike weather models, the projections of which are quickly validated by observations, validating climate models requires data records covering several decades. The realism of climate models can be checked for the grossest errors against the benchmark offered by the seasonal cycles. But the more fine-tuned validation of the climate simulations is difficult. Past decades are simulated to gauge the ability of any particular model to simulate the current climate, which provides a basis for gauging its ability to simulate future climates with different scenario assumptions in terms of levels of greenhouse gas emissions. But since models seek to integrate all the data available, it can be difficult to keep an alternate set of complete and independent data sets against which to judge the level of approximation to the real world obtained by any particular climate model. Further complicating the validation of the models is the huge expense of doing slightly different runs (experiments) on the same model, a means of testing assumptions and establishing an error bar against which to gauge the accuracy of any given forecast, which means that projections often either fail to include error bars or include error bars of such magnitude that the worth of the projection is highly questionable.

***Feedbacks: Linear models for a possibly more circular system***

The possibility exists that models fail to include important feedback processes which might amplify or counteract the effect of the increased levels of greenhouse gases in the atmosphere. The concept that interconnected (“coupled”) subcomponents of the larger earth system work to maintain the earth system at a certain threshold (that is, until it is perturbed to such extent that it enters a new threshold), is a central concept to the Gaia theory advanced by Lovelock, for example. But GCMs do not include many of the most important feedback mechanisms because these aren't sufficiently understood at this point.

Models attempt to include a parameterized value for the role of clouds in the earth system, a central feedback mechanism. But it is still not clear whether clouds constitute a negative or a positive feedback, resulting in a cooling or a warming effect, when the climate system is forced. This is one of the points at which significant uncertainty is introduced into the model. As the empirical meteorologist and hurricane expert Bill Gray explained:

I think that most of these modelers don't understand the process of convection, and how condensation and recycling of the atmosphere works. To get at that they have had to parameterize it, and they may not do it right, they may not do it right, and they do it in a manner that in time gives water vapor pick-up. We don't know how it works. This is not well understood at this time. It is very controversial. In order to model this, they have had to put in heating schemes and parameterize their things in.

Phenomena are sometimes sidelined or entirely left out of models because they aren't easily quantifiable. This is the case with important aspects of the biosphere. The overall warming or cooling of the globe is calculated in part

based on the known albedo effect of the earth, that is, the amount of sunlight the earth's surface doesn't absorb but reflects back into space. Snow and ice, for example, being better reflectors than earth or vegetation, increase the albedo of the Earth because they constitute better reflectors than earth or vegetation. Modelers can make rough estimates of the yearly effect of snow and ice on the albedo of the Earth, but estimating the overall albedo of the biosphere is much more difficult due to the heterogeneity of its composition: different plant species have different reflective capacities, and therefore differential absorption of heat. Moreover, the overall albedo effect of a single leaf differs whether it is turned up or down, as one side tends to be glossier and therefore more reflective. Though some models now are attempting to integrate land surface and vegetation, many others leave vegetation out altogether due to the difficulty of representing it with a level of certainty that is considered acceptable. Modelers tend to prefer to leave out aspects they don't understand well and which therefore introduce (heightened) uncertainty into the models. Of course, leaving them out causes the same uncertainty, but it is an invisible one.

Part of the problem is scale: on the whole, biologists have a hard time transcending the small-scale and extrapolating from close studies of small lots to the global scale. On the other hand, biologists can be heard complaining that mathematicians and physicists, with their strong emphasis on quantification, disregard the role of the biosphere because they don't know how to deal with that level of complexity and uncertainty.

To some extent, modelers make this necessity into a virtue. One modeler devalued the role of biology as follows:

LAHSEN: and where do the biologists fit in all this? I have heard that people haven't wanted to deal with biology in this because it is so difficult to quantify--right?

MODELER: mm-hmm [yes]. There are different reasons. There's that, but for most of what we are doing, fundamentally, biological things don't matter a damn! The question is, what do you want to do, with biology. Treating the fluxes of plant biophysics, that is something we do, and that is of some importance -- although it is not enormously important. We can do something reasonable with some fairly simple methods. [...] Well, one reason why a lot of people aren't all that interested in the biology is that we don't see yet any evidence that we need to include that in the simulation. Another reason is that there is little natural vegetation left, that is, one step beyond ecosystem models. Of what possible relevance are those today; how many of them are there out there today?! [His tone of voice is suggesting very few, and with little "possible relevance"]. We need to know what the distribution of plant types are, and some of the other biophysical properties, in order to do the physical climate simulation. If you wanted to do fairly long climate simulations, then you might well want to be able to deal with the biogeochemistry internally, for all the greenhouse gases. We want to do that kind of thing with [their Earth System model], but it is not really clear to me that you need to do that to deal with the climate change scenarios, for example, or even that it is desirable to do that. For a lot of purposes, you don't want to add in unnecessary complications -- unnecessary complications are just places to go wrong; it doesn't improve anything. And if your sensitivities in your feedbacks are not well known, then you have no confidence -- then you are much better off specifying something than you are putting in a feedback with the wrong sign -- which you may not know; you may not know the system well enough and not know how the system responds. You may be worse off by including things. And again, it depends on what kind of problem you are after. The first thing to do with a biogeochemistry model would be to go back and run the physical climate system, with specified concentrations of greenhouse gases. And given that simulation, can we use the biogeochemical model to reproduce the greenhouse gas concentration that we forced it with? Only when knowing the basic inputs to the system, and how the system evolved with time, can we actually reproduce the trace gas level. And until we can do that, it would be silly to include a prediction of the trace gases in the model itself, the physical climate model.

This modeler may be perfectly reasonable in what he is saying -- who are we to judge, we who aren't climate modelers?! And one can read the above interview segment as reflective of a certain humility: Although he argues that biology isn't terribly important to relatively short term climate simulations with increased greenhouse gas forcing -- a point I suspect many biologists would dispute -- he can also be seen simply to recognize the limited stage of current climate simulations. As such, he can be seen simply to argue that they had better first only include processes they consider the most important and which they know the best, before entering processes that aren't well understood and which would include great amounts of uncertainty into the model.

But this passage also suggests how modelers can tend to discount the importance of processes which they don't understand well or which they cannot easily quantify, making a virtue out of a necessity. What isn't easily estimated and quantified is sometimes dismissed as "unnecessary complications." As Stan Ruttenberg, a now retired scientist put it:

There is a quantification bias in [the atmospheric sciences] -- and scientists' opinion about what constitutes necessary and useful data changes through time. When there was no way to carry out measurements in the stratosphere, scientists would say that the stratosphere wasn't important. By contrast, the stratosphere is now a "hot topic," particularly the interaction between the stratosphere and the troposphere. Scientists tend toward areas of science which are solvable.

I have repeatedly encountered examples and suggestions of a tendency among modelers to dismiss as unimportant what is not included in their models (of course, they argue that things are excluded *because* they are unimportant!). For

example, in one of my first conversations with a climate modeler at NCAR (The National Center for Atmospheric Research), an atmospheric modeler described the oceans as unimportant compared to the atmosphere in terms of climate dynamics -- a puzzling argument given that the oceans form seventy-one percent of the earth's surface and function as the primary sink for carbon dioxide, and one which contrasts the recognition of the oceans now, only a few years later, as the "flywheel" in the climate change equation.

Another, important aspect which models leave out because it cannot be quantified and is inherently unknowable is the possibility of "surprises," that is, unforeseen sensitivities of the climate and the possibility of a "runaway climate:"

LAHSEN: What about what some people say, that because scientists tend to focus on the most likely scenarios, they don't consider enough the possibility of catastrophic changes?

MODELER: That is correct. But see, the problem scientists have is that it is not possible to quantify the probability of this surprise. And since it is an extreme event -- of its own nature, extreme events don't happen very often, or else they wouldn't be extreme events [smiles]. So, we have been having a hard enough time dealing with means and one standard deviation higher than that mean...! [laugh.] Extreme events -- it is an interesting issue, and scientists should think about it, and there are certainly some extreme events that you want to seriously consider. But in terms of forecasting or actually making accurate climate predictions, I think that that is a *long* way off, if ever!

LAHSEN: Do you feel that surprises are being considered enough?

MODELER: That's a hard question, 'cause I don't know what I don't know.

Scientists generally, of course including modelers themselves, tend to believe that the models do roughly represent the real climate system. Others,

however, including the following interviewee (a theoretician now in climate research who started out as a nuclear physicist and who himself has done some GCM modeling in the past) do not rule out that the possibility the models may not even “get the sign right:”

LEITH: Probably, in general, modelers have got the sign right, you know, off in the right direction. There could be a factor of two or three uncertainty in magnitude to the effect that probably the effect is in the right direction. That’s just based on intuition and feeling about the model and not caring particularly about it, one way or another.

This scientist expresses his belief that the models ‘probably get the sign right,’ leaving the chance of that not being the case. By ‘getting the sign right,’ what is meant is knowing whether the forcing due to greenhouse gases leads to warming *or to cooling*; and this scientist, who himself has done GCM modeling and been in the field for decades, thus considers it possible that models are not correct in showing that the greenhouse forcing leads to warming. By contrast, a modeler at the lunch table expressed his certainty of this result with the statement that “if you heat a kettle, it doesn’t get cold.”

The crucial and deciding factor in whether the models “get the sign right” is the role of feedbacks in the system. Feedbacks are the reactions generated by the climate system in response to forcing (natural or anthropogenic), reactions which in turn affect the nature of the change. A feedback is called “positive” if it intensifies the original change, and “negative” if it counteracts it. The climate models are linear in nature and no one disputes that it is unclear whether the models accurately represent natural feedbacks in the climate system, the most important of which is clouds. The effect of clouds is not well-known and thus not

easily parameterized. It is not known whether the net effect of feedbacks (e.g. clouds or changes in snow cover, which changes the amount of heat absorbed by the earth) are, overall, positive or negative, that is, whether the feedbacks in the earth system, individually or in total, work to intensify warming when atmospheric levels of greenhouse gases are increased, or whether they reduce or even cancel out the effect of the increased forcing.

At present, the latter is not an argument frequently advanced; the most frequently expressed view is that greenhouse gases warm the atmosphere. Any argument against the dominant paradigm of climate warming requires a unifying, explanatory theoretical framework, a requirement which is not easily met.

Empiricists (synopticians, observationalists and some experimentalists) who present bits and pieces of evidence against the theory of human-induced climate change unaccompanied by any overarching theoretical explanation are not seen to present a strong case against the dominant paradigm.<sup>48</sup> As one empiricist (an observationalist and experimentalist) said:

There's a general difference in approach between theoreticians and observationalists. And there's a tendency to say that you are not doing things properly unless you can construct a complete mathematical theory of every event that you have observed.

### ***Models as bad science?***

All atmospheric scientists -- modelers, theoreticians, observationalists and experimentalists alike -- recognize that the GCM simulations are faulty, a science



still in a stage of development. But they also recognize the models as useful, even necessary, tools by which to further understanding of the climate system, with or without forcing due to human activities. This recognition of the models as important and powerful tools tempers many criticisms of the models.

Approximation to the real climate system is generally the measure of how good a model is, but how well a model simulates the real world does not make it “good science,” and a model that simulates the world poorly is not necessarily “bad science.”

Whether a model is considered “good science” or not depends most centrally on *how it is used* and what purposes it serves -- besides, of course, on who is doing the judging. To many scientists, modeling is “good science” if it contributes to the advancement of scientific knowledge of how the real earth system works, or of how that particular model works (which, in turn, renders it more likely to later contribute to the advancement of knowledge). By contrast, modeling is described as “bad science” when the relationship between the model and the real world is not sufficiently questioned by those presenting and using the models. This lack of questioning happens when modelers deemphasize or fail to acknowledge the limitations and uncertainties of their models, convinced by their models and/or intent on ‘selling’ (i.e., gaining recognition and support for) their science inside and outside the scientific community.

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<sup>48</sup> In more extreme cases, contrarians who exclusively “poke holes” in the theory of human-induced climate change, without providing a larger theoretical framework for their arguments, are not only ignored by mainstream scientists but even derided by some for doing so, their contribution called “unscientific”.

Non-modelers (albeit, perhaps, with a certain unease) grant a model the status of “good science” even if it is far from approximating the real climate system, as long as it is used to develop understanding of the real climate system or of the limitations of the model itself, and as long as the model output is not presented as -- or doesn't come to be mistaken for -- accurate and reliable predictions. This perspective is reflected in, among other things, objections to presentation of model output as “data,” something that is seen as propping simulated output up to an undeserved status of accuracy and realism.

In interviews, on-modelers and modelers alike have described the effort of trying to model climate without better data by comparing it to “an infant trying to walk.” In the same context, the expectation that climate models produce answers about anthropogenic climate change, when observations are still so incomplete, has been described to me as “trying to drive a car without the wheels” or “trying to build the roof of a house before the walls are up.”

***“This does not look like the atmosphere I know”***

Empirical meteorologists in particular tend to be acutely aware of the limits to models' ability to reproduce the real climate. As close analysts of the actual atmosphere, this type of meteorologist knows that the models fail to reproduce important dynamics.

The following segments of an interview with a data analyst suggests the tension between models and observations, and, by extension, between

modelers, on the one hand, and empirical meteorologists, on the other.

When reading this interview transcript two years later (Spring 1998), the same scientist felt that this criticism was somewhat dated. At least in his institution, he feels that modelers' attitude has changed noticeably; modelers he works with now show more awareness of "what the models can and can't do." Moreover, observationalists have become a more central part of newer modeling efforts than just a year or two earlier, and more resources have been devoted to observational studies, such that the divide between modelers and empirical meteorologists, and between the models and the observations, has narrowed considerably in more recent modeling efforts. Nevertheless, this same scientist also said (Spring 1998) that modelers in other situations and other institutions still retain at least some of the attitude described below, hence I include it here, as long as the above qualifications are made clear:

LAHSEN: I'm interested in what you think of climate modeling as a scientific endeavor. So to start there and then going out, how they get represented, what kind of funding they get compared to other efforts that go into it. That whole, broad picture -- and examples are great.

EMPIRICAL METEOROLOGIST: I guess, a general opinion of that is that I think it's always important to emphasize that a model is simply a tool we are using that can be viewed as a means to an end and the end is a better understanding of the climate system. Whereas, sometimes, when you just talk to the modeling community you get almost the opposite opinion. The science is to build models and that is the end, whereas in reality models are just going to be tools, a stepping stone toward helping us better resolve some of these problems.

LAHSEN: How is it that the modelers sometimes "end" with the model?

EMPIRICAL METEOROLOGIST: Well, I'm just saying that what modelers do is that they develop and build the model, and if I had to level criticism --

and this is not intended toward any specific person here but just in general -- it is this. Building a model is a full-time job and it takes a lot of expertise just to do that, and I think that sometimes it's easy to lose track of what the model is being built for. It's not enough simply to build a sophisticated model that includes a lot of physics, physical parameterization etc., and to integrate the model in time and to take just sort of a general view of it, saying, 'yeah it looks like it's simulating something that appears to be like the real atmosphere.' Rather I think, that it's just as important to develop a model but, at the same time, to fully realize the limitations of the model and to always take a very critical look at the capabilities of the model and what kind of questions one may be able to answer and pursue using the model. The climate system is so complex, with all of the feedback loops, that it's very difficult to actually model all of those loops. So there's bound to uncertainties, sometimes big uncertainties, in the answer.

A perfect example of that are the climate change experiments being done, the ones looking at increased levels of CO<sub>2</sub>. And now people are putting in the aerosol forcing and integrating the models out in time, and they get some particular surface temperature pattern. If you're focusing on surface temperature, that looks somewhat like the observed temperature anomalies for the past ten or fifteen years and I've seen a lot of discussion in both the popular and scientific literature lately saying, "Gee, it looks like we're getting closer to solving the problem, then the models must be getting things down pat, because this is looking more like what we see in nature, and therefore what we're seeing in nature could very well be in response to human influence on climate." And while I wouldn't take as strong of a position to say that I really disagree with that, I would take a much more cautious approach.

To show how the above approach can be problematic, this scientist provided an example from his own research. He described how certain changes in the atmospheric phenomena he had studied turned out to be "the exact kind of response that the climate models predict with aerosol forcing plus CO<sub>2</sub>," which led some to conclude that the changes in circulation were human-induced. However, he cautioned against quick conclusions of this kind, noting that the changes could be natural.

LAHSEN: So the models show the same but that's based on increasing levels of CO<sub>2</sub>?

EMPIRICAL METEOROLOGIST: Yes. So if you could show me in a model that the changes that we've seen over the last few decades or so are uniquely due to that CO<sub>2</sub> and aerosol forcing, I would say that indeed we are seeing the human influence on climate. But right now, the question becomes more one of whether these variations are due to the CO<sub>2</sub> and I haven't seen any evidence of that. Like I said, these things have occurred for thousands of years before. So it's a very complicated problem and I guess the point that I'm trying to state is that, perhaps rightly so, there's been a lot of excitement that this simulated temperature field does look like what we've seen in the last fifteen years. On the other hand, this variability could be a natural time-scale variation in climate. So it's just not a simple process, running out the model and coming to conclusions based on that output. One of the things that need to be done with the models is not only [to put into the model] CO<sub>2</sub> plus sulfate aerosol, but [to ask]: is the model able to capture these variations that we observe in the circulation? Can it capture those kinds of modes of variability? Right now it's very exciting, it's very intriguing. I'm not saying that what we've seen isn't a human influence on climate, but I think it's still very much up in the air. And that's a specific example of what I think modelers have to be aware of.

Two years later, this same scientist says that there now is some evidence, "suggestive but not conclusive," that the changes are due to human emissions of greenhouse gases. When reading the two year old transcript in the Spring of 1998, he commented that he presently thinks that there will be a human influence and that it "will be evident in the not so near future."

The acceptance of the modeled projections by empiricists is not helped by cases in which modelers are perceived to hold a cavalier attitude toward empirical information. A passage in the British TV documentary *The Greenhouse Conspiracy*, staunchly critical of the theory of human-induced climate change, shows climate modelers Stephen Schneider and Tom Wigley foreseeing

increases in the frequency and intensity of storms as a result of climate change, saying that there are “strong reasons to believe the consequences could be severe.” They are then ridiculed by cuts to interview segments where they both say that they ‘don’t give much attention to data.’ This follows after an earnest, puzzled skeptic and empirical meteorologist, Reginald Newell, is aired saying: “I don’t know why models are taken seriously.”

As mentioned above, the scientific method involves checking predictions of a scientific model against further observations or measurements and adjusting or replacing models “as required by the new observations or measurements.” But this process is not always followed, at least not as quickly as critical empiricists would like.

For example, the following incident is still remembered and recounted by a group of empiricists: Some years ago, a group of three empirical meteorologists, two of them Senior Scientists (read: of considerable status at NCAR) wrote a letter to the group of scientists most centrally involved in the development of NCAR’s Community Climate Model, in which they proposed to do a point-by-point analysis of the model by comparing it to observations. They never received a response back. Suggestive of its importance to them, one of the three meteorologists still has a copy of the unanswered letter hanging on the wall in his office, and another has told me of the incident numerous times. They perceived it as a sign of arrogance and of a general disregard for input from observationalists on the part of the modelers .

When I subsequently interviewed one of the modelers of the group to which the letter was sent, I received a different perspective, a perspective suggesting the general challenge involved in modeling. The following segment from that interview suggests the challenge involved in attempts to integrate available observations and theory, a challenge in part rooted in the difficulty of cross-disciplinary communication and collaboration, and in the complexity of doing global modeling. In addition, this incident suggests the role of pride, frustration, and personality conflicts among scientists in the development of science:

LAHSEN: So, getting back to what we were talking about before -- that observationalists criticize modelers for not including enough observations, for not listening enough to that.

BOVILLE: Yeah. But you can't *include* observations. I mean, that is an analysis project. You have to try to *learn* from the observations, and you need observations to do good theoretical work, and you need observations and theory to do good modeling.

LAHSEN: How do you then include observations--you get data sets, right?

BOVILLE: Yes.

LAHSEN: What, then, about the infamous memo that [an NCAR scientist] has hanging on his wall, the memo in which they offered to go through the CCM to check it against the observations. And they never got a response from you guys. Are you familiar with that?

BOVILLE: [thinks] No. [Thinks some more] No.

LAHSEN: [laughs] Oh well. And now you are doing that more, with the CSM, but there was the criticism that you didn't include observations well enough [in the earlier CCM model].

BOVILLE: There will always be a tension there. Look at it this way: I spent ten years building a model and then somebody will come in and say 'well,

that's wrong and that's wrong and that's wrong.' Well, fine! And then they say, 'well, fix it!' [and my response to them is:] '*you* fix it! [laughs] I mean, if I knew how to fix it, I would have done it right in the first place!!! [Laughs]. And what is more, I don't like you anymore--all you do is you come in and tell me what is wrong with my model! Go away!' [laughter]. I mean, this is the field.

LAHSEN: [laughing] That's how it works, huh!? [Pause] So maybe if people came with the criticism more delicately or something..?

BOVILLE: Well, yes, it is partly that, but no, it is an expectation problem. Like, all right, but what do you want me to do about it? I mean, yes, I have some ideas and I will be working on the model, and that may fix some of these things, but unless *you* have a good idea about what it is that is wrong with my model -- what it is that causes it to not treat a process correctly, resulting in that effect -- then, all you really give me is more information that is grist for the mill, and -- you shouldn't expect any direct response, because I *have no* direct response to make.

LAHSEN: So part of the problem is that the observationalists, they don't know the models, so they can't go into it and say 'this is where you should change your parameterization'? They just come and say 'well, this is how it really is out there'--and you don't know how to translate that into the model?

BOVILLE: That's correct.

LAHSEN: So it is a difference in practice and a difficulty of communicating across disciplines?

BOVILLE: Yes, it is all of those things. And they tend to become even a personality problem simply because of the kinds of things where -- it's fine; you go and work with somebody for a while and they find some problems, and that is interesting. But how many times does it remain interesting to deal with the same people when they find problems!

LAHSEN: When they criticize all the time?

BOVILLE: It is not a comfortable relationship. So there is always going to be a tension from that point of view. There is no way out of it.

Empirical meteorologists' skepticism has not been lessened by past models' projections of huge temperature changes with a doubling of atmospheric



concentrations of greenhouse gases based on preindustrial levels. Until a few years ago, all climate simulations tended to show a level of climate sensitivity to increased forcing by greenhouse gases which was demonstrably too high. According to the calculations of some of these models in the past, global climate warming resulting to a doubling of CO<sub>2</sub> would be as high as nine degrees Celsius. These calculations were cut in half when modelers developed a technique by which to include the effect of aerosols in the models. Sulfate aerosols are small particles that derive, like carbon dioxide, from fossil-fuel combustion plus some natural processes. Their effect, however, is opposite to that of CO<sub>2</sub>, as they reflect sunlight and increase cloud-reflectivity which has a cooling effect on climate. Including the cooling effect of aerosols in their model, modelers obtained a closer resemblance between their simulation of past and present climate and observational data. As suggested in the comments by the empirical meteorologist quoted above, modelers tend to portray this improvement as proof of the increased realism of the models, some saying that the models now “get it right,” claims which are met with caution and some skepticism by many scientists, both theoreticians and empiricists (and, to repeat, many are some of both).

Past “wild” exaggerations of models are not forgotten by such scientists, as suggested in the two following interview excerpts with two empiricists:

GRAY: Here are some of those early model runs [looks at a document]. This is unbelievable! They're forecasting that the upper atmosphere will warm by seven degrees. Seven Degrees! I've never heard of temperature changes as high as that. Here is the GFDL model, they are getting warming of four degrees with a doubling of CO<sub>2</sub>, done in the late 1980s.

This is, I don't know, this, we've never had... These kinds of temperatures are very large.

Elsewhere in the interview, this same meteorologist drew a distinction between meteorologists who have studied weather for many years and modelers, whose depth of knowledge of the actual weather and climate he questioned:

GRAY: I have been working forty years in the field. People like Mel Shapiro, Harry van Loon, Hugh Ellsaesser -- we have been spending our careers in the field and there are a lot of others of us around. We have spent years in the field, we have looked at weather maps, we have been forecasters, we have done research with observations, we have thought a lot about this, and most of the people I know who have been in the field a long time and who really know the atmosphere -- and I have great respect for Harry van Loon, he knows what he is talking about. Mel Shapiro knows what he is talking about. Some of the modelers don't. They have never made a forecast in their lives, they have never drawn a weather map, they can't read a weather map. Many of these global modelers -- now, I don't mean to denigrate them; global modeling is very difficult. They are very careful mathematicians, they are very good. But they are so involved with running their models that they haven't put the time in thinking how the atmosphere works.

Other climate scientists object to the claim implicit in Gray's argument here that climate modelers need to know how to make a weather map, or even that they necessarily need to be very familiar with the details of weather dynamics, though of course it might be of help. Gray's comments point to a tendency among some weather scientists to suspect the depth and quality of modelers' knowledge with regards to the actual atmospheric system, as opposed to the simulated one, a criticism which Gray later extended to include more generally scientific institutions centrally involved in propelling concern about human-induced climate change:

What I resent most is that they [the IPCC] say there is a consensus of scientists. There is not. Not at all. And my point of view is that [IPCC Chairman of the scientific Working Group I] John Houghton is a great stratospheric radiation expert -- if you look at the people sitting on the panels making these decisions, not so many of them are meteorologists that have ever drawn a weather map or done a forecast and have been down in the trenches doing the day to day weather. They don't have a good feel for it. They listen to each other and agree and argue about this or that and they all go around in a circle. And here is reality way out here. And they are all agreeing, 'yes we have hashed out all our problems', 'we all agree on this.' And here is reality, out here [humorously stretches his arm further and further away, then pauses]. You have come to talk to a great critic.

Again pointing to a tension between data and models, another empiricist as well commented that the models are "way too sensitive, they are miles away" and that part of the problem is that "modelers don't believe in observations." He added that he was not saying that he doesn't believe in models, he just doesn't think that they have got them right yet. Comparing modeled versus observed results, he pointed to a difference of between 4-16 degrees in the case of past modeling results, commenting more than asking: "So why should we believe the models for doubling of CO<sub>2</sub>?" He went on:

What I am saying is that there is a hole in these models somewhere. Something's wrong with the physical mechanisms. What matters, I think, is the fact that we need to do right now what we should have done twenty years ago, which is to really thoroughly understand the physical mechanisms that are behind the models. You can write down the equations of motions and put them in the machine and grind them through, and you'll get something close to reality. Then it won't work, like this cooling, and we don't know why it is, now maybe it is that one of the mechanisms aren't right. We can't guarantee that we understand all the physical mechanisms. So you've got to put a magnifying glass to it, fly around [in airplanes]. We found a lot out about typhoons by just flying around and measuring the chemistry of typhoons. There was a lot physics that people just didn't seem to have in the books.

### ***Tuning and tweaking***

Some criticism of climate modelers is rooted in the earlier described hierarchy between scientists and technicians according to which scientists tend to perceive engineering and technical practices as inferior (or at least less “interesting”) to science, the latter associated with more creativity and intellectual challenge. Empirical meteorologists of Gray’s generation share important aspects of their attitude towards modeling with certain factions of physicists and theoreticians both inside and outside the atmospheric sciences. Much of this attitude is summed up by the following quote by a physicist in the work of Sherry Turkle on the effects of simulation practices on culture and cognition:

If you are really gifted at solving problems in mathematical physics, you might have as a corollary that somebody who has to resort to a computer to solve problems is not as clever as somebody who can solve them with mathematical techniques (Turkle 1995:65).

Climate modelers object to the label of “technicians,” but their strong reliance on computer models renders them vulnerable to it, especially when they engage in a particular type of modeling. The label of “technician” is particularly directed at those modelers who don’t develop their own models but who simply obtain permission to use a model built by others, and who subsequently perform “model experiments” by changing an equation or two and then “turning the knob” and letting the model print out a result (“output”). More precisely, this practice is widely and strongly criticized when the modeler running the experiment is perceived to not understand the output -- a strong possibility if he or she hasn’t

developed, or closely studied, how the model they are using works, how it processes the input. Such superficial use of models, pejoratively referred to as “button pushing” or “knob turning,” not is considered “good science” because it fails to contribute new knowledge insofar as the modeler doesn’t understand the output himself. It is also resented because it leads to “boring” (uninsightful) presentations at conferences. I don’t know how often this actually happens, but the practice is frequently mentioned and criticized by (non-GCM modeling) mainstream climate scientists, perhaps because it epitomizes “bad science” to them, which is how it is described.

All models are tuned or tweaked to some extent. Tuning and tweaking are adjustments done to the model when they are obviously unrealistic and/or when their output diverges too drastically from that of most of the other competing models, and when certain climate-related phenomena aren’t sufficiently understood. As one modeler put it in a talk at NCAR: “Successful tuning is parameterizing something we know nothing about.”

Because of the role of tuning and tweaking, and of the difficulty of validating models, Naomi Oreskes et al. have noted certain similarities between a work of fiction and a model: Both may resonate with our observations, without being “the real thing:”

[J]ust as we may wonder how much the characters in a novel are drawn from real life and how much is artifice, we might ask the same of a model; How much is based on observation and measurement of accessible phenomena, how much is based on informed judgment, and how much is convenience?” (Oreskes, Shrader Frechette and Belitz 1994)

The authors point out that models may confirm biases and support incorrect intuitions, wherefore they are most useful when used to challenge existing formulations rather than to validate or verify them.

A particular type of tuning or tweaking is a technique called “flux adjustment” or “flux correction,” which is used in all large coupled models at all the major modeling centers, with the exception of NCAR. Flux correction has generally been considered a necessary adjustment of models to render their output more in conformity with reality. In the coupled models (e.g., ocean-atmosphere models), the ocean might tend to “drift” away (a sign of their linear rather than more circular structure) to a value which is observably unrealistic. The model might, for example, show the ocean to turn into solid ice in large regions. To avoid this, modelers change some parameterizations. Though fully aware of the superficiality of these adjustments which fail to identify and address the actual errors in the model that are causing its lack of realism, most modelers have considered this practice necessary to obtain more realistic models. I asked an NCAR modeler about the practice of flux correction:

LAHSEN: it is basically giving the model a big wham, right, without knowing why it needed the correction, right?

BOVILLE: Right, they don’t know it. And the numbers are *big*! [Pause] This is something people tend not to realize. If you look at fresh water flux adjustment, which, for example, the [British] Hadley Center model has. The adjustments they have put into the fresh water fluxes, around the ice models, are as large as precipitation is in the [ITGZ?]. The precipitation numbers in that area are small. And this is totally fictitious. It is not based on *anything*. Feedbacks of the ice [lines?] are one of the big determinants of response to changes in [greenhouse gas levels]. How can you believe anything, when this is all being tuned with flux adjustment?

***Potential consequence of tuning and tweaking: model insensitivity***

The following example suggests how the use of flux correction can render models insensitive to forcing compared to the real climate system. Such examples are worth noting, given critics' portrayal of models as overly sensitive compared to the real climate system (cf. the above comments by observational meteorologists). While past models in some regards have been too sensitive, there are also examples where other past models inadvertently have been set up so as to become resistant to forcing. As an astrophysicist related to me:

FIROR: There is a well-known case of a climate modeler who produced a model that showed no climate change. And when other people looked into it, they found that he didn't have enough computer time to do a really sophisticated study, so he had just fixed ocean temperatures at its climatological value, and then put in CO<sub>2</sub>. Well, since most of the earth is ocean, that fixed temperature ties down the global temperature so that it couldn't change. So, that one assumption determined his result. He didn't have to do the calculation, if he had stopped to think; he had fixed his model up. But this was saying that an assumption made for fairly small reasons -- like, 'I don't have a big enough computer resources,' or 'I want to do it on a work station' -- can be very important. So it is worth looking into.<sup>49</sup>

Another scientist gave another example of how models can be tuned such as to become insensitive to forcing, then said:

LEITH: That could serve as kind of a warning to people when they are building their models: Be sure you don't build in the answer that you wanted, which, of course, is very easy to do because you tend to want, first of all, to simulate the observed atmosphere. What you should be careful NOT to do, however, is to simulate the observed atmosphere so well that you can't change it. You build in the answers of the present atmosphere in such a way that it won't respond to imposed external changes that you want to subject it to.

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<sup>49</sup> From interview with John Firor, 8/28/96.

LAHSEN: What is a way of doing that?

LEITH: There are ways in the adjustments of the physical processes, many of which have very fuzzy lines. Convection prescriptions, for example. There are a lot of constants floating around in there that people adjust to get things looking right. You have to be careful that you are not, at the same time ... You're getting the present climate, but there is a risk, at least, that you are also interfering with the sensitivity of the model climate to changes from external influences, which is, after all, what we're usually interested in.

***A structural shift towards computerized models  
and the generational aspect***

While climate modelers come in all ages, as a whole they tend to represent a younger generation which has grown much more deeply and directly dependent on technology in their scientific work compared to earlier generations of meteorologists. Some among the older generation of scientists distinguish themselves from the younger generation for this reason, seeing the latter as more distanced from the "real," in favor of simulated worlds, and less able to develop and operate by the aid of conceptual models without the mediation of technology in the form of computers. The generational dimension of modeling and its critics is also reflected in the following assertion by empirical meteorologist and contrarian, Hugh Ellsaesser:

The optimum attributes for developing [the two most complex and reliable types of climate models] are burning ambition and an uncluttered mind -- which helps explain why most such models have been developed by graduate students. I do not believe that most of us would agree that such people are in general the ones who best understand how the atmosphere as a whole works (Ellsaesser 1989:71)



And in a quote above, William Gray contrasted old-time meteorologists, trained in synoptic forecasting, who have worked in the field during or immediately after W.W.II, with modelers who, by contrast, 'don't really know the atmosphere.'

Due to the general tendency towards increasing specialization in science, those who model find it difficult also to develop their skills as empiricists and theorists, with the result that those who now engage in modeling come to rely very centrally on models and on sophisticated statistical runs based on model data. Older scientists are aware of this shift, and criticize or lament the extent to which computer modeling now fails to integrate conceptual models, that is, deeper knowledge of the system being modeled.

LAHSEN: When I talk with younger scientists, they talk about the difference they feel between their generation and some scientists of the now-retiring-age generation of scientists, and I hear of differences in their practice because of the way computers have become so central. What is it that's so different about that practice? What is so different about the scientific practice today?

LEITH: [Mentions by name a particular scientist of the older generation of scientists who] has been worrying about [the climate system] for a good many years and he tries to get a kind of conceptual model of what's going on, as far as the climate system is concerned. It is interesting that there are others of the older school who have been trying to do this before the modelers came along and said, "It's hopeless to do it that way. We're just going to have to simulate rather than understand." But as [Ed] Lorenz pointed out, years ago, even if you ever built the perfect atmospheric model and it was generating very realistic climate, that, by itself, would not give you any greater understanding. After all, you've been watching the atmosphere all this time and you don't really understand it very well, either. The model, by definition, is just as complicated as the atmosphere, so it's not adding the new level of understanding -- whatever that means -- that you would like. The modelers will say, "Well, for practical purposes, why do we have to understand it? Because with the

model we can do experiments and we can see the consequences of artificially imposed changes, so we can answer a lot of practical questions about what the nature of the climate system is. Why do we have to understand it?" The answer, of course, is two-fold. One is, people like to understand things. The other is that, if you understand it, if you have some kind of a simpler notion about what is important, you can check whether the model is doing things correctly or not. Or whether there has been some flaw.

Without conceptual models, climate scientists and others may be seduced into believing that their models are correct, especially if they appear to replicate the observed system in ways that they consider acceptable, that is, in ways that are not obviously and strongly deviant from what is observed or expected of the actual climate system. Though the simulations may resemble the real climate system for the wrong reasons, modelers typically establish the "realism" of their models and their projections through means of visual comparison with the observed climate system. Ed Lorenz' point mentioned by Leith is very important: if you get a "black box" model to reproduce the climate data, then in a sense you are right back where you started: you have a bunch of data which you don't understand.

***From a model of reality to the reality of the model:  
the seduction and contestation of models***

There is little disagreement among scientists that modeling is a useful practice, that simulation models are helpful heuristic tools in identifying what isn't well known about the components of the earth system and their interaction, which in turn helps identify what areas need more research. In the words of one modeler, "to the extent that our models don't reproduce the real world, we don't

understand the real world and how it works.” As simplified representations of the real world, models are conceived of as approximations of the real world, and are not, at a conscious level or without caveats, conceptualized or presented as accurate representations of the real climate system. In the words of one climatologist, “[a]ll models are of course wrong because, by design, they depict a simplified view of the system being modelled” (Trenberth 1997). As “approximations of the real world,” models are recognized to fall short of simulating the real world to perfection, but this is also a merit, as it is precisely in their simplification of the real complex system that models can be useful tools through which to investigate how the real earth system works.

Nevertheless, there is a significant amount of criticism of models among scientists, contrarians and non-modeling mainstream scientists alike, a criticism which works to limit acceptance of GCM-based projections of human-induced climate change. In addition to their ambiguous relationship to traditional methods in science, and more particularly in their ambiguous relationship to data and “reality,” an important reason for resistance to climate models is the tendency for modelers to present their work in ways that suggest an unwarranted level of realism and predictive capability of the models. In what follows, I will identify two ways in which models come to be associated with an unwarranted level of realism and predictive capability: (1) when modelers themselves fail to step back from their simulated worlds, and (2) in the process of dissemination, when GCM-based climate projections are interpreted by non-specialists, particularly by science bureaucrats and politicians.

***The seduction of simulations:  
over-selling and the confusion of models and reality***

Climate modelers as a group are among the most outspoken supporters of the theory of human-induced climate change. James Hansen, who started the current wave of concern about human-induced climate change with his statements before Congress during the summer of 1988, is a climate modeler, as are Stephen Schneider and Thomas Wigley, and all have been on the forefront among scientists in publicly expressing their confidence in the theory of human-induced climate change. Even modelers without such public high-profile - - modelers who don't profess great concern about the theory of human-induced climate change and who don't express as great faith in the precision of GCM-based projections -- tend to believe that their models present a good picture of the likely consequences of increased levels of atmospheric greenhouse gases. The prominence of modelers among the strongest advocates of concern about human-induced climate change might be linked to the tendency of modelers to be "seduced" by their own models.

Recent philosophers have theorized about the role and seductiveness of simulations in late twentieth century. Jean Baudrillard describes how the logic of presentation that was central to dominant thinking in Western societies prior to the advent of television and computers took for granted a distinction between form and content and between the tangible and the ideal. The assumption was that an image reflects a reality, even if it distorts this reality. Baudrillard identifies simulation with the current stage of "Western" thinking about images and

realities, arguing that the relationship between meanings and symbols has been severely undermined due to television and computers. In the age of simulation, according to Baudrillard, an image need not have any metaphysical relation to a reality because simulations have the power to separate themselves from, or even liquidate, what they are suggested to represent. Images are rooted in other images which themselves refer to yet other images. There's no relationship between systems of meaning and systems of simulation: there are only images. Baudrillard's term for symbols without reference to meanings and realities is "simulacra," and "hyperreality" is the term he uses to denote the status of simulacra in late twentieth century industrialized societies (Baudrillard 1983).

The following analysis of climate models' relation to data suggests that in some respects models do constitute "simulacra," or, at least, involve a questionable relationship to reality. The analysis, supplemented by segments of interviews with modelers, suggests that the process of modeling engenders a hyperreality of a sort. The practice of simulation involves a constant temptation to mistake the climate simulations for reality -- a temptation which leads to further criticism of the modeling enterprise by non-modeling scientists.

Aside from the fact that modelers as a group enjoy considerable and increasing recognition and funding relative to other groups of climate scientists, perhaps the most frequent criticism of modelers I have encountered among mainstream climate scientists is that modelers mistake their models for the real climate system. The more cautious critics will specify that only *some* modelers are guilty of this, not all. Modelers *are* aware of the uncertainties and

indeterminacies of their models, and may even joke about their frustrations when their models disagree with observations. Fitting model results with observations can be difficult. And modelers “tweak” or “tune” their models to fit observations to some degree. As an example: sitting around the lunch table one day, a group of scientists who work with simulations explained the role of “tweaking” to me and jokingly expressed the desire and suggestions for the design of a button modelers could use to blow up a satellite when it didn’t support their model output. They then jokingly discussed a second best option of inserting their model output straight into the satellite data output.

Nevertheless, modelers can tend to underplay or underestimate uncertainties and indeterminacies of their models, due to funding needs, perhaps, but also due to ego (wanting to have a good and competitive model) and a tendency for modelers to be seduced by their own creations. One modeler explained as follows why modelers can fall to the temptation to portray their models as more accurate than they really are, and the effect of this on the perception of modelers among empiricists as well as theoretical physicists:

BOVILLE: There are many ways to use models, and some of them I don’t approve of. [Pauses] It is easy to get a bad name as a modeler from both theoreticians and observational people, by running experiments and seeing something in the model and publishing the result. And pretending to believe what your model gives -- or, even, really believing it! [small laugh] -- is the first major mistake. I mean -- if you don’t keep the attitude that it’s just a model, and that it’s *not* reality... I mean, mostly people that are involved in this field really have that, they have the overtone that it is.

LAHSEN: They do tend to think that their model is the reality?

BOVILLE: Or even if they don't think that, they tend to oversell it, regardless.

LAHSEN: And why do they oversell it?

BOVILLE: Because people get wrapped up in what they have done. You know, I spent *years* building this model and then I ran these experiments, and the tendency is to think: 'there must be something here..' A much healthier attitude is... And then they start showing you all the wonderful things they have done. I mean, we are doing that now. And you have to be very careful about that. The way I have operated mostly in the middle atmosphere work, which leads to a fair amount of respect, I think, which is, you say, 'I ran this thing, and look at all the things I got wrong! This is wrong and this is wrong and this is wrong. And what is it that this tells me?' and I think people have a lot more respect for your work, and tend to listen to you better, if they get the idea that you are really interested in *why* things work the way they do, and not--and have a healthy skepticism about your own results.

As suggested in this above interview segment, the practice of modeling can engender a blurring in modelers' minds of reality and the simulated, a process which parallels the blending of observed reality and simulacra in the construction of data described above. Distinguishing between simulation and "reality" is, of course, inherently awkward since reality never can be separated from subjectivity: perceptions of reality are inevitably colored by subjective world views and particularity of context and interpretation. As should be clear from previous discussions, my point is not to suggest that there is an objective reality which we can in fact grasp directly, without such mediation. In the section about data my point was to show the technical and human mediations involved in the construction of data. In this section, my point is to suggest the effect of the scientific practice of simulation on modelers' perception of the actual climate system.

The following exchange with a modeler, Phil Duffy of Lawrence

Livermore National Laboratory, suggests how the simulated world can start to be taken for reality in modelers' minds:

LAHSEN: Do you think that modelers come to think of their models as the real?

DUFFY: [with no hesitation at all:] Yes! Yes. You have to constantly be careful about that. [Laughs]

LAHSEN: Can you tell me about that?

DUFFY: Well, just in the words that you use, you start referring to your simulated ocean as 'the ocean'. You know, 'the ocean gets warm,' 'the ocean gets salty.' And you don't really mean the ocean, you mean your modeled ocean. Yeah! [Pause] I mean, nobody *really* thinks that; if you step away from your model you realize, this is just my model. But there is a tendency to forget that just because your model says x, y, or z, doesn't mean that that's going to happen in the real world. You know; your model could be wrong. And it's especially--the other thing I really notice is, people who do models, and these are big models, and they -- computer programs that represent the models are very big and very complicated. They take a long time to run, and they generate tremendous amounts of output--so much output that it is actually really a problem of where do you put it, how do you look at it, you know, how do you ingest so much information. So modelers end up--I mean, I notice this myself-- modelers end up spending as much time studying the models as they do studying nature. Because--the model certainly isn't as complex as nature, but it is big and complex, and it presents its own problems. And that's not necessarily--it's not bad, because it's good for learning. When I say studying the models, I mean what we're doing is we're learning how to make better models, how to improve the models, you know, comparing the models with observations, that kind of thing. And that's -- people who study climate, the people I work with, although our ultimate goal is to predict future climate, we spend ninety percent of our time, at least, trying to get our models to predict -- basically testing our models, getting our models to predict today's climate; you know, can our model predict today's climate, can our model predict ice ages, you know. We spend 90% of our time testing our models against problems that we know the answer to. So we spend a lot of time studying the model; like, why did the model give this answer, how can we get it to give a different answer.



LAHSEN: You start moving into a more simulated world generally--I mean, you compare your simulation to one of another lab--

DUFFY: This is it, that's right. This whole institution across the street, what they do is they compare model a to model b. They also compare model a and b against reality, but I mean, their whole mission in life is to compare model a against model b against model c.

The same general point was confirmed in an interview with another climate modeler:

LAHSEN: What is the criticism [by non-modelers of modelers]? That you take your model for the real system?

BOVILLE: Yes, that is right. Theoreticians and observationalists tend to come at it from two different points of view. The root complaint [they have of modelers] is that people who spend too much time with their models tend to begin to mistake their models for the real world.

LAHSEN: Right. And you have to detach yourself sometimes, right? I mean, you spend so much time in the simulated world.

BOVILLE: You spend a lot of time working on something, and you are *really* trying to do the best job you can, of simulating what happens in the real world. Yet it is easy to get caught up in -- you start to believe that what happens in your model must be what happens in the real world. And often that is not true. And if you become caught up in it, the danger is that you begin to lose some objectivity on the response of the model, say; if you begin to believe that the model really works like the real world, then you begin to take too seriously how it responds to a change in forcing. Going back to trace gases, CO<sub>2</sub> models--or an ozone change in the stratosphere. If you really believe your model is so wonderful, then the danger is that it is very tempting to believe that the way it responds to change in forcing must be right.

LAHSEN: So that is where people can start to believe their own projections?

BOVILLE: That's right. And, on the CO<sub>2</sub> problem, I certainly believe that increases in CO<sub>2</sub> and other radiative gases will cause the globe to warm up, that the change that you get has a multiplying effect on water, as a positive feedback. I suspect that that has been overestimated in the past, but I don't *really* have any way to estimate that. So while I suspect that it

has been overestimated, that is not based on any sound physical judgment.

***The danger of being seduced by the models***

Elsewhere in the interview, Boville notes that “if you begin to believe that the model really works like the real world, then you begin to take too seriously how it responds to a change in forcing.” Taking the models “too seriously” can have important consequences, given the importance of modelers’ “expert opinions” on the threat of human-induced climate change for policy makers -- and, contrary to the suggestions from contrarians, the bias is not necessarily towards exaggerations of impending climate catastrophes. Consider the example about “surprises” above, the interview segment where a climate modeler concludes from his simulation that the effects of a change in the thermohaline circulation, widely theorized to result from increased forcing by greenhouse gases, isn’t likely to be catastrophic. The modeler slips from inserting the caveat “if the models are correct” to talking about the model output as “reality,” suggestive of the seduction of models:

MODELER: One of the surprises that people have been worrying about is whether the thermohaline circulation of the oceans shuts off [the big pump that could change the Gulf Stream]. [...] If the models are correct, the effect even of something like that is not as catastrophic as what most people think. You have to do something really nasty to [seriously perturb the system [...]] The reality is, it really is an ocean thing, it is basically an ocean phenomenon; it really doesn’t touch land very much.

LAHSEN: But wouldn’t it change the Gulf Stream and therefore...?

MODELER: Yes, look right here [shows me the map]. [Pauses, with a little hesitation now, he adds:] If the model is right. [Slight pause]. I put that caveat in at the beginning. [Laughs]. But right there is the picture.

As mentioned above, GCMs failed to predict ozone depletion, in part due to the inherent difficulty of calculating the incalculable, the unknown.

### ***“Privileged technicians”***

Superficial use of models is also resented because it can appear to outsiders as if such modelers have done a lot of work, when all they may have done is change an equation and ‘turn a knob’ to set the model going. To many empirical meteorologists who do laborious work such as building or analyzing data sets, this can be frustrating, particularly when they then perceive that modelers more easily obtain funding for their “experiments.” Valuations of (some practices of) modeling as inferior science are combined with resentment when models appear to be privileged in terms of funding both by scientists’ own institutions and by grant agencies.

Speaking generally, model experiments are sometimes privileged by policy-makers over research involving detailed observation and data and model analysis which in fact would improve the models. This happens in part because policy-makers may be more easily impressed by model output, especially when they aren’t fully aware of the limitations in terms of the predictive capacity of the models. Model output is attractive to policy makers because of its tangibility: as one modeler put it, “models never say I don’t know; they always give you an answer.” The answer may be wrong, but to the uncritical or unaware, the

precision and detail of the model "answer" can be very convincing, particularly to policy makers who need to decide whether the world, their country or their district is in danger from human-induced climate change and whether they therefore should support the development of protective policy measures.

The processes and considerations underlying the privileging of climate models are described as follows by an empirical meteorologist and climate model analyst:

LAHSEN: In terms of the importance that's given to these models in the outside world, in terms of allocation of funding, some people here have expressed frustration that it's hard for them to get research funding to do some of the footwork that's really necessary to get a good model, but that there's really much work to be done before running these models -- before they're actually as solid as they could be.

EMPIRICAL METEOROLOGIST: I'll state a comment with qualifications, I'm still new enough in the field where I haven't really been out there and experienced this stuff first hand, but I do run into a lot of colleagues who work along the lines of what I do in observational data that are very frustrated by the fact that the largest sums of money and the largest priorities do go toward modeling efforts. And trying to better understand the climate system through data analysis certainly is not receiving priority in the current funding environment and that is a source of frustration for many people.

LAHSEN: Someone said, "It's like trying to drive a car without the wheels." Is that some of what you feel? So you may have data and you might want to give it to a modeler but it won't be taken in, or how does that work? Or you don't get the funding to do that kind of . . .

EMPIRICAL METEOROLOGIST: I think it's more along the lines that there's just a lot of money being poured in for not only the personnel but for the resources, in terms of computer allocation -- computer time -- to do a lot of these modeling studies. And at the same time we can gain a tremendous amount of knowledge from just studying the observational record. And if you're in charge of a research center. . . Again, I don't really have the experience to say that I've been involved with all of this, but my observation is that right now, [where he works], you want to try to bring in as much money as you can and that money is found in areas of modeling.

So if you say, "Well, the division is going to become very involved in building climate system models," you're going to have success at the funding level. However, if you say, "We want to really expand our climate diagnostics effort, people working with observational data," you're not going to be as successful in bringing in money and people and talent.

LAHSEN: Why do you think that is?

EMPIRICAL METEOROLOGIST: Well, again, that's very complicated. At least part of the problem is because what the policymakers want is a very clear, definitive answer [to questions such as:] 'Do we need to worry about controls on power plants and those kinds of things because they are going to cause this kind of a climate change?', and 'What is the impact that it's going to have on the U.S. or Europe or some Third World country or wherever?' And the models are the only tool that we have to try to predict those future scenarios and so I think, by making the statement, "We can try to give you that kind of information through these climate model simulations," it therefore becomes a natural extension that policy makers try to fund these modeling efforts. They realize that in this time of climate change and trying to understand human influence on climate, the way that we can predict future scenarios is with the models and so therefore they fund the modeling efforts.

LAHSEN: So within NSF that's where the money gets earmarked for modeling? That's how it works?

EMPIRICAL METEOROLOGIST: It very much comes down to, at least the perception of applied versus pure science. As a scientist you've got to believe that all levels of science help build up our knowledge base and therefore basic research is good and deserves funding. On the other hand, when the model simulation policy maker or funding agent can clearly see the end result, they can get an answer out of that model and they can say, "This looks like what's going to happen in the future." And that is viewed more directly as having a direct application toward society. Whereas, if I am sitting here and I'm trying to study the partitioning of energy transport in the atmosphere -- And that's very important: for instance, that kind of a study -- energy transport in the atmosphere -- is a critical thing. We need to understand how it works through the observational data so that we can help build the models where they duplicate the processes. Those processes have to be right before we can get some kind of reliable prediction of climate change. But it's much harder for me to explain to someone why I should be funded for that, as opposed to someone saying, "Hey here's a model that tells you that Florida is no longer going to be in existence any more because the polar ice caps are going to melt." I mean, the impact of that is much more

dramatic on the public than [me] sitting here studying energy transports: that is much more abstract.

A year and a half later, this same scientist (quoted above) found the funding situation significantly changed, such that there now is more emphasis on, and resources available for, empirical studies, particularly studies seeking to establish patterns related to natural variability. Nevertheless, tension continues to exist around the allocation of resources, and there is at least residual resentment from past times where modeling was privileged relative to empirical studies.

### ***Scenarios***

The political and policy-related focus on the models comes at a price in that modelers find themselves pressured to construct their models in ways that introduce more uncertainty in the models than they like. One consequence of this is that models become less accurate, jeopardizing their status in the eyes of many fellow scientists and rendering the models more vulnerable to the label of “bad” science. As an example of how external demands impact the practice of modeling, many modelers would prefer to simply produce models suggesting the sensitivity of the climate system to doubling of CO<sub>2</sub> or other types of forcing. However, the policy-interface has pushed modelers to include a time-frame for when the consequences are likely to set in. Groups in the political world generally want clear and definite answers as to what would happen to the climate, when, and with what consequences, due to increasing greenhouse gas

emissions. This has pressed modelers to include “scenarios” in their models based on assumptions about future global levels of greenhouse gas emissions. This involves assumptions about future energy consumption and socio-economic changes at a global level, including changes involving population and economic growth, structural changes in economies, energy prices, technological advances, fossil fuel supplies, and the availability of nuclear and other renewable energy sources, as all of these factors exert major influence on future levels of CO<sub>2</sub> emissions.

As the IPCC has acknowledged, “[scenario outputs] are inherently controversial because they reflect different views of the future.” (Intergovernmental Panel of Climate Change (IPCC) 1992:9-10) . Continuation of current trends is called the “business-as-usual” (BAU) scenario, and the BAU scenario in the 1990 IPCC report was criticized by some as, rather, a “worst case scenario” over-predicting future atmospheric CO<sub>2</sub> concentrations (Gerholm). The IPCC reports have since changed to incorporate a range of different scenarios, but which is labeled the “most likely” scenario is still a matter of controversy. Modelers, however, generally do not spend much time thinking about the scenario assumptions that go into their models; they generally simply choose for their model the average of the IPCC range of five or so different scenarios to choose from, or else pick a scenario chosen by other modeling groups. Reflective of specialization, and of modelers’ tendency to ignore the human dimensions of their work, modelers often don’t know much about the scenario assumptions built into their own models. When I questioned a modeler

about the possibility that the scenarios in his model might be wrong, he exclaimed that the scenarios are the least of his worries; he is working overtime simply to get his model to simulate current climate in an acceptable fashion. Moreover, the tendency for modelers to chose a scenario based on what their colleagues have chosen is reflective of a certain “herd-mentality” among modelers -- a term modelers themselves have used to describe a tendency towards conformity when constructing their models.

### ***The certainty trough***

Policy makers and environmentalists often associate models with more certainty and actual tangibility than is warranted, as is also noted by modelers themselves. When asking modelers about the tendency of modelers to mistake the models for reality, I usually receive the answer that, on the contrary, modelers are the first ones to know the weaknesses of the models, and that it is those who *use* the models, particularly policy makers and those who study socio-economic impacts of future climate change, who interpret model output too literally and uncritically.

Presentations at NCAR serve as cases in point of how this can happen, even in the communication of a model output to scientists and colleagues within the same institution and general scientific field. In one presentation given by an NCAR modeler on the role of clouds in the climate system, comparing observations and model output, it became increasingly confusing to the audience whether the charts and figures on the overhead were based on observations or



on model output. I was thus confused and realized that I wasn't alone in this when an NCAR scientist stopped the presenter to ask for a clarification whether the overhead figures were based on observations or on model experimentation. The latter was the case. The modeler specified this, then continued his presentation.

This phenomenon has been noted by scholars of science. For example, Shackley and Wynne (1995) have described how the practice of GCM modeling is "black boxed" in the sense that those not involved with the modeling themselves, and who may be using its output for such purposes as calculating the possible environmental and socio-economic impacts of climate change, don't see the uncertainties and indeterminacies involved in the creation of a model output. MacKenzie has coined the term "certainty trough" to explain this general process by which perceived certainty of knowledge claims of a research specialty is greatest at some distance from the actual site of the knowledge production (MacKenzie 1990).

Modelers themselves have told me that grant proposals within the National Science Foundation among other granting agencies are treated with care, in subtle ways channeled towards reviewers who aren't likely to hold strong biases against the practice of modeling and the focus on CO<sub>2</sub> (a focus reflective of concern about human-induced climate change). Such privileging and protection of the modelers is then perceived by some critics as a violation of the practice of peer-review. This is where mainstream scientists' criticisms, views and sympathies might tend towards those of the contrarians, the point where

such mainstream scientists may, at least initially, form a sympathetic audience and supportive authority for accusations such as those advanced by Seitz in the Chapter 8 controversy.

The broad social expectation and value that scientists be “untainted” by subjectivity, values and politics -- an ideal scientists as a whole subscribe to and that many continue to believe or suggest possible -- muffles discussion among scientists of the uncertain political aspects of scientific work, including the models. Moreover, pride and the need for continued funding discourage open recognition among modelers of what is wrong in their models, suppressing articulations of the shortcomings and uncertainties in the models and the global observations that they integrate. Consider the following statement by a modeler:

BOVILLE: What I try to do -- which is far from universally successful, but I do it -- is that I say ‘this is what is wrong in my model, and I think this is the same in all models, and I think it is because of the way we’re resolving the equations, that we have these systematic problems.’ And it often gets you in trouble with the other people doing modeling. But it rarely gets you in trouble with people who are interested in the real world. They are much more receptive to that, typically, than they are if you say ‘here, this is my result, doesn’t this look like the real world? And this looks like the real world, and everything is wonderful’.

LAHSEN: How do you get in trouble with modelers with that?

BOVILLE: Because -- there are two ways to do it. One is that you show what is wrong with your model and they pick on those. That is not the real one. The real one is that -- when I present it, I say ‘this model is at least as good as everyone else’s, and these problems are there and they are in everybody else’s models too’. They often don’t like that. Now, even if I am not singling out a particular model, which I have done on occasion, [smiles] not necessarily as being worse than mine, but as having the same flaws--now, when they are trying to sell some point of view and I am going in there saying ‘hey, this is where I go wrong [in my model], and you are doing the same thing! And you can’t be doing any better than that because I know that this isn’t a coding error problem.’ [Laughs]

This segment suggests the role of competition and egos in some discursive constructions of GCMs. While the above modeler says he moderates his claims about his model, and so is more likely to secure understanding and respect for his work by non-modeler climate scientists, such moderation of claim-making about the models is not encouraged by the current situation of increasing competition for research funds. In addition, environmental forces support strong claims-making activities on behalf of the models. Speaking to a full room of NCAR scientists in 1994, Dan Albritton, a prominent scientist, administrator, and frequent governmental advisor on global change, warned the crowd to be cautious about public expressions of reservations about the models. "Choose carefully your adjectives to describe the models," he said: "Confidence or lack of confidence in the models is *the* deciding factor in whether or not there will be policy response on behalf of climate change."

Awareness of this also shapes the practices of the environmental movement and its opposition in U.S. society, forces which respectively highlight the fit and the lack of fit between the models and the actual climate system. Thus, some climate modelers, environmental activists and sympathetic media channels describe the models as "sophisticated," "based on a realistic foundation" (Masood 1995), and as "increasingly accurate assessments of the future" that "correspond" to observations and with increasing accuracy "mimic" the real world (Nixon 1995; Jones 1995). By contrast, the environmental opposition, including contrarian scientists and their sympathetic media,

foreground the limitations and uncertainties of the models. Thus, a 1995 article in *The Economist* described how scientific evidence has not “wholly supported this idea” that human emissions of greenhouse gases are changing the climate, stressing that the evidence suggesting that humans are changing the climate is “based on other runs of the model, not on real life.” Holman Jenkins Jr., editorial writer for *The Wall Street Journal*, used even stronger language against the models in a 1993 editorial. Associating climate models with “unsound science,” he called them “hypothetical disaster scenarios” part of the “now-fading outbreak of climatic doomsterism” led by Mr. Gore and his “crusading” crowd of “hysterics.” And, he claimed, “as climatologists begin gazing up from their computer models at the real world, global warming looks like a flash in the pan” (Jenkins Jr. 1993). As for Rush Limbaugh, he has called claims of ozone depletion and global warming due to humans’ abuse of the environment “[p]erhaps the biggest environmental frauds perpetuated on us in recent years,” blaming concern about global warming on climate modelers and their faulty models. The models were also a primary focus of the series of Congressional Hearings titled: “Scientific Integrity and Public Trust: The Science Behind Federal Policies and Mandates” set up by Dana Rohrabacher, Chairman of the 104th and 105th Congress’ Subcommittee on Energy and Environment in the Committee on Science in 1995-96 and staunch critic of the theory of human-induced climate change.

## ***Conclusion***

This chapter has sought to describe and analyze climate modeling as a scientific practice, suggesting the newness of this scientific practice and the different relationship to data it involves compared to more empirical and traditional scientific methods of investigating the physical world. I have also illustrated the power of the process of modeling in shaping the consciousness of modelers, and suggested how uncertainties and limitations of model output tend to be de-emphasized (or “black-boxed”) as they are absorbed by sympathetic extra-scientific audiences, including policy-makers. In the process of their construction and dissemination, these new scientific tools and products, aided by a favorable socio-political context and important advocates, can seduce both their producers and consumers into confusing the simulations with the real world.

More distant from the seductive influence of modeling, yet close to the observational data on which models depend for their construction and validation, empirical scientists are more frequently critical of models and of the preoccupation with human-induced climate change. As I have suggested -- thereby also building on the earlier chapter on the development of computer modeling and the field of climate research -- such empirical scientists are also likely to resent the ways in which the development of climate modeling has changed their field and come to be prioritized both within the field of meteorology and in the political world. This can render some mainstream empirical scientists sympathetic to contrarian views and actions.

**CHAPTER 7.**  
**SPIRALS OF SILENCE AND DISSENT:**  
**ENVIRONMENTAL VALUES, GATE-KEEPERS, AND THE IPCC**

The rise of controversies over scientific matters in what Samuel Hays calls the “environmental era” reflects new levels of disagreement within the scientific community, and undermines attempts to preserve traditional conceptions of science as objective and able to provide “truth” and order to political emotion and factionalism (Hays 1987). Climate change is a new frontiers of science that environmentalism has helped press forward, and the ensuing debates to which these new frontiers generally have given rise have put intense pressures on the traditional methods of fashioning agreement, consisting in give and take of open discussion in journals and meetings and more private interchanges in ‘peer reviews’ of research proposals and results. A good part of the scientific turmoil over this issue of climate change within the mainstream scientific community must thus be understood in the context of the big changes the field has undergone due to the changed, politicized context in which climate scientists now work. Much of this turmoil centers around the IPCC.

Becoming a focus point for environmental debate has profoundly impacted meteorology and what is now known as the atmospheric sciences. “Earth Day 1970,” was the brief answer by one NCAR scientist to my question as to what has provoked profound, recent changes at NCAR and elsewhere within the atmospheric sciences. Others have agreed. Some have also pointed to the United Nations Conference on the Human Environment in Stockholm 1972 as an

important date, a conference which sought to establish international recognition of the link between the environment and development. Some scientists have expressed regret that transformations provoked by the rise of the environmental paradigm have politicized their field and led scientists into the kind of entrenched positioning that now pervades social and professional interaction among key players on different sides of the issue. The politicization is overdetermined by a complex range of factors, including professional investment in particular theories, conflicts of ego and personalities, and conflicts associated with competition for funding. In addition, the politicization reflects differences in values and worldviews, differences which have significant generational aspects and which are played out around environmental issues.

The standard and pervasive way of explaining and justifying claims to the effect that scientists now agree that humans are changing the global climate is to refer to the IPCC consensus, with its most recent conclusion that “the balance of evidence suggests a discernible human influence on global climate” (Houghton, et al. 1995). Many mainstream scientists express that the IPCC is the best possible system or process they can imagine -- mirroring prevailing opinion of the general scientific practice of peer review. But in my research, I have also encountered an unease with the process. As one IPCC-affiliated scientist wrote in an Email to me, after having read my piece on the controversy over the 1995 IPCC report:

The question I would like to pose to you is the following: Is it possible that, even though the IPCC as an organization may not itself be involved in conspiracy, certain scientists in the IPCC process could be propagating

a one-sided interpretation of global change phenomena to suit their own hidden agenda and personal biases? This question has bothered me for a number of reasons, since I started getting involved with the IPCC. Firstly, given the large level of uncertainty, how can some scientists within IPCC speak (with so much vigor, enthusiasm and confidence) about the situation 50 years and even 200 years from now? Secondly, why do they display such intense, almost personal, feelings against individuals who believe otherwise, and why do they tend to downplay the scientific achievements of the latter?

This chapter reflects on the points raised by the above scientist. I begin by analyzing the role of environmental values and beliefs among climate scientists -

- “environmental” values and beliefs here defined by concern that the environment should be protected against human activities.<sup>50</sup>

After this, I describe the subtle and not so subtle role of boundary-work and “gate-keeping” among IPCC leaders concerned to protect their social and environmental vision and scientific authority, and to shelter themselves from attacks from their powerful opposition of contrarians, industry groups and the US right-wing. They are both propelled and aided in their actions by the tendencies of individuals to subscribe to whichever paradigm (socio-cultural, scientific and political) is accepted by the majority at any given time.

Other chapters in my dissertation illustrate the considerable opposition they suffer from vested interests and public relations orchestrated campaigns concerned to oppose the theory of human-induced climate change, regardless of whether it may be correct or not. The “gate-keeping” behavior and “boundary-work” described in this chapter must be understood in light of these intense



attacks suffered by the IPCC and by some high-profile mainstream scientists supporting the theory of human-induced climate change. Moreover, the chapter on the controversy over the 1995 report showed that accusations of “conspiracy” (and of political repression etc.) tended to distort more than illuminate the processes and events actually involved; in the case of that controversy, the accusations were clearly propelled by certain political interests, and they reflected poorly the actual process of negotiation of interests, meanings, and interpretation of procedures, involved in the formation of IPCC reports. The accusations of the IPCC and its supporters -- and of the theory of human-induced climate change generally -- often involve and evoke strong capitalist interests and old anti-communist rhetoric, rhetoric reminiscent of the Cold War era which is still prevalent among groups of the US right-wing. This line of accusations often reflects more about the accusers than about the accused. Starting with that large caveat to contextualize and qualify what follows, I want to point to the existence of certain pressures and mechanisms which can work to discourage scientists from pursuing research and voicing opinions that conflict with the claims and orientation of the mainstream, as represented in IPCC. Certain dynamics within the scientific mainstream and the IPCC can tend to discourage well-reasoned, sincere scientific work pointing to countervailing evidence and alternative theories which challenge the dominant paradigm of human-induced climate change. Gate-keeping behavior and boundary-work can

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<sup>50</sup> This definition of an ‘environmentalist’ is derived from Kay Milton’s definition of “environmentalism” as “a concern that the environment should be protected” (Milton 1996:27).

also undermine democratic participation in issues related to human-induced climate change, which, as a new environmental problem, is characterized by the uncertainty and thus requires interpretation and value judgments for its definition.

### ***Environmentalism within the mainstream scientific community***

The concept of human-induced climate change is part of a discourse that relates environmental change to something bigger, often a perceived crisis of industrial society. Together with acid rain and ozone depletion, climate change is part of a new era in the politics of pollution. Before the early 1980s, pollution problems were considered within a local framework of impact and prevention. These (and other) new environmental issues shift the focus on pollution from a local, isolated level to a global level foregrounding interconnectedness between peoples and processes across traditional local and national boundaries. The IPCC as an institution represents the new era in pollution politics, and it also serves to promote this new understanding of humans' effect on the global environment.

As mentioned above, the IPCC was formed in response to the 1988 rise in concern about humans' possible "dangerous interference" with the climate system. The United Nations' Environmental Program (UNEP) and the World Meteorological Organization (WMO) set up the IPCC to assess the science of climate change and provide the scientific basis for the international negotiations to stabilize global emissions of greenhouse gases -- negotiations based on the

recognition of the new, global nature of important environmental problems.

The guideline for the IPCC specifies that it is "to assess on a comprehensive, objective, open and transparent basis the scientific, technical and socio-economic information relevant to understanding the risk of human-induced climate change, its potential impacts and options for adaptation and mitigation" (U.S. Government 1996:333). As the chapter on the controversy over the 1995 IPCC reports rendered evident, the objectivist language obscures the large role of interpretation, conflict, interests, and environmental concern in the formation of the assessment reports.

In spite of the objectivist language, leaders and other participants in the IPCC process tend to hold environmental values. The IPCC is closely integrated with the United Nations' Framework Convention on Climate Change (FCCC), which is designed to take precautionary measures to anticipate, prevent or minimize the causes of climate change and mitigate its adverse effects. This establishes the precautionary principle as the guiding principle at the heart of the FCCC, aligning it with the environmentalist paradigm (O'Riordan and Jäger 1996:19). An environmentalist bent to the IPCC process has been noted by analysts. Sonja Boehmer-Christiansen (Boehmer-Christiansen 1994a; Boehmer-Christiansen 1994b) has described leaders within international scientific enterprises such as the IPCC as motivated, in part, by an environmentalist agenda. Similarly, Lin Gan has found that the United Nations' Environmental Program overseeing the IPCC is "largely governed by various kinds of environmentalists" (Gan 1993:262).

Scientists in atmospheric and global change research generally characterize themselves, as well as their colleagues, as environmentally conscious and concerned. When William Gray, a university-based atmospheric scientist (who, as we see elsewhere in this dissertation, is on the skeptical side of the debate), asserted as an unquestioned fact that most scientists are environmentalists, I asked him to explain:

GRAY: .... most scientists are environmentalists and...

LAHSEN: Most scientists are?

GRAY: Yes, I think so. Don't you think so?

LAHSEN: I don't know.

GRAY: Well I, I would think so, yes. They have a good feel for the population problem and the use of natural resources and the general environmental problems that humankind is going to have. And so, when they hear about something like [global warming, even if they aren't very familiar with this field or issue] they tend to be open and more acceptant of it.

That scientists tend to be environmentally inclined is not surprising, of course, given the general dominance of environmental values in American society in the 1990s (Kempton, Boster and Hartley 1995).<sup>51</sup>

The writings of IPCC scientists and supporters also reflect environmentalist worldviews. For example, Sir John Houghton, Chairman of the IPCC Working Group I and thus a top leader within the IPCC, has written a book on global warming in which he outlines his environmentalist views. He describes how Christian values shaped his precautionary and preservationist

environmental values (Houghton 1994). Other books by atmospheric scientists also express environmental concern along with the argument that humans need to change their ways to address the many and complex issues the perceived climate threat involves (Firor 1990; Somerville 1996; Schneider 1990 (1989)).

### **The inculcation of an environmental habitus**

Environmentalism pervades the culture of climate scientists; it has become an important shaping factor in how a majority of climate scientists perceive their world, and an important criteria in defining the character and moral fiber of each other.

Some skeptics, and all the contrarians -- characterized more by their individualist philosophy than by environmental concern -- object to the social pressure they experience among scientists, and in society generally, towards supporting an environmentalist ethos and agenda. In an interview with me, contrarian Richard Lindzen pointed out the extent to which validation as a moral person depends on that person's acceptance of dominant environmental values and beliefs. By resisting favored "environmental" views and actions, a scientist may risk losing important moral status among colleagues and, to the extent that this is portrayed in the media, in large segments of the general public. There is some truth to Lindzen's claim in a *The New York Times* article that "if you are

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<sup>51</sup> Kempton et al.'s study reflected expressed values, but not in any direct way how those values affect

questioning the basis of global warming, you are definitely treated as someone who hates the earth" (Stevens 1996b). Lindzen objects to what he sees as a "loss of perspective" among environmental groups, saying that with regards to global warming and other issues, environmental groups have gone "off the deep end" (Stevens 1996b), and he draws analogies between the social pressures towards conformity with dominant environmentalist views and the eugenics movement of the early 1920s:

It is one of those unfortunate situations where an advocacy movement adopted a scientific issue. Like all advocacy groups, whether it is eugenics or environmentalism, it assumes the moral highground. So that as long as the issue is in fashion, to go along with it is to establish yourself as a good person.<sup>52</sup>

Elsewhere, Lindzen -- whose family emigrated from Europe to the US due to anti-Semitism and persecution under Hitler -- has compared the environmental movement to the Nazi movement in Weimar, Germany (Gelbspan 1995:54). Leaving aside evaluation of Lindzen's analogies and of whether an environmental bent to scientific research agendas is "good" or "bad," the point here is that Lindzen as well as other contrarians take issue with the environmental paradigm shaping social and scientific processes in the atmospheric sciences.<sup>53</sup>

As an example of the role of environmentalism in the atmospheric sciences, let me describe how an environmentalist ethos has become

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actual behavior.

<sup>52</sup> Interview, December 5, 1995.

<sup>53</sup> One problem with Lindzen's assertions is that he tends to imply that scientific pronouncements would be objective, were it not for the involvement of advocacy groups. However, as scholars studying science in

institutionalized at NCAR -- a national center for atmospheric research visited by many non-NCAR scientists as well as non-scientists every year, and how NCAR tends to engender an environmentalist "habitus." As developed by Pierre Bourdieu, "habitus" refers to the habits and inclinations (as manifest in thoughts and behavior) that are inculcated in individuals through their interactions and surroundings. Importantly, individuals' habitus is shaped less through direct teaching than through experience of social processes and structures. Objective structures are an important means through which habitus is shaped, influencing individuals' inclination to act and react in certain ways -- in ways which reflect the particular social conditions of the individuals:

The habitus ... provides individuals with a sense of how to act and respond in the course of their daily lives. It 'orients' their actions and inclinations without strictly determining them. It gives them a 'feel for the game,' a sense of what is appropriate in the circumstances and what is not, a 'practical sense' (*le sens pratique*). The practical sense is not so much a state of mind as a state of the body, a state of being, it is because the body has become a repository of ingrained dispositions that certain actions, certain ways of behaving and responding, seem altogether natural.

(Bourdieu 1991: 12-13, in the *Editor's Introduction* by J. B. Thompson).

Applying this theoretical framework to NCAR, it is clear that the organization orients visitors and employees towards an environmentalist habitus. NCAR offers tours to the general public, and to bus-loads of elementary and high school kids, almost every day of the week. Visitors are introduced to NCAR with a ten-to-fifteen minute video in the main entrance hall of the NCAR structure

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society have pointed out, virtually all research, basic or applied, involves social processes and shared values and norms.

known as the Mesa Lab. The video first depicts a satellite picture of the globe, the powerful symbol which has come to evoke the fragility of the planet as perceived by environmentalists, including understandings of global interconnectedness between humans, and between humans and their habitats. Viewers then see shots of NCAR's Mesa Lab from the outside, with its beautiful natural surroundings. NCAR's first buildings were built in the foothills of the Rocky Mountains, enjoying stunningly beautiful natural surroundings in which mule deer casually walk or lie close by. Other wildlife, such as bears, coyote, and rattlesnakes, are also occasionally spotted in the area. A narrator's voice accompanies the video's pictures of NCAR and its natural surroundings: "There is beauty, unity, balance -- a mirror of nature's own design. When you are confronted by nature, by such power and beauty, you don't try to compete with it, you try to join with it." This is a quote from I. M. Pei, the famous architect who designed NCAR's Mesa Lab after the architecture of the Anasazi Indians in Southern Colorado. The suggestion is that NCAR, like nature, involves beauty, unity, and balance, and that it is important to live in harmony with nature. The video explains that NCAR was built for one purpose: to understand the atmosphere. "Our atmosphere is complex and interdependent, in a vital relationship with all living things," we are told, as pictures of the serene nature around NCAR abruptly end with scenes from an industrial city filled with cars, noise, pollution, and people. "Unfortunately, humans often choose to live at variance with the atmosphere rather than in harmony with it," the narrator continues, as we are shown high-rises, more cars, and traffic jams. "To live in



harmony with the world around us, we must understand its architecture, how it works and reacts to changes in its delicate balance," we are told. The connection to NCAR is made more explicit yet: "It is nature's design for the earth, and especially for the atmosphere, that is the primary concern for NCAR. Tracking the negative impact of human activities, trends can be slowed down if enough of us understand the problem and support efforts to solve it." This pitch for public support of NCAR through appeal to environmentalist values -- including the dominant environmental belief that nature is fragile, its delicate balance easily disturbed by human activities -- is followed by a more detailed description of NCAR scientists and their activities. The video ends with yet another appeal to the viewers: after another round of natural imagery (mountains, moon, moving clouds, silent nature, a person reflecting in nature), we are told: "If such beauty and balance are to survive, we must gain a greater awareness of our planet. To understand our atmosphere is to bring into sharp focus our place on this Earth, and our responsibility to it." The video ends with a quote from NCAR's founding director, the late Walter Orr Roberts, which reads: "No field of science offers a greater potential for the good of all mankind than does the field of atmospheric science" -- certainly not an uncontested claim among the sciences!

Aside from the natural surroundings of NCAR's Mesa Lab, NCAR employees are exposed to an environmentalist ethos through the value placed on alternative transportation and recycling. NCAR provides "ecopasses" (passes to public transportation in the area) free of charge to encourage employees to commute by means of public transportation, and NCAR has its own shuttles

which transport passengers from the local bus stops to its Mesa and Foothills Labs. Every year, NCAR's numerous divisions compete against each other based on which division has more of its employees commute by bicycle. Recycling is encouraged and facilitated by the placement throughout the buildings of recycling bins for different recyclable types of materials -- various sorts of plastic and paper products, glass, and even organic waste for composting. Employees are also urged to bring their own glasses and mugs when frequenting NCAR's cafeterias. In recycling areas, the cans for non-recyclable trash are marked by unpleasant pictures of a landfill, reminding employees and visitors to carefully recycle what can be recycled, and fighting the general tendency to ignore what happens to trash once discarded in a trash can. In all of these ways and more, a new type of thinking is encouraged, one which is conscious of the interconnections between people's actions and pollution; the predispositions of employees and visitors are subtly (and sometimes less subtly) adjusted towards environmentalist views and behavior.

In my research, I have encountered scientists of all persuasions in terms of the issue of human-induced climate change who were concerned to establish that they do care about the environment. Though I didn't experience this among contrarians, such self-identifications with environmental values and behavior were often accompanied by references to what type of transportation they use to and from work, and, even, in a couple of instances, to what type of office lighting (fluorescent or non-fluorescent) they used. As Lindzen pointed out, among atmospheric scientists, morality is increasingly wound up with notions of good

environmental views and behavior; as much as -- or perhaps even more than -- material status symbols, environmental rhetoric and behavior have become important means of obtaining personal and moral validation. For example, speaking about the difficulty of reducing local pollution and greenhouse gas emissions, the director of one of the large U.S. climate modeling labs, himself a climate modeler, spoke of his personal efforts to cut emissions of carbon dioxide:

DIRECTOR: I happen to know from personal experience that it's very difficult [to cut emissions]. I use half the energy that most people do.

LAHSEN: How do you do that?

DIRECTOR: Well I drive a very small car. I have done everything imaginable to my house. To talk about doing something twice as big as that, you are almost of the edge of being labeled a nut. Interesting sociology: I am the boss and I drive the smallest car in the parking lot. It isn't because of global warming. I am just saying, okay, what happens in my own behavior? -- rather than speaking theoretically about all this. The point is, in order to keep greenhouse gases from going up, you have to have a drastic reduction in your per capita fossil fuel use.

Perhaps suggestive of the influence of a director on the self-presentation and behavior on employees, the self-presentation of the following climate modeler resonates with that of the director -- his director -- above:

LAHSEN: In terms of action [on behalf of human-induced climate change], what would you like to see done? I assume you are concerned, right?

MODELER: Yes, I have my own political views which I tend to keep to myself.

LAHSEN: And you like to keep it that way?

MODELER: Absolutely. Because, I don't, I mean, I really am a strong believer..

[After an interruption, he continued:]

I allow my political views to affect my actions. So, in a sense I am consistent! [slight laugh]. I insulated my house. I took the trouble to do that, to caulk around the windows, and to make my hot water heater more efficient, and all those kinds of things, you know, very practical kinds of ways. I drive my family bunker some times, but -- that's another issue! [What gas mileage a car gets] is not the only predictor when I buy a car, but it is one predictor. I just recently bought a [new car], and it doesn't get quite the mileage I would like. But that is one predictor [of what car I choose to buy]. It gets about thirty miles per gallon.

LAHSEN: That's even pretty good.

MODELER: Nah, I have [another brand of car], it gets fifty!

LAHSEN: A diesel?

MODELER: Yes.

LAHSEN: But aren't those worse with their emissions?

MODELER: It depends... that black stuff that comes out falls right to the ground again. That's a classic aerosol! So it does pollute, but it's more efficient, if you think about CO<sub>2</sub>: Per gram of CO<sub>2</sub> that flies out of the pipe, you get more power. It is much more efficient, by a factor of five, probably.

Strong individualist values and more concerned about the economy and the preservation of free market forces than about potential environmental threats, contrarians resist such environmental awareness, deriding it as irrational and religious in nature, and equating it with (much resented) political correctness. In the words of Richard Lindzen (cf. above), the environmental movement has, in his view, "gone off the deep end."

Even as I encountered frequent informal and general attributions of environmental values among the large majority of mainstream scientists, scientists tend to be cautious about publicly expressing their own environmental

values; it is not always prudent for scientists to acknowledge their own personal values and beliefs, whether on the concerned or the critical side of the issue of human-induced climate change. Such caution in public expression is also reflected in the disinclination of the above scientist to publicly express his environmental views and values ("I have my own political views which I tend to keep to myself"). Scientists tend to be reluctant to openly acknowledge their own values and beliefs, fearful that it might undermine their scientific authority and general status as 'objective conveyers of truth.' Perhaps for this reason, one atmospheric scientist and administrator referred to most people at his major research lab as "closet environmentalists," a term suggesting the sensitivity of open recognition of personal values.

Attempts by some scientists to explain the ethical and moral underpinnings of their work and positions on climate change have been used against them to undermine their authority. Richard Lindzen expressed, in a 1989 *Science* article, his conviction that there "has to be" natural feedback processes that counteract the warming produced by humans. He said that his conviction of this was based in part on "theological" beliefs (Kerr 1989). Critics of Lindzen have repeatedly pointed to Lindzen's statements in this article to undermine his credibility on the climate issue, suggesting that his scientific views are colored by his 'religious views.'

A second high-profile example of how scientists use as ammunition opponents' acknowledgments concerning their personal values and beliefs (perhaps, as is standard in science, particularly those related to religion) also

involves Richard Lindzen. In this example, Lindzen used the same criticism he was subjected to due to his casual reference to theology. He used it to undermine the scientific authority of Sir John Houghton, the IPCC leader, by referring to Houghton's above-mentioned description of how his Christian background shaped his environmental views. The context in which Lindzen did this was a hearing in Minnesota in May 1995, which was held to determine the environmental costs of coal burning by state power plants. During his testimony at this hearing, Richard Lindzen was asked by the examining attorney to expand on an earlier reference in his testimony that Sir John Houghton was "motivated by religious need to oppose materialism."

LINDZEN: Well, I suppose it was that John had indicated that he was motivated by something other than simply the science of purported warming. [...]

ATTORNEY: I would like to ask, motivated to do what?

LINDZEN: Motivated in his participation and involvement. That he felt his involvement in this process...

ATTORNEY: The IPCC process?

LINDZEN: Well, yes, and the consequences, namely curbing energy usage and so on, had to be seen in a larger religious context.

Given such "cheap shots," and the generally impoverished level of communication between the opposing sides in this debate, it is not surprising that scientists tend to keep their opinions to themselves. This tendency to attack opponents on the basis of their values and beliefs -- often, with suggestions of sinister plotting and "political agendas" -- is typical of high-profile scientists on

both sides of the climate issue. Usually, the same scientists shroud themselves in objectivist claims, portraying themselves as objective observers and denying the play of personal values, political strategizing, and contextual contingencies in their work. In the process, they put up a smokescreen which increases the difficulty for non-experts of calibrating conflicting scientific statements.

### **Resilient or fragile nature?**

A standard interpretation might be to conclude that the debate about human-induced climate change involves environmentalists on the one side, and non- or even “anti”-environmentalists on the other. This is the conclusion one draws from reading Carl Deal's *The Greenpeace Guide to Anti-Environmental Organizations* (Deal 1993). However, critics of the IPCC and of the focus on human-induced climate change can not be so quickly labeled, nor can the scientists professing concern about human-induced climate change. One must ask: what does it mean to be an environmentalist?

Since the 1970s, environmentalism (again, here understood as “a concern that the environment should be protected”) has become the dominant paradigm in the US and elsewhere (Hajer 1995; Kempton, Boster and Hartley 1995). Whether a person is or is not an environmentalist depends on how one defines the term. While one might define as environmentalist someone who assumes nature (understood as the non-human, physical and organic environment) to be

fragile in the face of human activities, and who advocates protective measures, actually identifying non- or anti-environmentalists is not always easy.

Richard Lindzen's conviction that there "has to be" natural feedback processes that counteract the warming produced by humans involves the assumption that nature is resistant to humans' polluting activities. Similar assumptions of natural counter-acting reactions to human forcing are expressed by other contrarians. For example, Patrick Michaels, atmospheric scientist at the University of Virginia, has written:

In order to do justice to science, however, it is necessary to replace a failed vision with another that is more insightful. I believe the atmosphere-environment paradigm will change from one of 'vulnerability' to one of 'resilience' because the response to the greenhouse insults will turn out to be remarkably small and primarily benign.<sup>54</sup>

As for the now retired Lawrence Livermore National Laboratory affiliated meteorologist, Hugh Ellsaesser, he aligned his views with those of the Environmental Conservation Organization (ECO), which also believes in a resilient nature. ECO was established in 1990 by real estate developers and others opposed to wetlands regulation, and is described by Carl Deal as one of the broadest coalitions organized to counteract environmental regulation (Deal 1993:51). In their newsletter, ECO promotes the view of nature as resilient, meshing this view with free-market rhetoric:

The most powerful lesson pouring from the pages of history is the fact of nature's resilience, nature's adaptability, and the ability of the human species to continually improve its condition. The deepest valleys that have occurred along the path of progress have accompanied unsuccessful

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<sup>54</sup> Patrick Michaels, in statement to the University of Virginia, 1995.



attempts to impose man-made controls on the natural behavior of people (Environmental Conservation Organization 1995).

By contrast, scientists concerned about human-induced climate change think that nature might surprise humans by sudden, violent reactions to human forcing. For example, Wallace S. Broecker, a geochemist and oceanographer at Columbia's University's Lamont-Doherty Earth Observatory, has publicly described the climate system as "an angry beast" which humans are "poking ... with sticks," referring mainly to human emissions of greenhouse gases (Stevens 1998).

The premise of resilient nature runs counter to a fundamental premise of most persons referred to as "environmentalists" in common language, and at some level one has to define the term "environmentalist," if it is to retain any meaning. Nevertheless, judging from most public discourses related to environmental issues, we are all "Green" now.<sup>55</sup> In interviews and writings -- even as the term "environmentalist" is sometimes associated with irrational extremism -- contrarians often present themselves as environmentally conscious, as desiring a clean and safe environment and believing in some level of caution to prevent environmental degradation. For example, in an interview, Lindzen expressed that he, like everyone else, values the environment. Similarly, in his memoir, Seitz portrays positively the public's interest in the environment and health:

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<sup>55</sup> This point is also made by Hajer (1995: 14).

We are currently seeing in the United States a highly desirable interest on the part of the public in both the environment and human health as influenced by the expansion of technology. (Seitz 1994:381).

In his interview with me, Seitz also responded as follows when I inquired into his environmental views:

SEITZ: We clearly have to keep an eye on the environment. Just as a note, Europeans learned long ago that they have to preserve their forests, or they paid a big price through soil erosion and all those things. None of the groups with which I associate myself say we can turn our back on the environment. [...] By and large, civilized people have long ago learned that they've got to take care of their habitat.

LAHSEN: Okay, but then what kind of power do you think we have, for example, in terms of altering the climate; do you think that humans actually can alter it?

SEITZ: It could happen. But I am sure we will have plenty of advance signals, as measured.

Supporting Seitz' skepticism regarding human-induced climate change, then, is his assumption that nature is kind enough to give humans ample warning before it gets uncontrollably out of balance, in marked contrast to Broecker's nature which is like an angry and unpredictable beast.

While contrarians do not consider themselves "anti-environmentalist" -- the term applied by Greenpeace and others to persons of contrarians' persuasion -- their discourses suggest views and assumptions which conflict with dominant environmental views in US society, and which exclude significant concern about the new types of environmental problems identified above. Whereas the contrarians subscribe to a more traditional approach to environmental problems, one which grew out of a focus on local environmental problems such as local air pollution, most mainstream scientists subscribe to an approach which has

emerged with awareness of new environmental problems characterized by their trans-boundary and unpredictable nature, and by their potentially catastrophic consequences. Contrarians portray as imaginary and illusory most new environmental problems. For example, Hugh Ellsaesser's has characterized issues of environmental concern as "imaginary problems," and, in this context, warned: "Look for the Bodies Before You Panic." This insistence on present and tangible evidence and clear causal relationships conflicts with the paradigm of new environmental problems, the latter conceptualized as inherently unpredictable, invisible, creeping, and as involving unclear or not easily established causal relationships.

### **Environmental and Scientific advocacy**

I have frequently witnessed strong boundary-work on their part of mainstream scientists to distance themselves from environmental non-governmental organizations, at least to the extent that the latter simplify and sensationalize environmental phenomena. Disinterestedness is still a dominant ideal, if not an assumed possibility, and scientists are trained to see and acknowledge complexities when presenting problems -- while the opposite holds true among environmental activists, who tend to dramatize issues such as to instigate concern and action. Some NCAR scientists have expressed to me that they ignore canvassers for Greenpeace when they occasionally come to NCAR to seek support.

However, some scientists have accepted a new paradigm according to which scientists must not and cannot remain isolated in their ivory tower. One reflection of this acceptance -- and, in the cases, also reflective of an overall environmental stance on the part of such scientists -- is a greater level of formal and informal ties between atmospheric scientists and environmental organizations. For example, a former NCAR director now serves as trustee on the boards of several environmental non-governmental organizations; Stephen Schneider and other scientists published in a book on global warming edited and published by *Greenpeace*; and when controversies such as that around the IPCC chapter 8 erupt, the involved scientists often exchange Emails with representatives of environmental organizations such as Greenpeace, the Environmental Defense Fund, and Ozone Action.<sup>56</sup>

Particularly exaggerated rhetorical constructions of the threat of human-induced climate change sometimes take place at the interface between scientists and the media. This often happens when complex scientific announcements and findings are simplified and interpreted by scientists, journalists and politicians for personal or public consumption. Such exaggerations are often aided by sufficiently ambiguous scientific statements by "hawks," in the climate debate, statements which lend themselves to exaggeration and misinterpretation. Aside from the media's inclination towards sensationalist stories, strategic decisions

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<sup>56</sup> Throughout my fieldwork, it also happened on numerous occasions that scientists familiar with me and my work forwarded email messages sent to them by these and other environmental organizations through listserves, messages addressing the politics of the environmental opposition and pointing out contrarians' "political agendas," ties with industry groups, and the financial benefits contrarians enjoyed through such interactions.

and assumptions on the part of scientists and environmental activists can propel such exaggerations; scientists as well as activist organizations have been known to use a variety of tools to raise public concern about the issue, including scare tactics. Michael Oppenheimer, atmospheric scientist and a high-profile director within the Environmental Defense Fund, has publicly acknowledged having used the tactic of offering 'scary scenarios' on the assumption that this was necessary to raise public awareness. In the book on human-induced climate change, titled *Dead Heat: The Race Against the Greenhouse Effect*, which Oppenheimer co-authored with Robert H. Boyle (Oppenheimer and Boyle 1990), the authors wrote: "What is needed is a knockout punch -- warming must be understood to threaten the continuation of life on Earth or no one will pay attention until things get out of hand" (quoted in (Cohen 1995:29)). A journalist later contacted Oppenheimer about this quote, and Oppenheimer explained that he had been "unduly gloomy" about the populace. He said: "I think I was wrong [...] I was more skeptical [back then] about what people were moved by." He also clarified that he didn't mean to suggest that global warming threatened the continuation of life on Earth, though he didn't dismiss the possibility that events could spin out of control and lead to catastrophe (Cohen 1995:29). Some years back, Stephen Schneider made a comment similar to that of Oppenheimer and Boyle, a comment for which he has been much attacked, also by mainstream scientists. While saying that hopefully scientists don't have to choose between being honest and effective in their communication with the media, Schneider said:

On the one hand, as scientists, we are ethically bound to the scientific method, in effect promising to tell the truth, the whole truth, and nothing but ... On the other hand, we are not just scientists but human beings as well. ... We need ... to capture the public's imagination. That, of course, entails getting loads of media coverage. So we have to offer up scary scenarios, make simplified, dramatic statements, and make little mention of any doubts we might have. ... Each of us has to decide what the right balance is between being effective and being honest (quoted in Seitz 1994:382)

Contrarians and their supporters frequently refer to this statement, but leave out Schneider's qualifying comment that hopefully scientists don't have to choose between being honest and being effective.

While "hawks" can be seen downplaying uncertainties related to projections of human-induced climate change, contrarians similarly downplay uncertainties when making their pronouncements that there will be no such warming. In an interview with me, Patrick Michaels expressed his assumption that the human-induced climate warming won't be significant, and supported this assumption by referring to what he perceives to be the political mission of the IPCC:

MICHAELS: But yes, the greenhouse effect is warming the planet. We have said that, in each one of these lectures, we say that the greenhouse effect has something to do with the warming, and it will probably continue to. The issue is not whether it exists. Whether it exists is irrelevant! -- if it is not large.

LAHSEN: And what makes you assume that it won't be large?

MICHAELS: Because of what has happened in the last one hundred years. I mean, the IPCC 1992 report was developed for one specific reason: to provide scientific backing for the Rio treaty. That is why; I am not kidding.<sup>57</sup>

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<sup>57</sup> Patrick Michaels, personal interview. Charlottesville, Virginia, October 23, 1995.

Michaels' level of certainty is shared by other contrarians. Using the data collected by Granger Morgan and others, Stephen Schneider has demonstrated how Richard Lindzen (the only contrarian included in Morgan et al.'s data) differed from other scientists -- leading scientists in the scientific mainstream -- in his expressed level of certainty, in spite of the uncertainties in the science on which Lindzen is basing his assertions. Compared to the non-contrarian, mainstream scientists' expression of uncertainty with regards to future manifestations of human-induced climate change, Lindzen expressed, with a much greater level of certainty, that no significant temperature change will happen due to human activities.<sup>58</sup> Lindzen's faith is based on hypotheses and incomplete evidence, such as his theory that atmospheric concentrations of water vapor won't allow the amplification of temperatures from the doubling of atmospheric carbon dioxide (based on preindustrial levels) by more than about one degree Fahrenheit (Stevens 1997). Lindzen's critics fault him for professing unwarranted sureness in a field of research rife with uncertainty; "I don't know what line from God he has," Schneider is quoted as saying in the *New York Times*, citing Lindzen's estimate of future warming, which Schneider sees as overprecise, as expressing "more certitude than the facts allow" (Stevens 1997b). Lindzen's focus on a doubling of carbon dioxide based on pre-industrial levels is also problematic in that it fails to account for the possibility and consequences of even greater increases in atmospheric concentrations of

greenhouse gases; while calculations of future consequences use a doubling of greenhouse gases based on pre-industrial levels as a benchmark, this is an arbitrary number -- a convention rather than an actual threshold.

As pointed out above, all sides in the debate accuse the other side of being motivated by personal and political agendas to obtain funding and to promote particular social changes, while the accusers claim themselves to be entirely apolitical and objective. With the exception of Stephen Schneider (as described below), the scientists I have studied have tended to be remarkably constructionist in their renditions of their opponents' positions, and remarkably unreflexive or silent as to the personal or socio-political agenda underlying their own position. The word "agenda" is perceived to be associated with sinister and secretive plotting, and there is little discursive room for talking about the significant value dimensions inherent in any position-taking concerning a threat such as climate change.

In spite of resistance to it, scientific advocacy, and the communication of scientific projections in spite of uncertainty, is becoming more of a norm among scientists, in part due to the politicization of science in late twentieth society, but also to the inherent uncertainty of new environmental problems. Characteristic of what Ulrich Beck (Beck 1992) calls our "risk society," we are producing risks and hazards as part of modernization. Though this often only can be done in approximating ways, policy makers want these risks and hazards to be understood and quantified. In the context of having to thus define risks which

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<sup>58</sup> Stephen Schneider, Woodlands Conference, Rice University, Houston, Texas (April 3, 1998).



are inherently uncertain and trans-scientific in nature, scientists are asked to communicate their science in ways that are intelligible to policy-makers and lay persons, a particularly noticeable point of entry of subjective judgments and opinions. As Beck writes:

[I]n definitions of risks the *sciences' monopoly on rationality is broken*. There are always competing and conflicting claims, interests and viewpoints of the various agents of modernity and affected groups, which are forced together in defining risks in the sense of cause and effect, instigator and injured party. There is no expert on risk. Many scientists do go to work with the entire impetus and pathos of their objective rationality, and their effort to be objective grows in proportion to the political content of their definitions. But at the center of their work they continue to be reliant on social and thus prescribed expectations and values. Where and how does one draw the line between *still* acceptable and *no longer* acceptable exposures? How susceptible to compromise are the presupposed standards? Should the possibility of an ecological catastrophe be accepted, for instance, in order to satisfy economic interests? What are necessities, *supposed* necessities, and necessities *that must be changed*? (Beck 1992:29)

Among climate scientists, the person who has been most candid about the dynamics of scientists' new roles as policy advisors and scientific advocates is Stephen Schneider. Schneider has written about the role of subjective judgment and values in scientific pronouncements, and advocated that scientists explicitly mark the points at which they move from describing the science to making policy suggestions (Schneider 1990 (1989)).<sup>59</sup> He has also written about the disciplinary- and value-related assumptions and strategies involved in the definition of a given threat as involving "high" or a "low" probability (Schneider 1990:33). Scientists who appear frequently in the media are often resented, and

presented as limelight seekers with big egos and the tendency to "shoot from the hip;" they are said to undermine the credibility of scientists by making public statements about new environmental problems prematurely and with unwarranted certainty. Stephen Schneider is seen by many to be guilty of this; his name occurs frequently when scientists -- mainstream, skeptical, and contrarian -- criticize the politicization of science. The circumvention of peer review (as when scientists go straight to the media and make pronouncements that have not been reviewed and generally accepted within the scientific community) is not well-seen among scientists.<sup>60</sup>

In the documentary *The Greenhouse Conspiracy*, produced by Hilary Lawson for a TVF production for channel 4 in Britain, Schneider is singled out for criticism when the argument is made that global warming is an invention by self-promoting scientists, politicians, and the media, who have "connived to make a good story" (Lawson 1990). That the climate is okay does not usually make the headlines, the narrator of this 'anti-greenhouse' documentary explains, and "the best prophets of doom are the ones filmed the most," we're told, as the camera cuts to Schneider being filmed for some news production. Another interview segment features Schneider saying "The rate of change is so fast that I don't hesitate to call that catastrophic... for ecosystems" is followed by Lindzen

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<sup>59</sup> In my opinion, this would be a step in the right direction, although it leaves unacknowledged the role of social factors in other aspects of scientists work as well, such as the choice and formulation of research projects, and how they are communicated ("constructed") in social interactions.

<sup>60</sup> For example, Seitz refers to a quote by Stephen Schneider when describing what has gone wrong in science (Seitz 1994:382). However, Seitz' criticism of Schneider is undermined by his own obvious political engagements, including his participation as co-author of the non-peer-reviewed Marshall Institute reports, among other things.

expressing his embarrassment by the kind of claims he hears scientists make about climate change, and his concern that they discredit “his” science, ruining climatologists’ credibility now when “there might be problems in the future that will require scientific judgment. By ruining our credibility now, Lindzen argues, we leave society with a diminished resource of some importance.” The camera then cuts back to Schneider saying: “Of course you always tell the truth, but how many of us ever get on the evening news for more than twenty-three seconds? Can you give the whole story in twenty-three seconds? You have to selectively give bits of information,” he says. The camera cuts back to Schneider at the news photo-shoot, in the limelight.

Schneider himself is minutely aware of the disapproval of advocacy among scientists. In articles and books, Schneider explains some of this in terms of jealousy, but in terms of the unwritten rules in science, still prevalent among scientists, which decree that recognition must be based on years of careful work, backed up by scores of publications appearing in the most strictly peer-reviewed journals dealing with narrowly defined topics. Published deeds that stand the test of time are supposed to build one's recognition, not clever phrases that capture the public's attention through the media, writes Schneider (Schneider 1990 (1989); Schneider 1993). Yet, he argues, scientists have a responsibility to communicate their work to the broader public, in ways and forums accessible to nonexperts. Besides, he adds, if scientists don't popularize their views themselves, someone else will do it for them, possibly with problematic errors and interpretive spins.

While Schneider has been widely attacked for his acceptance of a scientific advocacy role, he articulates a gradual change within atmospheric science, one which the IPCC has been central in bringing about. Sir John Houghton, chairman of the IPCC described the increased comfort of many scientists in taking on an advocacy position, and in communicating in ways more accessible to policy makers and a general public.

HOUGHTON: Well, you see, I mean, we have broken new ground in the IPCC in the science-policy interface. In the way we try to involve governments in the reports. Governments come with all sorts of agendas of their own, and they try of course to bring these to the meetings. [...] The [governmental] delegates get up and talk on grounds that are other than scientific. And you get up and say 'sorry, but this is a scientific meeting. If you want to talk science, fine, if you want to talk politics, please go away.' And uh..

LAHSEN: Do you think it is possible to make that distinction so clearly?

HOUGHTON: well it is... [small pause] pretty clearly, actually, in the scientific work group. But it's harder when you get to the economics. But in the science section [i.e., Working Group I of the IPCC] you can do that. Mind you, people will sometimes talk about their motivations... their way of talking about [and] producing scientific arguments can be a political one. But you have to take a scientific argument for what it is, and say, okay, we will either use that argument or not.

And it all comes into the balance of the report. Because climate is a very uncertain business. There are things we don't know. So the uncertainties have to be expressed, and the way you express that, it has to be a very balanced expression. And maintaining that balance in the face of political pressure from any side is quite hard. But we do that, and I believe we have obtained balance.

LAHSEN: And, it seems to me that there are scientists who make the leap from -- you know, 'this is the science and there are all these uncertainties' -- to acting on it. I think that sometimes that is what some have a problem with, would you agree? There are

uncertainties, and the whole scientific ethic, traditionally, seems to be that you don't speak out until you know absolutely for certain. Right?

HOUGHTON: Well, but we've moved a long way, actually. I remember when I first began in the IPCC and we had our first meetings, there was *great* reluctance on the part of many scientists to actually do anything, or say anything.

LAHSEN: There was resistance to say anything?

HOUGHTON: There was resistance to being told to try to make predictions of the future, as to what might happen, you know, best estimates as to what might happen in the future. There was *enormous* resistance to that. Because they said 'we don't know enough to do that.' And I and a few others argued 'well we are scientists. We actually know more than anybody else about the subject. If we don't say everything we know, then we are running away from our responsibility.' Because then other people who won't have the same inhibitions that we've got who probably could have undo public influence.

I was head of the British Met Office at that time, and you know, made weather forecasts every day. There is a lot of uncertainty associated with making a weather forecasts, yet nevertheless you make them -- because they are usable. And your forecaster makes his best story, about what the weather will be. And the public knows that it is uncertain, because they have experienced weather forecasts before. But they still find the forecasts useful, because there's a large part of them who say 'we will do this' or 'we will do that' depending on the forecasts -- knowing that it could be a bit wrong, but that it's nevertheless the best guidance that they've got. When we are projecting climate change, it is essentially the same thing. We are making up estimates as to what might occur, we try to put error bars on it, we try to give the uncertainty ranges because of course people have no experiences with climate forecasts, so we have to try to explain just what are our feelings of uncertainty. We might say: global temperature will rise by this, and rainfall will change by that, and different regions will... Whatever we can say. We try to give the best information we can give you. And the important thing to do then is to plan on that basis -- realizing that it is uncertain. And in your commercial firm, or the government, you put that into your planning for the future. You say, 'well, the likelihood is that we are moving to this sort of climate

regime and therefore we should take notice of that and what kind of planning does that need?'

LAHSEN: how did you see that change -- that in the beginning there was more resistance to taking that kind of step? And how did that change happen?

HOUGHTON: Hm. Because it was exposed in the media, or people were talking about it, scientists began to realize their responsibility to actually say what they know. And that is what we do -- that is the idea we've tried to put over in our reports as well as we could: to decide what we know and also what we didn't know, in the clearest possible way. [This] began to be generally accepted and it took us a period of a couple of years before people -- before many people - - felt comfortable with it. And there were some people who didn't feel comfortable with it *at all* during the years! And there are some who don't feel terribly comfortable with it now, actually. But there are scientists who over this period in general have come much more to realize that the science they do is relevant to policy, and who have come to recognize that they have a responsibility to explain that relevance, to explain their science in a way relevant to policy makers. We were breaking fresh ground in 1988. And scientists were very -- some scientists were reluctant to do that. [...]

There has been a very interesting evolution, and I think the IPCC has helped to drive that, the evolution, in a very interesting and

unique way, actually. I don't know of other bodies in the world that have done quite like that, in the way it works.

***The Spiral of Silence:  
Forming the reports, and the use of consensus formation***

As a means of establishing its authority, the IPCC emphasizes the great number of participating scientists and different countries they represent: For example, the 1992 IPCC Supplement report starts with a preface which explains the purpose and structure of the IPCC and the processes involved in producing their reports:

The conclusions presented in the Supplement are based entirely on the supporting scientific material published here, which has been prepared by leading scientists and exposed to a widespread and thorough peer review... Generation of the background papers involved, either as lead authors or contributors, 118 scientists from 22 countries. A further 380 scientists from 63 countries and 18 UN or non-governmental organizations participated in the peer review of both the background material and the Supplement. The text of the Supplement was agreed in January 1992 at a plenary meeting of Working Group I, held in Guangzhou, China, attended by 130 delegates from 47 countries. It can therefore be considered as an authoritative statement of the contemporary views of the international scientific community" (Intergovernmental Panel of Climate Change (IPCC) 1992)

Yet, the consensus forged by the IPCC is what in the Science and Technology Studies literature is called "suboptimal essential consensus," meaning that it is shaped by the most vocal and powerful scientists. As I point out in other chapters, the IPCC process is also shaped in important ways by industry and other interest groups. My focus here, however, is on the scientists within the

IPCC who act as defenders of the IPCC and of the theory of human-induced climate change.<sup>61</sup>

A consensus functions optimally “when each member of the group knows the justificatory standards and current beliefs of all the other members, especially as changes come about as the result of social interaction” (Fuller 1988:214). When this is the case, a consensus is called an “essential consensus.” However, even in the best designed essential consensus, perfect information of this kind is unlikely to be readily available, if only because not everyone is constantly, explicitly, and while heard by everyone, stating his or her beliefs. This is the case with the IPCC. A consensus which is less than essential is called “suboptimal,” and a suboptimally functioning essential consensus produces what has been termed “the spiral of silence.”<sup>62</sup> This happens when those who either disagree with a standing belief or who have no strong views on the issue simply remain silent. If the public forum is presumed to be a democratic one and thus equally accessible to all, then there is a strong temptation to take the more highly visible (or audible) positions as the ones most representative of group opinion. For example, if a particular belief is either attacked or defended frequently, members tend to presume that the belief has a relatively large

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<sup>61</sup> What is not mentioned by skeptics is that the reports also don't necessarily reflect the more proactive groups that are part of the IPCC process; significant concessions are made by groups who would like the wording to be more forceful. Leaders of the IPCC complain that the process has become too political because of who is allowed to participate], claiming that the role of expertise at times gives way to political considerations such as including as lead authors scientists from less represented and less developed countries. Leaders also express concern about their ability to forge a meaningful consensus with the participation of oil producing countries and fossil fuel industries, who, unabashed about their economic interests, fight to water down the consensus statement to such extent that it undermines any sense of urgency and hence preventive action.



following, when, of course, it may just have a few very articulate spokesmen (this is what Noelle-Neuman's research suggests, as described in Fuller 1988). What results from this tendency to not suspect silent disagreement in democratic forums -- since there is no immediately apparent reason why some would be holding back -- is that the silent members of the group, who might otherwise have remained uncommitted, start to move toward what they perceive to be the prevailing opinion.

The IPCC process involves a core group of particularly influential, vocal, and high-profile scientists. It is scientists of this group who, as the IPCC scientist quoted at the beginning of this chapter, "speak (with so much vigor, enthusiasm and confidence) about the [ecological] situation 50 years and even 200 years from now," and who "display such intense, almost personal, feelings against individuals who believe otherwise," in the process tending to "downplay the scientific achievements of the latter." It can be difficult for less established scientists to not defer to the authority and opinion of such powerful figures. At present, little research has been done into the role of particularly influential scientists and particular values, assumptions, and political ambitions inform IPCC assessments of the science supporting concern about human-induced climate change (Shackley and Skodvin 1995). Thus, one can also mostly only speculate, and the following constitutes largely speculation in this regard, while

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<sup>62</sup> The term was applied by social psychologist Elisabeth Noelle-Neuman, based on the work of Alexis De Toqueville (Fuller 1988:214)

drawing on particular instances when possible. Many of these instances require in-depth examination, wherefore the following must be understood as tentative examples of how extra-scientific considerations shape the IPCC reports, and of the particular influence some scientists have in the process of forming the reports.

### **Struggles over language**

The most recent IPCC consensus statement -- that “the balance of evidence suggests a discernible human influence on global climate” -- sounded particularly confident. However, as also suggested in the chapter on the controversy over the 1995 IPCC report, further scrutiny renders evident that the process of forming such consensus can be quite chaotic, marked by significantly more disagreement than is suggested in official renditions of the reports by IPCC leaders and supporters.

The scientific reports by the IPCC involve excruciatingly long deliberations over how to characterize current scientific knowledge about climate change. Given their strong influence, these reports form a central site for the struggle over how to characterize models and related climate change research, the basis for policy action. The struggle over words is pervasive, reflecting the central importance of words in the age of information where knowledge is power; critics dissect the document showing how the middle chapters do not support the statements in the

summary, how, for example, an "estimate" in a middle chapter -- which few policy-makers read -- becomes a "prediction" in the widely read Executive Summary. In the field generally, "validating" a model means checking model output against observations, but representing a subtle shift in language, the word "validate" is at times replaced with "verify", which perhaps sounds similar but suggests that the truth value or the realism of a modeled scenario has been established -- which is somewhat misleading. A similar slip happens from "projection" -- i.e., an extrapolation into the future based on current trends -- to "prediction", which suggests the ability to foresee the future.<sup>63</sup> It is due to such sensitive use of language that political scientist Sonja Boehmer-Christiansen refers to the IPCC summaries as "skillful exercises in scientific ambiguity" using "language which simultaneously allowed Greenpeace to call for a target of reducing emissions by sixty percent, and the UK Treasury to conclude that no action was needed until more scientific certainty was available each citing the same source" (Boehmer-Christiansen 1994a; Boehmer-Christiansen 1994b).

In this context, readers may also recall the above chapter on "The Detection and Attribution of Conspiracies" in which I quote an IPCC Lead Author at length. The Lead Author expressed the difficulty of communicating an assessment of human-induced climate change in a

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<sup>63</sup> Suggestive of the antagonism between the opposing sides in the debate, and of the proclivity to detect conniving and even conspiracies among adversaries, Patrick Michaels, testifying at a recent hearing

way that provides useful information to policy makers, “without completely discrediting the scientific approach to the problem” by leaving out too many caveats. He also suggested his own environmental concern and how it shapes word choice in the formation of the IPCC reports:

So, boy, the number reiterations that individual sentences can go through in order to express a particular concept in a way that doesn't overstate but doesn't totally diffuse the issue by stressing the uncertainty. It is a very difficult road to tread, I think.

The role of micro- and macro-sociological dynamics in the production of the IPCC reports is acknowledged in some forums by IPCC sympathizers, particularly in informal conversations and jokes. Having attended her first IPCC meeting, an economist described her surprise at the level of disagreement she found at the meetings. The following are the notes from our conversation, shortly after this IPCC meeting at NCAR. It had taken place a month after the controversy over the 1995 IPCC report erupted, and the goal of the meeting was to decide on how to summarize that report in a more accessible manner for governments:

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She was surprised at the degree to which differences of perspectives prevailed at the meeting on the economic part of technical paper II; “from the outside, I’d thought that they had come to more of a consensus,” she said. They were still a couple of stages away from being ready to agree on an outline for this summary.

The first day there was a smaller meeting of economists doing modeling of costs of emissions reductions, and where they disagreed, they understood where they disagreed and why. The second day, the meeting included “everybody”; it was a more diverse group with people from different disciplines, of different training. They had different

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described above, accused the scientists supporting the theory of human-induced climate change of deliberately having chosen the word “projection” because it so easily is mistaken for “prediction.”

understandings of what the other folks were doing. Some remarks were off on a tangent, not necessarily central to what was to be done. "The time schedule struck me as hopelessly ambitious", she said; the policy deadline didn't give them much time.<sup>64</sup>

Another example of how the IPCC reports are shaped by individuals in the pool of attendees is a scientist's comment that El Nino wasn't mentioned much in the 1990 report, possibly because "someone who spoke for El Nino interests wasn't at the meeting."<sup>65</sup>

Speaking of how the IPCC forms consensus statements, Stephen Schneider has publicly made humorous comments about the ad hoc nature of the process. The first comment was made during the showing of a film on Australian weather forecasts in which a panel is filmed in the process of forming the consensus statement about the possible near future weather trend. One person on the panel suggested a statement, which was quickly corrected by several others. After a number of gradual changes, the first person then restated the (to be) official statement, while trying to maintain an air of composure and authority even as the statement now conflicted considerably with his initial statement. At this point in the showing of the documentary, the viewing audience laughed, and Stephen Schneider interjected "that is how IPCC statements are made!," which was followed by more laughter. At another public engagement at

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<sup>64</sup> She also noted the heightened vigilance among IPCC leaders that they not make themselves vulnerable to criticism, and that throughout this meeting the IPCC chairman, Bert Bolin, stressed the importance of following procedures very carefully because lawyers from opposing groups were trying to stick them with procedural issues. They were urged to make sure that drafts got out to everyone, etc. IPCC leaders are now urging all participants in the process to make sure that they follow all procedures to the tee, while expressing their resentment against their attackers. They are vexed that the attackers are focussing on procedural issues around the IPCC reports, rather than on the science itself, and portray this as reflective of

Rice University in March 1997, Schneider made the humorous comment that how any particular IPCC consensus statement turns out may reflect "who had more coffee during the break."

Some scientists within the scientific mainstream -- that is, apart from the high-profile contrarians -- complain that the IPCC peer review process is flawed, and that not all views are equally considered. Some complain that their point of view isn't represented; one scientist, a widely respected atmospheric scientist, told me he was asked to review one of the reports. He wrote several pages of comments but while his name was listed as a reviewer and his scientific authority thus used, there was no effort made to include his comments, nor did anyone write him back to explain why they weren't included, he complained. Bias in scientific peer review generally has been amply documented, and is not particular to the IPCC process: evaluation processes generally tend to be social and political in nature.<sup>66</sup> Nevertheless -- and particularly given the tendency of the IPCC to portray itself as the consensus of thousands of scientists -- it should be pointed out that a recent research by M. Granger Morgan and David W. Keith

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how the critics, said to lack scientific justification for their attacks, must satisfy themselves with attacks on procedure and resort to "defense lawyer tactics."

<sup>65</sup> Michael Glantz, personal conversation, 5/23/96.

<sup>66</sup> Bias in peer review generally has been demonstrated by many studies. A recent study published in *Nature* (May 22, 1997) showed clearly the role of nepotism and sexism in the peer review process in Sweden, Sweden even being associated with greater gender equality compared with most other countries in the world (Wennerås and Wold 1997). In their study of peer-review scores for postdoctoral fellowship applications in the Swedish medical Research Council (MRC), microbiologist Christine Wennerås and clinical immunologist Agnes Wold found that peer reviewers overestimated male achievements and and/or underestimated female performance (they showed this by means of multiple-regression analyses of the relation between defined parameters of scientific productivity and competence scores). They write that for a female scientist to be awarded the same competence score as a male colleague, she needed to exceed his scientific productivity 2.5 times -- translating into publishing approximately three extra papers in *Nature* or *Science* (the two most recognized international journals), or twenty extra papers in excellent specialist journals of their field.

(Morgan and Keith 1997) found a greater degree of disagreement regarding key climate variables and the nature of the climate system (though not regarding climate sensitivity) than is often conveyed in scientific consensus documents such as the IPCC reports.<sup>67</sup>

That more scientists may disagree with the IPCC than is widely believed was also realized by one of the IPCC key Lead Authors of the IPCC:

The categories of these reviews fall into three categories, the good, the bad and the ugly. The good ones are where people have made very knowledgeable and constructive, and sometimes critical comments, comments that are well-founded and that give you a chance of actually incorporating them into the manuscript. The "bad" are the ones where they do the same thing but that are intensely critical. They have some scientific credibility but disagree with statements that are made. Usually the good people don't disagree in total; they may disagree with some small points. And the bad ones are people who have justifiable disagreements, sort of on the larger scale. And then there are the ugly, who just say 'this is a lot of garbage,' and they don't tell you why, or they tell you 'this is wrong, it contradicts paragraph three section four,' or whatever, and when you actually check, you see that it doesn't. There are many more people in that group than I thought there were, in the ugly. They constitute this group of greenhouse critics. There are a lot of people out there; there is only a handful that have a big public image. They are often interviewed by the press or make statements. But there is a lot of other people who are essentially disbelievers, you know, they just don't believe that there is a problem, or they have a misconception as to what the scientific issues actually are. And they think that scientists are saying that we should take immediate action to reduce the emissions of greenhouse gases and we have to make a major change in the whole global economy to achieve that. Well, you know, I don't know any scientists who are saying that, but there are people out there who think they are saying that, so they criticize the whole issue because they think the way to counteract the possibility of drastic action is to say that there is no problem.

I will not discuss here the fact that IPCC scientists in reality hardly can be said to advocate drastic reductions in greenhouse gas emissions; although literature

written by Singer, including the 1996 "Leipzig Declaration," often suggests that the IPCC advocates draconian cuts in greenhouse gas emission (upwards of sixty percent), no IPCC scientists that I have encountered in fact advocate even something close to that (Singer 1996). For the purposes here, the point is that the Lead Author identified a much larger group of dissidents than he expected during the review process of the 1995 IPCC report.

### **The issue of expertise**

[T]he growing importance of expertise and the technical framing of political issues works to only further marginalize citizen participation, the cornerstone of democratic governance (Fischer 1991:349)

Also of interest in the Lead Author's statements above is his delegitimization of the strongest dissenters as authorities on the scientific issues reviewed. Similar dismissals of "non-experts" have been exercised by other key scientific participants in the IPCC. For example, in an article analyzing the signatories of a declaration drafted by Fred Singer, the writer quotes Kevin Trenberth -- a key IPCC Lead Author and a high-profile, widely recognized climate expert -- characterizing two signatories as "nonentities." The two Leipzig signatories were listed as "Gaynor, John E. / Envir. Tech. Lab., Boulder, CO" and "McVehil, George E. / Air Quality & Meteor. Englewood, CO." The IPCC Lead

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<sup>67</sup> The authors used structured interviews and "expert elicitation" methods, focussing on widely recognized experts in the field.



Author claimed to not recognize any of them, saying: "None of them is known professionally at all in climate research. They are nonentities" (Olinger 1996).<sup>68</sup>

The Olinger article suggests how scientists whose expertise is uncertain and challenged might pose as experts -- or else be presented as such by interested actors. The case described by Olinger involves the "Leipzig Declaration," an effort at signature collection orchestrated by Fred Singer during the summer of 1996 to counter IPCC claims of scientific consensus concerning human-induced climate change. Singer solicited signatures from scientists by sending the declaration to members of the American Meteorological Society and the American Geophysical Union, and to a general group of collaborators and sympathizers. Singer released the "Leipzig Declaration" during the July meeting of government representatives in Geneva to discuss abatement measures, including international controls and taxes on energy. The news release by Singer's organization announcing the release of the Leipzig Declaration included the following title, written with large, capitalized letters in bold: **EUROPEAN AND AMERICAN SCIENTISTS WARN AGAINST "PREMATURE" ACTIONS ON GLOBAL WARMING : Joint Statement Finds "No Scientific Consensus."**

The letterhead from Singer's organization, the Science and Environmental Policy Project (SEPP) listed Singer as the President, and Frederick Seitz, Fred Singer, and William Nierenberg, plus two other Ph.D.s, on the Board of Directors and Science Advisors to SEPP. All of the people on the boards were named and

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<sup>68</sup> The article was forwarded to climate scientists via email by Ozone Action, an environmental activist organization.

listed with their institutional affiliations and their degrees. All but two were Ph.D.s -- but the field in which they had received it wasn't specified. One was an M.D. and another had a M.S. in the field of public health. By contrast, the list of about eighty signatories largely omitted reference to formal education, here listing people only by name and institutional affiliation (e.g., "University of Berlin"), except in cases where the persons weren't listed with any institutional affiliation at all. In those cases, the signatories were identified instead by discipline (usually "meteor" for "meteorologist") and/or geographical location (e.g., "Kohler, Max A / Meteor. Silver Spring, MD") and "Dietze, Peter /Numberg, Germany"). It was implied that all these persons were scientists, as the signed declaration started with the sentence "As scientists, we..." The statement declared that they considered dangerous international leaders' discussion about constraining energy use and mandate reductions in CO2 emissions from fossil fuel combustion, and that they, the signatories, "cannot subscribe to the so-called 'scientific consensus' that envisages climate catastrophes and advocates hasty actions."

In the article, titled "Cool to the warnings of global warming's dangers," journalist David Olinger describes in a sardonic tone that many of the signers of Singer's Declaration weren't climate experts, some of them even largely lacking formal training as scientists. Olinger recognizes some of the signatories as having "well-established national reputations," and mentions Neil Frank, former director of National Hurricane Center, David Aubrey, a coastal research scientist at the prestigious Woods Hole Oceanographic Institute, and Frederick Seitz,

“former president of the National Academy of Sciences.” But many other names on the Leipzig list are unrecognizable to leading climate researchers, and it includes several scientists “whose daily bread has been buttered by industries that produce greenhouse gases,” writes Olinger. Olinger mentions, among others, Patrick Michaels (“the global warming critic whose newsletter is financed by the Western Fuels Association” and Robert Balling, (“the Arizona State University climate scientist whose research has been supported by coal companies and Kuwait”). Olinger then describes another category of signatories, whose expertise is questionable because of limited scientific training:

[Another signatory is] Richard F. Groeber, whose scientific credentials do not include a college degree. In Springfield, Ohio, Groeber is better known as the operator of Dick's Weather Service. He tracks weather data at his private station, but avoids the trickier job of forecasting. A long-time observer of Ohio weather, he suspects global climate trends are related to sunspots, not greenhouse gases.

Is Groeber a scientist?

"I sorta consider myself so," he said. "I had two or three years of college training in the scientific area, and thirty or forty years of self-study."

The phonetic presentation of Groeber's reply works as a marker class, suggesting that Groeber lacks the polished self-presentation characteristic of persons of higher education. The description of Groeber as a data collector who 'avoids the trickier job of forecasting' also works to undermine his authority in the field of climate projection.

Olinger describes another signatory, listed as “Leep, Roy / meteor. Tampa, FL”:

At WTVT in Tampa, Roy Leep has a sophisticated array of meteorological equipment, a long-standing reputation for reliable forecasts and a seal of approval from the American Meteorological Society. A brief version of his forecast appears each day in *The Times*.

The article comments:

What Leep doesn't have is a Ph.D. in any scientific field, or for that matter, a bachelor's degree. He was taking meteorology courses at Florida State University and broadcasting radio weather reports when WTVT hired him in 1957.

And yet a third signatory:

In San Francisco, the "scientist" who signed the declaration is KPIX weatherman Brian Sussman, who thinks "the jury is still out" on global warming. He has a bachelor's degree in meteorology.

By contrast to IPCC leading scientists, who have earned their reputations through peer-review processes,<sup>69</sup> the expertise of many of the signatories of the Leipzig Declaration in the area of climate change is questionable.<sup>70</sup> It seems obvious that societies should and would want to seek expert advice from the most knowledgeable persons on a given topic.

However, the issue of expertise raises some troublesome questions and problems. How is expertise to be defined? Who qualifies as an expert? According to the Random House dictionary, an expert is "a person with special skill or knowledge in a particular field"; a person "possessing or showing special

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<sup>69</sup> Of course, peer review is not an asocial and impartial process. For examples of bias in peer review processes, see Wayt W. Gibbs (1995) and Wennerås and Wold (1997).

<sup>70</sup> Yet the Leipzig Declaration continues to be presented by contrarians and journalists as an important document countering the IPCC reports. Having attended a conference in the Pacific North-West organized by The Fraser Institute, described as an "independent, free-market think tank," journalists for a Seattle publication wrote a long article recounting contrarian arguments on the issue of human-induced climate change. The article also referred to the Leipzig Declaration to make its point that "there does not exist today a general scientific consensus about the importance of human-induced climate change: 'And why do so many journalists ignore the Leipzig Declaration on Global Climate Change... signed by almost 100 scientists?'" (Hamer and Parks 1997).

skill or knowledge” (Random House 1993:229). But does a person have to have an advanced degree in that particular field? And if not, is it necessary that she or he have an advanced degree in a neighboring field? Might a person not become a sort of climate expert through self-learning? While in theory there may be at least a vague definition of who qualifies as an expert (i.e. someone who has published in the peer reviewed literature on climate change), in practice there is no agreement on the issue. And even if there were a consensus among mainstream scientists in the area, this is likely to be contested or overlooked in the larger debate, as lay persons, journalists, and marginal and extra-scientific groups interested in promoting either side of the issue uphold as “experts” those who claim to be experts or whose views and science (or “science”) support the interests, values or assumptions or the appropriators.

Clear identification of expertise is also rendered difficult by the interdisciplinary nature of climate science; it is not always easily established in this field involving such heterogeneity of issues, scientific fields and approaches. The “community” of climate scientists is often identified as involving a core group of only three- to five-hundred scientists, internationally. However, once one includes all scientists contributing bits of pieces to the study of climate, the community includes many thousands of scientists. It is therefore not always easy to define the border between authentic and inauthentic climate scientists; it requires interpretation. Thus, IPCC scientists dispute the claims to expertise by persons such as Fred Singer and Frederick Seitz, while Singer presents himself as an “atmospheric physicist,” and while Seitz himself feels sufficiently

conversant with the issues to make public statements about it and to publish the influential, albeit non-peer-reviewed, reports on climate through the Marshall Institute. In the fields of atmospheric science and global change generally, people might obtain expertise in climate research despite having obtained their official degree in other fields, such as physics or biology. As a result, an important criteria among scientists for whether or not a person is a climate expert is whether they have published peer-reviewed articles in the field or not. Neither Seitz nor Singer meet this criteria.

One must also be alert to the potential (and widespread) role of self-serving boundary work in scientists' attempts to undermine the scientific talent and expertise of other scientists. For one, scientists tend to downplay the science of competitors and neighboring disciplines, while considering their own discipline and projects to be more important. As one scientist commented:

DAN CAYAN: Well, yes, I think that it is natural that the more you look at one side, at one side of an elephant, the more you are convinced that that is the most important one, because that is the one that you are familiar with.

Some argue that climate modelers as the true experts on human-induced climate change, while arguing that old-time, empirical meteorologists -- meteorologists such as William Gray, a hurricane expert -- are not climate experts. But, as we will see in a later chapter on climate modeling and modelers, the latter are not unbiased experts. Here is another dilemma with regards to expertise: experts often have a personal or professional stake in what science is valued or debunked, such that, in some instances, the public might be better

served by the *informed* opinion of a less interested party. In the context of chapter 8 of the 1995 IPCC report, Ben Santer acknowledged the problem of experts' personal stake in, and lack of distance to, the science being assessed:

SANTER: There was a real attempt on our [i.e., the lead authors'] part, and on the part of others, to make [the chapter] up to date and to reference work that was relevant and ... that fulfilled the IPCC criterion of being available in draft form. I mean, the reality of the thing was that we [the lead authors] had done a lot of the pattern-based detection work. And other groups were now becoming more involved with that, but our studies looking for combined greenhouse gas and aerosol effects on climate were the first such to do that. And it was tough. You couldn't step back and say, 'well, okay, we're not going to deal with any of our own work at all.' This is a field where most of the emphasis five years ago was just on looking at global mean changes in observation; it's only relatively recently that people have been looking at patterns. And some of those first studies were by Tim Barnett and by Tom Wigley and myself [the lead authors], so it's kind of unfortunate that the people who were the lead authors had done much of the work, much of the relevant work.

LAHSEN: It's kind of a Catch-22, because you want people who are more knowledgeable in the field to do the evaluation.

SANTER: It *is* a Catch-22. If you get people who are not knowledgeable about the idiosyncrasies of the studies to evaluate them, then you have trouble. If you get people who are now knowledgeable about them, then you have all the individual biases, professional animosities, or jealousies, that you have to deal with. So it's really tough to get people who can see very clearly that the important thing is the science, and who portray it fairly and with the right balance, too. I think I could have done better there. [...] And I 'd do a better job next time around to make sure that balance is at the forefront. That is a difficult lesson to learn, it really is, because you are so close -- you can't see the forest for the trees; you're so close to your own research, it's very difficult to stand back and say 'well, what is the broader picture I want to show here', and to realize, as I do now, that the main thing is getting the balance and getting the science fairly represented -- not to focus on your own work.

Thus, on the one hand, there are non-experts, scientific and non-scientific, who may not have sufficient knowledge of the particular area of science. On the other

hand, there are experts whose knowledge may be undisputed, but who may be prejudiced, because of their acceptance of the dominant paradigm and because of their stake in, their better knowledge of, and their affinity with, their own work. Too strict definitions of expertise disempower non-experts, discourage democratic participation, and exclude even scientists in the same or related fields but of different subspecialization. Moreover, scientific expertise, in whichever area, should not be a requirement for participation in debate about human-induced climate change. Given the undeniable component of perspective and value judgment in the estimation of risk (Otway 1992), the public needs to be involved in the process of defining what constitutes the “dangerous interference” with the natural climate system that the FCCC is designed to counter. This will have to involve democratic deliberation over what constitutes acceptable and unacceptable risks, and acceptable and unacceptable distributions of environmental consequences and financial responsibility.

### **Disciplining scientists: the role of social pressure and “gate-keepers”**

The harshest critics talk about the IPCC as controlled by a small group of scientists, the “secretariat,” who use their positions to promote their environmentalist agenda and to secure funding for their research. As should be clear from much of the preceding discussion, I take issue with many of the accusations against the IPCC. However, I have witnessed the control exercised by powerful scientists involved in the IPCC. In what follows, I will describe several such examples, including the problems I myself experienced when



seeking permission to study IPCC procedures. Concerned to maintain IPCC scientific authority, and, particularly, to avoid public attacks and controversies such as the one that erupted around Chapter 8 of the 1995 IPCC report, IPCC leaders vigilantly protect the public image of the institution.

On two or three separate occasions, I was denied access to IPCC meetings and documents and discouraged from seeking such access. The following are my fieldnotes from one such occasion:

Many people at NCAR weren't even aware that the IPCC was having a meeting at NCAR during July 11 and 12, 1996. When I found out, I wanted to attend the end of it. As I approached the open doors to the room where the meeting was taking place, I could hear the person presenting. "There are people out there who are saying that there is a conspiracy going on -- that the IPCC is part of a conspiracy." The room erupted in laughter. The person then went on to explain the arguments such persons and groups advance: "The first argument is that the historical data doesn't support the theory, that the models are wrong. The second argument is that even if there is some warming, it will be beneficial," he continued. "The third argument is that even if there is warming, and even if it isn't beneficial, there is nothing we can do about it; the costs of doing anything are too high, they will damage the infrastructures, the global economy." His voice turned concerned: "And we have no mechanisms for addressing this well..."

The subject discussed was of course of great interest to me, especially since I, at the time, was writing about the prevalence of conspiracy theories among actors involved in the climate debate. I had been standing near the doors and heard this much, when Thomas Wigley came to the opening of the doors to give some instructions to his secretary who was just arriving. He saw me out of the corner of his eyes but ignored me as he talked to his secretary. He was familiar with me, my project and credentials, including my affiliation with NCAR. I had also had what I perceived to be a positive interview with him, in which he at least

momentarily appeared open to the idea of having me study one aspect of the IPCC review process. Yet, when I asked if I could sit in on the meeting, he looked very uncomfortable and expressed in an anxious but definitive tone: "No, I really don't think so. I don't think they would let you. It is very political."

In an interview with IPCC officials on July 12, 1997, I asked them about the possibility of studying the IPCC process more closely. The following is an excerpt from that interview. It shows the reluctance of the IPCC to open itself to scrutiny, as well as some of the associated reasoning:

LAHSEN: Are all the reviews that come in of a particular chapter kept and filed?

HOUGHTON: Yes.

An IPCC technical assistant: Yes. Some of the comments of the lead authors on the reviews, why they were accepted, why they were not accepted.

LAHSEN: So an author can not just dump something?

HOUGHTON: Naeh, no, I mean, some very simply... It is an enormous job for the Lead Authors. I wouldn't like to suggest that it is completely fool-proof, but it's meant to be, and [the authors] do take it very seriously. They take it very seriously indeed.<sup>71</sup>

LAHSEN: And are those files accessible?

HOUGHTON: They are in the IPCC office. They are in principle accessible. But, I mean, don't expect anybody in the office to make any effort to -- to, you know, dig things out.

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<sup>71</sup> My interview with Ben Santer showed that to be true: it is an enormously demanding task to be Lead Author of an IPCC chapter. Santer was meticulous in his care to gather all documents associated with his chapter, making an admirable effort, especially considering the lack of assistance in doing this and the time and organizational skills it demanded. Lead Authors receive no assistance nor guidance in terms of how to store, organize, and process the large amount of documents of relevance to the chapter, including review comments.

LAHSEN: Well, say one person wanted to go through it. And I have asked Tom Wigley a while back whether I could go through the review chapters of one particular chapter, to get a sense of who the people are who are coming with certain types of arguments, and just...

HOUGHTON: Well, in principle it is available. I mean, we'd like to know exactly what you would do with it when you're finished with them, before you could... But in principle they are there and it is open. It's an open process. But we won't... I mean, just to protect my staff, who are desperately overworked [slight laughs by Houghton and the IPCC technical assistant]. And we've got to be very careful..

LAHSEN: So if I wanted to do it, I would go to England?

HOUGHTON: You would have to go to Bracknell, yes. You could have access to certain of those files, I think. But don't expect a lot of help in going through it all, because it's a very busy office, and we're very busy at the moment. And we would also have to have some definite agreement with you about exactly what you could or could not publish as a result of that. Because... [joking] you might be working for [Global Climate Coalition attorney] Donald Pearlman, or something like that, for all that I know.

LAHSEN: [laughs] Ah...

HOUGHTON: Well, hey, it's a pity, because we want to be very open, we want our process to be very open. But all those people in DC, who want to have access to our files and such, and then go through and pick out a single thing and say 'look what I found.' You know.

LAHSEN: It is a difficult thing for me too. Because I asked and there it was said that it wasn't a good idea; I was denied access to the reviews of an IPCC chapter because there was concern that I might find things that would be used by...

HOUGHTON [interrupts]: No, if you had a genuine purpose, and we were convinced you had a genuine purpose, I would be very happy for you to spend time doing that. You know, we want our process to be open, we want to be quite clear. We would have to have a right, probably, to look at anything you wrote as a result. I mean, to edit -- not in a scientific sense but in a procedural sense -- anything that you produced as a result of it.

LAHSEN: Something else happened this morning as you were having your meeting, and I asked Tom Wigley if I could come in. And he said 'no, no, I don't think so.' You know, that just seems a pity because I think it would be very good to have people come in and...

HOUGHTON: I wouldn't object to that, to you coming in. We've had people coming in before but we've had someone -- unfortunate -- I mean, an unfortunate experience, really. Have you come across Sonja Boehmer-Christiansen's work?

LAHSEN: Yes, I have.

HOUGHTON: Yes, you see, we let her sit in on a lot of our meetings -- actually, she sat in on a great deal of our efforts. She wrote some quite good stuff to begin with, but then recently [...] some of her recent material has been very unobjective about the IPCC. And quite wrong, actually. I mean, she has made some statements that are totally incorrect, about our procedures and those sorts of things. So we have to be cautious.

Sonja Boehmer-Christiansen is a British political scientist who attended IPCC meetings in the past and consequently wrote articles in *Nature* and *Global Environmental Change* arguing that the focus on human-induced climate change was propelled by scientists' interest to gain support for their research programs and environmental agenda, combined with the interests of alternative energy industries (Boehmer-Christiansen 1994a; Boehmer-Christiansen 1994b). Her argument, while cogent and persuasive in some regards, relied on a simplistic interest-based interpretive framework which resonated with contrarians' perceptions of vast conspiracies and sinister, self-serving motives on the part of mainstream scientists. Boehmer-Christiansen also failed to produce any evidence for her of the instrumental involvement of alternative energy producers,

for which I have encountered no evidence, in spite of persistent inquiry among a heterogeneous group of scientists, administrators, and policy makers.

The earlier occasion at which I had requested permission to study the IPCC took place prior to the controversy over the 1995 IPCC report. During my interview with Wigley, I had inquired into the possibility of studying the pool of reviews to the IPCC Chapter 8 of which Wigley was Lead Author. Wigley himself had recognized that the reviews of Chapter 8 -- sent in by a greatly heterogeneous group of scientists -- constituted what he referred to as a "sociological goldmine." Wigley responded to my request by saying that I should send my resume and a letter to the persons higher up in the IPCC, telling them what I do, and what I would like to do with the reviews. I did as he said, and my request was denied -- for reasons, I was told, having to do with concerns that I might find things which would be used unfairly against the IPCC by their strong critics.

Pressures to conform with the theory of human-induced climate change operate at numerous levels, and may be more or less explicit and obvious. Such pressures are often overdetermined, rooted in a complexity of impulses, interests, and concerns. Some times the pressure may be rooted in professional anxiety on the part of those who have staked their scientific reputations on the theory of human-induced climate change, or on the part of younger and less established scientists who are in structurally more vulnerable positions to challenge the theories and claims of the most powerful figures in their field. One scientist explained as follows how dissenting views might be attacked:

LAHSEN: Some people feel muffled by institutional processes, pressures which tend to make people align themselves with the dominant theory

BRASSEUR: Yes, I think that is true. [...] What you see is that everyone is copying everyone else, and so you have a kind of mainstream science going on which is dominated by a few people, and the review process is such that in many cases, if you want to do something which appears completely crazy, or different from what other people are doing, it is very difficult to get support for that. The reviewer will say 'no, no, no, the person isn't doing it the way I want'.

LAHSEN: Is that ego, is it discipline, or is it just out of unawareness?

BRASSEUR: It is because they just believe, you know, everybody just believes that this one [dominant] way is the way to do it. It is a snowball effect. And then you have few people who resist that. And they become very negative; they are out of the club, and usually they become too negative, because they are trying to challenge what the mainstream is. The mainstream is usually right -- ninety percent of the time -- it is just not very open to new ideas. But it is a consensus. You get all these assessment reports that involve all of the community, every two years, and the consensus of everyone is found, with the exception of two or three people. The consensus is very high quality research and science, it is just that it doesn't deal very well with people who may have different ideas. You know, 'no, no, we prove it like this', and ninety percent of the time they may be right, but ten percent of the time they might be wrong. And that can lead to a big discovery every once in a while.

My research suggests that social considerations related to the dominance of the climate change paradigm and its advocates affect some scientists' choice of research topic, scientific argument, and public presentation of their work. Such considerations also affect what articles are accepted for publication, and how the IPCC reports portrays the scientific evidence. For example, the IPCC's receptivity to research probing the role of sunspots in temperature oscillations appears to have been limited, at best. Although one can mostly only speculate, the resistance appears to be rooted in some combination of impulses to maintain environmental

concern, and to protect professional reputations and prevailing view that the effect of oscillations in solar activity are too minor to explain the observed globally averaged temperature rise of .5 or .6 degrees Celsius since industrialization began. At present, the majority view is that the rise in temperature is due to human emissions of greenhouse gases -- the "human fingerprint" in the climate record which some scientists have claimed to have identified with "ninety-five percent" probability (Hasselmann, et al. 1995).

Minority views bear the burden of proof when challenging the majority view. Thus, as the paradigm of human-induced climate change has grown in strength, it is difficult for individual scientists to counter it; doing so in a way that would not simply point out an inconsistency but which challenged the entire theory of human-induced climate change would require -- besides some degree of courage -- a unified theoretical framework. Only presenting snippets of evidence which counter the theory -- for example, demonstrating that a particular, local temperature data set shows no warming trend, or, even, shows a cooling trend -- is not considered a serious challenge. Contrarians along with other critics are often criticized for their practice (or "attempts") of "poking holes" in the dominant theory but without providing an alternative hypothesis which could then be integrated into models and be tested and challenged. Failing to provide a comprehensive, unified alternative theory (to the dominant theory that increases in greenhouse gases will result in warmer temperatures), and going against prevailing scientific opinion, contrarians must suffer attacks on their scientific reputations of a kind they most likely would not experience if they did the same

quality work but without challenging prevailing scientific opinion. As an example of how contrarians' scientific talent can be put down in the public eye, Thomas Wigley is quoted in a newspaper article making the following remark about Patrick Michaels: "He does try to do good science. I don't think he succeeds very well" (Stradling 1997).

Thus, when two Danish scientists identified a correlation between climate (i.e., average temperature over a period of years) and sunspots (i.e., the naturally variable spots on the surface of the sun which affect the amount of heat it radiates), their study was less influential than it might have been because they were unable to also provide a physical mechanism or theory by which to explain the correlation, since the correlation could be coincidental rather than causally-related. Of course, sunspots were part of the counter-evidence frequently advanced by contrarians against the mainstream and the IPCC. The influential 1989 Marshall Institute report by Robert Jastrow, Frederick Seitz, and William Nierenberg claimed to explain the observed temperature rise in entirety in terms of solar-activity changes (Jastrow, Nierenberg and Seitz 1989:29).<sup>72</sup> Similarly, in an 1995 interview with me as well as in his recent book (as early as page seven), *Hot Talk, Cold Science* (Singer 1997:7), Fred Singer referred to the work by Friis-Christiansen and his colleagues. In the latter, Singer described the "striking correlations that have been observed between sunspot cycles and climate,"

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<sup>72</sup> The Marshall Institute authors claim that "[t]he effect of the wild cards introduced into the greenhouse studies by natural and solar variability is that no conclusion about the magnitude of the greenhouse effect in the next century can be drawn from the 0.5 degree Celsius warming that has occurred in the last 100 years. By some accounts by mainstream scientists, the Marshall Institute study used an inaccurate sunspot record (Kellogg 1991:505)



citing a 1991 article by Friis-Christiansen and his colleague, K. Lassen (Lassen and Friis-Christiansen 1995).

In a 1996 article in the Danish newspaper, *Weekendavisen*, Friis-Christiansen expressed his acceptance of the burden of proof with regards to his theory that natural variability in solar radiation explains at least a part of the observed temperature rise, but he also described how the IPCC refused to include among the list of other uncertainties related to the theory of human-induced climate change, the at least *potential* role of changes in sunspots.

Translated from Danish, Friis-Christiansen wrote the following:

After many years of partaking in the assessment efforts in the United Nations' climate panel of about 2,500 researchers, the IPCC, I have considerable knowledge about the demands of proof placed on minority views. As a natural scientist, I find this quite in order, and when I as a member of the Danish delegation participated in the IPCC's meeting in China in January 1992, it was therefore not with the expectation of being able to convince the IPCC about the influence of sunspots on climate. To do so, I needed to have a physical mechanism by which to explain the correlation. However, I did have a reasonable hope that the IPCC would support efforts to research possible mechanisms. For that reason, at the final editorial treatment of the IPCC's 1992 report I proposed, among other things, that the solar effect on climate be included on the list of significant sources of uncertainty, in the same category as other uncertainties related to the effects on climate of greenhouse gases, aerosols, clouds, ocean circulations, icecover, and land surface processes. The IPCC was not of the opinion that the solar influence should be included on this list! (Friis-Christiansen 1996)

This suggests the sensitivity of research which for one reason or another challenges the majority view, including the argument that humans are responsible for the observed rise of .5 or .6 degree Celsius in global temperature

since pre-industrial levels.<sup>73</sup> During the last couple of years, several articles in prestigious scientific journals have acknowledged the work by these and other scientists studying the influence of solar variation in oscillations of the climate, suggesting that the role of the sun might be more important than many previously believed (Kerr 1996).

The potential role of social pressures for scientists to conform to the dominant paradigm of the scientific focus on, and concern about, human-induced climate change is not easily identified and measured. It is clear, however, that certain scientists have developed strong positions on the issue, and that they have thus invested their professional reputations in the theory of significant human-induced climate change. Moreover, some scientists appear motivated by their desire to see action taken to reduce greenhouse gas emissions. The following example shows the intimidating reactions scientists may experience if their research appears biased against environmental concern.

Two scientists published an article about a study they did of the amount of time carbon dioxide remains active in the atmosphere before it decays and loses its heat-trapping effect. (While the two scientists do not identify themselves primarily as “ecologists,” I will refer to them as such here for the sake of

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<sup>73</sup> A few influential scientists have relied on uncertain statistical studies (Hasselmann, et al. 1995; Karl 1995; Santer, et al. 1995; Santer, et al. 1996) to assert with “ninety-five percent certainty” that the observed warming trend is due to human activities rather than natural variability. The climate is inherently variable without the influence of humans, and therefore any detected climate change cannot be automatically concluded to be due to human influence. For example, during the ice ages, long before the industrial revolution where humans began to emit globally significant levels of greenhouse gases, global temperatures were about five degrees Celsius cooler than they are today, with atmospheric CO<sub>2</sub> levels correspondingly lower, estimated at about sixty percent of what they are today (Schneider 1990 (1989):20). Similarly, paleo-records show that periods in the distant past have exhibited temperatures at least as warm

convenience.) Their intent with the article was to point out the importance of the terrestrial biosphere in looking at the atmospheric lifetime of carbon dioxide. The terrestrial biosphere is often left out of the GCM models, and the two ecologists wanted to show the serious scientific consequences of this omission. At the time, they felt that their study was at a stopping point, and that it was therefore a good time to write up their findings. However, at that stopping point, the lifetime of the atmospheric carbon dioxide happened to be low. This meant that their result could be taken to suggest that carbon dioxide wasn't such a problem, since the gases, as depicted in their study, appeared to be absorbed out of the atmosphere relatively fast (once absorbed, the heat-trapping capacity of greenhouse gases is stopped). The two ecologists thought that their decision to stop and publish at a point where the numbers appeared lower than, and more different from, other studies, would work to show the important role of the terrestrial biosphere and other factors within the (relatively) short term, a point often overlooked by GCMs. The ecologists' estimated lifetime of carbon dioxide would have come closer to other studies had they continued their model further forward in time. This is because the curve of atmospheric concentrations of carbon dioxide drops significantly at first when the biosphere is included, but later levels out. Other studies focused more on the fact that the absorption of carbon dioxide levels out after a certain amount of time, suggesting that the gases

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as those currently observed. Although accompanied by reservations as to their reliability, the recent statistical studies and the accompanying claims of certainty had a strong impact in the media.

remained in the atmosphere for a longer period of time where they could exert their heat-trapping effect.

The two ecologists failed to foresee the reactions their article was to provoke among scientists supporting the theory of human-induced climate change. Through Email and other means, a group of scientists circulated, and heatedly criticized, the work by the two ecologists. Suggestive of the partisan dimension of climate politics, the atmosphere in their study was called a “Republican atmosphere.” This was because it suggested that nature was resilient to human activities.<sup>74</sup> Recounting the incident to me, one of the two ecologists who wrote the article said that the “bad reaction” came from people who were concerned about human-induced climate change and about the environment generally; these people feared that the work would fuel a conservative, anti-action agenda. The co-author of the criticized article said that they might have done things differently had they received input from others earlier. He emphasized that without that input, the deciding point for them was that they felt somewhat at a stopping point there, and that their intent was to make a technical point (i.e., the importance of including the terrestrial biosphere in models), not a political point.<sup>75</sup>

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<sup>74</sup> There is a history to this, : during his time in the White House as president Bush's Chief of Staff, John Sununu wanted a climate model installed on his desktop computer. NCAR scientist Warren Washington with this, though of course the model had to be many times more simple than those run on supercomputers. Colleagues of Washington jokingly told him to make the ocean very unabsorbant of carbon dioxide, since the oceans can work as a sink for carbon dioxide which otherwise would remain in the atmosphere as a heat-trapping gas. But Sununu wanted a 'Republican ocean,' one which was very deep and very absorbant of carbon dioxide.

<sup>75</sup> More understanding critics of the article noted that it was a good scientific contribution and that it just didn't give a full picture, and that thus looking only at a relatively short timescale could be misinterpreted. Stuart Gaffin and Brian O'Neill, the Environmental Defense Fund, January 25, 1995, interview.

Some of the contrarians find their scientific reputations severely attacked by mainstream scientists. For example, Patrick Michaels was the subject of the following comment by Thomas Wigley in a recent article in a Virginia newspaper: “[Patrick Michaels] does try to do good science. I don’t think he succeeds very well” (Stradling 1997). In my interviews, I have encountered many similar put-downs of Michaels as well as Balling and Idso. Michaels, Balling and Idso are all empirical climatologists -- “data people” -- and as such are ranked lower within the scientific hierarchy, which places theoreticians highest (mathematicians at the very top, followed by physicists). Climate modelers form an intermediate position within this hierarchy, and there is considerable tension between them and empirical scientists, as also described in the chapter on climate modeling. Numerous climate modelers have made comments to me to the effect that when debating with the top climate modelers, these ‘data collectors’ are simply out of their league.

Challenging dominant theories -- which, of course, are attached to influential theorists -- is more difficult for the less powerful and established groups within international scientific circles. For many Third World scientists, it is a problem simply to make their research known, for a range of reasons including, among other things, (1) the difficulty and expense of gaining access to the internet, (2) the sometimes prohibitive expense of having their local, national or regional journals included in the citation index, and (3) bias among scientists and journal editors in the developed countries against scientists with addresses

outside their part of the world (Gibbs 1995).<sup>76</sup> In the context of challenging dominant theories, an African scientist involved in the IPCC said that if you do good work, it will eventually be recognized, "but if you challenge someone whose authority is respected, someone who is seen as an authority in the field, then it is probably easier to do that if you're from the US" (or from the richer, more industrialized nations of the world generally, I would add).

A recent incident at NCAR suggested how less established scientists are discouraged from interacting with contrarians. Roger Pielke, Jr., a junior social scientist at NCAR had wanted to invite Fred Singer to Boulder in March 1998 to speak in a class he taught at the University of Colorado. Throughout the semester, the class was visited by a number of scientists from the Boulder area, all of them representing the mainstream and/or the IPCC. Singer was thus invited to introduce students to a contrarian's point of view. When Roger Pielke Jr. contacted Singer, Singer requested a speaking engagement at NCAR during that same visit. Pielke managed to arrange this, and soon found himself in the middle of a local controversy. He was contacted by phone by Thomas Wigley, who, as a Senior (i.e., tenured) Scientist at NCAR has considerable power and influence. Wigley made it clear, in no uncertain terms, that NCAR had "no place for someone like [Roger Pielke]!" This statement had some impact on Roger Pielke, who, as a junior, tenure-track scientist, knows the importance of Senior Scientists in deciding on promotions within the organization, as well as the

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<sup>76</sup> Scientists in the developed countries generally assume that the work hasn't been done if it is not circulated on the internet and in the citation index, with the result that work appearing only in local journals

particular power wielded by Thomas Wigley as a key leader within the IPCC.

In the heated exchange around Singer's visit, Wigley made it clear that, in his opinion, Singer should not be given the opportunity to speak at NCAR, because Singer has 'publicly criticized respectable NCAR scientists.' Wigley's anger towards Singer was partly rooted in the controversy over the 1995 report. Ben Santer, who found himself at the center of that controversy when Seitz and Singer singled him out as responsible for the revisions, also happens to be Wigley's former student.

The controversy over Singer's visit ended anti-climatically with the visit itself, which was rather uneventful. It was clear that NCAR scientists didn't need to silence Singer; judging from reactions to Singer's presentation at NCAR that I witnessed, few if any of the scientists who attended his talk were positively impressed by Singer's scientific arguments; no one was converted. Singer presented a highly tentative argument that airplanes might have caused the observed rise in surface temperatures in recent decades. At the discussion in the end, NCAR scientists politely pointed to data sets and to scientific research which accounted for the observed temperature changes, none of which Singer appeared to be familiar with. It thus confirmed many NCAR scientists' perception of Singer as someone who no longer keeps up with the scientific literature in a consistent manner.

### ***Conclusion***

Given their subjection to vituperous attacks by contrarians and public relations campaigns based on debatable missteps, it is understandable that certain scientists on the side of the IPCC have taken on a self-protective stance. Nevertheless, the above-described resistance to scrutiny and challenge, and the examples of intolerance of conflicting views and theories, are not only unfortunate and counter to their claims that the process is unbiased, open and transparent. These examples, and the behavior they involve, also undermine democratic participation and understanding of the IPCC and the theory of human-induced climate change, in so far as they obstruct the public's access to contextual information by which to calibrate competing scientific arguments. This "fortress mentality" also, ultimately, undermines their own authority and legitimacy, and it constitutes precisely the kind of maneuvering which renders them vulnerable to the attacks they seek to avoid.



**Chapter 8.**  
**THE U.S. CLIMATE DEBATE AND STRUGGLES FOR POWER: OLD AND  
 NEW SCIENTIFIC ELITES.**

In this chapter, I focus on one particular group of scientists highly skeptical and publicly outspoken against the theory of human-induced climate change, a group which includes Frederick Seitz. These scientists constitute an older generation of physicists whose status and access to political influence in U.S. society and government grew out of their contribution to the wars of the twentieth century. I show the important influence of these scientists in shaping the U.S. climate debate, suggest some reasons for their influence, and show how their resistance to the climate issue connects with wider social and political forces in U.S. society. Through historical and structural analysis, I show that their position on the issue of human-induced climate change is rooted in important changes in the relationship between science and society during the last half of the twentieth century, which has had profound repercussions for their scientific discipline and for their personal and professional status and influence as scientists.

***Physicists and the postwar “science pact”***

Postwar U.S. science policy was based on the assumption that government support of all science, including basic science of no immediate or

obvious relevance to society, always eventually resulted in social benefits exceeding the initial investment. This policy assumption, in addition to the prestige of scientists due to their role in winning World War II, secured new and high levels of federal funding for science without much accountability, and without demands on scientists to integrate their research with broader policy issues (Guston and Keniston 1994; Pielke 1997). Another important aspect of the postwar social contract with science was the presence of scientists as advisors in the inner circles of policy-making, the U.S. Congress and White House involving scientists centrally in political decisions concerning both science *in* and science *for* policy. By some accounts, the U.S. differs from other countries in the extent of this involvement of scientists in the affairs of the State (Brickman 1979; Golden 1991; Smith 1994:48). In 1957, the science advisory apparatus was formalized with the official appointment of a Presidential science advisor and an advisory committee on science in the White House, important symbols of the social status of science and scientists. During this time, the science-policy interface was dominated by an elite group of physicists involved in defense-related science.

Physicists' role in national security during WWII and the Cold War brought them to power in U.S. science, government and society (Kevles 1995 (1971)). Physicists devised not only the atomic bomb but also a number of other technical innovations, including radar, rockets, and proximity fuses, helping to win WWII, which earned them broad public deference during the postwar decades. Due to their centrality in maintaining American military dominance, physicists assumed

key roles as advisors on defense policy, as trainers of students who joined the weapons laboratories, in addition to carrying out basic research under military contracts. Some physicists acted in support of an end or a slowing of the arms race, but, as Daniel J. Kevles writes, "[w]hichever side they took on issues of arms control and defense, physicists remained honored and empowered because they remained essential in determining the shape and capabilities of American national security" (Kevles 1995 (1971):ix).

A few members within this elite came to form the core of the George C. Marshall Institute, a conservative think tank in Washington, D.C., the George C. Marshall Institute, established in 1984 to influence opinion and policy. While it presents itself as objective and unbiased, Marshall Institute analyses generally favor of unregulated free-market forces, national military defense technology, and nuclear power, and oppose environmental regulation. During the Reagan Administration, the Marshall Institute was concerned to promote Strategic Defense Initiative (SDI, also known as "Star Wars"), but it has since turned to climate as a major focus, forming perhaps the most influential, and certainly the most prestigious, faction among U.S. "contrarians." In the words of Marshall Institute chairman, Frederick Seitz, the Marshall Institute "encourages research on defensive anti-ballistic missiles, on space science, and critical studies of factors that could have a major effect on the environment" (Seitz 1994:384).

Increasingly denied access within the scientific community to the level of status they desire and to which they may feel entitled, contrarians as a group (that is, not only the physicists among them who are affiliated with the Marshall

Institute) have obtained influence and status, as well as resonance for their scientific and political views, through political groups and the media outside the scientific community. In the process, they often circumvent peer-review, at the price of losing considerable scientific credibility in the eyes of many fellow scientists. In the eyes of many mainstream scientists, the credibility and status of contrarian scientists has also been undermined by their willingness to participate in media campaigns by fossil-fuel industry groups to counter concern about human-induced climate change. Many mainstream scientists consider such collaborations problematic, and it has rendered contrarian scientists vulnerable to characterizations as puppets for industry -- or, in the colorful words of journalist Ross Gelbspan, a strong supporter of the IPCC, "interchangeable ornaments on the hood of a high-powered engine of disinformation" (Gelbspan 1995).<sup>77</sup>

Scientists, journalists, politicians, and environmental activists professing concern about human-induced climate change dispute the expertise of the contrarians, generally upholding climate modelers as the primary experts on the issue. As I describe elsewhere in this dissertation, climate modelers are scientists from a variety of fields (including physics) who produce the computer-simulated projections of future climate changes based on dominant understandings of the effect of increased atmospheric concentrations of human-

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<sup>77</sup> There is a tendency among mainstream scientists to automatically dismiss scientific arguments from scientists associated with industry groups. As a mainstream solar physicist explained, "even good scientists are suspicious of, reject, or don't even bother to comment on things if the source looks at all tainted;" "Immediately, their first reaction is: 'oh yeah, he works for a coal company and therefore I won't

emitted heat-trapping “greenhouse gases.” Climate modelers were among the first scientists to promote concern about human-induced climate change, and their social and scientific practices form a primary focus of contrarians’ attacks. The models are faulty, argue contrarian and skeptical scientists, asserting that climate modelers fail to see or publicly admit this. For their part, certain scientists and sympathetic environmental activists, politicians, and journalists, discredit those advancing such criticisms as “pseudo-scientists” and/or non-experts in the field of climate research.<sup>78</sup>

The expertise of the high-profile Marshall Institute physicists on the climate issue is thus often disputed, and the reports they issue through the Institute are not peer-reviewed. As a result, their reports generally aren’t accepted or discussed within the mainstream scientific community; to the extent that they are, this is more often as political, rather than scientific, documents. However, through their past contributions in various fields within physics, the Marshall Institute scientists have enjoyed significant access to government, and a tremendous amount of status both inside and outside the scientific community; they are not captured by opponents’ attempt to reduce all contrarians to lower rank “pseudo-scientists.” Perhaps the most prominent and dominant members of

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even read the article. This can get you in trouble [because in some cases, the industry] side has assembled some very good scientists.”

<sup>78</sup> To name just one example where the “pseudo-scientist” label was applied to contrarians as a whole (and, problematically, to skeptical scientists as well), Secretary of the Interior, Bruce Babbitt referred to critics of concern about human-induced climate change as “pseudo-scientists” in a speaking engagement at the University of Colorado at Boulder, Colorado, September 22, 1997, titled “Global Warming: A Call for Action on Climate Change.” For an example of the boundary-work of mainstream climate scientists seeking to discredit contrarians as experts on the issue of climate change, see the June 25, 1996, op ed to the Wall Street Journal involving the controversy over Chapter 8 of the 1995 IPCC report. In this op ed, IPCC

the Institute are Frederick Seitz, William Nierenberg, and Robert Jastrow.

They are the authors of several reports on climate change put out by the institute, including one in 1989 (Jastrow, Nierenberg and Seitz 1989), described below, which was particularly influential during the Bush Administration. In what follows, I will refer to this group as the "Marshall Institute scientists," even though the Institute also has other affiliated scientists, both physicists and non-physicists (for example, Richard Lindzen, a theoretical meteorologist, is on the Board of the Marshall Institute). S. Fred Singer shares many if not all of the characteristics of the Marshall Institute physicists, and perhaps ought to be included in this chapter's analysis. Like them, Singer received a Ph.D. in physics from an Ivy League school -- more precisely from Princeton University, from where Seitz also received his Ph.D. Moreover, Singer has in some capacity served as advisor to the government, and, according to his vitae posted on the internet, earned a White House Commendation under President Eisenhower for early design of space satellites. However, I am not sufficiently certain about the extent to which Singer can be lumped with the Marshall Institute physicists Fred Seitz, William Nierenberg and Robert Jastrow, for which reason I will focus primarily on the latter. It is my perception that Singer doesn't command the same level of respect from his scientific colleagues that the Marshall Institute physicists do.

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scientists write that "Dr. Seitz is a condensed-matter physicist, not a climate scientist," in this and other ways seeking to delegitimize his claims and expertise on the issue of human-induced climate change.

One reason for that may also be that Singer has come to be perceived as more of a politician than a scientist by mainstream scientists who know of him.<sup>79</sup>

Robert Jastrow, president of the Marshall Institute since 1985, received his Ph.D. in physics in 1948 from Columbia University, immediately after which he had a post in the Princeton Institute of Advanced Study among other places. During the late 1950s and early 1960s, he served as head of the theoretical division at NASA's Goddard Space Flight Center, before becoming director of the Goddard Institute of Space Studies in New York City in 1961, where he remained for twenty years. Frederick Seitz, chairman of the Marshall Institute, received his education at Stanford University (mathematics) and at Princeton University (physics), from where he graduated in 1934. In 1940, Seitz published a book on the theory of solids, an important book in the development of solid state physics and materials physics from which generations of students have since learned their solid state physics, and which served to define the field. Seitz became a Professor of Physics at the University of Illinois in 1949, and was, along with other colleagues, important in bringing about great growth in solid state physics in the department. Illinois became the major center of solid state physics in the U.S.. Seitz served as Physics Department Head at Illinois from 1957 to 1964, and as Dean of the Graduate College and Vice-Chancellor for Research at

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<sup>79</sup> A March 1998 presentation by Singer at the National Center for Atmospheric Research confirmed for many scientists in the audience that Fred Singer isn't keeping up with important scientific developments in climatology; in this talk, Singer sought to establish a connection between emissions from airplanes and the observed temperature increase in the surface temperatures during the last half of the twentieth century. Among other things, Singer was not aware of recent work pointing to the role of changes in ocean oscillations in the observed changes.

Illinois until 1965, at which time he became the first full time President of the National Academy of Sciences. He became President of Rockefeller University in 1968 and served until 1978. Throughout his career, Professor Seitz served on numerous governmental panels and committees, and advised major political figures of the period on important scientific issues. He has made numerous scientific contributions to the understanding of the physics of solids; contributing significantly to the understanding of quantum mechanics, defect properties of solids, radiation damage, color centers, and transport properties of solids.

William Nierenberg, a third member of the Marshall Institute, received his Ph.D. in physics from Columbia University in 1947 (one year before Robert Jastrow graduated from the same department). After a series of professorships at different universities inside and outside the United States, Nierenberg served as director of Scripps Institute of Oceanography and as vice chancellor of marine sciences at the University of California, San Diego. Nierenberg is also a member of numerous prestigious institutions, including the National Academy of Sciences and the American Academy of Arts and Sciences.

The military affiliations of the Marshall Institute scientists can be deduced from examination of their bio-blurbs and arguments in reports put out by the institute, as is their role as nuclear researchers and advocates. The above-mentioned three Marshall Institute scientists were all trained by nuclear physicists and/or have worked in nuclear physics. For example, while a graduate student at Princeton University, Fred Seitz studied under Eugene Wigner, a



renowned nuclear physicist.<sup>80</sup> They have held important advisory roles to the U.S. government and military on nuclear and other defense-related scientific issues: between his post in Princeton's Institute of Advanced Study and his first position at NASA's Goddard Space Flight Center, Jastrow worked as a nuclear physicist in the U.S. Naval Research Lab in Washington (1958-61). Nierenberg worked as a research scientist at the Manhattan Project while in graduate school (1942-45), after which he became a member of the "Mine Advisory Committee at the National Research Council" (54-), "consultant to the Committee on Nuclear Constants" (58-); consultant to the National Security Agency (1958-60) and of the President's Special Project Committee (58-); Assistant Secretary General of Science in NATO (1960-62) and Advisor at large to the Department of State (68-). Nierenberg also enjoyed the prestige of being a member of the National Science Board (72-78); chairman of the National Advisory Committee on Oceans and Atmosphere (71-75); member of the White House Task Force on Oceanography (1969-70) and chairman of NASA's Advisory Council (78-)] (Jacques Cattell Press 1982).

As for Seitz -- who was trained under the prominent nuclear physicist Eugene Wigner -- he was (aside from the above-mentioned functions) Chairman and Vice Chairman of the Defense Science Board in the 1960s; Science Adviser to NATO 1959-60; and member of the Advisory Group associated with the White House Conference on Anticipated Advances in Science and Technology 1975-

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<sup>80</sup> The biography of S. Fred Singer shares important similarities with those of these three scientists: As a graduate student in physics at Princeton in the 1940s, his mentor was John Archibald Wheeler (Singer

76. As he writes in his memoir, Seitz was also in one function or another associated with the President's Scientific Advisory Committee (PSAC) throughout its existence. In addition, he was Chairman of the Naval Research Advisory Committee from 1960-62 (Reed Reference Publishing Company 1994). Seitz is currently also chair of Scientists and Engineers for Secure Energy (SE2), a scientific think-tank funded by the nuclear industry to promote nuclear power (Deal 1993).<sup>81</sup> These extensive affiliations with nuclear research of some of the Marshall Institute scientists suggests that they are part of what anthropologist Charles Schwartz has called the "self-perpetuating clique" of nuclear scientists -- scientists who have dominated the science-government interface in the U.S. for most of the twentieth century (Schwartz 1996:154).<sup>82</sup> It is clear that they have participated in many of the same organizations and panels over the decades.

Of course, the name of the institute also highlights a military person and a period in World history where the U.S. military ruled. As a website summary about the man, George C. Marshall, put it:

It was the Marshall Plan that spread 13 billion of 1946 dollars to rebuild a shattered, starving Europe and preserved half the continent from the Communist steamroller. It was Marshall who was the supreme power in a United States Army that he rearmed, reorganized and directed from a puny shambles to the greatest and most victorious military force in history.

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1997:175), a renowned scientist who wrote "the fundamental paper on the theory of fission" (Kevles 1995 (1971):328). I am not sure whether Fred Singer is on the Board of the Marshall Institute.

<sup>81</sup> In the above two paragraphs, I need to check the source to make sure I write out correctly all the abbreviations, plus find out through more recent sources when the various positions and functions ended.

<sup>82</sup> It must be stressed, however, that the Marshall Institute scientists form a very specific scientific faction within U.S. society, and that they must not be equated with physicists in general, nor with nuclear scientists in general.

This description also points to the political values associated with this historical, military figure after whom the Marshall Institute is named. The political values of the Marshall Institute physicists are conservative in nature, an important reason why they have become so engaged on the anti-greenhouse side, and why the Marshall Institute has obtained funding from right wing foundations. The Marshall Institute is a product and a part of the newly strengthened U.S. right wing. The political arousal of conservative economic elites with very deep pockets concerned by the protest politics of the 1960s and 1970s led to what Sara Diamond calls the “conservative labyrinth.” Philanthropist Richard Mellon Scaife alone spent \$100 million between 1979 and 1981 to build the right wing movement. The resulting network of conservative foundations and level of mobilized financial resources -- what Sara Diamond calls the “conservative labyrinth” (Diamond 1995) -- has no parallel in the liberal camp (Ricci 1993:168). By the early 1980s, the created network included nine foundations, dozens of corporate backers and about seventy major organizations, in categories ranging from military lobbies, electoral vehicles, media watchdogs, and campus outreach (Diamond 1995:205). This network includes the Marshall Institute. In 1996 alone, the Marshall Institute received \$155,000 from Richard Mellon Scaife’s family foundation, the Sarah Scaife Foundation (Sarah Scaife Foundation 1996).

During the 1980s, foreign and military policy above all else captured the right wing's attention. But with the end of the Cold War, the environment

became an important focus, conservatives now seeking to identify the Reds in the Greens.<sup>83</sup> The central principles of the right are threatened by the widespread public perception of crisis in late twentieth century society -- a crisis symbolized by the threat of global warming. These principles relate to the economy and the nation-state in global context (military and diplomatic) and are reflected in preoccupation with protecting "free market" or "libertarian" capitalism, and in promotion of anti-communism and U.S. military might and dominance in the world (Diamond 1995). The U.S. right wing considers the fight against U.S. participation in international agreements to reduce greenhouse gases a matter of "life and death" because such efforts are seen to constitute a "sledgehammer to the economy," as expressed to me by a Director within the libertarian Cato Institute, another think-tank of the "conservative labyrinth" established with the money of Mellon Scaife and other conservative and industry sources.

The right-wing and libertarian frameworks resonate with the discourses of the contrarians as a whole, while it conflicts with new discourses and developments emerging from concern about human-induced climate change in our "risk society" (Beck 1992). The discourses of the contrarians suggest a pronounced faith in science and technology, and in unregulated market-forces, as leading to the betterment of humanity. There is a historical dimension to this

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<sup>83</sup> Many quotes to this effect can be found in *The Greenpeace Guide to Anti-Environmental Organizations* by Carl Deal (1993). For example, Deal quotes the President of the Mountain States Legal Foundation (MSLF), a leading organization in the environmental opposition, according to Deal. MSLF exhibits the anti-Communist rhetoric characteristic of many organizations concerned to counter environmentalist concern. In the words of the MSLF President, William Perry Pendly, "The environmental movement is the last refuge of the Left;" "Because of the collapse of communism, because the wall has come down, because the Soviet Union is no more...the environmental movement is the last refuge of people who favor government over people." (Deal 1993:64)

to the extent that the discourses of the Marshall Institute contrarians -- as of the contrarians as a whole -- tend to evoke worldviews characteristic of modernity prior to the newer, emergent discourses (and implied worldviews) rooted in the protest politics of the 1960s and strengthened by the end of the Cold War. The latter are centrally structured by concern about transboundary and seemingly inherently unpredictable and uncontrollable "new environmental issues" -- issues such as acid rain, ozone depletion, and human-induced climate change, which often are even global in their repercussions and which have been important in recent shifts in definitions of national security to include environmental security (Prins 1991; Hajer 1993). In interviews with me and in public announcements, Seitz, Nierenberg, and Singer (another physicist contrarian of the same generation) have, to various degrees, articulated their modernist faith in "Progress," understood as a universal process of improvement in the human condition resulting from humans' ability to control nature and to develop scientific and technological knowledge and know-how.

The contrarians as a whole tend to support, if not advocate, nuclear technology; Seitz, Nierenberg, and S. Fred Singer (the latter a physicist but, to my knowledge, not a member of the Marshall Institute) have all expressed their support for nuclear technology, their faith that it is or can be made safe, and their criticism of what they consider "irrational" environmentalist impulses against it. As mentioned above, Seitz is also chairman of the nuclear promoting think tank, Scientists and Engineers for Secure Energy (SE2). In the view of the Marshall Institute scientists, we aren't going to see "anything serious happen" in terms of

human-induced climate change for decades if not one or two centuries.<sup>84</sup>

Their general claim that “To date, there is no significant evidence that we’re in impending danger”<sup>85</sup> conflicts with the strong suggestions by other scientists that global warming already is changing global climate,<sup>86</sup> and with widespread environmental concern about it. Although Nierenberg endorses “plan[s] for moving towards alternative energy” -- which is what environmentalists are pushing for -- his time frame for doing such shift, and his support of nuclear energy, positions him at odds with most of U.S. society and the environmental movement. During my interview with him in May 1996, Nierenberg expressed his faith in humans’ ability to make nuclear energy a viable and safe energy option:

I’m sure we can lay out today the problems of nuclear energy disposal, the weakness in the structure of the reactors, and a couple of other problems. In twenty years, we can solve them cold, and in forty years we can have nuclear energy all over the place again. [...] We know what the problems are and they are very specific problems, they happen to be for instance the weaknesses of wells under constant neutron bombardment, you know, in building these things and containing the problem. The waste disposal problem, we could certainly solve those. If we focused on them for twenty years, you’d have nuclear energy to substitute for a hell of a lot of things forty years from now.

All of this places these contrarians in conflict with the environmental movement as a whole.<sup>87</sup> As Hajer writes, in 1970s, the nuclear issue “became the metaphor

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<sup>84</sup> Nierenberg, interview, May 31, 1996

<sup>85</sup> Seitz, interview, December 1, 1995

<sup>86</sup> See for example the testimony by James Hansen in 1988. Hansen started the current wave of concern about human-induced climate change by claiming 99% certainty that human activities played a part in the devastating US drought the summer of that year.

<sup>87</sup> Here as well, Singer exhibits strong similarities with the Marshall Institute physicists. For example, in 1995 Singer wrote: “[N]uclear energy should have a bright future. Reactors, factory-built to a standard design, will reduce cost and increase safety. Uranium is plentiful and cheap, and likely to be so for many decades...” About the only problem seems to be the disposal of spent nuclear fuel. But even here, it is not a technical problem but a political one... [T]he nuclear waste issue is simply a device to shut down atomic reactors... It’s difficult to fathom the logic there; the nuclear power opponents tend to be with the same

for all that was wrong with society” (Hajer 1995:91). The nuclear issue came to form the “logical topic” for what he calls the “survival discourse” associated with Limits to Growth, which also shaped the focus on human-induced climate change

In terms of the nature of the relationship between the Marshall Institute physicists and “hawks” in the climate debate, it is of significance that important hawks in the climate debate -- Carl Sagan and Stephen Schneider, among others -- also promoted concern about nuclear winter and nuclear fall in the early 1980s. In both cases, their claims were based on computer simulations. Using computer models, the authors of an article known as TTAPS (from the initials of the authors names) calculated that the detonation of nuclear bombs during a nuclear war would produce smoke in the atmosphere which would block sunlight from the surface of the earth for months, resulting in plummeting temperatures which would severely undermine the life habitats of plants and humans (Turco, et al. 1984). Cornell University astrophysicist, Carl Sagan, joined by environmental activists, is said to have advocated a reduction in strategic weapons arsenals of 90 to 99 percent in order to get below the threshold of nuclear explosives that could trigger such nuclear winter (Bailey 1993:11-2). Using another, more complex climate model, scientists Stephen Schneider and Starley Thompson, both NCAR scientists at the time, contested the dramatic conclusions of Sagan and the TTAPS authors. While still promoting concern about the issue,

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organizations that .. clamor for a reduction in fossil fuel burning to reduce the emission of carbon dioxide into the atmosphere. Perhaps they cannot reconcile themselves to the fact that the world now has at its

Schneider and Thompson reduced the projected temperature cooling due to nuclear explosions by a factor of two to four, consequently down-grading the threat to a nuclear “fall” rather than “winter” (Bailey 1993:113-5).

***The rise of protest politics and the  
ousting of scientists from the White House***

The above outlined differences have their roots in the 1960s. The rise of protest politics of all sorts during the 1960s gave rise to the peace movement and environmentalism and undermined the public deference and political power to which the physicist elite had grown accustomed. New tensions in science and technology policy in the 1960s and '70s gave rise to the new notion that science and technology, no longer an assumed good, ought to be “assessed.” Earth Day 1970 symbolized and further strengthened the rise in environmental concern, while the establishment of the Congressional Office of Technology Assessment (OTA) in 1973 reflected the new view of science as needing to be controlled and assessed. The protest politics also led to a split within the scientific community between scientists working on the side of military defense and scientists involved in non-defense related science (Smith 1994:45-6). During the same decade (1968-1978), Congress gained new power and grew more involved in all aspects of policy-making, diminishing the power of the executive branch and the existing scientific advisory network. As a whole, these developments worked to

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disposal an essentially inexhaustible energy source, safer and cleaner than anything in the past (Fred Singer, quoted in *Nuclear Energy Insight* September 1995, p.8.



compromise the privileged and uncontested voice in policy-making of the established elite of physicists, rendering science policy more open and contentious, reflective of the complex lines of cleavage in U.S. politics (Smith 1994:49).

Seitz and other Marshall Institute colleagues personally experienced this reduction of their scientific power and privilege. Seitz was associated with the President's Scientific Advisory Committee (PSAC) when President Nixon dissolved the Committee in 1973, ousting all science advisors from the White House due to the tension between certain "peace scientists" in the Committee (PSAC) and the Administration. As Smith writes, "disputes over supersonic transport (SST) and the Antiballistic Missile Treaty (ABM) were only the most visible manifestations of deep-seated political differences among certain PSAC members and consultants-at-large and the Nixon administration" (Smith 1994:48).<sup>88</sup> In his memoir, published in 1994, Seitz strongly criticizes the anti-militarism of the Committee members who provoked this loss of scientific power in the Executive branch of government under Nixon (Seitz 1994:297). The behavior of these members of PSAC, writes Seitz, "did their best to demean any military officer, and, on occasion, even civilian members of the Department of Defense" (Seitz 1994:299). Himself president of the National Academy of Sciences during the rise of popular sentiment against the central institutions of U.S. society, and, at that time, accustomed to unchallenged elite status and

access to political power and scientific funding, Seitz did not approve of this nor many of the other social changes set into motion by the new social movements and politics of the 1960s and 1970s. Throughout these and the following decades up until the present, Seitz has remained a strong defender of basic and defense-related science and a strong critic of what he sees as the 'irrationality' of environmental activists and of recent changes in science and science policy with the rise of new environmental issues and climate modeling. In his memoir, Seitz also mentions the establishment of the OTA (Office of Technology Assessment) in the mid-1960s. He says that due to "the changing spirits of the times," the OTA "soon turned into a rallying hall for the nay-sayers who were available in abundance" -- nay-sayers who brought about an 'imbalance' in the OTA with their emphasis on the "negative side effects" of science and technology (Seitz 1994:385n9).

This same period also involved a relative shift away from physics in favor of the new and important field of biomedical science, which experienced new scientific and technological breakthroughs as well as important federal backing resulting from President Nixon's decision to launch a "War on Cancer." Between 1976 and 1992, the administrations of Carter, Reagan and Bush sought to reshape the remnants of the postwar science pact by recreating a solid framework of research support and reestablishing an important role for science in national defense and economic growth. But too many things had changed. In

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<sup>88</sup> Science advisory functions were not entirely abolished insofar as a research staff established by National Science Foundation took over some of the advisory functions of the old White House science office. But

1991, the Nobel prize winning physicist Leon Lederman issued a report relating the results of an informal survey among physicists. As Daniel L. Kleinman writes: "[Lederman] found that their morale was universally low due to the changing times, their field now suffering from inadequate funding for increasingly expensive and larger scale non-environmental research" (Kleinman 1995:189).

The frustration of physicists engaged in basic science only grew when the Clinton Administration came into power. The multi-billion dollar Superconducting Super Collider (SSC), a particle accelerator for high-energy physics proposed in the early 1980s, symbolized the continuing power and influence of physicists as well as the continued importance given to basic science in U.S. society and science policy. The Superconducting Super Collider came with a price tag of \$4 billion, and it would cost several hundred million dollars a year to operate. The proposal for its construction was considered by Congress beginning in the mid-1980s, and in 1987 President Reagan approved funding for the construction of the Superconducting Super Collider, his Administration seeing many spin-offs from it of interest to the Department of Defense, especially in technologies required by the Strategic Defense Initiative, a pet project of President Reagan (Schwartz 1996:155). But the project was turned down in late 1993 by the Democratic-dominated Congress during the first year of the Clinton-Gore Administration. The defeat of the Superconducting Super Collider was a severe blow to the field of high-energy physics, and it was accompanied by bad times

for the physics community as a whole. Kevles attributes the "sharp change in fortune" for physics in part to recent recession and a sluggish economy, but he attributes it, above all, to the end of the Cold War. The death of the Superconducting Super Collider project symbolized the end of an era for physics in U.S. society, marking the beginning of a new, less privileged relationship between physicists and the federal government (Kevles 1995 (1971):xii).

At the same time, climate scientists enjoyed a relative rise in funding, prestige and access to political power -- for the same reasons that demoted the elite physicists: the end of the Cold War, the rise of concerns about the environment and about the sustainability of industrial societies, and the science policies of the Clinton Administration. This increase in status and funding of the new, interdisciplinary field of climate research is also partly tied to the fact that atmospheric research grew more theoretical with the development of numerical modeling and physics-based methods of weather and climate forecasting. The marked rise in status and funding of climate research (and of climate modeling in particular) conflicts with a continuing (albeit currently changing) tendency within the sciences to consider interdisciplinary work inferior and "less scientific" compared to less applied and narrowly disciplinary research involving fewer simplifications and uncertainties. Some physicists claim that the existence of great uncertainty in such interdisciplinary, environmental research renders it difficult to attract "good scientists" into the field,<sup>89</sup> comments subtly reflecting and

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<sup>89</sup> See for example the comments by Japanese physicists in the *Science* article about why environmental sciences are largely neglected in Japan (Normile 1997), as well as the recognition by a US Atmospheric

reinforcing the assumption among some scientists that the environmental sciences have fewer top-notch scientists in their midst. The same pecking order which traditionally has endowed theoretical mathematicians and physicists with the greatest amount of prestige places atmospheric scientists further below on the scientific totem pole.

### ***Fighting back***

The physicists within the Marshall Institute are not formally trained in climate science, but their physicist subculture encourages them to approach even the most complex scientific problems with confidence, including the climate change issue. As a climate scientist told me about the Marshall Institute scientists:

Jastrow, Seitz, Nierenberg -- the Marshall Institute in general, I know all of these guys. They are all good scientists--[or rather] they *were*: they are all retired, and they have a kind of hubris -- an arrogance, you know [...] Physicists can answer any question quickly. These [global environmental] problems are sort of trivia that can be handled by a good physicist on a Friday afternoon [over] a beer. That is the attitude they have. [...] They downplay the science of any other community. And they are really arrogant.

An IPCC official also pointed out the role of physicists on the contrarian side, making a similar point to that above:

IPCC OFFICIAL: [There is a group of physicists among the contrarians who] feel that they are experts [on the climate issue]. There is a long standing tradition in the physics community that holds that physicists can solve any problem, just by thinking about

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Scientist that many are uneasy with interdisciplinary research because "mediocre science can masquerade in interdisciplinary costume" (Fleagle 1994:52-3).

it. There was a group in the U.S. called JASON. It was in the summer time that they met, [hence the name:] J.A.S.O.N., July, August, September, October, November. These physicists meet down in Southern California, and they were convinced that they could solve any problem. [...] They were convinced they could solve the acid rain problem intellectually. They didn't care about models, and clouds, and other detail. They thought they could do it from first principles of physics. And there is some of that left over.

Another IPCC official pitched in:

SIR JOHN HOUGHTON: You see, there are scientists who have been working at the highest levels in science or governments, who feel as if they can make statements about any scientific area. But what they have to do first is their homework!

Conditioned by their physicist subculture, the Marshall Institute scientists exhibit the subcultural style described as characteristic of their field by Sharon Traweek. In her ethnography of high-energy physicists, Traweek writes that success within the field of physics is won by means of self-assertion and bravado, which may include disdaining the work of others. This is meant and seen within this subculture as an expression of willingness to "expose mediocre work, no matter who has done it":

The desired presentation of self can be characterized as competitive, haughty, and superficially nonconformist [...] One group leader said that to convince others of the validity of one's work one had to have great confidence and be very "aggressive"; he added that one needed a certain "son-of-a-bitchiness" (Traweek 1988:87-8).

In their interviews with me, both Fred Seitz and Bill Nierenberg exhibited this style -- though at the time I did not understand it and remained a bit baffled at what I perceived as a strangely respectful but competitive interaction. Though welcoming and friendly, they both continually challenged me during the interview.

This was an interaction I did not encounter among other scientists I had interviewed. Nierenberg inquired into my mathematical knowledge. He tested whether I 'remembered enough of my math to know that the logarithm of an exponential is linear,' and, after I admitted that I didn't know the calculated effect of CO<sub>2</sub> (based on the logarithm of the concentration), he commented dryly that "well, this is very simple mathematics." In a similar vein, Frederick Seitz questioned me on my foreign language skills. Assuming that I spoke few, if any, foreign languages, Seitz was obviously surprised and pushed a bit off-balance when hearing that I am fluent in Danish and French besides English. Also, when I asked Nierenberg to explain some political dimensions of the production or reception of climate related research to which he hinted, he replied forcefully, challenged me by saying: "that is *your* job!" He exhibited the same tone and style of interaction in the interview segment below, which followed his account of how Thomas Wigley, above-mentioned IPCC lead author and a "hawk" in the climate debate (i.e., a proponent of the view that a human influences on climate has been detected), experienced difficulty finding a publisher for an article he wrote with two colleagues. In the article, eventually published in *Nature* (Wigley, Richels and Edmonds 1996), the authors argued that immediate, drastic changes in energy sources in the near and short term are not as cost-effective as a more gradual change. While warning that their "results should not be interpreted as suggesting a 'do nothing' or 'wait and see' policy," they did suggest that societies have the luxury of some time before needing to make a more aggressive switch towards alternative energies. This argument thus

converges to some extent with arguments characteristic of the Marshall

Institute reports.<sup>90</sup> In our interview, Nierenberg commented:

NIERENBERG: [some people] really tried to block the publication of [Wigley's] paper.

LAHSEN: So how do you explain that?

NIERENBERG: I don't know, *you* explain it! That is *your* job. I'm giving you the facts!

LAHSEN: all right, but I am just interested in...

NIERENBERG: No! You explain it. You explain it. And if you can't, there is *no point* to the whole thing! I think it is quite obvious, but if you don't think it is obvious, you can forget it!

LAHSEN: [calming, slightly bemused tone:] all right, all right...!

NIERENBERG: You see, everything I have given you is a fact. Explaining it is a political question, and you do that; that is your business.

LAHSEN: But I mean, I am trying to understand...

NIERENBERG: I am just giving you the straight facts! They blocked the publication of his paper for almost a whole year.

LAHSEN: Mmm-hm. Because...?

NIERENBERG: That's a fact!

LAHSEN: Right. What I am interested in is this...

NIERENBERG: I mean, *why* would anyone block the publication of his paper?! They didn't block it on *scientific* grounds, I can tell you!

LAHSEN: Right. So it's because they want certain things done, and you don't agree that that is really the rational way...

NIERENBERG: what do you mean -- that what isn't the rational way?

LAHSEN: what they are doing. I mean, I think that many of these people...

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<sup>90</sup> See for instance the above-mentioned 1989 Marshall Institute report by Jastrow, Nierenberg and Seitz.



NIERENBERG: I think that censoring a scientific paper for political reasons is wrong. Period. Now, if you disagree with that, state it!

LAHSEN: No.

NIERENBERG: [With a calmer tone] Okay. They didn't offer scientific reasons for blocking it!

Nierenberg also compared the accomplishments of social scientists with those of physical scientists, arguing that while physical scientists are doing a decent job at addressing the (physical) problems that they are responsible for, social scientists like myself aren't living up to our responsibility of solving the many social problems currently raging in the world:

NIERENBERG: Physical scientists really don't do badly in this world; as problems sort of develop we can cope with them, you know, we find solutions, ideal ones and so on. But the social sciences are lagging; the gap between social science and the problems they are supposed to work on is growing. For the physical scientists, the gap stays the same or is perhaps actually narrowing. Or maybe staying the same. But not getting worse. But in the social sciences, it is hopeless. Social science as a science is totally unable to cope with these developments.

As in the above exchange, feeling on edge and a bit perplexed at our interaction, I didn't have a response to this. I was puzzled that he could think so instrumentally about social problems, myself seeing the latter as a complex interaction between multiple socio-cultural processes and socio-economic structuring. This style of self-presentation and interaction with others is likely to be misunderstood or considered intolerable by some scientists, and to further antagonize the debate between Marshall Institute scientists and climate scientists of different social and scientific backgrounds.

Manifesting their subcultural combative and confident style, the Marshall Institute scientists have not given up bids for socio-political influence, despite their alienation from wider U.S. society since the 1960s. Their involvement in the Marshall Institute is one manifestation of this, as was their central involvement in the controversy over the revisions of Chapter 8 of the 1995 IPCC report described above.

In his editorial letter to *The Wall Street Journal*, Fred Seitz implicitly posed the Marshall Institute as a more credible alternative to the IPCC, when he commented: "Clearly, governments will have to look elsewhere than the IPCC for sound science on climate change." This is precisely what the Bush Administration did. In 1992, when resisting signing the climate treaty (Framework Convention on Climate Change, FCCC) in Brazil, Bush referred to the 1989 Marshall Institute report to justify his position. The Marshall Institute scientists have also been an important source of expertise for Republicans in Congress. Against past traditions of support of environmental protection among Republicans and conservatives in U.S. society (Ehrlich and Ehrlich 1996:21, 254), environmental debate has assumed strong partisan dimensions in recent years. With regards to the issue of global warming in particular, it has assumed a particularly strong partisan dimension since the late 1980s and early 1990s, largely due to the important socio-economic consequences, and to Al Gore's high-profile concern about the climate issue. As a Senator during the Bush administration, Gore led other Democrats in an attack against the Bush Administration's lack of response to the rising scientific and public concern about

global warming, calling global warming “the most serious threat we have ever faced” (Gore 1992:40). The June 1988 testimony of NASA scientist and climate modeler, James Hansen, thrust global warming to the forefront of issues of environmental concern when he asserted “ninety-nine percent” certainty that the unusually warm temperatures recorded in the 1980s were due to the buildup of greenhouse gases, and that rising global temperatures would make extreme heat waves more likely (U.S. Congress 1988). Hansen testified before the U.S. energy committee hearing on global warming during an unusually hot summer resulting in a devastating drought, all of which heightened public reception to his claims.

Republicans played down the issue while Democrats seized upon it. President Bush pointed to gaps in the scientific knowledge and proposed more scientific research, while Democrats pushed for more immediate and direct remedial action. The issue grew even more politically intense and partisan when it was discovered that the Bush Administration had ‘rewritten the science’ to secure support for their policy-rationale. On May 8, 1989, the Office of Management and Budget admitted to having altered Jim Hansen’s Congressional testimony to weaken his conclusion that enough was known about the phenomenon to warrant immediate action (Rowlands 1995:76).

Skepticism also characterized the Bush Administration’s position in the intensifying international negotiations around global warming. Emphasizing scientific disagreement and describing the scientific evidence as insufficient, the Bush Administration justified U.S. policy inaction on the issue, stressing instead

the need for more research. "What we need are facts, the stuff that science is made of," Bush said and proposed a 60 percent increase in spending for climate change research, enabling the 1990 enactment by Congress of the interdisciplinary, multi-agency U.S. Global Change Research Program (USGCRP). The USGCRP was formed to coordinate research related to global change (which includes human-induced climate change) and to reduce scientific uncertainties on the issue by heightening understanding of atmospheric, oceanic, and earth processes. The USGCRP coordinates and supports a wide range of research questions and problems in fundamental research seeking a predictive understanding of Earth-system behavior on time scales of primary human interest. It includes, according to one list, "153 projects in thirty-five major areas of research under seven areas of 'scientific priorities'" (Sarewitz 1996:84-85), and the \$5.7 Billion budget for its first four years alone marks it as one of the largest science programs ever conducted (Pielke 1994:315). The creation of the USGCRP has led to the earmarking of an annual sum of now more than one and a half billion dollars in federal funds for climate change related research, thus guaranteeing support for many scientific projects which either were developed or repackaged to fit into this category. Thus, while not satisfying environmental inclinations towards policy action, Bush's policy choice benefited climate scientists by promoting research over remedial action.

Growing international concern during the late 1980s led to the establishment of the Intergovernmental Panel on Climate Change (IPCC), the international scientific body on climate change under the auspices of the United

Nations' Environmental Program (UNEP) and the World Meteorological Organization (WMO). The IPCC reports provide the scientific basis for international diplomatic negotiations under the FCCC to stabilize, if not reduce, emissions of greenhouse gases. In 1990, the IPCC issued its first assessment report which acknowledged large uncertainties but supported concern about human-induced climate change and outlined possible preventive action. In spite of this new authority on human-induced climate change, President Bush preferred to defer to the scientists of the George C. Marshall Institute. Thus, when asked about the validity of the IPCC's report one week before the Second World Climate Conference in Geneva in November 1990, Bush is reported to have answered: "My scientists are telling me something very different," referring to the 1989 Marshall Institute report (Rowlands 1995:80). Bush's Chief of Staff John Sununu also preferred to defer to the scientific authority of the Marshall Institute when the IPCC issued a report supporting concern for human-induced climate change. Himself an engineer by education, Sununu took a particular interest in the climate issue, even insisting on having a climate model installed on his computer in the White House. The Marshall Institute itself has in self-descriptions prided itself on its influence on the Bush Administration: the 1992 Marshall Institute *Programs and Initiatives* informs its readers that their 1989 report "dominate[d] White House thinking" on climate change during George Bush's presidency, and "provided the foundation for the [Bush] Administration's resistance to scientifically unjustified limits on carbon dioxide emissions" (Atmosphere Alliance 1995:17).

The Bush Administration's moves to divert attention from human-induced climate change, and from environmental issues generally, resulted in highly unfavorable national and international reactions, peaking around the June 1992 Rio Summit. President Bush initially resisted signing the Framework Convention on Global Climate Change and signed only a weaker version of it, insisting on the "removal of unacceptable clauses" (Bromley 1994). Later that same year, Arkansas Democrat Bill Clinton defeated George Bush in general elections. With Al Gore as his Vice President and an emphasis on policy response to global warming, the Clinton Administration brought about a major shift in the national science funding and in the U.S. position in international negotiations. As the U.S. State Department's Undersecretary for Global Affairs under President Clinton put it:

The essential difference between the Clinton Administration and the previous Administration on climate is that we take the science of this issue very seriously and, as a consequence, are developing a national climate change policy to use in playing a leadership role in promoting an effective global response [quoted in Glasser 1995:137)].

The Clinton Administration's national science policy was also shaped by Clinton's choice of Dr. John Gibbons as his Science Advisor. Gibbons was Director of the Congressional Office of Technology Assessment (OTA), in the capacity of which he frequently provided Congressional representatives with scientific input about climate change policy decisions. The 1991 OTA study "Preparing for an Uncertain Climate" constituted an early attempt to link science more directly to policy-making. Whereas the Bush Administration delayed policy action on climate change, stressing the need for more research and

encouraging basic science, the Clinton Administration demanded new levels of policy-relevance and scientific accountability for all science, including climate research. Stressing the need for science to be “assessed” and of use to policy-makers, the Clinton Administration increased and prioritized funding for the socio-economic dimensions of environmental change and the development of science policy tools such as “integrated assessments” (Glasser 1995:136). Clinton has decreased support for basic science and encourages defense research with civilian applications and benefits as well (Kleinman 1995).

Republicans in Congress were eager to regain the majority in Congress in 1994 after the 1992 Democratic gains in the Executive branch of government. When they won the majority in Congress with their “Contract with America,” the environment became an important issue in their agenda. Much of the partisan feuding over the credibility of the science supporting concern about human-induced climate change played itself out in the Subcommittee on Science and Energy (within the House of Representatives’ Science Committee), chaired by the Republican representative from California, Dana Rohrabacher. Rohrabacher is a vocal critic of environmental regulation and of global warming theories. Rohrabacher became chairman in 1994 when Republicans gained their majority in Congress. He commented in a “Statement at Press Conference Energy and Environment Authorization,” on June 7, 1995, that the theory of global warming was “at best unproven, at worst... liberal claptrap” (Applegate 1995). Rohrabacher also said that

trendy science that is proposed up by liberal/left politics rather than good science has cost us billions... We will not permit scare mongers and chicken littles to successfully push federal policies that tax out people into lower standards of living, raise the price of products they buy and regulate them out of a job. No where is scientific nonsense more evident than in global warming programs that are sprinkled throughout the current year budget (Quoted in Brown, Jr. 1996) .

During the winter of 1995-96, Rohrabacher held a series of hearings within his Subcommittee to probe the findings, quality, and societal relevance of U.S. government-funded environmental research, focusing on research related to ozone depletion and human-induced climate change. Rohrabacher and others at the hearings suggested that government-supported scientists were motivated to create or exaggerate problems in order to secure government funding. The hearings focused on whether Congress should continue to fund the U.S. Global Change Research Program. Mainstream scientists were pitted against contrarians at the hearings, which gave equal representation to scientists on both sides of the issue of global warming, thus prominently displaying “contrarian” scientists. Republican staffers on the Science Committee and other groups critical of the theory of human-induced climate change considered the panels “balanced,” while mainstream scientists and other groups concerned about human-induced climate change were appalled at the symmetry. They considered it unrepresentative of the distribution of conflicting scientific opinion on the issue, arguing that critics of the theory of significant human-induced climate change constitute only a tiny group. As I describe elsewhere, a greater amount of mainstream scientists appear to sympathize with at least some of the arguments advanced by contrarians.



***The losing battle against the new paradigm***

In spite of their assertiveness, the Marshall Institute scientists lost their access to the White House after the 1992 elections. With Al Gore as his Vice President and an emphasis on policy response to global warming, the Clinton Administration brought about a major shift in national science funding and (at least rhetorically) in the U.S. position in international negotiations. With the shift in Administrations, the Marshall Institute scientists suddenly found themselves criticized rather than consulted by the top national leaders. Vice President Gore has repeatedly discredited scientists skeptical of the science and concern supporting human-induced climate change, helped by other leaders within the Administration who categorically dismiss all critics as “pseudo-scientists”<sup>91</sup>

Though Seitz and other Marshall Institute scientists still enjoy influence in the U.S., particularly among Republicans in Congress, their ideas, as described above, are in important respects in conflict with dominant views in mainstream U.S. society.<sup>92</sup> Seitz in particular appears to feel at odds with large segments of the society around him. In an interview, Seitz criticized trends in broader society - in science, universities, and government structures alike -- everywhere identifying trends (“political correctness”) which he found incomprehensible and reprehensible. Seitz portrayed U.S. society as a whole as having abandoned standards of “good science,” responsible science funding practices, and correct

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<sup>91</sup> Secretary of the Interior, Bruce Babbitt, September 22, 1997, “Global Warming: A Call for Action on Climate Change,” speaking engagement at the University of Colorado at Boulder, Colorado.

<sup>92</sup> One might also compare the positions of the contrarians with the anthropological exploration by Kempton et al into environmental values in U.S. society. The authors find that environmental values and the

educational curriculum. Besides suggesting that “political correctness” has removed classes on Western Civilization from the course list at Stanford University (of which he is alumni), Seitz asserted that (new generations of) scientists aren’t of the caliber they used to be and that people in governmental agencies fail to support good science, basing their decisions of what to fund on political rather than scientific considerations:

LAHSEN: You brought up the notion of political correctness in the context of [what science is funded]. What is the rationalization that is involved?

SEITZ: all I can do is speculate. The government did not fund much science, especially not in places like, oh, the National Bureau of Standards, or Naval Research laboratory, and government agencies, before W.W.II. Then in W.W.II, because of decisions made by President Roosevelt, the whole scientific community was asked to serve the needs, and we all did. Results were effective enough that government funding began, and at first what you had in the agencies were well-seasoned people who had been through that [and who] had high standards, and the funding was done without much regard to political issues but to the needs of science, in the Office of Naval Research, the Atomic Energy Commission, and then ultimately the National Science Foundation. Now a whole new generation has taken over the operation, and many of them are not of the quality -- they are not as wetted to the scientific traditions in the sense that the older generation were. And if I were guessing, I would guess that that is where our trouble is.

A strong upholder of traditional notions of what constitutes “good” science, Seitz frequently singles out simulation practices for criticism, suspicious of their relationship to reality. In his interview with me, Seitz repeatedly returned to the subject of climate modeling when illustrating what he considers inferior and politicized science.

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precautionary principle pervades American culture, even in social sectors in which one might not expect this to be the case, such as the dry-cleaning industry (Kempton, Boster and Hartley 1995).

[Climate modelers] are enjoying good funding for ingenious experiments with computers. They call them experiments, but they are not tied necessarily to observations out there, in the global world. And of course they would like to see their funding continue, so they find it convenient.

Seitz contrasted modelers with the group of scientists with whom he identifies -- scientists who come out of the "traditional base of science," that is, scientists using the "techniques of science that have been built up with great care and sharpness over centuries;" "scientists who "use observations as [their] base, then combine it with speculation and theory, then see where [they] come out." Seitz finds the practice of modeling inherently not very scientific:

LAHSEN: Okay so again, the science that is being done right now, you say that it is not good science because it is not based enough on observations, right?

SEITZ: That's right

LAHSEN: So, inherently about models, you would say that it is not a very scientific method?

SEITZ: Yes.

Given this construction of simulation practices, combined with his conservative socio-political values and beliefs, it is not surprising that Seitz is critical of the current emphasis on modeling in federal science funding practices, and of the theory of human-induced climate change, which depends most centrally on simulation experiments exploring possible present and future effects of human activities on climate, and which produce projections with the potential to result in governmental regulation.

To Seitz, the trend towards greater accountability of scientists to society and the stress on modeling, and on interdisciplinary and applied science generally, represent not only the end of the postwar social contract with science; it also represents how society, in his view, has changed for the worse, with serious implications for science.

In his eyes, recent social developments towards greater oversight over science and more conditional funding practices, as described above, undermines what he considers to be “good” science. He understands this to be the outcome of federal agencies’ top-down decision-making which increasingly dictates the direction of science and overemphasize applied science. By contrast to such top-down direction of science -- which Seitz describes as frequently guided by “the mood of the times” and by “some sort of attitude” rather than scientific understanding -- Seitz believes that “what a scientist is to investigate ought to be determined by ... what the scientist who is doing [the science] thinks is the right direction.”<sup>93</sup> In his memoir, Seitz criticizes the Congressional emphasis on applied science, which started in the 1960s, believing it to “almost certainly [result in] the production of much mediocre science without any significant improvement in the output of new technology.”

Similar criticism of federal funding practices can be found among some mainstream scientists.<sup>94</sup> Also resonating with the view of some mainstream

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<sup>93</sup> Quote from December 1, 1995 interview.

<sup>94</sup> As political scientists David Guston and Kenneth Keniston write, “most if not all of the current conflicts between government and science have roots in the beginnings of their relationship in the immediate postwar period.” However, Guston and Keniston also qualify the sharp distinction between the funding practices and scientific freedom of the “Golden Age” and today, writing that “Congressional inquiries, the

scientists is the criticism by Seitz and other Marshall Institute scientists of the new role of activism and environmentalism in science; while most mainstream scientists consider themselves to be environmentalists, as express general support for the precautionary principle, many retain the ideal that science be objective, even as this ideal is undermined by increasing evidence of the role of values and beliefs in the production of science. In his memoir, Seitz writes about the tactics of those he labels environmentalist “extremists” -- individuals who “encourage fear of countless hazards that lie on the fringes of the truly demonstrable” and whose claims often are “couched in terms far removed from the traditional, objective language of good science (Seitz 1994:381). He then refers to the now (in)famous quote by Stephen Schneider, the above-mentioned high-profile climate modeler and proponent of concern and action on behalf of human-induced climate change. In a statement which Schneider has since defended as taken out of context, Seitz quotes Stephen Schneider saying that scientists need to capture the public’s imagination by making “simplified, dramatic statements, and make little mention of the doubt we might have. ... Each of us has to decide what the right balance is between being effective and being honest” (1994:382).<sup>95</sup> Marshall Institute scientists, as well as some mainstream scientists, consider many issues of environmental concern irrational, and statements such as that by Schneider only support their perception that scare tactics rather than rational, scientifically-informed examination and

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insistence on relevance, the use of grants and contracts as instruments of policy -- all have a long history” (Guston and Keniston 1994:17)

scientific knowledge have shaped many widespread environmental beliefs in U.S. society. In his two hour interview with me, Seitz disdainfully referred to “political correctness” on three different occasions when explaining the current preoccupation with climate change and the privileging of applied research, including climate research. As Seitz explains it, behind the pressure for political correctness and concern about human-induced climate change are “extremists” who “look for power,” seeking control over society by dictating life-styles rather than allowing people to choose their own:

SEITZ: There is a group of people that look for power, because you gain control over society if you can say that we should live according to this life-style rather than [living according to] a life-style because we think it is the right one.

LAHSEN: They gain control by saying what kind of life-style people should live?

SEITZ: That’s right. What they should do, how they should drive their automobile, how they should heat their houses, whether they should have air-conditioning or not.

Typical of the nature of the exchanges between actors on different sides of this issue, Seitz does not recognize his own advocacy and attempts to influence (control) society. Subscribing to a fundamentally realist understanding of the world, he fails to question his own ability to be impartial. Suggestions to this effect also prevail among mainstream scientists. Few scientists are willing to recognize the connections between their own scientific positions and personal values and beliefs, probably due to the scientific ideal of disinterestedness, but

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<sup>95</sup> Schneider has pointed out that this quote leaves out a last sentence, in which Schneider said that ideally scientists can be both effective and honest in their communication to the public.

also to the fact that scientists lose scientific authority by making such acknowledgments.

Whereas contrarian scientists have found sympathizers among certain factions of meteorologists and among Right Wing groups in the U.S. society, they have obtained less influence outside of the U.S.. Certain contrarians and their supporters seek to establish organizational branches in Europe and to influence policy makers at the international meetings of the IPCC or under the FCCC, but their efforts appear to influence opinion to a much lesser extent in such non-U.S. forums. According to journalist Andrew Rowell, the George C. Marshall Institute intended in 1996 to open a London office to counter concern about human-induced climate change (Rowell 1996: 141). However, the lack of resonance of Marshall Institute discourses in foreign contexts was noticeable in a hearing on climate change in the Canadian Parliament (May 3, 1990) at which Frederick Seitz and J. J. Frelk, the Executive Director of the Marshall Institute, made a presentation to the Standing Committee on Environment of the Canadian Parliament on 3 May, 1990, during hearings on climate change. The following is an excerpt from an internal government report on the hearings:

Members of the Committee were not satisfied by this testimony. Mr. Seitz presented himself as a physicist and a generalist. However, he resorted to personal judgments and deep nationalism: "your Committee is calling [on] extremists who want to alter our way of life" (sic) and again "if we adopt reduction policies, this will result in power station shutdowns and loss of communications." Mr. Frelk tried to minimize the impact of Mr. Seitz' comments.

According to this report, the committee also questioned the sources of funding for the Marshall Institute, and the witnesses refused to give any information, except to say that it was not from government nor industry (Lutes 1997).

Seitz' views are not shared in their entirety by other Marshall Institute scientists -- and the fact that Seitz is more outspoken and categorical in his criticism is perhaps also reflected in the attempts by Frelk, the Marshall Institute Executive Director, to "minimize the impact of Mr. Seitz' comments." For example, it is not clear that William Nierenberg sees a generational aspect to the scientific conflict over human-induced climate change. As former director of Scripps Institute of Oceanography where climate-related research and modeling is carried out, Nierenberg's criticisms of climate modeling were also more subdued in his interview with me. Nierenberg expressed that basic science ought to be better funded and that too much money goes towards modeling. He also pointed out that in some areas, the models "disagree completely," and suggested that the models haven't made significant progress since 1979:

As far as I am concerned, the situation is fairly simple. The science has been simple. I've been in it since 1979. As Chairman of the Academy, I wrote the big report in 1993, the models. A lot of them, then too [i.e., then as now], gave a range for doubling of CO<sub>2</sub> between 1.5-4.5 degrees centigrade. That is practically still the same. You could say, why are they now spending a billion and a half dollars a year?

Nierenberg's criticisms in the interview centered primarily on social pressures within science, society and government that discourage the production and



dissemination of science which does not support concern about human-induced climate change and immediate, preventive measures.<sup>96</sup>

While possibly at some variance with Seitz' views, Nierenberg strongly supports Seitz in public and in his interview with me expressed outrage at (unspecified) disparaging remarks he has heard about Seitz from a (largely unspecified) group he refers to as "them." Readers may recall the following remarks by Nierenberg in the chapter on the controversy about the 1995 IPCC report:

NIERENBERG: You can take somebody extraordinarily distinguished, almost -- maybe the most distinguished living scientist we have. You see the names they call him because of his position. Absolutely extraordinary man!

LAHSEN: Who is this?

NIERENBERG: Fred Seitz. Head of the--president of the National Academy of Sciences, he was. He is probably most centrally responsible for the great strength of our country in solid state physics. His famous book, the Theory of the Solid State [check title], published by McGraw Hill, just spawned a whole generation of scientists in the field. He himself has a terrific scientific record of publications, in a whole variety of fields. He was also president of Rockefeller University, you can go on and on and on. The things they call him are unbelievable. Usually by people who don't know him! [And who] Don't know his background, which is even funnier. You know, a member of the NAS, and also of the Academy of Engineering, American Philosophical Society, the American Academy of Arts and Sciences, all earned for a lot of different reasons too. And so on. And, uh..

LAHSEN: So where does this criticism come..

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<sup>96</sup> He provided the example of a modeler, Thomas Wigley of NCAR (who happens to be one of the most outspoken believers in the reality of human-induced climate change. Cf. Lahsen 1998 [if my Conspiracy paper gets published in time]), who supposedly experienced great difficulty when seeking to publish an article in which he said that societies need not make immediate drastic action on behalf of human-induced climate change.

NIERENBERG: Out of ignorance. Usually, the personal criticism, I realize that these people blow out of their tops, these newspaper reporters come and say, “hey look, there is this article by these four guys”, and they don’t even know who we are.

It is precisely due to his background and credentials that Seitz’ contrarian views carry such weight in public arenas, primarily among persons either sympathetic to, or unaware of, his political views.<sup>97</sup>

As mentioned above, a key commonality between Seitz, Nierenberg and other Marshall Institute scientists is their skepticism regarding the impending reality of human-induced climate change, their support for basic science, and their faith in nuclear technology, upheld by their assumption that humans can control such technologies. This is a crucial point distinguishing these scientists from scientists concerned about human-induced climate change and from broader U.S. society, the latter characterized by concern about human-induced climate change as well as other risks resulting from scientific and technological innovation and know-how (Kempton, Boster and Hartley 1995; Otway 1992). By contrast, the environmental concern and science of the IPCC-affiliated or -supporting mainstream U.S. climate scientists, among other scientific groups, resonates with the broad-based environmental concern in U.S. society documented by scholars. Among them, nuclear energy is rarely proposed as a

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<sup>97</sup> In the earlier chapter on the controversy over the 1995 IPCC report, I demonstrated how Seitz’ charges circulated unscrutinized primarily among persons and media channels sympathetic to a skeptical point of view on the issue of human-induced climate change.

remedy, and, in popular writings, such scientists express their concern about unintended side effects of technology.<sup>98</sup>

High-energy physicists were among the most prominent members of their profession, key figures in national strategic defense and science policymaking, and winners of many of the Nobel prizes awarded in physics to Americans (Kevles 1995 (1971):xi). The fact that, for the first time, three environmental scientists -- chemists, who did important research related to ozone depletion -- won a Nobel Prize in 1995 both reflected, and further secured, the new (albeit contested) status of the environmental sciences within the larger scientific community (Stevens 1995). And it did not go uncriticized: S. Fred Singer soon made public statements to the effect that their Nobel Prize was granted primarily on political grounds, rather than on merit (Singer 1995).

Environmental scientists are now leaders of key scientific institutions, and the privileged knowledge producers within U.S. society. They assume prestigious functions as advisors to the Clinton Administration and leaders of the IPCC, in addition to other prestigious international scientific organizations. For example (and, perhaps, to make it worse, in the eyes of the Marshall Institute scientists), Stephen Schneider enjoys considerable success in U.S. society. He has a prestigious endowed chair in the biology department at Stanford University (of which, to top it off, Seitz is alumni) and he was also among the handful of

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<sup>98</sup> For give just one example, Solar Physicist and former NCAR director, John Firor, writes in his 1990 book *The Changing Atmosphere* that "Our ability to foresee unintended, harmful side effects of a newly introduced technology is severely limited" (p. 105).

scientists summoned to brief President Clinton on climate change during the months prior to the 1997 COP3 (FCCC) meeting in Kyoto.

The establishment of the Intergovernmental Panel on Climate Change has brought prominent climate scientists into new, close interaction with not only national but also *international* political leaders. The direct government involvement with the IPCC process provides the reports with political clout, and IPCC scientists with direct access to policy-makers (Masood 1997). Although it is easy to overstate the influence of science on policy, this is clearly an important source of power. While political leaders still make the final policy decision, in late twentieth Century technocracy, scientific experts increasingly shape at least the deliberative framework within which political leaders must choose.

### ***Conclusion***

Concern about human-induced climate change and the emphasis on simulation practices symbolize many of the changes since the 1960s of which the Marshall Institute scientists don't approve, both inside the scientific community and in the world at large. The physicists of the Marshall Institute are critical of -- if not threatened by -- a new scientific hierarchy and new types of science. Moreover, their environmental views and their technocratic and pro-nuclear values now place them at odds with large parts of U.S. society. The same society that earlier granted them unquestioned funding, status and political influence now often discredits them, distrustful of nuclear power and concerned about the environment. For their part, Clinton officials, environmental activists,

and many scientists reduce the motives of the Marshall Institute scientists to money, calling them “pseudo-scientists,” failing to honor the considerable scientific status some of them enjoy or have enjoyed in the past. Dismayed, appalled, misunderstood and politically conservative, they have lashed back against the tide of change, relying on the aggressively self-promoting subcultural style required for success in their own field.

## Chapter 9. DISCURSIVE AFFINITIES: CONTRARIANS AND THE U.S. RIGHT-WING

“[T]he central preconception of the paranoid style [is] the existence of a vast, insidious, preternaturally effective international conspiratorial network designed to perpetuate acts of the most fiendish character” (Hofstadter 1967:14)

The discourse coalition approach outlined earlier in the dissertation provides a way of analyzing strategic action within a larger socio-cultural, historical and political context, while avoiding simplistic interest explanations. This line of analysis shows how various actors perpetuate or contest a given bias -- without necessarily being conscious of this fact. They may also do so for very different reasons, without necessarily sharing deep values. As a theoretical framework, the discourse coalition approach can explain the simultaneous convergence and divergence within discourse coalitions such as the one between U.S. right-wing groups and contrarian scientists that I am to describe below. While different in important respects, a central theme unifying all of the discourses described below is their portrayal of human-induced climate change as a plot, and the expressed anxiety and suspicion about developments in the international arena having to do with the emergence of new international governmental and other institutional structures. The concluding sections of this final chapter discuss and contest characterizations of scientific proponents of concern about human-induced climate change as “radical” and “extremists.” I describe these scientific actors as “ecological modernists,” based on Maarten

Hajer's description of "environmental modernization" as involving an orientation towards technocracy and limited social reform rather than radical social change. I also describe the one-sidedness of the renditions of each other by contrarians and "hawks," and the ways in which their worldviews diverge, their fears and preoccupations oriented toward different phenomena in a changing world.

### ***The Paranoid Style***

The social construct shared by the following right-wing groups in U.S. society is the perception of a vast, international sinister network of groups and interests trying to undermine American sovereignty and way of life. This is accompanied by strong support for unregulated capitalism and by a tendency to discursively construct countervailing forces as conspiratorial in nature. As such, these discourses exhibit strong characteristics of what Richard Hofstadter calls the "paranoid style". The essay "The Paranoid Style in American Politics" by Richard Hofstadter (Hofstadter 1967), then Professor of American History at Columbia University, outlines the historical precedents of this style. Hofstadter points out that the style is not particular to this time in history, nor to American society, though he suggests the possibility that "certain features of U.S. history have given the paranoid style more scope and force among us than it has had in many other countries of the Western world." Historical examples of movements in U.S. society with strong elements of the paranoid style include the anti-Masonic movement of the 1820s and 1830s, and the anti-Catholic movement.

Inside the U.S., the style is often to be found in the left-wing press, Hofstadter notes. However, writing during the McCarthy era, Hofstadter gives particular weight and attention to the role of the paranoid style within right-wing politics in the U.S. As I showed in the chapter on the conflict around the 1995 IPCC report, the paranoid style is manifest on both sides of the debate about human-induced climate change. However, I have encountered the most extreme forms of it among political groups of the U.S. right-wing described below, particularly fundamentalist Christians, followers of Lyndon LaRouche, and actors within the U.S. Wise Use movement.

Hofstadter outlines three basic elements of right-wing thought:

- (1) The belief that there is a sustained conspiracy to undermine free capitalism and to bring the economy under the direction of the federal government, and to pave the way for socialism and communism. Climatic examples of this were Roosevelt's New Deal and the passage of the income tax amendment to the Constitution in 1913.
- (2) The contention that top government officialdom has been so infiltrated by Communists that American policy, at least since the days leading up to Pearl harbor, has been dominated by sinister men who were shrewdly and consistently selling out American national interests.
- (3) The perception that the country is infused with a network of Communist agents so that the whole apparatus of education, religion, the press, and the mass media are engaged in a common effort to paralyze the resistance of loyal Americans.

In what follows, we will see the relevance of these elements in several U.S. right-wing groups, and how these elements also appear, albeit often in more muted forms, in the discourses of certain key contrarians as well.



***The political and religious U.S. right on internationalism and global warming***

**The Prophecy Club**

With regards to climate change, the most conspiratorial discourses are found among born-again Christian fundamentalists and followers of Lyndon LaRouche (the latter group is described further below). The Prophecy Club airs its radio programs in twenty U.S. states and on three worldwide short-wave radio channels, publishes a bi-monthly newsletter, and sells a long series of tapes spreading their world view based on prophecy. *The Prophecy Club Newsletter* provides a quick introduction to their lines of thinking, which -- conservative, nationalistic, Real Politikal and highly pro-capitalist -- revolve around beliefs that foreign communist forces, usually Russian-led, are planning to destroy the United States to bring about a one world government through international instruments such as the United Nations. This is perceived as a conspiracy by foreigners to undermine the U.S., foreigners having infiltrated the U.S. government and secured co-conspirators there, Republicans and Democrats alike. If it wasn't clear already that this is an undesirable development, public speeches, live and recorded and sold on videos through the Prophecy Club, explain how and why these plans for a one world government are a plot by Satan.

One video advertised in this July/August 1996 issue of the newsletter, a video titled *Saviors of the Earth?*, "reveals" that environmental leaders aren't up to what they say they are up to. In this video, a "Dr. Coffman reveals [that]

environmental leadership is not interested in protecting the environment.”

Reflective of the strongly pro-capitalist framework of the Prophecy Club, Coffman’s video upholds the value of private property and manifests an aversion to “a cascade of new treaties, laws, and regulations.” The expressed belief is that environmental leaders are conspiring with people within the United Nations to confiscate Americans lands and subject the American population to a feudal system in which “95% of all Americans will become agrarian peasants.”

Converging with discourses of the Wise Use movement -- with groups such as ECO described in Chapter 4 -- the video asserts that these forces are colluding to place one half of U.S. land area into wilderness, and that the United States, and the whole world with it, is headed towards catastrophe and inferno. The signs of this are already apparent in recent changes in weather and climate, which are understood as signs of evil forces which will bring the current world to its end, to the final hour in which Christ returns and saves the righteous.

According to the born-again fundamentalism of the Prophecy Club, recent changes in the weather -- along with flooding, earthquakes, and the ozone hole - - are all the work of humans. However, more specifically yet, these changes are the work of evil communist forces operating through the United Nations. The general framework of the Prophecy Club will be described here through excerpts from a taped lecture, available on video through The Prophecy Club, titled *Environmental Warfare: Floods, Tidal Waves, Asteroids, and Signs in the Sun, Moon, and Stars*, by Steven C. Wright.

Lecturing from a podium, the video shows Steven Wright discussing news stories concerning unprecedented temperatures (hot and cold) around the world. He explains, with a heavy Southern accent, the powers at work behind these events:

CBS News, in July 1993, reported record colds [...] Idaho had thirty-nine degrees -- in July, now! Caster, Wyoming, had thirty-nine degrees, South Central City, Wyoming, 20 degrees temperature, ladies and gentlemen -- in July, if you can believe that! [Continues mentioning extreme temperatures in different places]. I want to mention a thing about 120 degree weather. I have said since the early 1970s, that in America we are going to have 130 degree temperatures. [He takes on the voice of some imaginary person: "Oh, come on, Steve, you're extreme, do you believe *everything*?" [in his own voice:] I don't know of anybody that was saying that, but I am sure there are, I just don't know who they are. But, ladies and gentlemen, we're only two degrees away from 130 degree weather. I will not project, if you will, beyond that on my own, except to say that now, I am considering it possible we actually may have 140, 150 degree temperatures, in America. I was right about the rest and I believe I will probably be right about that. [...]

Okay, let's go on here, ... former Secretary of Defense under John Kennedy -- and a man named Robert McNamara, one of the most evil men that ever had any position in this country -- what they have done to this country! And most people don't have a clue about that. He disarmed us, he left us a sitting duck. But that is another subject, but that is also factual. But anyway, he wrote a book around thirty years ago, and he talked about weather pattern modification, and how scientists had so many breakthroughs that he projected -- he saw the day coming, he said, in which we would be able to manipulate weather to bring all the Third World countries down, that tried to refuse communism. Could it be he knew something thirty years ago that most people still don't know? I think so. That has been a self-fulfilling prophesy.

Scientists working with the United Nations are said to have perfected the techniques of weather modification to the extent of now being able to prompt

such calamities.<sup>99</sup> Steven Wright, on a Prophecy Club video-taped lecture, explains:

I want to ask the question, ladies and gentlemen, why are these things [natural disasters, higher temperatures] happening in concert with the talk of a new World Order? You are going to have to tighten your seat belts here and, guard your loins, because I am going to give you some truth. To me, it is not by coincidence that Secretary Mikael Gorbachev introduced something at the end of 1987 to the Soviet People, he called a -- and I quote: "What we are witnessing is a new World Order of communism, which we will never turn off of that path" end of quote. By the way, he and George Bush were like that [he crosses two fingers together], him and George Bush. Is it any wonder that just a few years later in 1990, George Bush went on nationwide television and introduced five times in one speech something he called a New World Order? Do you think it is a coincidence that, after five or six years of Gorbachev introducing that to the Soviet people, we've had the worse natural disasters in America and worldwide in any time of recorded history? That is not coincidentally, ladies and gentlemen. There is more to it than meets the eye, and I will give you some documentation on that...

As I describe in the historical chapters, the superpowers of the world did in the 1970s explore weather modification techniques to serve their interests, including geopolitical interests. Wright's Real Politikal discourse involves an overdetermined view of all events in a world view, part of a conspiratorial world view which leaves no room for chance and incoherence:

Based upon what I have told you so far, isn't it interesting that when you look at earthquakes, it is almost as if it is by design. Isn't it something that earth quakes never hit rural areas, but that the floods do? Earth quakes wouldn't hurt anything except farm crops, there would be no devastation of life. But the major earth quakes in our days and especially since 1988 and 1989, they have been in Tokyo, they happened in new York, they happened in Los Angeles, San Francisco, they hit major metropolis areas. Why? Because that is where the most devastation can take place. But where are all the floods? You know, none of those cities have had

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<sup>99</sup> For an explanation of weather modification, see Chapter 3.

massive flooding, damage from flooding. Where does that happen? Out in the rural farming areas, because why? Because it kills the crops, it affects agriculture. Folks, that is not coincidence, and you're going to know that it's not coincidence by the end of tonight. [...]

Weather update here. 1974, Doctor and Professor Gordon McDonald with Dartmouth College -- notice, not the *National Inquirer*, not *The Globe*, but Dartmouth College -- testified before a Senate Subcommittee concerning weather modification. He also served for the Institute of Geophysics and planetary Physics at UCLA. Quote: "Scientific ability to stir"-- notice: "to stir," to manipulate, hurricanes, where they want them to go. This has been perfected, ladies and gentlemen, it has been going on for decades behind the scenes. Scientific ability to steer earthquakes and manipulate polar ice caps and guide tidal waves and to modify and create earthquakes. He admitted that, and this is a known scientist. November 14, 1977, the Vancouver, Canada, *Providence* newspaper article by a Dr. Andrew McCaulski, I quote: "there is proof the Soviets have been playing havoc with Canadian and United States weather." [...] By the year 2000, which we are very rapidly approaching, just a little over four years, the science could manipulate and alter the jet stream and control the wind and create the patterns in high and low pressure systems. [...]

Okay, November issue of *Reader's Digest*. [He reads the title:] "The United Nations is out of control." I could have told them that ten years ago. But they didn't ask me. According to the *Times* article, October 31st 1995 [issue], [reads]: "Sloppy management of the United Nations. The United Nations celebrated its 50th birthday.." I have a reason for mentioning this. [Reading from the article again]: "The U.N. needs to go on 'a low fat diet.' What started in 1945 as a lofty ideal, staffed by 1500, has expanded to an out of control bureaucracy of 50,000 people" Let me go on here. "Excessive salaries, waste, fraud, and very little accountability"-- I can preach on that but won't. Let's read some things here. Right here [the article] says that the World Health Organization, which is one of the many U.N. agencies, has a scientific committee. It says that right here. I am going to prove to you that I believe [*sic*] that the United Nations is involved in what is going on, including weather pattern modification.

1977. The Soviets are first testing electromagnetic fields and blew a hole in the ozone. I believe some scientists say that, because it is interesting that we never had a hole blown in the ozone until within 72 hours after the Soviets conducted their first major experiment with these [electromagnetic fields]. A few years later, I think around 1981, that time period we suddenly found another smaller hole in the ozone hole, within a few days

after the Soviets had also been testing. A smaller hole; notice, the Soviets are getting better at what they are doing. Going on here, December 14, 1994, CBN News reported a mysterious black-out in ten Western states. In Florida and elsewhere, they are currently testing weapons that cut off the electrical system of automobiles. [They say] the idea is to stop criminals. I say, yeah right! They are going to use that on you and I, ladies and gentlemen. They have the ability to cut the electricity from your automobile if they suspect that you are involved with criminal activity somehow. [...]

March 13, 1981, CBN News: It is known that the Russians are working hard to control the human mind through remote electronic means. We do know that they are bombarding this country with electro-waves. We know that to be a fact.

Prophecy Club discourse, as expressed by Wright above and as manifest in Prophecy Club materials generally, thus fuses the three basic elements of right-wing thought outlined by Hofstadter: the anti-communism, the strong pro-capitalist, anti-government interference values, and the perception that dangerous forces are at work in American institutions, including the government and, in this case, the environmental movement. A central institutional actor in the perceived conspiracy are the United Nations, plotting towards a world government and the dominance of communism.

### **The Cato Institute**

It is difficult to know how many people have adopted the environmental and Green movements as substitutes for national socialism or communism.

Bolch and Lyons (1993); Cato Institute  
publication

Established in the 1970s, the libertarian Cato Institute describes itself as

a public policy research foundation dedicated to broadening the parameters of policy debate to allow consideration of more options that are consistent with the traditional American principles of limited government, individual liberty, and peace (Bolch and Lyons 1993).

The Cato Institute, located in Washington, D.C., fuses elements of The Prophecy Club with what Ulrich Beck calls a “techno-scientific rationality”, that is, a rationality involving a strong, modernist faith in progress and technology, and in humans’ ability to use and control technology towards improvement of “the human condition.” This rationality involves discounting of risks associated with processes of industrialization and technological developments (Beck 1992:xx).

Linking the discourses of the Prophecy Club and the Cato Institute are the three basic elements of right-wing thought outlined by Hofstadter; the anti-communism, the strong pro-capitalist, anti-government interference values, and the perception that dangerous forces are at work in American government and society. But they differ profoundly in their perceptions of environmental threats: Wright with The Prophecy Club believes that environmental changes are happening and assumes them to be more catastrophic than most environmental advocates. However, the threats aren’t about humans relationship with the planet (and each other), as perceived by those subscribing to the paradigm of sustainability. Rather, environmental threats, including climate change, are caused by evil forces which will result in cataclyptic events which, in turn, ultimately will lead to the salvation of the righteous. By contrast, according to Cato Institute thinking, the threats are illusory, figments of the irrational

imagination of environmentalists and other people with communist and fascist tendencies.

In *Apocalypse Not*, a Cato Institute publication, the authors, economist Ben Bolch and chemist Harold Lyons, continually compare the environmental movement with socialism and communism, including Marxism, and associates the environmental movement with 'irrational utopianism' and 'utopian crusades,' and even with certain tendencies of the Reich:

Modern socialist utopian movements, like much of the environmental movement, stress the need to look far into the future (as in the thousand-year Reich) and to impose great present sacrifices to ensure the goodness of the outcome of that distant future (Bolch and Lyons 1993:6-7).

The authors thus discourage, even disparage, long-term visions, and compare the environmental movement to the Reich, due to their shared tendency to assume a long-term vision. They then focus exclusively on scientific inability to predict the future as their basis for rejecting the ethical practice of considering the consequences of one's current actions for future generations -- a practice which, of course, often conflicts with short-term economic gain. Bolch and Lyons emphatically claim that the environmental movement holds great political and monetary power, a power which plays to the interests of politicians, who "quickly learn how to manipulate environmental issues to their own benefit" (Bolch and Lyons 1993:23). The power, political and financial, of the opposing side (their own) is not mentioned.



## EIR

EIR (Executive Intelligence Review) calls itself a "News Service." It is associated with Lyndon LaRouche and publishes reports as well as the quarterly magazine *21st Century: Science and Technology*. EIR's magazine is widely available in local bookstores around the U.S.; like The Prophecy Club, which reaches millions of Americans through their radio shows, videos and newsletter, the influence of the LaRouchites in U.S. society might not be negligible. One of the key editors of both publications is Rogelio Maduro, a science writer and follower of LaRouche. Lyndon LaRouche gained his influence as a Chicago right-wing politician in the 1980s. Before that time, he was associated with the extreme left. Some years back, LaRouche spent several years in prison for tax evasion.<sup>100</sup>

The table of contents of the 1989 EIR Report , *The Greenhouse Effect Hoax: A World Federalist Plot*, provides a quick introduction to the tenets of EIR thoughts, which run along the same lines as The Prophecy Club, with the important exception that the EIR authors do not believe the threat of human-induced climate change to be real. The EIR report includes a preface titled "a secret agenda" and the first chapter is titled "The grand conspiracy," with the subsections: "Has the Bush administration gone green?" "Ecologists and Soviets act in tandem" and "The greenhouse gang follows Gorbachov. The second chapter, titled "What the scientists say," includes a section explaining that "The

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<sup>100</sup> This is obviously a very cursory description of Lyndon LaRouche, one which needs to be deepened.

greenhouse effect is a fraud" and interviews with "leading meteorologists."

These interviews are of mainstream as well as contrarian scientists and includes Kevin Trenberth, an NCAR scientist very active within the IPCC whose public appearances suggest his strong concern about human-induced climate change.<sup>101</sup> The interviews with the scientists are remarkably "normal" compared with the rest of the book, suggestive of how scientists' authority may be evoked even if they don't explicitly connect with, and support, the political arguments being made.

Adhering to the strongly nationalistic framework of us versus them, the type of zero sum nationalism also seen in *The Prophecy Club*, Maduro perceives the manifestations of globalization as signs of collusion with communist powers. Maduro's gaze always singles in on what he perceives as suggestions of connections with socialism and communism, which he further links with environmentalist leaders and movements around the world, including the U.S. New Age movement. Again, convergent themes of Brundtland's and Gorbachov's presentations on the subject of international global environmental security are pointed out, and the agenda of the Brundtland Commission -- also known as the World Commission on Environment and Development (WCED, mandated by the United Nations General Assembly) -- "revealed:"

Couched in Orwellian verbiage about 'sustainable development,' [the agenda of the Brundtland Commission] is to re-shape the international political and legal framework, to impose its utopian fascist world order. [...] Less politely, this is the framework for a global Gestapo of "ecological

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<sup>101</sup> In an interview with me, Kevin Trenberth showed no awareness about this report, nor his inclusion in it.

shock troops" capable of closing down industries, imposing "environmental taxes," etc. anywhere in the world.

According to EIR, The Conservation Foundation was set up for the express purpose of giving a patina of respectability to Adolph Hitler's eugenics policies, and it is said to be run by the leading European oligarchic families; its aim, we are told, is to revive Hitler's policies, but, "because of the obvious political difficulties of this, it uses the cover of 'protecting nature'" (p.29). Current manifestations of this reprehensible eugenic agenda is the environmental movement's concern about the growing world population.<sup>102</sup> Their belief structure ties together their brand of Christianity with a valorization of science and technology, progress, individualism and economic and population growth, and the group poses itself against Hitler, communism and environmentalism, perceiving all of these strands as inter-linked. At other times, however, the followers of LaRouche themselves express anti-Semitic sentiments (Toumey 1996:85).

Judging from more recent publications of *21st Century Science and Technology*, the end of the Cold War has toned down some of their direct linking of environmentalism with Soviet leaders, though the anti-Communist rhetoric persists. They now focus more on their arguments about "neo-colonialism" and "new feudalism," painting the environmentalist movement as a plot by "a small group of the British oligarchy:"

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<sup>102</sup> This appears to be the point where EIR's views connect the religious Right's "right to life" values with the "abundance paradigm," according to which there are no limits to possible growth on the planet.

The overall British objective today, as it has been since the time of the American Revolution, is to obliterate the republican ideas of our Founding Fathers, who created the United States as a nation that would use the most advanced science and technology to develop and industrialize its vast, unexplored territories and create a new world where individuals could develop to their fullest potential.

The article continues: radical environmentalists are shutting down this nation's industrial and agricultural productive capability, and they are "not just a group of misguided Earth-lovers... their marching orders are coming top-down from a small group of oligarchs, centered on the British crown". These oligarchs "created the environmental movement specifically to carry out policies of deindustrialization, depopulation, and destabilization worldwide (Hecht 1995).

### ***The shared story-line with contrarians***

The causal relationships between the arguments advanced by contrarians, industry groups, and the U.S. right-wing, are not easy to map, but it is clear that they share similar a story-line. For example, EIR arguments about eugenics and the perception (or representation) of the great wealth and power enjoyed by the environmental movement resonate with contrarians' arguments. In what follows, I will discuss how the discourses of three prominent contrarians, Hugh Ellsaesser, Patrick Michaels, and S. Fred Singer resonate with those of the right-wing and libertarian groups described above. All three contrarians manifest similar anxiety and conspiratorial rhetoric when describing the links between the global warming issue and globalization.

**Hugh Ellsaesser**

On the list of fifteen people comprising the scientific advisory board in the above mentioned issue of *21st Century*, is the name of Hugh Ellsaesser, a contrarian. Hugh Ellsaesser was an active scientist until his retirement from Lawrence Livermore National Laboratory (LLNL) about a decade ago, with which he retains an official affiliation. Representative of his marginalization at Lawrence Livermore and in the mainstream scientific community as a whole, Ellsaesser sits in an office in a trailer all by himself, as mentioned in Chapter 2.<sup>103</sup> Immediately next to his trailer are the two trailers in which all the other climate researchers at Lawrence Livermore have their offices, side by side. There is very little interaction between Ellsaesser and the other researchers, something for which each side faults the other side. Past attempts have only impressed each side of their inability to communicate and agree with each other. As a result, their interaction on the laboratory's grounds are largely limited to mainstream scientists occasionally finding copies of Ellsaesser's writings in their mail boxes -- placed there by Ellsaesser, presumably -- and to letter writing against each other on the editorial pages of journals and newspapers such as *The New York Times* and *The Wall Street Journal*, as in the case of the controversy over Chapter 8 of the 1995 IPCC report; Ben Santer is also at Lawrence Livermore, occupying one of the trailers next to that containing Ellsaesser's office.

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<sup>103</sup> The climate scientists at LLNL literally have their office in trailers.

As mentioned in the chapter on the controversy over the 1995 report, Ellsaesser has expressed that he is “not too sympathetic with a lot of the ideas LaRouche has.” Nevertheless, Ellsaesser associates with the LaRouchites, serving on the editorial board of their magazine. And there are important convergences between the discourses of Ellsaesser and those of the LaRouchites; they converge around a similar story-line concerning new international structures such as the United Nations, even as diverge in other respects. In an interview with me, Ellsaesser expressed his perception of the ‘global warming scare’ as a plot by which the United Nations. It is his perception that the United Nations, through means of the ideology of political correctness, is trying to take over the U.S. with a “one world government.” Ellsaesser describes this, and the global warming scare, as motivated by sinister, hidden motives and socialist forces.

Segments from interview with Hugh Ellsaesser (November 18, 1996):

LAHSEN: One thing I am puzzled by is -- you called the IPCC report a “political document,” and there is this argument that scientists should be neutral and insulated from the political world...

ELLSAESSER: The first IPCC report came out in 1990. I was one of the people to whom it was sent to be reviewed. And being concerned and, well, concerned, I wrote some forty pages of comment and sent them in. Well, two of those comments that I sent in had to do with the misuse of the word “carbon monoxide” where they meant “carbon dioxide.” And that was never corrected; it came out just the way it was before. Which tells me rather clearly that they never looked at the comments. And yet they listed all these people in the back as reviewers, taking credit for having, well, having assimilated the reviews of the reviewers. Which they had not done. They had not assimilated the reviews of the reviewers. There was just recently a fellow at Colorado State, Roger Pielke Sr., he has just

recently requested the IPCC to remove him from their list of reviewers because he objected to how they [used his name].

LAHSEN: How do you think that happens?

ELLSAESSER: UNEP, which is the mother organization of the IPCC, and the UN, are very interested in a world government. They want a world government which is able to operate independent of a majority or a consensus at the United Nations. They want to be able to levy taxes independent of the others, and to raise armies independent, so that they do not have to be beholden to a majority within the United Nations. And, one of the ways in which they can hope to move in that direction is through these problems like global warming and the ozone problem, because those problems as presently visualized will result in transfers of trillions of dollars from the developed world to the undeveloped world. And the United Nations, of course, is the organization that is set up to funnel the money from one to the other. So, there is tremendous pressure on them to see to it that these problems are seen as problems, as global problems. And not to look for any benefits that might occur. That goes down through the people writing the IPCC reports. Bolin, Houghton, Watson...

LAHSEN: So you believe that these people high-up in the IPCC and the U.N. have this political agenda?

ELLSAESSER: Yes. And then there is the political correctness which works its way down. You have to conform to the ideas that they have, you need to accept them, or you have to withdraw. [...]

LAHSEN: So, this world government, what is giving you that perception? What do you base it on?

ELLSAESSER: I base it on my own reading of what I have seen, but I don't know if you have heard of ECO? E-C-O [the Environmental Conservation Organization]. They have just recently devoted an entire issue [of their newsletter] to this particular problem which they had researched and developed at an international meeting about three months ago. World government is being pushed, and is being almost forced down the throats of individual countries, including the United States.

LAHSEN: Is that what ECO concludes, or is that what you conclude?

ELLSAESSER: ECO has documented what I felt was true. Until they [documented it], it wasn't very recognized. But since they have come out

with this research volume, I have been expressing my feelings about this more.

LAHSEN: And who is ECO, do you know?

ELLSAESSER: [goes to his file cabinet to look it up]. Just a second here. ECO stands for "Environmental Conservation Organization".

LAHSEN: Do you know who sends it out, who it is by?

ELLSAESSER: Yes, Henry Lamb sends it out

LAHSEN: [reading what he has pulled out to answer my questions:] "National Association of Landowners working together to protect property rights and economic opportunity, while promoting responsible environmental stewardship." So this is part of the wise-use movement?

ELLSAESSER: Well, it is -- I suspect it is part of wise-use.

### **Patrick Michaels**

Patrick Michaels exemplifies how scientists become linked with powerful conservative and industrial groups through their shared story-line, as described in Chapter 4. Michaels perceives dangerous, ulterior agendas behind the facade of international treaties, supported by the media and by high-ranking government officials. The danger, in his view, is that these new frameworks undermine U.S. sovereignty, its national and economic interests, its future and well-being.

Patrick Michaels is one of the youngest contrarians (born 1950). Besides his position as associate professor of environmental sciences at the University of Virginia and senior fellow in environmental studies at the Cato Institute. The CATO Institute sponsored his book, targeted for a general audience, titled *Sound and Fury: The Science and Politics of Global Warming* (1992).



In his newsletter and other forums that are not strictly scientific, Michaels reaches out to a popular audience. His newsletter is written in highly polemical and popular language. The newsletter includes scientific arguments and facts, but blends these with political commentaries criticizing the scientific mainstream as manipulative, deceptive and politically motivated (one headline, for example, reads: "Conspiracy, Consensus, or Correlation," and associates key hawks in the debate with non-experts (often referring to Hollywood stars involvement on that side of the debate) and with Communism (cf. for example "MacLaine, Sagan, Gorbachev team up to save the planet", in which Michaels derides and describes how Carl Sagan, actress Shirley MacLaine ("who espouses astrology and channeling") recently had featured in Mikhail Gorbachev's first "'State of the World' conclave" in San Francisco (*World Climate Report* 1995: 1(5):3)

Michaels also uses his newsletter to advocate. Among other things, he advocates greater industry funding of research in favor of the current situation of exclusive government funding of climate research. The libertarian philosophy underpinning Michaels' scientific views is more than suggested in titles of his newsletter such as "Free science, free markets." In a telephone conversation with me prior to our October 1995 interview, Michaels warned me not to mistake the chicken for the egg, as he notes Easterbrook to have done in *A Moment on Earth* when Easterbrook refers to Stephen Schneider and Jim Hansen as liberals and to Patrick Michaels and other contrarians as conservatives. He described

himself as a former liberal who was active in the environmental organization, the Sierra Club, around the Three Island incident, but that he pulled away from it in the late 70s when he saw “what liberal friends did, how they distorted the facts to get at the nuclear industry.”

In the interview with me, Michaels explained that the whole focus on greenhouse warming started in the 1970s with a textbook and a paper by James Hansen which, among other things, artificially changed the albedo of clouds, thereby increasing the level of warming calculated to result from a doubling of atmospheric concentrations of carbon dioxide from preindustrial levels

MICHAELS: The textbook that fed everybody has a solar constant off by 80 watts per meter square to begin with. It is 100 watts per square meter more than what it is supposed to be. And they have artificially changed the albedo of high clouds. Anybody will look at that and say: ‘something is wrong here. Why is this paper being published? What agenda is being served?’ Why does a model -- in which the basic parameter is off by eight percent -- receive so much attention? A very good question. And then, at that point, tremendous amounts of money began to be thrown at global change modeling.

Another one of the.. clear snowy days, if you will, was mid October 1983 when two fellows from the EPA -- John Hoffman and another guy -- somehow commandeered the evening news so well that they were the top story. They said that global warming would be apparent and disastrous within a decade, that sea levels were going to rise by -- oh some ridiculous amount -- and this would be obvious, etc. etc. At that point, when people were doing that, I presumed that they knew full well that the Northern hemisphere temperatures were doing nothing for the last 45 years, while they were saying that. People who would do that, clearly, to me, were in the process of beginning what I considered to be a considerable distortion.

Some of Michaels’ criticisms resonate with points I have made above (see the historical chapters) in terms of certain interests behind the focus on human-induced climate change. My point here is to bring into focus the ways in which

Michaels' discourse meshes with the conspiratorial imagination of the U.S.

right-wing, including fears that the United Nations will undermine U.S.

sovereignty and drain it for resources in favor of the poorer countries of the world.

Michaels' conspiratorial imagination is reflected in his suggestion that the EPA officials managed to "commandeer the evening news;" another characteristic of the paranoid style, as described by Hofstadter, is that "very often, the enemy is held to possess some especially effective source of power: he controls the press; he directs the public mind through 'managed news'; he has unlimited funds..." (Hofstadter 1967:32). Michaels explained:

MICHAELS: But public choice theory was operating here. Do you know Public Choice Theory? The idea that if you have monopoly in science funding, that the recipients, the beneficiaries of the monopoly will do anything they can to make sure that they continue to be the beneficiaries. And in this case we do have monopoly science. We have a federal government.

And throughout this entire process, working his way into the nexus of the funding, was a man who believed that he was put on this planet to save it. The man, then Congressman, then Senator, now Vice President -- Al Gore. And he got himself into a position where he was the oversight for the chair of the Subcommittee on Science, [Space?] and Technology in the Senate, which oversees the National Science Foundation's money. And he actually got into the political oversight. And I -- you know, it is not a cynic, it is a logician who will say that 'oh my god, this issue is going to be horribly distorted.' And it has been. The bottom line on the issue of global warming is simple: there was something wrong in the initial forecast ...

LAHSEN: So what is driving current temperature changes?

MICHAELS: What temperature changes? 1994, it was the recession of Mount Pinatubo. But yes, the greenhouse effect is warming the planet. We [Michaels and other skeptics whom he associates himself with] have

said that, in each one of these lectures, we say that the greenhouse effect has something to do with the warming, and it will probably continue to. The issue is not whether it exists. Whether it exists is irrelevant! -- if it is not large. And it's not primarily a summer day time phenomenon.

LAHSEN: And what makes you assume that it won't be large?

MICHAELS: Because of what has happened in the last 100 years. I mean, the IPCC 1992 report was developed for one specific reason: to provide scientific backing for the Rio treaty. That is why; I am not kidding.

The following exchange took place in the context of suggestions on the part of Michaels that mainstream scientists didn't acknowledge what Michaels considers clear evidence that the model that "works best" shows no more than 1.3 degree temperature with a doubling of CO<sub>2</sub> once everything, including plants' absorption of CO<sub>2</sub> is taken into consideration:

MICHAELS: So -- I mean, is no one seeing this? Are people that stupid? Are people with IQs of 180 really that ill-read? No. They can't be. Anyone that would make that argument would have to have an ulterior motive.

LAHSEN: And you think it is just money?

MICHAELS: No! I think it is the level of personal bias. There was a very interesting event last week. I was in London to give a talk to the Institute of Economic Affairs. You know them? [...] One issue they were looking at was global warming. They set up a panel in the afternoon. [...] [My debating partner] was a guy from a group called the Global Climate Institute, by the name of Aubrey Mayer. There was some paper that I submitted for the conference, and Mayer held it up and said: 'listen what this paper said, Mayer said' -- I am answering to your question as to why -- 'It says, this treaty [Rio] is unprecedented in its ability to dictate the domestic energy policy of the signatories, and represents a notable transfer of national sovereignty to international authority.' I thought 'oh my god, here comes the 'see this guy is paranoid' stuff'. But Mayer said: 'that is exactly correct. That is what the treaty is designed to do; to allow the UN to dictate the policies of

sovereign nations.' [he takes on a serious, low voice when he says this]. And then it said [he looks in the paper] that this treaty is primarily a vehicle for transferring the wealth from the producer nations to the non-producer nations. And again, I look at Mayer [expecting denial or attack], and Mayer said: "That's correct."

Yes, you were asking, why is the other side doing what it is doing. Are they being dishonest, or what is their motivation. Well, their motivation -- there are multiple motivations, on any issue. There's [the fact that] everyone gets to go around and have a good time and get research money. How many times have I picked up a phone and been told by scientists "Hey, we know. You're right. But it has to be good for research." I mean -- I -- we could have the tape recorder running and I could call someone who just won a MacArthur fellowship who would tell me that. ... That is part one.

Part two -- I think the Aubrey Mayers of the world are very candid. This is a vehicle to affect a policy, a policy that in their model of the world helps to equalize the unevenness of wealth distribution, and does so from an international authority. I was surprised because I thought this sentence of this paper was a little hot -- basically it said that the treaty said that even if the net benefits of carbon dioxide are positive, if you added up all the costs and all the benefits, if a few nations felt that they were negatively impacted -- even though everybody else was doing better, they could use the treaty to demand emissions reductions. In other words, this treaty, it not only transfers wealth, but it increases the authority of individual nations in a clearly non-majoritarian fashion. This is a very, very serious treaty. And, you know, not a lot of people have read it. Those who have are *very* concerned. I am!

The focus on the United Nations and the theme of hidden and sometimes not so hidden agendas also appears frequently in Patrick Michael's journal. A 1993 article in the journal, suggestively titled "Conspiracy, Consensus, or Correlation?," argues that there is no scientific consensus on climate change and that the IPCC's small steering group represents the science in ways that misleads its audiences by overblowing the possible negative effects of any possible climate changes and downplaying uncertainties in the science. In the Fall 1994 issue, Michaels had two colleagues review the 1994 IPCC

supplementary report, *Radiative Forcing of Climate Change* which was scheduled for publication in 1995. Professor Robert Davis (University of Virginia) found that the body of the report presented a more balanced view than the more widely read summaries of the report, and raised suspicion concerning the motivations of the IPCC leaders who drafted the summaries;

The Executive Summary bears little relation to the individual chapters of the report in either substance or tone. In reading the executive summary, one is left with the impression that the writer(s) did not really care about what the science panels wrote for each chapter, so they shaded their conclusions to reflect their own purposes. [...] With these kinds of inconsistencies, one must wonder if the report is meant to serve as a scientific or political document.

Davis expressed concern, given the uncertainties in the science, about a section in one of the chapters of the report which ranks countries according to the level of their emissions of greenhouse gases:

the section...includes the ranking of emissions by country, the 'trading' of emissions (or removal) of various greenhouse gases to remain within imposed quotas, and the comparison of the 'merits' of various fossil fuels. It thus becomes clear that these indices could ultimately be used as the basis for a global redistribution of wealth from first-world, industrial economies with high Global Warming Potentials (GWPs) to nations with low GWPs.

Patrick Michaels variously suggests that the workings of these plots are hidden and open, and he can't quite decide which is more outrageous. In the same issue mentioned above is an article on Agenda 21, part of the United Nation's Framework Climate Convention attempting to bring about international agreements to reduce emissions of greenhouse gases. The *World Climate Review* article starts with an excerpt from Agenda 21 (section 1.4) which states

that the objectives of Agenda 21 "will require a substantial flow [\$600 billion] of new and additional financial resources to developing countries," and then emphasizes the importance of the climate issue to support this: "without the climate issue to drive the process, there would be no Agenda 21." Appealing to American traditional isolationist tendencies, to nationalistic sentiments, and to Americans' general aversion to taxes, the article calculates the cost to American citizens:

The aforementioned \$600 billion becomes \$6 trillion, or the U.S. GNP, when extended out to eight years...Further, the developed nations (i.e. those with large GNP and high average personal income) are supposed to agree to send over 0.7% of their GNP to the developing nations so they can develop sustainability (whatever that means), which works out to \$42 billion/year from the good ole USA in current dollars (8)

"Many of the Agenda goals are laudable," the article grants, but with expressed belief in progress and in the virtue of free enterprise, the article suggests that the concern about poverty voiced by writers and signers of Agenda 21 is contradicted by its concern to raise costs to include environmental considerations: "Whatever it is, sustainable development will result in fewer people buying fewer things. That's a heck of a way to fight poverty."

Michaels, then, does not subscribe to sustainability but to continued economic growth. This suggests Hajer's theoretical framework of conflicting discourse coalitions differently positioned with respect to ecological modernization, a key story-line of which is sustainability. Michaels does not subscribe to the ecological modernist story-line which supports sustainability,

foregrounding moral and economic justification for environmental safeguarding and preventive action. Rather, Michaels foregrounds the problem of poverty, and proposes continued economic growth and consumerism as the solution, an argument in conflict with discourses supporting sustainability, based on the assumption of limited global resources.

### **S. Fred Singer**

In important respects, the story-lines above resonate with the arguments advanced by S. Fred Singer, as we already saw examples of in Chapter 2 on the controversy over the 1995 report.

Fred Singer received his Ph.D. from Princeton in the 1940s. He worked as a junior physicist at the Applied Physics laboratory of Johns Hopkins University, and, in 1953, joined the physics faculty at the University of Maryland. From 1964 to 1967, he served as Dean of the new School of Environmental and Planetary Sciences at the University of Miami. Singer developed rocket and satellite technology during this time, designing the first satellite instrument for measuring atmospheric ozone, and also served the U.S. government in various capacities. In 1962, he was asked by the Department of Commerce to head the activities that started the U.S. Weather Satellite Service. In 1967, Singer reentered government as Deputy Assistant Secretary of the Interior for Water Quality and Research, thereby leaving behind his previous planetary research in favor of (in his words) research “more down to Earth” (Singer 1997:175). In 1970, Singer



became EPA Deputy Assistant Administrator for policy. During this time, however, he claims to have spent most of his time as chairman of an intergovernmental working group on the environmental effects of supersonic transport (SST). This exposed him to the conflicting claims about the environmental impact of SST, which puzzled him. Singer identifies this as the first time he became what he calls an “environmental contrarian” (Singer 1997:175). The reason was that he came up with a finding that was unpopular within scientific circles at the time, yet one which he describes as a “significant contribution to stratospheric science.” As he writes:

I figured out that human production of methane (from such innocuous activities as cattle raising and rice growing, among others) would about equal the natural sources and therefore lead to an increasing methane concentration in the troposphere. Aside from being a greenhouse gas, because of its long lifetime methane could percolate into the stratosphere, there to participate in photochemical reactions that would lead both to the creation of water vapor and the destruction of ozone. In fact, methane should be the major source of stratospheric water vapor. I still remember that when I submitted this paper to *Science*, it was turned down. The referee identified himself as my good friend Julius London; he advised me not to publish such a paper if I wanted to maintain my scientific reputation. (This was at a time when there was much ideological strife about SSTs, and anyone who suggested that human activities were already putting as much water vapor in the stratosphere as a future SST fleet was not very popular. I suppose that’s when I first became an environmental contrarian.) Anyway, *Nature* accepted my paper in 1971 (Singer 1997:175).

Singer writes that his government experience led him to be interested in the policy implications of science. From 1971 to 1984, at the University of Virginia, and from 1984 to 1987 at George Mason University, he increasingly involved himself with issues related to the environment and energy policy, focusing on such subjects as oil economics, natural resources adequacy, and

the effects of population (Singer 1997:175). Since then, as the only one among the contrarians, Singer has left the academic world altogether before retirement age, in favor of political engagement as a scientist on the contrarian side. His shift from being an active scientist to someone more engaged with the science-politics interface is reflected in his publication record: In the 1970s, Singer published actively in the scientific literature on ozone and other global environmental issues. Since then, a literature review (undertaken by the staff of representative George Brown, Jr., Ranking Democratic Member of the Democratic Caucus of the Committee on Science in the U.S. House of Representatives) suggests that Singer published only one recent peer-reviewed article on ozone depletion.<sup>104</sup> He is not considered an “active” scientist by mainstream scientists, nor one of the mainstream. This is often used against him in the boundary-work of critical scientists, science bureaucrats, politicians, and journalists.

Some mainstream scientists familiar with Fred Singer have expressed puzzlement at this move by Singer away from the mainstream scientific community in favor of what the prevailing scientific culture considers the less respectable role as a political advocate associating with industry- and right-wing political groups. In 1990 Singer established the Science and Environmental Policy Project (SEPP) (included in the *Greenpeace Guide* to “anti-environmental

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<sup>104</sup> In a 1995 Congressional hearing on ozone depletion, Singer testified that he had also published peer-review articles in *EOS* and *Technology: The Journal of the Franklin Institute*. In his report on congressional politics concerning climate change, Representative George Brown writes that neither *EOS* nor *Technology* are traditional journals, and that what Singer has published in them wasn't peer-reviewed in any standard sense of the term.

organizations"). Literature from SEPP describes it as a Washington based affiliate of the Institute for Contemporary Studies that monitors how scientific data is used in formulating federal environmental policy. SEPP receives funding primarily from wealthy private individuals, I was told in an interview with Singer and his wife, Candace Crandall, who is Executive Vice President of SEPP. Prior to joining the Project in 1990, Crandall was, for three years, head of communications and press relations at the Center for Strategic and International Studies, Washington's largest defense and foreign policy think tank. She also served as managing editor for publications produced for the Royal Embassy of Saudi Arabia. Crandall explained that most of the support for SEPP comes from private sources which make "small donations" of about \$2500 each. In the past, Fred Singer has also received money from oil companies to carry out his advocacy work on the contrarian side. Through SEPP, Singer is active and highly visible in the popular press, as described above; he frequently writes articles, letters to the editor and press releases. A 1995 self-promoting pamphlet from SEPP expresses with pride that it has been cited "hundreds of times over the last two years," on having been requested to testify on Capitol Hill, and on having helped plan ECO's (the Environmental Conservation Organization) first annual Congress and serving as "ECO's only scientific member." The SEPP pamphlet describes ECO as "a coalition of some 250 organizations representing seven million U.S. households," and presents this as a sign that "the general public is becoming involved." By contrast -- and for readers to interpret as they please --

*The Greenpeace Guide* describes ECO as “a front group for real estate developers and other businesses opposed to wetlands regulations” (Deal 1993)

SEPP donors, Singer and Crandall explained, are often against regulation, as are Singer and Crandall. The arguments put forth by Singer and Crandall evoke a story-line similar to that of the other actors of the U.S. right-wing described above (including libertarians). For example, Singer often suggests that international treaties on behalf of human-induced climate change - under coordination of the United Nations -- are pushed forward by covert communist or socialist forces intent to undermine capitalism. Readers might recall my discussion of this in the chapter on the controversy about the 1995 IPCC report. In that chapter, I referred to an article by Singer titled "Global Warming: do we know enough to act?" In this article, Singer writes on "the hidden-agenda problem" asking: "Why do so many different groups focus on greenhouse warming? Because the issue provides a wonderful excuse for doing things that they already want to do, under the guise of saving the planet." Evoking anti-communist sentiments, Singer warns against the danger he perceives from the involvement of groups who, after the collapse of socialism in Eastern Europe, have attached their now “hidden political agenda” to the climate issue, in the process seeking to undermine “business, the free market, and the capitalistic system.” Singer suggests that such groups seek to realize their ulterior, communist desires through the issue of global warming, using it as a vehicle for international action, "preferably with lots of treaties and protocols to

control CO<sub>2</sub> or perhaps even methane," and to use it as "a launch platform for an ambitious foreign aid program." (Singer 1991:45-46).

### ***Ecological modernization***

In their rhetoric, contrarians often closely align scientific actors advocating concern about human-induced climate change with ideologies and activities on the radical Left, in part because the latter support international frameworks such as the United Nations. In the following sections, I will point out that contrary to such portrayals, scientists advocating concern about human-caused climate change tend to adhere to an ecological modernist framework which seeks reform but not radical social change.

In his memoir, Frederick Seitz suggests that Stephen Schneider is a radical environmental "extremist" (Seitz 1994:381-2);<sup>105</sup> Singer portrays himself as a defender of science in the face of a collusion between mainstream scientists and government officials with their "army of activist allies" who make "unfounded claims of impending calamity" (Science & Environmental Policy Project 1997). In other writings, e.g., the Leipzig Declaration (see Chapter 7), Singer suggests that the IPCC subscribes to, and promotes, a vision of "climate catastrophe," and that it advocates "hasty action." However, while mainstream scientists as a whole tend to be environmentally concerned and liberal politically,

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<sup>105</sup> I base this claim on the fact that Seitz reproduces an infamous quote by Schneider -- albeit without using Schneider's name -- in a section of his memoir titled "the extremists." This section describes what Seitz portrays as "[s]ome of the most radically extreme attitudes" of experts, whose expertise he questions by placing the word in

in their activities as scientists, they often exhibit many of the same technocratic tendencies of contrarians. While Beck and others have pointed out that environmental risks can call into question the central institutions of late twentieth societies, as a whole, mainstream scientists do not fundamentally challenge status quo. Rather, they adhere to a brand of environmentalism identified by Maarten Hajer as “ecological modernization” (Hajer 1995).

“Ecological modernization” is the term Maarten Hajer has developed to describe a dominant strand of environmentalism, one which I argue characterizes the discourses and orientations of IPCC and mainstream scientists in general. While new environmental problems have the potential to “cast doubt on the social basis of the central institutions of late twentieth societies, including science, the legal system, representational political institutions, and the market economy” (Hajer 1995:37),<sup>106</sup> ecological modernization seeks to reform but not fundamental change extant social structures. As Hajer points out, environmentalism is not an internally consistent body of ideas, nor a set of beliefs held by a uniform and easily identifiable group of actors. Rather, environmentalism is a complex, internally inconsistent set of beliefs involving a great heterogeneity of approaches, assumptions, actors, interests, and discourses. Analyses identifying environmentalism with radical social critique fail to acknowledge that it was not only the counter-culture or the middle-class which

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quotation marks, whereafter he quotes Schneider as an example of such attitudes and behavior which he constructs as corruptive of the good standards and traditions in science.

<sup>106</sup> This development of this general point and theoretical framework is usually contributed to Ulrich Beck (Beck 1992), upon which Hajer also draws.

raised its voice against environmental decline in the early 1970s; similar critiques emerged from technocratic elites. Thus, this same period also gave rise to the Limits to Growth report and to U.N. conferences such as that held in Stockholm 1972, which typified a hierarchical and technocratic top-down approach, rather than more radical 'bottom-up' approaches advocated by the counter-culture. Those employing this ecological modernist approach sought to remedy acknowledged environmental problems through organized management. As Hajer writes:

The policy discourse of ecological modernization recognizes the ecological crisis as evidence of a fundamental omission in the workings of the institutions of modern society. Yet, unlike the radical environmental movements of the 1970s, it suggests that environmental problems can be solved in accordance with the workings of the main institutional arrangements of society (Hajer 1995:3).

Discourses characteristic of ecological modernization, then, recognize the structural nature of environmental problems but nevertheless uphold existing political, economic, and social institutions as capable of addressing the problems. Ecological modernization thereby rejects the critique of modernity often advanced by social movements, presenting a policy strategy which is based on a belief in progress and in the "problem-solving capacity of modern techniques and skills of social engineering." It involves confidence in the possibility of mastery and control, drawing on modernist policy tools such as expert systems and science (Hajer 1995:33).

Although ecological modernization should not be conceptualized as one united set of ideas, it generally involves (1) the argument that societies need to

anticipate and prevent environmental problems (i.e., the precautionary principle) and (2) that pollution prevention is both economically feasible and beneficial; (3) an inclination to look to science for answers to problems; (4) a conceptualization of nature as a public good or resource which should not be seen as a free good nor be used as a 'sink' for polluting activities; (5) the principle that the burden of proof should rest on the polluter, not on the party impacted by the pollution; and (6) acceptance of the existence of a comprehensive environmental problem. This framework leads subscribers of the paradigm of ecological modernization to seek an end to the antagonism between the state and the environmental movement that characterized the early decades of the environmental movement (Hajer 1995:26-9).

### ***Different preoccupations***

Arguments advanced by mainstream scientists and the IPCC, resonate with the above outlined principles of ecological modernization: they involve awareness of global environmental problems and of the danger of the side-effects of technology, while nevertheless rarely, if ever, involving radical social critique. While generally supporting regulation on behalf of the environment and exhibiting a more qualified instrumentalist optimism, the discourses of mainstream scientists, including IPCC leaders, do not fundamentally question the value of science, capitalism and existent political structures.



The differences between the two opposing sides in the debate are summed up in the 1994 book on and by Norman Myers and Julian L. Simon, titled *Scarcity or Abundance: A Debate on the Environment*. The editor of the book in which Myers and Simon explain their highly divergent perceptions of the state of the environment notes the difficulty of imagining two more different views of the prospect of planetary survival than those expressed by the two authors. In the words of the editor, Julian Simon

tells the story of environmental plenty if not bliss, of progressive improvement in the human condition. The reassurance voiced by Franklin Delano Roosevelt in his inaugural address, "that the only thing we have to fear is fear itself," aptly captures Simon's message. Human ingenuity and institutional adaptation in the long run are the most powerful forces of all, he insists, prompting opportunity and the search for solutions.

Norman Myers, by contrast,

tells a story of ecological degradation that is potentially catastrophic in its effects. It is a few seconds before midnight, and the erosion if not the collapse of planetary life-support systems, species extinction, and the material as well as spiritual impoverishment of humankind are but ticks of the clock away (Myers 1994:xiii).

These two views are also to some extent reflected in Ulrich Beck's contrast between modernist and reflexive modernist perceptions of society and risk (Beck 1992). Contrarians and their sympathizers are resistant to the change of industrial society away from a preoccupation with the distribution of wealth and toward a preoccupation with the distribution of risks, late industrial society being confronted with risks of a new nature -- risks that are inherently invisible, unknowable, incalculable yet also potentially catastrophic. The axial principle of

industrial society is continued economic growth and the distribution of goods -- a preoccupation detected in conservative and contrarian discursive foregrounding of third world poverty and of the need to continue economic growth (limits to which are not suggested) to remedy this problem. By contrast, the axial principle in reflexive modernization (in "risk society") is the distribution of 'bads' or dangers (Beck 1992:3).

Certain strands of arguments among mainstream scientists do endorse at least some level of redistribution of resources at a global level, from wealthier, industrialized countries to financially and technologically aid the "South." As we have seen above, this is a key fear of contrarians. Such arguments are supported by reference to the fact that the industrialized world has created the largest share of the greenhouse gas problem, and by reference to the role of poverty in unsustainable practices. The need for such assistance is explained as a necessary action by which to reduce the environmental and social risk; aiding poor nations towards sustainability protects rich nations, as poverty is the cause of unsustainable practices which might result in social unrest, practices which also endanger the global environment and hence humanity as a whole. In his 1989 book on global warming, Stephen Schneider described the issue of human-induced climate change as interwoven with the "problem of global economic development," arguing for the transfer of "knowledge, technology, and capital" from rich to poor nations due to the former's responsibility in creating the problem through their "disproportionate per capita use of energy" (Schneider 1990 (1989):268).

Nevertheless, constructions of mainstream scientists as “radicals” and “extremists” conflict with the inherent conservatism of the ecological modernist framework which characterizes the scientific mainstream and the IPCC. Even calls to the effect that the wealthier nations of the world ought to assist the less developed countries around issues related to environment and development do not threaten the fundamental structures of society; they remain vague and more closely resemble voluntary welfare on a global level.<sup>107</sup> Moreover, calls for the transfer of resources are less than strong and repeated among mainstream scientists; while such transfer might be voiced as a moral responsibility and as a value, it is often not discussed at all (most mainstream scientists avoid that level of policy issues altogether), and it is just as often accompanied by resignation and recognition of the extreme difficulty of getting industries to transfer green technologies to less developed countries for free or at prices below market-value. As Hajer writes, the paradox of the new environmental conflict, where everyone agrees that environmental decline is a problem, is that policies do not match social expectations (Hajer 1995:43). The general coalition of actors and institution which has developed around the issue of sustainable development -- of which the framework of ecological modernization is a part -- can only be kept together by virtue of the vagueness of the proposed goals, at the same time as it may ask for considerable social change (Hajer 1995:14).

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<sup>107</sup> While embattled, welfare within national borders, as well as international assistance of various sorts that have been in place for decades, have not threatened existant economic and political strutures. Rather, one might argue that they have worked to preempt social upheaval which might have constituted a more fundamental challenge to status quo.

### ***Goliath versus David – one-sided renditions***

Contrarians as well as the above mentioned groups often emphasize the powers of the environmental movement, while omitting references to the powers exercised by counter-vailing forces associated with industry groups and the right-wing. For example, in his interview with me, Patrick Michaels said that contrary to popular perception, environmentalists are really the Goliath against which other forces, "David," fights. In his newsletter, Michaels has written about the environmental advocacy movement's ability to induce political action through scare stories:

These lobbying groups have (combined) budgets in the hundreds of millions of dollars, and employ about 50,000 people; their support is highly valued by many political figures. As with any large groups [sic.], self-perpetuation becomes a crucial concern. 'Global warming' has become one of the major battle cries in their fund-raising efforts.<sup>108</sup>

Similarly, when explaining some of the reasons why "we are continually bombarded by statements that there is some monolithic consensus of scientists that climate catastrophe has begun" -- when in fact "the data provide no evidence of man-induced global warming," Richard Lindzen cites several factors. Singled out for attention is the concern of groups within the "environmental advocacy movement" about their self-perpetuation, the media which "unquestioningly accept the pronouncements of these groups as objective truth" and the ability of scare stories to induce political actions for this 'advocacy movement.' He suggests that with the "huge" lobbying power, this advocacy

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<sup>108</sup> This was in a copy of Michaels' newsletter, but I have momentarily misplaced the exact reference.

movement buys politicians, exerting a "powerful influence" on the government. These arguments by the contrarians concerning the motives and media power of their opponents are mirrored among mainstream scientists and their sympathizers in the world of politics and the media, as we saw in Chapter 2. Scientists tend to be more circumspect about making public statements to this effect, but a common explanation of the contrarians is that they are bought by industry groups and therefore deserve no attention.

My point here is to show the one-sided nature of the renditions by both sides; how they mirror each other in their arguments and in their resistance to acknowledge the role of interests, power and influence among themselves and other groups on their side of the issue of human-induced climate change. Clearly, scientists, environmental groups, and industry groups are all concerned about their self-perpetuation. They exert power to maintain their funding and influence, and to impose their views. The most important thing about these arguments is what they fail to say. Thus, returning to the arguments about David and Goliath by Lindzen and Michaels described above, it is clear that while they are minutely aware of the interests driving the environmental movement, Lindzen and Michaels play down, if they mention at all, the considerable power of industry groups and the right-wing in U.S. society around this issue. All of this requires a discussion about the relative power of the competing groups, but that is outside the scope of this dissertation. However, I do want to note two things. First, I want to refer to the chapter on the public relations activities of major fossil fuel companies in the U.S. In that chapter, I noted that during the year 1993

alone, the American Petroleum Institute (API) -- just one of the more than fifty members of the Global Climate Coalition -- spent \$1.8 million on public relations campaigning intended, among other things, to defeat a proposed tax on fossil fuels. I mentioned that journalist Ross Gelbspan has calculated that the spending of API alone, a single company, thus spent "only slightly less than the combined yearly expenditures on global warming of the five major environmental groups that focus on climate issues -- about \$2.1 million, according to officials of the Environmental Defense Fund, the Natural Resources Defense Council, the Sierra Club, the Union of Concerned scientists, and the World Wildlife Fund" (Gelbspan 1995:34). Dr. Radford Byerly, who worked in Washington, DC, on Congressional Committees related to science and technology for decades, confirmed that although some environmental organizations do have significant resources, what they spend on lobbying on the Hill is a "drop in a bucket" compared to what is spent by industry. The largest automobile manufacturers in the U.S. might have two senior lobbyists on the Hill for each issue of relevance to the company. Actual numbers are hard to come by, however, as private for-profit companies do not disclose all their expenses related to lobbying and public relations efforts; while there are disclosure laws requiring companies to disclose lobbying costs, the laws have loopholes.<sup>109</sup>

Secondly, while environmental organizations also obtain money from wealthy individuals and foundations (e.g., the Heinz Foundation), they enjoy great levels of support at the grass-roots level. By contrast, the industry groups

and conservative elites involved in the climate debate do not enjoy and depend for their influence on that level of public backing; here money, not people-power, is the biggest factor. Contrarians often point out that the majority isn't always right, whether that majority consists of scientists or the general public. That is obviously true, but the fact remains that in democratic societies, people are entrusted with the right and ability to participate in the decision making process through the means of voting. The financial backing of contrarian view points by industry groups and conservative elites is often obviously anti-democratic, representing the "one dollar, one vote" rather than the "one person, one vote" type of influence. Also, that level of financial backing has no equivalent on the proponent side of the issue of human-induced climate change, considered as a whole. The network of conservative foundations and the level of mobilized financial resources -- what Sara Diamond calls the "conservative labyrinth" -- has no parallel in the liberal camp, and it is transforming politics in U.S. society (Diamond 1995; Fischer 1991; Ricci 1993:168).

### ***Conclusion***

Contrarians and scientific advocates of concern about human-induced climate change have different anxieties. The latter are preoccupied by environmental degradation and what they perceive as a crisis of industrial society. By contrast, contrarians are afraid of changes in society which undermine existent structures and processes, particularly unregulated capitalism,

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<sup>109</sup> Dr. Radford Byerly, personal conversation, March 26, 1998.

unlimited economic growth, and structures related to the nation-state which have served to secure its boundary and sovereignty. A key distinction between contrarians and most mainstream scientists, and particularly the scientific advocates of concern about human-induced climate change, is that while the latter looks to international governmental frameworks for solutions, contrarians tend to see international governmental frameworks as an important part of the problem. Contrarians typically subscribe to an older, nationalistic framework, and some of them in particular are preoccupied by international conspiratorial networks which they perceive to be threatening the United States. Their sense of national boundaries is disturbed by perceptions of enemies within the borders. As they describe it, the world is coming apart, the national unit threatened and "infiltrated;" the boundaries of the nation state are eroding in a globalized world characterized by debate and concern about global distributions of wealth and environmental dangers.



## Epilogue

As I hand this dissertation in to the Graduate Office at Rice University, I recognize that important conclusions have yet to be drawn from the wealth of data and arguments embedded in it. In future research, I intend to further define and qualify the nature of the differences between the scientists on all sides of this debate. The focus of the dissertation remains at the level of rhetoric, and this focus needs to be supplemented by a focus on actual outcomes. Rhetoric aside, what actions on behalf of this threat are concretely supported by the different scientific actors in this debate, and to what effect?

The FCCC meeting in Kyoto, Japan, in December 1997 rendered evident the symbolic nature of the negotiations. While impressive in terms of the level of international, diplomatic collaboration involved, and in terms of the sacrifices necessary to meet the pledged global reductions in emissions of greenhouse gases, the agreement is in fact very modest; if the threat of human-induced climate change proves to be real, what was agreed upon in Kyoto (i.e., reducing global emissions of greenhouse gases to 1990 levels within the next ten to fifteen years) constitutes a very small step. A sense of accomplishment is deserved, especially if the treaty is ratified and honored. However any sense of accomplishment must be seen in the context of the profound social changes required at a global level to truly render the world as a whole healthy, just, and sustainable.

Important questions remain. Aside from self-serving agendas, genuine social and environmental concerns underlie the focus on human-induced climate change. However, is this focus well-directed? Might the same environmental dynamics that are put into focus by the concern about human-induced climate change be foregrounded and addressed more effectively through a different lens? The climate issue subsumes environmental problems that are older and more immediate, tangible, and undisputed -- problems such as air and water pollution, toxic waste, overexploitation of finite resources, poverty, overconsumption, overpopulation, and a general lack of preparedness for weather anomalies, whether due to natural variability or to human activities. But to what extent does the focus on human-induced climate change attract attention to these more familiar environmental problems, and to what extent does it divert attention from them? The stakes in the climate issue are high, to the extent that the climate issue has become an emblem issue as to whether or not there is an environmental crisis or not. As suggested by the quotations in the introduction, global warming has become a symbol of whether there is such a crisis; it has come to serve as the looking-glass through which the modern world seeks to judge how well it is doing. If the climate threat proves to be minor for most people in this world, what will be concluded about how we are doing? What would the consequences of this be for the environmental movement? The danger is that by focusing so insistently on human-induced climate change, these other, very real and immediate problems might be sidelined as we wait to find conclusive proof of this still largely hypothetical and uncertain threat.

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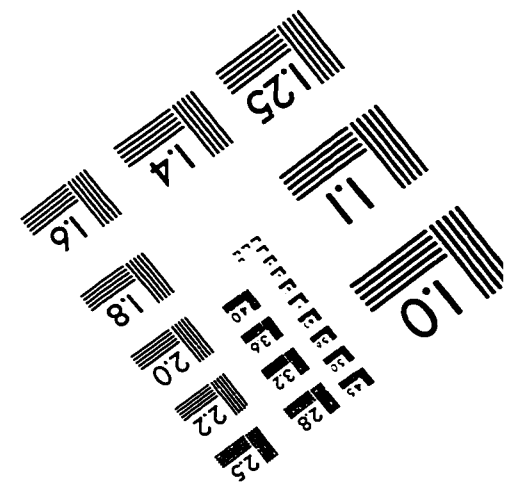
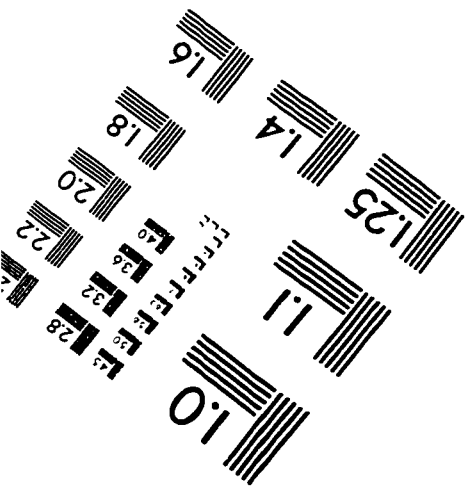
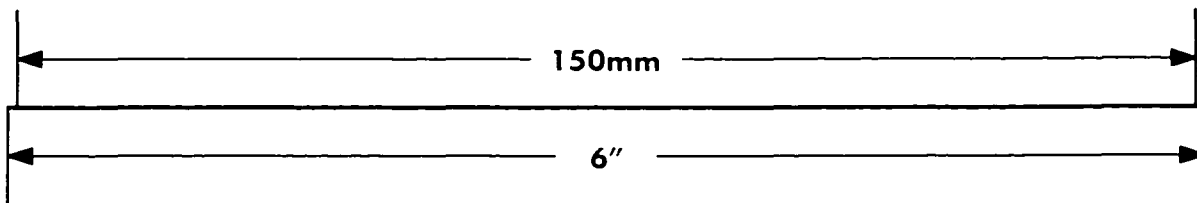
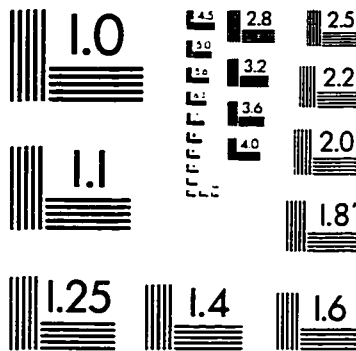
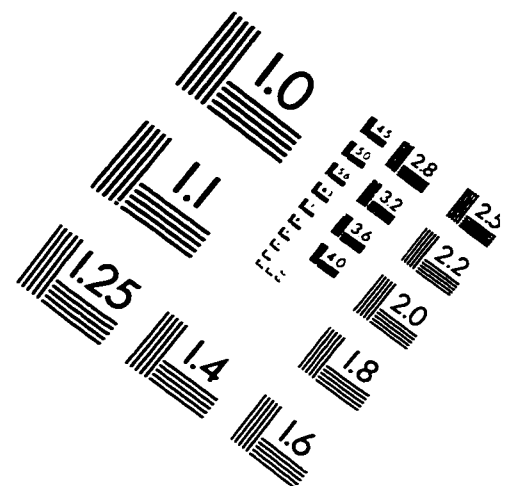
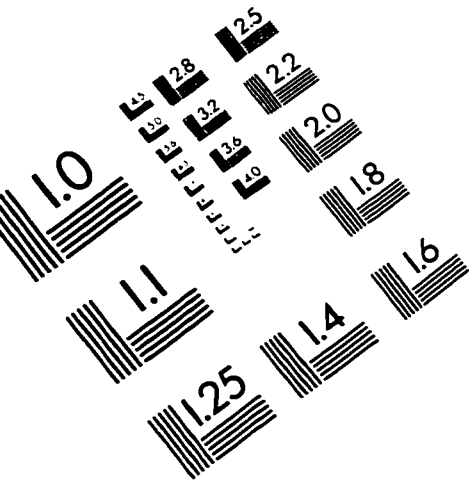
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