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Abstract

One step toward explaining the rise of climatic change as a political issue is the recognition that interest in it has been nurtured through the political activities of scientists. Beginning in the 1950s, entrepreneurial members of the scientific establishment forged links between their research and the public and private interests of political actors who might be in a position to support them. These entrepreneurs adopted three major strategies between 1957 and 1974. First, they attempted to develop autonomous domestic and international atmospheric science institutions in which they could direct research funding to their own priorities, which included research on anthropogenic climatic change. They justified their research in terms of its contribution to intellectual progress. Second, they took advantage of jurisdictional disputes among some agencies and Congressional factions with regard to weather modification policy to recruit allies. Their support for one side in the dispute was connected to its commitment to their research. Finally, they adopted the rhetoric of the environmental movement to make the issue appear more relevant and important to a different set of agencies. None of these strategies was particularly successful; alternative strategies might have been more fruitful. While their activities may have seemed natural to the actors involved, the paper suggests that more deliberate political strategizing on the part of scientific entrepreneurs may be both plausible and useful.

Strategies of Research Policy Advocacy: Anthropogenic Climatic Change Research, 1957-1974*

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1. The Need for Advocacy and the Choice of Strategies

In his recent book Global Warming, Stephen Schneider provides a gripping vision of a future he believes is likely, perhaps towards the end of the next century. Water resources will be scarce and distributed differently than they are now. Rising sea level will slowly require costly protection or abandonment of coastal property. Farmers will have to adapt to changing precipitation and temperature conditions; where they cannot, as in many poor countries, crops will fail. Economic growth will be inhibited by forced, unproductive investments required to maintain housing and industrial facilities in a more variable and dangerous climate. Habitats of many species will be threatened. Rates of some diseases and heat-related death will rise. [1]

The climatic conditions that account for this grim scenario are the projected results of the absorption and re-radiation of infrared wavelengths by carbon dioxide ($CO_?$), methane, chlorofluorocarbons (CFCs), and other "greenhouse" gases. These gases are produced by everyday activities, such as electricity production and rice cultivation, in both the industrialized and non-industrialized nations. Before atmospheric scientists rediscovered the greenhouse effect hypothesis in the 1950s,[2] most considered these gases to be harmless, particularly compared to more visible pollutants, like smoke and smog.

The consequences of the greenhouse effect, if it occurs, will be global; greenhouse gases mix with the existing atmosphere in a period of months. Their effect on climate, however, is slow and indirect, depending, for example, on the responses of the oceans and clouds. While scientists seem to agree that warming is the most likely result of an atmosphere that has a higher greenhouse gas composition, they have no direct evidence to support that hypothesis. The global climate is a very large, complex system, and the mechanisms of its operation are poorly understood. Any anthropogenic influences (i.e., those having human sources) that may be acting on it are camouflaged by spatial and temporal variations in the weather. Since climate is defined by a range of values for variables like temperature and precipitation that is so wide that large swings in them are not unusual, random noise inhibits detection of the greenhouse effect, even at the grossest level. More fine-grained prediction of the distribution of "winners" and "losers" from the greenhouse effect, in terms of geographical regions, economic sectors, and political entities, is not possible at this time, despite intense speculation.[3]

The threat posed by the greenhouse effect, then, is massive in scope, but distant in time, hard to detect, global, a gradual and uncertain process, and one that will probably vary widely by location. As Mancur Olson, James Q. Wilson and others have argued, an issue with characteristics like these is unlikely to be the focus of political action. [4] Even though many institutions and individuals probably have a very large stake in the maintenance of the climate, they have more pressing and concrete concerns that tend to dominate their political attention. In addition, some actors may be threatened by the discovery of the exact nature of the threat, since such knowledge might bring pressure to change ingrained habits and profitable ways of doing business. Nonetheless, the issue has risen on the public agenda over the past thirty years and has provoked a modest institutional commitment from Federal agencies.

My research suggests that one step toward explaining the issue's rise is the recognition that interest in the greenhouse effect within science and government has been nurtured through the political activities of scientists. Scientists worked to develop support for research on anthropogenic climatic change not only by repeating and explicating claims about the magnitude of the danger described above, but by forging links between their research and interests that were more certain and accepted by those who might be in a position to support them. These interests have been both public, such as interest in the advance of basic scientific knowledge and interest in general protection of the environment, and private, such as an agency's interest in extending its control over a particular policy domain.

It is not surprising that scientists, who have the expertise to assess the likelihood of climatic change and the extent of its consequences and who can make a stimulating and good living doing so, have been the main advocates of the issue in policy circles. The notion of policy entrepreneurship, exemplified by those activities which are the focus of this paper, is well-established in the social study of American science. Henry Etzkowitz argues that entrepreneurial activity has been central to post-World War II scientific culture in the U.S. [5]

Dan Greenberg calls "evangelism" an essential characteristic of American science. [6] From an anthropological perspective, Karin Knorr-Cetina describes scientists' constant awareness of "resource relationships" in their practice. [7] Entrepreneurship is also well-accepted among the practitioners themselves; as John von Neumann put it, "the best way to get something done is to propagandize everybody who is a reasonable potential support."[8]

However, all policy entrepreneurs have limited time, energy, and resources. They must choose among many potential opportunities and possible strategies for recruiting allies. In this paper, which covers the period 1957 to 1974, [9] I argue that scientific advocates of research on the greenhouse effect (and other hypothesized mechanisms for anthropogenic climatic change[10]) adopted three major strategies to cultivate interest in this research. First, these advocates worked to develop autonomous domestic and international atmospheric science institutions in which they could direct research funding to their own priorities, which included research on anthropogenic climatic change. They justified their research in terms of its contribution to intellectual progress. Second, they took advantage of jurisdictional disputes among some agencies and Congressional factions with regard to weather modification policy to recruit allies. Their support for one side in the dispute was connected to its commitment to their research. Finally, they adopted the rhetoric of the environmental movement to make the issue appear more relevant and important to a different set of agencies. Greenhouse effect research funding was said to be a part of defending the public interest in the natural environment. These strategies were adopted roughly chronologically, but their implementation overlapped extensively.

I make the assumption, without specific documentation to support it, that the policy entrepreneurs in this case, primarily the administrators of laboratories and government agencies, loosely coordinated their activities regarding climatic change research. This assumption appears warranted by their small number, the large number of opportunities they would have had to discuss the issue, and the internal cohesion of the period's scientific elite, of which these advocates were members. The degree of coordination and the possibility that better coordination might have produced better results (from the entrepreneur's perspective) remain open issues. Whether these administrators conceived of these strategies as strategies is also not clear from the published record. [11] Their statements and actions may have seemed quite natural to them, particularly, as I discuss below, in their effort to pursue climatic change research in relatively autonomous institutions. One of the points of this paper, to which I return in the conclusion, is to suggest that deliberate political strategizing on the part of research policy advocates is possible, because alternative courses of action are often open.

After describing each of the strategies adopted to secure support for greenhouse effect research, I postulate counterfactual, but plausible, alternatives that might have been adopted. The objective of these exercises, imperfectly attained, is to evaluate the "returns" advocates achieved on their "investment" in each strategy. These "returns" included financial support and increased interest from higher levels of government, particularly potential future supporters of research. Both of these indicators were difficult to measure in the actual case, and their hypothesized levels in the counterfactuals are speculative. A "structured, focused comparison" [12] with another research program of the same period might provide additional insights into the actual and potential success of greenhouse effect research policy entrepreneurs. [13]

2. Context: Shrinking Globe, Advancing Technology, and the New Expert

The fate of policy entrepreneurs, like their economic counterparts, depends not only on their skills, brains, and guts, but also on the opportunities offered by their environment. Many larger events and processes influenced the activities of atmospheric research policy entrepreneurs and the responses to them during the late 1950s, 1960s, and early 1970s. The three subtle, long-term processes alluded in the heading of this section are important, but somewhat hidden, aspects of the entrepreneurs' environment that deserve some elaboration before I take up the main argument in the following three sections.

Marshall McLuhan coined the term "global village" in 1964.[14] International trade and cultural intercourse expanded rapidly in the postwar era; concern about global issues naturally expanded with them. The broadening of security, economic, and political affairs .provided a conceptual context that made fears about global environmental damage, including climatic change, seem increasingly plausible. The 1972 publication of Limits to Growth marked a watershed in the development of globalist environmentalism, and it summed up worries that had been developing, both popularly and scientifically, in the preceding decades. [15]

The scientific community, through its development of new indicators and its internationalist spirit, contributed to this process of global integration. More importantly, science aided technological change, the handmaiden of the globalization of political issues.

Many inventions have played a role; the atomic bomb's has been among the most important. The ability to quickly deliver weapons of mass destruction inter-continentally was developed and revealed to the public during the 1950s and 1960s. Global destruction became as plausible (or, rather, more plausible) than global integration. Even though the idea that humans can change the weather is as old as religion, the awesome power of atomic weapons surely made the notion of anthropogenic climatic change more viable.

The analogy between weather and climate modification and nuclear technology was commonly made in the U.S. John von Neumann's 1955 article (in Fortune) "Can We Survive Technology?" held that the unintended environmental effects of attempts to influence the weather (as well as the deliberate military use of weather warfare) constituted a threat comparable to that of nuclear weapons. [16] Policy-makers took the analogy seriously enough to propose legislation that incorporated the methods of regulating nuclear technology into the regulation of weather modification. Senator Clinton Anderson, for example, put forward a bill in 1966 that called for the provisions of the Price-Anderson Act, which restricts liability for accidents at nuclear power plants, to apply to weather modification operations. Atomic Energy Commissioner James T. Ramey took the analogy further by suggesting that licensing schemes for such operations ought to parallel those for nuclear power plants.[17]

Nuclear technology had more direct effects on climatic change research as well. Fallout from atmospheric nuclear testing was popularly believed to be a climatic change agent in its own right. For instance, George Kimble of Indiana University noted in a 1962 article:

> If there's one thing the farmers who live around me are agreed *upon*, it is that the weather is not what it used to be: it's worse .... If there's another thing most of them are agreed upon, it is the reason for these presumed changes: "The bombs are what done it."[18]

Concern about fallout led a number of agencies, including the Atomic Energy Commission (AEC) and the Department of Commerce, to investigate its dispersion in the atmosphere.[19] While scientists concluded that fallout did not affect the weather, research initiated in response to atmospheric nuclear tests led to new findings about other hypothesized climatic change agents, including greenhouse gases.[20] These tests provided a ready-made experiment for tracing the atmospheric and oceanic circulation of particular elements, such as carbon, by injecting a large, known amount of them at a specified time and place. Radioactive tracer research fostered the development of professional careers, like Lester Machta's, and laboratories, like the Geophysical Fluid Dynamics Laboratory (GFDL) of the Weather Bureau (originally in Washington, DC, and then in Princeton, NJ), that were important in the study of anthropogenic climatic change.[21]

At least two other technological developments of the period, satellites and computers, had notable effects on the efforts of scientists to understand and publicize the greenhouse effect. Satellites permitted much easier observation of very large-scale weather patterns than had been possible before. Although public concern about climatic change by no means outstripped that about the daily forecast, even casual viewers of television were exposed to the notion of continent-sized cloud movements. For atmospheric experts, the new vantage point was a windfall. Vast amounts of data that had been previously gathered by widely scattered ground-based observers (or had simply been unavailable) could now be gathered by space-based instruments.

The decreasing cost of computation allowed climate models, a key tool for the analysis of anthropogenic influences on climate, to become far more sophisticated. The theorized relationships among variables in the climate have become increasingly complex and interactive; tests of these theories require large amounts of data in order to be adequate. But the computing power needed to construct and manipulate a model rises rapidly as it becomes more detailed; the massive improvements made in mainframe computers during the 1960s and 1970s were a necessary condition for the great strides made in climate modeling. From the mid-1960s on, proponents of research on anthropogenic climatic change stressed the promise and importance of advances in computing and used model results to justify their program.[22]

A final contextual process that should be kept in mind in considering the body of the paper is the transformation of the political role of experts that has occurred throughout the industrialized world, and especially in the U.S., in the past three decades. The norms of science are sometimes said to include "organized skepticism;" scientists should refuse to state conclusions, at least to non-specialists, beyond what their data will unequivocally support.[23]

Samuel Hays, in his history of U.S. environmental politics, points out that this norm has been placed under great pressure by environmental problems that require extrapolation from limited evidence in order for policy to be made. In such cases, standards of proof and evidence may vary among scientists. Once committed, however, positions based on disputed evidence tend to harden.[24]

Predictions about climatic change require inferences from very limited data. As particular scientists came to support particular hypotheses about anthropogenic climatic change, they often came under attack from the upholders of the standard of organized skepticism. In response, they may have become more active advocates. Certainly, concern about the appropriateness of raising a public alarm about the greenhouse effect, given substantial uncertainties, has been a constant theme in discussions at high levels within the scientific community.[25]

The exact importance of the three processes discussed in this section -- the broadening of political horizons, rapid development of key technologies, and the growing politicization of experts -- in the history of greenhouse effect research is uncertain. Although they usually enter the narrative below indirectly, they nonetheless influenced the ability of advocates of this research to devise particular strategies, the commitment of these advocates, and the responses of other political actors to them. The three processes may be seen as enabling entrepreneurial activities, but they did not by any means guarantee entrepreneurial success, which was, in my interpretation, mixed.

3. Strengthening the Autonomy of Atmospheric Science

Although this case history is shaped by a larger context, including the processes discussed above, the action in it did not have an appreciable impact on those processes. Computers and satellites, for example, were not developed with climatic change research in mind. At no point in the period under consideration did climatic change garner much public attention; a count of stories on the topic, for instance, showed that no more than about 20 articles were devoted to it in the popular literature in any year before 1980.[26] No Presidents took any interest in it, so far as the published record shows. Although some of the research that the issue's advocates fought for required the use of oceanographic vessels, airplanes and satellites (Big Science by the standards of the day) the program was small compared to most government budgets, amounting to a few million dollars a year at most.

The greenhouse effect hypothesis was developed within normal science.[27] The question of human impact on the climate relates directly to the great puzzles of energy and element flows within and between the lithosphere, hydrosphere, atmosphere and biosphere that are a major part of the research agenda for all the earth sciences. In fact, the hypothesis was first put forth around 1900 by two independent scientists, Arrhenius and Chamberlain, with little success.[28] As Victor and Clark describe, it only received serious attention once it rested firmly on a theoretical consensus in meteorology and oceanography that developed in the first six decades of this century and on related experimental data.[29] The idea that the greenhouse effect could occur within several generations was first offered in the mid-1950s.

In the earliest years of government-funded research on the question, during the late 1950s and early 1960s, scientists primarily sought to explore it within the established atmospheric research program. Even though, as discussed below, they sometimes appealed for support on grounds that made practical sense to 1950s politicians, their strongest desire seems to have been to pursue the matter within institutions in which they could set the priorities and in terms with which they felt comfortable. This approach was typical of the larger scientific enterprise, exemplified most prominently by the postwar Kilgore/Bush debates over the governance of Federal support for basic research. Vannevar Bush, the spokesman for research scientists in the late 1940s, advocated peer review as the method for allocating research funding; his opponent, Senator Harley Kilgore called for more direct political oversight and the application of some political criteria, such as geographical location, in the process.[30] Like many of their predecessors and colleagues, atmospheric scientists interested in climatic change worked to create research management structures that put a layer of professional insulation between specific projects and political oversight.

Two studies are commonly considered to be responsible for the revival of scientific interest in the greenhouse effect, Gilbert Plass's study of CO_I's impact on the Earth's radiative balance and Hans Suess's finding that at least some CO_2 released from fossil fuels was not being absorbed by the oceans (as many scientists had believed). Suess and Roger Revelle termed the effect of rising CO_2 on climate "a large-scale geophysical experiment."[31] Similar metaphors, suggesting that the problem was a scientific curiosity, recurred in presentations for non-technical audiences. At a Congressional hearing in 1956, for instance, Revelle justified research into the CO_2 content of the atmosphere by saying that "we ought to adequately document" the great experiment. Plass stated: "We shall be able to test the carbon dioxide theory against other theories of climatic change quite conclusively during the next half-century."[32]

The research of Plass, Suess, Revelle, and others suggested that monitoring the CO_2 content of the atmosphere would provide important clues about the possible time scale of the greenhouse effect. The faster the CO_2 level rose, the sooner the greenhouse effect could be expected. This project was rather tardily, but uncontroversially, inserted into the program of the International Geophysical Year (IGY), 1957-58, at the behest of U.S. scientists.[33] Seven CO_2 measuring devices were prepared for this project, one emplaced at Mauna Loa, Hawaii, another in Antarctica, and five to be used on vessels;[34] the Hawaiian monitoring station has been maintained almost continuously since the IGY and has provided undisputed documentation that the great "experiment" is indeed going forward rather rapidly.[35]

Congress approved the funding of the IGY on a project-by-project basis in its appropriations for the National Science Foundation (NSF), but it generally deferred to the scientific community's priorities. Judging from the transcripts of oversight hearings, CO_2 monitoring seems not to have been an exception.[36]

The IGY was funded on an ad hoc basis with the explicit understanding that it would not be an ongoing program.[37] Other, more stable, atmospheric science research organizations were also established in the late 1950s and early 1960s. A program in atmospheric sciences was established by NSF in 1958. The National Center for Atmospheric Research (NCAR), funded through NSF, was founded in June, 1960. GFDL began operation about the same time. [38] Although it was a small part of their regular programs, research on anthropogenic climatic change was apparently supported by these organizations as a matter of course.

> For example, a 1962 report stated:
>
> "At NCAR a broad variety of scientific talent is engaged in an interdisciplinary effort to probe the large-scale dynamics and structure of the atmosphere with the eventual goal of determining what kinds of manmade influences can affect large weathern patterns. [39]

Later reports sporadically mention this objective, without particular emphasis; it seems to be part of the routine at NCAR. This institutionalized long-term interest paid dividends. For instance, NCAR's V. Ramanathan discovered the greenhouse effect of chlorofluorocarbons (CFCs) in 1975 and continues to be among the leading experts on the greenhouse effect today. Similar evidence demonstrates a minor, but continuing, interest in anthropogenic climatic change in other programs. [40]

Prominent proponents of the greenhouse effect hypothesis held leadership positions in the scientific community, including Revelle, director of the Scripps Institution; W.O. Roberts, director of NCAR; and Robert White, head of the Weather Bureau and its successors. [41] These individuals were therefore able to exert a strong influence on program development and to use discretionary funds to build and maintain programs at times as well. For instance, according to one source, the planning of the IGY CO_2 monitoring program was supported by the discretionary funds of the Scientific Committee on Ocean Research (SCOR) of the International Council of Scientific Unions (ICSU), which Revelle chaired.[42]

As this example suggests, American scientists were leaders in the effort to create relatively autonomous international atmospheric science institutions as well; these slowly took up research related to anthropogenic climatic change. For instance, in 1967 the World Meteorological Organization (WMO) and ICSU established the Global Atmospheric Research Program (GARP) under a Joint Organizing Committee (JOC) made up of scientists. GARP's research agenda was generated by U.S. scientists, especially M.I.T.'s Jule Charney. By the mid-1970s, GARP was making important contributions to the study of the greenhouse effect.[43] GARP might have gotten underway more quickly had scientists been confident that WMO was capable of carrying it out. ICSU and its constituent groups were reluctant to participate in GARP's planning in the early 1960s and only did so when they felt assured that WMO's priorities would not dominate those of the scientific community. [44] Although they seem to have gained some autonomy in this period, atmospheric scientists did not achieve a consensus on the very tricky and complex issue of the dynamics of climate and weather and human impacts on them. In fact, autonomy appears to have fostered the profusion of competing hypotheses about anthropogenic climatic change that marked the 1960s. In addition to fallout (see above) and greenhouse gases, at least nine other potential change agents received serious consideration: [45]

1) Particulates The most serious competitor to the greenhouse effect hypothesis was the idea that dust particles raised by agricultural and industrial activities changed the global albedo (reflectivity) enough to cool the earth. Reid Bryson argued that dust in the air (combined with the replacement of forest with desert) made the earth reflect more visible light and therefore convert less incident energy into heat. This hypothesis was received skeptically by greenhouse effect researchers, but despite their claims that it was disproven as early as 1974, it was still well enough regarded to dominate the climatic change portions of the 1979 Global 2000 report to the President. [46]

2) Rocket Exhaust Heat and water vapor injected into the highest part of the atmosphere by rockets stirred concern in the early 1960s, as the technology for launching objects into orbit matured. The WMO commissioned a panel of experts to consider this topic in 1965; the panel did not find a cause for immediate concern.[47]

3) Intentional Weather Modification Cloud-seeding programs regularly confronted opposition from those who felt that they had a detrimental effect on weather and climate. In one of the most significant of these instances, some 80,000 Quebecois signed a petition protesting weather modification activities in New York in 1967.[48] As discussed in the next section below, atmospheric scientists claimed that support of basic research would allow them to better assess the potential unintended consequences of weather modification.

4) Jet Contrails The hypothesis that jet flights, particularly of a large fleet of supersonic transports (SSTs), would bring cloudiness to normally clear polar regions was a source of scientific disagreement. Internal conflict within the National Academy of Sciences panel on weather and climate modification on this point resulted in a front page story in the New York Times in 1965. The controversy continued through the debate about Federal funding of the SST in the late 1960s and early 1970s.[49] (See section 5 below.)

5) Auto Exhaust Vincent Schaefer put forward the argument in 1966 that particles in auto exhaust would cause a rise in precipitation downwind from cities. This proposition received wide media coverage. [50]

6) Urban Heat Cities are warmer than the surrounding countryside. This "heat island" changes local weather patterns, an effect which gave rise to a multi-year NSF-funded study of the St. Louis area to determine if such local effects had regional or larger-scale impacts. [51] Cities are one of many large human-made land features that were hypothesized to influence weather and climate; others included irrigation works and reservoirs. [52]

7) Total Thermal Output The Soviet climatologist M.I. Budyko argued that, within the foreseeable future, the heat output of all human activities would be large enough to affect the global energy balance. Such an injection of heat would have an unpredictable effect on the climate. This notion was well-known enough to be included in an elementary economics text in a discussion of environmental limits to growth. [53]

8) Oxygen Depletion At the 1968 annual meeting of the American Association for the Advancement of Science, Lamont Cole's paper, "Can the World Be Saved?," argued that fossil fuel burning, marine pollution, and deforestation would cause a net drop in atmospheric oxygen content. Later measurements showed that such fears were unfounded. [54]

9) Oil Spills Charles Weir suggested that an oil spill in the Arctic Ocean could change the heat budget of the Arctic region by melting ice, since the oil would absorb light that the ice ordinarily would reflect. This region plays a major role in

determining the northern hemisphere's weather patterns, so such a change in the Arctic albedo could have large-scale consequences. [55]

These diverse hypotheses, and the resulting disputes within the scientific community about such basic results as whether human activities were likely to cause surface temperatures to rise or fall, seem to have confused the public. A Time magazine essay, "The Age of Effluence," in 1968, for instance, used the dispute between the warming, cooling, and oxygen depletion theories to exemplify the difficulty of obtaining useful scientific advice on environmental problems. "It seems undeniable that some disaster may be lurking in all this, but laymen hardly know which scientist to believe." [56]

Nonetheless, public confusion did not prevent real scientific progress through the endeavors of relatively autonomous atmospheric research institutions. Some of the proposed climatic change processes (like total thermal output) were shown to be negligible. Improved means for assessing hypotheses about climatic change (like better general circulation models) were developed. The greenhouse effect was largely supported by scientific findings, although its exact mechanisms became murkier.[57]

The results of the strategy of seeking autonomy, assessed in about 1965, are modest. While they had not actively sought allies outside the scientific community, advocates of increased research on climatic change had induced serious consideration of the issue within their own community, catalyzing the normal messiness that characterizes the early stages of research on a complex problem. But, due to a lack of money, researchers were unable to pursue all promising leads during this period; even a project with as high a scientific priority as the CO_2 monitoring program was starved of funds in 1964 and faced financial difficulties in other years.[58] Important topics, like the response of the oceans to a warmer atmosphere (as opposed to their ability to absorb $C0_2$), received no attention during the 1960s.[59] The close integration of climatic change research with the larger agenda of atmospheric science makes it difficult to determine the exact amount of money spent on this research, but the ad hoc IGY funding and the institutionalized support of atmospheric science centers were certainly insufficient to pursue the research program envisioned by Revelle, Roberts, and their colleagues.

One might wonder, then, whether more progress would have been made in greenhouse effect research in the 1960s, if scientists had called for a separate research program on the topic and brought Congressional attention to it more vociferously, using strategies described below, even though such a program might have been more carefully overseen than the actual institutions were overseen. By 1963, the data showed fairly clearly that the CO_2 level was rising rather rapidly and, after all, Revelle had noted in his 1957 testimony that a 20% rise in CO_2 (which could be expected by about 2000) could cause "considerable change" in the climate. [60] A 1982 retrospective by a high WMO official noted, "It is rather surprising that during the 1960s no more interest was shown in expanding monitoring of atmospheric CO_2 to other remote places on the globe." [61]

A call for a directed greenhouse effect research program was perhaps most likely to bring a positive response during the Kennedy Administration, which had a demonstrated interest in meteorology.[62] Congress was willing to consider dramatic specialized science projects at the time; one example is the ill-fated Mohole, an effort to drill a hole to the Earth's mantle. [63] However, such a proposal would have faced substantial hurdles in gaining support. Potential impacts were both further off in time and more uncertain than in later years. [64] (Indeed, the global temperature seems to have cooled slightly between 1940 and 1970.[65]) The research had no pork-barrel aspect that might have attracted powerful Congressional allies. Posing the problem as an environmental or energy issue, strategies that were followed after 1970, would have made little sense in the political culture of the early and mid-1960s. The environmental movement was in its infancy and was concentrating on visible environmental insults, like pollution of rivers and urban air.[66] As for energy policy, while there was some interest in the greenhouse effect as a justification for nuclear power during the decade, other side effects of fossil fuel, such as smog and diminishing reserves, made a much more compelling case for an energy transition. [67]

A somewhat more plausible alternative strategy would have been to argue that the greenhouse effect was a security issue. Some advocates of greenhouse effect research used standard Cold War rhetoric to supplement purely scientific justifications when they sought funding during the late 1950s and early 1960s. The threat of being beaten in a weather arms race by the Soviet Union was a boilerplate argument, used by the Advisory Committee on Weather Control (which counted Edward Teller among its members) to justify greater support for all types of atmospheric research in 1958; it was hauled out repeatedly by advocates in later years. [68] Revelle and others concerned about the greenhouse effect occasionally offered the additional argument that the Soviet Union's geopolitical position would improve if a rise in temperature freed its northern ports of ice and lengthened the growing season of its heartland.[69]

A purely security-based rationale for greenhouse effect research would have been difficult to devise, though. Regional forecasts were little more than guesswork. Moreover, even if atmospheric scientists had thought of this strategy and developed enough data to convince politicians to defer to them, the approach might have provoked internal dissension. Some researchers were not keen to tie their fortunes to that of the military; the civilian nature of the IGY, for instance, was a strong selling point for it. Moreover, the strategy of autonomy was a natural reflex of scientific leaders at the time, their default option, although more blatantly political strategies for the support of special projects, like Mohole, were not unheard of. Advocates of greenhouse effect research had "an understandable reluctance ... to openly engage in debate" [70] on the basis of scientific uncertainty; despite some advances, early models of climatic change were still very crude and yielded inconsistent results.[71] The policy entrepreneurs might reasonably have felt that the generation of many competing hypotheses about anthropogenic climatic change would have a salutary effect on the debate and might well turn up threats more severe than the greenhouse effect. Some had confidence that any substantiated threat of the greenhouse effect would be easily managed with a technological fix, such as the deployment of reflective material in the atmosphere.[72] In sum, Revelle said in 1966:

> Our attitude toward the changing content of carbon dioxide in the atmosphere that is being brought about by our own actions should probably contain more curiosity than apprehension. [73]

Revelle and other policy entrepreneurs were leery of a special greenhouse effect research program for political reasons as well; goal-directed programs, especially those proposed by non-scientists, could multiply, squeezing basic science. Directors of top oceanographic laboratories hesitated to support early proposals for the International

Decade of Ocean Exploration in the mid-1960s on such grounds. [74] Wariness of mission research played a role in the case at hand throughout the period I have analyzed. Some elements of research on anthropogenic climatic change were included in the budget category "inadvertent weather and climate modification" and were considered part of the larger weather modification program.[75] In defending and attempting to expand these elements, advocates of greenhouse effect research opposed a more utilitarian approach to research management and sought to recruit allies by promoting their work as basic atmospheric science. A call for a program devoted exclusively to greenhouse effect research would have undermined this second major strategy.

4. Weather Modification: The Uses of A Jurisdictional Dispute

> "I should like to emphasize that the question of the inadvertent modification of the weather needs to be as carefully examined as the advertent steps we take." - Robert M. White, Administrator, Environmental Sciences Service Administration, 1966 [76]

I have not discovered the exact reasons for the establishment of the budget category "inadvertent weather and climate modification," under which some greenhouse effect research was subsumed within the weather modification program beginning in the late 1950s. The thrust of this program was precipitation enhancement, especially rain-making by seeding clouds in the Western states, which had little to do with climatic change, particularly on a global scale. Bureaucratic convenience probably played a role in uniting the two under one rubric.

Atmospheric scientists were probably not enthusiastic about including climatic change research within the weather modification budget, unless they took the subversive view that being within the program would help them to show the impracticality of precipitation enhancement more easily. Skeptical meteorologists strongly opposed advocates of cloud seeding and their enthusiastic Congressional supporters. This schism emerged in the late 1940s and defined the politics of weather modification for three decades. [77] Its net effect was jurisdictional chaos and sporadic changes in the emphasis of Federal weather modification activities. The tenacity of the combatants is reflected by the large number of reviews that fruitlessly called for the reorganization of governance of these activities; they included reports by the House Committee on Government Operations (1965), NSF (1965), NAS (1966, 1971, 1973), Rand (1968), the National Advisory Committee on Oceans and Atmosphere (NACOA) (1972, 1973), and the General Accounting Office (1974).[78]

The controversy that swirled around the weather modification program forced anthropogenic climatic change researchers and their allies to defend this research regularly. Although they aspired to autonomy, they would have been unable to establish a normal science fully independent of normal politics, even if they had been satisfied with the level of funding in the 1960s. While the President and the Budget Bureau ignored the subject until the mid-1970s. [79] some in Congress took an interest much earlier.

Atmospheric scientists interested in the greenhouse effect saw their funding to be in competition with the proposed rapid expansion of weather modification operations that required large capital investments, such as the purchase of airplanes. Policy entrepreneurs like Thomas Malone, chairman of the NAS Committee on Atmospheric Sciences, therefore argued in favor of a "broad" view of weather modification, which encompassed more basic research, including research on inadvertent climate modification, and opposed the "narrow" view of weather modification as only rain-making. Basic understanding of the physical basis of the climate, they argued, would eventually lead to better weather modification methods. [80]

The argument was deployed on behalf of a bureaucratic champion, the Commerce Department, against the claims of the Interior Department. Those advocating greenhouse effect research seemed to assume that the budget game was zero-sum, although in retrospect it seems possible that, had rain-making support expanded as dramatically as Senators from and states had proposed (by almost 700%, from about $9 million in fiscal 1967 up to about $70 million per year by 1970),[81] the crumbs of that rapidly growing pie left over for anthropogenic climatic change research might have been more than the slice they actually received during the turf war that occurred. In cultivating bureaucratic allies, the scientists also became enmeshed in a Congressional jurisdictional dispute between the Commerce and Interior Committees. The debate had scientific and cultural as well as bureaucratic dimensions. Leading scientists simply did not think that the effectiveness of cloud seeding had been or could be demonstrated, a belief that has been borne out over time.[82] Moreover, most of the advocates of cloud seeding were businessmen or academics from western land grant institutions who lacked elite credentials; [83] a hallmark of the politics of science of this era was the opposition of Ivy League scientists to any allocations that smacked of pork barrel politics, as cloud seeing proposals did. [84]

NSF was assigned the responsibility for reporting on all Federal weather modification activities in 1958 on the recommendation of the Advisory Committee on Weather Control. The Interdepartmental Committee for Atmospheric Science (ICAS) had nominal responsibility for coordinating the various agencies' programs, but neither ICAS nor NSF had the authority to change the weather modification plans of any other agency. [85] The Bureau of Reclamation (in the Department of the Interior) began cloud seeding operations in 1961. The following year, NSF's annual report on weather modification implied its opposition to Interior's program; "the program of weather-control research should be an integral part of basic meteorological research." In 1963, NSF was somewhat more explicit, devoting substantial space to interdisciplinary historical climatic change research and stating that this "somewhat modified approach to weather modification activity undoubtedly will be continued and expanded." [86]

However, NSF did not seek to be the lead agency for the oversight of all Federal weather modification activities. Some cloud-seeding operations would surely be supported, and NSF was uncomfortable with the idea of administering programs that were not basic research projects. [87] The role of Interior's rival thus devolved on Commerce (which housed the Weather Bureau), with NSF's support. Since the White House stayed on the sidelines, Congress was the central arena for this turf battle in the mid-1960s. For example, after hearings before the Senate Interior Committee (in which only Department of Interior witnesses testified), the Bureau of Reclamation's rain-making program received a $1 million boost for fiscal 1965.[88] In an effort to put some clout behind the "majority" view among atmospheric scientists - that "large-scale engineering and development efforts in weather modification" were "premature" - NSF and NAS appointed special study panels on the issue, whose reports became touchstones in Congressional debate.[89] Indeed, in the words of Science correspondent John Walsh, the pessimistic interim report of the NAS panel in 1964 cast "a big wet blanket" on Congressional supporters of would-be rain-makers.[90]

The debate grew more intense as the lines were drawn for the "major battle" in 1966.[91] The NSF Director wrote:

> "The basic requirement to be placed on this research program is that it be vigorously directed toward developing an understanding of our finite physical environment sufficiently comprehensive to guarantee the ability to detect changes resulting from cultural influences upon the air environment before they become a significant factor to the future of our culture; and to assure our ability to predict the influence of mankind's activities upon the physical environment well enough that the evaluations and predictions of our scientists can be accepted as being sufficient upon which to base decisions with respect to land usage, the disposal of waste materials and energy, and the conscious attempts to modify weather and climatic factors for the benefit of mankind.[92]

Western Senators, on the other hand, especially Clinton Anderson of New Mexico, chairman of the Interior Committee, tended to see the scientists as more interested in producing scientific papers than water for their states.[93] As Gordon McDonald, the chairman of the NAS panel put it, "Congress has displayed considerable impatience."[94]

The disagreement crystallized in the form of two bills introduced in the 89th Congress in 1966, one sponsored by Anderson, which would have placed the Interior Department in control of weather modification policy, and the other sponsored by Senator Warren Magnuson of the Commerce Committee, which would have put the Commerce Department in this position. Although both departments were pushing for a bigger program, the Commerce bill called for a "broad" program, with an emphasis on basic research. As these bills were under consideration, the NAS panel revised its interim report and sought a compromise. Its final report was less pessimistic about the prospects for rain-making than its 1964 report had been, but it also stated that research on anthropogenic climatic change should be one of four main programmatic thrusts of Federal weather modification work.[95]

This line was endorsed by such greenhouse effect research advocates as Roberts, director of NCAR, and White, administrator of ESSA,[96] but no compromise was achieved. Both bills died. The momentum for a Congressional allocation of authority in weather modification policy dissipated in the late 1960s. The only action Congress took, in 1968, stripped (or relieved, depending on one's interpretation) NSF of its responsibility for reporting on Federal weather modification activities.[97]

With Congress deadlocked (and because proposals to bring in the President's Science Advisor to adjudicate the conflict fell on deaf ears),[98] desultory bureaucratic infighting ensued. The advocates of the "broad" view of weather modification were not particularly successful on this ground either. Congressional activity had provoked a preemptive response from the bureaucracy, as all agencies expanded their proposed weather modification budgets.[99] ICAS, even though lacking full authority, was able to rein in the most grandiose plans. Nonetheless, funding for Federal weather modification activities nearly doubled between the fiscal years 1966 and 1971, rising from $9 million to $16 million. As defined by ICAS budget figures though, research on "inadvertent weather and climate modification" actually declined by about 20%, from $434,000 to $360,000.[100]

In financial terms, then, the strategy of taking sides in the tug-of-war over weather modification policy appears to have yielded meager results from the entrepreneur's point of view, assessed about 1970. For instance, in 1968, the research director of ESSA stated that his agency planned to establish another station for monitoring CO_2. ESSA was already funding the Mauna Loa Observatory (MLO), and NSF was supporting data collection in Antarctica. For several years, however, ESSA was unable to procure funds for the third station, and it was not constructed until fiscal 1974.[101] In fiscal 1971, for instance, the agency[102] asked for $889,000 for the monitoring program in its submission to the House Appropriations Committee, but was granted only $300,000 to support MLO.[103] It is true that these restrictions were imposed during a time in which science budgets were generally declining. However, while general science funding did not revive until 1976, the budget at hand rose before that date and then stagnated during the mid-1970s.[104]

The alliances that the advocates made with the Commerce Department and its Congressional benefactors were thus not strong enough to provide funding satisfactory to them, nor did these alliances prove particularly helpful in the future. And the strategy seem to have earned the enmity of the powerful Senator Anderson. Without first-hand access to decision-makers, I cannot determine the precise contribution that the arguments of atmospheric scientists for a broad program made to the stalemate in weather modification policy, but they were surely an important resource for Commerce.

Scientific leaders might have had the clout to broker a compromise if they had moved sooner than they did, swallowing their technical and other misgivings in the recognition that Anderson and other Congressional westerners were determined to fund some cloud-seeding no matter what.[105] Their fears of a funding squeeze may not have been realized under Interior; climate research programs might have been able to ride a weather modification gravy train and expand their studies more quickly than they actually did. On the other hand, even though ardent supporters of precipitation enhancement operations did not waver during the contest, they were unable to produce demonstrable benefits that might have rallied uninterested Members of Congress to their side or swayed key scientists. Their technical failure, rather than technical criticism of their opponents, may have been the most important factor in the deadlock.

The scientific community may have had an alternative to full-scale involvement in this turf battle. The claim that better basic climate science would contribute to better cloudseeding techniques was a dubious one; most processes being studied by climatologists were at a scale far larger than individual storms. Since inadvertent climatic change had little relation to intentional rain-making, researchers might have called for the separation of the two. The risk in doing so may have been the loss of what little funding they already received under the aegis of weather modification. Early weather modification experiments had raised expectations that control of precipitation and storms would become routine.[106] The technology was glamorous, and anthropogenic climatic change researchers may have correctly believed that they could get by best by clinging to its shadow.

By the late 1960s, this belief, if it had been held, was surely gone. The growth of the American environmental movement opened up new opportunities for the advocates of research on anthropogenic climatic change. Although weather modification programs continued to encompass research on the greenhouse effect through the early 1970s - it remained in NOAA weather modification budgets, an aspect of the annual reports on weather modification, and within the purview of NAS weather modification panels[107] - such research also began to be funded under programs to protect the environment during the late 1960s, and then, in the mid-1970s, under programs aimed at establishing energy policy.

To take advantage of the changing political environment, research policy entrepreneurs moved away from arguments that connected climatic change to weather modification and began to consistently label the problem as one of pollution. The greenhouse effect could be construed in many ways. From being a great experiment, it had become a tool for improving human control over the weather. Now, it became a threat to a valued resource.

5. Climatic Change as an Environmental Issue

In 1990, the greenhouse effect is popularly seen as an environmental problem of the first order. Before 1979, however, the issue was absent from the agendas of the major U.S. environmental organizations.[108] The U.S. Environmental Protection Agency first became involved in the issue in 1983.[109] Scientific advocates of research on the issue were the first to connect it to the larger environmental agenda, but they did so slowly. As C. C. Wallen, a WMO official, put it, "It took some time before meteorologists realized that they had a stake in seeing to it that the composition of the atmosphere was not changed so as to jeopardize the health of living beings or alter the world's climate."[110]

Several reasons may account for the sluggishness of the response of atmospheric scientists to the enormous burst of public interest in environmental affairs that marked the late 1960s and early 1970s. Some atmospheric scientists may have hesitated to connect themselves to a movement they perceived as radical and anti-technology; Walter O. Roberts stated some misgivings along these lines in a 1973 interview."[111] Some were concerned that the public alarm not be raised about a scientific matter that was still highly uncertain."[112] Another explanation is that the two strategies described above limited the appeal of an environmental rationale for climatic change research. It may have appeared too goal-directed for research advocates concerned about scientific autonomy. Other advocates may have been too enmeshed in the micro-politics of weather modification to change their focus; I believe that bureaucratic entanglements were very important impediments to a change in the strategy of justifying climatic change research. Despite public acceptance, the idea that anthropogenic climatic change was an environmental problem did not have much impact on science or policy until about 1970.

Anthropogenic climatic change was seen as an environmental problem by some in the late 1950s and early 1960s. Carbon dioxide was portrayed in occasional news reports as pollution; for instance, a 1961 New York Times article stated that CO_2 levels were believed to be rising because of "the steady discharge of fumes by smoke stacks, automobile exhaust pipes, jet engines, forest fires, and burning rubbish heaps."[113] However, more commonly, scientists and journalists tended to see CO_2 as an inevitable byproduct of society, not a potentially controllable emission. The view that, in Roberts' words, "We cannot do anything about carbon dioxide," is still widely held today."[114]

Even on the rare occasions when atmospheric research policy entrepreneurs deliberately appealed to the environmentally concerned during the 1960s, a positive response was not automatic. A 1965 report of the Presidential Science Advisory Committee, Restoring the Quality of Our Environment, for instance, devoted a chapter to the greenhouse effect. The report raised the alarm that this big, invisible issue was slipping through the cracks of the Federal government's pollution control effort. Press coverage of the report ignored this point, as did the bureaucratic response, focusing on the more visible problems of smog and water pollution."[115] For example, although NSF established a new Environmental Sciences Division in 1966 at the same time that the NSF Director reasserted his interest in the CO_2 build-up, the new division seemed to devote no particular attention to the problem.[116]

Beginning in about 1966, scientists began to hypothesize that agents that were more clearly identifiable as pollutants than $C0_{2i}$ such as particulates and auto exhaust, had an influence on the weather and climate (see Section 3). These hypotheses strengthened the perception that "the atmosphere is not a dump of unlimited capacity."[117] By 1968, the notion that pollution could modify the climate was a common place. In an article sparked by the variety of anthropogenic climatic change hypotheses, U.S. News and World Report wrote: "No matter which [agent of climatic change] proves to be more important in the long run, this much seems clear: Pollutants are having an effect on the weather."[118]

This view did not directly infiltrate the somewhat stagnant U.S. policy debate on weather modification. It was first put forward by U.S. scientists in international fora and then trickled back to the domestic arena. In 1969, while his boss, Robert White, was still deeply involved in the effort to redefine the weather modification program to include more atmosperic research,[119] ESSA's Robert McCormick was instrumental in convincing the WMO's Commission on Atmospheric Sciences to call on the WMO Executive Committee to establish a global monitoring network for pollutants suspected of changing the climate, specifically carbon dioxide and particulates.[120] The WMO never considered anthropogenic climatic change to be a part of its weather modification program.[121] Indeed, non-U.S. scientists, especially Scandinavians, who had been conducting greenhouse effect research since the late 1950s, had consistently seen the issue as an environmental one.[122] The views of foreign scientists may have helped to bring about the change in the outlook of American researchers by serving as a sort of repository of justifications. Justifications that worked in political circumstances in other nations and internationally could be drawn upon as times changed at home.

The U.N. Conference on the Human Environment (UNCHE) in Stockholm in June, 1972, was a turning point in the movement toward an environmental rationale for U.S. Federal climatic change research activities. In the wake of UNCHE, carbon dioxide monitoring was moved from the budget category "inadvertent weather and climate modification" to a new one labelled "global monitoring of climatic change." The financial difficulties of the late 1960s were alleviated as funding for this item rose by 400% between fiscal years 1971 and 1975.[123] The organizers of UNCHE had made a special effort to solicit the views of non-governmental organizations, such as ICSU, in devising the conference agenda. [124] This agenda-building process, outside of Federal bureaucratic constraints, facilitated the more liberal use of environmental rhetoric in support of greenhouse effect research by American atmospheric scientists.

The 1970 Study of Critical Environmental Problems (SCEP) had the primary purpose of contributing to the UNCHE agenda. SCEP was a multidisciplinary team assembled by M.I.T. management professor Carroll Wilson; its steering committee included prominent greenhouse effect research advocates Roger Revelle and Thomas Malone. The SCEP report, Man's Impact on the Global Environment, released in August, 1970, restated McCormick's call for a global monitoring network and suggested that such a network be recommended by UNCHE. It also provided an authoritative summing-up of the existing scientific literature on the greenhouse effect and anthropogenic climatic change more generally. [125] In their exhaustive review of the history of greenhouse effect science, Victor and Clark note that this document, because of its environmental concern, has a "dramatically different" tone than previous reports on the problem.[126]

However, the content of the report was less significant for the public debate than the timing and targeting of its release. Even though the report's authors considered the greenhouse effect likely to be a more important long-term threat to the climate, public attention was focused on SCEP's statements about the supersonic transport airplane (SST).[127] Federal funding for the SST was highly controversial at the time of SCEP, and the issue was a crucial one for the development of the modem environmental movement, serving as a focus for organizing and ultimately providing one of its most dramatic early victories. As mentioned above, the deliberations of the 1964-1966 NAS weather and climate modification panel had revealed a disagreement about the effects of the SST on climate, particularly cloudiness over polar regions. Although that panel came to the tentative conclusion that large-scale SST traffic would not result in any climatic change, the issue had remained contentious among scientists. [128] SCEP recommended that large-scale deployment of the SST be delayed until concerns about its effect on the climate, especially due to particulates and cloudiness, could be resolved.[129] According to a history of the SST debate, "what had formerly been a seemingly eccentric scare theory ... had been given an aura of legitimacy and 'the stamp of seriousness." [130] SCEP's recommendation about the SST was covered on the front page of the New York Times.[131]

The legitimacy given by SCEP to the issue of SST-caused climatic change forced proponents of the SST to respond substantively; they had previously referred back to the NAS's 1966 conclusion that the SST's impact on climate would be negligible or dismissed it as "vivid speculation." [132] Among other moves, the Department of Transportation initiated a Climatic Impact Assessment Program (CLAP), which received $7 million in fiscal 1972. Although this sum was small compared to SST development, it was quite large by the standards of anthropogenic climatic change research. [133] Perhaps, as alleged by critics, the CIAP's work was not very good; it nonetheless moved the field forward if by no other means than by providing fodder for critical scientists to chew over.

SCEP's participants and organizers surely sensed that the SST portion of their report would garner the most attention, but they used the spotlight of that attention to cultivate interest in anthropogenic climatic change theories that they considered even more pressing, such as the greenhouse effect. In the following year, building on the warm reception given to SCEP, Wilson, Malone, William Kellogg of NCAR and others organized a study similar to SCEP but with international participation, the Study of Man's Impact on Climate (SMIC), for the purposes of solidifying the place of inadvertent climatic change on the agenda of UNCHE (thereby guaranteeing higher-level governmental attention to it) and ensuring that the scientific basis for UNCHE's recommendations was firm. [134] U.S. participants in SMIC included McCormick and Lester Machta, then head of NOAA's Air Resources Laboratory. SMIC was a "turning point" in the process of convincing scientists and concerned policymakers that climatic change was a global pollution problem. [135]

In the wake of SCEP, SMIC, and UNCHE, a "sense of urgency" infused those concerned about the greenhouse effect, in the words of the 1973 NAS panel on weather and climate modification. [136] The urgency was heightened by a number of frightening naturallycaused weather-related anomalies in 1972-74, including the failure of the Peruvian anchovy fishery due to a weather and ocean current pattern known as El Nino, drought in the Soviet Union's breadbasket, and unseasonably cold winters in much of the U.S.; and by the related inflation in food prices. In a 1974 speech to the U.N. General Assembly, Secretary of State Henry Kissinger called for better ICSU/WMO research on climatic disasters and indicated the U.S.'s willingness to take the lead.[137]

With a little help from Mother Nature, then, climatic change research reached the agenda of top U.S. policy-makers. Kissinger's main concern seems to have been the prospect of political instability in the Third World stemming from climate-related disasters. [138] Those who were given the responsibility for implementing his mandate, however, now viewed the problem, in large part, as one of global pollution. As White later stated, if natural climatic disasters had not spurred an increase in interest in climatic change, fear of anthropogenic climate change would have. [139]

The strategy of identifying anthropogenic climatic change as an environmental problem, then, seems to have resulted in a surge of interest in the problem from top governmental officials, international organizations, and non-governmental and academic organizations and researchers by the mid-1970s. Indeed, a deliberate effort was made to create an international network of concerned high-level individuals who could make a sustained assault on the issue. [140] Research budgets rose rapidly, as noted above.

The U.S. commitment to this research, though, was closely tied to international obligations and concerns. It was therefore ripe to be undermined by the failure of other nations to meet their reciprocal obligations and by the ebbs and flows of U.S. concern about the international environment. The rise in support for climatic change research levelled off after fiscal 1975; under President Gerald Ford, OMB turned down NOAA efforts to expand the program in the following two fiscal years.[141] The monitoring component of the proposed research may have been growing large enough (up to a projected $41.8 million annually in fiscal 1978)[142] to attract the attention of anti-inflation budget cutters who were slashing science across the board, but the failure of any other nation to complete a monitoring station probably contributed to the difficulty. The program's advocates stated that it was "impractical" for the U.S. to "do it all alone," yet that was just the position that the program was in. The Global Environmental Monitoring System envisioned by UNCHE, a mainstay of the United Nations

Environmental Program (UNEP) established after UNCHE and within which the CO_2 monitoring effort was located, was extremely slow to take shape.[143] Might those concerned about the greenhouse effect have avoided this difficulty by promoting a purely American research program based on environmental concern?[144]

Despite public statements to the contrary, the U.S. probably had the global reach necessary to implement an adequate monitoring program. SMIC had called for 12 monitoring stations in remote areas; by 1974, the U.S. had built or was constructing four such stations in the Arctic and Antarctic as well as the Central and Western Pacific.[145] Most of the modeling work that was being done in the mid-1970s was being done in the U.S. From the scientists' point of view, the U.S. Committee for the Global Atmospheric Research Program noted that "the degree to which [our proposed national climatic research program] should be regarded as national as opposed to international is not of critical importance for our purposes," even though it described the proposed division of labor between the proposed national and international research programs in detail.[146] An internationalist approach to this research may have suited the ideological aspirations of American atmospheric science well[147] and, arguably, may yet be useful to lay the groundwork for a policy response that will have to be international, but it seems plausible that the U.S. could have gone it alone in research and let the rest of the world free ride.

A purely domestic research program would have faced tough questions from those who would have preferred to share the burden with others who were likely to benefit from the knowledge developed. In addition, I doubt that entrepreneurs promoting such a program would have found domestic allies easily. Securing the backing of public interest environmental groups on its behalf would have been difficult; by and large, they did not put a high priority on global environmental issues until the early 1980s.[148] EPA (not likely to be a very powerful ally in any case) was running a data center for the CO_2 monitoring network, but the mid-1970s were a period of explosive growth of Congressional mandates for EPA, like the Toxic Substances Control Act (1975), that surely would have made tough competition for a long-term, low visibility issue like anthropogenic climatic change.[149] A pitch might have been made for the support of agricultural interest groups on the basis of potential greenhouse-induced water shortages, but if the U.S. Department of Agriculture (USDA) is any measure of their potential interest, such a pitch would not have been well received. When USDA finally took an interest in the issue around 1980, it confined itself to the question of how the increased CO_2 content of the atmosphere might increase crop yields, rather than take a broader view of the overall impact of the greenhouse effect, including the impact of projected changes in temperature and precipitation.[150]

The first oil shock of 1973, however, provided an opportunity for greenhouse effect research policy entrepreneurs to secure new domestic allies. They took advantage of it; at least as early as 1974, Roger Revelle organized a group of greenhouse experts with the purpose of putting useful information in energy policy-makers' hands.[151] Energy use is the primary generator of greenhouse gas emissions; choices among energy strategies, for instance, placing an emphasis on nuclear power (long favored by Federal energy planners) over fossil fuel, could have the effect of slowing the rate of greenhouse gas accumulation, presumably allowing more time for study and development of responses. The environmental externalities of various energy sources are wideranging. Tradeoffs among them, including climatic impacts, could reasonably be construed as important factors to be considered in a rational approach to energy policy, an approach which many scientists expected to be taken in the 1970s.

By 1977, the Department of Energy was interested enough in the issue to be designated the lead Federal agency on it.[152] Under DOE's leadership, Federal spending on greenhouse effect research eventually resumed its climb in the late 1970s.[153] When the Carter Administration proposed expanding production of synthetic fuels, which emit more greenhouse gases per unit of energy than other energy sources, concern about the greenhouse effect was strong enough for Congress to call hearings on the issue and to require, in 1980 legislation, an NAS report on the topic.[154] Entrepreneurial activity had facilitated the insertion of the issue into an active and important policy debate.

The process of building domestic support for anthropogenic climatic change research, then, was slow. The pronouncements and programs of international meetings and organizations helped raise the profile of the issue domestically and dramatized the view that it was an environmental concern. They bridged a gap in which domestic advocates of the issue were still enmeshed in bureaucratic battles over weather modification and were only beginning to develop an environmental rationale that could plausibly win the support of domestic allies.

6. Conclusions

I have argued that scientists concerned about possible human influences on the climate, particularly the greenhouse effect, adopted three major strategies in trying to win more research support and bring greater attention to the issue between 1957 and 1974. First, beginning in the late 1950s, they viewed the issue as a scientific puzzle, one which scientific institutions could and should resolve, at least for the foreseeable future. This approach encouraged scientific interest in the issue and the proliferation of anthropogenic climatic change hypotheses. However, a pure science rationale did not command sufficient support from potential funding sources to carry through even the relatively modest research program on the greenhouse effect envisioned in the early 1960s.

This rather complacent strategy was supplanted to some extent in the mid-1960s, because of the affiliation of anthropogenic climatic change research with weather modification. This affiliation caused interested atmospheric scientists to defend their research as a potential contribution to weather modification techniques, part of a broad, rather than narrow, approach to weather modification. In the turf battle over weather modification policy, the scientists' side came out on the losing end, by and large. The pace of research funding on "inadvertent weather and climate modification" lagged behind that of other areas of weather modification in the late 1960s.

Later still, with growing momentum after 1970, anthropogenic climatic change was presented as an environmental issue, first in international fora and then as one of a number of such issues that, advocates argued, ought to affect U.S. energy policy. The climate was portrayed as a natural resource that needed to be defended from the onslaught of industrialism. In the early 1970s, this approach brought the attention of high policy-makers to the issue and, with it, increased budgets. After a pause in the mid-1970s, these trends continued into the late 1970s.

An interesting generalization that may be drawn from this case is that successive strategies of persuasion constrain each other. Greenhouse effect experts committed to viewing their work as basic research, for instance, could not easily call for a mission-oriented effort. The happenstance that anthropogenic climatic change research was located within the weather modification program and the choice by the proponents of this research to engage in the bureaucratic turf wars that engulfed this program concentrated their energy and narrowed their vision, so that when new opportunities appeared with the rise of the environmental movement, advantage could not be taken immediately. Later still (and outside this paper's focus), one might note that the strong connection to energy policy may have slowed the incorporation of greenhouse gases other than CO_2 into the research program, inhibiting a possible widening of the range of potential political allies.[155]

An important way that early strategies constrained later ones in this case was by directing scientific research and thus influencing what results might be available to support later arguments. By considering greenhouse effect research to be basic science, for example, proponents of this research opened the door to many competing hypotheses about anthropogenic climatic change, making any call for a directed research program much more difficult, even though most of the entrepreneurs discussed in the body of the paper did not change their estimation that the greenhouse effect was the most plausible and serious threat to the climate. However, the relationship between political strategies and scientific results is difficult to reconstruct. If anything, I would guess that I have overestimated the confidence that Revelle, Roberts, Malone, White, and others had in their assessment that the greenhouse effect would probably occur; the range of plausible political strategies may have been more constrained than the paper suggests. This is a topic for future research.

In a 1979 editorial, Michael Glantz argued that "crisis awareness" was necessary for pluralistic societies to address the threat of the greenhouse effect and other environmental threats that are equally gradual and diffuse.[156] My research suggests that any strategy of research advocacy must be long-term and cannot rely on crisis awareness. In the U.S. polity, little moves very quickly. Peaks of public awareness, like the 1972 weather anomalies, may be opportunities for pushing an existing strategy harder or extending it farther, but the past constrains the options in such instances as does the necessity for extended future interactions. Support that is not sustainable over a period of years will not be particularly valuable. On the other hand, Glantz appropriately suggests that those concerned about the greenhouse effect choose their strategy of advocacy very deliberately. Advocates may choose the indicators, impacts, and time frame they emphasize. These choices will affect their ability to win allies.

These findings and comments may not have much import for the future of greenhouse effect research in the U.S. Since the 1970s, when my research leaves off, the issue has reached a much higher level of public attention and political interest. The dynamics hypothesized may not apply well. For instance, the manipulation of public opinion may play a role in future policy-making, but I am not certain that the past constrains the present as much in public relations strategies as it does advocacy among experts and in the Congress.

Similarly, more actors are involved in the issue now, making the level of coordination hypothesized above less feasible. More appropriate analogies may be found in smaller programs, such as the fusion program described by Joan Bromberg in a 1982 article.[157]

On the other hand, it may be that when obscure programs rise high on the political agenda, as climatic change research did in the past several years, the constraints of earlier strategies of advocacy carry over. If the key entrepreneurs are the same people and the same bureaucratic entities remain the major actors, then this result seems possible. In such cases, it would behoove all of those involved to critically examine their history and identify hidden chains.

Notes

1. Schneider, Stephen, Global Warming: Are We Entering the Greenhouse Century (San Francisco: Sierra Club Books, 1989), chapter 6.

2. The early scientific history of the greenhouse effect is discussed below, at the beginning of section 3.

3. See Schneider, op. cit., pp. 105-119, for a recent projection of regional impacts.

4. For instance, see Olson's The Logic of Collective Action and the critique in Wilson's Politics of Regulation. The literature on this subject is large.

5. Etzkowitz, Henry, "Entrepreneurial Science and Entrepreneurial Universities," Minerva 21:198-233 (1988).

6. Greenberg, Daniel S., The Politics of Pure Science (New York: Plume Books, 1967), chapter 2.

7. Knorr-Cetina, Karin, The Manufacture of Knowledge (New York: Pergamon, 1981), esp. pp. 80-86.

8. Quoted by Etzkowitz, op. cit., p. 210.

9. The Kennedy School project under which this research was conducted involved two other researchers who also studied the history of the greenhouse effect in the U.S. We divided the period from 1957 to the present among us; 1974 was chosen as one dividing line, because it approximately marks a new political era in the U.S. The other line was the ascension of the Reagan Administration in 1981.

10. See below, Section 3.

11. Interviews or archival research might provide insights into individual motivations and attempts to coordinate political activities. Such research would be a good next step to follow up this paper.

12. This is the famous methodological prescription of Alexander George.

13. The level of "investment," as well as the "returns," of the entrepreneurs may have varied over time, in part in response to the changing state of the scientific debate. In a future paper with David Victor, I intend to explore the relationships between the scientific and political discourses more carefully.

14. In McLuhan, Marshall, Understanding Media (New York: McGraw-Hill, 1964).

15. Meadows, et al., Limits to Growth (New York: Universe Books, 1972). The modem environmental movement is often considered to have started with the publication of Rachel Carson's Silent Spring (Boston: Houghton-Mifflin, 1962).

16. Von Neumann, John, "Can We Survive Technology," Fortune, June, 1955, p. 108. Senator Clinton Anderson claimed to carry this von Neumann article around with him in his breast pocket. Other examples of the analogy include Walsh, John, "Weather Modification," Science 147:274-276 (January 15, 1965); and NSF, Annual Report on Weather Modification. 1964, p. 2. The Pentagon took the notion of weather warfare seriously enough to try it out in Vietnam. Between 1963 and 1972, according to the New York Times, some 2,602 weather modification missions were carried out. On this topic, see articles by Seymour Hersh and John Wilford throughout the week of July 3, 1972, summed up in Hersh, Seymour, "Weather as a Weapon of War," New York Times,, July 9, 1972, sec. IV, p. 3. The Times also noted, "the DOD's Advanced Research Project Agency is sponsoring research to determine how much and what kind of tinkering is required to disturb the climate on a global scale - an indication that the Pentagon is not sure of the ecological impact of weather warfare." (Wilford, July 3, 1972, p. 2). Another interesting report is Shapley, Deborah, "Rainmaking: Stockholm Stand Watered Down for Military," Science 176:1404 (June 30, 1972). An inquiry into defense-related climate research would be a good follow-up to this paper.

17. Exchange between Senator Clinton Anderson and James E. McDonald, in U.S. Senate, Committee on Interior and Insular Affairs, Subcommittee on Water and Power Resources, Weather Modification (1966), p. 145. Testimony of James T. Ramey, ibid., p. 196.

18. Kimble, George, "But Somebody Does Something About It," New York Times Magazine, July 8, 1962, p. 11. A similar story is "Is Man Upsetting the Weather?," U.S. News and World Report, November 11, 1963, p. 46-48. In April, 1955, the U.S. Congress' Joint Committee on Atomic Energy held hearings on the effects of nuclear explosions on the weather. These were clearly aimed at allaying public mistrust and had little scientific content. See Hewlett, Richard G., and Holl, Jack M., Atoms for Peace and War (Berkeley: University of California, 1989), pp. 291-295.

19. The annual reports of the AEC and the Department of Commerce regularly report on fall-out research between 1960 and 1974. See, e.g., AEC, Annual Report. 1974, pp. 98-100.

20. The opinion of experts on the climatic impact of fall-out is given in "Is Man Upsetting the Weather," op. cit., p. 47 and in U.S. Senate, Committee on Interior and Insular Affairs, 1966, op. cit., testimony of W.O. Roberts, p. 362. Fall-out is not among the change agents reviewed in two reviews in 1970: Landsberg, Helmut, "Man-Made Climatic Changes," Science 170:1265-1274 (December 18, 1970) and Council on Environmental Quality, Annual Report. 1970. The Limited Test Ban Treaty of 1963 prohibited atmospheric nuclear testing by the Soviet Union and the U.S., but it was continued by other nations, including China and France. A sample of atmospheric research findings that used nuclear tests to provide tracers can be found in the papers of the International Symposium on Trace Gases and Radioactivity, reported in Journal of Geophysical Research (JGR), volume 68 (1963).

21. On the usefulness of atmospheric tests, see U.S. House of Representatives, Committee on Appropriations, National Science Foundation - International Geophysical Year, testimony of Roger Revelle, p. 113; and Suess, Hans, "Fuel Residuals and Climate," Bulletin of Atomic Scientists 17:374-375 (1961), p. 375. An early article by Machta is Hagemann, F., et al., "Stratospheric Carbon 14, Carbon Dioxide and Tritium," Science 130:542-552 (September 4, 1959); he is cited as an authority on the greenhouse effect in Andelman, David, "20% Rise Feared in Carbon Dioxide," New York Times, May 17, 1972, p. 6. On GFDL, see NSF, Annual Report on Weather Modification. 1967, p. 67; and Victor, David and Clark, William, "The Greenhouse Effect in the U.S.: A History," unpub. mss., Kennedy School of Government, Harvard University, April, 1990, pp. 33-34 and 49-50.

22. See, for instance, U.S. Senate Committee on Interior and Insular Affairs, 1966, op. cit., testimony of W.O. Roberts, p. 353; U.S. House of Representatives, Committee on Appropriations, Subcommittee on State, Justice, Commerce, and the Judiciary, Departments of State. Justice, and Commerce. the Judiciary. and Related Agencies Appropriations for 1971. Part 3: Department of Commerce, testimony of Robert White, p. 853.

23. "Organized skepticism ... calls for the suspension of judgment until the requisite evidence is there." Zuckerman, Harriet, "The Sociology of Science," in Smelser, Neil, ed., Handbook of Sociology (Menlo Park, CA: Sage, 1988), p. 515. Zuckerman, while noting that some sociologists have taken this norm to apply to individuals, argues that it applies instead to the scientific community as a whole.

24. Hays, Samuel, Beauty. Health, and Permanence (New York: Cambridge University Press, 1987), p. 338. Mazur, Allan, "Disputes Between Experts," Minerva 11:243-262 (1973).

25. For instance, disagreements on this point within the WMO Executive Committee in 1976 were strong enough to cause a rare breach in the formalistic tone of official reporting, Abridged Report of the WMO Executive Council. 1976, p. 38. I do not have the detailed anthropological or biographical information that would be needed to substantiate the claim that what Mazur calls the "dynamics of technological controversy" (the hardening of

positions referred to in the text) played an important role in this case. However, a starting point for future research on the topic might be the career of Stephen Schneider, probably the greenhouse effect research best-known by the public.

26. These data are taken from a literature search (on Newsweek, New York Times, Science, Science News, Time, Wall Street Journal, and Washington Post) run by David Festa for the Kennedy School program and shown in figure 1, p. 88 in an unpublished draft manuscript, "History of the Greenhouse Effect, 1957-1988" (September 29, 1989) and from my own survey, conducted without the benefit of electronic data bases.

27. The term is Thomas Kuhn's, from The Structure of Scientific Revolutions (Chicago: University of Chicago, 1962).

28. This early portion of the scientific history is reviewed in Ausubel, Jesse, "Historical Note," Annex 2 in NAS, Carbon Dioxide Assessment Committee, Changing Climate (Washington: NAS Press, 1983).

29. Victor and Clark, op. cit., pp. 7-13. This paper is the best consolidated source on the scientific history of the greenhouse effect in the U.S. Victor and Clark point out that early versions of the greenhouse effect hypothesis referred only to events on a geological time scale. The development of theories about decadal and annual scale fluid dynamics of the atmosphere and oceans permitted its application to the fossil fuel-based carbon dioxide buildup.

30. Greenberg, op. cit., chapter 6 and Smith, Bruce, American Science Policy Since World War II (Washington: Brookings, 1990), chapter 3.

31. Plass, Gilbert N., "The Carbon Dioxide Theory of Climate Change," Tellus 8:140-154 (1956). Suess, H.E., "Natural Radiocarbon and the Rate of Exchange of Carbon Dioxide Between the Atmosphere and the Sea," in Nuclear Processes in Geologic Settings (Washington: NAS, 1953). Revelle, Roger, and Suess, Hans, "Carbon Dioxide Exchange Between Atmosphere and Ocean and the Question of an Increase of Atmospheric CO_2 During the Past Decades," Tellus 9:18-27 (1957). A typical use of the experiment metaphor in later years is found in Landsberg, op. cit., p. 1267.

32. U.S. House of Representatives, Committee on Appropriations, National Science Foundation - International Geophysical Year (1956), testimony of Roger Revelle, p. 473. Plass, Gilbert N., "Carbon Dioxide and Climate," Scientific American 201:41-47 (1959). This attitude, which appears callous to the modern observer, is especially evident in a statement made by Revelle before the House Appropriations Committee in 1957 (op. cit., p. 113):

> "From the standpoint of geophysicists, these [atmospheric nuclear] tests are really quite useful. Everything you do has hazards and dangers. These people on these island stations in the Pacific and in Antarctica are all subject to very considerable danger. Science in some respects is a hazardous business. The amount of information we get from a sudden shock to the atmosphere, like suddenly introducing radioactive tracers in the air, is really very great."

The metaphor was pointedly appropriated by a House report in 1977: "It is not prudent to allow Earth to perform the experiment in the hopes of obtaining better data." U.S. House of Representatives, Science and Technology Committee, The National Climate Policy Act of 1977, p. 6.

33. WMO #55, IGY #1, The International Geophysical Year. 1957-58. Meteorological Programme (Geneva: WMO, 1956), pp. 19-20.

34. U.S. House of Representatives, Committee on Appropriations, Review of the First Eleven Months of the International Geophysical Year (1958), testimony of H. Wexler, p. 39.

35. Historical data on the increase in greenhouse gas concentrations are summarized in U.N. Environmental Program and the Beijer Institute, The Full Range of Responses to Anticipated Climatic Change (April, 1989), chapter 1.

36. There is no controversy over the matter in the oversight hearings, U.S. House, Committee on Appropriations, 1956-58, op. cit. Walter McDougall argues that top policy-makers actually had a strong incentive to stay out of IGY planning. The IGY was approved by President Eisenhower, at least in part, he suggests, in order to provide a non-threatening precedent for U.S. overflight of Soviet territory by satellites that were to be built under the aegis of the IGY. McDougall, Walter, The Heavens and the Earth (New York: Basic Books, 1985), pp. 118-123.

37. Bullis, Harold, "The Political Legacy of the IGY," in U.S. House of Representatives, Committee on International Relations, Subcommittee on International Security and Scientific Affairs, Science. Technology, and American Diplomacy (1976), pp. 45-58.

38. On the initiation of NSF atmospheric science program, see NSF, Annual Report. 1958, p. 37. On the founding of NCAR, see "Atmospheric Institute Founded," New York Times, October 10, 1958, p. 33; and "NSF to Open Research Center," ibid., June 27, 1960, p. 5. GFDL was founded in 1960, according to Victor and Clark, op. cit., p. 32.

39. NSF, Annual Report on Weather Modification. 1962, p. 2.

40. For NCAR, see NSF, Annual Report. 1966; NSF, Annual Report on Weather Modification, 1966; and Ramanathan, V., "Greenhouse Effect Due to Chlorofluorocarbons: Climatic Implications," Science 190:50-52 (September 8, 1975). The NSF supported CO_2 monitoring at Mauna Loa and in Antarctica for a number of years, as indicated in Sullivan, Walter, "Air Found Gaining Carbon Dioxide," New York Times, September 11, 1961, p. 29, and U.S. House, Committee on Appropriations, 1974, op. cit., testimony of Robert White, p. 704. The beginning of the Department of Commerce's modeling work is noted in NAS, Panel on Weather and Climate Modification, Weather and Climate Modification: Problem and Prospects (Washington: NAS, 1966), vol. 1, p. viii and other Commerce programs are described in U.S. Senate, Committee on Commerce, Weather Modification (1968), testimony of W. Baum, p. 32.

41. The Weather Bureau was incorporated into the Environmental Sciences Service Administration (ESSA) in 1966. ESSA was disbanded in 1970, and many of its functions were taken over by the newly-formed National Oceanic and Atmospheric Administration (NOAA). White headed each of these bureaucratic incarnations. All were housed in the ommerce Department.

42. Miles, Edward, "COSPAR and SCOR: The Political Influence of the Committee on Space Research and the Scientific Committee on Ocean Research," in Evan, William, ed., Knowledge and Power in a Global Society (Beverly Hills: Sage, 1981), p. 140. Other examples include White's reprogramming of NOAA funds to support CO_2 monitoring [U.S. House of Representatives, Committee on Science and Technology, Subcommittee on Environment and Atmosphere, Environmental Monitoring (1977), p. 122] and NSF reprogramming of funds for climate dynamics research [U.S. House of Representatives, Committee on Science and Technology, Subcommittee on Atmosphere and Environment, Climate Research and a National Climate Program (December, 1976), p. 13].

43. For example, GARP #16, The Physical Basis of Climate and Climate Modeling (Geneva: WMO/ICSU, 1974), is cited by Wang, et al., "Greenhouse Effects Due to Man-Made Perturbations of Trace Gases," Science 194:685-90 (November 12, 1976), p. 689.
44. Skolnikoff, Eugene, Science, Technology, and American Foreign Policy (Cambridge: MIT Press, 1967), pp. 173-176.

45. For reviews, see Landsberg, 1970, op. cit., and CEQ, 1970, op. cit. Since they depended on different mechanisms than the greenhouse effect, the other anthropogenic influences on climate were not alternatives to it, but might have added to, negated, or otherwise changed its influence. For example, Damon and Kunen argued that cooling due to particulates was dominating in the northern hemisphere and greenhouse warming was prevailing in the southern hemisphere, where there was less industrial activity. Damon, Paul E., and Kunen, Steven M., "Global Cooling?," Science 193:447-453 (August 6, 1976).

46. Bryson, Reid, "Is Man Changing the Climate of the Earth?," Saturday Review, April 1, 1967, pp. 52-56; McCormick, R.A., and Ludwig, J.H., "Climate Modification by Atmospheric Aerosols," Science 156:1358-1359 (June 9, 1967). Reports of the early demise of the cooling hypothesis can be found in Wang, op. cit., and Kellogg, W.W., "Mankind's Impact on Climate: The Evolution of an Awareness," Climatic Change 10:111-133 (1987). Jesse Ausubel, personal communication, 1989, p. 4, states that the Global 2000 report was dominated by cooling scenarios. A report stating that the Mauna Loa Observatory showed no rise in turbidity (a finding which disconfirmed the cooling hypothesis) appeared in the New York Times on February 18, 1978, p. 9.

47. "Is Man Upsetting the Weather?," op. cit., p. 47; WMO, Special Environmental Report #1, A Brief Survey of the Activities of the WMO Relating to the Human Environment (Geneva: WMO, 1970), p. 6.

48. Malone, T.F., "Weather Modification: Implications of the New Horizons in Research" (1967), reprinted in U.S. Senate, Committee on Commerce, 1968, op. cit., p. 88.

49. Sullivan, Walter, "Jet Trails Effect on Climate Studied," New York Times May 1, 1965, p. 1; U.S. Senate, Committee on Appropriations, Civilian Superson Aircraft Development for FY71 (1971), testimony of H. Newell, p. 139.

50. Schaefer, V., "Ice Nuclei from Auto Exhaust and Iodine Vapor," Science 154:1555-6 (December 23, 1966); reported in New York Times, December 23, 1966, p. 13. The story was also reported in the major weekly news magazines.

51. NSF, Annual Report. 1970, p. 59; ibid., Annual Report, 1972, p. 39.

52. There is a short review on this point in Landsberg, op. cit., p. 1269.

53. Changnon, Stanley, Jr., "Atmospheric Alterations from Bioxpheric Changes," in Sewell, W.R.D., ed., Modifying the Weather: A Social Assessment (Victoria: University of Victoria, 1973) p. 164; Weinberg, Alvin, and Hammond, R. Philip, "Global Effects of Increased Use of Energy," Bulletin of Atomic Scientists March, 1972, pp. 5-8. Heilbroner, Robert, and Thurow, Lester, Understanding Macroeconomics, 6th edition, (Englewood Cliffs, NJ: Prentice Hall, 1978), p. 15.

54. "Age of Effluence," Time, May 10, 1968, pp. 52-53; "Oxygen Crisis," Newsweek, January 8, 1968, p. 45; New York Times, June 25, 1970, p. 26.

55. "Oil Spills Could Change Climate," New York Times, December 10, 1975, p. 19.

56. "Age of Effluence," op. cit., p. 53.

57. See Victor and Clark, op. cit., pp. 45-47, for their discussion of the renewed interest in the carbon cycle, beginning about 1970, after scientists in the 1960s had considered it to be fully understood.

58. Keeling, Charles, "Atmospheric Carbon Dioxide Variations at the Mauna Loa Observatory," Tellus 28:538-551 (1976). Earl W. Barrett also provides some of the funding history of this program in "Inadvertent Weather and Climate Modification," CRC Critical Reviews in Environmental Control 6:15-90 (1975), esp. p. 17 and pp. 30-31. Keeling, C.D., and Brown, C.W., "Concentration of Atmospheric Carbon Dioxide in Hawaii," JGR 70:6053-6076 (1965), p. 6075 has a note on the severe "financial and logistical obstacles" to his work. A similar comment is found in Conservation Foundation, op. cit., p. 10.

59. Victor and Clark, op. cit., pp. 41-42.

60. The data are described by Sullivan, 1961, op, cit., and in more detail in Bolin, Bert and Keeling, Charles, "Large Scale Atmospheric Mixing as Deduced from the Seasonal

and Meridional Variations of Carbon Dioxide," JGR 68:3899-3920 (1963). Revelle's testimony is in U.S. House, Committee on Appropriations, 1957, op. cit., p. 106. Suess also stated his opinion that the greenhouse effect could have a substantial impact in the reasonably near future in Suess, 1962, op. cit.

61. Wallen, C.C., (Chief of Special Environmental Applications, WMO, 1972-76; Deputy Director, GEMS, UNEP, 1976-1981), "Monitoring the Atmospheric C02 Concentration," in Bach, W.; Crane, A.J.; and Longhetto, A., Carbon Dioxide: Current Views and Developments in Energy/Climate Research (Dordrecht: D. Reidel, 1982), p. 8.

62. For instance, Kennedy and Presidential Science Advisor Wiesner catalyzed U.N. activity on climate research in the early 1960s. See Perry, John S., "The Global Atmospheric Research Program," Reviews of Geophysics and Space Physics 13 (1975), p. 661.

63. Greenberg, op. cit., pp. 171-206.

64. Conflicting estimates of the temperature impact of doubling the CO_2 content of the atmosphere ranged from a rise of .01 degrees Celsius [Flohn, H., "Theories of Climatic Change from the Viewpoint of the Global Energy Budget," pp. 339-344 in Changes of Climate, proceedings of the Rome symposium organized by UNESCO and WMO (UNESCO: Paris, 1963)] to up to 3.8 degrees Celsius [Conservation Foundation, On the Implications of the Rising Carbon Dioxide Content of the Atmosphere (New York: CF, 1963)].

65. Sullivan, 1961, op. cit. Hays, James D., "The Ice Age Cometh," Saturday Review, March 24, 1973, pp. 29-32.

66. One example of the state of the debate is an article on air pollution in the New Yorker in 1968. It devoted one paragraph to global climate change, which concluded: "The average person, however, is not worrying about melting ice caps when he looks up at the murky sky but is simply wondering what the air is doing to him." Iglauer, Edith, "The Ambient Air," New Yorker, April 13, 1968, pp. 51-70, quote from p. 51.

67. Statements by nuclear power advocates that use the greenhouse effect as a justification can be found in Bamberger, Werner, "A Nuclear Fleet for Cargo Urged," New York Times, April 20, 1968, p. 66 and Smith, Gene, "Utilities Urged to Back A-Power," New York Times June 10, 1969, p. 63. In transcripts of hearings of the Joint Atomic Energy Committee in 1969 on Environmental Effects of Producing Electric Power, the issue commands far less than 1 % of the 1000-odd pages. The relevant testimony is by Lee DuBridge, pp. 25-26, and J.H. Ludwig, p. 498.

68. Sewell, W.R.D., "Weather Modification: Social Concern and Public Policy," in Sewell, ed., op. cit., p. 7; New York Times, January 1, 1958, p. 1; New York Times, March 28, 1959, p. 6; U.S. Senate, Committee on Commerce, Weather Modification

(1966), statement of Senator Howard Cannon, p. 2; New York Times, January 14, 1966, p. 8.

69. U.S. House, Committee on Appropriations, 1956, op. cit., testimony of Roger Revelle, p. 473; NSF, Annual Report on Weather Modification. 1962, p. 2. Apparently, there has been a reciprocal use of this argument by Soviet opponents of attempts to prevent the greenhouse effect.

70. Kellogg, William, Effects of Human Activities on Global Climate (WMO #486) (Geneva: WMO, 1977), p. 1.

71. On this point, in addition to note 62 above, one might note the variation in the predictions of the leading general circulation models, as described by Victor and Clark, op. cit., pp. 49-51. Even a single model, such as that of Manabe and Wetherald at GFDL, can produce widely varying predictions depending on the exact specifications entered into it. Compare, for instance, Manabe, S. and Wetherald, R.T., "Thermal Equilibrium of the Atmosphere with a Given Distribution of Relative Humidity," Journal of Atmospheric Sciences 24:241-259 (1967) with ibid., "The Effects of Doubling the Carbon Dioxide Concentration on the Climate of a General Circulation Model," Journal of Atmospheric Sciences 32:3-15 (1975).

72. This possibility was suggested a number of times, for instance, by President's Science Advisory Committee, Environmental Pollution Panel, Restoring the Quality of Our Environment (Washington: USGPO, 1965), p. 127.

73. Revelle, Roger, "The Role of the Oceans," Saturday Review, May 7, 1966, p. 41.

74. Wenk, Edward, Jr., The Politics of the Ocean (Seattle: University of Washington, 1972), pp. 216-218.

75. Really a collection of more or less separate agency programs, as described below.

76. U.S. Senate, Committee on Commerce, 1966, op. cit., testimony of Robert M. White, p. 173.

77. A report in Science in 1963 stated that the "Weather Bureau has maintained a consistently negative attitude [about the prospects for successful rain-making] since 1946." Langer, Elinor, "Weather Bureau," Science 141:508 (August 9, 1963).

78. These reports are reviewed in General Accounting Office, The Need for A National Weather Modification Research Program (Washington: USGPO, 1974), pp. 9-15. These reports were followed by one from the Domestic Council in 1974 and another from NAS in 1975.

79. The President and the Budget Bureau often took an interest in specific scientific programs, like space and oceanography (especially in the early and mid-1960s). See the

many cases documented by W. Henry Lambright in Presidential Management of Science and Technology (Austin: University of Texas Press, 1983) and James E. Katz in Presidential Politics and Science Policy (New York: Praeger, 1978), especially chapter 5 on OMB.

80. Examples include Malone's testimony in U.S. Senate, Commerce Committee, 1968, g cit,, p. 136; W.O. Roberts' testimony in U.S. Senate, Committee on Interior and Insular Affairs, 1966, p. 353; and Revelle, Roger, "Water Resources Research in the Federal Government," Science 142:1027-1033 (November 22, 1963).

81. Newell, Homer E., A Recommended National Program in Weather Modification, Interdepartmental Committee for Atmospheric Science (ICAS) report #10a (Washington, DC: ICAS, 1966), p. 111-3. The 1966 Anderson bill proposed funding weather modification programs at $155 million over three years, ramping up from $35 million to $50 million to $70 million. Actual fiscal 1970 funding was approximately $12 million, according to National Academy of Sciences, Panel on Weather and Climate Modification, Weather and Climate Modification: Problems and Progress (Washington: NAS Press, 1973), p. 22.

82. By 1973, enthusiasm for weather modification had notably soured. In the preface to the proceedings of 1973 conference, the Secretary-General of the WMO noted: "National governments are advised that, at present, weather modification operations related to precipitation are a waste of money and effort because they cannot be scientifically evaluated." WMO #339, Proceedings of the WMO/IAMAP Scientific Conference on Weather Modification (Geneva: WMO, 1974).

83. Holden, Jr., Matthew, "Politics and Weather Modification," in Sewell, op. cit., p. 264. The contrast between the proponents and opponents of cloud seeding can be seen by glancing at the witness lists of most of the Congressional hearings on weather modification. See, for example, U.S. Senate, Committee on Interior and Insular Affairs, Weather Modification (1964). However, cloud seeding was originally invented by a Nobel Prize winner, Irving Langmuir (albeit while working for General Electric).

84. One gets this sense of scientific elitism in the news reports of Science on weather modification, e.g., Walsh, op. cit., Carter, Luther J., "Weather Modification: Panels Want Greater Effort," Science 151:428-430 (January 28, 1966), and ibid., "Weather Modification: Senate Bills Stir Rival Agencies," Science 151:805-808 (February 18, 1966). The question of pork barrel science funding reached a fever pitch in the siting of an accelerator for high energy physics research during the 1960s. See Greenberg, op. cit., chapters 10 and 11.

85. McDonald, Gordon, "Federal Government Programs in Weather Modification," in Fleagle, Robert G., Weather Modification: Science and Public Policy (Seattle: University of Washington Press, 1968), p. 70; NSF, Annual Report. 1959, pp. 8-9.

86. Two histories of the Federal weather modification program are McDonald, op.cit. and Sewell, op. cit., pp. 2-42. Quotes are from NSF, Annual Report on Weather Modification, 1962, p. 33; and NSF, Annual Report on Weather Modification. 1963, p. 2.
87. Holden, op.. cit., p. 289.

88. U.S. Senate, Committee on Interior and Insular Affairs, Subcommittee on Irrigation and Reclamation, Weather Modification (1964); NSF, Annual Report on Weather Modification. 1964, p. 2; Walsh, op. cit., p. 275.

89. The quotes are drawn NSF, Annual Report on Weather Modification. 1964, p. 6. The two reports are National Academy of Sciences, Panel on Weather and Climate Modification, Weather and Climate Modification: Problems and Prospects. vols. 1 and 2 (Washington: NAS Press, 1966) and National Science Board, Special Committee on Weather Modification, Weather and Climate Modification (Washington: NSF, 1965).

90. Walsh, op. cit., p. 275.

91. McDonald, op. cit., p. 77.

92. NSF, Annual Report on Weather Modification. 1965, p. 14.

93. Fleagle, Robert G., "Background and Present Status," in Fleagle, op. cit., p. 12.

94. McDonald, op. cit., p. 70. McDonald sees the dispute as one that pits the executive against the legislature. However, this interpretation seems less plausible than the one I make in the text, given the lobbying of the Interior Department and the role of the Commerce Committee.

95. Compare, for instance, U.S. Senate, Committee on Commerce, 1966, op. cit., testimony of W.O. Roberts with U.S. Senate, Committee on Interior and Insular Affairs, 1966, op. cit., speech of Senator Anderson, p. 324. Conclusions of the NAS panel, 1966, op. cit., at vol. 1, p. 10; vol. 2, chapter 4. See also Carter, op. cit., reports of January 28, 1966 and February 18, 1966,

96. U.S. Senate, Committee on Commerce, 1966, op . cit., Robert White, p. 173 (also testimony of Leland Haworth, p. 93; Thomas Malone, p. 136; and Herbert Hollomon, p. 185 in these hearings); U.S. Senate, Committee on Interior and Insular Affairs, 1966, op. cit., testimony of W.O. Roberts, p. 353.

97. NOAA, Report on Weather Modification. FY69-71 (NOAA: Rockville, MD, 1973). NOAA was later assigned the job, but, as this title reflects, not until after three years went by.

98. In marked contrast to Science Advisor Hornig's strong interest in the SST during the same years, described in Horwitch, Mel, Clipped Wings: The American SST Conflict (Cambridge: MIT Press, 1982), p. 136. In late 1970s, the White House Science Office

ordered the NRC Climate Research Board study, CO, and Climate: A Scientific Assessment (1979).

99. Gordon McDonald, head of the 1966 NAS weather modification panel, states that all agencies combined had planned to be spending $149 million annually by fiscal 1970, three times the amount his panel had proposed. McDonald, op. cit., p. 77.

100. Newell, op. cit., p. III-3; NAS, 1973, op. cit., p. 160. The ICAS report breaks down spending on inadvertent weather and climate modification by agency; of the $434,000 spent in fiscal 1966, $21,400 was spent by NSF, $170,000 by ESSA, and $50,000 by the Department of Defense. See also McDonald, 1968, op. cit., p. 81; Sewell, op. cit., pp. 14-15. In considering these figures, however, it is important to remember that research justified on bases other than weather modification was often of substantial importance to the understanding of anthropogenic climatic change. For example, the development of general circulation models at the Geophysical Fluid Dynamics Laboratory was supported for the study of the spread of fallout from nuclear tests (1967) and drought in the Sahel (1974), but the same models were also used to examine the effects of rising greenhouse gas concentrations. NSF, Annual Report on Weather Modification, 1967; Hammond, Allen L., "Modeling the Climate: A New Sense of Urgency," Science 185:1145-7 (September 27, 1974), p. 1146. Victor and Clark, op. cit., describe the many uses of GCMs on pp. 48-50. This point suggests looking at the recent consolidated Federal budgets for global change with a skeptical eye; in many cases, what looks like new spending is an old program with a new rationale. On the other hand, not all of the money spent on inadvertent weather and climate modification went toward greenhouse effect or even climate research; some was directed toward temporary, local effects.

101. U.S. Senate, Committee on Commerce, op. cit., 1968, testimony of George S. Benton, p. 96; U.S. House of Representatives, Committee on Appropriations, 1974, op. cit., testimony of Robert White, p. 743.

102. Then NOAA, see note 39 above.

103. ESSA and NOAA submissions in U.S. House of Representatives, Committee on Appropriations, Appropriations for State Justice Commerce .art 3 for FY71 (1970), p. 853. Similar statements can be found for the following years at the following page numbers in the same document series. 1966 (FY67): 545; 1967 (FY68): 521, 541, 604; 1968 (FY69): 976; 1969 (FY70): 737; 1970 (FY71): 725, 853, 937; 1971 (FY72): 236, 239; 1972 (FY73): 910, 913; 1973 (FY74): 703-5, 746; 1974 (FY75). In earlier years, CO_2 monitoring was not split out from basic research or weather modification budgets.

104. On the history of science funding, see Smith, Bruce, op. cit. The fate of climatic change research funding in the mid-1970s is discussed below, section 5.

105. Holden, op. cit., p. 280, comments on the skillful use Anderson made of the Congressional committee system to advance his vision of the weather modification program.

106. The popular analogy made between weather modification and nuclear technology described in section 2 above is one indication of weather modification's cache. A sense of it can be gained in Landsberg, H.E., "Climate Made to Order," Bulletin of the Atomic Scientists 17:370-374 (November, 1961). A number of extraordinarily ambitious weather modification projects, like the damming of the Bering Straits, were proposed in the 1960s.

107. NOAA, Annual Reports on Weather Modification. FY69-71, FY72. FY73; NAS, 1973, op. cit.; see NOAA budget presentations listed above in note 103.

108. Pomerance, Rafe, "The Danger of Climate Warming: A Public Awakening," EPA Journal, December, 1986, pp. 15-16.

109. EPA, Can We Delay a Greenhouse Warming? (Washington: EPA, 1983).

110. Wallen, C.C., WMO Bulletin 31:86-94 (1982), p. 86.

111. Jacobsen, Sally, "Walter On Roberts on the Atmospheric Global Pollution and Weather Modification," Bulletin of Atomic Scientists, March, 1973, p. 46. At the same time, environmentalists probably had trepidations about the wisdom of weather modification techniques with which they may have affiliated atmospheric scientists. Roberts, for instance, had called for "maximum possible mastery of the atmospheric environment," in U.S. Senate, Committee on Commerce, 1966, op. cit., p. 104.

112. Scientific caution is reflected in almost every document of the period. See, for instance, CEQ, 1970, op. cit., pp. 103-104.

113. Sullivan, 1961, op. cit., p. 29.

114. Jacobsen, op. cit., p. 42. For references to present holders of this view, see Mathews, Jessica, "Global Climate Change," Issues in Science and Technology 4:61-68 (1987).

115. President's Science Advisory Committee, op. cit., pp. 48-49; Walsh, John, "Pollution: PSAC Panel Takes a Panoramic View," Science 150:1006-1008 (November 19, 1965).

116. NSF, Annual Report. 1966, p. xiv and pp. 34-40; U.S. Senate, Committee on Commerce, 1966, op. cit., testimony of Leland Haworth, p. 93.

117. Quote from NAS, 1966, op. cit. vol. 1, p. 10. See also Schaefer, op. cit. ; McCormick, op. cit.; Bryson, op. cit., pp. 52-56. These ideas were probably circulating well before publication.

118. "Is Man Spoiling the Weather? What the Experts Say," U.S. News and World Report, August 19, 1968, p. 61.

119. White eventually led the final effort to redefine the weather modification program, in 1974. Domestic Council, Environmental Resource Committee, Subcommittee on Climate Change, Federal Role in Weather Modification, reprinted in Congressional Research Service, Primer on Climatic Variation and Change, prepared for the House Science and Technology Committee (1976).

120. Wallen, C.C., WMO Bulletin 31:86-94 (1982), p. 87; and WMO Bulletin "Air Pollution Meteorology," 18(4):230-232 (October, 1969). McCormick later became the head of EPA's Meteorological Division.

121. At the two major conferences organized by the WMO on weather modification, for example, there were no papers on inadvertent modification. WMO #339, Proceedings of the WMO/IAMAP Scientific Conference on Weather Modification (Geneva: WMO, 1974). WMO #443, Papers Presented at the Second WMO Scientific Conference on Weather Modification, co-sponsored by IAMAP and AMS, (WMO: Geneva, 1977).

122. Scandinavian research on the greenhouse effect was begun as part of a pollution monitoring network in the 1950s. See, for example, Bischof, Walter, "Periodical Variations of the Atmospheric Carbon Dioxide Content in Scandinavia," Tellus 12:216-226 (1960). Tellus, edited by Bert Bolin, carried regular reports on this research.

123. The CO_2 monitoring items in NOAA budgets can be found in note 103 above.

124. Kay, David, and Skolnikoff, Eugene, "International Institutions and the Environmental Crisis: A Look Ahead," in Skolnikoff and Kay, eds., Global Eco-Crisis, (Madison: University of Wisconsin Press, 1972), p. 310.

125. Study of Critical Environmental Problems, Man's Impact on the Global Environment (Cambridge: MIT Press, 1970), part I, chapter 2 and part II, chapter 1.

126. Victor and Clark, op. cit., p. 38.

127. As with climatic change more generally, concern about SST-caused climatic change brought forth by scientists was adopted by the activist community only after a substantial lag. The primary fears of anti-SST citizens' groups were sonic booms and economic waste. They did not incorporate fears about climatic change voiced as early as 1965 (see note 47 above) into their literature until early 1971. See Hohememser, Kurt, "The Supersonic Transport," Bulletin of Atomic Scientists, December, 1966; Samuelson, Robert J., "The SST and the Government: Critics Shout into a Vacuum," Science 157:1146-1151 (September 8, 1967); Ruppenthal, Karl M., "Heat, Cold, Radiation, and the Boom," Nation, May 29, 1967; Shurcliff, William, SST and Sonic Boom Handbook

(Citizens League Against the Sonic Boom, 1969); "The SST: Riding a Technological Tiger," Time, October 3, 1969; and Horwitch, op. cit., chapter 15, pp. 275-281, pp. 297-299, p. 309, and pp. 318-320.

128. NAS, 1966, op. cit., vol. 1, p. 11; Sullivan, 1965, op. cit., p. 1. Among those who voiced concern on the matter in the late 1960s were Vincent Schaefer, a member of the NAS panel; W. O. Roberts, director of NCAR; and Congressman Emilio Daddario, later the prime mover behind the Congressional Office of Technology Assessment. These views are included in Saturday Review, (untitled sidebar), May 7, 1966, p. 64; and Sullivan, Walter, "There Is Peril, Too, In Growing Technology," New York Times, March 24, 1968, p. IV:15.

129. Horwich, op. cit., p. 287; SCEP, op. cit., p. xi, pp. 15-18.

130. Horwich, 1982, op. cit., p. 283.

131. Webster, Bayard, "Scientists Ask SST Delay Pending Study of Pollution," New York Times, August 2, 1970, p. I.

132. Landsberg, 1970, op. cit., p. 1272; Finger, F.C.; Mclnturff, R.M., "Meteorology and the Supersonic Transport," Science 167:16-25 (January 2, 1970), p. 24.

133. Ultimately most of this money was devoted to research on ozone depletion. Lindsey, Robert, "US Aide Concedes SST Could Affect Stratosphere," New York Times, August 5, 1970, p. 70; "Study Panel Named on Effects of SST," New York Times, February 4, 1971, p. 69; Horwich, 1982, op. cit., p. 291, 313-314, and 335-337; NAS, 1973, op. cit., pp. 2123.

134. Study of Man's Impact on Climate (SMIC), Inadvertent Climate Modification (Cambridge: M.I.T. Press, 1971).

135. Cain, Melissa, "Carbon Dioxide and Climate," pp. 75-100 in Kay and Jacobson, eds., op. cit., p. 90. SMIC is one of the most heavily-cited early documents on anthropogenic climatic change. See, for example, Congressional Research Service, Weather Modification: Programs, Problems, Potential, and Policy (1978), p. 439.

136. NAS, op. cit., 1973, p. xii. The panel selected 1980 as the target date for "when the knowledge base for understanding inadvertent climate change should be in order."

137. Speech reprinted in New York Times, April 16, 1974, p. 12. This speech was cited as the basis for the reformulation of the WMO research program at the June, 1974 meeting of the WMO Executive Committee, Abridged Report of the WMO Executive Committee. 1974, (Geneva: WMO, 1974), p. 58.

138. The CIA prepared a study in the spring of 1974 that projected 40 years of bad weather. The existence of the report was revealed in the New York Times on May 21, 1976, p. 2., and it is discussed by Deborah Shapley in her review of Stephen Schneider's The Genesis Strategy, New York Times Book Review, July 18, 1976, pp. 3-4.

139. White, Robert, "Climate at the Millenium," in WMO #537, Proceedings of the World Climate Conference (Geneva: WMO, 1979), p. 4.

140. In a letter to Maurice Strong, the chairman of the Stockholm conference, Carroll Wilson wondered "how and in what ways one might develop a kind of network of the rather limited number of key influential people in a certain number of countries around the world who are globally conscious and who have a vision extending to the end of this century and beyond and who have a deep concern for the environment in its broadest sense." Wilson to Strong, October 30, 1972, Wilson papers, M.I.T. Archives, Box 44, File 1818.

141. U.S. House of Representatives, Committee on Science and Technology, Subcommittee on Environment and Atmosphere, National Climate Policy Act (1976), testimony of Robert White, pp. 287-295.

142. U.S. House of Representatives, Committee on Science and Technology, op. cit., (1975), p. 47.

143. U.S. House of Representatives, Committee on Science and Technology, Environmental Monitoring, 1977, op. cit., testimony of Edward Epstein, pp. 118-125. As of 1977, no stations operated by countries other than the U.S. were meeting the requirements laid down by the WMO for background pollution monitoring. By 1981, three such stations (out of 8 called for) were operating. WMO Bulletin 30:200 (1981), 30:277 (1981) and 31:380 (1982).

144. Or perhaps in an entente with the Soviet Union. The mid-1970s were a period of détente in U.S.-U.S.S.R. relations, and the Soviets have traditionally been strong in meteorology. The Soviet Union covers one of the few large areas of the planet that would have been inaccessible to U.S. observers.

145. By 1976, the U.S. was operating stations at Pt. Barrow, Alaska and Western Samoa as well as Hawaii and Antarctica. Ibid.

146. U.S. Committee for GARP, Understanding Climatic Change: A Program for Action (Washington: NAS, 1975), p. 105.

147. The U.S. Committee for GARP called for the 1980s and 1990s to be designated as "international climatic decades." U.S. Committee for GARP, op. cit., p. 106.

148. Subak, Susan, "A Survey of U.S. Environmental Organizations' Activities in Sustainable Development," paper for the Ford Foundation, 1988 (cited by Festa, op. cit.,

p. 77); Cahn, Robert, ed., An Environmental Agenda for the Future (Washington, DC: Island Press, 1985).

149. The data center relied on financial support from the U.N. Environmental Program, WMO Bulletin 28:190 (1976). It's worth noting that EPA's interest in this issue did not emerge until its capability for addressing short-term, visible issues was being suppressed by the early Reagan Administration. On EPA's workload in the 1970s, see Andrews, Richard, "Energy and Environment: Implications of Overloaded Agendas," Natural Resources Journal, 19:487-502 (1979).

150. This research is summarized in the Department of Energy's state of the art reports, published in 1985. I still believe the potential for water shortages has been overlooked as a possible coalition-building strategy for advocates of additional greenhouse effect research.

151. These were eventually published as National Research Council, Geophysics Study Committee, Energy and Climate (Washington: NAS Press, 1977). They were first presented to the 1974 meeting of the American Geophysical Union.

152. "DOE Sets Interagency CO_2 Research Priorities," Science News 112:375 (December 3, 1977).

153. U.S. Department of Energy, Carbon Dioxide and Climate: Summaries of Research in FY88 (Oak Ridge: DOE, 1989), p. xii.

154. The report was Carbon Dioxide Assessment Committee, Changing Climate (Washington: NAS, 1983).

155. Victor and Clark, op. cit., pp. 51-53, note that the radiation absorption spectrum of the greenhouse gas N_20 has been known for many years, but this gas was only recently incorporated into climate models. They connect this failure to both the relationship of the researchers to energy policy and the dominance of the goal of comprehending the carbon cycle in the internal scientific research agenda.

156. Glantz, Michael, "A Political View of C02," Nature 280:189-190 (July 19, 1979).
157. Bromberg, Joan, "'TFTR: The Anatomy of a Programme Decision," Social Studies of Science 12:5 pp.59-583 (1982).