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Money for Keeling: Monitoring CO_2 levels[1]

FUNDING IS OBVIOUSLY a necessity for scientific research, but the details of the funding of a given program have rarely been studied in detail. This essay looks into one case of special interest: the support for monitoring the level of carbon dioxide gas (CO_2) in the atmosphere. At first this program seemed of only slight interest, even within the scientific community, but by the end of the 20th century it had produced arguably the most important sequence of data in any field of geophysics, if not all science—the iconic graph known as the "Keeling curve." This constitutes the best evidence, and most impressive warning, that we risk serious global warming through human emission of greenhouse gases. (Figure 1)

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The following abbreviations are used: AIP, American Institute of Physics, College Park, MD; SIO, Scripps Institution of Oceanography archives, La Jolla, CA.

1. This text is adapted from an essay on the Web site: Spencer Weart, "The discovery of global warming," http://www.aip.org/history/climate/Kfunds.htm (2002–2005). The chief adaptation was to remove unnecessary overlap with Weart, "Global warming, cold war, and the evolution of research plans," *HSPS, 27:2* (1997), 319–356 (q.v. for additional material), and to add explanatory material from other essays on the climate Web site. My interest in funding goes back to my work with Paul Forman and J.L. Heilbron, "Physics circa 1900: personnel, funding, and productivity of the academic establishments," which constituted the entirety of *HSPS, 5* (1975).

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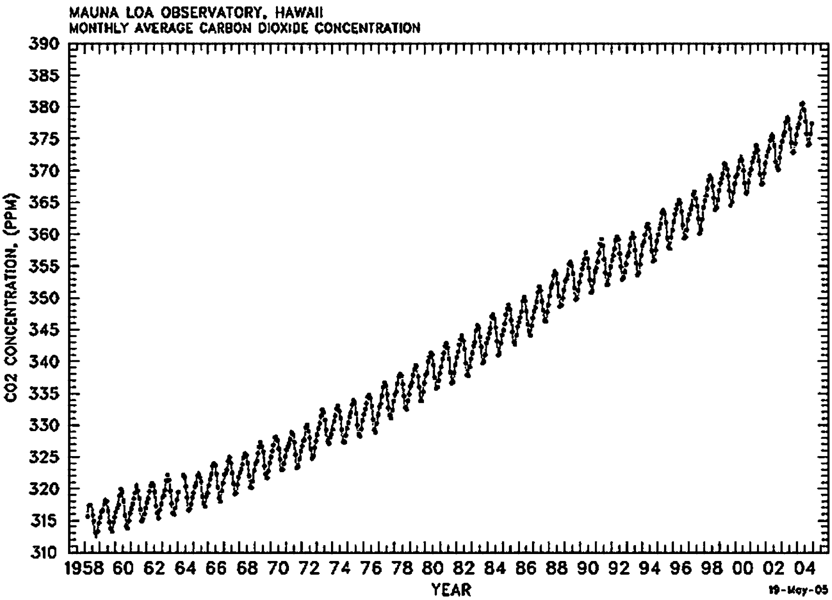

FIG 1 The "Keeling curve" of CO_2 measured at Mauna Loa, Hawaii over nearly half a century. *Source:* Scripps Institution of Oceanography. Reproduced by permission.

1. PERSONAL MECHANISMS

At the outset, nobody thought measuring carbon dioxide levels was pressing or even especially interesting. Nineteenth- and early 20th-century studies were done simply to satisfy general scientific curiosity, to fill in a table of atmospheric parameters with a single rough value measured at some convenient place. The funding came from the usual sources for academic research. An individual would work on CO_2 for a few months, supported on his salary as a professor, with perhaps a little help from a grant awarded mainly for other matters. No wonder, then, that in the 1950s researchers lacked what a few were beginning to call for—reliable measurements of how much CO_2 was in the atmosphere, and how humans might change the amount by burning fossil fuels.

They had in mind the so-called "greenhouse effect" theory, promoted around the start of the 20th century by Svante Arrhenius, who held that a changing level of the gas would alter global temperature. Convincing counter-arguments drove the theory into disrepute within a decade or two. Nevertheless, textbooks continued to mention it occasionally if only to dismiss it. Beginning in 1938 the engineer and amateur climatologist Guy Stewart Callendar worked hard to revive interest in the theory. A few scientists found that Callendar's insistent arguments could not be dismissed easily. Might the CO_2 level in the atmosphere actually change? If so, could that do anything to alter global temperatures? Perhaps a fall of CO_2 levels might provide the long-sought explanation of the ice ages. Also, what seemed less interesting to most scientists, a rise in the level might lead in some distant future to global warming.[2]

The topic came up at a conference in Stockholm in 1954. The participants were gathered to discuss how air masses carried around gases needed by crops, such as nitrogen and CO_2—for example, how factory emissions might fertilize growth downwind. The participants agreed that there ought to be a network of stations to provide regular data on such gases, and especially CO_2.[3] In response, universities set up a network of 15 measuring stations throughout Scandinavia. Their measurements of CO_2 fluctuated widely from place to place, and even from day to day, as different air masses passed through. "It seems almost hopeless," one expert confessed, "to arrive at reliable estimates of the atmospheric carbon-dioxide reservoir and its secular changes by such measurements."[4]

The gas became more closely linked with possible climate change the following year, when Hans Suess's carbon-14 studies found that a rising fraction of the CO_2 in the atmosphere derived from the burning of fossil fuels.[5] Roger Revelle, the leader of the Scripps Institution of Oceanography in California and a scientist who found every topic fascinating, took up the CO_2 question in collaboration with Suess. Revelle's funding came from the Office of Naval Research and a variety of other sources concerned in general with the oceans. In 1956 he demonstrated, to the surprise of other scientists, that the oceans would not absorb any excess gas as rapidly as humanity could emit it.[6] Nobody had properly understood the complex buffering mechanism in seawater, which, as Revelle now explained, retards the absorption of an acidifying gas like CO_2. No less important, global population and industrialization were both climbing up steep exponential curves. Thus the amount

2. For background on this and more details on the work through 1960 see Weart (ref 1).
3. Erik Eriksson, "Report on an informal conference in atmospheric chemistry held at the Meteorological Institute, University of Stockholm, May 24–26, 1954," *Tellus, 6* (1954), 302–307.
4. Stig Fonselius et al., "Microdetermination of CO_2 in the air, with current data for Scandinavia," *Tellus, 7* (1955), 258–265; Fonselius et al., "Carbon dioxide variations in the atmosphere," *Tellus, 8* (1956), 176–183. "Hopeless" expressed by C.-G. Rossby, in "Current problems in meteorology," in Bert Bolin, ed., *The atmosphere and the sea in motion* (New York, 1959), 9–50, on 15.
5. Hans E. Suess, "Radiocarbon concentration in modern wood," *Science, 122* (1955), 415–417.
6. Roger Revelle and Hans E. Suess, "Carbon dioxide exchange between atmosphere and ocean and the question of an increase of atmospheric CO_2 during the past decades," *Tellus, 9* (1956), 18–27.

of gas that industry was likely to emit during the 21st century and beyond would overwhelm the capacity of the oceans to absorb it. This refuted one of the principal arguments that people had used against Arrhenius's theory of the CO_2 greenhouse effect; scientists no longer could argue that the oceans were a boundless sink that could automatically dispose of our emissions.

Revelle and Suess wanted good observations to test the theory, enough CO_2 measurements all around the world averaged into a global "snapshot" of the current level. Decades later, someone could take another snapshot and see if the level had risen. That would be an important scientific contribution: it would check Revelle's theory of ocean uptake, and thus might help solve the vexing old puzzle of the cause of ice ages. Perhaps it might also say something about global warming in future centuries.

By fortunate chance, Revelle and Suess were able to command enough money to launch such a project. The International Geophysical Year (IGY, 1957–1958) was just being planned to give the world's earth scientists a billion dollars of extra funding precisely for the purpose of making a snapshot of the global environment. As some historians have explained, the IGY had many additional purposes, mostly relating to the cold war, in areas ranging from national prestige to espionage.[7] At the urging of Revelle and Suess, the U.S. National Committee for the IGY granted Scripps some small funding for CO_2 monitoring. By a further fortunate chance, Revelle had near at hand the right man to run the program.

Charles David (Dave) Keeling (1928–2005) was a postdoctoral student at the California Institute of Technology interested in the geochemistry of the atmosphere. Underlying his interest was a personal drive. A dedicated outdoorsman, who spent all the time he could spare traveling woodland rivers and glaciated mountains, Keeling chose research topics that would keep him in direct contact with wild nature. For him monitoring CO_2 in the open air was a perfect research project. Keeling's case was not an unusual example of crucial "support" provided for geophysics from simple love of nature.

Like some of the Scandinavians, Keeling began studying CO_2 in the atmosphere with a look at the gas's biological interactions, vaguely invoking possible applications to agriculture. Appealing to another problem of deep concern in California, he also drew support from a grant from the Los Angeles Air Pollution Foundation. But Keeling's true scientific interest was the pure study of geochemistry, the factors that control the composition of the atmosphere. That basic question pushed him toward measurement at a level of accuracy that required him to devise his own instruments, better than any then on the market. As Keeling measured carbon isotopes in the air at various locations around California, laboriously refining his techniques, he found that at some locations he was getting stable numbers—the true base level of atmospheric CO_2. The Scandinavians who were monitoring the

7. For general background see Weart, "International cooperation" (June 2005), http://www.aip.org/history/climate/Internat.htm.

gas had never hoped to see any such thing, and failed to hunt out all the errors in their techniques, as Keeling managed to do.[8]

Keeling recognized the significance of the base level, for he was aware of the greenhouse theory of climate change. His mentor at Caltech, the geochemist Harrison Brown, was one of the few people of the time who appreciated how the explosive exponential growth of population and industry over the coming century would consume resources on a geophysical scale.[9] Keeling also read the theoretical studies of atmospheric transmission of infrared radiation that Gilbert Plass published in 1956—studies that refuted the other main argument that had been raised against Arrhenius's theory of the CO_2 greenhouse effect. Scientists had thought that adding still more gas could not make any difference to the absorption of infrared radiation in regions of the spectrum already "saturated." But Plass demonstrated that in the cold and rarified upper atmosphere, adding or removing CO_2 would indeed make a difference.[10] Even so, the level of the gas seemed to be a matter of no immediate significance, and not likely to yield information without great effort. This was not research any practical-minded agency was likely to support. Only the surge of IGY funding allowed Keeling to pursue his ambitious plans.

Revelle hired the young geochemist to come to Scripps and conduct the world survey. One of their aims, as Keeling recalled it, would be to "establish a reliable 'baseline' CO_2 level which could be checked 10 or 20 years later." To detect a rise of the CO_2 level during the 18-month term of the IGY scarcely seemed possible.[11] Obedient to his sources of funding, Keeling scrupulously measured CO_2 variations in the sea and air at various locations. But his heart went into the atmospheric "baseline" value. To study his measurements in detail he needed recording (strip-chart) infrared spectrophotometers. Others argued that the costly instrument's accuracy was far greater than anyone needed to measure the fluctuating level of the gas. Not a man to let go of an idea easily, Keeling went personally to funding officials and argued vigorously until they gave him permission to purchase what he wanted.

Keeling set up his recording spectrophotometers at remote sites like Little America in Antarctica and the summit of Mauna Loa in Hawaii. This was made

8. On the inadequate instrumental technique, see Eric From and Charles D. Keeling, "Reassessment of late 19th-century atmospheric carbon dioxide variations," *Tellus, 38B* (1986), 87–105, on 88. Here and below see also Jonathan Weiner, *The next one hundred years: Shaping the fate of our living Earth* (New York, 1990), 15–25; Gale E. Christianson, *Greenhouse: The 200-year story of global warming* (New York, 1999), 152–154, and Keeling, interview by Weart, Jan 1995, Niels Bohr Library, AIP.

9. Harrison Brown, *The challenge of man's future* (New York, 1954).

10. G.N. Plass, "Effect of carbon dioxide variations on climate," *American journal of physics, 24* (1956), 376–387. For details see Weart, "Basic radiation calculations" (Jan 2005), http://www.aip.org/history/climate/Radmath.htm.

11. Keeling, "The influence of Mauna Loa Observatory on the development of atmospheric CO_2 research," in John Miller, ed., *Mauna Loa Observatory. A 20th anniversary report (National Oceanic and Atmospheric Administration special report, September 1978*) (Boulder, CO, 1978), 36–54, quote on 40 (online at http://www.mlo.noaa.gov/HISTORY/Fhistory.htm).

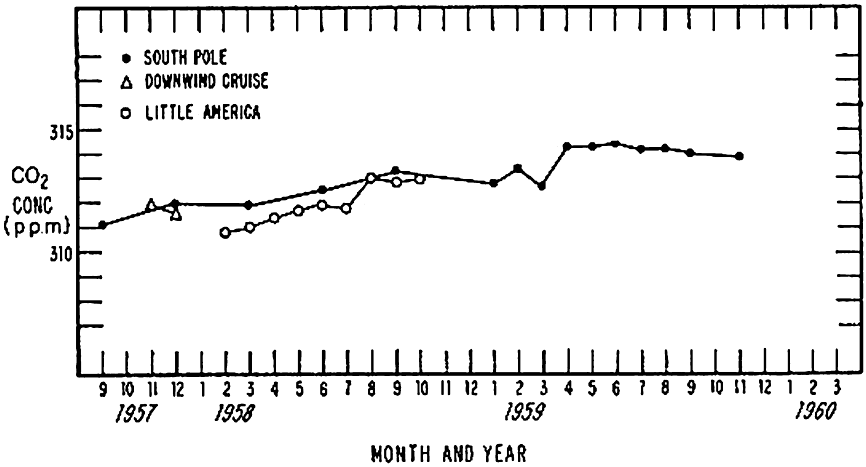

FIG 2 A rising level of CO_2 in the atmosphere, first demonstrated in 1960 in Antarctica, was visible after only two years of measurements. *Source:* C.D. Keeling (ref. 12), 200. Reproduced by permission.

possible thanks to the cold ear, for the U.S. Navy provided the base in Little America as part of its exploration of operations in all environments, while the road up Mauna Loa was built for military installations to monitor satellites. Keeling's instruments proved to be worth their cost. They allowed him to stalk down the local sources of variation and extract a remarkably accurate and consistent baseline number for the level of CO_2 in the atmosphere. In late 1958, his first full year of Antarctic data hinted that he had actually detected a rise.

But the IGY was now winding down. Recognizing the importance of Keeling's program, Suess asked Revelle to transfer some ten thousand dollars of IGY money, earmarked for Suess's studies of carbon-14, to support the work; Revelle agreed. Meanwhile Keeling applied to the U.S. National Science Foundation (NSF). The NSF had been created in 1950 without much funding, but now its budget was rapidly rising, not least because Americans were concerned about the recent launching of the *Sputnik* satellite (nominally as part of the IGY) and other signs of Soviet scientific prowess. Keeling eventually got NSF funds to carry on for the time being. In 1960, with two full years of Antarctic data in hand, he published an epochal finding. The baseline CO_2 level in the atmosphere had risen detectably—and at approximately the rate of human industry's emissions minus the amount that Revelle calculated the oceans would absorb.[12]

Alongside the NSF, the U.S. Weather Bureau helped support Keeling's work, especially at the Bureau's station on Mauna Loa. As the Mauna Loa data accumulated,

Similarly, Keeling interview (ref. 8). For all this, see also Keeling, "Rewards and penalties of monitoring the Earth," *Annual review of energy and the environment, 23* (1998), 25–82.

12. Keeling, "The concentration and isotopic abundances of carbon dioxide in the atmosphere," *Tellus, 12* (1960), 200–203.

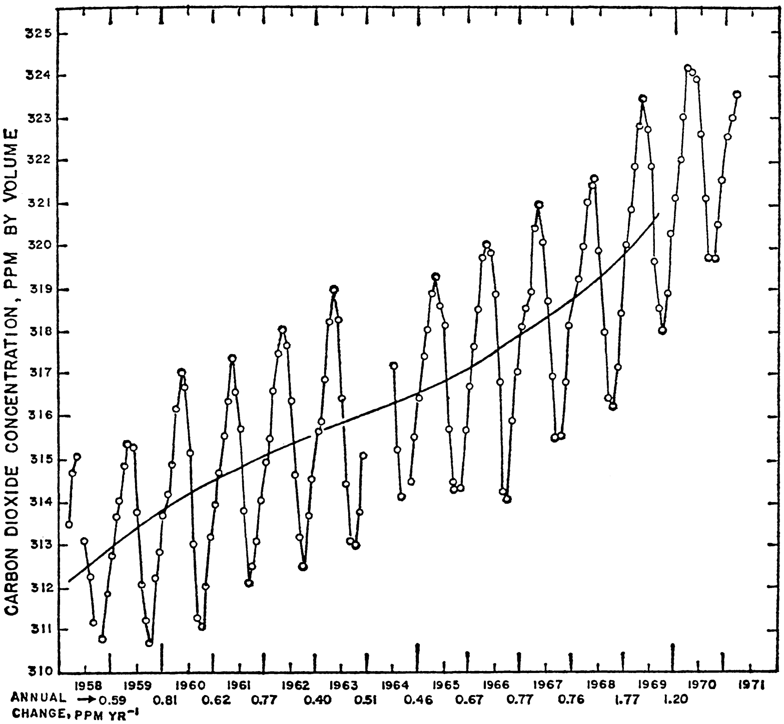

FIG 3 The "Keeling curve" of atmospheric CO_2 level measured at Mauna Loa, Hawaii, as seen in 1971, showing the general rise, an annual cycle of uptake and release of the gas from plants and soil in the northern hemisphere, and the funding hiatus of 1963–64. *Source:* Carroll L.Wilson and William H. Matthews, eds., *Inadvertent climate modification. Report of conference, Study of Man's Impact on Climate (SMIC)* (Stockholm, 1971), 234. Printed with permission by MIT Press.

the record grew increasingly impressive. The CO_2 levels had an interesting annual variation, but they kept climbing noticeably higher, year after year. However, just as the data were beginning to look solid, the U.S. Congress went into one of its periodic spells of frugality and pruned back the federal budget, science agencies included. The Weather Bureau had to stop funding research not directly related to forecasting. Keeling was forced to abandon the expensive and time-consuming Antarctic monitoring. The Mauna Loa Observatory meanwhile suffered deep funding cuts—at one point the staff were told the station would probably be closed. That would mean terminating the CO_2 monitoring program, which needed roughly $100,000 a year to function. During the spring of 1964, the observations halted altogether. Only by strenuous efforts did Keeling and his allies manage to scrape up enough money from the NSF to resume gathering data. The hiatus is visible for all time to come in Keeling's graph of the history of the Earth's CO_2.[13]

13. Keeling interview (ref. 8); Keeling, "influence" (ref. 11); Keeling, "Rewards" (ref. 11), 46.

Keeling afterwards reflected on what might have happened if his plans had been waylaid before he had nailed down his results. The Scandinavian scientists did eventually work out the flaws in their techniques, but they were inevitably plagued by random variations at their sites, swept by winds from farmland and ocean. Keeling rightly pointed out, "Many years might have passed before data of the quality of the Mauna Loa record would have been forthcoming."[14]

What had begun as a temporary job for Keeling was turning into a lifetime career. To sustain the Mauna Loa observations over the next two decades, he and his supporters had to keep up what one administrator called "a nontrivial fight."[15] Atmospheric studies were divided among many Federal agencies, which made for bureaucratic problems as officials argued over which of them, if any, should take charge of which aspects of CO_2 research. Scientists mostly had to seek support from either the NSF or the Weather Bureau, later incorporated into a National Oceanic and Atmospheric Administration (NOAA).

Keeling's rising curve increasingly impressed scientists, and crude computer studies began to show that significant global warming might come as early as the next century. Although that still seemed far away, the government gradually reacted. In 1965, when the President's Science Advisory Committee formed a panel to address environmental issues, it included a subpanel of leading climate experts. They reported that greenhouse warming was a matter of real concern. There could be "marked changes in climate," they reported, "not controllable through local or even national efforts." That put the issue on the official agenda at the highest level of government—although only as one item in a long list of environmental concerns, many of which seemed more pressing. Still, the panel gave high-level endorsement for monitoring CO_2 levels "for at least the next several decades."[16] The endorsement only ensured a bare minimum of funds for the next few years, however. By 1970, severe cuts again threatened the program.

Keeling's weak position was further weakened by personality issues. Some officials would have preferred to pursue CO_2 monitoring without him. "Keeling's a peculiar guy," Revelle later remarked. "He wants to measure CO_2 in his belly. . . . And he wants to measure it with the greatest precision and the greatest accuracy he possibly can."[17] As Keeling stubbornly sought more money to improve his results and complained angrily about shortfalls, he struck some people as a demanding "know-it-all." But when it came to monitoring CO_2, Keeling really did know it all better than anyone else. There was nobody so reliably skilled nor so dedicated. Although a few other scientists in the

14. Keeling, "influence" (ref. 11), 52.
15. Berrien Moore III, personal communication, Sep 1989. Here and below also Keeling, interviews by Weart, confidential communications from others, and papers in various files I saw at NOAA Air Resources Laboratory, Silver Spring, MD.
16. President's Science Advisory Committee, *Restoring the quality of our environment. Report of the Environmental Pollution Panel* (Washington, D.C., 1965), 26. The panel recommended funding through "the U.S. Weather Bureau and its collaborators," 127.
17. Roger Revelle, interview by Earl Droessler, Feb 1989, AIP.

United States and around the world also undertook studies of CO_2 levels, in the end Keeling and his staff at Scripps kept control of the core of the monitoring work.

Besides putting up with Keeling's exacting demands, agencies had to deal with a kind of science they found unattractive. "Monitoring" a gas in the atmosphere seemed just dull plodding around a beaten track, calling to mind the discredited statistical climatology of an earlier generation. The NSF was supposed to fund pathbreaking science, and officials looked for striking new results, new ideas that could be published in leading scientific journals—not just that steady, relentless upward march of data points, year after year after year. A reviewer who grudgingly supported one of Keeling's proposals in 1979 added the remark, "CO_2 monitoring is like motherhood. . . . It does appear, however, that the former is even more expensive." Keeling did produce interesting findings by studying variations in the rate of rise of the gas, from its seasonal cycle to decade-scale accelerations and slowdowns. But renewing his support was always a struggle. Officials at NSF told him bluntly that they would not support "routine monitoring" indefinitely.[18]

The straightforward solution would have been to fund CO_2 monitoring as just one piece of a grand program to monitor all aspects of the global environment. Here the story of support for greenhouse gas monitoring becomes part of the general tale of government funding for environmental matters. Research on problems like climate change was not the particular responsibility of any government official. As the 1965 panel remarked, "no agency or program is concerned with the average condition of our environment."[19] An Academy panel added in 1966 that for climate as for most environmental fields, support was "diffused among many agencies." Thus "there exists no single natural advocate in the Federal structure, nor is there a clear mechanism for making budgetary decisions." In the mid-1960s, a variety of government agencies together spent roughly $50 million a year for all aspects of meteorological research. That was not much, and climate change studies caught only a few percent of that.[20] Research on the topic had to fit in as minor components of programs that had been set up to work on more immediate problems.

The best hope of climate scientists lay in the large sums of money the government was devoting to short-term "climate modification," chiefly rain-making through "seeding" clouds—a matter of deep concern among both farmers and military officers. Perhaps a bit of that could be diverted toward research on, say, "inadvertent climate modification." The phrase was often used during this period by people concerned about greenhouse warming.[21] But defining the greenhouse effect as "inadvertent climate

18. Anonymous review, Oct 1979, folder "Grant - NSF ATM79-25965 Proposal," Keeling office files, SIO; Keeling, "Rewards" (ref. 11), 51–52, 56–57.
19. President's Science Advisory Committee (ref. 16), 26.
20. National Academy of Sciences, Committee on Atmospheric Sciences Panel on Weather and Climate Modification, *Weather and climate modification: problems and prospects* (2 vols., Washington, D.C., 1966), *1*, quote on 20, budget on 16.
21. David Hart, "Strategies of research policy advocacy: anthropogenic climate change research, 1957–1974" (Cambridge, Harvard University report 92–08, 1992).

modification" made it sound like one of the countless byproducts of economic progress, a sort of smog that could be handled easily by improving technology. Leading experts suggested that if global warming ever became annoying, there were technical schemes, not excessively costly, that could counteract it. In short, climate change was of far less interest to the government, and to the public, than chemical pollution, dying lakes, and countless other environmental problems.

2. BUREAUCRATIC MECHANISMS

In the early 1970s, a few climate scientists sought an opportunity to mount a concerted push for public support for the monitoring program. A month-long workshop at the Massachusetts Institute of Technology in 1970, and an international conference in Stockholm in 1971, put global climate change on the table as a significant policy concern. The scientists were spurred to the task by new data and calculations, which convinced them that the world's climate might change far sooner and more drastically than had seemed possible only a decade earlier. There was now evidence from ancient ice sheets and the seabed of rapid climate changes in the past. A fresh look at mechanisms driving the climate system found such changes only too plausible. There was even a possibility, some exclaimed, that the next ice age might get started within their own lifetimes. Others thought it more likely that there would be global warming, or no long-term climate change at all. With the issue so unsettled, panel reports and individual scientists could plausibly insist that more research was urgently needed.

That was the traditional "insider" approach to policy. Other scientists thought government action would follow only if they could reshape public attitudes. A reshaping had in fact begun in the mass environmental movement that burst into full flower in the early 1970s ("Earth Day" 1970 is widely seen as the takeoff point). Some climate scientists adopted the mood and rhetoric of the movement, describing climate change in dramatic terms as a threat to the well-being of the living planet.

Newspapers and television paid attention, for it happened that the early 1970s saw a spate of unusual climate disasters around the world, including devastating droughts in the American Midwest and the African Sahel. Starvation threatened millions. Some experts worried that with climate so uncertain, the world's population might soon outrun its food supply. Further reports by panels of scientists demanded more environmental research, including climate studies, as well as more monitoring of the effects of human actions on the environment. Bureaucrats got the message and put carbon dioxide into a new category, "Global Monitoring of Climatic Change." Under this title the total funding, stagnant for many years, doubled and doubled again between 1971 and 1975.[22]

22. Hart (ref. 21), 32–33, citing U.S. House of Representatives, Committee on Appropriations, "Appropriations for State, Justice, Commerce" for FY 1967–1975.

The Mauna Loa Observatory was rescued from the repeated threat of budget cuts when a unit of NOAA took over its operation and maintenance in 1973. This unit was the Air Resources Laboratories within NOAA's Environmental Research Laboratories—names that reflected the new national concern about the environment. Formally, the money came out of a budget for "inadvertent climate modification" and fell within the agency's general responsibility for monitoring "air quality." The administrative structure for the monitoring descended from an organization that the Weather Bureau had created in 1948 to help the Atomic Energy Commission track the dispersion of radioactive fallout from atomic bomb tests. This responsibility later expanded to include tracking of urban smog and other materials artificially added to the atmosphere. CO_2 was stuck into this budget for lack of any better place to put it.[23] Meanwhile, NSF continued to award Keeling grants for specific research projects on the odd variations of his curve from year to year. Eventually the curve was found to reflect many things, from El Niño events to the economic collapse of the Soviet Union.

Further support came from officials and scientists who began to call for international funding and organization of environmental monitoring. In his battle for U.S. government funds, Keeling rounded up verbal endorsement from the prestigious World Meteorological Organization, and he got some money directly from the new United Nations Environmental Programme.[24] But hopes that other nations would join the effort with their own monitoring work were disappointed, and NOAA's program remained the only truly global one. Keeling, notoriously meticulous, was trusted to set standards for the tricky measurements of air samples brought to him in glass flasks from spots around the world. These measurements confirmed the Mauna Loa results for the inexorable annual rise of CO_2, and added important information about the seasonal carbon cycle.

In a summary published in 1976 that unequivocally demonstrated the long-term rise of atmospheric CO_2, Keeling's group ended, as was customary in scientific papers, with an acknowledgment of support. They cited a series of six NSF grants and added that NOAA and its predecessors had provided "station facilities, field transport, and staff assistance." The scientists might also have noted, had they not taken it for granted, that their institutional homes provided an essential long-term foundation of salaries and facilities. Nearly all senior scientists get their pay, their office, and (perhaps hardest to secure) their parking space in their capacity as a university professor, or sometimes as a staff member of a public or private research institute. Keeling's group in the Scripps Institution of Oceanography at the University of California received its basic money not only from Federal government grants but also from the State of California and through private institutional fund-raising and endowments.[25]

23. L. Machta, "A history of the Air Resources Laboratory," Draft, Feb 1990.
24. Keeling, "Rewards" (ref. 11), 51–52.
25. Charles D. Keeling, R.B. Bacastow, Q.E. Bainbridge, et al., "Atmospheric carbon dioxide variations at Mauna Loa Observatory," *Tellus, 28* (1976), 538–551, on 551. In such papers institutions are acknowledged at the outset by listing authors' affiliations.

Improved computer models were plausibly indicating that greenhouse gas emissions would cause a global warming within the foreseeable future. Meanwhile, with public concern aroused by the recent catastrophic droughts as well as by city smog and other environmental problems, the entire human interaction with the atmosphere was looking increasingly problematic. Many scientists were now urging the government to give climate research a sound organizational structure. As just one example, the National Academy of Sciences established a Committee on Climatic Variation, which in 1974 presented recommendations for a national climate research plan. The President's Domestic Council worked to pull this and many other suggestions together and drafted a National Climate Program Act centered on an increase of funding for research and monitoring. In May 1976 a Congressional committee began hearings, the first ever to address climate change as their main subject.[26]

The talk dragged on with little public attention and little result. Without the backing of some unified community or organization, the movement for reorganization was impeded by the very fragmentation it sought to remedy. Agencies put forth various proposals, but each of them threatened to usurp the activities of existing research bureaucracies. And the effort came during cramped economic times, as Congress sought ways to cut the budget. But the worst weakness was what one participant called "a failure to demonstrate to funders of such research the practical benefits that can result within a time frame of relevance to their mandate."[27] Lawmakers cared far more about the few years until the next election than about the 21st century. In the end, the sole useful consequence was that the Nixon administration set up an interagency group to coordinate climate research. Only in a limited sense could that be called an official U.S. climate program, and for practical purposes it counted for little.

The NOAA monitoring program's budget leveled off after 1975 as part of a general saturation of environmental concern. But now a new actor came on stage. In the mid 1970s David Slade, a vigorous and strong-minded manager in the Energy Research and Development Administration (ERDA), took an interest in climate change. More than almost any other manager, he saw the greenhouse effect as a matter of serious concern to the energy industries. It fitted into the agency's mandate to take a hard look at energy and the environment in general. Meanwhile some scientists were warning officials that climate change was a serious issue. The widely-respected geochemist Wallace Broecker, for one, wrote to ERDA's chief in 1976 to insist that the agency "make every possible attempt to explore the effects of CO_2."[28]

26. Domestic Council, Environmental Resources Committee, Subcommittee on Climate Change, *A United States climate program* (Washington, D.C. 1974). Further details at Weart, "Government: The view from Washington, D.C." (Feb 2005), http://www.aip.org/history/c1imate/Govt.htm.
27. J.A. Laurmann, "Climate research," *Science, 191* (1976), 1002–1005.
28. Broecker to Robert Seamans, 17 Mar 1976, copy in Revelle Papers, MC619:32, SIO.

With such backing, Slade sought the authority and budget to support research programs including better CO_2 monitoring. His first move was the creation in 1977 of a prestigious Study Group on the Global Effects of Carbon Dioxide, chaired by the veteran nuclear energy administrator Alvin Weinberg. As an advocate of nuclear power—the leading alternative to energy production from fossil fuels—Weinberg became a strong supporter of greenhouse effect studies. Next, Slade set up an Office of Carbon Dioxide Research, a vessel into which he hoped to pour important sums of money.[29] He mobilized scientists around the country to help draft an elaborate research program. The plans called for increasing the annual budget for greenhouse effect research more than tenfold, to $20 or $30 million, including a global CO_2 monitoring network costing around $200,000 per year.[30]

As these discussions proceeded, ERDA was integrated into the newly created Department of Energy (DOE). That only increased the uncertainty among government officials about who should pay for Keeling's program and other CO_2 monitoring. The DOE, NSF, NOAA, and even the U.S. Geological Survey were all supporting aspects of the work. But only Slade at DOE seemed interested in taking responsibility as the "lead" Federal agency for CO_2 monitoring with an aggressive program. From 1977 to 1980 his small budget doubled, redoubled, and redoubled again, driving an expansion of many kinds of research related to CO_2 and global warming. However, not all of this represented a net increase of total Federal funding for climate research. Much of the money was only shifted about administratively. Keeling's work continued to live on money provided precariously by several grudging agencies.

Slade's operation was the closest the nation had ever come to a centrally organized program to study climate change, but DOE was a poor place for such work. Those who dealt with any branch of the new department (including the author of this essay) were often frustrated by its bureaucratic quagmire of paperwork and regulations. Pushing a research program like Slade's meant an interminable succession of meetings with scientists, other Federal science agencies, the Office of Management and Budget, a variety of advisory and oversight committees, and foreign groups, and endless writing, revising, and re-writing of plans and reports. As one of Slade's staff members complained, "Nobody gets to do any work except bureaucrats and secretaries." Sometimes it seemed that more time was spent on convening workshops to discuss research than on the research itself. "If anything has been meetinged to death it's CO_2," the staffer remarked. "If conferences could solve problems, it should be solved by now."[31] Scientists began to worry.

29. D.H. Slade, "Plan for the DOE carbon dioxide effects program," 14 Oct 1977, William Mitchell Papers, NOAA Air Resources Laboratory, Silver Spring, MD. See "DOE sets interagency CO_2 research priorities," *Science news* (3 Dec 1977), 375.

30. Various plans in Revelle Papers, MC6 12:15, SIO.

31. William P. Elliott, Diaries (1977–1989), Elliott papers on carbon dioxide and climate change, AIP. Quotes from 13 Nov 1978 and 21 Aug 1980.

Particularly outspoken was Broecker, who wrote to a Senator that "the time for one complete loop (scientists to agency to scientists) is much too long. . . . This system has functioned but it has led to delays and I'm sure has left huge gaps in the research program."[32]

Reinforcing this criticism of the DOE bureaucracy was the fact that the new department had no record of work specifically on climate change, nor any special expertise in the field. And Slade's aggressive bureaucratic maneuvers made him unpopular with some administrators in other agencies. His ambitious plans ran into a wall. Congress was again fighting particularly hard to balance the Federal budget, and Slade's program had grown large enough to attract the attention of frugal administrators. In 1980, the increases in his CO_2 funding came to a halt. Meanwhile the Congressional National Climate Act of 1978 had established a National Climate Program Office within NOAA. The agency immediately expanded its program of collecting air in flasks by adding ten stations scattered around the world. Its global CO_2 monitoring program has continued to this day.[33]

The assignment of responsibility to NOAA made a problem for Keeling. The weather bureaucrats had grown weary of his demands and were getting ready to push him aside so that they could monitor CO_2 as they saw fit. Meanwhile NSF officials had decided to terminate their support of Keeling's "routine" measurements. At the start of 1981 it seemed that he had run out of sponsors. But Scripps's director made a personal appeal to Slade's boss, DOE Director of Energy Research Edward Frieman, who agreed to pick up the bill. Slade and his colleagues in DOE negotiated with NOAA, and the Mauna Loa monitoring, including exacting standard setting, continued under Keeling's control.[34]

The reprieve was short-lived. In 1981, Ronald Reagan became President, eager to suppress "alarmist" environmentalism. Reagan's Secretary of Energy (a former governor of South Carolina, trained as a dentist) told people that there was no global warming problem. The DOE's recent attempt to take over and expand greenhouse effect studies, smacking of bureaucratic empire building, made a juicy target for cuts. To the dismay of the Department's own mid-level scientist-administrators, its new leadership announced plans to curtail funding for climate research. In particular, they would entirely terminate DOE's funding of CO_2 monitoring. Undercut by criticism of his administrative methods, Slade was peremptorily removed from his post and Frieman too was forced out.

32. Broecker to Sen. Paul Tsongas, 7 Apr 1980, "CO_2 history" file, office files of Wallace Broecker, Lamont-Doherty Geophysical Observatory, Palisades, NY.
33. A list of stations with dates is at the NOAA site http://www.cmdl.noaa.gov/ccgg/flask/ccgnetwork.dat.
34. Keeling, "Rewards" (ref. 11), 56–61; Robert G. Fleagle, *Global environmental change. Interactions of science, policy, and politics in the United States* (Westport, CT, 1994), 126.

Supporters of climate research rallied, finding a leader in Representative Albert Gore, Jr. Gore had been concerned about the greenhouse effect ever since he was an undergraduate at Harvard, where he had heard Revelle deliver a talk on the subject. Supported by a few other representatives, Gore held a Congressional hearing in March 1981, featuring testimony by persuasive scientists like Revelle and Stephen Schneider. The hearings threw a public spotlight on the threat to the CO_2 program. Painfully embarrassed by media attention to greenhouse warming, the Reagan administration backed off and the DOE's program survived.[35]

The CO_2 budget was so small, and now so little appreciated, that DOE managers tried to persuade NOAA to take over Keeling's work. But under Reagan, environmental research was being starved at NOAA, as at every Federal agency. The result of months of negotiation was a compromise, in which NOAA took on administration of some activities while DOE continued to provide money. Backed up by DOE (as well as by the World Meteorological Organization), Keeling could return to NSF for funds from time to time. And he found other occasional but important sources. For example, in 1982 his research team got an unexpected grant from the Electrical Power Research Institute, a private group underwritten by the electrical power industry, whose leaders recognized that they needed a better understanding of the greenhouse effect.[36] For Scripps's day-in, day-out global CO_2 monitoring, however, DOE remained the chief support. Despite further troubles and administrative shifts, the program has continued at a modest level down to the present. (Figure 1)

All this bureaucratic effort—which drained countless hours and emotional energy from administrators, politicians, and most of all from Keeling and other scientists—concerned an annual sum of barely $200,000, an insignificant fraction of DOE's multi-billion-dollar research budget.

3. CONCLUSION

This story exemplifies several main themes of American science funding in the second half of the 20th century. The chief theme that historians have noticed in general, and which also appears in this story, is the impact of the cold war. The fact that around half of the research on anything in this period depended on military or

35. Main supporters were Reps. George Brown (CA), a long-time friend of science, and James Scheuer (NY). Stephen H Schneider, *Global warming: Are we entering the greenhouse century?* (San Francisco, 1989), 121–130; James E. Jensen, "An unholy trinity: Science, politics and the press" (unpublished talk), 1990.
36. Elliott (ref. 31), 18 June 1982; Keeling "Rewards" (ref. 11), 60–61, 66; Keeling, Stephen C. Piper and Martin Heimann, "A three-dimensional model of atmospheric CO_2 transport based on observed winds," in David H. Peterson, ed., *Aspects of climate variability in the Pacific and the western Americas* (AGU monograph 55), ed. (Washington, D.C., 1989), 165–363, on 231–232.

national prestige motives is so familiar by now to science historians that one needs to note that roughly half of the support did *not* come from such sources. Other practical matters such as agriculture and air pollution, as well as pure academic curiosity, also figured in Keeling's story and many others.

The trend most obvious and familiar to scientists who lived through the period was a shift from personal to bureaucratic funding mechanisms. In the 1950s, the geophysics community was small enough so that everyone knew everyone else's qualifications, and leaders could make decisions on a personal basis (physicists, astronomers and most other scientists worked similarly). Officers of agencies like the U.S. Committee of the IGY or the Office of Naval Research could bestow funds like Renaissance princes upon those they trusted. (Figure 4) It was such patronage mechanisms that Revelle and Seuss used to deliver IGY money to Keeling, that Keeling himself drew upon to extract additional funds to buy his spectrometer, and that left Seuss and Revelle with no qualms about diverting money from a grant intended for a different purpose.

FIG 4 The "Renaissance prince" patronage model followed by the early postwar Office of Naval Research (ONR), as seen by an oceanographer. *Source:* Drawing by Mike Dormer, reprinted with permission from National Research Council, Ocean Studies Board, *Fifty years of ocean discovery. National Science Foundation 1950–2000* (Washington, D.C., 2000). Reproduced by permission.

This personal model became less prevalent through the 1960s, and by the 1980s it was all but extinct outside a few sheltered places like the Defense Department's Advanced Research Projects Agency. The shift of government research support to the NSF and other large new agencies made for more rigorous supervision, whether through formal peer review and committee mechanisms or, as in the DOE, by rule-bound bureaucrats. But the underlying reason for the shift was probably the sheer growth of research communities. Ever larger populations meant that a few leaders like Revelle could no longer know and judge everyone. Moreover, the funding required for a research program increased even faster than the number of researchers, since each step forward in knowledge required more elaborate instrumentation and collaboration. Keeling's advanced spectrometer, and the coordination required to gather CO_2 flasks around the world, were more costly than their predecessors, yet they were tiny compared with the grand programs projected in other areas of meteorology and oceanography, to say nothing of space astronomy or high-energy physics. The large sums called up increased Congressional insistence on tight oversight and controls.

The United States was not alone in experiencing these changes, but they were more pronounced there. In most other countries the central government bureaucracy, as the employer of the majority of academics, had already before 1940 played a main role in supporting research. And in the postwar era, the funding of research nowhere grew to such gargantuan bulk as in America.

The spread of formal oversight structures was a problem for Keeling because his work did not fit easily into any of the slots. This feature was probably most common in the scientific fields (increasingly prevalent) that involved interdisciplinary work. Geophysics, inherently interdisciplinary, included research programs that fell between two chairs, subject to agency infighting over who would be privileged, or obliged, to pay for a given program.

Mingled with this infighting came another trend: the politicization of some scientific decisions. Through the 1960s, science advice flowed upward from the research community to politicians and the White House (some would say "downward"). The direction began to reverse when President Richard Nixon grew disgusted with physicists' opposition to ballistic missile defense and other Cold War initiatives. Under the early Reagan administration, top officials not only rejected unwelcome advice, but actively sought to suppress research that might lead to further unwelcome advice. The tendency was at first most obvious in environmental areas like global warming and pollution control, but in time it spread into sexual education and other areas of public health, and onward to basic research involving embryos. This politicization did not appear in other advanced democracies. It appears to correlate with the unequalled rise to power in America of right-wing anti-intellectual anti-elite attitudes. While increased bureaucratization of research support may be inevitable, interference for political ends is an error that should and can be opposed by all citizens.

SPENCER R. WEART

Money for Keeling: Monitoring CO_2 levels

ABSTRACT

C.D. Keeling's measurements of the level of carbon dioxide (CO_2) in the atmosphere since 1957, tracking a rise that threatens global warming, form one of the most important scientific data sets ever created. Yet the relatively small funding Keeling required was rarely secure. He could begin his measurements only because of a one-time injection of funds into geophysics during the International Geophysical Year. The original aim was to take a "snapshot" which could be repeated a few decades later to find whether the level of the gas had risen as predicted. Keeling and his sponsors made personal appeals to divert additional funds so he could refine and extend his measurements; in consequence, with just two years of data he showed that the level was rising. In the following decades, maintaining funding was problematic. Agencies saw the work as "routine monitoring" rather than cutting-edge research. In the 1970s, the rise of an environmental movement helped reframe climate change and CO_2 emissions as a threat. Funding expanded within a context of monitoring atmospheric pollution and government agency empire building. But in the early 1980s, political reaction against environmentalism again threatened Keeling's program. The story reflects larger trends over the past half-century towards the bureaucratization and politicization of science funding.

KEY WORDS: Charles David Keeling, carbon dioxide, climate change, global warming, atmosphere, funding, measurement.