

Social Studies of Science

<http://sss.sagepub.com>

Scientific Elites and the Making of US Policy for Climate Change Research, 1957-74

David M. Hart and David G. Victor

Social Studies of Science 1993; 23; 643

DOI: 10.1177/030631293023004002

The online version of this article can be found at:
<http://sss.sagepub.com/cgi/content/abstract/23/4/643>

Published by:



<http://www.sagepublications.com>

Additional services and information for *Social Studies of Science* can be found at:

Email Alerts: <http://sss.sagepub.com/cgi/alerts>

Subscriptions: <http://sss.sagepub.com/subscriptions>

Reprints: <http://www.sagepub.com/journalsReprints.nav>

Permissions: <http://www.sagepub.co.uk/journalsPermissions.nav>

This paper presents a case study of the role of scientific élites in mediating between science and politics, securing support for research and shaping the interests of the rank and file. We apply a 'garbage can' model, which posits that science, policy and politics typically evolve in separate, unconnected streams, each with its own momentum. In this model, élites may act strategically as 'policy entrepreneurs' to take advantage of occasional temporary opportunities, or 'windows', to influence the policy and science streams. Our case study is of US policy toward research related to the Greenhouse Effect, from the International Geophysical Year in 1957 to the aftermath of the 1972 UN Conference on the Human Environment.¹ We trace the evolution of two research programmes – carbon-cycle research and atmospheric modelling. The major political strategies followed by the relevant élites connected with these programmes were concerned with the pursuit of professional autonomy, with weather modification and with environmentalism. Changes in élite strategy followed mainly from events outside science, in the policy and politics 'streams', rather than from scientific findings.

Scientific Elites and the Making of US Policy for Climate Change Research, 1957–74

David M. Hart and David G. Victor

As Michael Mulkay has pointed out, scientific élites mediate the relationship between bench scientists and the political community, setting the priorities of the former by allocating resources such as jobs and grants and negotiating for these resources with the latter.² These élites received substantial academic attention in the 1960s and 1970s,³ but in recent years social scientific interest in them has waned somewhat. Sociologists and anthropologists have concentrated on working scientists,⁴ whereas the few political scientists and policy analysts interested in science have focused on politicians.⁵ Exactly how scientific élites mediate between the bench and the hearing room

Social Studies of Science (SAGE, London, Newbury Park and New Delhi), Vol. 23 (1993), 643–80

– and, in particular, the ways in which they advance agendas in the political arena – deserves closer attention.

With the intention of helping to fill this gap, along the lines proposed by Mulkay in 1976,⁶ this paper presents a case study of elite behaviour in the development, through 1974, of US policy and scientific research related to the possibility that humans were changing the global climate.⁷ Although the greenhouse effect is now a central concern of international environmental policy, our study pertains to a period in which no regulatory or other response was seriously considered. Policy debate centred on what ought to be studied and how much money should be spent on particular lines of research. ‘Policy’, therefore, means the allocation of funding for research and the organization and management of research institutions, not ‘greenhouse policy’ as it is used today.

We use the term ‘elite’ to denote a small group of leading scientists and administrators in a field. The oceanographic and atmospheric science elites whom we follow in this paper include such figures as Robert White (chief of the US Weather Bureau and its successor agencies), Roger Revelle (director of the Scripps Institute of Oceanography), and Thomas Malone (chairman of the National Academy of Sciences’ Committee on Atmospheric Science). These individuals (and others like them) were active in Federal policy making and influential among their professional colleagues, because of their positions, prestige and personalities.

We take a particular interest in three aspects of ‘elite’ behaviour: (1) the use made of scientific information in gaining funding and high-level policy attention; (2) the implicit strategies adopted with respect to other actors in the policy process; and (3) the manner by which control was exerted over research programmes of less politically powerful scientists. The links between elite behaviour, science policy and research programmes are complex. Scholars have made many efforts to construct ideal typical models of these relationships. We describe three of these here in order to contrast them with a fourth – John Kingdon’s ‘modified garbage can’, which we have adopted to frame our case.⁸

At one end of the spectrum is Michael Polanyi’s view that the scientific agenda is autonomous from politics and other social forces.⁹ This agenda unfolds through the internal logic of experimentation, discovery and debate. Public funding for science accelerates progress on that agenda, more or less efficiently depending on the extent to which the government attempts to attach strings to funds. Scientific

élites are responsible for defending the boundaries of their fields from the intrusions of the incompetent, in part by articulating the unique cultural and material benefits provided by science.

At the other extreme, the relationship of science to policy has been seen as one of enthusiastic and opportunistic responsiveness. The fictional 'Dr Grant Swinger', whose motto is 'something always comes along', personifies this view.¹⁰ Scientists will adapt their rhetoric and, if necessary, their agendas to reflect the perceived objectives of policy-makers, be it the 'war on cancer' or 'star wars'. Scientific élites, in this perspective, are little more than special pleaders for privileged interest groups. Their authority and expertise give them a powerful claim on resources as long as they take care to construe favourably their contribution to a politically attractive cause. By controlling resources and through their professional standing, élites can reorient the activities of the rank and file when circumstances demand such a change.

A third view sees science as shaped gradually by society on a time scale measured in decades or centuries. Social movements, like Progressivism, may spawn new scientific agendas if they can construct suitable problems and mobilize resources to support research. Similarly, a major policy shift, such as the emergence of the Cold War, can transform the scientific agenda. In such cases, contending political coalitions may recruit scientific support and generate contending research programmes.¹¹ Scientific élites may provide buoyancy to, or be a drag on, such influences on science, depending on their social positions, the goals of the movement or policy, and other factors. For instance, Middtun and Baumgartner suggest that French energy modellers made little change in their work in response to the environmental movement in the 1970s because of their dependence on the state, whereas many new American energy models were built because leading modellers could seek non-state sources of support.¹²

A fourth ideal type, which we find to make the best sense of our case, is the 'garbage can' model of the policy process.¹³ In this model, science, policy and politics evolve in separate, unconnected streams. Each stream has an internal momentum and its own dynamics, but on occasion, 'policy windows' open that cause the streams to interact. These windows provide opportunities to link 'solutions in search of problems', which are being developed by scientists, with political 'problems in search of solutions', to use Kingdon's terms. During the limited time that a window is open, science, policy and politics may all be reoriented before settling into new routines. This is a loosely

coupled system, and the interaction between scientific and political agendas is jerky and inconsistent.

Scientific élites may play a central role in identifying policy windows and seizing advantage of them. In advocating that their ideas should be incorporated into policies during these moments of opportunity, members of an élite act as 'policy entrepreneurs'. They invest resources, such as time and authority, in this advocacy. Successful policy entrepreneurship creates allies and wins resources for their scientific field.

In the garbage can model, the pursuit of autonomy (as in the first model above) is only one strategy that science policy entrepreneurs might adopt, for instance, when they perceive the streams to be very far apart. The strategic behaviour of entrepreneurs in the garbage can model also has some resonance with the blatant opportunism described by the second model above, but it is typically constrained by the internal momentum of scientific research programmes and of existing entrepreneurial strategies. Social and political events may provide openings for change (as in the third model above), but in the garbage can model, members of scientific élites, in their roles as advisers to government and as public spokesmen, may be able to shape this process; they are more active than the third model envisions them to be. In addition, change is sudden, rather than gradual, in the garbage can.

We hope to show that the garbage can model, while inelegant, allows a sensible interpretation of the actions and statements of leading scientists and administrators in the case of early US climate change research. It provides insights into the mechanisms by which élites mediate between science and policy, and thus extends Mulkay's original proposal. We do not claim that our work provides a rigorous and general test of the various models or that this case is representative, but we hope that it provides ideas that are useful to other researchers. In addition, this case is of interest because of the continuing political and public concern about global climate change, for which the events described here provide a basis.

The case study begins with a brief overview of the historical context of research on anthropogenic climate change from its origins in the nineteenth century to the International Geophysical Year (IGY) of 1957–58. Since the IGY, the US government has regularly supported research programmes on key topics related to climate change. The bulk of the paper addresses the interplay of science, policy, and politics in the governance of these programmes.

Setting the Stage: The Emergence of the Greenhouse Effect as Two Interesting Problems in the Early Cold War

In the nineteenth and the first half of the twentieth centuries, scientific interest in the idea that human activity could change the global climate was sporadic; neither scientists nor governments created sustained research programmes in this area. Only after World War II did scientific attention coalesce into two distinct discourses, each with an élite cadre of leaders, that together ultimately constituted a research programme on the greenhouse effect. One, the carbon-cycle discourse,¹⁴ encompassed the questions of whether and why atmospheric concentrations of carbon dioxide (CO₂) were increasing. The other, the atmospheric modelling discourse, included an interest in what would happen to the climate if higher concentrations of CO₂ were reached. Although not entirely compartmentalized, the two discourses were divided by discipline and institutional bases. Both benefited from US military patronage.

The greenhouse effect hypothesis – that changes in the abundance of atmospheric CO₂ would affect climate – was first put forward in 1824.¹⁵ The earliest calculations of the radiative strength of CO₂ were made in the 1860s¹⁶ and, by 1900, both T.C. Chamberlain and Svante Arrhenius had independently proposed models predicting that temperature would rise if enough CO₂ was added to the atmosphere, whether from natural sources or by people. Chamberlain's theory proposed that cycles of CO₂, outgassing by volcanoes and uptake by weathering of rocks could explain the cycles of glacial and interglacial periods. Arrhenius explored the prospect that CO₂ from fossil fuels might raise temperatures.¹⁷ This work was largely ignored for the next five decades. In the 1930s and 1940s, G.S. Callendar published a series of papers on climate change, CO₂ concentrations and fossil fuel CO₂ emissions,¹⁸ but he left no programmatic or institutional legacy.

The invention of nuclear weapons appears to have raised the legitimacy of the idea that humans could change the climate, and Federal nuclear programmes supported the first institutionalized research on the topic. In the early 1950s, scientists like John von Neumann and politicians like Senator Clinton Anderson linked nuclear weapons and climate change together as comparable threats to civilization.¹⁹ Fallout from atmospheric nuclear testing was popularly believed to be a climate change agent; in 1962, George Kimble of Indiana University noted:

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'.²⁰

These fears prompted the Atomic Energy Commission (AEC), the Office of Naval Research (ONR) and the Department of Commerce to sponsor investigations of the dispersion of fallout in the atmosphere.²¹ While scientists ultimately concluded that fallout did not affect the weather,²² this Federal support stimulated interest in the two subjects that, many years later, fused into a coherent greenhouse effect research programme: the carbon cycle and global atmospheric circulation.

Carbon-cycle researchers try to understand the distribution and flows of carbon in nature. Chamberlain's geological theory, involving volcanoes and erosion, was an early attempt to describe the carbon cycle. The oceans are also a major part of the carbon cycle, and oceanographers dominated the study of the carbon cycle in the US through the 1970s.²³ Atmospheric nuclear tests provided a ready-made experiment for tracing the circulation of carbon by injecting a large, known amount into the atmosphere at a specified time and place. Important early papers on carbon cycling grew out of radioactive tracer studies funded by the Atomic Energy Commission (AEC).²⁴ The Office of Naval Research also supported this work, because basic studies of oceanic properties were important for characterizing the role of the oceans in the carbon cycle.

By measuring the proportions of radioactive and non-radioactive isotopes of carbon, Hans Suess established by 1955 that some, but not all, of the carbon released by burning fossil fuel dissolved in the ocean.²⁵ This 'Suess effect' motivated Roger Revelle, the director of Scripps Institute of Oceanography, to propose a more thorough investigation of ocean/atmosphere carbon exchange. If CO₂ (as Gilbert Plass argued [see below]) had a unique infra-red spectrum and (as Suess argued) remained in the atmosphere, then an anthropogenic greenhouse effect was possible. In 1957, Revelle and Suess (with ONR and AEC funding) collaborated to develop the calculations and models to explain the chemical and dynamical processes that governed uptake of CO₂ in the oceans.²⁶

Revelle's work with Suess was the first of his many contributions, both scientific and political, to the history of anthropogenic climate change research. His scientific leadership made Scripps the top carbon-cycle research centre in the US. More importantly, as we

show below, Revelle regularly advised Federal agencies and leaders, using his influence to advance this research programme.

The second relevant discourse was built around efforts to model the behaviour of the atmosphere, and to predict how it would change if concentrations of CO₂ increased. This discourse grew out of mathematical meteorology. Like carbon-cycle research, atmospheric modelling was advanced by US military support.

John von Neumann, the father of scientific computing, proposed the development of a computer model for weather forecasting immediately after World War II. In 1946, he secured Navy funding for a Numerical Meteorology Project, based at the Institute for Advanced Study in Princeton. The Project's first successful forecasts were made from 1951 to 1953. Von Neumann was careful to cultivate the Air Force, Navy and the Weather Bureau for additional support of the project: for example, he timed his efforts to be helpful to the Weather Bureau for the 1952 budget cycle. Similar tactical efforts were launched with prominent scientists within the Department of Defense. In 1954, a Joint Numerical Weather Prediction Unit (JNWPU) was established at Princeton, supported by the Weather Bureau and the military.²⁷ In 1955, with the same sponsors, von Neumann and Harry Wexler of the Weather Bureau formed a spin-off group from JNWPU, under Joseph Smagorinsky, to conduct research into the general circulation of the atmosphere. In 1960, this project, initially based in Washington, moved to Princeton and, after a series of name changes, became the Geophysical Fluid Dynamics Laboratory (GFDL).²⁸

The core of the atmospheric modelling discourse, then, can be traced to von Neumann's projects. Other researchers also contributed to it. For example, in 1956, Gilbert Plass published an article on the infra-red spectra of CO₂ and water vapour and the prospect of global warming that is widely cited by reminiscing scientists as marking the beginning of a broad revival of carbon dioxide theories of climate change.²⁹ Plass's theory, like Chamberlain's before him, was motivated by the theory of climate change on a geologic scale due to changing CO₂ concentrations, but it also provided support for Callendar's theory of climate change due to combustion of fossil fuels. Like Callendar, however, Plass did not build an institutional base to carry on this work, nor did he seem to engage deeply in the ongoing debate about the likelihood of an anthropogenic greenhouse effect.³⁰

In the very early days of carbon-cycle research and climate

modelling, then, élite scientists, like Revelle and von Neumann, responded to the opportunities offered by the Cold War; it even provided scientists with working materials, such as isotopes and satellite imagery. In responding to events outside of science, élite scientists were entrepreneurial, selling their military benefactors on the value that their results would provide. Moreover, by creating new institutions and political alliances, the élites in each field provided incentives to less politically active colleagues to pursue work in carbon cycling and atmospheric modelling. These themes are pursued in more detail in the next three sections.

Climate Change Research During the IGY and Immediately After

By 1957, the two discourses and two élites that would direct sustained attention to anthropogenic climate change were established. The carbon-cycle discourse paid special attention to the oceans, and was coordinated by élite oceanographers like Roger Revelle. The atmospheric modelling discourse evolved out of numerical weather prediction, the success of which gave John von Neumann, Joseph Smagorinsky and others prominent in the nascent field of atmospheric science, excellent contacts, credibility and access to funds.

In the late 1950s and early 1960s, both élites seem to have followed a strategy of seeking to establish relatively autonomous institutions that insulated their research programmes from political oversight. Atmospheric modellers took a role in setting up institutions like the National Center for Atmospheric Research (NCAR) in 1960; their funding through large line items in Federal budgets allowed some research related to anthropogenic climate change to proceed as a matter of course. On the other hand, fund-raising for new, widely dispersed atmospheric monitoring stations, which were central to the advance of the carbon-cycle research programme, stalled after the euphoria of the International Geophysical Year (which had stimulated such monitoring) had worn off. The IGY was not institutionalized, and Revelle and his colleagues did not mobilize strongly to secure broad support. We attribute this lack of entrepreneurship to the lack of a suitable policy window before 1965, and the absorption of this élite's energy elsewhere in the 'garbage can'.

The Suess effect provided the scientific rationale for atmospheric monitoring. Suess had shown that all of the CO₂ produced by humans would not be absorbed by the oceans; therefore, if CO₂ emissions were rising over time, atmospheric CO₂ concentration should also be

rising. Following the rhetoric that humanity was 'conducting a great geophysical experiment', the most quoted line of his famous 1957 paper with Suess, Revelle repeatedly argued that the great experiment ought to be 'adequately documented'.³¹ Revelle proposed to begin to collect a reliable data series for atmospheric composition, including the proportion of CO₂, during the IGY.

As chair of the Scientific Committee on Ocean Research (SCOR) of the International Council of Scientific Unions (ICSU), Revelle drew upon discretionary funds for planning meetings for the IGY atmospheric monitoring programme.³² He then shepherded the programme, which was conceived after IGY planning was well under way, through both the international and US approval and appropriation processes. The oceanographic élite apparently encountered no political difficulty with this allocation, at either level. American scientists seem to have dominated international planning for IGY. At the national level, 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; atmospheric monitoring of CO₂ seems to have been no exception.³³

Scripps Institute, which Revelle headed, hired C. David Keeling to run the atmospheric monitoring programme. Keeling's station at Mauna Loa, Hawaii, has been maintained almost continuously since 1957, and has provided perhaps the only undisputed fact in the debate about the greenhouse effect – namely, that atmospheric CO₂ is rising steadily. These measurements were the first systematically done in the 'free' troposphere, away from local pollution sources.³⁴ The fact that they were unambiguous quickly became clear. By 1961, Walter Sullivan of *The New York Times* was reporting that CO₂ levels were rising because of 'the steady discharge of fumes by smoke stacks, automobile exhaust pipes, jet engines, forest fires, and burning rubbish heaps'.³⁵ Only about half of the carbon dioxide estimated to be released by human activities seemed to be absorbed by the ocean; the rest remained in the atmosphere.

Despite its scientific importance and the ease with which it was funded as part of the IGY in 1957, the CO₂ monitoring programme struggled for money during the early 1960s. In 1964, Keeling was unable to produce any useful measurements at Mauna Loa because of financial difficulty.³⁶ Other stations also encountered severe financial difficulties.³⁷ Yet, the oceanographic élite does not seem to have aggressively pursued funding at this time.

This hesitancy is interesting. After all, Revelle had noted in his 1957 testimony that a 20% rise in CO₂ (which could be expected by about 2000) could cause 'considerable change' in the climate. A 1982 retrospective by a high official of the World Meteorological Organization (WMO) commented, 'It is rather surprising that during the 1960s no more interest was shown in expanding monitoring of atmospheric CO₂ to other remote places on the globe'.³⁸ Furthermore, the Kennedy Administration had a demonstrated interest in meteorology. Elite scientists might have relied on Cold War rhetoric to appeal to the Administration for the support of research related to climate change; the advocates of this research occasionally offered the 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.³⁹

Historical reviews by scientists have suggested that the élite was timid because of uncertainty about the effects of CO₂ on the climate. As one put it, leading oceanographers had 'an understandable reluctance . . . to openly engage in debate', since the inference of a greenhouse effect from rising CO₂ levels was not conclusive.⁴⁰ Nevertheless, atmospheric models, although they were crude in the early 1960s, pointed in an alarming direction, as we discuss below. It seems to us that these models ought to have stimulated curiosity and data collection, rather than deterred it. In addition, the continuity of belief in the greenhouse effect among the hypothesis's most important advocates, like Revelle and Bert Bolin (both of whom remained involved in the issue from the 1950s to the 1990s), is far more striking than the degree of scepticism with which they used model results.⁴¹

A better explanation centres on the institution-building endeavours of the oceanographic élite in the 1960s. This decade brought Big Science to oceanography, in the form of projects like Mohole (an ill-fated attempt to drill through to the Earth's crust underwater), the International Indian Ocean Expedition, and the International Decade of Ocean Exploration (IDOE).⁴² The directors of oceanographic institutions engaged in a running battle to maintain control over the agenda of these programmes, using the argument that 'basic research' was best managed by institutions autonomous from public oversight. They hesitated, for example, to support early proposals for the IDOE because it seemed too mission-oriented.⁴³

These efforts took the time and energy of this small group, and the atmospheric monitoring stations had little role in their plans. In addition, greenhouse effect research seemed to be a 'mission';

entrepreneurship on behalf of a costly set of atmospheric monitoring stations would undermine the rationale of 'basic science' that provided Scripps and its like with autonomy.⁴⁴ Such entrepreneurship was pursued, as we describe below, when the political and policy streams in the 'garbage can' allowed atmospheric monitoring to be reframed as 'basic science'. Commitment to the strategy of building autonomous institutions constrained elite behaviour in the early 1960s.

During the same period, atmospheric modellers began to produce full studies of how the atmosphere would respond to elevated levels of CO₂, but this was not the major focus of their work. In fact, they viewed the rise in CO₂ as little more than a changing parameter with which to examine the general problem of atmospheric thermal equilibrium.⁴⁵ Like carbon-cycle researchers, atmospheric modellers adopted the view of the greenhouse effect as a geophysical experiment. Unlike atmospheric monitoring stations, though, support for atmospheric models was safely institutionalized, and the modellers had little incentive to seek policy attention for climate change research.

Atmospheric models, like carbon dioxide monitoring, demand substantial capital investments, particularly in computers; extensive training is also required. Because of these huge start-up costs, modelling research was highly concentrated until the late 1970s. Initial development centred on one institution, GFDL (a unit of the Commerce Department), and only one other institution, NCAR (funded by the National Science Foundation) emerged in which systematic research on (and ultimately modelling of) the atmosphere was conducted.⁴⁶ Both received their funding on an institutional, rather than a project, basis. NCAR did use the threat of anthropogenic climate change as a justification for the modelling work it pursued. For example, in 1962, a 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 weather patterns.⁴⁷

NCAR's expression of concern about anthropogenic climate change, though, was pro forma, one of a long list of interests. It is unlikely that modelling research would have been threatened without it.

Federal investments in building institutions to study the transfer of heat and circulation in the atmosphere produced their first scientific

returns very rapidly. In 1961, Fritz Moller co-authored a paper with S. Manabe of GFDL on the radiative balance of the atmosphere which included consideration of CO₂ and water vapour. The topic of the paper was suggested by GFDL Director Smagorinsky, who was orchestrating the research programme begun by von Neumann and Wexler in 1955. This work provided the basis for models of the general circulation of the atmosphere. Manabe went on at GFDL to develop the first full models to test the sensitivity of the atmosphere to changes in CO₂. Moller continued his work on the radiation balance and, in 1963, published a refined paper.⁴⁸

Atmospheric modelling, with its roots in numerical weather prediction and its original military sponsorship, seems to have followed the pattern of postwar physics in establishing and maintaining relative autonomy. With this margin of comfort, modellers could comfortably swim in their own science 'stream', to use Kingdon's term. The great 'experiment' of the greenhouse effect was an interesting scientific problem, but not one that demanded a political effort on their part.

Failed Entrepreneurship in the Mid-1960s

From the perspective of the 1990s, when the threat of global warming is seen by many as urgently requiring a very large research investment, the mid-1960s appears to have been a fallow period in the scientific history of the greenhouse effect. The raw materials were available for a synthesis of carbon cycle and atmospheric modelling results to project the rough timing of anthropogenic climate change. Such a synthesis would have been useful in the search for additional political support and funding for a greenhouse-oriented research programme, but there was no momentum towards it from either élite. The two discourses did not intersect until after 1965, when a debate over weather modification provided a policy window that was compatible with the oceanographic élite's vision of climate change research as basic research. Their push failed to achieve its goal, however, for reasons that have more to do with policy and politics, than with whether their scientific findings were compelling.

Carbon-cycle research in the 1960s was chiefly concerned with the refinement of calculations of fluxes among the cycle's various 'boxes' – namely, atmosphere, oceans, biosphere and earth. By the middle of the decade, a consensus had been achieved on the basic facts, and

lasted until the late 1970s.⁴⁹ This consensus, with Keeling's data, provided a crisp answer to the question of how fast the atmospheric CO₂ level was rising: a steady (if exponential) 0.2% per year.⁵⁰ Carbon-cycle researchers did not, by definition, ask the question: How fast are *greenhouse gas* levels rising? The fact that trace gases other than CO₂, such as methane (CH₄), nitrous oxide (N₂O) and chlorofluorocarbons (CFCs), also absorb and re-radiate infra-red wavelengths went unexamined, even though spectral analysis techniques that would have permitted this finding were well-known.⁵¹ N₂O, for example, was known to be an active absorber in the infra-red; in fact, this absorption was used in the 1920s to identify N₂O as a component of the atmosphere. Interest in other greenhouse gases would have opened up a very large new research agenda, but such questions were pre-empted because the debate about the greenhouse effect was framed in terms of carbon-cycle research.⁵²

The atmospheric modelling community in the 1960s did not demand information about the internal workings of the various boxes, nor about the range of potential greenhouse gases. Their interest was not how quickly atmospheric CO₂ was rising, but rather what the climate would be like after such a rise. The crucial study of this period, a 1967 paper by Manabe and R.T. Wetherald (hereafter MW67) of GFDL, presented the effects on global temperature in the GFDL model when certain perturbations in the global radiation budget, including (but not centred on) doubled CO₂, were plugged in.⁵³ Following MW67, 'double CO₂' became the benchmark for the modellers (and later for the larger discourse about the greenhouse effect as a whole), presumably because it was easy to handle in the model and because it was an interesting test of model sensitivity.

The modellers ignored information that was emerging from oceanography that is crucial to understanding climate change, because they were interested only in climate equilibria. For example, in 1966, a parameter for oceanic thermal diffusivity (the rate at which heat is dispersed from the top of the ocean to the bottom) was calculated by Walter Munk. This parameter could have helped atmospheric modellers to forecast the timing and rate of atmospheric response to rising CO₂.⁵⁴ If the oceans have a high thermal diffusivity then the entire hydrosphere must be heated before atmospheric temperature rises; the oceans would have a large 'thermal inertia' effect. On the other hand, if diffusivity is low, atmospheric temperature will respond more rapidly to increased concentrations of greenhouse gases. But oceanic thermal diffusivity was the territory of

oceanographers, and MW67 looked only at equilibrium atmospheric temperature levels, rather than at the specific process and timing by which those levels would be reached. By design, equilibrium calculations ignored the rate of change.

As we argued above, we think the view that these two discourses meandered along separately because their leaders were uncertain about the greenhouse effect is untenable. During the mid-1960s, a profusion of competing hypotheses about climate change were put forward. In addition to fallout and greenhouse gases, agents from dust to rocket exhaust to oil spills received consideration as possible influences on climate.⁵⁵ These diverse hypotheses generated disputes within the scientific community about such basic results as whether human activities were likely to cause surface temperatures to rise or fall. In 1968, for instance, a *Time* magazine essay, 'The Age of Effluence', used the dispute between warming and cooling theories to exemplify the difficulty of obtaining useful scientific advice on policy issues. 'It seems undeniable that some disaster may be lurking in all this, but laymen hardly know which scientist to believe'.⁵⁶ Such competition within science seems more likely to stimulate efforts of devotees of a particular hypothesis than to deter them.

A second possibility is that the entrepreneurship of the members of these élites in this period was restrained because they had confidence that, like an experiment, a greenhouse effect could be controlled. 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'.⁵⁷ PSAC, for example, considered the deployment of reflective material in the atmosphere as a technological fix to counter rising CO₂.⁵⁸ If people were inadvertently changing the climate for the worse, they could deliberately change it for the better. This vision of control reflected the optimism of other proposed 'mega-projects' (such as vast civil engineering works using nuclear explosives) that were driven by Cold War competition. Such proposals diminished in the late 1960s, just as the intellectual climate outside of science changed and the greenhouse effect emerged as an important scientific priority.

The weakness of these two arguments is that élite behaviour changed during the mid-1960s *before* any significant change in knowledge about either the prospects for a technological fix or the likelihood of the greenhouse effect. The champions of climate change research became more entrepreneurial when a window opened that

allowed them to position their work as a basic science problem, rather than as the subject of a Federal mission. Such positioning made it possible for Revelle and his colleagues to pursue heartily larger funding and the more thorough integration of carbon-cycle research and atmospheric modelling.

An early indication of this change is a chapter in a 1965 report on the environment of the President's Science Advisory Committee (PSAC), that put projections of fossil fuel use, the associated rise in atmospheric CO₂ and its climatic consequences together on a time scale for the first time. The chapter, written by Revelle, issued a warning that the greenhouse effect was slipping through the cracks of the Federal government's research and pollution control programmes.⁵⁹ It seems to have made little substantive impression on policymakers, although NSF Director Leland Haworth did mention the concern in Congressional testimony, and in the introduction to the 1966 NSF annual report.⁶⁰

The carbon dioxide/climate problem (as the issue came to be known) did not spark the PSAC report; rather, far more visible environmental insults, like water pollution and smog, were the major motivations. Nor did Revelle's chapter spring from new scientific evidence – although it did refer to the ongoing research programmes, including the work of Manabe and Wetherald. Revelle simply seems to have taken an otherwise unrelated opportunity presented to him as a member of a PSAC panel to try to bring the science and policy streams together. In the context of President Johnson's call for more basic research on pollution,⁶¹ such entrepreneurship made good political sense.

In the mid-1960s, the main policy venue for arguments that portrayed greenhouse-related research as basic science was the Federal weather modification programme. This programme both stimulated these arguments and proved to be the biggest barrier to efforts to integrate and expand carbon-cycle and atmospheric modelling research. Bureaucratic and Congressional opposition frustrated the entrepreneurship of the oceanographic and atmospheric science élites in this arena.

Beginning with the experiments of Nobelist Irving Langmuir in the late 1940s,⁶² the heart of Federal weather modification efforts was precipitation enhancement (especially rain-making by seeding clouds in the Western states), which had little to do with climate change on a global scale. The efficacy of these efforts was the subject of a schism, lasting thirty years, between the bicoastal atmospheric scientific élite

and those from the nation's interior, both academic and commercial, who ran cloud-seeding operations. Even though there were reasonable scientific grounds for doubting the effectiveness of cloud seeding (doubts that have been borne out over time),⁶³ the dispute also had an important cultural dimension. In the mid-1960s, the technique's academic advocates tended to be from less prestigious institutions; they were also associated with commercial operators who aimed to make money by making rain.⁶⁴ Elite scientists resented any research allocations that smacked of pork barrel politics, as cloud seeding proposals did.⁶⁵ They also saw 'basic research' funding to be in competition with the expansion of 'applied' weather modification operations, which would have required large funding – for instance, for the purchase of airplanes.

The net effects of this schism were 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 called unsuccessfully for the reorganization of governance of these activities. These 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).⁶⁶

An important tactic in the struggle was the creation of a 'broad' view of weather modification by members of the élite, like Robert White, an atmospheric modeller turned administrator, who held key weather and environmental policy positions in the Commerce Department in this period. While the 'narrow' view of weather modification was that of trial-and-error attempts to manipulate nature,⁶⁷ the 'broad' view called for a deeper understanding of the principles behind the weather – that is, more basic research. Knowledge of the physical basis of the climate, it was claimed, would eventually lead to better operational weather modification methods.⁶⁸ As the NSF put it, 'the program of weather-control research should be an integral part of basic meteorological research'.⁶⁹ One element of the broad view was that research on 'inadvertent' weather modification was at least as worthy of support as (in White's words) 'the advertent steps we take'.⁷⁰ The greenhouse effect was said to be among the most important subjects for research in this area, although it seems reasonable to question the contribution such research might have made to precipitation enhancement or other local weather modifications that were the main objectives of Federal policy.

The broad view was deployed in support of Commerce against the claims of the Interior Department's Bureau of Reclamation. As the home of the Weather Bureau and the National Oceanic and Atmospheric Administration (NOAA), Commerce had an internal professional constituency and a turf interest in the broad view. This turf fight was mirrored by a jurisdictional dispute between the Senate Commerce and Interior Committees. NSF and NAS appointed special study panels on weather and climate modification, whose reports became touchstones in the Congressional debate; these panels attempted to put some clout behind the 'majority' view among atmospheric scientists – that 'large-scale engineering and development efforts in weather modification' were 'premature'.⁷¹ 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.⁷²

In 1966, the debate grew more intense, as the lines were drawn for a 'major battle' over competing bills. One bill, sponsored by Senator Clinton Anderson, would have placed the Interior Department in control of weather modification policy; the other, sponsored by Senator Warren Magnuson of the Commerce Committee, would have put the Commerce Department in this position. Western Senators, like Anderson (who chaired the Interior Committee), tended to see scientists as more interested in producing scientific papers than water for their States. As Gordon McDonald, the chairman of the NAS panel put it, 'Congress has displayed considerable impatience'.⁷³

While the bills were under consideration, the NAS panel revised its interim report and seemed to seek 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 climate change should be one of four main programmatic thrusts of Federal weather modification work.⁷⁴ This line received the endorsement of members of the atmospheric science elite, such as Thomas Malone, chair of the NAS Committee on Atmospheric Science and W.O. Roberts, the director of NCAR.⁷⁵ The NAS study, like the PSAC report, offered no new scientific information to support climate change research; like its many successors, it served as an authoritative compilation and summary of existing information.

Congress never passed a bill resolving the matter, nor did the agencies involved come to a negotiated settlement. It appears that the advocates of the 'broad' view of weather modification were not

particularly successful in the desultory infighting that characterized the late 1960s. Funding for Federal weather modification activities nearly doubled between fiscal years 1966 and 1971, rising from \$9 million to \$16 million. As defined by Federal budget figures, though, research on 'inadvertent weather and climate modification' actually declined by about 20%, from \$434,000 to \$360,000.⁷⁶ In 1968, for instance, the research director of Commerce's Environmental Science Service Administration (ESSA, a precursor of NOAA) stated that his agency planned to establish another station for monitoring atmospheric CO₂. (ESSA was already funding Keeling at Mauna Loa.) For several years, however, ESSA was unable to procure funds for another station, and it was not constructed until fiscal 1974.⁷⁷ In the worst year, the agency received only a third of the amount it requested for this line item.⁷⁸

In spite of their entrepreneurship in support of a programme on 'inadvertent weather modification' centred on greenhouse effect research, the atmospheric and oceanographic élites were unable to squeeze through the policy window they perceived to secure funding in the second half of the 1960s. Furthermore, élite activity had little apparent influence on the trajectory of greenhouse-related research. The carbon dioxide monitoring programme did not grow, but it did keep extending the upward curve with its limited data. Atmospheric modelling continued to be carried on in a parochial way, with only the most tenuous relationship to public problems. No new incentives were offered to sway the commitments of researchers proceeding in established directions.

On the matter of research funding, politics and policy were in stalemate. The scientific élites attempted to use the science in hand as a 'solution in search of a problem', but were unable to forge a convincing link. They helped to stymie the grandiose plans of Senator Anderson and other proponents of rain-making, but research on inadvertent climate modification was not accepted as a substitute for rain-making as a legitimate use of Federal dollars. In a period of technological optimism, 'inadvertent climate modification' lacked the glamour of weather modification.

Almost the same scientific case became convincing in a different political context after 1970. Early weather modification experiments had raised expectations that control of precipitation and storms would become routine.⁷⁹ These and other disappointed hopes helped to contribute to the rise of the American environmental movement in the late 1960s and early 1970s. This movement created a policy

window that the élite successfully used to gain financial and political support, and to reorient rank and file research.

The Early 1970s: The Greenhouse Effect as an Environmental Problem

Environmentalism in the US has deep historical roots. The European encounter with the untamed New World landscape provoked both a dominant attitude of exploitation and conquest, and dissident perspectives calling for moderation and preservation.⁸⁰ American political history has been marked by occasional revivals of the latter, typically in periods in which social movements flourish and many new political associations are formed.⁸¹ The late 1960s to the early 1970s was the most recent of such periods. The scope of environmentalism is truly breath-taking, from efforts to preserve urban parkland to concern about the global climate.⁸² The 'green' impulse is malleable. Although one would be hard-pressed to support the claim that scientific findings catalyzed the rebirth of environmentalism, they have shaped the movement's particular directions within the large degree of freedom its guiding ideas permit. At the same time, environmentalism has acted as a midwife for new scientific agendas in many fields, legitimating them and providing constituencies for their results.

The conversion of the greenhouse effect into an environmental problem deemed worthy of sustained public research support in the early 1970s illustrates these interactions. Elite oceanographers and atmospheric scientists helped to define anthropogenic climate change as an environmental issue; in the process, they broadened the scope of environmentalism, and secured public resources for research on the problem. They also engineered a retooling of non-élite scientists, so that, by 1974, a coherent new thrust had emerged around such problems as detection of climate change.⁸³

From the perspective of the 'garbage can' model, the interest of the American public in environmental problems appeared as a policy window to these élites, who had by this time been working for more than a decade on anthropogenic climate change with meagre funding. During this period, politicians could gain by appearing environmentally-conscious – but, as always, they sought problems that could be 'solved' (from the public point of view) without disrupting established routines. The greenhouse effect was abstract and not threatening in the near future, but the research that was called for

could be portrayed as the single most important step that could be taken in a key area of the new and attractive realm of international environmental policy.

As in the 1960s, the established research programmes inched forward in the early 1970s. Climate modellers continued to refine their estimates of the atmospheric response to elevated CO₂. Notably, the GFDL model was improved to represent the general circulation of the atmosphere in three dimensions, and to explore changes in the circulation with elevated CO₂. New data from deep sea expeditions and satellites that had a deep impact on conceptions of the carbon cycle were just beginning to appear. Nevertheless, the essence of the science of the greenhouse effect remained roughly as it had been – there were no new findings that made it significantly more or less urgent. Yet, when important scientific figures reflect on the history of the issue, they agree that the Study of Critical Environmental Problems (SCEP) of 1970, and the Study of Man's Impact on Climate (SMIC) of 1971, were pivotal publications.⁸⁴

Neither SCEP nor SMIC was technically novel. Most of the reports presented in SCEP, for example, were write-ups of ongoing research; many of these were previously published, or were later published in more complete form elsewhere. What was unusual about them was their interdisciplinary and environmentalist tone. SMIC's inscription is a Sanskrit prayer: 'Oh, Mother earth, ocean-girdled and mountain-breasted, pardon me for trampling on you'. These images convey a dramatically different impression than those of the scientific literature of only a few years earlier, which had seemed to welcome the inadvertent experiment of global climate change – provided that the experiment was well documented and that scientists could learn from it.

SCEP and SMIC were not the first attempts to frame the greenhouse effect as an environmental issue. The 1965 PSAC report discussed above was about pollution; the 1966 NAS weather modification report stated that 'the atmosphere is not a dump of unlimited capacity'.⁸⁵ By 1968, the notion that pollution could modify the climate was a commonplace. *US News and World Report* wrote: 'No matter which [agent of climate change] proves to be more important in the long run, this much seems clear: Pollutants are having an effect on the weather'.⁸⁶ As early as 1969, the World Meteorological Organization's (WMO) Commission on Atmospheric Sciences called for the establishment of a global monitoring network for pollutants suspected of changing the climate, specifically carbon dioxide and particulates.⁸⁷

By and large, though, according to C.C. Wallen of the WMO:

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.⁸⁸

One reason why this policy window was difficult to recognize may have been that many scientists saw CO₂ as an inevitable byproduct of society, not as a potentially controllable emission. The view that, in NCAR Director W.O. Roberts' words, 'We cannot do anything about carbon dioxide', is still widely held today.⁸⁹ Some may have hesitated to connect themselves to a movement that they perceived as radical and anti-technology. At the same time, environmentalists may have had trepidations about the wisdom of weather modification techniques, which they may have associated with atmospheric scientists. Roberts, for instance, in Congressional hearings, had called for 'maximum possible mastery of the atmospheric environment'.⁹⁰ Finally, the strategy of seeking allies within the debate on weather modification took entrepreneurial time and energy, and may have crowded out consideration of the possibilities that environmentalism afforded.

SCEP and SMIC were able to break through these barriers for three reasons. First, they were institutionally outside the norm. While its steering committee included Roger Revelle and Thomas Malone, SCEP's guiding spirit was Carroll Wilson, an MIT management professor with a long and broad involvement in American and international science policy.⁹¹ SCEP was prepared at a meeting in the hills of Massachusetts in July 1970. This meeting was consciously intended to be informal and collegial, bringing together leaders and practitioners from a range of scientific specialties. Wilson did not want the result to be just another stuffy Academy review. His approach seems to have created a critical mass that was both comfortable with the idea of linking science and the environment, and energized to do something about it.

Second, SCEP received wide publicity because of its statements about the supersonic transport airplane (SST).⁹² In 1970, Federal funding for the SST was highly controversial; the issue was a crucial one for the development of the modern environmental movement, since it served as a focus for organizing and ultimately providing one of its most dramatic early victories. Even though SCEP considered the greenhouse effect to be a more important long-term threat to the

climate than the SST, it did recommend 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.⁹³ 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"' by SCEP.⁹⁴ SCEP's recommendation about the SST was covered on the front page of *The New York Times*.⁹⁵ The legitimacy given by SCEP to the issue of SST-caused climate change played a large role in the establishment of the Climatic Impact Assessment Program (CIAP) by the US Department of Transportation, which received \$7 million in fiscal year 1972. Although this sum was small compared to SST development, it was quite large by the standards of anthropogenic climate change research.⁹⁶

Third, and most importantly, Wilson organized SCEP and SMIC with the intention of contributing to the UN Conference on the Human Environment (UNCHE) in Stockholm in June 1972. In devising the conference agenda,⁹⁷ the UNCHE organizers had made a special effort to solicit the views of non-governmental organizations. SMIC was a successor to SCEP: it included a more international group of participants specifically to give SCEP's conclusions with regard to climate more clout in the Stockholm agenda. 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.⁹⁸

The two reports' calls for a global atmospheric monitoring network was approved by the Conference with little discussion.⁹⁹

The imprimatur of the UNCHE opened a domestic policy window that was converted into more funding for research on the greenhouse effect. For example, CO₂ monitoring was moved from the budget category 'inadvertent weather and climate modification' to a new one labelled 'global monitoring of climate change'. The financial difficulties of the late 1960s were alleviated as White's agency sought and secured funding increases of 400% for this item between fiscal years 1971 and 1975.¹⁰⁰ By 1973, (yet another) NAS panel on weather and

climate modification stated that a 'sense of urgency' infused the atmospheric science elite concerned about climate change.¹⁰¹

The urgency was heightened by a number of frightening weather-related anomalies in 1972–74, including the failure of the Peruvian anchovy fishery due to a weather and ocean current pattern now known as El Niño/Southern Oscillation (ENSO), drought in the Soviet Union's breadbasket, and by unseasonably cold winters in much of the US, with related inflation in food prices. In a speech to the UN General Assembly in 1974, Secretary of State Henry Kissinger called for better international research on climatic disasters, and indicated that the US was willing to take the lead.¹⁰² With a little help from Mother Nature, then, climate change research reached the agenda of top US policy-makers.

Kissinger's main concern seems to have been the prospect of political instability in the Third World stemming from climate-related disasters.¹⁰³ The administrators and scientists to whom politicians and policy-makers turned to implement Kissinger's mandate, however, viewed the problem to be the need for better data and forecasting capabilities, including the monitoring of global pollution and projection of its probable impact. Carbon dioxide and other potential agents of climate change were definitely on this agenda, even though the elite saw the anomalies themselves as natural. As Robert White later stated, if natural climatic disasters had not allowed for an increase in interest in climate change, fear of anthropogenic climate change would have.¹⁰⁴

None of these developments – SCEP, SMIC, UNCHE, the bigger budget nor the Kissinger speech – can be said to have been rooted in scientific advances. The same individuals, holding essentially the same views – such as Revelle, White and Malone – continued to play leading roles in the debate. Now, however, they had a message to which their audience was more receptive.¹⁰⁵

If anything, uncertainty within the scientific community about the impact of the greenhouse effect (but not the general principle behind it) was growing during these years. Although Keeling's data showed a continued rise in atmospheric CO₂, this was not correlated with observed temperature, which declined between 1940 and 1970, after rising between 1900 and 1940. The temperature data gave credence to the idea that cooling caused by particulate emissions outweighed greenhouse warming.¹⁰⁶ The disagreement among the 'coolers' and the 'warmers' was sharp enough to cast serious doubt on the viability of the warming hypothesis. Although this uncertainty is not evident

in statements by leading 'warmers', the debate may have fuelled their interest in research policy entrepreneurship. The stations proposed to monitor atmospheric CO₂ (which comprised a large fraction of the budget item discussed above) would also measure turbidity, with the intent of resolving whether particles emitted by human activities would be dispersed globally and remain in the atmosphere for a long enough period of time to disrupt the climate.¹⁰⁷

The debate about anthropogenic climate change also opened up in another way. In 1974, two atmospheric chemists argued for the first time that chlorofluorocarbons (CFCs) were destroying the earth's ozone layer.¹⁰⁸ As a direct result of concern about CFCs as potential destroyers of ozone, V. Ramanathan tested CFCs for infra-red absorption: he found that on a molecule-for-molecule basis they were 10,000 times more effective than CO₂ in trapping heat.¹⁰⁹ During the following year, a study of the radiative properties of other gases – CH₄, N₂O and several others – was also published, and it was found that they, too, were strong infra-red absorbers.¹¹⁰ Thus concerns about the global environment were extended to the ozone layer – and, by accident, also came to encompass a wider range of greenhouse gases, not just CO₂. This effectively coupled the emerging discipline of atmospheric chemistry to the issue of the greenhouse effect, since these 'newly-found' greenhouse gases are chemically active in the atmosphere and, therefore, their radiative effects depend on the extent of this activity.

With an important tweak from Carroll Wilson, research policy entrepreneurs had mobilized to reconstruct the greenhouse effect as a threat to a valued resource, rather than as a way to improve human control over the weather or (before that) a great experiment. Although fears of climate change in no way spurred the UN to hold its 1972 Stockholm Conference, that Conference provided a convenient focal point for their advocacy, as well as (in its resolution on global monitoring) an important resource in the ensuing domestic policy process. While concerns about climate played only a minor role in the SST debate, linkage of the two by SCEP provided valuable publicity and momentum. As a result of these efforts, research on anthropogenic climate change received better funding, and enjoyed higher credibility and public interest than ever before.

This entrepreneurship was not inspired by new findings. Indeed, leading oceanographers and atmospheric scientists seem to have been slow to recognize the policy window, perhaps because of the inertia of earlier strategies of political action. Wilson's lack of prior involve-

ment in the issue seems to have made him more open to perceive the opportunity. We find it difficult to imagine that the 'weight of scientific evidence' would have inevitably moved both policy and science in the directions that they went without élite entrepreneurship and the windows opened by the environmental movement.

Conclusion

We have traced the scientific and political evolution of research on anthropogenic climate change from its beginnings in the 1950s until it reached a new plateau in 1974. During that period, this research was composed of two distinct scientific discourses – one on the carbon cycle and the other on atmospheric modelling – that, by and large, proceeded independently of one another. The threat of global warming was only one motivation for these research programmes. However, élite scientists and administrators were able to use this threat as a rationale to secure allies and resources, with increasing success over time. Their strategies of doing so varied as the opportunities they perceived in their political environment changed.

In the late 1950s, the relevant élites aimed to establish relatively autonomous scientific institutions in which they could pursue their own broad agendas for oceanography and atmospheric science; these included greenhouse effect research as a small component. While the atmospheric modellers succeeded, carbon-cycle research stalled; the oceanographic élite seems to have been occupied with more pressing matters than seeking the expensive atmospheric monitoring stations this research required. In the mid-1960s, élites in both discourses joined and attempted to label research on anthropogenic climate change as basic research on inadvertent weather modification. This rationale did not gain strong enough allies to overcome Congressional and executive branch supporters of precipitation enhancement. Weather modification policy remained deadlocked by jurisdictional disputes well into the 1970s. The rise of the environmental movement provided better opportunities to secure funding for both carbon-cycle and modelling research, and to link the two discourses into a common research programme on the greenhouse effect. The climate, scientific leaders discovered, could be portrayed as a natural resource that needed to be defended from the onslaught of industrialism. However, the élite did not seize this opportunity easily; the entrepreneurship of an outsider seems to have been important in setting an example and providing new energy.

The history of research on anthropogenic climate change between 1957 and 1974 accords nicely with Kingdon's 'garbage can' model of policy making. Science, policy and politics were loosely coupled, and change was discontinuous. The scientific agenda and the political strategies of the scientific élite had substantial inertia. Hence, research policy advocacy was not always vigorous or convincing. Rather than growing from scientific findings, opportunities to shift or accelerate the advance of the agenda often emerged unpredictably from previously unrelated policy arenas. Whether or not leading scientists perceived and took advantage of such opportunities depended on their history, entrepreneurial skill and other factors.

Élite entrepreneurship served as a mechanism by which the scientific agenda was influenced by politics. By seizing an opportunity, the two élites we have examined legitimated and provided support to particular lines of research: they thereby helped to redefine which problems were important. The élites also shaped the beliefs of policy-makers and the public when such opportunities arose – but usually by relying on old scientific information, and not on late-breaking news.

The same scientific case can yield quite different policy results in different circumstances. Very little new scientific information about the prospects of global warming was produced between the late 1960s and early 1970s. In 1969, the projections of CO₂ rise, and of the resultant change in temperature, had little bearing on the turf battle over weather modification. But in 1974, after the greenhouse effect had been positioned as an issue of pollution, essentially the same projections energized the scientific community and helped secure financing for further research on the subject.

The reports of ad hoc, blue-ribbon groups, like NAS panels and SCEP, that summarize the state-of-the-art, played important roles in élite mediation between the bench and the hearing room. These documents provided integrated assessments of the greenhouse effect, showing causes, effects, and forecasts. Representing a presumed consensus, they were a more important political tool for the élite than the normal publications that practitioners use to build the state-of-the-art (that is, technical journal articles). Generating them at strategic moments assisted entrepreneurs to link opportunities to research agendas.

The case also illustrates the limits of the imagination of small, specialized élites. Atmospheric scientists and oceanographers concerned about climate change did not stress social scientific research opportunities that might have assisted them in building their case.

They failed, for example, to consider carefully the potential sources of CO₂, apart from fossil fuel use. The idea that deforestation in developing countries could make a substantial contribution to CO₂ levels, which is widely accepted today, was discounted because scientists assumed that the green revolution would cause slash-and-burn agriculture to dwindle.¹¹ Even today, the international research programme related to global climate change largely excludes the social sciences.

Specialization also constrains the use of policy windows by politicians and bureaucrats. The greenhouse effect was defined through the decades that we have explored (and well into the 1980s) as an 'energy problem'; indeed, in the years after our narrative ends, the Department of Energy was designated as the lead agency on the matter in the US. This conception constrained interest in greenhouse gases other than CO₂ (which, by current estimates, accounts for only perhaps half of the projected greenhouse effect), and slowed the development of a more comprehensive approach to research policy.

The image of a 'garbage can' policy process is not an entirely depressing one for us. A continuing public debate over research policy is healthy for democracy and for science, even if one argues that one result is that a threat as important as the greenhouse effect was not adequately surveyed as early as possible. Indeed, one can argue equally effectively that the internal logic of the scientific agenda in a more Polanyian world might have caused the problem to be ignored, or at least insufficiently integrated for even longer than it has been. Dr Grant Swinger, on the other hand, might have spent only a couple of years on the greenhouse effect before moving on to the next big thing. In the 'garbage can', both sustained attention and rapid change are possible.

●NOTES

This paper is contribution number I.8 to the project on Social Learning in the Management of Global Environmental Risks, located at the Center for Science and International Affairs (CSIA), John F. Kennedy School of Government, Harvard University. Partial support was provided by grants from the US Social Science Research Council, the US National Science Foundation (Grant No. SES-9011503), and the John D. and Catherine T. MacArthur Foundation. This paper is based on two longer papers by the authors: David Hart, 'Strategies of Research Policy Advocacy: Anthropogenic Climatic Change Research, 1957-74', CSIA working paper 92-08

(September 1992) and David Victor and William C. Clark, 'The Greenhouse Effect in the US: A History of the Science up to 1985', available from the authors. We thank Jesse Ausubel, Bill Clark, James Fleming, William Kellogg, Gordon McDonald, Tom Malone, Steve Schneider and Gene Skolnikoff for helpful comments on these papers.

1. This paper assumes basic familiarity with the physical bases of the greenhouse effect; for an introduction, see G.J. MacDonald, 'Scientific Basis for the Greenhouse Effect', *Journal of Policy Analysis and Management*, Vol. 7 (1988), 425–44; or S.H. Schneider, 'The Greenhouse Effect: Science and Policy', *Science*, Vol. 243 (10 February 1989), 771–81.

2. M. Mulkay, 'The Mediating Role of the Scientific Elite', *Social Studies of Science*, Vol. 6 (1976), 442–70.

3. Some examples in politics and economics include R. Lapp, *The New Priesthood* (New York: Harper & Row, 1965); D.K. Price, *The Scientific Estate* (Cambridge, MA: Harvard University Press, 1965); and R.C. Wood, 'Scientists and Politics: The Rise of an Apolitical Elite', in R. Gilpin and T. Wright (eds), *Scientists and National Policy-Making* (New York: Columbia University Press, 1964), 41–72. In sociology, the tradition of research on élites stems from Robert Merton's interest in stratification (for example, 'The Matthew Effect', reprinted in R.K. Merton, *The Sociology of Science: Theoretical and Empirical Investigations* [Chicago, IL: The University of Chicago Press, 1973], 439–59) and includes Harriet Zuckerman's *The Scientific Elite* (New York: Free Press, 1977).

4. For example, B. Latour and S. Woolgar, *Laboratory Life* (Princeton, NJ: Princeton University Press, revised edn, 1986).

5. For example, H. Lambricht, *Presidential Management of Science and Technology* (Austin, TX: University of Texas Press, 1985) and B. Smith, *American Science Policy Since World War II* (Washington, DC: Brookings, 1990). Historians have done better. See, for example, J. Logsdon, 'Missing Halley's Comet: The Politics of Big Science', *Isis*, Vol. 80 (1989), 254–80; and J. Bromberg, 'Anatomy of a Program Decision', *Social Studies of Science*, Vol. 12 (1982), 559–83.

6. Mulkay, op. cit. note 2, 464.

7. A complete history of greenhouse science and politics in the US would have to extend beyond the role of élites. Indeed, this paper is part of a social science research enterprise (see above) designed to provide full histories of the greenhouse effect (and other global environmental issues) in the United States and in other countries.

8. This section draws on B.J. Nelson, *Making an Issue of Child Abuse* (Chicago, IL: The University of Chicago Press, 1984) and J.W. Kingdon, *Agendas, Alternatives, and Public Policies* (Boston, MA: Little-Brown, 1984).

9. M. Polanyi, 'The Republic of Science', *Minerva*, Vol. 1 (1962), 54–73.

10. Swinger was created by (and continues to be created by) Daniel Greenberg, editor of *Science and Government Report*. For a typical 'interview', see 'Gold in the Greenhouse Effect', *SGR* (15 July 1988), 1–2. Latour & Woolgar, op. cit. note 4, provide a less caricatured and cynical, but still congruent, description of the evolution of the research agenda from the viewpoint of the bench scientist.

11. P.A. Sabatier, 'An Advocacy Coalition Framework of Policy Change and the Role of Policy-Oriented Learning Therein', *Policy Sciences*, Vol. 21 (1988), 129–68.

12. A. Middtun and T. Baumgartner, 'Negotiating Energy Futures: The Politics of Energy Forecasting', *Energy Policy*, Vol. 14 (1986), 219–41.

13. Kingdon, op. cit. note 8. See also M.D. Cohen, J.G. March and J.P. Olsen, 'A Garbage Can Model of Organizational Choice', *Administrative Science Quarterly*,

Vol. 17 (1972), 1–25; and March and Olsen, 'The New Institutionalism: Organizational Factors in Political Life', *American Political Science Review*, Vol. 78 (1984), 734–49.

14. There was a mature debate on the carbon cycles of small geographic areas by the early 1960s. By 'carbon cycle' in this paper, we mean the global carbon cycle, rather than the local carbon cycle.

15. In 1824, Fourier described the actions of gases in the Earth's atmosphere as similar to a 'hothouse', thus suggesting the term 'greenhouse effect'. However, Fourier's work was not built upon directly and bears something of the same relation to later atmospheric science as Mendel's work with peas does to modern genetics. See R. Revelle, 'Introduction: the Scientific History of Carbon Dioxide', in E.T. Sundquist and W.S. Broecker (eds), *The Carbon Cycle and Atmospheric CO₂: Natural Variations Archean to Present, Geophysical Monograph*, No. 32 (Washington, DC: American Geophysical Union, 1985), 1–4.

16. J. Tyndall, 'On Radiation Through the Earth's Atmosphere', *Philosophical Magazine and Journal of Science*, Vol. 4 (1863), 200–07, cited in W.W. Kellogg, 'Mankind's Impact on Climate: The Evolution of an Awareness', *Climatic Change*, Vol. 10 (1987), 113–36. Radiative strength is the capacity to absorb and re-emit radiation. The stronger the radiative activity, the greater the capacity to affect climate.

17. S. Arrhenius, 'On the Influence of Carbonic Acid in the Air upon the Temperature of the Ground', *Philosophical Magazine and Journal of Science*, Vol. 41 (1896), 237–76; T.C. Chamberlain, 'A Group of Hypotheses Bearing on Climatic Changes', *Journal of Geology*, Vol. 5 (1897), 653–83; Chamberlain, 'The Influence of Great Epochs of Limestone Formation Upon the Constitution of the Atmosphere', *ibid.*, Vol. 6 (1898), 609–21; Chamberlain, 'An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis', *ibid.*, Vol. 7 (1899), 545–84, 667–85, 751–87.

18. Most of Callendar's papers directly related to the CO₂ hypothesis appeared in the pages of the *Quarterly Journal of the Royal Meteorological Society* between 1938 and 1942. For a review of this work, see the exchange between Callendar and G.D. Robinson, *Quarterly Journal of the Royal Meteorological Society*, Vol. 73 (1947), 127–50 and Vol. 74 (1948), 81–82. For more on the early history of the greenhouse effect, see J. Ausubel, 'Annex 2: Historical Note', in US National Research Council, *Changing Climate* (Washington, DC: National Academy Press, 1977), 488–91. For abstracts of many of the papers in the history of the greenhouse effects, see M.D. Handel and J.S. Risbey, 'An Annotated Bibliography on Greenhouse Effect Change', *Climatic Change*, Vol. 21 (1992), 91–256.

19. J. von Neumann, 'Can We Survive Technology?', *Fortune* (June 1955), 108. Senator Anderson claimed to carry this von Neumann article around with him in his breast pocket. Other examples of the analogy include J. Walsh, 'Weather Modification', *Science*, Vol. 147 (15 January 1965), 274–76; and NSF, *Annual Report on Weather Modification, 1964* (Washington, DC: US GPO, 1965), 2. In 1966, Anderson put forward a bill 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. See US Senate, Committee on Interior and Insular Affairs, Subcommittee on Water and Power Resources, *Weather Modification* (1966), 145 and 196.

20. G. Kimble, 'But Somebody Does Something About It', *The New York Times Magazine* (8 July 1962), 11. A similar story is 'Is Man Upsetting the Weather?', *US News and World Report* (11 November 1963), 46–48. In April 1955, the US Congress's 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 R.G. Hewlett and J.M. Holl, *Atoms for Peace and War* (Berkeley, CA: University of California Press, 1989), 291–95.

21. The annual reports of the AEC and the Department of Commerce regularly report on fall-out research. See, for example, AEC, *Annual Report, 1974*, 98–100.

22. The opinion of experts on the climatic impact of fall-out is given in 'Is Man Upsetting the Weather?', op. cit. note 20, 47, and in the US Senate, Committee on Interior and Insular Affairs (1966), op. cit. note 19, testimony of W.O. Roberts, 362.

23. In the 1970s, deforestation was recognized as an important part of the carbon cycle, and that it made a major contribution to rising atmospheric CO₂ concentrations. At that time, ecologists who studied tropical ecosystems joined the carbon cycle discourse.

24. In the early 1950s, E.C. Anderson, supported by the US Army's Office of Ordnance Research through the Los Alamos Laboratory and the Atomic Energy Commission, wrote some of the earliest papers on the distribution of C-14 in nature: see J.A. Arnold and E.C. Anderson, 'The Distribution of Carbon-14 in Nature', *Tellus*, Vol. 9 (1957), 28–32. A sample of papers that used nuclear tests to provide tracers can be found in the International Symposium on Trace Gases and Radioactivity, reported in *Journal of Geophysical Research*, Vol. 68 (1963), 3745–4015. On the usefulness of atmospheric tests, see US House of Representatives, Committee on Appropriations, *National Science Foundation – International Geophysical Year* (1957), testimony of Roger Revelle, 113; and Hans Suess, 'Fuel Residuals and Climate', *Bulletin of the Atomic Scientists*, Vol. 17 (1961), 374–75.

25. H.E. Suess, 'Natural Radiocarbon and the Rate of Exchange of Carbon Dioxide Between the Atmosphere and the Sea', in *Nuclear Processes in Geologic Settings* (Washington, DC: National Academy of Sciences, 1953), 52–56; Suess, 'Radiocarbon Concentration in Modern Wood', *Science*, Vol. 122 (2 September 1955), 415–17. At the time Suess was at the US Geological Survey.

26. R. Revelle and H.E. Suess, 'Carbon Dioxide Exchange Between the Atmosphere and Ocean and the Question of an Increase of Atmospheric CO₂ During the Past Decades', *Tellus*, Vol. 9 (1957), 18–27. Two related papers in the same issue are H. Craig, 'The Natural Distribution of Radiocarbon and the Exchange Time of Carbon Dioxide Between Atmosphere and Sea', *ibid.*, 1–17; and Arnold & Anderson, op. cit. note 24. For more on the sources of excess CO₂ in the atmosphere and an attempt to reconcile some of the issues raised in these three papers, see G.S. Callendar, 'The Effect of Fuel Combustion on the Amount of Carbon Dioxide in the Atmosphere', *Tellus*, Vol. 9 (1958), 421–22.

27. W. Aspray, *John von Neumann and the Origins of Modern Computing* (Cambridge, MA: MIT Press, 1990), Chapter 6; J. Smagorinsky, 'The Beginnings of Numerical Weather Prediction and General Circulation Modelling: Early Recollections', *Advances in Geophysics*, Vol. 25 (1983), 3–37.

28. Wexler was trained as a meteorologist at MIT by Carl Gustaf Rossby: see A. Spilhaus, 'Measurements of Ozone in the US – An Historical Note', *EOS*, Vol. 72 (1991), 410–11. Rossby, who later moved to the University of Chicago, was a close adviser to von Neumann on the numerical weather prediction project described here.

Other project scientists, such as Jule Charney and Norm Phillips, took the top posts in meteorology at institutions across the country when they left Princeton. Robert M. White, later head of the Weather Bureau and NOAA, was also a modeller by training, beginning his career at the Air Force's meteorology lab in Cambridge, MA, where he encountered Phillips and others from von Neumann's group in the late 1950s: White interview, 25 June 1991.

29. G.N. Plass, 'The Carbon Dioxide Theory of Climatic Change', *Tellus*, Vol. 8 (1956), 140–54.

30. Plass continued to publish on atmospheric radiation into the 1960s, but his later papers were almost entirely in journals which the flagbearers of the CO₂/climate debate would not normally read. Several papers written by Plass in the 1960s are cited in M. Chahine, 'The Atmosphere', Chapter 5 in R.N. Colnell (ed.), *Manual of Remote Sensing* (Falls Church, VA: American Society of Photogrammetry, 1983).

31. Revelle & Suess, op. cit. note 26, 19; Revelle testimony in US House of Representatives, Committee on Appropriations, *National Science Foundation – International Geophysical Year* (1956), 473. The rhetoric of the 'geophysical experiment' was not alarmist. Early students of climate change welcomed higher temperatures and considered cooling a 'deterioration' of climate. See note 18 and accompanying text.

32. E. Miles, 'COSPAR and SCOR: The Political Influence of the Committee on Space Research and the Scientific Committee on Ocean Research', in W. Evan (ed.), *Knowledge and Power in a Global Society* (Beverly Hills, CA: Sage, 1981), 133–52, at 140.

33. H. Bullis, 'The Political Legacy of the IGY', in US House of Representatives, Committee on International Relations, Subcommittee on International Security and Scientific Affairs, *Science, Technology, and American Diplomacy* (1976), 48–50. The atmospheric monitoring programme was discussed in the hearings of the US House of Representatives, Committee on Appropriations: (1) *National Science Foundation – International Geophysical Year* (1956), op. cit. note 31, 473; (2) *National Science Foundation – International Geophysical Year* (1957), op. cit. note 24, 106–08; (3) *Review of the First Eleven Months of the International Geophysical Year* (1958), testimony of H. Wexler, 39.

34. Atmospheric CO₂ measurements extend back to the nineteenth century, but were extremely unreliable. More accurate measurements date from 1920, but no single group made measurements over an extended period of time, and calibration between isolated and sporadic research efforts was poor. A Scandinavian CO₂ monitoring programme predates Keeling's in the 1950s, but its sites were so close to industrial facilities that the readings could not reliably be generalized to the global atmosphere. For a description, see S. Fonselius et al., 'Carbon Dioxide Variations in the Atmosphere', *Tellus*, Vol. 8 (1956), 176–83.

35. 'Air Found Gaining Carbon Dioxide', *The New York Times* (11 September 1961), 29. Two early publications of the IGY programme data are C.D. Keeling, 'The Concentration and Isotopic Abundances of Carbon Dioxide in the Atmosphere', *Tellus*, Vol. 12 (1960), 200–03; and B. Bolin and C.D. Keeling, 'Large Scale Atmospheric Mixing as Deduced from the Seasonal and Meridional Variations of Carbon Dioxide', *Journal of Geophysical Research*, Vol. 68 (1963), 3899–920. A recent publication of the data is C.D. Keeling, *Atmospheric CO₂ Concentrations – Mauna Loa Observatory, Hawaii 1958–1986*, Environmental Sciences Division, Oak Ridge National Laboratory, Publication No. 2798 (1986).

36. C.D. Keeling, 'Atmospheric Carbon Dioxide Variations at the Mauna Loa Observatory', *Tellus*, Vol. 28 (1976), 538–51. Earl W. Barrett provides some of the funding history of this programme in 'Inadvertent Weather and Climate Modification', *CRC Critical Reviews in Environmental Control*, Vol. 6 (1975), 15–90, esp. 17 and 30–31; Keeling and C.W. Brown, 'Concentration of Atmospheric Carbon Dioxide in Hawaii', *Journal of Geophysical Research*, Vol. 70 (1965), 6053–76, has a note on 6075 on severe 'financial and logistical obstacles'.

37. Conservation Foundation, *Implications of Rising Carbon Dioxide Content of the Atmosphere* (Washington, DC: Conservation Foundation, 1963), 10.

38. C.C. Wallen, 'Monitoring the Atmospheric CO₂ Concentration', in W. Bach, A.J. Crane, A.L. Berger and A. Longhetto (eds), *Carbon Dioxide: Current Views and Developments in Energy/Climate Research* (Dordrecht: D. Reidel, 1982), 3–29, at 8.

39. On the Administration's interest in UN activity on climate research in the early 1960s, see J.S. Perry, 'The Global Atmospheric Research Program', *Reviews of Geophysics and Space Physics*, Vol. 13 (1975), 661. For comments on the USSR's potential benefit from the greenhouse effect, see US House, Committee on Appropriations (1956), op. cit. note 31, 473; NSF, *Annual Report on Weather Modification*, 1962, 2. Apparently, there has been a reciprocal use of this argument by Soviet opponents of attempts to prevent the greenhouse effect.

40. W.W. Kellogg, *Effects of Human Activities on Global Climate* (WMO No. 486) (Geneva: World Meteorological Organization, 1977), 1.

41. Revelle died in July 1991. Bolin is still active.

42. A 'Big Science' programme for atmospheric researchers, launched in the same period, was the Global Atmospheric Research Project (GARP). GARP's organizers spent the five years between 1962 and 1967 haggling over organizational structure, with the main point at issue being whether its programme would be designed to support basic or applied research objectives. See Eugene Skolnikoff, *Science, Technology, and American Foreign Policy* (Cambridge, MA: MIT Press, 1967), 173–76; and B. Bolin, 'The Objectives of the Global Weather Experiment and Overall View of the Accomplishments of GARP', in *International Conference on the Results of the Global Weather Experiment and their Implications for WWW*, GARP No. 26 (Geneva: World Meteorological Organization, 1985), 4–16.

43. E. Wenk, Jr, *The Politics of the Ocean* (Seattle, WA: University of Washington Press, 1972), 216–18. See also D. Palmer, L. King and W.W. Shannon, 'Oceanographers and the Federal Patron: Perceptions of Agency-University Relations', *Social Studies of Science*, Vol. 18 (1988), 73–90, esp. 85–86; and D.S. Greenberg, *The Politics of Pure Science* (New York: Free Press, 1965), 171–206.

44. In 1966, Congress placed oceanographic research under the control of a Marine Sciences Council chaired by the Vice-President (and modelled on the Space Council), much to the dismay of the President's Science Advisory Committee (PSAC). This mechanism advanced utilitarian priorities and further encroached on basic research in oceanography. A short account of this period can be found in Lambright, op. cit. note 5, 30–37.

45. S. Manabe and F. Moller, 'On the Radiative Equilibrium and Heat Balance of the Atmosphere', *Monthly Weather Review*, Vol. 89 (1961), 503–32. For a scientific history of earth radiation budget research, see G.E. Hunt, R. Kandel and A.T. Mecherikunnel, 'A History of Presatellite Investigations of the Earth's Radiation Budget', *Reviews of Geophysics*, Vol. 24 (1986), 351–56; and F. House, A. Gruber,

Hunt and Mecherikunnel, 'History of Satellite Missions and Measurements of the Earth Radiation Budget (1957–1984)', *ibid.*, 357–77.

46. On the founding of NCAR, see 'Atmospheric Institute Founded', *The New York Times* (10 October 1958), 33; and 'NSF to Open Research Center', *ibid.* (27 June 1960), 5. For GFDL, see Smagorinsky, *op. cit.* note 27.

47. NSF, *Annual Report on Weather Modification*, 1962, 2.

48. Manabe & Moller, *op. cit.* note 45; F. Moller, 'On the Influence of Changes in the CO₂ Concentration in Air on the Radiation Balance of the Earth's Surface and on the Climate', *Journal of Geophysical Research*, Vol. 68 (1963), 3877–86. These early models were incapable of accurately describing the atmospheric response, because they did not include non-radiative transfer of heat through the atmosphere and because they were only one-dimensional (that is to say, altitude only, not global circulation). Other early GFDL papers on modelling of the general circulation are S. Manabe, J. Smagorinsky and R.F. Strickler, 'Simulated Climatology of General Circulation with a Hydrologic Cycle', *Monthly Weather Review*, Vol. 93 (1965), 769–98; Manabe and Strickler, 'Thermal Equilibrium of the Atmosphere with a Convective Adjustment', *Journal of the Atmospheric Sciences*, Vol. 21 (1964), 361–85; Smagorinsky, Manabe and J.L. Holloway, 'Numerical Results from a Nine-level General Circulation Model of the Atmosphere', *Monthly Weather Review*, Vol. 93 (1965), 727–68.

49. In the late 1970s, results from extensive sampling of the oceans overturned the consensus by revealing that the movement of carbon within the oceans was much more complex than the box model allowed.

50. Conservation Foundation, *op. cit.* note 37, 6.

51. The work was done in 1975 and 1976: see V. Ramanathan, 'Greenhouse Effect Due to Chlorofluorocarbons: Climatic Implications', *Science*, Vol. 190 (6 October 1975), 50–52; W.C. Wang, Y.L. Yung, A.A. Lacis, T. Mo and J.E. Hansen, 'Greenhouse Effects Due to Man-Made Perturbations of Trade Gases', *Science*, Vol. 194 (12 November 1976), 685–90.

52. Interestingly, the first paper on the sources of methane (CH₄) in the atmosphere was given at the 1963 International Symposium on Trace Gases and Radioactivity, in which Bolin and Keeling presented their first effort at an atmospheric model based on the carbon dioxide fluxes detected by Keeling: see T. Koyama, 'Gaseous Metabolism in Lake Sediments and Paddy Soils and the Production of Atmospheric Methane and Hydrogen', *Journal of Geophysical Research*, Vol. 68 (1963), 3971–73; Bolin & Keeling, *op. cit.* note 35.

53. S. Manabe and R.T. Wetherald, 'Thermal Equilibrium of the Atmosphere with a Given Distribution of Relative Humidity', *Journal of the Atmospheric Sciences*, Vol. 24 (1967), 241–59. An important contribution of this paper was its inclusion of feedback mechanisms by which the direct impact of CO₂'s re-radiation of heat on temperature might be mediated. These feedbacks were the impetus for developing more complete two- and three-dimensional models of the global circulation and its response to rising concentrations of carbon dioxide. In addition to those for CO₂, calculations were performed for fluctuations in the solar constant, cloudiness, tropospheric humidity, stratospheric water vapour and ozone concentration. For example, in response to interest in the effect of supersonic transport aircraft on the surface temperature of the earth due to emissions of water vapour into the stratosphere, MW67 predicted a 2°C increase if stratospheric water vapour concentration rose fivefold.

54. W.H. Munk, 'Abyssal Recipes', *Deep Sea Research*, Vol. 13 (1966), 707–30.

55. For reviews, see Helmut Landsberg, 'Man-Made Climatic Changes', *Science*, Vol. 170 (18 December 1970), 1265–74, and Council on Environmental Quality, *Annual Report for 1970* (Washington, DC: US GPO, 1970). 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: see P.E. Damon and S.M. Kunen, 'Global Cooling?', *Science*, Vol. 193 (6 August 1976), 447–53.

56. 'Age of Effluence', *Time* (10 May 1968), 52–53.

57. R. Revelle, 'The Role of the Oceans', *Saturday Review* (7 May 1966), 41.

58. 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, DC: US GPO, 1965), 127.

59. PSAC, op. cit. note 58, 48–49; see also appendix Y4.

60. NSF, *Annual Report, 1966*, xiv and 34–40; US Senate, Committee on Commerce, *Weather Modification* (1966), 93.

61. PSAC, op. cit. note 58, iii. Malone argues that this report was a vehicle for PSAC to attain greater control over atmospheric science research from the Weather Bureau: Malone interview, 15 May 1991.

62. W.R.D. Sewell, 'Weather Modification: Social Concern and Public Policy', in Sewell (ed.), *Modifying the Weather: A Social Assessment* (Victoria, BC: University of Victoria Press, 1973), 1–49.

63. By 1973, enthusiasm for weather modification had notably soured. In the preface to the proceedings of the 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'. See WMO No. 339, *Proceedings of the WMO/IAMAP Scientific Conference on Weather Modification* (Geneva: WMO, 1974).

64. Matthew Holden, Jr, 'Politics and Weather Modification', in Sewell (ed.), op. cit. note 62, 261–322, at 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, US Senate, Committee on Interior and Insular Affairs, *Weather Modification* (1964).

65. One gets this sense of scientific élitism in the news reports of *Science* on weather modification: see, for example, Walsh, op. cit. note 19; E. Langer, 'Weather Bureau', *Science*, Vol. 141 (9 August 1963), 508; L.J. Carter, 'Weather Modification: Panels Want Greater Effort', *ibid.*, Vol. 151 (28 January 1966), 428–30; and Carter, 'Weather Modification: Senate Bills Stir Rival Agencies', *ibid.* (18 February 1966), 805–08.

66. These reports are reviewed in General Accounting Office, *The Need for A National Weather Modification Research Program* (Washington, DC: US GPO, 1974), 9–15. They were followed by one from the Domestic Council in 1974 and another from NAS in 1975.

67. In addition to rain-making, the weather modification programme included projects on fog and hail suppression and storm guidance.

68. Examples include Malone's testimony in US Senate, Commerce Committee, *Weather Modification* (1968), 136; W.O. Roberts's testimony in US Senate, Committee on Interior (1966), op. cit. note 19, 353; and R. Revelle, 'Water Resources Research in

the Federal Government', *Science*, Vol. 142 (22 November 1963), 1027–33. In our interview with him on 25 June 1991, White did not recall any direct conflict between intentional and inadvertent weather modification funding priorities, although he clearly opposed the former and supported the latter.

69. NSF, *Annual Report on Weather Modification*, 1962, 33.

70. US Senate, Committee on Commerce (1966), op. cit. note 60, testimony of Robert M. White, 173.

71. The quotes are drawn from NSF, *Annual Report on Weather Modification*, 1964, 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, DC: NAS Press, 1966) and National Science Board, Special Committee on Weather Modification, *Weather and Climate Modification* (Washington, DC: NSF, 1965).

72. Walsh, op. cit. note 19, 275.

73. Quotes are from Gordon McDonald, 'Federal Government Programs in Weather Modification', in Robert G. Fleagle (ed.), *Weather Modification: Science and Public Policy* (Seattle, WA: University of Washington Press, 1968), 69–86, at 70 and 77. McDonald sees the dispute as one that pits the executive against the legislature. However, we believe the argument that the fight was among cross-branch coalitions makes more sense. See also Robert G. Fleagle, 'Background and Present Status', in *ibid.*, 3–17, at 12.

74. Conclusions of the NAS panel (1966), op. cit. note 71, at Vol. 1, 10; Vol. 2, Chapter 4. See also Carter, op. cit. note 65, reports of 28 January 1966 and 18 February 1966.

75. US Senate, Committee on Commerce (1966), op. cit. note 60, testimony of Thomas Malone, 136 (also testimony of Leland Haworth, 93; Robert White, 173; and Herbert Hollomon, 185 in these hearings); US Senate, Committee on Interior and Insular Affairs (1966), op. cit. note 19, testimony of W.O. Roberts, 353.

76. Homer E. Newell, *A Recommended National Program in Weather Modification*, Interdepartmental Committee on Atmospheric Science, report 10A (Washington, DC: ICAS, 1966), III–3; National Academy of Sciences, Panel on Weather and Climate Modification, *Weather and Climate Modification: Problems and Progress* (Washington, DC: NAS Press, 1973), 160. The ICAS report breaks down spending on inadvertent weather and climate modification by agency; of the \$434,000 spent in fiscal 1966, \$214,000 was spent by NSF, \$170,000 by ESSA, and \$50,000 by the Department of Defense. Overall spending on weather modification was far less than its Congressional and other supporters had hoped for. 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 and about an order of magnitude greater than was actually spent: see McDonald, op. cit. note 73, 77.

77. US Senate, Committee on Commerce (1968), op. cit. note 68, testimony of George S. Benton, 96; US House of Representatives, Committee on Appropriations, *Appropriations for State, etc.* (1974), testimony of Robert White, 743.

78. In fiscal 1971, for instance, the agency (then NOAA) asked for \$889,000 for the monitoring programme in its submission to the House Appropriations Committee, but was granted only \$300,000. ESSA and NOAA submissions in US House of Representatives, Committee on Appropriations, *Appropriations for State, Justice, Commerce, part 3 for FY71* (1970), 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), 703–05, 746; 1974 (FY75). In earlier years, CO₂ monitoring was not split out from basic research or weather modification budgets.

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. On the history of US science funding, see Smith, op. cit. note 5.

79. The popular analogy made between weather modification and nuclear technology described above is one indication of weather modification's cache. A sense of it can be gained in H.E. Landsberg, 'Climate Made to Order', *Bulletin of the Atomic Scientists*, Vol. 17 (November 1961), 370–74.

80. L. Marx, *The Machine in the Garden* (New York: Oxford, 1964) is a cultural and literary history of dissident views.

81. For instance, the 1890s saw the formation of the Sierra Club, and the 1930s, the Wilderness Society and the National Wildlife Federation. For an introduction, see J.Q. Wilson, *American Government* (Lexington, MA: D.C. Heath, 4th edn, 1989), 210–22.

82. A general history is S.P. Hays, *Beauty, Health, and Permanence: Environmental Politics in the United States, 1955–1985* (New York: Cambridge University Press, 1987). Written before the 'hot summer' of 1988, this book makes no mention of climate change as an environmental issue.

83. The retooling is evident in several areas. First, while temperature data sets have been systematically collected back to the 1850s, it was not until the late 1970s that these were scrutinized for their evidence concerning *detection* of climate change and its prediction by the models. An early effort along these lines, in the context of the warming/cooling debate, is Damon & Kunen, op. cit. note 55. Second, in the mid-1970s, the climate change issue became coupled with the new field of atmospheric chemistry (through non-CO₂ greenhouse gases) and the fear of stratospheric ozone depletion. Fear of environmental effects – on both the climate and the chemistry of the atmosphere – provided a rationale for understanding better the spatial and temporal distribution of these gases; monitoring programmes such as ALE/GAGE began in the late 1970s to collect such data: the results are reported in WMO, *Atmospheric Ozone: 1985*, 3 Vols (Geneva: World Meteorological Organization, 1986).

84. Study of Critical Environmental Problems, *Man's Impact on the Global Environment* (Cambridge, MA: MIT Press, 1970); Study of Man's Impact on Climate, *Inadvertent Climate Modification* (Cambridge, MA: MIT Press, 1971).

85. NAS (1966), op. cit. note 71, Vol. 1, 10.

86. *US News and World Report*, op. cit. note 20.

87. C.C. Wallen, *WMO Bulletin*, Vol. 31 (1982), 87; and *WMO Bulletin* 'Air Pollution Meteorology', Vol. 18 (1969), 230–32. Robert McCormick of ESSA (in 1969, later head of EPA's Meteorological Division) was instrumental in the passage of this resolution.

88. Wallen, op. cit. note 38, 86.

89. Sally Jacobsen, 'Walter Orr Roberts on Atmospheric Global Pollution and Weather Modification', *Bulletin of the Atomic Scientists*, Vol. 29 (March 1973), 42. For references to present holders of this view, see Jessica Mathews, 'Global Climate Change', *Issues in Science and Technology*, Vol. 4 (1987), 61–68.

90. Jacobsen, op. cit. note 89, 46; US Senate, Committee on Commerce (1966), op. cit. note 68, 104.

91. Wilson was Vannevar Bush's assistant during World War II, and the first general manager of the Atomic Energy Commission, among other posts. In an interview on 21 January 1991, William Kellogg, the rapporteur of the climate working group at SCEPT, recalled Wilson calling him out of the blue to invite him to participate in the meeting. See also Kellogg (1987), op. cit. note 16.

92. As with climate change more generally, concern about SST-caused climate 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 climate change (voiced as early as 1965 by the NAS panel on weather and climate modification) into their literature until early 1971. See K. Hohenemser, 'The Supersonic Transport', *Bulletin of the Atomic Scientists*, Vol. 22 (December 1966); R.J. Samuelson, 'The SST and the Government: Critics Shout into a Vacuum', *Science*, Vol. 157 (8 September 1967), 1146–51; K.M. Ruppenthal, 'Heat, Cold, Radiation, and the Boom', *Nation* (29 May 1967), 685–89; W. Shurcliff, *SST and Sonic Boom Handbook* (Cambridge, MA: Citizens League Against the Sonic Boom, 1969); 'The SST: Riding a Technological Tiger', *Time* (3 October 1969), 91–92; and M. Horwitch, *Clipped Wings: The American SST Conflict* (Cambridge, MA: MIT Press, 1982), Chapter 15, 275–81, 297–99, 309, and 318–20.

93. SCEPT, op. cit. note 84, xi and 15–18.

94. Horwitch, op. cit. note 92, 283.

95. Bayard Webster, 'Scientists Ask SST Delay Pending Study of Pollution', *The New York Times* (2 August 1970), 1.

96. Ultimately most of this money was devoted to research on ozone depletion. Robert Lindsey, 'US Aide Concedes SST Could Affect Stratosphere', *The New York Times* (5 August 1970), 70; 'Study Panel Named on Effects of SST', *The New York Times* (4 February 1971), 69; Horwitch, op. cit. note 92, 291, 313–14, and 335–37; NAS (1973), op. cit. note 76, 21–23. Perhaps, as alleged by critics, the CIAP's work was not very good; it nevertheless moved the field forward if by no other means than by providing material for critical scientists to discuss.

97. David A. Kay and Eugene B. Skolnikoff, 'International Institutions and the Environmental Crisis: A Look Ahead', in Skolnikoff and Kay (eds), *Global Eco-Crisis* (Madison, WI: University of Wisconsin Press, 1972), 310.

98. Wilson to Strong, 30 October 1972, Wilson papers, MIT Archives, Box 44, File 1818. Obviously, this statement was made after the Stockholm Conference, but it reflects the way that Wilson viewed his entrepreneurial activity. One result of the Wilson–Strong correspondence was the establishment of the International Federation of Institutes of Advanced Studies (IFIAS), which subsequently commissioned studies on the impacts of climate change.

99. Recommendation 79 of UNCHE. See UN document A/CONF.48/8 (1973).

100. The CO₂ monitoring items in NOAA budgets can be found in note 78 above.

101. NAS (1973), op. cit. note 76, xii. The panel selected 1980 as the target date for 'when the knowledge base for understanding inadvertent climate change should be in order'.

102. This speech was reprinted in *The New York Times* (16 April 1974), 12. It was cited as the basis for the reformulation of the WMO research programme at the June 1974 meeting of the WMO Executive Committee, *Abridged Report of the WMO Executive Committee, 1974* (Geneva: WMO, 1974), 58.

103. 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 21 May 1976, 2, and it is discussed by Deborah Shapley in her review of Stephen Schneider's *The Genesis Strategy*, *The New York Times Book Review* (18 July 1976), 3–4.

104. R. White, 'Climate at the Millenium', in WMO No. 537, *Proceedings of the World Climate Conference* (Geneva: World Meteorological Organization, 1979), 4.

105. It is important, however, not to overestimate the extent of penetration of concern about the greenhouse effect. It was not yet on the agenda of major environmental groups, and, of course, it did not become a household phrase until after 1988.

106. For the cooling hypothesis, see S.I. Rasool and S.H. Schneider, 'Atmospheric Carbon Dioxide and Aerosols: Effects of Large Increases on Global Climate', *Science*, Vol. 173 (9 July 1971), 138–41.

107. By the mid-1970s, observational data had largely resolved that cooling/warming debate, in favour of the latter. For an assessment of the evidence for the cooling and warming hypotheses, see Damon & Kunen, op. cit. note 55. The cooling arguments suffered for at least two reasons. First, particulate emissions did not reach the levels originally forecast – that is, the energy system became cleaner through less extensive use of coal and greater use of scrubbers, for other environmental reasons, to remove particulates that cause cooling. Second, with the discovery in the mid-1970s of many greenhouse gases in addition to CO₂, it became clear that the increased forcing from greenhouse gases would outweigh the offsetting (cooling) effect of particulates.

108. M.J. Molina and F.S. Rowland, 'Stratospheric Sink for Chloro-fluoromethanes: Chlorine Atom-catalyzed Destruction of Ozone', *Nature*, Vol. 249 (28 June 1974), 810–12.

109. Ramanathan, op. cit. note 51.

110. Wang et al., op. cit. note 51.

111. SCEP, op. cit. note 84.

David M. Hart is a PhD candidate in the Department of Political Science at MIT.

David G. Victor is the Director of the 'Implementation and Effectiveness of International Environmental Accords' Project at the International Institute for Applied Systems Analysis, Laxenburg, Austria.

Authors' addresses: (DMH) Political Science Department, Building E53–470, Massachusetts Institute of Technology, Cambridge, Massachusetts 02139, USA; (DGV) Implementation and Effectiveness of International Environmental Accords Project, International Institute for Applied Systems Analysis, A–2361 Laxenburg, Austria.