Chapter 6

Climate Science and Politics in the United States

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Introduction

Although the theory of carbon dioxide-induced climate change originated in Europe, it was in the United States that it first became the object of sustained scientific study and, later, of national and international policy concern. In the late 1990s, US science continues to dominate global change research, particularly in the areas of Earth systems modeling, satellite data collection, and global warming detection (although European laboratories have taken the lead in the core science of atmospheric modeling).

Perhaps ironically, given this longstanding scientific leadership, the US pursued a relatively conservative policy course as the issue moved to the international negotiating table. While global change research budgets soared under President Bush, American diplomats resisted binding commitments at the 1992 UN Conference on Environment and Development (UNCED), where the Framework Convention on Climate Change (FCCC) was hammered out. Even the nominally pro-environment Clinton Administration — with an outspoken advocate for strong greenhouse policies in Vice President Al Gore — has so far failed to achieve more than incremental movement on a global warming treaty, in the face of heavy resistance from the Republican majority in the US Congress.

In this chapter, we trace the history of American climate science and politics from the International Geophysical Year to the present. We argue that the US climate science/policy interface exhibits a particular constellation of features:

1) Policy entrepreneurship, with scientists taking the lead in raising the profile of the climate issue within the government.

2) Individualism in research planning, leading to tensions, redundancies, and fragmentation in global change research. The vast scale of the US effort — about $1.8 billion annually since the mid- 1990s — magnifies these features and renders coordination and consensus-building all the more difficult.

3) Until recently, a comparatively low investment in directly policy-relevant science. Since 1994, Congressional hostility to established science advice mechanisms has limited (or altered) available channels for science/policy interchange.

4) Public, intense, and long-lasting debates over the validity of climate models and data. In these debates, unstable boundaries between science and politics are part of what is being negotiated. They render the social construction of scientific claims highly visible, while simultaneously masking the uses of science in constructing political positions.

5) In part as a result of (3) and (4), slow movement — relative to European nations — from research science to policy choice.

Each of the features listed above is an important key to the US climate science/policy interface. Their combination illuminates contrasts with the other nations covered in this volume. Most importantly, it helps explain the gear-grinding steady state of US climate policy at the turn of the millennium.

Another way to understand the American context is represented by a "constitutionalist" argument that has emerged from recent science-studies scholarship. This view takes the pluralism, balance of powers, and open-forum democracy of American politics — which inevitably treats science similarly to any other concentration of power — as fundamentally good (Ezrahi 1990; Jasanoff 1992; Jasanoff 1996). Constitutionalists argue that by its prominent display of a very wide range of views, American climate politics has in fact avoided the over-rationalization of an extremely complex problem which its deeply embedded in fundamental social and economic institutions. The notion — embedded in the FCCC and the Kyoto Protocol — that global society and the global economy can be reengineered with relative ease is clearly dubious, if not simply false. The debates played out in American climate politics in fact raise very plausible examples of just how difficult and potentially destabilizing any effort to reengineer the world will be. The constitutionalist view points to the need, in pluralist societies — including the international community (perhaps the ultimate example of political pluralism) — to patiently build trust and belief, allowing all stakeholders a voice in the outcome (Haas 1990b; Jasanoff et al. 1998).

In this limited space we can only sketch the history and current state of US climate science and policy. Many, more extensive studies of these subjects are available. We intend not to duplicate these, but rather to emphasize the particularity of the United States context. To achieve this goal, we minimize discussion of international institutions and events.

History of US Climate Science and Policy

This section offers a brief history of climate science/policy interfaces in the United States. We characterize five roughly-bounded phases:

- Revival of scientific interest (1950s)
- Sporadic policy concern, led by scientist policy entrepreneurs (1960s)
- Sustained policy interest, with construction of multiple science/policy interfaces (1970s)
- Climate change as mass politics (1980s)
- Climate change as ideology (1990s)

Revival of Scientific Interest: 1950s

As we saw in Chapter 2 (this volume), the 1950s brought a revival of the 19th-century concept of carbon dioxide-induced global warming. Plass, Suess, and Callendar each noted the likelihood that rising CO2 concentrations might affect the climate (Callendar 1958; Plass 1956; Suess 1953). Revelle and Suess estimated that about half of the CO2 resulting from combustion of fossil fuels remained in the atmosphere. In a now-famous phrase, they wrote that humanity was conducting, unawares, "a great geophysical experiment" on the Earth's climate (Revelle and Suess 1957, 19). With Revelle's encouragement, measurements of CO2 were initiated by Keeling in Antarctica, and soon afterward at Mauna Loa, Hawaii, during the International Geophysical Year (Keeling 1960). By the mid-1960s, these data — from locations almost entirely unaffected by local urban/industrial emissions — had confirmed earlier speculations, pointing to the reality of a global rise in greenhouse gases.

But the link between global warming and rising CO2 remained theoretical, based on simple physics and atmospheric chemistry. Climatology, at the time a statistical science consisting mainly of data collection and analysis, rather than theory, could provide little in the way of confirmation or denial. Accurate, detailed global data on any aspect of the atmosphere were simply unavailable [Edwards, forthcoming #489; Chapter 2, this volume]. At the time of the Revelle and Suess article, environmentalism did not yet exist as a major social movement; despite the gravity of their pronouncement, Revelle and Suess's tone reflected more scientific curiosity than alarm.

This decade also saw the development of numerical modeling, first as a tool of weather prediction and then as the first workable method for the scientific study of the global atmosphere. By the mid-1960s, the world's first primitive-equation general circulation models (GCMs) were operational at the University of California at Los Angeles (UCLA), the Geophysical Fluid Dynamics Laboratory (GFDL), and the National Center for Atmospheric Research (see Chapter 2). Although GCM developers thought of their models primarily as a tool for testing theories of the atmospheric circulation (and potentially for long-term weather forecasts), they were aware from the beginning that long runs of atmospheric simulations would exhibit "climates" of their own.

Sporadic Policy Concern: 1960s

In the 1960s, scientist policy entrepreneurs began to seek issue "handles" by which to establish the importance of climate science to government. Global warming at first was only one of these, and not the most successful; the nature and extent of human impacts on climate remained an open question well into the following decade. Only sporadic policy attention was achieved, but this was sufficient to expand research budgets and secure an initial foothold for global atmospheric issues.

Emergence of a sustained climate change research program

During this period, US climate science was dominated by basic research. This is reflected in the fact that the two main Federal agencies responsible for climate studies were the National Oceanic and Atmospheric Administration (NOAA) and the National Science Foundation (chief patron of NCAR, UCLA, and most other university-based atmospheric research programs). The new theory-driven approach, whose core tool was the computer model, was gradually replacing the older, empirical-statistical science of climatology. The latter focused primarily on local and regional climatic conditions, associated with the practical concerns of agriculture and conceived as a steady state with possible cyclical variability.

However, the primitive 1960s GCMs were essentially useless as a climate forecast method; they offered no competition at all with carefully compiled empirical data for recent decades. At best, they could very roughly simulate average global circulation patterns. Their strength in the field of climate studies lay, instead, in their potential as an <u>experimental</u> tool. In a model, the many variables thought to influence climate could be manipulated and their relationships and relative importance understood. GCMs also allowed, for the first time, detailed investigation of the full three-dimensional structure of the atmosphere. Research on fundamental processes such as cumulus convection, radiative transfer, and atmospheric thermal structure — much of it ultimately contributing to GCM development — therefore dominated the scientific agenda of this period. The major first-generation GCM groups were founded by theoreticians building a new tool for scientific study of the atmosphere.

The origins of a sustained climate change research program thus had little to do with genuine fears of global warming. Instead, climate change studies stemmed from interest in the vertical structure of the atmosphere, something GCMs now made it possible to investigate theoretically. In the early 1960s, GFDL’s Joseph Smagorinsky, Syukuro Manabe, and colleagues needed to construct radiative transfer algorithms for GCMs.[1] To do so, they began to study the radiative properties of the atmosphere (Manabe 1998; Manabe and Möller 1961; Manabe and Strickler 1964). By the end of the decade, this interest led to a sustained investigation of greenhouse gases and global climate change (Manabe 1970; Manabe 1971; Manabe and Holloway 1970; Manabe and Wetherald 1975).

One of Manabe's early studies — using a one-dimensional model, not a GCM — examined the effects of a doubled concentration of CO2 on model results. He did this not out of concern that CO2 doubling might really occur, but simply in order to test the model's sensitivity to changes in various variables (such as water vapor, ozone, and cloudiness), all of which were also manipulated in the experiment (Manabe and Wetherald 1967). Once global warming became a serious policy concern in the following decade, this so-called 2xCO2 experiment became a de facto standard in climate studies — since carbon dioxide concentrations are expected to double (from pre-industrial times) sometime in the middle of the next century, a convenient point for policy-related projections. But although Manabe and other general circulation modelers were sometimes peripherally involved in the climate change policy discussions covered below, their own interests were <u>not</u> much governed by concern with anthropogenic climate change.

It is important to emphasize the degree to which early climate change concerns were model-driven. As already mentioned, virtually no reliable data was then available on long-term global climate trends. The data problems discussed in Chapter 2 were still very far from being resolved. The <u>first</u> global data set describing the full global circulation (including the southern hemisphere) in three dimensions was painstakingly constructed from historical records at NCAR during 1968-69 (Baumhefner 1998). Although the first weather satellite, TIROS 1, was launched in 1960, it was not until the 1970s that satellite instruments capable of retrieving data useful for climate studies (such as microwave radiances) were placed in orbit. The World Weather Watch, a modernization and expansion of the global weather data network to take advantage of computerization and new telecommunications capacities, began in the late 1960s. The Global Atmospheric Research

Program, oriented toward massive data collection for testing and refining GCMs, was initiated in the latter half of the 1960s but did not conduct its first global experiment until 1979 (National Oceanic and Atmospheric Administration 1981).

The paucity of global data meant that even the present-day global climate system was then very poorly and incompletely understood. Climate modeling leapt past data-based studies, initiating an era in which models and data would evolve in what one data analyst called an “incestuous” relationship (Baumhefner 1998).

Climate change as weather modification

Climate change first entered policy discussions in the early 1960s (Conservation Foundation 1963). From then until the mid-1970s, it was generally considered a speculative aspect of “weather modification,” which might be either deliberate or unintentional. Frequently, discussions of CO2-induced climate change appeared as subsections of longer studies addressing cloud seeding, airport fog control, and weather or climate as a possible weapon of war (see Kwa, this volume). Aviation, the military, and agricultural interests were the chief clients for weather modification studies; none of these groups was much concerned about long-term changes “Inadvertent” anthropogenic climate change was generally mentioned as a real, but somewhat remote, possibility. Plans for deliberate weather modification thus served as a convenient issue “handle” for climate scientists.

Scientific advisory bodies, rather than policy units, raised most of the early policy concerns related to “inadvertent climate modification” (National Research Council 1966; Panel on Weather and Climate Modification 1966). Executive-branch attention remained sporadic until the early 1970s, consisting primarily in expanded support for research at NCAR and GFDL — scientific organizations generally unconcerned with policy.

Among the most significant early reports was a 1965 study of environmental pollution by the President's Science Advisory Committee (PSAC), which included a long appendix on carbon dioxide and global warming. Two key greenhouse policy entrepreneurs — Revelle and GFDL director Smagorinsky — participated, as did Keeling. Noting steady documented rise in CO2, the report focused on potential global warming but hedged on its likely extent. PSAC recommended extended monitoring of CO2 and global temperature, as well as further work on GCMs. It also outlined possible geo-engineering measures for warming reduction, such as spreading reflective particulates across vast ocean areas to increase Earth's albedo (Environmental Pollution Panel 1965). (In the weather-modification era, such discussions of human manipulation of geophysical processes were a relative commonplace.) While the report made no policy recommendations, it did establish the place of carbon dioxide as a major "pollutant." This furthered the agenda of Revelle and Smagorinsky, whose main goal was to expand research and data collection on the global atmosphere and oceans.

However, other studies of this period often paid cooling scenarios equal (or greater) attention than anthropogenic global warming, raising fears of a new ice age. In part, this reflected the highly theoretical, data-poor character of climate science, at a time when the relationship between the cooling effect of anthropogenic particulate aerosols and the heating caused by a carbon dioxide buildup remained extremely uncertain (Rasool and Schneider 1971). Alarmism over global cooling continued well into the 1970s (Ponte 1976), later returning to haunt some of its former exponents, such as the outspoken Stephen Schneider (Bray 1991; Lindzen 1992). Thus the nature and meaning of climate change remained open to a wide range of interpretations at this point.

Sustained Policy Interest: 1970s

In the early 1970s, two important scientific working groups, the Study of Critical Environmental Problems (SCEP) and the Study of Man's Impact on Climate (SMIC) underscored climate change concerns (Study of Critical Environmental Problems 1970; Study of Man's Impact on Climate 1971). The entrepreneurial professor Caroll Wilson of MIT's Sloan School of Management, a long-time government science advisor and a member of the Club of Rome, organized both studies. Smagorinsky of GFDL attended on a part-time basis, and Revelle consulted, ensuring a prominent place for the CO2 issue in both studies. Scientists and policymakers alike often cite SCEP and SMIC as a turning point in the emergence of anthropogenic climate change as a political issue.

One key motivation for these working groups (especially SCEP) was the then-controversial supersonic transport (SST) issue — a more urgent and focused issue "handle" than weather modification. Some scientists feared that SST exhaust, released in the stratosphere, might cause climatic changes and/or alter the ozone layer. The SST became the first in a series of global atmospheric issues — most notably, ozone depletion, acid rain, and nuclear winter — lending credibility to the idea that human activity could affect Earth's atmosphere not only at a local and regional level (as "pollution"), but on a global scale. Indeed, after SCEP's release, possible SST effects on climate made the front page of the New York Times (Hart and Victor 1993). In 1971, the Dept. of Transportation established a Climate Impact Assessment Program (CIAP), with an annual budget of about $7 million, to pursue this topic (Mormino et al. 1975). CIAP represented the first sustained attention to climate-related policy by the executive branch, but the demise of the SST program minimized the impact of its efforts by removing the cause for concern.

Both SCEP and SMIC cited GCMs as "indispensable" in the study of possible anthropogenic climate change, arguing that GCMs were "the only way that we now conceive of exploring the tangle of relations" involved in climate. They recommended an expanded program of climate, ocean, and weather modeling research (Study of Critical Environmental Problems 1970, 88). They also recommended elaborate global monitoring programs, including a network of 100 monitoring stations to sample air and precipitation chemistry, measure solar radiation, and gather other climatological data. Wilson had planned the reports as contributions to the UN Conference on the Human Environment (UNCHE), scheduled for Stockholm in 1972. The UNCHE rewarded him by approving calls for a global monitoring network "with little discussion" (Hart and Victor 1993, 664). Domestically, however, SCEP and SMIC served more to organize the agenda of the research community than to arouse the interest of policymakers in long-term global warming.

Instead, the most significant early executive-branch concerns with climate began with climatic anomalies of the year 1973, when a long-term drought in the African Sahel reached crisis proportions, Peruvian anchovy fisheries failed, and the Soviet Union's wheat crop failed disastrously, resulting in the first major US wheat sales to the USSR (Ausubel 1989; Hechler 1980, 972). In the Kissinger/Nixon Realpolitik frame, these issues translated into potential problems of geopolitical stability. These practical and geopolitical concerns provoked executive interest in climate monitoring and near-term (annual to decadal) prediction. The White House Domestic Council first proposed a "United States Climate Program" in 1974, leading eventually to the National Climate Program Act (NCPA) of 1978.

The NCPA authorized $50 million annually for climate monitoring, forecasting, and basic research. Its chief concerns involved short-term climatic effects: "on agriculture, energy supply and demand, land and water resources, transportation, human health, and national security" (Congressional Record 1998c). In addition to supporting new research, the NCP was to serve a coordinating function for the many existing Federal programs related to climate. It emphasized research and reporting (rather than action) on climatic change, and focused primarily on local and regional scales. Long-term, global climate change was only one focus among many. Both ice-age and warming scenarios were then in full play among scientists; indeed, an early, Nixon-era stimulus to policy concern was a Central Intelligence Agency report which warned of global cooling (Ausubel 1989, 2). In addition, the Program strengthened the basis of the climate community's "scientific internationalism" (see Chapter 1, this volume) through its support for international projects such as the Global Atmospheric Research Program and the World Climate Program, as well as US-based global data collection (Board on Atmospheric Sciences and Climate 1986).

From today's perspective, however, among the NCPA's most striking features is its national framing of climate issue. Despite some support for international activities, the NCPA focused on forecasting only the US climate. Scientists knew that such forecasting would require global models and global data networks, but the political approach to climate recognized a national interest in climate mainly within the borders of the US. Over the next decade, this would evolve to a recognition of global concerns as legitimate focus of climate policy.

In recognizing the need for a coordinating body, the NCPA marked the proliferation of scientific interest in the topic during the 1970s. As with most US science, individual scientists and small groups usually chose their own projects and directions, with support contingent on peer approval. The climate field, more than many scientific disciplines, had a unifying project — namely, building and refining GCMs — but work oriented toward climate policy was rare. Although it did support a considerable amount of greenhouse research, the NCP's major result was to strengthen user-oriented "climate services" and develop impact assessment methods.[2] The NCP marked a transition between the short-term, weather-modification frame that dominated US climate policy during the 1960s and

1970s and the concern with long-term, quasi-permanent climate change that took the stage in the 1980s.

The first major greenhouse program developed in the Department of Energy as an outgrowth of the 1974 energy crisis. The DOE's relationship with climate issues dated to its earlier incarnation as the Atomic Energy Commission (AEC). AEC-and DOE-sponsored studies of radioisotope "tracers" contributed to knowledge of atmospheric chemistry and global circulation patterns, while computer modeling of nuclear blasts developed modeling techniques closely related to those used in GCMs (Richtmyer 1957). As the DOE, the agency came to bear responsibility for environmental analysis of energy resources.

When the Carter Administration created a multi-billion-dollar synthetic fuels program to reduce US reliance on imported oil, scientists pressed the case for concern about greenhouse warming (Geophysics Study Committee 1977). Responding to this, in 1977 the DOE initiated a Carbon Dioxide Research and Assessment Program. The nascent National Climate Program then assigned "lead agency" responsibility for the carbon dioxide question to the DOE. With an annual budget of $10-15 million, the DOE program became the largest greenhouse research effort in the US (Ausubel 1989).

The White House Office of Science and Technology Policy (OSTP) also became concerned about the possible environmental effects of the synfuels program, including acid rain as well as climate change. In 1979 OSTP requested a "fast-turnaround" study of the climate question from the National Research Council. Convened by renowned MIT meteorologist Jule Charney, the study group predicted warming in the range of 1.5°C to 4.5°C for CO2 doubling (Ad Hoc Study Group on Carbon Dioxide and Climate 1979). This conclusion was based primarily on results from GCM work by Manabe's group at GFDL and James Hansen's group at NASA's Goddard Institute for Space Studies (GISS). In response to the "Charney report," the Carter Administration amended its synthetic fuels program in 1980 to include "a comprehensive study of the projected impact, on the level of carbon dioxide in the atmosphere, of fossil fuel combustion, coal-conversion and related synthetic fuels activities" (Congressional Record 1998a). Notably, however, the chief environmental policy unit in the Carter White House, the Council on Environmental Quality, concluded at the same time that cooling scenarios posed a greater threat than global warming (Council on Environmental Quality 1980).

Thus the 1970s saw the first sustained policy attention to climate change. Scientist policy entrepreneurs such as Revelle, Smagorinsky, and NCAR's William Kellogg continued to articulate reasons for government concern, especially greenhouse warming and aerosol-driven cooling, but they rarely stepped beyond recommendations for further study which promoted their existing scientific agendas (Kellogg 1977; Kellogg 1987). The first Congressional action on climate aimed to develop practical short-term forecasting and climate services, rather than to address the carbon dioxide warming issue.

By the second half of the 1970s, the number of institutions conducting serious climate research had grown dramatically. These included NCAR, GFDL, several NASA laboratories, and a number of NSF-sponsored university programs, all focusing on basic research, and the NOAA-led National Climate Program and the DOE CO2 program. Problems with harnessing to policy the vast, heterogeneous research base generated by the individualist model of science support had already emerged by the time of the NCP.

To the extent that they occurred at all, top-down attempts to focus and coordinate research in the interests of policy relevance were systematically resisted by scientists (except in a few service institutions such as the National Climatic Data Center), because they ran against the grain of the individualist model. No central organization emerged to focus greenhouse research, nor did any adopt a research agenda principally aimed at providing directly policy-relevant results. Instead, a pattern emerged — already evident in the 1960s — of periodic committee reports assessing the state of the field. To the extent that these addressed practical policy concerns, such as local and regional impacts of climate change and feasible mitigation measures, they tended to defer attention into the future, when better science and faster computers would allow more accurate estimates.

The momentum of this pattern — individualism in research agendas, punctuated by state-of-the-art assessments —produced strains in the US once the greenhouse issue reached the international negotiating table (see below). The weather modification paradigm — with few practical results to show for decades of effort (see Kwa, this volume) — had failed to excite strong public or government interest, and the SST controversy fizzled when the program was abandoned. The association of climate change with energy politics in the latter half of the decade provided the first successful issue handle, arousing the sustained concern of the White House, the DOE, and the EPA (see below).

Climate Change and Mass Politics: 1980s

During the 1980s the global warming issue gradually became a concern in its own right, and US climate politics came to focus on global (rather than merely national) concerns.

This occurred for several reasons. First, climate science was maturing, with a narrowing range of theory- and model-based predictions (for the most part eliminating cooling scenarios).

Second, the opportunistic policy entrepreneurialism of the 1960s and 1970s gave way to a more concentrated effort among elite scientists to focus on global warming concerns, in part because of executive branch pressure to produce coherent policy recommendations. The “Charney Report”— and in particular its 1.5°-4.5°C warming prediction — marked the stabilization of a scientific consensus that continues into the present in the work of the IPCC. This consensus has been maintained by a considerable effort (social as well as scientific); it represents the climate-science community’s awareness that a stable baseline is necessary both for the viability of climate change as a political issue and for the community's own credibility (van der Sluijs et al. 1998). The existence of this consensus, as a public representation if not as a fact among insiders, was a major factor in moving the greenhouse issue into the public sphere. However, by itself it was far from enough to govern policy considerations, since the key issue for policy was the human impacts of warming. The science of this era could not predict these with confidence.

Third, two other major issues of the period — nuclear winter and ozone depletion — generated enormous public concern. As genuinely global atmospheric problems, these prepared the way for widespread awareness of the greenhouse problem.

The "nuclear winter" hypothesis resulted from an investigation into the atmospheric effects of massive fires resulting from nuclear war (Crutzen and Birks 1982). A group led by Richard Turco (and including Carl Sagan) explored the effects of such fires using simple computer climate models (Sagan 1983). Their simulations predicted that after a major nuclear exchange, temperatures over land in much of the Northern Hemisphere might drop below freezing for several months. Darkness resulting from the smoke cloud would impair photosynthesis, potentially causing massive crop failures and other environmental damage. Turco coined the phrase "nuclear winter" to describe these results (Turco et al. 1983). Although other climate scientists disputed the Turco/Sagan group's claims, the metaphor stuck (Covey et al. 1985; Thompson et al. 1984). The concept had extraordinary political import, since it cast doubt on Reagan Administration claims that a nuclear war could be meaningfully "won" (Scheer 1982). Immediately taken up by the anti-nuclear-weapons movement, it made its way into the mass media and the popular imagination, playing a significant role in the widely viewed TV movie The Day After (1983). Stanford scientists Paul Ehrlich and Donald Kennedy collaborated with Sagan and atmospheric scientist Walt Roberts (founding director of NCAR) on a popular book, The Cold and the Dark: The World After Nuclear War (Ehrlich et al. 1984).

Similarly, the dramatic discovery of an “ozone hole” over Antarctica in 1985, and the series of remarkably successful negotiations over ozone-depleting chemicals, drew attention to the vulnerability of the global atmosphere (Benedick 1991).

A final ingredient in preparing ground for climate change as mass politics was the decline and end of the Cold War, leaving in its wake a sort of “apocalypse gap” readily filled by global warming doomsday scenarios; the Conference Statement of the 1988 Toronto Conference on the Changing Atmosphere noted that “humanity is conducting an unintended, uncontrolled, globally pervasive experiment whose ultimate consequences could be second only to a global nuclear war” (World Meteorological Organization 1988, 710).

The Reagan Administration became notorious for militant anti-environmentalism, especially during Reagan’s first term (1981-84). Among other things, the incoming administration cut the DOE CO2 program’s budget and dismissed its leader as part of a systematic attack on the agency itself (which it attempted to dismantle), as well as on other government bodies concerned with climate change such as NOAA and the Environmental Protection Agency (EPA). The report mandated by the 1980 Energy Security Act — finally completed by the National Research Council in 1983 (Carbon Dioxide Assessment Committee and National Research Council 1983) — expressed measured concern about greenhouse warming and called, like most studies before it, for further research rather than policy action.

But the NRC report was overshadowed by a more radical study of warming-related sea level rise, released on the same day by the EPA and drawing frightening conclusions about threats to coastal cities, island nations, and low-lying lands across the globe (Hoffman et al. 1983). This was the kind of human-impacts information most likely to concern policymakers. Like the 1979 Charney report, EPA’s modest greenhouse program (about $1 million/yr.) employed the GISS GCM, rather than the somewhat more sophisticated climate models in use at NCAR and GFDL. GISS leader Hansen later became notorious for declaring global warming a reality before Congress (see below). Jesse Ausubel has noted the irony of the situation: “in a surprising turnabout for the [Reagan] Administration, its ‘own’ report, the EPA report, based on the Hansen [climate] model and involving several assumptions from the ‘high’ end of plausible scenarios, was much more alarming in tone than the report to the government from the NRC” (Ausubel 1989, 3). Conflicts between the two reports’ conclusions set the stage for a major public debate pitting the Reagan Administration against the EPA.

Aside: science and policy in the American context

This is the place for an important aside about the structure of science-based policymaking in the US. Policy controversies here frequently take the form of debates about the scientific basis of the policy. This happens for a number of reasons. First, American government has an exceptionally open constitutional structure. The bulk of legislative activity is a matter of public record, and most administrative decisions must have an explicit basis subject to specific procedural (and sometimes substantive) standards. The latter frequently require public hearings and the solicitation of expert advice and stakeholder opinion. This open, pluralist system creates many points where policy choices can be challenged; indeed, this is the system’s fundamental purpose. American science, too, values open communication of methods and results, as well as critical challenges by peers, as basic mechanisms of knowledge production. Thus in the case of policy based on science, two systems specifically intended to promote challenge-response-revision cycles multiply each other’s effects (Jasanoff 1990).

Second, US-style pluralist democracy places science in an ambiguous role. On the one hand, science is recognized and respected as a knowledge source. At the same time, since policies based on controversial science are easily challenged at the science end, all sides seek to enroll experts. Thus scientists are treated as representatives of interest groups to be accommodated at the negotiating table. The charitable assumption — dating back to a 1950s vision of scientific authority — is that scientific knowledge will command policy consent, so that the most credible science can be translated directly into political power (Ezrahi 1990). However, stakeholders — especially the economically powerful groups targeted by possible regulations, such as the fossil fuel industry — are well aware that closure of debate can be delayed by challenging key scientific results or, sometimes even more effectively, by raising the level of certainty required of scientific knowledge.

Third, American journalistic standards, partly driven by market demand, require that media reports on such debates include all opinion poles. (Journalists are presumed, probably correctly, incompetent to judge the credibility of scientists qua experts.) Meanwhile, the general public — based on information filtered through the media — is expected to make complex judgements of the credibility of scientific knowledge for itself.

The result of all this is that especially when stakes are very high, as in the climate change case, small minorities can retain a disproportionate grip on public debates over quite long periods. On the other hand, scientific controversy can also arouse public interest and thereby generate movement in the policy arena.

Congressional action on global warming

The intense debate over the conflicting EPA and NRC conclusions may have been responsible for the first major Congressional attention to global warming; it thus serves as an example of the potential of scientific controversy to provoke policy action. “Hostility, sometimes quite open and extreme, became a feature of the agency scene that only began to abate in the late 1980s” (Ausubel 1989, 4). Together with ongoing battles over environmental issues (and over the EPA itself) between the Democratically-controlled

Congress and the Reagan Administration, this constellation of factors brought the greenhouse debate into the legislative arena in its own right, detached from the weather modification and energy politics issue "handles." While this went on, non-governmental organizations joined the issue for the first time. Some, especially the Natural Resources Defense Council, developed credible independent expertise on the issue. The first exclusively climate-oriented NGO, the Climate Institute, emerged in 1986. Allied with the EPA and key Congressional players, these organizations succeeded in passing the Global Climate Protection Act (GCPA) in 1987.

The tone of the GCPA is vastly different from that of its predecessor, the National Climate Program Act, nine years earlier. The GCPA, summarized:

- Expresses certain congressional findings regarding global climate protection, including [that] there is evidence that manmade pollution may be producing a long-term and substantial increase in the average temperature on the surface of the Earth, a phenomenon known as the "greenhouse" effect….

- Provides that U.S. policy should seek to: (1) increase worldwide understanding of the greenhouse effect and its consequences; (2) foster cooperation among nations to coordinate research efforts with respect to such effect; and (3) identify technologies and activities that limit mankind's adverse effect on the global climate.

- Directs the President, through the Environmental Protection Agency, to develop and propose to the Congress a coordinated national policy on global climate change.

- Directs the Secretary of State to coordinate such U.S. policy in the international arena (Congressional Record 1998b).

The EPA had thus “won” its contest with the DOE, emerging as the lead agency in developing national climate policy. Notably, this policy was now framed as a global rather than a merely national concern. However, it was not until the following year that global warming achieved the status of mass politics.

Global Warming as Mass Politics

In June 1988, during testimony before the House Energy Committee, NASA climate modeler James Hansen asserted “99 percent” certainty that the unusually warm global 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 {Hansen 1988}.

The hearings on global warming had been deliberately scheduled for the hot summer months. Coincidentally, their impact was strongly enhanced by a devastating US drought. In addition to widespread water shortages and crop failures, the summer featured huge forest fires throughout the western states, most notably in Yellowstone National Park, a major icon of unspoiled nature for US citizens. Hansen’s testimony is widely regarded as the point at which global warming entered mass politics. From that point on, the issue has steadily received substantial, high-profile media coverage.

At the same time, pressures began to mount outside the United States for action on global warming. The UN Environment Programme (UNEP) and the WMO established the Intergovernmental Panel on Climate Change (IPCC) in 1988. Soon afterward, the government of Canada convened the Toronto Conference on the Changing Atmosphere (in the same month as Hansen’s Congressional testimony), which issued a strong call for action, promoted the IPCC, and initiated the process leading to the 1992 FCCC. US scientists were heavily involved in all of this, although leadership came from the UK, Canada, Sweden, and international bodies such as UNEP, the Group of Seven (an economic summit group comprised of Canada, France, Germany, Italy, Japan, the UK, and the US), and the WMO. By the end of 1990, the European Community had adopted an explicit climate change policy, subsequently endorsed by the EFTA states, committing member states to stabilizing CO2 emissions at 1990 levels by the year 2000. Soon afterward, the EC began considering a carbon tax.

For the reasons discussed above, as climate policy became a public issue, contests between what Samuel Hays has called “frontier” and “high-proof” scientists took center stage in the United States (Hays 1989). “Frontier” scientists — such as Hansen and the outspoken Stephen Schneider, then at NCAR (Schneider 1989a; Schneider 1989b) — tended to prioritize theory, asserting that models, while still imperfect, are an appropriate scientific tool. Rather than wait for an unambiguous “signal” of climate change to show up in the observational data, they were willing to accept intermediate levels of data/model agreement as preliminary, if uncertain, confirmation of model results. “High-proof” scientists such as MIT’s Richard Lindzen, by contrast, prioritize observation (Lindzen 1992; Lindzen 1990), seeking high levels of empirical confirmation before accepting model results. This group tended to perceive variation among models, and systematic errors within them, as evidence of their fundamental inadequacy.

The Reagan Administration — in keeping with its general strategy on environmental issues — had taken the high-proof side, proposing further basic research. President Bush followed his predecessor’s approach, pointing to gaps in scientific knowledge. Meanwhile, Democrats pushed for more immediate and direct remedial action. Then-Senator Al Gore led a Democratic attack on the Republican policy, at one point calling global warming “the most serious threat we have ever faced” {Gore 1992: 40}. The collision of the Democratic push for action, Bush's self-proclaimed environmentalism, and Republican caution was temporarily resolved by another major increase in research budgets, this time with a nominal mandate to advise policymakers, in the form of the interdisciplinary, multi-agency US Global Change Research Program.

Proposed by Reagan in January, 1989, in one of his last acts as President, the USGCRP was formally created in 1990 to coordinate most government-sponsored research related to global environmental change, broadly defined. The organization supports a huge range of fundamental research, ultimately seeking a predictive understanding of Earth-system behavior on time scales of primary human interest. It includes literally hundreds of projects in thirty-five major areas of research {Sarewitz 1996: 84-85}. Since its creation, the USGCRP budget has typically exceeded $1.5 billion annually, marking it as one of the largest science programs in history {Pielke 1994: 315}. Although many of the projects included in the USGCRP predated the program (notably NASA plans for earth-observing satellites), much of the budget was "new money" — an unprecedented level of investment in environmental science research. Yet despite lip service to policy relevance, the program has been repeatedly criticized for failure to deliver information on the subjects of most concern to policymakers (Brunner 1996).

Skepticism also characterized the Bush Administration's official position in the intensifying international negotiations around global warming, although the administration was internally divided on the issue. Presidential science advisor Allan Bromley thought the problem serious, convening a number of White House conferences to discuss it (Bromley 1994). But Bush's influential chief of staff, John Sununu, remained among the skeptics. At one point, after challenging the scientific basis of global warming concerns based on his reading of a <u>Newsweek</u> report on the subject, Sununu requested — and received — a one-dimensional NCAR climate model to run on his personal computer, though it is unclear whether he actually used it (Washington 1990). In the end, emphasizing scientific disagreement and describing the available evidence as insufficient, Bush avoided policy action on the issue. He proposed, instead, a 60 percent increase in spending for climate change research.

The Bush Administration's moves to divert attention from anthropogenic climate change, and from environmental issues generally, resulted in highly unfavorable national and international reactions, peaking around the June 1992 UNCED. Bush initially resisted the Framework Convention on Global Climate Change, eventually signing only a weaker version of it, after the "removal of unacceptable clauses" (Bromley 1994). Later that year, Democrat Bill Clinton defeated Bush in general elections.

With the pro-environment Al Gore as his Vice President, Clinton attempted to shift the emphasis from further research to national policy responses, particularly in the US position in international negotiations. The new administration's policy was also shaped by Clinton's choice of John Gibbons as Science Advisor; as former Director of the Congressional Office of Technology Assessment (OTA), Gibbons had provided Congress with scientific input about climate change policy decisions (Office of Technology Assessment 1993). The Clinton Administration demanded new levels of policy relevance and accountability for all science, including climate research. It 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). A 1993 "Climate Change Action Plan" proposed $1.9 billion in "new and redirected" spending to help match the European commitment of returning national greenhouse gas emissions to 1990 levels by 2000, but attempted to do so primarily by "leveraging" private investment rather than via binding regulations. Only at the 1996 Conference of Parties to the FCCC (COP-3) did Clinton allow Tim Wirth, Undersecretary of State for Global Affairs, to announce that the US would now support binding targets and timetables for emissions reductions. At this writing, the US has agreed to sign the Kyoto Protocol (COP-4; see

Jamieson, this volume). However, since ratification by the Senate is required for the treaty to enter into force, few interpreted this as more than a symbolic gesture.

Thus despite having devoted considerable energy to the issue, after six years as President, Clinton's achievements on climate policy remain rather modest. USGCRP budgets have grown since 1992, and the program has increased its emphasis on integrated assessment. A substantial Climate Change Technology Initiative (about $1 billion/yr.) focuses research and development efforts on renewable energy sources and energy-efficiency programs. Yet Congressional resistance to even moderate action on climate change has, if anything, increased.

Most of the factors discussed so far came into play in producing the US "policy gridlock," which continues into the present (Skolnikoff 1990). First, policy entrepreneurs — using climate issues to promote their research agendas — had created a wide variety of climate programs over several decades, producing a correspondingly wide (and confusing) range of research results. Second, the entrenched heritage of individualism in research planning led to competing, uncoordinated research agendas. Long after the establishment of the comprehensive USGCRP, integrating these — especially the quite different agendas of satellite data collectors and climate modelers — into a common project remained problematic (National Research Council 1995). Third, the dearth of programs specifically designed to produce policy-relevant results, such as regional and local impact predictions and cost-effective policy options for mitigation, left policymakers with little to do besides call for more research. Finally, and most importantly, the pluralist character of American politics ensured that an issue as complex and far-reaching as climate change would be debated for many years, with all parties probing the limits of scientific knowledge.

From the mid-1980s on, the Reagan and Bush Administrations' solution to tensions between increasing pressure for action on global warming, on the one hand, and their desire to stem the growth of environmental regulation and prevent a possible carbon tax, on the other, was continually to expand budgets for basic research. While it succeeded in holding the line against policy action, this strategy somewhat ironically produced the world's largest global change research program — one on which all others rely, especially for (very expensive) satellite data, and which has served as the basis for the ever-increasing momentum of the issue on the global political scene.

Climate Change as Ideology

Perhaps the most interesting feature of the US climate science/policy interface in the 1990s is the degree to which the climate change issue has become ideological. Contests between warming "hawks" and "doves," as they dragged on over many years, became aligned with generic positions on environmental policy and, especially, with Republican and Democratic party lines. The credibility of scientists became a murky problem which much of the general public felt incompetent to resolve; climate change became a symbol, something in which one either believed or did not. This feature distinguishes the US scene from those of most European nations, but aligns it with some Third World positions (such as China's), where the issue has become little more than a bargaining chip in a struggle to gain recognition and benefits from the developed world.

An industry-led greenhouse backlash

The ideological character of the controversy owes much to deliberate strategy on the part of greenhouse stakeholders. An industry-led "greenhouse backlash" reached its peak in the first half of the 1990s (Klineberg 1997). The backlash marked an ideological period in US climate politics which continues at this writing. Its beginning is frequently traced to a 1989 Forbes magazine article (Brookes 1989). According to a spokesperson for the Global Climate Coalition (GCC), the movement originated because

> [i]ndustry realized, especially coming out of the clean air debate here in the US and the Montreal protocol discussions and debate, 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 [the global warming] issue. …So we were formed in 1989 to get onto the crux of the issue before it got too far ahead of industry... (Eric Holdsworth, interviewed by ML, 22 November 1995)

The GCC is an umbrella group within the National Association of Manufacturers formed by a range of fossil fuel producers and fossil-fuel-dependent industry groups; it has supported scientific "contrarians" and promoted their work.

Another dominant player on the industry side is the Western Fuels Association, a $400 million non-profit 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. Western Fuels' annual report for 1993 explains its rationale for attacking global warming theories:

> We experienced a loss in 1993. ...But for the climate change debate and the Midwestern flooding that disrupted coal deliveries, we would have operated in the black…. With the exception of the National Coal Association, 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 (Western Fuels Association 1993, 5).

Thus, in a section titled “Balancing the Argument Over Global Climate Change,” the organization describes its “positive and pro-people vision” of national energy and environmental policy. The annual report goes on to note that as a 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” (Western Fuels Association 1993, 13). Among them were some of the most vocal: Patrick Michaels, Robert Balling, and Fred Singer (Balling 1992; Michaels 1992; Singer 1998).

One strategy was to picture the effects of increasing CO2 concentrations as benign or even beneficial to human beings. In 1991, Western Fuels financed a videotape on carbon dioxide’s effects on plant growth, The Greening of Planet Earth, at an estimated cost of $250,000. The production, featuring appearances by eleven scientists, was said to be influential in the Bush White House and also appreciated within the governments of OPEC (Gelbspan 1995, 34). 6,000 copies of the video were in circulation by mid-1992, and it had been broadcast 362 times in 61 media markets. 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).

Another player in the greenhouse backlash was the George C. Marshall Institute, a conservative think tank established in 1984 to influence opinion and policy. According to its literature, the Institute “provid[es] policymakers with rigorous, clearly written and unbiased technical analyses on a range of public policy issues” (George C. Marshall Institute 1998). However, Marshall Institute analyses generally favor unregulated markets, improved military technology, and nuclear power, while opposing environmental regulation. During the Reagan Administration, the Marshall Institute was concerned to promote the Strategic Defense Initiative, but it has since turned to climate as a major focus, forming perhaps the most influential, and certainly the most prestigious, faction among US "contrarians." The Institute — part of what Sara Diamond calls the "conservative labyrinth" (Diamond 1995) — has no parallel in the liberal camp (Ricci 1993, 168).

Building on its Reagan Administration ties, the Marshall Institute sought, and achieved, particular influence over the Bush Administration. Asked about the conclusions of the first IPCC report one week before the Second World Climate Conference in 1990, Bush replied, "My scientists are telling me something very different" (Rowlands 1995, 80), referring to a 1989 Marshall Institute study (Jastrow et al. 1989). The 1992 Marshall Institute Programs and Initiatives informed its readers that their 1989 report "dominate[d] White House thinking" on climate change and "provided the foundation for the [Bush] Administration's resistance to scientifically unjustified limits on carbon dioxide emissions" (Atmosphere Alliance 1995, 17).

Blurred boundaries between science and political action have not been limited to the contrarian camp. Michael Oppenheimer, atmospheric physicist and chief scientist of the Environmental Defense Fund, publicly acknowledged having used extreme language to raise public awareness. In Dead Heat: The Race Against the Greenhouse Effect, co-authored with Robert Boyle (Oppenheimer and Boyle 1990), Oppenheimer 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" (cited in Cohen 1995, 29). Oppenheimer later explained that he had been "unduly gloomy" about the populace.

Climate scientist Stephen Schneider, notorious for outspoken views on the importance of near-term action on climate change, once made a similar comment — for which he was often attacked, even by mainstream scientists:

> 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 caveat that scientists should not have to make the choice between honesty and effectiveness.

Congressional handling of the Kyoto Protocol

Partisan feuding within the House Science Committee (see discussion below of the Scientific Integrity hearings) has subsided since the 104th Congress. As Harlan L. Watson, Staff Director of the House Subcommittee on Energy and Environment, put it: having been in the legislative minority for so long, the Republicans who took power in the 104th Congress had grown accustomed to "throwing grenades" rather than engage in more constructive negotiations (interviewed by ML, 5 August 1998). Republicans generally toned down their style after 1996, when the strident anti-environmentalism associated with their 1994 "Contract with America" boomeranged back against them.

Recently, Republicans have become concerned that the EPA may try to enforce the Kyoto Protocol "through the back door," before its ratification in the Senate. One reason was a leaked memo ("option paper") from the EPA which suggested that the agency might have jurisdiction to regulate carbon dioxide if they defined it as a pollutant. (Given that the vast majority of atmospheric carbon dioxide is produced by natural processes, this reframing of the greenhouse issue would have been difficult to defend.) Republicans already mistrusted the EPA due to the close relation between its director, Carole Browner, and Vice President Gore; Browner is said to have helped write Gore's book Earth in the Balance (1992), a book which has helped position Gore as a national leader in environmental politics. During the summer of 1998, Republican representatives sought to include in the 1999 appropriations bill a clause prohibiting the EPA from using government funds "to develop, propose or issue rules or regulations or decrees or orders for the purpose of implementation or in contemplation of the implementation of the Kyoto Protocol" (US House of Representatives 1998, H6219). This would, in effect, have constituted a gag order preventing the EPA from engaging in any educational activities concerning human-induced climate change. It could also have prevented the EPA from discussing development of alternative ("green") technologies. At the last minute, however, an amendment was adopted to exempt educational activities from this prohibition.

The exchanges around this bill suggest the fears and suspicions of a substantial number of Congressional representatives concerning the science and the international negotiations related to global warming. One called the Kyoto Protocol "anti-American" because, in his view, it would impose "costly penalties on Americans, while allowing many countries, many Third World countries to "continue to pollute our environment at will" (US House of Representatives 1998, H6219). Expressing mistrust towards climate scientists studying global warming, another suggested that scientists have promoted the issue because it is "profitable" for them to do so, and suggested that only the US will be bound by the Kyoto Protocol (US House of Representatives 1998, H6221).

Science *as* Politics: Models vs. Data in the 1990s

The enrollment of scientists in larger political and ideological agendas is hardly unique to the climate change issue; indeed, as mentioned above, it is a standard feature of US environmental politics. In the final section of this chapter, we present several representative episodes in recent US climate politics. These serve to illustrate the range of actors and the nature of debate over climate policy in the 1990s. To a large degree, issues about the reliability of model-based climate projections, the relative trustworthiness of different forms of data, and the nature of uncertainty — rather than pragmatic questions of whether and how to mitigate against anthropogenic climate change — have dominated these debates. One way to think about this is that debates about science serve as proxies for absent, or muted, political discussions (Jamieson 1990; Jamieson 1991). Another one, developed by the science-studies community, is that in the United States context the assumed boundaries between science and politics tend to collapse when policy decisions hang heavily on uncertain science.

The Scientific Integrity Hearings

In 1995, the US House of Representatives Subcommittee on Energy and Environment convened a series of hearings entitled "Scientific Integrity and the Public Trust." Chaired by Rep. Dana Rohrabacher, the hearings were part of a sweeping attack on established Federal environmental policymaking techniques by the 104th Congress's newly-elected Republican majority. (This was the self-styled "Republican Revolution.") Each hearing addressed a particular environmental issue where "abuse of science" was alleged to have occurred: climate change, ozone depletion, and dioxin regulation.

In each case, the challenge took a similar form. Scientific witnesses of the high-proof school were called (as well as others). Some of them, such as Patrick Michaels and S. Fred Singer, testified that empirical observations failed to bear out the theoretical predictions of the science "establishment" — theories embodied, at least in the cases of climate change and ozone depletion, in computer models. These "skeptic" scientists went on to claim that the observational data failed to confirm the models. Many, including Michaels, Singer, and Sallie Baliunas, also claimed that their interpretations of observational data, and/or their own alternative theories or models, had been systematically ignored by the science establishment (e.g., in the case of climate change, by the IPCC). This establishment's self-interest in maintaining government funding for its research was alleged to be among the corrupting influences leading to this deliberate suppression of "sound science."

"Sound science," in this context, was the phrase used by Republican representatives to promote a new set of standards in science-for-policy: near-absolute empirical confirmation before action.

> Rep. Doolittle, in his prepared statement for the ozone hearing, stated that "sound science must be the basis for all future decisions we make on this important issue." In seeking to clarify the definition of sound science, Ms. Rivers asked "...[W]hat would you consider to be sufficient evidence for action to be taken in this area?" Mr. Doolittle responded, "I think we need a clear scientific conclusion that there is a definite cause for the problem and that so-called problem is producing definite effects. Theories or speculation about it are not sufficient. We need science, not pseudo-science. I think we've been in an era of pseudo-science where these dire consequences are portrayed in order to achieve a certain political objective." Similar statements were made by other Members in the global change hearing with respect to projections from computer models and in the dioxin reassessment hearing with respect to choices of models of dioxin receptor activity (Brown Jr. 1996, Section IV.D).

The "sound science" slogan referred directly to high-proof standards: "science programs must seek and be guided by empirically sound data" rather than theory or models. Other groups, such as the Union of Concerned Scientists, later adopted the same phrase — with a quite different meaning — in an attempt to block Republican co-optation of the term (Union of Concerned Scientists 1996). In a report on the hearings Rep. George

W. Brown Jr., the committee's ranking minority member, accused the Republican majority of a "totally unrealistic view both of science's present capabilities and of the relationship between data and theory in the scientific method." This approach to science, he said, "can lead to near paralysis in policymaking" because it requires an "impossible standard" of certainty.

This strategy of debating the standard of scientific proof in a political forum, as a way of preventing or delaying issue closure, has a long history in US environmental politics. The heavy reliance of climate science on theory — and especially on computer models, intrinsically untrustworthy in the eyes of many — renders it particularly vulnerable to this kind of attack.

The Chapter 8 Controversy

On June 12, 1996, just days after formal release of the IPCC Second Assessment Report (SAR), the Wall Street Journal published an op-ed piece entitled "A Major Deception on Global Warming." The article, by Frederick Seitz — a respected physicist, President Emeritus of Rockefeller University, and (not coincidentally) Chairman of the Marshall Institute's Board of Directors, accused some IPCC scientists of the most "disturbing corruption of the peer-review process" he had ever witnessed (Seitz 1996). Seitz's proclaimed distress stemmed from the fact that the lead authors of the SAR's Chapter 8 had altered some of its text after the November, 1995 plenary meeting of IPCC Working Group I (WGI), in Madrid, at which time the chapter was formally "accepted" by the Working Group.

Chapter 8 dealt with the question of whether models and data together can yet support conclusions about whether climate change is occurring ("detection") and how much of this change, if any, can be attributed to human activities ("attribution"). The chapter was the source of the now-famous IPCC statement that despite large remaining uncertainties, “the balance of evidence suggests that there is a discernible human influence on global climate.” Quoting several sentences deleted from the final version of Chapter 8, Seitz argued that the changes and deletions “remove[d] hints of the skepticism with which many scientists regard claims that human activities are having a major impact on climate in general and on global warming in particular.” According to Seitz, since the scientists and national governments who accepted Chapter 8 were never given the chance to review the truly final version, these changes amounted to deliberate fraud and “corruption of the peer-review process.” Not only did this violate normal peer review procedure, Seitz charged; it also violated the IPCC’s own procedural rules.

The Wall Street Journal op-ed set off a lengthy chain of exchanges lasting several months. The main participants in the public controversy were Seitz, Chapter 8 lead author Benjamin Santer, other Chapter 8 authors, the Co-Chairmen of the IPCC (Sir John Houghton and Bert Bolin), and climate-change skeptics S. Fred Singer and Hugh Ellsaesser. In a letter to the Wall Street Journal, Singer wrote that Chapter 8 had been “tampered with for political purposes.” The IPCC, he claimed, was engaged in a “crusade to provide a scientific cover for political action” (Singer 1996). Semi-privately, in electronic mail exchanges involving many additional participants (and widely copied to others), the debate became intense and sometimes quite bitter. Both the public and the private exchanges themselves became objects of further press reports, covered in the scientific journal Nature and widely disseminated by the news wire services (Masood 1996). As they continued, debate spread from the initial issues about peer review and IPCC procedure to include questions about the validity of Chapter 8’s scientific conclusions. Contrarians claimed that Chapter 8 dismissed or ignored important scientific results that disconfirmed the global warming hypothesis. They argued that the allegedly illegitimate changes to Chapter 8 made this problem even more acute (Edwards 1987).

Tellingly, this controversy was virtually ignored in Europe, where environmental policy controversies are less commonly channeled through scientific debates. In the US context, it illustrates two other common strategies for using climate science as a form of politics: first, challenging scientists' handling of formal procedures, and second, invoking what are essentially epistemological issues about data/model relationships. The accusations about failure to handle peer review correctly are serious, but their tenor in this case — raised in the Wall Street Journal rather than within the science community — marked them as a legalistic move (but see the section on "The certainty trough" below); no IPCC scientists ever objected to the alleged breach of procedure.

The Microwave Sounding Unit Data

The basis of contrarian claims in both the scientific integrity hearings and the Chapter 8 controversy was the microwave sounding unit (MSU) satellite data. In their "raw" form these data, collected since 1979, apparently show a slight cooling of the troposphere (lower atmosphere), at an apparent rate of about –0.06°C per decade. They are contradicted by radiosonde data from the same period, which show an average warming trend of about 0.1°C per decade since the 1950s.

A closer look at the MSU data sets, along the lines suggested by Suppe and Norton (this volume), reveals a kind of model/data symbiosis. Satellites cannot read lower-atmosphere temperatures directly. Instead, tropospheric temperature readings are created from top-of-atmosphere sensing records by means of models of atmospheric structure and chemical constituents. Furthermore, efforts to reconcile MSU and radiosonde data sets largely succeed "if the diverse response of the troposphere and surface to short-term events such as volcanic eruptions and El Niño are taken into account." (Both of these events occurred several times during the period of MSU records.) "After adjustment for these transient effects, both the tropospheric and surface data show slight warming (about 0.1°C per decade for the troposphere and nearly 0.2°C at the surface) since 1979" (Houghton et al. 1996, 27-28). These adjustments are carried out with models. In addition, recent research suggests that instrument differences among satellites, orbital drift, and problems in the radiosonde data (caused by such factors as changes in launch site locations during the observation period) may account for much of the discrepancy (Hurrell and Trenberth 1997; Wentz and Schabel 1998).

Skeptical opinions based on MSU records thus rely on data that are at least as model-bound, and probably more so, than those taken from the radiosonde networks (which measure temperatures by direct contact with the atmosphere at many altitudes). There is a certain irony here, since one might have expected proponents of "sound science" based in empirical data to place their greatest trust in the direct instrument readings.

Climate modeling as politics

Thus in US climate policy debates, scientists frequently stand accused of overreliance on models. Yet when we (PE and ML) ask modelers about the tendency to mistake models for reality, we usually receive the answer that modelers are in fact the first to see their weaknesses. Instead, they continue, it is those who <u>use</u> the models for policy purposes who interpret model output too literally and uncritically.

Still, community norms, pride in one's product, and the need for continued funding can discourage open recognition of the shortcomings and uncertainties in the models. Science-studies scholars have frequently noted this phenomenon. For example, Shackley and Wynne (1995) have described how the practice of general circulation modeling is "black-boxed" as results are presented to others, in the sense that the uncertainties and indeterminacies of models are hidden from those not involved in building them. In addition, the strong connection of modeling with political debate tends to encourage strong claims-making on behalf of the models. Speaking to a roomful 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 connection also shapes the practices of the US environmental movement and its opposition in the 1990s. As noted above, these communities highlight (respectively) the fit and the lack of fit between the models and data. Thus some climate modelers, environmental activists, and sympathetic media channels describe the models as “sophisticated,” as “based on a realistic foundation,” and as “increasingly accurate assessments of the future” that “correspond” to observations and with increasing accuracy “mimic” the real world (Masood 1995; Nixon 1995; Jones 1995). By contrast, the environmental opposition, including contrarian scientists and sympathetic media channels, foreground the limitations and uncertainties of models. Thus a 1995 Economist article claimed that 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., writing for The Wall Street Journal, used even stronger language in a 1993 editorial. Associating climate models with “unsound science,” he called them “hypothetical disaster scenarios” that were part of the “now-fading outbreak of climatic doomsterism” led by Mr. Gore and his “crusading” crowd of “hysterics.” “[A]s climatologists begin gazing up from their computer models at the real world,” he claimed, “global warming looks like a flash in the pan” (Jenkins Jr. 1993).

In the US context, then, modelers thus cannot avoid being drawn into political debate. The by now ritualized caveats about uncertainty and limitations in scientific presentations of model results are routinely stripped away as claims are translated into the political arenas, or else they are amplified by opponents in order to deny the legitimacy of model-based knowledge. Mackenzie coined the term “certainty trough” to describe this phenomenon, in which the perceived certainty of knowledge claims is greatest at a moderate distance from the actual site of knowledge production (Mackenzie 1990) — such as among the policy “consumers” of model outputs. In the climate case, this is a one-sided view of the situation, since for contrarians it is instead the uncertainty of knowledge claims that peaks.

At the Millennium: US/European Competition in Climate Modeling

Since modeling is the essential scientific tool by which the climate problem is defined, the competition to build the “best” climate models has political dimensions beyond the obvious contest for national prestige. As the greenhouse issue has risen in prominence outside the US, European countries — none of which can hope to match the vast American investment in the field overall — have nevertheless discovered ways to compete head-on in the narrower field of climate modeling. Europe has always exercised administrative leadership in the IPCC. More recently, however, it has also come to dominate the field of climate forecasting.

Climate modeling efforts in the US are dispersed, with numerous, sometimes competing modeling efforts — sometimes even within a single institution — each involving a handful of people or less. By contrast, European countries have tended to establish a single national center, pooling the efforts of many scientists in these institutions. The scientific advances of such national centers as the UK’s Hadley Centre (see Shackley, this volume) and Germany’s Max Planck Institute (see Krück, Borchers, and Weingart, this volume) have produced pressures for greater centralization and coordination of modeling efforts both within and between the US modeling centers. NCAR, for example, came under attack in 1994-95 by people who thought its four or more independent modeling efforts at NCAR ought to be consolidated into one large effort. NCAR responded by introducing a single, integrated earth system modeling effort, the Climate System Model (CSM). Management considered such integration necessary if NCAR was to be competitive in the field.

This development has also served to secure ever greater resources for climate modeling, at the expense of other scientific projects. Currently, acquiring and operating the supercomputers at NCAR costs $11-12 million annually. Different scientific groups write proposals for experiments on the computers, producing a competition for computer time among NCAR scientists (including many who study areas of atmospheric science other than climate modeling). 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 now uses about half of NCAR's total available computer time, a huge increase compared to earlier, less complex models.

In spite of such efforts, the US is losing ground in the international competition. As recently as 1994, the US was by most accounts the leader in climate modeling, but many scientists now agree that the Hadley Centre and the Max Planck Institute German national climate centers have usurped this position. As a result, the 1995 IPCC report relied heavily on the Hadley and Max Planck models, reflecting the new prominence and leadership of these two centers. One reason for the European successes in climate modeling is their greater level of coordination and concentration, exercised through top-down management with explicit policy objectives. As a result, even though the US spends a total of about $120 million annually on climate modeling, the Max Planck Institute's roughly $10 million budget produces better results.

Although NCAR has, as mentioned, attempted to emulate the European style, the US as a whole has not opted not to follow this pattern. Indeed, it would probably be impossible to do so in the American context. The reasons include the fact that the total body of modelers in the US is much larger, more diversified, and accustomed to a greater level of individual freedom in choosing research directions. While this diversity of effort can bring unforeseen benefits, it is hurting the US to the extent that it spreads total national resources more thinly. In a science where computer power is centrally important, this can have profound consequences. Having near-total national resources for climate modeling centered in one place has given the British and German national centers a conspicuous edge over US modeling groups. Where US scientists sometimes had to obtain computer time at institutions other than their own — even some NCAR models, for example, are now run on Dept. of Energy computers at Los Alamos National Laboratory —European modelers had easy access to computers at their own institutions. Therefore, European modelers have been able to carry out ensemble runs (multiple runs of the same model for purposes of sensitivity analysis), while this has rarely been possible for their US counterparts, due to limited computer resources. While US modeling centers nevertheless conceive themselves to be on the cutting edge (e.g., at this writing NCAR presents its CSM as among the best, if not the best, in the world), there is concern about how long the US will be able to maintain global leadership in areas that depend on high performance computers.

A major blow to the US climate modeling community came in 1996, when National Science Foundation was denied procurement of four NEC SX-432 supercomputers for NCAR as the result of intervention by a Congressional representative from Wisconsin — home of Cray Research, NEC’s major competitor for the supercomputer contract. The NEC acquisition would have been the first purchase of a Japanese supercomputer by a US government agency, representing a setback for Cray, which was already in financial difficulty. Because of this setback, which still has not been recouped, the supercomputers owned by the NSF have fallen from a ranking among the top 10 supercomputers in the world to 123rd in the world. At present, a simulation that would tie up the NCAR machine for eight continuous months can be done in a week and a half in Britain (Robert Corell, interviewed by ML, 12 August 1998).

US failure to maintain its formerly large lead in climate modeling is also strongly connected with the policy gridlock of the 1990s. Leaders in the US global change science establishment realize this, and are presently building resolve to superimpose on US climate research some “high-end, very focused institutional efforts,” despite resistance from many segments of the science community (confidential interview). During the summer of 1998, the DOE proposed the Accelerated Climate Prediction Initiative (ACPI), a plan to bring “the promise of simulation to the challenge of climate change” using new, ultra-fast computers developed under the DOE’s Accelerated Strategic Computing Initiative (ASCI). The ACPI could lead to teraflop capability at NCAR by the year 2002, even tens of teraflops by the year 2005. As an NCAR director put it: “We would, of course, be delighted by that. Our problem is: what the hell do we do between now and 2002, while our friends overseas are running ahead?” (Bill Buzbee, interviewed by ML)

Conclusion

The received version of the history of US climate science and politics describes an orderly, logical flow from science to politics, on the standard model of issue identification by science, followed by policy analysis, leading to policy action. On the surface, at least, this perspective has an undeniable logic. The trajectory of government interest in global climate change moved from science agencies (NSF, NOAA) in the 1960s to policy analysis and regulatory agencies (DOE, EPA, OSTP) in the 1970s. Congressional action followed. In the 1980s, as the issue moved into the international arena, further legislation was passed, greatly expanding the scope of scientific research, weakly promoting policy modeling, and involving a large number of Federal agencies. The White House and the

Department of State becoming involved as negotiations toward the FCCC began toward the end of the decade (Ausubel 1989).

But the version of this history offered here, influenced by the science-studies perspective on the science/policy interface, adds new dimensions to this linear, science-driven approach. In the 1970s, for example, we have shown that legislative action did not blindly follow the lead of the scientific internationalists who promoted climate as a public policy issue. Instead, the first US climate legislation was nationalist in orientation and emphasized user services rather than climate change research. In the 1980s, international political events such as concern about nuclear winter, the ozone depletion treaty, and the "apocalypse gap" left by the Cold War's end played a significant role in preparing the ground for global climate change to become a mass issue. Ideological contests over environmental issues between a Democratic Congress and Republican Presidents (Reagan and Bush) and international pressures for action raised the profile of the global warming issue as much as any particular scientific results. This kind of mutual influence is what science-studies scholars call the "co-production" of science and society (Jasanoff et al. 1998).

In the US context, the co-productionist view emphasizes the importance of mutual trust-building for meaningful political action. In a pluralist political culture with a similarly pluralist approach to science, the authority of scientific knowledge will never be recognized until that knowledge can pass political tests similar to those exacted of every interest group. These include the recognition and assimilation of multiple viewpoints, especially those of economic stakeholders, and the opportunity for public scrutiny of both results and methods. This is a slow, often painful process that can result — as it has in the last decade — in an apparently permanent gridlock.

Compared with European nations, the US has been extremely slow to adopt meaningful climate policy. This has everything to do with the differing relations of science to politics described in detail by the following chapters. Yet in its form if not its content, US climate politics resembles nothing so much as climate politics in the international arena. There, stakeholder recognition and public scrutiny of science have been seen as crucial to the policy process. The process of building mutual trust is a long one that passes necessarily through a stage of cacophony. The history of the US climate science/policy interface suggests that this stage will continue for some time to come.

Notes

[1] These algorithms simulate the balance between incoming (solar) and outgoing radiation. This exchange determines the Earth's average temperature, and is heavily influenced by the atmosphere's chemical composition, especially "greenhouse" gases such as water vapor, CO2, methane, and others.

[2] In a letter to Secretary of Commerce Juanita Krep, the bill's sponsor, Rep. George E. Brown Jr., wrote: "I see climate services and impact assessments… as the real areas of pioneering in this Program. It is a chance to use technology to increase the socioeconomic resilience of our society" (Hechler 1980, 988).