NAOMI ORESKES, ERIK M. CONWAY, AND MATTHEW SHINDELL*

From Chicken Little to Dr. Pangloss: William Nierenberg, Global Warming, and the Social Deconstruction of Scientific Knowledge

ABSTRACT

In recent decades, historians and sociologists of science have been largely concerned with the social construction of scientific knowledge. This paper examines an important historical episode in the social *deconstruction* of scientific knowledge. In the early 1980s, a consensus emerged among climate scientists that increased atmospheric carbon dioxide from burning fossil fuels would lead to mean global warming of 2–3°C, probably by the mid-twenty-first century, and would have serious deleterious effects, including sea level rise of at least seventy centimeters. This consensus was challenged, however, by a committee of the U.S. National Academy of Sciences, chaired by physicist William A. (Bill) Nierenberg, whose 1983 report arguably launched the climate change "debate." Drawing on perspectives provided by two economists on his committee, Nierenberg reframed the question not as a matter of climate change per se, but as a matter of the human capacity to adapt to change when it came, a capacity, his report asserted, that was very great. Thus, while accepting the scientific conclusion that warming would occur, Nierenberg rejected the interpretation that it

*Naomi Oreskes, University of California, San Diego, History Department and Science Studies Program, 9500 Gilman Dr., La Jolla, CA 92093; noreskes@ucsd.edu; Erik M. Conway, California Institute of Technology, 4800 Oak Grove Ave., Pasadena, CA 91109; erik.conway@charter.net; Matthew Shindell, University of California, San Diego, History Department and Science Studies Program, 9500 Gilman Dr., La Jolla, CA 92093; mshindell@ucsd.edu. Conway's contribution to this work is drawn from independent research conducted in July 2004 and does not represent the opinions of JPL, Caltech, or their funders.

The following abbreviations are used: MIT JGC, Papers of Jule G. Charney, 1936–1981, MC184, Massachusetts Institute of Technology Archives, Cambridge, MA; NAS AMPS, Records of National Academy of Sciences, Assembly on Mathematical and Physical Sciences, National Academy of Science Archives, Washington, DC; SIO WAN, Papers of William A. Nierenberg, MC 13, Scripps Institution of Oceanography Archives, La Jolla, CA.

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would be a *problem*. In later years, he would play a major role in political challenges to the scientific conclusions themselves. Reframing was Nierenberg's first step on the road to the deconstruction of scientific knowledge of climate change.

KEY WORDS: global warming, climate, knowledge deconstruction, controversy, debate

The scholarly project of history of science has been primarily concerned with the production of scientific knowledge. Call it progress, growth, advancement, development, reception, stabilization, establishment, or what you will, and read it as socially constructed or not, the focus has generally been on the processes by which scientific knowledge gets *accepted*. The extensive historical and philosophical literature from the 1970s and 80s on controversy and closure, for example, was more focused on closure than controversy, the latter being viewed as a messy but necessary process ending in knowledge production. Social constructionists and actor-network theorists have similarly explored the ways in which strong and extensive social networks help to get preferred positions stabilized as knowledge or fact.

Recently, some scholars have come to see the inverse as equally important: the processes that prevent, impede, reject, deny, deconstruct, or even destroy scientific claims. Robert Proctor, Londa Schiebinger, Michael Smithson, David Michaels, Gerald Markowitz, Adrienne Mayor, David Healy, Myanna Lahsen, and others have stressed that social processes and commitments are implicated in the construction of ignorance as well as knowledge, a practice that is at least as interesting and worthy of study as the reciprocal.[1]

1. Robert N. Proctor, *Cancer Wars: How Politics Shapes What We Know and Don't Know about Cancer* (New York: Basic Books, 1995); Robert N. Proctor, "The Anti-tobacco Campaign of the Nazis: A Little Known Aspect of Public Health in Germany, 1933–1945," *Biomedical Journal* 313 (1996): 1450–53; Robert N. Proctor, *The Nazi War on Cancer* (Princeton: Princeton University Press, 1999); Londa Schiebinger, *Plants and Empire: Colonial Bioprospecting in the Atlantic World* (Cambridge, MA: Harvard University Press, 2004); Robert Proctor and Londa Schiebinger, ed., *Agnotology: The Cultural Production of Ignorance* (Stanford: Stanford University Press, 2008 in press); Gerald Markowitz and David Rosner, *Deceit and Denial: The Deadly Politics of Industrial Pollution* (Berkeley: University of California Press, 2002); Gerald Markowitz and David Rosner, *Emergency Preparedness and the States: The First Two Years After September 11* (New York: Milbank Memorial Fund, 2004); Michael Smithson, *Ignorance and Uncertainty: Emerging Paradigms* (New York: Springer Verlag, 1989); Michael Smithson, "Science, Ignorance, and Human Values," *Journal of Human Values* 2 (1996): 67–81; David Michaels, *Manufactured Uncertainty: Contested Science and the Protection of the Public's Health and Environment* (New York: Oxford, 2007); Adrienne Mayor, "Suppression of Indigenous Fossil Knowledge from Claverack, New York, 1705, to Agate Springs, Nebraska, 2005," in *Agnotology*, ed. Proctor and Schiebinger; and

Why have scholars been reluctant to address these destructive processes as fully as the constructive ones? One reason for historians is the specter of whiggishness—that claims about the deconstruction of knowledge implicate the author in positivist presumptions about the advancement of scientific knowledge. To focus on factors that undermine knowledge production is to risk being accused of accepting a vision of scientific progress that was long ago refuted. Admittedly, it *is* a challenge to talk about efforts to block knowledge without slipping into presumptions about the accuracy of that knowledge or into false dichotomies between "science" and "politics."

We consider the challenge worth taking up, because of the importance of the historical issues at stake and the lines of inquiry that thereby might be pried open. In recent years, particularly in the United States, challenges to scientific knowledge claims have become commonplace as a strategy in political and social disputes, disputes that open large windows into broader questions of science and culture. From corporate boardrooms to community activists, diverse constituencies have found it useful to challenge scientific knowledge claims. The recently enacted "Data Quality Act" enshrines this strategy into law, permitting affected parties to challenge the scientific evidence on which the U.S. Environmental Protection Agency has made regulatory decisions.[2] Whether the claims of experts are true of the natural world or not, the strategies used to challenge them are worthy of study both from an epistemic and social standpoint.

Resistance to scientific claims originating outside of expert communities is well documented. In the recent history of science, the obvious case is tobacco. Allan Brandt, Stanton Glantz, Laura Bero, and others have shown how the U.S. tobacco industry funded research designed not to stabilize scientific

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discussion in Naomi Oreskes, "The Humanistic and Religious Foundations of Deep Time," *Science* 314 (2006): 596–97; David Healy, *Let Them Eat Prozac: The Unhealthy Relationship Between the Pharmaceutical Industry and Depression* (New York: New York University Press, 2004); Myanna Hvid Lahsen, "Experiences of Modernity in the Greenhouse: A Cultural Analysis of a Physicist 'Trio' Supporting the Conservative Backlash Against Global Warming," *Global Environmental Change* (forthcoming); and Naomi Oreskes and Erik Conway, "Challenging Knowledge: How Climate Science Became a Victim of the Cold War," in *Agnotology*, ed. Proctor and Schiebinger.

2. Charles Herrick, "Objectivity Versus Narrative Coherence: Science, Environmental Policy, and the U.S. Data Quality Act," *Environmental Science and Policy* 7 (2004): 419–33; Chris C. Mooney, *The Republican War on Science* (New York: Basic Books, 2005), 102–20.

knowledge, but to de-stabilize and deny it, particularly by challenging the scientific evidence that linked tobacco to adverse health effects and questioning whether that evidence was sufficient to prove causation.[3] Gerald Markowitz, David Rosner, and David Michaels have documented similar efforts by the U.S. chemical industry.[4] These industry efforts attempted to create "doubt" by fostering alternative interpretations of available data.

The motivations for interested industries to challenge scientific evidence of the dangers of their products are self-evident. Less self-evident is how and why scientists, in some cases prominent ones, participated in such projects, particularly given the potential risks to hard-won reputations.[5] For the very same communities, networks, and individuals involved in knowledge construction may also, in some cases, be involved it its deconstruction. Myanna Lahsen has noted that in the recent history of climate science, many claims to expert knowledge have come from inside the scientific community, although not necessarily in ways one might expect. It is common to find scientists working on the same or closely related topics to disagree about methods, data, and interpretations. This is the very definition of scientific debate. But in the climate arena, much of the "debate" was triggered by individuals who, although they were prominent scientists, were not climate scientists and were not actually doing climate research.[6] So how did they make their claims credible?

This paper invites a larger conversation on the diverse forms of social deconstruction of scientific knowledge through an analysis of a recent episode in the history of science: the attempt by one leading scientist to challenge the emerging scientific consensus on global warming in the early 1980s. He did so not by challenging specific expert knowledge claims, but by reframing the matter of global warming as a social scientific, rather than a natural scientific, issue.

3. Allan Brandt, *The Cigarette Century: The Rise, Fall, and Deadly Persistance of the Product that Defined America* (Boulder, CO: Perseus Books Group, 2007); Stanton A. Glantz, John Slade, Lisa A. Bero, Peter Hanauer and Deborah E. Barnes, *The Cigarette Papers* (Berkeley: University of California Press, 1996).

4. Markowitz and Rosner, *Deceit and Denial* (ref. 1); Michaels, *Manufactured Uncertainty* (ref. 1).

5. For example, Frederick Seitz, a major advisor to the R. J. Reynolds Corporation, was a former President of the National Academy of Sciences. See Oreskes and Conway, "Challenging Knowledge" (ref. 1).

6. Myanna Hvid Lahsen, "Climate Rhetoric: Constructions of Climate Science in the Age of Environmentalism" (PhD dissertation, Rice University, 1998); Lahsen, "Experiences of Modernity" (ref. 1).

During the 1990s and beyond, many challenges to climate science focused on its specifics—whether temperature records are reliable enough to prove that warming has actually occurred, whether the recent warming is demonstrably different from past warm spells, whether climate models can produce accurate forecasts of future changes, and the like. But *before* those challenges emerged in the late 1970s and early 1980s, physicist William A. Nierenberg took a different tack, and in doing so arguably launched the climate change debate, transforming the issue from one of scientific concern to one of political controversy.

Nierenberg was the lead author of the first major report on climate science issued by the National Academy of Sciences that challenged the emerging consensus view on global warming. It did so not by focusing on the specifics of that view, but on the interpretation of its meaning and significance for society in general. Whereas the tobacco industry deconstructed expert scientific claims by challenging the causal links, Nierenberg's deconstruction took the form of questioning whether those causal links mattered. And it took shape not in a corporate boardroom, but in the venue most responsible in the United States for the production of certified knowledge—the National Academy of Sciences.

THE "PROBLEM" OF ANTHROPOGENIC GLOBAL WARMING

The casting of the "greenhouse effect" as a policy issue in the late 1970s was an event long in the making. In the 1930s, Guy Stewart Callendar developed the first sustained argument that burning fossil fuels was changing the chemistry of the Earth's atmosphere in ways that could have and perhaps already were having consequences for the global climate.[7] However, because the absorption spectrum of CO_2 appeared to overlap with that of water vapor, many scientists doubted that modest increments of CO_2 would have an appreciable effect. In the 1950s this argument was refuted through spectroscopic measurements by Gilbert Plass.[8] Following Plass's work, Hans Suess and Roger Revelle declared that humanity was performing a "great geophysical experiment," by returning

7. James Rodger Fleming, *The Callendar Effect* (Boston: American Meteorological Society, 2007), 65–87, esp. 78.

8. Spencer Weart, *The Discovery of Global Warming* (Boston: Harvard University Press, 2004).

to the atmosphere in less than a century fossil fuels stored over hundreds of millions of years of geological time.[9]

In their now-famous *Tellus* article, published in 1957, Suess and Revelle argued primarily for the importance of monitoring—a project commenced during the International Geophysical Year and pursued for the next four and half decades by Charles David Keeling. Political scientists David Hart and David Victor have thus suggested that the only policy concern expressed at the time was *science* policy—to define anthropogenic climate change as worthy of sustained funding and institutional support.[10] While it is certainly true that Revelle and others saw CO_2 and climate in part opportunistically—as an important venue for research support—it is equally clear that they perceived potentially broad social and political ramifications. In an interview with *Time* magazine in 1956, for example, Revelle stressed that sea level rise from melting ice could one day cause "salt water to flow in the streets of New York and London."[11]

By the early 1960s, a number of scientific committees had raised the issue of "inadvertent weather modification" caused by increased atmospheric CO_2. These included the National Academy of Sciences Committee on Scientific Problems of Weather Modification, headed by geophysicist Gordon MacDonald, and the Board on Environmental Pollution of the President's Science Advisory Committee (PSAC). The latter's 1965 report, with an appendix by Roger Revelle, Charles David Keeling, meteorologist Joseph Smagorinsky and geochemist Harmon Craig, predicted that "by the year 2000 there will be about 25% more CO_2 in the atmosphere than at present [and] this will modify the heat balance of the atmosphere to such an extent that marked changes in climate, not controllable through local or even national efforts, could occur."[12] The report touched on possible mitigation schemes, such as atmospheric cloud seeding to block sunlight.[13] It also reached the Johnson White House, where speech writer

9. Roger Revelle and Hans E. Suess, "Carbon Dioxide Exchange Between Atmosphere and Ocean and the Question of an Increase of Atmospheric CO_2," *Tellus* 9 (1957): 18–27; see also other articles in same volume.

10. David M. Hart and David G. Victor, "Scientific Elites and the Making of U.S. Policy for Climate Change Research, 1957–74," *Social Studies of Science* 23 (1993): 643–80.

11. "One Big Greenhouse," *Time*, 28 May 1956, http://www.time.com/time/magazine/article/0,9171,937403,00.html (accessed 26 Nov 2007).

12. *Restoring the Quality of Our Environment,* Report of the Environmental Pollution Panel, President's Science Advisory Committee, The White House (Washington, DC: U.S. Government Printing Office, Nov 1965), 9; see also Appendix Y4.

13. Ibid., 127.

Bill Moyers included a reference to its concern in a Congressional message: "This generation has altered the composition of the atmosphere on a global scale through . . . a steady increase in carbon dioxide from the burning of fossil fuels."[14]

The CO_2 question was of particular concern to Gordon MacDonald, who served on President Richard Nixon's Council on Environmental Quality. In the early 1970s, MacDonald, along with Alvin Weinberg, helped persuade the Department of Energy to create a CO_2 research program.[15] By the mid-1970s, climate modelers had begun to forecast the likely change in temperature associated with atmospheric CO_2 increase, and several more reports were issued. One of these reached the Carter White House.

"The Long Term Impact of Atmospheric Carbon Dioxide on Climate," released in April 1979, was a report of the JASON committee—the secretive group of scientists, mostly physicists, with high level security clearances who have advised the U.S. government on science and technology since the early 1960s.[16] In the late 1970s, Assistant Secretary of Energy Edward Frieman had turned to the JASONs to evaluate the DOE programs on CO_2 and climate. Gordon MacDonald, with his long-standing interest in the issue, led the study.

The JASON scientists concluded that atmospheric CO_2 could be expected to double by the year 2035, leading to mean global temperature increases of 2–3°C. Of particular concern was the effect of polar amplification: the JASONs forecasted a polar warming of as much as 10–12°C. The cause for concern

14. Lyndon Johnson, Special Message to Congress, 1965. On 2 Feb 1965, Presidential Assistant Bill Moyers sent the speech to Johnson with a message: "Mr. President, Attached is a copy of the Natural Beauty Message which we hope to send up Wednesday at Noon. It is a long message, but we believe that is what is needed. If we are to meet our deadline, we need your approval this morning." Papers of Lyndon Baines Johnson, SP 2–3 Box 71, Lyndon Baines Johnson Library and Museum, Austin, TX.

15. Sheldon Ungar, "The Rise and (Relative) Decline of Global Warming as a Social Problem," *Sociological Quarterly* 33 (1992): 488. Early Energy Department programs are also discussed in MIT JGC and by Charles David Keeling, "Rewards and Penalties of Monitoring the Earth," *Annual Review of Energy and the Environment* 23 (1998): 25–82. On Keeling, see Richard Somerville, "Charles David Keeling," in *The Earth System: Physical and Chemical Dimensions of Global Environmental Change,* ed. Michael C. MacCracken and John S. Perry, in vol. 1 of *Encyclopedia of Global Environmental Change,* ed. R. E. Munn (Hoboken, NJ: John Wiley & Sons, 2001), 484–85.

16. Gordon MacDonald et al., "The Long Term Impact of Atmospheric Carbon Dioxide on Climate," JASON Technical Report JSR -78–07, prepared for the U.S. Department of Energy, 1989; See also Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Penguin, 2006), 134–38.

became clear when one noted "the fragility of the world's crop producing capacity, particularly in those marginal areas where small alterations in temperature and precipitation can bring about major changes in total productivity."[17]

At the White House Office of Science and Technology Policy, Frank Press, Science Advisor to President Carter, asked the National Academy of Sciences for a second opinion. An Academy committee, headed by MIT meteorologist Jule Charney, affirmed the JASON conclusion: "If carbon dioxide continues to increase, [we] find no reason to doubt that climate changes will result, and no reason to believe that these changes will be negligible."[18] In the press release accompanying the Charney report, the Academy stressed that this conclusion was not just the opinion of the dozen or so members of the committee, but a summation of work done by scores of scientists over the previous decade in the U.S. and abroad: "A plethora of studies from diverse sources indicates a consensus that climate changes will result from man's combustion of fossil fuels and changes in land use."[19]

A consensus view had emerged: global warming would happen and its impact would not be negligible. This view was communicated to the U.S. government by illustrious scientists spanning the disciplines of physics, chemistry, meteorology, and oceanography. It also led in 1988 to the establishment of the Intergovernmental Panel on Climate Change, and in 1992, to the U.N. "Framework Convention on Climate Change." Pledging to translate "the words spoken here into concrete action to protect the planet," U.S. President George H. W. Bush signed the convention. But he almost didn't. Shortly after Bush took office, a prominent physicist was invited to the White House to brief him on climate issues and to present an alternative reading: that global warming was not a problem and no policy action was necessary.[20] The man was William A. (Bill) Nierenberg, who throughout the 1990s was a prominent advocate for that position, and in later years would question whether there was even any good

17. Ibid., 1.

18. Verner Suomi, Introduction to Jule G. Charney et al., *Carbon Dioxide and Climate: A Scientific Assessment,* National Research Council, Ad Hoc Study Group on Carbon Dioxide and Climate (Washington, DC: National Academy Press, 1979), vii.

19. Memo, Climate Research Board, Assembly on Mathematical and Physical Sciences, National Academy of Sciences, "An Evaluation of the Evidence for CO_2-Induced Climate Change," NAS AMPS, Film Label: CO_2 and Climate Change: Ad Hoc: General.

20. Leslie Roberts, "Global Warming: Blaming the Sun," *Science* 246 (1989): 992–93; William A. Nierenberg to the Editor, *Science* 247:4938 (1990): 14; Marjorie Sun, "Global Warming Becomes Hot Issue for Bromley," *Science* 246 (1989): 569.

scientific basis for concern.[21] The origins of his alternative view can be traced back to the year of the JASON and Charney reports.

THE CO_2 PROBLEM OR THE "CO_2 PROBLEM"?

The JASON report had emphasized the serious negative consequences of global warming, at one point even using the word "disaster." Yet it also contained this sentence: "The warming of the climate will not necessarily lead to improved living conditions everywhere."[22] Improved conditions? Everywhere? Who was responsible for the suggestion that global warming would be mostly good? The evidence suggests that it was Bill Nierenberg.

Nierenberg was part of the generation of bright young men whose lives were transformed by the Manhattan Project. Raised in the Bronx by immigrant parents, Nierenberg had attended the prestigious Townsend-Harris High School and the City College of New York, where he studied physics, won a prestigious fellowship to spend a year in Paris, and returned to New York in 1939 fluent in French and fearful of fascism.

In September 1942 he entered Columbia University for his PhD and soon found himself working on isotope separation in the Manhattan Project. After graduating, he taught nuclear physics at the University of California, Berkeley, and in 1953 became director of Columbia University's Hudson Laboratory, created to continue scientific projects begun on behalf of the U.S. Navy during World War II. He subsequently held a series of positions at the interface between science and politics, including NATO's assistant secretary general for scientific affairs. In 1965 he became Director of the Scripps Institution of Oceanography, reinforcing its commitment to applying scientific knowledge to national security problems.[23]

Nierenberg's appointment at Scripps was broadly supported in the weapons community, with the notable exception of Edward Teller, who complained that

21. Nierenberg made these kinds of claims in many venues and formats, but see particularly Robert Jastrow, William Nierenberg, and Frederick Seitz, *Global Warming: What Does the Science Tell Us?* (Washington, DC: The George C. Marshall Institute, 1990); Robert Jastrow, William Nierenberg, and Frederick Seitz, *Scientific Perspectives on the Greenhouse Problem* (Ottawa: The Marshall Press, Jameson Books, 1990).

22. MacDonald et al., "The Long Term Impact" (ref. 16), iii.

23. Charles Townes and Walter Munk, "Obituary, William Aaron Nierenberg," *Physics Today* 54:6 (2001), http://www.aip.org/pt/vol-54/iss-6/p74.html.

with Nierenberg he could never get a word in edgewise.[24] Nierenberg loved to hold forth on virtually any topic, and while colleagues agreed that he was brilliant, they also viewed him as opinionated, generally convinced that he was right about most things. Brilliant or brash, Nierenberg was a highly effective administrator, and strategic in his choice of scientific work. One colleague who worked with him closely for nearly twenty years has said she never knew a scientist who chose his topics more carefully.[25] In the 1970s, Nierenberg had chosen climate. As Scripps Director, he built the Climate Research Division into a world-class unit, hiring experts in climate modeling, cloud physics, aerosols, and other areas. One might therefore suppose that he shared the concerns of his predecessor Roger Revelle about the environmental impact of CO_2-induced warming. The available evidence suggests otherwise.

While Nierenberg clearly saw climate as a fertile area for scientific research and institutional growth, he equally clearly rejected his colleagues' emerging consensus about it. In reviewing Energy Department climate programs in the late 1970s, he began to articulate an alternative view: that CO_2 was nothing to be particularly worried about. There were lots of "man-induced perturbations" in the environment, he suggested, and CO_2 was "not particularly different from others that have been dealt with."[26] Reviewing a draft of the Charney report in 1979, he suggested that "man has survived extreme climate changes in the past and will do so in the future." Foreshadowing what would soon become an oft-repeated position, he argued that the real issue was not climate change per se, but "the degree of [our] adaptability to climate change." He also argued that, since fossil fuel use would peak within the next seventy-five years—as petroleum reserves were depleted—CO_2 levels would peak, too.[27] Between one hundred

24. Charles F. Kennel, Richard S. Lindzen, and Walter Munk, "William Aaron Nierenberg 1919–2000," *National Academy of Sciences Biographical Memoirs* 85 (2004): 1–20; http://www.nap.edu/readingroom/books/biomems/wnierenberg.html

25. D. Day, personal communication, Oct 2007.

26. William A. Nierenberg, "Draft, August 11, 1978, Review of the May 1978 Comprehensive Plan for CO_2 Effects Research and Assessment," SIO WAN, MC 13, Box 172, Folder: Review of the May 1978. . . . Nierenberg also argued that their scientific expertise was not sufficient to answer the questions posed, and therefore there was a need for increased numbers of scientists, particularly in "chemical oceanography, biological oceanography, physical oceanography, soil chemistry, geochemistry, geobiology, and so on"—almost all fields covered by his institution. He did not ask for more money for social scientific research, even though he would argue, as discussed further below, that global warming was really a problem of human adaptability.

27. This is a strange claim, because it ignores coal. Geologists in the 1970s were well aware that although U.S. oil and gas production might be nearing peak, there were centuries of reserves of coal, both in the U.S. and elsewhere. This suggests that this may be an early example of what

and three hundred years from now "the planet should be back to where we are now as far as CO_2 alone is concerned."[28] Climate scientists had been suggesting that the government had to *do* something about greenhouse gases, but Nierenberg concluded that was not so, primarily because humans were capable of adapting to whatever changes ensued.

How did Nierenberg's reading square with others at the Academy? The answer is that two readings of the Charney report were emerging, and one was potentially compatible with Nierenberg's view.

The dominant reading of the Charney report focused on consensus. As Academy President Philip Handler put it, "the group's conclusions are reassuring to scientists in that they confirm the general conclusions of earlier studies."[29] John Perry, the chief staff officer for the Academy's Climate Research Board, took similar comfort from the fact that the JASON committee conclusions lay "comfortably within the envelope of the many prior discussions of the carbon dioxide issue."[30]

This envelope contained three claims. One, that carbon dioxide was a greenhouse gas and its atmospheric concentration had steadily increased since Keeling had begun tracking it two decades earlier. Two, that atmospheric doubling was likely during the next half century or so, given present rates of fossil fuel combustion. And three, that doubling would likely lead to average global temperature increases of 2–3°C, with still greater warming at the poles; no existing climate model predicted "negligible warming."[31] If the predictions of polar amplification were correct, it would mean the total melting of the north polar ice cap. As a leading member of the Academy's polar research board put it, the significance was "potentially momentous."[32] And if it was momentous, the implication was that something had to be done to stop it.

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would later become an extensive habit among those who challenged the scientific evidence of global warming—cherry-picking the data.

28. William Nierenberg to John Perry, 14 Aug 1979, SIO WAN, MC 13, Box 88: Folder: NAS Climate Research Board, Aug 1979.

29. Handler to Press, 25 Oct 1979, in "Evaluation of the Evidence," NAS AMPS (ref. 19).

30. Perry to Charney, 9 May 1979, in "Evaluation of the Evidence," NAS AMPS (ref. 19).

31. Jule G. Charney et al., *Carbon Dioxide and Climate: A Scientific Assessment*, Report of an Ad Hoc Study Group on Carbon Dioxide and Climate, Climate Research Board, Assembly of Mathematical and Physical Sciences—National Research Council (Washington, DC: National Academy of Sciences, 1979), 1.

32. J. Murray Mitchell to A. L. Washburn, 28 Nov 1979, NAS AMPS, Climate Board: Review Panel on Carbon Dioxide General 1979–1981.

There was, however, an alternative reading, which hinged on the question of how soon effects would occur. The JASONs had suggested 2035 as a plausible guesstimate for CO_2 doubling, but in the proposal for the Charney report the Academy had noted that *effects* might be felt sooner. “Plausible projections of future carbon dioxide concentrations suggest several-fold increases by the middle of the next century; experiments with models of the earth’s climate system suggest major associated climate changes that might become evident in our own century.”[33] Even if the effects were far off, pollution released now would induce climate effects later, so one still might need to act now to prevent those effects. Vern Suomi, Chairman of the Academy’s Climate Research Board and a member of Charney’s ad hoc committee, thus concluded that a “wait and see policy may be untenable.”[34]

But if climatic effects were far away, then a wait and see policy might be reasonable, while more research was done. Philip Handler, for example, having expressed satisfaction with the Charney report’s consistency with previous work, nevertheless agreed that there was much more to be done. The report did not provide all the answers, nor did it even ask all the questions. Geochemist Preston Cloud, famous for his work on the origin of oxygen in the Earth’s atmosphere, went further, suggesting that the JASON report was overblown and hoping that the Academy would respond with “a sober, balanced, re-evaluation of this complex, emotional issue, on which the last word may be long in coming.”[35]

The historian Spencer Weart has noted that scientists’ response to climate change has repeatedly been, “More Research is Needed,” and this certainly was the case here.[36] In the wake of Charney’s strong conclusions, many Academy members responded by emphasizing the unanswered questions, the uncertain feedbacks, and other complications. Some emphasized the existence of negative feedbacks—factors that might slow or stop warming, or even lead to cooling—as well as various simplifications and inaccuracies in the models. Ocean

33. Proposal for Support of Carbon Dioxide and Climate: A Scientific Assessment, submitted by the National Academy of Sciences, MIT JGC, Box 11, Folder 364: Climate Research Board, p. 1.

34. Handler to Press (ref. 29). Handler is quoting Suomi. This comment was particularly telling coming from Suomi, as he had earlier insisted, in light of the JASON report, that it was essential to “establish a sense of proportion in the discussion.” Having made the effort to do just that, Suomi was now concerned, and Suomi was not a modeler.

35. Handwritten note by Preston Cloud on memo, John S. Perry to Preston Cloud, Robert G. Fleagle, and Richard S. Lindzen, Climate Research Board, Assembly on Mathematical and Physical Sciences, National Academy of Sciences, in “Evaluation of the Evidence,” NAS AMPS (ref. 19).

36. Weart, *Discovery* (ref. 8).

absorption of heat was one prominent example of an uncertain feedback; biospheric uptake of CO_2 was another.[37] The Charney panel had concluded that none of these "vitiated the principal conclusion that there will be appreciable warming."[38] Transfer of heat into the deep oceans, for example, might slow the warming, but would not stop it.[39] But others remained unconvinced, because the rate of change clearly mattered. If measurable warming were one hundred years away, that could preclude the need for immediate policy measures.[40]

At the White House, this issue was seen as primary. One member of the JASON committee responsible for its 1979 report recently recalled briefing some members of the U.S. government, one of whom asked, "So when will these effects happen?" When the scientists replied, "Well, maybe in forty years," the official replied, "Get back to me in thirty-nine."[41]

At the White House Office of Science and Technology Policy, Senior Analyst Richard Meserve wrote to Vern Suomi suggesting that a second report was needed to address "when these substantial increases can be expected."[42] Meserve wanted the new committee to assume "a series of plausible scenarios" for CO_2 accumulation, and on that basis to "render its best judgments of the timing and nature of resultant atmospheric changes and of the uncertainty in making these judgments."[43]

Similar thoughts were expressed in Congress. The 1978 National Climate Act had established a national climate research program; Connecticut Senator Abraham Ribicoff was planning to introduce an amendment to it enabling a closer look at CO_2. As Chairman of the Governmental Affairs Committee, Ribicoff had organized a symposium on "Carbon Dioxide Accumulations in the Atmosphere, Synthetic Fuels, and Energy Policy." President Jimmy Carter was proposing a major effort to increase U.S. energy independence by developing "synfuels"—liquid hydrocarbons from coal, oil shales, and tar sands—and scientific experts had noted that this could accelerate CO_2 accumulation.

37. S. H. Wittwer to John S. Perry, 20 Aug 1979, NAS AMPS, Climate Board: Review Panel on Carbon Dioxide & Climate, General 1979–1981.

38. Charney et al., *Carbon Dioxide* (ref. 31), 2.

39. Ibid.

40. Agenda, Climate Research Board Study Group on Stratospheric Monitoring, National Academy of Sciences, Apr 1980, NAS AMPS, Ad Hoc Meeting: Agenda.

41. Henry Abarbanel, personal communication, 26 Oct 2006, La Jolla, CA.

42. Richard A. Meserve to Verner E. Suomi, 5 Oct 1979, Climate Research Board Study Group on Stratospheric Monitoring, NAS AMPS, Ad Hoc: Meeting: Agenda.

43. Ibid.

Ribicoff concluded that more research *was* needed. His amendment authorized the Academy to undertake a comprehensive study of the question of CO_2 and climate, in order to assess the effects of various levels of atmospheric carbon dioxide and "the economic, physical and social impacts of such climate change."[44] It would also recommend how future domestic and international research, monitoring, and assessment should be structured, how the United States could best cooperate in any international efforts, what domestic resources should be dedicated to such efforts, and how often reports should be made to the President, Congress, and the Secretary of Energy. It appropriated up to $2 million for the work.

Ribicoff's amendment was incorporated into the Energy Security Act, which signed into law in June 1980 by President Jimmy Carter, created the Synthetic Fuels Corporation to promote the development of synthetic fuels from coal, oil shale, and tar sands. The worry that global warming might be the Achilles heel of American energy policy was implicitly recognized by Title VII, which provided up to $3 million for "a comprehensive study of the projected impact, on the level of carbon dioxide in the atmosphere, of fossil fuel combustion, coal-conversion, and related synthetic fuel."[45] While the formal charge to the new committee was not formulated until June of the following year, a committee was already in place by October 1980, with Nierenberg as its chair.

BILL NIERENBERG AND THE CARBON DIOXIDE ASSESSMENT COMMITTEE

Academy records do not reveal how or why Nierenberg was chosen for the job. John Perry, the staff member who was intimately involved in all aspects of the study, has no recollection, but suggests that both Nierenberg's overall stature and his well-known conservative politics would have been viewed as assets.[46] In 1981, Nierenberg joined the transition team of the new Reagan administration, advising on candidates for positions at scientific agencies; Academy leaders likely viewed that as an asset, too.

44. Abraham Ribicoff to Philip Handler, 30 Oct 1979, Climate Research Board Study Group on Stratospheric Monitoring, NAS AMPS, Ad Hoc: Meeting: Agenda.

45. Energy Security Act Text, found in SIO WAN, Box 88, Aug 1979. See also http://www.mbe.doe.gov/me70/history/1971-1980.htm.

46. John Perry, interview by author, Washington, DC, 29 Oct 2007.

Nierenberg also seems to have done a certain amount of groundwork, if not actual lobbying, for the job. In August 1979, as the Charney group was compiling its conclusions, Perry was pondering a possible follow-up on the "biospheric aspects of the problem: the role of soils, forests, and other elements of the terrestrial and marine biota in determining the future atmospheric levels of CO_2."[47] Charney and his colleagues had focused on what climate models predicted would happen to the atmosphere, and how the biosphere would respond. Following normal Academy patterns, Perry suggested to members of the Climate Research Board that the new committee should not undertake original research, de novo, but simply review the adequacy and conclusions of existing work.[48]

Nierenberg wrote a long and thoughtful letter arguing against that view. A new review should indeed be done "*de novo*," he suggested, including the "atmospheric climate modeling contract and radiation, biosphere and the oceans and geology."[49] Nierenberg argued strongly for an integrated approach to this issue, feeling that it was a mistake to "fractionize" the question, and it would be better to integrate biology, geology, and oceanography. Given the interdisciplinary nature of the problem, he argued, the membership of the new committee should be chosen "with more than ordinary care."[50]

Nierenberg meanwhile wrote many additional letters to colleagues on the issue, situating himself to do the integrated assessment that he had proposed and by October he had been asked to form a "small, *ad hoc* group . . . to recommend a way to proceed."[51] When the opportunity came to consititute the larger committee, he included more than just biology, geology, and oceanography. He included economics, too.

Nowadays, Academy committees often include a social scientist or two, but in the past that was less common. Thus, it is a striking feature of the CO_2 assessment committee that its members included two economists, William Nordhaus, Professor of Economics at Yale, and Thomas Schelling, Professor of Economics at Harvard. Schelling was the more prominent of the two. In 2005, he won the Nobel

47. John Perry to Members, Climate Research Board, Assembly on Mathematical and Physical Sciences, National Academy of Sciences, 3 Aug 1979, NAS AMPS, Climate Board: Review Panel on Carbon Dioxide & Climate, General 1979–1981.

48. Ibid.

49. William Nierenberg to John Perry, 10 Aug 1979, NAS AMPS, Climate Board: Review Panel on Carbon Dioxide & Climate, General 1979–1981.

50. Ibid.

51. John Perry to Richard Meserve, 30 Oct 1979, NAS AMPS, Climate Board: Review Panel on Carbon Dioxide & Climate, General 1979–1981.

Memorial Prize in economics, but in the 1980s he was already a very famous economist. Having worked on the Marshall Plan and in the Truman White House before becoming an economics professor, first at Yale and then at Harvard, his views had long been widely sought in Washington, D.C. His 1960 book, *Strategy of Conflict*, analyzing the Cold War in terms of game theory, had made him well known in policy circles. In 1984 he would be elected to the Academy—an honor that rarely comes without first having firm connections there—and his views on global warming were already well known.

In 1980 Schelling had chaired the first academy committee to look at the economic and social dimensions of anthropogenic warming—a committee that included Nierenberg, Nordhaus, and, among others, McGeorge Bundy. The committee's report, in the form of an eleven-page letter to the Academy, suggested that the Charney report had not quite framed the question properly. The reason had everything to do with timing—the issue that accounted for the two emerging alternative readings. If warming wouldn't happen for a million years, we would all agree it was nothing to worry about. If it were about to happen tomorrow, then we'd all be very worried. So the subject was not climate change per se, but its rate, and whether we can "learn faster than the problem can develop."[52]

Schelling's views resonated with those in the Academy, and perhaps elsewhere in Washington, who felt that there was a serious foreign policy risk to over-emphasizing the U.S. role in climate change. In July 1980 Mexican officials had blamed the United States for a severe drought, accusing the U.S. of diverting their rain by weather modification programs. An article in the *Washington Post* dismissed the concern as just another example of "blaming the power of Uncle Sam," suggesting that the furor arose from speculations by Mexican meteorologists "suddenly thrust from academic obscurity into the national limelight." Another article, authored by the chief of Australia's leading government scientific agency, reported evidence of climate change due to "increased carbon dioxide in the atmosphere."[53]

If CO_2 did affect the Mexican or Australian climate, then it *would* be the fault of the United States, because the U.S. was the world's principal producer of greenhouse gases. In the early 1980s, many global problems were being framed in terms of north-south conflict, and climate change could easily be viewed in that light.

52. Thomas Schelling et al. to Philip Handler, 18 Apr 1980, "Ad hoc Study Panel on Economic and Social Aspects of Carbon Dioxide Increase," on 11. Courtesy of Janice Goldblum, National Academy of Sciences Archive.

53. *Australian Science Newsletter,* Jul 1980, vol. 7, no 7, clipping in SIO WAN MC 13, 88: NRC/CRB Jul–Aug 1980. Christopher Dickey, "Dry Mexico Thinks U.S. Hijacked Its Hurricane," *Washington Post*, 7 Jul 1980.

Jesse Ausubel, a former staffer at the Climate Research Board who would soon return to work on the Nierenberg report, wrote to John Perry suggesting that the Academy should be judicious in discussing the topic. If scientists weren't careful, he warned, "CO_2 may much more rapidly become a focus of North-South (or other) political conflict than most of us had expected, . . . [O]ne can imagine the combination of [scientific] scenarios and reports of 'evidence' producing a volatile situation." At early meetings of the Nierenberg committee in late 1980, it quickly became clear that Nierenberg shared Schelling and Ausubel's view.

While the natural scientists on the committee all expressed the opinion that global warming was a serious, if not grave, concern, Nierenberg repeatedly tried to bring forward suggestions that it might not be. In some cases this took the form of evidence from the physical sciences. For example, at one early meeting he drew attention to a proposal by Val Worthington, chair of the Oceanography Department at Woods Hole, suggesting that the oceans were not currently producing bottom water—the cold dense surface waters, formed in polar regions that carry atmospheric constituents, including carbon dioxide, into the deep sea. If Worthington's idea was correct, then Keeling's observed increase in atmospheric CO_2 might not be the result of human activities, but rather of a "failure of the ocean circulation." No one took up the idea with any enthusiasm. The meeting minutes record, "Most oceanographers do not agree with these ideas."[54]

But Nierenberg's principal tactic was to rely on the arguments provided by the two economists. At the first full discussion of the issues facing the committee, both Schelling and Nordhaus introduced the idea that climate change was not necessarily bad, that most likely it would have both negative and positive effects.[55] Nordhaus wanted to evaluate costs and benefits, suggesting that although he "suspected that the impacts of increasing carbon dioxide would be negative," they might not be, and it would be hard to prove either way, given the complexity of social and economic systems.[56]

54. Climate Board Carbon Dioxide Assessment Committee, Second Session, 28 Oct 1980, SIO WAN, 88: NRC/Climate Research Board Jul–Aug 1980, on 3. This view was subsequently disproved by studies of tritium and CFCs, which showed that these recently produced products have been carried into the deep ocean. Later carbon isotope studies also showed that the carbon isotope context was only consistent with a fossil fuel source. See Prosenjit Ghosh and Willi A. Brand, "Stable Isotope Ratio Mass Spectrometry in Climate Research," *International Journal of Mass Spectrometry* 228 (2003): 1–33. And humans of course have good records of the total amount of coal, oil, and gas mined and drilled in the twentieth century.

55. Climate Board Carbon Dioxide Assessment Committee, Second Session, 28 Oct 1980, SIO WAN (ref. 54), 6.

56. Ibid., 7.

Schelling went further, suggesting that attempts to model climate impacts might be fruitless, because the world would change beyond our imagining in the time frame involved. "Analysis of the problem requires some model of the future as distant from today as 1905," he argued, by which time "everything is likely to change." Schelling thus implied that that while climate change was uncertain, technological improvement was not, and the latter might solve the former with no need for policy intervention. Nierenberg thus found support for his views not from his fellow natural scientists, but from the economists on the committee. And it was the economists' view that the final report would place front and center.[57]

TODAY'S PROBLEM OR TOMORROW'S?

John Perry was evidently grappling with these issues—when climate change would happen, and whether or not it would be bad—when he wrote a memorandum late in 1981 listing some papers from the late nineteenth and early twentieth centuries that were commonly viewed as setting out the theory of greenhouse gases and climate change. These included papers by Svante Arrhenius, Thomas C. Chamberlin, and John Tyndall; Perry noted that reading them revealed not only their "intellectual quality and lucid style," but also the "misquotations and mythology that have accreted over the years."[58] Perry seemed to be considering the possibility that the problem had perhaps been exaggerated. Arrhenius, he noted, calculated the likely temperature increase from doubling carbon dioxide as 4.9–6.1°F, "not the 9° often quoted." Tyndall emphasized the beneficence of the greenhouse effect, without which "the warmth of our fields and gardens would pour itself unrequited into space, and the sun would rise upon an island held fast in the iron grip of frost."[59] Perry was not asserting that climate change would be desirable; he was simply noting that some prominent scientists in the past had thought so and it was scientifically reasonable to consider the possibility that they might have been right.

57. On 15 June 1981 the National Research Council formally charged the new committee with the task of reviewing and updating the conclusions of the Charney report, "in light of subsequent research and independent studies of similar scope," under the provisions provided by the Energy Security Act.

58. John Perry to members of the CO_2/Climate Review Panel, 2 Dec 1981, NAS AMPS, Climate Board: Review Panel on Carbon Dioxide & Climate, General 1979–1981.

59. Ibid. The quote is from John Tyndall, "On Radiation Through the Earth's Atmosphere," *Proceedings of the Royal Institution* (23 Jan 1863), also found in *Philosophical Magazine* 25 (1863): 30–54.

Ultimately Perry rejected this view. In an editorial published in the journal *Climatic Change*, he explained why. Most people assumed that we could "continue to meet our energy needs through a judicious use of the world's ample remaining stocks of fossil fuels, mainly coal, during a long transition to a sustainable global energy system," but "damage to the earth's climate may be the major flaw in this strategy."[60] Echoing Revelle and Suess, he noted that burning fossil fuel "returns to the atmosphere carbon that was extracted by ancient plants many millions of years ago." Following the Charney report, he noted that "the panel members tried but were unable to find any factors that could reduce these expected changes to negligible proportions." Quoting Schelling, he noted that the issue would "pose exceedingly difficult and divisive policy questions for all the world's nations." And following Alvin Weinberg, the Oak Ridge National Laboratory director who helped to establish programs on carbon and climate in the early 1970s, he concluded that these studies supported the conclusion that climate change might be "the primary limiting factor on energy production from fossil fuels over the next few centuries."

Even if doubling of CO_2 might not happen for forty or fifty years, effects might be felt sooner. "Physically, a doubling of carbon dioxide is no magic threshold," Perry noted, it was just a convenient point of comparison. "If we have good reason to believe that a 100 percent increase in carbon dioxide will produce significant impacts on climate, then we must have equally good reason to suspect that even the small increase we have already produced may have subtly altered our climate." So he concluded, "[c]limate change is not a matter for the next century; we are most probably doing it right now," and his title, "Energy and Climate: Today's Problem, not Tomorrow's," encapsulated this view. Schelling had earlier expressed the hope that we could "learn faster than the problem can develop." Perry now concluded: "The problem is already upon us; we must learn very quickly indeed."[61]

Meanwhile the consensus among climate scientists was broadening. One important component came from an Academy sub-committee, reporting to Nierenberg's group, empanelled to update the Charney report, and to consider the effect of ocean heat absorption on the rate of climate change.

The committee was chaired by Joseph Smagorinsky, director of the world renowned Geophysical Fluid Dynamics Laboratory (GFDL) in Princeton, New

60. John Perry, "Energy and Climate: Today's Problem, Not Tomorrow's," *Climatic Change* 3 (1981): 223–25, on 223.

61. Ibid., 225.

Jersey. Smagorinsky was an author of the 1965 appendix to the PSAC report that had first alerted the Johnson administration to climate issues.[62] Other members included some of the pioneers of climate modeling: Syukuro Manabe and Kirk Bryan, Jr., of GFDL, James Hansen of the Goddard Institute for Space Studies, and Francis Bretherton, Director of the National Center for Atmospheric Research (NCAR). V. Ramanathan and Stephen Schneider, also of NCAR, were invited as outside experts to assist in the discussion of energy-balance models.

The Charney report had noted that the role of the oceans in the climate system was important but poorly understood. (Models at this time represented the ocean as a 1-m water layer, sometimes referred to as "swamp ocean.") A major question was whether ocean heat absorption could counteract the atmospheric warming effects of greenhouse gases. It was well known that the upper fifty meters or so of the ocean, which is subject to mixing by winds, absorbs both heat and CO_2, and that the time frame for this upper mixed layer to equilibrate with the atmosphere was only two to three years. But how quickly could this heat transfer to the rest of the ocean? The answer was important, because heat transfer into the deep ocean could slow the climate response to increased atmospheric CO_2, perhaps long enough to obviate the problem.

Tritium from H-bomb tests had taken about ten years to mix down to a level of 300–700 meters, so if one assumed that heat behaved as a "passive tracer"—more or less in the same manner as tritium from hydrogen bomb fall-out—then the incremental heat related to atmospheric CO_2 would penetrate and warm the oceans to depths of a few hundred meters within a few decades. To warm the entire ocean, however, would take many centuries, perhaps a millennium.[63] This was based solely on consideration of downward mixing; it did not account for any feedback, such as slowing of the rate of heat transfer into the ocean as it warmed. In short, the upper, mixed layer would more or less keep pace with atmospheric warming, but the rest of the ocean would warm far more gradually. The committee thus concluded that heat transfer to the deep ocean was "too slow to be of dominant importance on a global scale for time scales less than 100 years."[64]

62. *Restoring the Quality of Our Environment* (ref. 12), Appendix Y4.

63. Joseph Smagorinsky et al., *Carbon Dioxide and Climate: A Second Assessment* (Washington, DC: National Academies Press, 1982), 26–27.

64. Ibid., 3.

The committee's charge to update the Charney report led them into a lengthy discussion of the reliability of climate models, their sensitivity, and the manner in which they handled the vexed matter of clouds. Two recent studies had suggested that the effect of increased CO_2 on surface temperatures would be "much less than estimated by the majority of the scientific community."[65] One was a report published in *Science* by Sherwood Idso, a soil scientist with the Department of Agriculture, who had also complained in letters to the Academy and to officials at the Department of Energy that climate models exaggerated the likely effects of carbon dioxide.[66] The other was a paper in the *Journal of Applied Meteorology*, by MIT meteorologists R. E. Newell and T. G. Dopplick, making essentially the same point.[67]

In response to these challenges, the Smagorinsky group reviewed the results of various modeling groups for the equilibrium surface temperature produced by a doubling of atmospheric CO_2 from 300 to 600 ppm. Simple one-dimensional models gave values of 1.2 to 3.5°C, similar to results obtained for more "realistic" general circulation models. This suggested that even the simple models were "able to simulate certain basic mechanisms and feedbacks," and were useful for comparing model results with observed climate variations.[68] About 1°C of the predicted change came directly from the CO_2 effect, and another degree from various feedbacks, particularly the effect of increased water vapor from increased evaporation in a warmer world.[69] Other positive feedbacks included the ice-albedo and vegetation-albedo effects.

65. Ibid., xvii.

66. Edward Frieman, personal communication, 16 Mar 2006; Draft, Climate Research Board, Assembly on Mathematical and Physical Sciences, National Academy of Sciences, 16 Apr 1981, NAS AMPS; Second meeting, Climate Board Ad Hoc CO_2/Climate Review Panel, Assembly on Mathematical and Physical Sciences, National Academy of Sciences, 3 Apr 1981, NAS AMPS, Climate Board: Review Panel on Carbon Dioxide and Climate: Meetings: Agenda and Minutes, Apr 1981. See also Sherwood B. Idso, "The Climatological Significance of a Doubling of Earth's Atmospheric Carbon Dioxide Concentration," *Science* 207 (1980): 1462–63; Stephen H. Schneider, William W. Kellogg, and V. Ramanathan, "Carbon Dioxide and Climate," *Science* 210 (1980): 6–7. Idso would later become affiliated with the Greening Earth Society, an organization funded by the Western Fuels Association, and the Center for the Study of Carbon Dioxide and Global Change, funded in part by Exxon Mobil. On Exxon Mobil Funding see http://web.archive.org/web/20011220050223re_/www.exxonmobil.com/contributions/public_info.html, and also materials at the headquarters of the American Meteorological Society, Washington, DC.

67. Reginald E. Newell and Thomas G. Dopplick, "Questions Concerning the Possible Influence of Anthropogenic CO_2 on Atmospheric Temperature," *Journal of Applied Meteorology* 18 (1979): 822–25.

68. Smagorinsky et al., *Carbon Dioxide* (ref. 63), 16.

69. Ibid., Table 2.1, on 16.

The Idso and Newell and Dopplick studies disputed these conclusions. Using a static radiative flux model (one in which atmospheric parameters were fixed) to calculate the effect of doubled CO_2 on the tropical ocean, Newell and Dopplick concluded that the climate sensitivity at low latitudes was less than 0.25°C. They acknowledged that values at high latitudes might be higher, and that their work was not intended "to diminish the importance of the anthropogenic CO_2 problem." Idso went further. Claiming to use a "truly independent experimental approach," based on empirical observations of changes in downward radiative flux related to diurnal or seasonal changes in surface air temperature, he calculated a universal "empirical response function" that related radiative forcing to surface temperature changes, a constant which he calibrated by reference to the Arizona monsoon. Idso used this response function to predict future changes for CO_2 doubling, obtaining a value of ≤.26°C—an order of magnitude lower than conventional wisdom. "Until this discrepancy is resolved," he declared, "we should not be too quick to limit our options in the selection of future energy alternatives."[70]

The Smagorinsky committee tried to make peace by suggesting that there was no actual discrepancy between the two approaches: the Newell-Dopplick and Idso analysis of *surface* energy balance was correct as far as it went, but it was incomplete. When one considered the complete surface-atmosphere system, including the tropospheric response and various feedbacks related to water vapor, the oceans, and thermal inertia, one obtained the higher values.[71]

Newell and Dopplick had omitted many known pertinent parameters, they argued, including the transfer of heat from the tropics to high latitudes (which drives the entire climate system), and the effect of increased surface heat flux on both atmospheric temperature and humidity, which in turn affects surface temperature. These omissions led them to over-estimate heat flux away from the surface. Hence, it was "not difficult to appreciate why Newell and Dopplick indicate an extremely small sensitivity of surface temperature to an increase in atmospheric CO_2," the committee wrote.[72]

Idso had calibrated his response function with day-to-day and seasonal variation; the committee considered these data inappropriate to understanding

70. Idso, "Climatological Significance" (ref. 66): 1462–63.

71. Others were not so charitable. Stephen Schneider and colleagues lambasted Idso for claiming to refute their own work, but without giving sufficient details of his methods for anyone else to replicate or analyze them. Schneider, Kellogg, and Ramanathan, "Carbon Dioxide" (ref. 66), 6.

72. Smagorinsky et al., *Carbon Dioxide* (ref. 63), 22.

long-term climate change. An approach calibrated from short-term surface temperature changes was "misleading" when used to estimate the effects of long-term forcings.[73] Moreover, in one of his scenarios, Idso had compared the temperature of a hypothetical airless Earth, heated by solar radiation alone, with our actual Earth, to evaluate the degree of warming attributable to atmospheric effects. But this was a faulty comparison. "The radiation from our present warmer and wetter atmosphere is considerably greater than [Idso's] initial forcing [i.e., a dry atmosphere], reflecting powerful amplifying feedback processes, and thus represents a mixture of cause and effect. Failure to distinguish clearly and consistently between cause and effect permits erroneous and virtually arbitrary conclusions to be drawn."[74] But the most important problem of all was that his approach was "not energy conserving."[75] It might, therefore, violate the first law of thermodynamics.

Richard Lindzen, a meteorology professor at MIT and Academy member, had also taken issue with the models, but on a different basis. He argued that that they failed adequately to account for the effects of clouds.[76] The issue at stake was complex. In a warmer world, more heat leads to more evaporation, and therefore more water vapor in the atmosphere. This can produce more clouds, and since clouds block the sun, it could slow or stop the greenhouse effect—a negative feedback. However, clouds also trap heat (think of a camping trip and how cold clear nights can be), so global warming might also lead to warmer nights, negating the daytime cooling effects. And water vapor is itself a greenhouse gas, so if warming leads to more evaporation, that strengthens the greenhouse effect.

Lindzen argued strongly for the sun-blocking effect, and therefore that global warming would be largely self-cancelling. The committee pointed out that Lindzen had provided no *evidence* for his claim that clouds would cancel the greenhouse effect—and that lacking data either way, there was no reason to presume the canceling effect. It could equally well be that night-time warming

73. Ibid., 20.

74. Ibid. So much for making peace!

75. Ibid., 21. A later analysis by Robert D. Cess and Gerald L. Potter developed this point, concluding that both Newell and Dopplick and Idso *had* made assumptions that violated energy conservation; Robert D. Cess and Gerald L. Potter, "A Commentary on the Recent CO_2–Climate Controversy," *Climatic Change* 6 (1984): 365–76.

76. Lindzen would later become associated with TASSC, The Advancement of Sound Science Coalition, an organization funded by Philip Morris to challenge the scientific evidence of the hazards of second-hand smoke. For Lindzen's links to TASSC and the second-hand smoke campaign, see the collection of the Legacy Tobacco Documents Library, http://legacy.library.ucsf.edu.

would dominate.[77] If Lindzen's point was that the science of cloud physics was not settled, then he was clearly right, but if his point was that global warming wouldn't happen, the committee felt he was almost certainly wrong. More research on this question *was* needed.

As for the overall reliability of models, no one on the committee denied that they were highly simplified. Most of them, for example, had only one spatial dimension, but they nonetheless insisted upon their value. One could learn a lot from simple models, just as one might learn from a simple laboratory experiment. The real question was whether there was any evidence for systematic bias. Hansen, Manabe, and others defended their efforts, along with those of their colleagues, by noting that you could vary many aspects of the models—changing humidity, omitting or including reflection office and vegetation, adjusting the rate at which the Earth adjusted to increased CO_2—but you still got values of 1.2–3.5°C for doubling CO_2. The basic result was very robust.[78] Without further evidence, one had to assume that the errors might go either way: the models might exaggerate the problem, or they might underestimate it. The suggestion that the Earth was insensitive to CO_2—or that existing models exaggerated the sensitivity—was "based on incomplete methods or observation."[79] Hansen noted that the studies Idso cited actually "confirm rather than refute the implications of model studies."[80]

The issue of model reliability was a clearly sensitive one, as it struck at the heart of the work of many committee members.[81] It had also been raised by

77. The point is discussed at length in a number of documents generated by the Carbon Dioxide Assessment Committee in a microfiche collection of NAS AMPS. See Climate Board, Carbon Dioxide Assessment Committee, Ad hoc CO_2/Climate Review Panel, Minutes of First Meeting, 2–3 Mar 1981, Agenda and Minutes, Apr 1981, particularly 4–5, handwritten notes, Ad hoc Review Panel, "Mtg CO_2/Climate, Damon Room," no date, but likely the first meeting, 2–3 Mar 1981, NAS AMPS, Agenda and Minutes, Apr 1981 and Draft, 16 Apr 1981, second meeting, 2–3 Apr 1981.

78. Ibid., 3.

79. Smagorinsky et al., *Carbon Dioxide* (ref. 63), 19.

80. Climate Board, Carbon Dioxide Assessment Committee, "Agenda and Minutes, Apr 1981," NAS AMPS (ref. 77), 3. This pattern would later be repeated by climate change skeptics and deniers, for example, Michael Crichton, whose book, *State of Fear*, cites several studies that support, rather than refute, the reality of anthropogenic global warming.

81. For one of our views on model reliability, see Naomi Oreskes, Kristin Shrader-Frechette, and Kenneth Belitz, "Verification, Validation, and Confirmation of Numerical Models in the Earth Sciences," *Science* 263 (1994): 641–46; Naomi Oreskes, "Evaluation (Not Validation) of Quantitative Models," *Environmental Health Perspectives* 106 (1998): 1453–60.

the JASON committee in a follow up to their 1979 report. Climate models, they noted, had "so many disposable parameters that an artificial validation against present climate may be easily obtainable."[82] In other words, you could adjust the model to force a match with the real climate. But no one on the JASON committee was actually a climate modeler—nor were Idso or Lindzen—and Smagorinky's committee felt they knew little about actual modeling practice. In theory you could put anything you liked into a model, but in practice there were actually relatively few adjustable parameters. Modelers' success in simulating contemporary climate did not come from adjusting parameters to make the model work, but rather from "correct understanding and modeling of important processes."[83] They did not fudge, and they resented the implication that they did. No one was claiming that climate models could make specific, quantitative predictions of exactly what would happen precisely when. The claim was simply that the models helped one to understand what, on the basis of scientific principles, was likely to happen.[84]

The Smagorinsky committee's report was issued in July 1982, and it reaffirmed Charney's.[85] Frank Press, now President of the National Academy of Sciences, forwarded a copy of the 1982 report to his successor in the Reagan White House, Science Advisor George A. Keyworth, with a cover letter summarizing the key conclusions. "Climate models quite consistently indicate that major increases in atmospheric carbon dioxide will produce significant changes in climate. . . . Recent assertions that climatic effects will be negligible are convincingly refuted."[86] The committee put it more bluntly: *"The present study has not found any new results that necessitate substantial revision of the conclusions of the Charney report."*[87] Meanwhile, still more reports were emerging.

82. H. Abarbanel et al., "The Carbon Dioxide Problem: DOE Program and a General Assessment," Department of Energy, Technical Report DOE/ER/30002-T2 (1980), JASON Technical Report JSR 80–06.

83. Climate Board, Carbon Dioxide Assessment Committee, "Agenda and Minutes, Apr 1981," NAS AMPS (ref. 77), 5.

84. Climate Board, Carbon Dioxide Assessment Committee, "Draft, 16 Apr 1981," NAS AMPS (ref. 77).

85. Smagorinsky et al., *Carbon Dioxide* (ref. 63), 1–7.

86. Frank Press to George A. Keyworth, 14 Jul 1982, letter found in Oreskes' personal copy of Smagorsinky report (ref. 63), purchased online.

87. Smagorinsky et al., *Carbon Dioxide* (ref. 63), 1, emphasis in the original.

A SNOWBALL OF WARMING CONCLUSIONS

The 1979 JASON report had created a bit of a stir in Energy Department circles; it had also generated resentment from climate scientists who wondered why physicists were treading on their territory.[88] In 1980 and 1981, three more JASON reports on climate came out, partly in response to these complaints. One emphasized the need for more work in testing models, and for the development of good monitoring systems to detect climate change as it began to occur, especially near the poles. Another focused on other gases besides CO_2, including methane and ozone. A third provided a full rebuttal of Idso's argument, concluding that Isdo had used poor quality data. He had claimed the warming effect of doubling CO_2 was 0.26°C; using his methods, but better data from satellites, the JASONs found the result to be 1.8°C. Using a somewhat more sophisticated approach, the result was 3°C—the same amount claimed by Charney and Smagorinsky.

Gordon MacDonald summarized these reports, as well as those of various other committees, conferences, and consultations sponsored by the Department of Energy, in a volume published in 1982. The 250-page book, published by Harper and Row, gave detailed treatments of the sources of CO_2, how it gets distributed in the environment, how that affects climate, and how serious the future effects of increased CO_2 might be. An entire chapter was dedicated to the potential break-up of the West Antarctic Ice Sheet. MacDonald's book now made it easy for any scientist wishing to get up to speed on the question to do so.[89]

The following year, the Environmental Protection Agency (EPA) weighed in on the issue of sea level rise. Scientists on the Strategic Studies Staff of the Office of Policy Analysis addressed the likely range of future sea level rise. The 120-page report had over a hundred reviewers—and its authors made it a point to say that no policy was being suggested or implied.[90] They did ask for money for more research, but they also affirmed the basic issue at stake:

> Concentrations of atmospheric CO_2 and other greenhouse gases will continue to increase in the coming decades. Two National Academy of Sciences panels have concluded that higher levels of these gases will almost certainly produce a

88. Henry Abarbanel, personal communication; see also Finkbinder, *The Jasons* (ref. 16), 134–37.

89. Gordon J. MacDonald, ed., *The Long-term Impacts of Increasing Atmospheric Carbon Dioxide Levels* (Cambridge, MA: Ballingen/Harper & Row, 1982).

90. J. S. Hoffman, D. Keyes, and J. G. Titus, *Projecting Future Sea Level Rise*, Report for the U.S. Environmental Protection Agency (Washington, DC: Government Printing Office, 1983): iii–iv.

> large global warming. That warming, by thermally expanding the ocean and by causing the transfer of ice and snow resting on land to the oceans, should raise sea level substantially faster than the rise that has taken place during the past century.[91]

While a precise prediction was not possible, the likely range of future sea level could be suggested. A global rise of 144 to 217 centimeters by the end of the twenty-first century was likely, and as much as 345 centimeters could not be ruled out. Either way, one could confidentally expect major coastal impacts, including shoreline retreat, accelerated erosion, flooding, saltwater intrusion, and various economic effects.

Thus, to the list of major reports affirming the importance and significance of CO_2 and climate—PSAC, the Special Commission on Weather Modification, JASON, and Charney—one could now add Smagorinsky, MacDonald, and the EPA. These reports represented the combined expertise and expert opinion of leading scientists across the country—scientists who collectively had served four different U.S. presidents, in reports issued by the White House, diverse U.S. government agencies, and the U.S. National Academy of Sciences. They represented physicists and earth scientists, Democrats and Republicans, scientists in government and scientists in academia, scientists with security clearances and those without. But their conclusions troubled the Reagan White House.

The Reagan administration had come to power in 1980 on a platform of unleashing the power of private enterprise. In the realm of energy, their policy was to encourage big business to use the country's reserves of uranium and coal to achieve self-sufficiency, and the administration began to make clear to the Academy that strong statements about global warming would be viewed as undercutting that commitment. In meetings with the Climate Research Board, Energy Department officials told Academy members that they "did not approve of . . . speculative, alarmist, 'wolf-crying' scenarios."[92] They simply wanted "guidance on the on-going research program."[93] Moreover, there was no need for alarm, the new senior policy analyst at White House Office of Science and Technology, Tom Pestorius, insisted, because "technology will

91. Ibid., vi.

92. John Perry to Carbon Dioxide Assessment Committee, 27 Sep 1982, SIO WAN, Box 91: NAS Climate Research Board/CO_2 Committee, Aug–Sep 1982, p. 2.

93. Climate Board, Carbon Dioxide Assessment Committee, Fourth Session, 28–29 Sep 1981, Washington, DC, SIO WAN, Box 90: NAS Climate Research Board/CO_2 Committee, p. 20.

ultimately be the answer to the problems of providing energy and protecting the environment."[94]

Meanwhile, at the Energy Department the man who had run its climate programs since the 1970s, and had nurtured Dave Keeling's CO_2 measurements program, had been removed.[95] His replacement, Fred Koomanoff, had informed Keeling that his funding would be discontinued as the Reagan administration took steps to trim the Department's climate research programs.[96]

The problem got worse when Democrats in Congress held hearings on the climate research program. Koomanoff was a key witness, and he emphasized model uncertainty—the very point that Lindzen and Idso had been pushing. In response, the environmental advocacy group Friends of the Earth accused the administration of trying to eliminate science that disagreed with their ideology. The administration had expressed "skepticism about the seriousness of climate change," they noted, and now it was cutting research that "might disprove its conceptions." Why? To avoid any scientific justification for environmental regulation, which they considered an "unwarranted intrusion on individual and corporate decision-making."[97]

Koomanoff's testimony unsettled the climate scientists, but the difficulty went further. In a meeting with the Academy, he made it clear that the administration expected the scientists to toe the line. "Those who must make decisions . . . deserve better than to hear divergent voices from the scientific community."[98] Climate scientists were in a bind, because while their research results may have been congenial to Friends of the Earth, it was the Energy Department that was footing the bill for their research.

Nierenberg gave the Administration everything it wanted: a report that in his own words was "conservative," that presented a united front, insisted that no action was needed now, and concluded that technology would solve the

94. John Perry to Carbon Dioxide Assessment Committee, SIO WAN (ref. 92), 3.

95. That man was David Slade. See Keeling, "Rewards and Penalties" (ref. 15); and also Anthony Ellsworth Scoville, "Why the U.S. Ignores the Greenhouse Effect," reprint in SIO WAN, Box 90:12 NAS/Climate Research Board/CO_2 Committee, Apr–Jun 1982.

96. Keeling, "Rewards and Penalties" (ref. 15).

97. Friends of the Earth Testimony submitted to the Subcommittee on Natural Resources and the Subcommittee on Investigations and Oversight of the Committee on Science and Technology, U.S. House of Representatives, 25 Mar 1982, SIO WAN, Box 90: 12, NAS/Climate Research Board/CO_2 Committee, April–June 1982; see also Scoville, "Why the U.S. Ignores" (ref. 95).

98. John Perry to Carbon Dioxide Assessment Committee, 27 Sep 1982, SIO WAN (ref. 92).

problem with no need for government intervention.[99] Instead of Chicken Little, he gave them Dr. Pangloss.

CHANGING CLIMATE: THE NIERENBERG REPORT (1983)

In 1983, the full report of the Nierenberg Committee, "Changing Climate: Report of the Carbon Dioxide Assessment Committee," was released. At 496 pages, the report was nothing if not extensive. Standing in marked contrast to the twenty-two pages of the Charney report or even the 170 pages of the 1979 JASON report, it certainly appeared to be the integrated study that Nierenberg had advocated. But it was not really integrated at all. It was, in fact, highly dis-integrated.

Today, most Academy reports are consensus documents to which all members sign their names. Certain individuals may mostly write certain chapters, but they do not normally sign them. Similarly, NRC staff may ghostwrite sections or even whole chapters, but authorship is reserved for the scientists who make up the official members of the panel. Generally, both the committee members and the NRC staff work hard to ensure that the conclusions of individual chapters are compatible both with each other and with the report's summary and conclusions. If they are not, the differences are acknowledged.[100]

In the Nierenberg report individual chapters, and in some cases even sections of chapters, were credited to separate authors. The chapter on detection and monitoring was authored by six scientists who were not members of the committee. Some sections were written by the NRC Staff member, Jesse Ausubel, who had raised the issue of the foreign policy implications with John Perry and was not a PhD scientist.[101]

In the report preface, Nierenberg explained that the CO_2 issue was "so diverse in its intellectual components that no individual may be considered an expert on the entire problem," and that they felt themselves "incapable of judging and endorsing as a group the details of each paper's findings and analysis."

99. W. A. Nierenberg, Opening Statements, CO_2 Dinner Symposium, 19 Oct 1983, SIO WAN, Box 87: Department of Energy-CO_2, Washington, DC, 6 Oct 1983.

100. Peter D. Blair, email communication, 11 Jan 2007.

101. Today Ausubel claims to have been a "main author" on this report, viz., "Reports for which he was main author include *Changing Climate* (National Academy, 1983)," http://phe.rockefeller.edu/jesse/. John Perry also recalls that Ausubel wrote the synthesis, with "some guidance and editing by Nierenberg." Email communication, 27 Nov 2007.

However, there was a large core of common ground "which all members could wholeheartedly and responsibly endorse," and these were presented in the report synthesis.[102] In fact, the conclusions of the individual chapters were very different from one another, and with the exception of the two chapters written by the economists, very different from the synthesis.

The chapters written by the natural scientists were consistent with what natural scientists had already said. No one challenged the basic claim that warming would occur, with serious physical and biological ramifications. Revelle's chapter on sea level rise, for example, noted that "[a] collapse of the West Antarctic Ice Sheet would release about 2 million km^3 of ice before the remaining half of the ice sheet began to float. The resulting worldwide rise in sea level would be between 5 and 6 m[eters]."[103] As a result, "[t]he oceans would flood all existing port facilities and other low-lying coastal structures, extensive sections of the heavily farmed and densely populated river deltas of the world, major portions of the state[s] of Florida and Louisiana, and large areas of many of the world's major cities."[104] Florida could be expected to lose 24% of its total area, Louisiana 27%. The last time sea level was that high was 125,000 years ago, and the world was a very different place. "Disintegration of the West Antarctic Ice Sheet would have . . . far-reaching consequences," Revelle concluded.[105] Even without that, thermal expansion alone would produce 70 cm of rise—a not insignificant figure.

Other chapters addressed potential impacts on climate regimes, water availability, marine ecosystems, and more. The physical scientists allowed that many details were unclear—more research *was* needed—but they broadly agreed that the issue was potentially very serious, with major changes in the offing. Fundamentally the conclusion was the same as before: CO_2 has increased due to human activities, CO_2 will continue to increase unless changes are made, and these increases can be expected to have significant adverse impacts on weather, agriculture, and ecosystems. None of the physical scientists suggested that accumulating CO_2 was not a problem, or that we should simply wait, see, and adapt when and if changes occurred.

But the economists did, and this position provided the bookends of the report. Chapter 1, written by Nordhaus, Ausubel, and Gary Yohe, an economics

102. William Nierenberg et al., *Changing Climate: Report of the Carbon Dioxide Assessment Committee* (Washington, DC: National Academy Press, 1983), xv.

103. Roger R. Revelle, "Probable Future Changes in Sea Level Resulting from Increased Atmospheric Carbon Dioxide," in Nierenberg et al., *Changing Climate* (ref. 102), 443–482, on 441–42.

104. Ibid., 442.

105. Ibid., 446.

professor at Wesleyan University brought in mid-stream as a consultant, focused on future energy use and carbon dioxide emissions. The long and detailed chapter was perhaps the first serious study of the problem that looked at many variables and did not assume linear extrapolations.[106] It began by acknowledging the "widespread agreement that anthropogenic carbon dioxide emissions have been rising steadily, primarily driven by the combustion of fossil fuels." The emphasis here, however, was not so much on what was known, but on what was not known: the "enormous uncertainty" beyond 2000, and the "even greater uncertainty" about the "social and economic impacts of possible future trajectories of carbon dioxide."[107] This uncertainty provided the basis for an argument that no meaningful action could be taken now.

Using a probabilistic scenario analysis, the authors projected atmospheric CO_2 concentrations to the year 2100, using various assumptions regarding energy use, costs, and increased economic efficiencies. The range of possible outcomes was extremely large: from as little as 337 ppm in 2025 (a value *lower* than that already reached in the early 1980s) to as much as 2212 ppm in 2100 (eight times the pre-industrial level).[108] They considered the most likely scenario to be CO_2 doubling by 2065. This was slower than some other estimates, largely because they assumed increasing economic efficiencies, decreasing fossil fuel use in response to scarcity, and some absorption of CO_2 by the oceans.

The authors acknowledged the "substantial probability that doubling will occur much more quickly," including a 27% chance that it would occur by 2050, and allowed that it was "unwise to dismiss the possibility that a doubling may occur in the first half of the twenty-first century." Yet, in their discussions, this is effectively what they did, because the conclusion was built on the presumption that the changes were sufficiently far off as to make it unwise to attempt any intervention now.[109]

The only potentially effective action in the present to accelerate a shift to other energy sources would be a large permanent carbon tax, and that would be hard

106. John Perry, email communication, 27 Nov 2007. Examination of economic models relate to climate change is beyond the scope of this paper.

107. William D. Nordhaus, Gary W. Yohe, and Jesse H. Ausubel, "Future Carbon Dioxide Emissions from Fossil Fuels," in Nierenberg et al., *Changing Climate* (ref. 102), 87–185, on 87.

108. For the interested reader: 337 ppm was already reached before the report came out in 1983. So this value had to have been based on assumptions of cuts in fossil fuel use in the future as the world shifted to other energy sources. Various projections made in the late 1970s assumed that the price of oil would go so high as to drive that shift.

109. Nordhaus, Yohe, and Ausubel, "Future Carbon Dioxide Emissions from Fossil Fuels," (ref. 102), 138.

to implement and enforce. "A significant reduction in the concentration of CO_2 will require very stringent policies, such as hefty taxes on fossil fuels," they noted.

> Moreover, these taxes must be global [to be effective]. To the extent that such an approach can offer guidance, therefore, it suggests that there are unlikely to be easy ways to prevent the buildup of atmospheric CO_2. The strategies suggested later [in the report] by Schelling—climate modification or simply adaptation to a high CO_2 and high temperature world—are likely to be more economical ways of adjusting. . . . Whether the imponderable side effects on society—on coastlines and agriculture, on life in high latitudes, on human health, and simply the unforeseen—will in the end prove more costly than a stringent abatement of greenhouse gases, we do not now know.[110]

Rather than confront their own caveat that changes might happen much sooner than their model predicted, they assumed that serious changes were so far off as to be essentially discountable, and that weather modification and adaptation, when the time came, would be effective responses.

Schelling picked up the thread of this argument in the final chapter of the report, where the social scientists' reframing of the question became explicit. From Callendar to Revelle, MacDonald to the EPA, scientists had focused on CO_2. Understanding the acute sensitivity of the Earth's radiative balance to trace amounts of greenhouse gases, scientists viewed the rapidly rising concentrations with concern, if not alarm. They were not worried about climate change in general—because scientists understood that climate was naturally variable—but rather about the unidirectional change caused by rapid human exploitation of the Earth's stored energy resources.

Schelling thought this didn't make sense, because CO_2 was only a worry *because* it could lead to climate change, and lots of other things caused climate change, too. These included both natural variability and other human activities, such as land use changes and the production of atmospheric dust. The impact of carbon dioxide therefore needed to be assessed together with "other climate-changing activities"; what caused those changes really didn't matter. It was wrong to single out CO_2 for special consideration. Common sense might suggest that if carbon dioxide is the cause of climate change, then controlling it is the obvious solution, but common sense would be wrong. It was a mistake to assume a "preference for preventive over ameliorative programs and for dealing with causes rather than symptoms," Schelling argued, because in some cases treating symptoms is actually cheaper and just as effective. Thus, "[i]t would be wrong to

110. Ibid., 151.

commit ourselves to the principle that if fossil fuels and carbon dioxide are where the problem arises, that must also be where the solution lies."[111] An ounce of prevention might not be worth a pound of cure, and in this case, it probably wasn't. It might well be cheaper and easier to modify the climate or adapt to it.

Schelling's turn to weather modification as a remedy for global warming was a bit peculiar, because in his 1980 report he had noted that "current capabilities give no ground for hoping that controlled weather modification could compensate for global changes in climate."[112] To suggest that in the future those capabilities could so compensate was highly optimistic, resting on a presumption of major technological breakthroughs. It was also ironic given that the history of weather modification research was one of uncertainty, anxiety, and embarrassment. Scientists had persistently doubted that modification really worked; General Electric had given up its program for fear of legal liability; and the U.S. government had been embarrassed by revelations of cloud seeding over the Ho Chi Minh Trail during the Vietnam War.[113] In reaction, Congress had called for an international agreement to prohibit weather modification as weaponry, and in 1977 the United States and U.S.S.R. signed the Convention on the Prohibition of Military or any Other Hostile Use of Environmental Modification Techniques.[114]

Moreover, some of the earliest warnings about climate change had come from the National Academy of Sciences Committee on Weather and Climate Modification. Its members, who included Gordon MacDonald, had realized that if one *could* change the weather deliberately, it stood to reason that one might also do so inadvertently. It was this realization that first led MacDonald to his concern over CO_2-induced climate change, which he called "inadvertent weather modification."

Despite this fraught history, Schelling presumed that future technological innovation would make weather modification feasible, practical, controllable, and affordable. If it didn't, then we could adapt to whatever changes ensued. And if technology did not make adaptation possible, then we could migrate.

111. Thomas C. Schelling, "Climate Change: Implications for Welfare and Policy," in Nierenberg et al., *Changing Climate* (ref. 102), 449–482, on 449.

112. Schelling et al. to Handler, "Ad hoc Study Panel" (ref. 52), 8.

113. James Fleming, "The Pathological History of Weather and Climate Modification: Three Cycles of Promise and Hype," *Historical Studies in the Physical and Biological Sciences* 37:1 (2006): 3–26; James Fleming, "Global Climate Change and Human Agency, Inadvertent Influence and 'Archimedean' Interventions," in *Intimate Universality: Local and Global Themes in the History of Weather and Climate*, ed. James Rodger Fleming, Vladimir Jankovic, and Deborah R. Coen (Sagamore Beach, MA: Science History Publications, 2006), 223–48.

114. http://www.state.gov/t/ac/trt/4783.htm

Migration was a major component of human history, Schelling noted, and when humans moved, they adjusted to different climates—sometimes radically different ones. The history of the United States was a history of migration through climate zones, and in the past people had adapted to those changes, often enthusiastically. It was presumptuous to assert that we should make changes now to avoid effects that future generations might not even mind. Emissions control would be extraordinarily expensive and would only work if undertaken by the whole world, or at least the world's major energy users—a very unlikely prospect. But mitigation and adaptation could be efficacious even if only undertaken locally, so this was the most logical response.

Schelling's discussion was framed by the underlying presumption that the changes under consideration were "beyond the lifetimes of contemporary decision-makers."[115] So far off were these changes, and so profound the uncertainties associated with them, that it was nigh impossible to talk meaningfully about them. Not only did we not know how much energy future populations would use, and therefore how much CO_2 they would produce, we did not know how they would live, how mobile they would be, what technologies they would have at their disposal, and even what climates they might prefer. Moreover, fossil fuel prices were likely to rise, leading to slowing consumption rates, which would delay any anticipated changes. Schelling thus returned to the argument he had made in 1980: if changes were far away, then it would be impossible to predict how troubling they would be. Perhaps by 2100 everyone would be living indoors, with agriculture pursued in controlled hydroponic environments.

However, the physical scientists on the committee did *not* think that the anticipated changes were beyond the lifetimes of contemporary decision makers. Most of them thought that troubling changes might be much closer. Nierenberg's committee had thus produced a report with two quite different views: the physical scientists viewed accumulating CO_2 as a serious problem; the economists argued that it wasn't. A fair synthesis might have tried to reconcile the conflicting views, or at least to account for their different

115. This presumption contradicted the suggestion, in the proposal for the Charney report and in Perry's editorial, that effects would be discernible by the end of the century (less than 20 years from when Schelling was writing). It has also been contradicted by actual events. Still, given that much of the discussion taking place surrounded the dates 2050 and 2100, one could take Schelling's point: life in 2100 would no doubt be different in all kinds of ways that were, in 1983, unimaginable. On the other hand, in hindsight Schelling's argument that the problem is not CO_2 but climate change is clearly wrong. Changing the chemistry of the atmosphere turns out to change the chemistry of the oceans as well, and this may turn out to be more significant for the biosphere than mean atmospheric temperature changes.

presumptions. Instead, Nierenberg's synthesis exclusively followed the position advocated by the social scientists. It did not disagree *with the scientific facts* as laid out by Charney, the JASONs, and all the other physical scientists who had looked at the question in his own report. Instead, it rejected the interpretation of those facts as a *problem*.

> Viewed in terms of energy, global pollution, and worldwide environmental damage, the 'CO_2 problem' appears intractable. Viewed as a problem of changes in local environmental factors—rainfall, river flow, sea level—the myriad of individual incremental problems take their place among the other stresses to which nations and individuals adapt.[116]

In short, Nierenberg reframed the issue as just one of many different changes and challenges facing human society. And since humans had adapted to change throughout history, it stood to reason that we could do so again.

Adaptation would occur primarily through improvements in irrigated agriculture and water delivery systems to address the agricultural impacts; dikes, dams, and landfills to address sea level change; and where necessary, migration. Moreover, premature action would be more costly than delay. "[T]he knowledge we can gain in coming years should be more beneficial than a lack of action will be damaging; a program of action without a program for learning could be costly and ineffective. [So] our recommendations call for 'research, monitoring, vigilance, and an open mind'."[117]

As for international agreements to control greenhouse gas emissions, the synthesis was highly dubious: "[J]ust as we as individuals have little incentive to curtail our emissions, we as a nation have little incentive to curb CO_2 emissions. By curbing our CO_2 output, we make little contribution to the solution and do not know whether we will receive any benefits. . . . [A] CO_2 control strategy could only work if major nations successfully negotiated a global policy [and] there are few examples where a multinational environmental pact has succeeded."[118]

116. Nierenberg et al., *Changing Climate* (ref. 102), 3.

117. W. A. Nierenberg et al., "Synthesis, Carbon Dioxide Assessment Committee," in Nierenberg et al., *Changing Climate* (ref. 102), 5–86, on 61.

118. Ibid., 70. This was an ironic comment, given that the Vienna Convention (1985) and Montreal Protocol (1987) on Substances that Deplete the Ozone Layer would soon be negotiated, as well as the UN Framework Convention on Climate Change (1992). The Kyoto Protocol, which attempted to put teeth into the UN Framework Convention, might have had some impact had the United States not refused to participate. To the degree that Nierenberg's pessimissm influenced others, it could also be viewed as a self-fulfilling prophecy.

At junctures where an important uncertainty was broached, the synthesis consistently took the most sanguine view: that CO_2 use would naturally fall off as future demand for fossil fuel decreased, that deforestation would probably slow down, that weather modification could be made to work, and that the actual increase in mean global temperature for doubling CO_2 was likely to be at the low end of earlier estimates—closer to 1.5°C than to 4.5°C.[119] This last conclusion particularly flew in the face of the prior scientific results; neither Charney nor Smagorinsky's group had suggested that the actual mean temperature increase was likely to be at the low end of their estimates. Nierenberg's synopsis referred the reader to Chapter 4, but this was a set of excerpts from the Smagorinsky panel; no evidence was presented to support the low-end assertion.

Overall, the synopsis emphasized the positive over the negative, the unknowns over the knowns, and the low-end of harmful impact estimates rather than the high-end. Nierenberg quoted, for example, Revelle's 70 cm estimate for sea level rise, but left the question of the West Antarctic Ice Sheet disintegration to the vague statement that "more rapid rates could occur subsequently."[120]

The body of the report contained many challenges to this optimistic angle, noting at various junctures the severity of potential impacts, many of which "could well be a divisive rather than unifying factor in world affairs."[121] Indeed, in his 1980 report, Schelling had above all stressed the "inherently divisive" nature of the whole issue of climate change, both in terms of its potential impacts and in terms of any potential remedies. Yet the conclusion of the Nierenberg report was almost Panglossian—that increased CO_2 might well be a good thing. It had already proven to be beneficial to science, and might well prove broadly beneficial to society as well. The final paragraph of the synopsis concluded:

> The CO_2 issue has proven to be a stimulus to communication across academic disciplines and to cooperation among scientists of many nations. While it may be a worrisome issue for mankind, it is in some respects a healthy issue for science and for people. It is conceivable that CO_2 could serve as a stimulus not only for the integration of the sciences but for increasingly effective cooperation of world issues.[122]

Evidently, CO_2 could lead to world peace.

119. Ibid., 2.
120. Ibid., 2.
121. Ibid., 3, and discussion in chapter 9.
122. Ibid., 81.

From Nierenberg's personal papers and other writings, it is clear that he was particularly moved by the argument that humans had a long history of adapting to climate change in the past, and there was no reason to think they would not continue to do so in the future. True, some climatic effects—like serious sea level rise—might actually make some areas of the world uninhabitable, but this could be addressed through migration. "Not only have people moved," Nierenberg wrote, "but they have taken with them their horses, dogs, children, technologies, crops, livestock, and hobbies. It is extraordinary how adaptable people can be."[123] Yet the report referred to no historical, sociological, or anthropological studies on the effect of those migrations, and in his 1980 report Schelling had noted that past migrations might *not* be a reasonable analog, because "today's political barriers hamper migration, and national boundaries are not likely to be more open in the future."[124]

In the face of the French revolution Marie Antoinette allegedly dismissed the hunger of the peasants who had no bread with the infamous quote, "let them eat cake." Bill Nierenberg's response to climate change was, in essence, let them migrate.

ALVIN WEINBERG'S REVIEW

When the Nierenberg report was sent for peer review, one reviewer noted the obvious disconnect between the physical and social scientific perspectives presented in the report, and the way in which the synopsis took the latter at the expense of the former. That reviewer was Alvin Weinberg.

Weinberg was the retired director of the Oak Ridge National Laboratory, a distinguished physicist who had led the lab for nearly twenty years and written often and eloquently on the relationship between science and society. In his widely read "Reflections on Big Science," he had made an impassioned plea for the importance of large national laboratories in addressing large societal problems.

Weinberg had been one of the first physicists to recognize the ramifications of global warming, arguing in 1974 that that climate impacts might limit our use of fossil fuels before geology did. This perspective meshed with his advocacy of nuclear power, which he believed was the only energy source that could enable better living conditions for all humanity. In this respect he held common ground with Nierenberg, who like most nuclear physicists supported

123. Ibid., 53.
124. Schelling et al. to Handler, "Ad hoc Study Panel" (ref. 52), 6.

the expansion of civilian nuclear power. But Weinberg critiqued Nierenberg's report with barely suppressed rage.

A basic principle of Academy policy is that the conclusions of any report must be consistent with the evidence presented, and the summary must provide an accurate reflection of the report as a whole.[125] Alvin Weinberg immediately noted that this report failed to pass that bar. But he went much further. The report was "so seriously flawed in its underlying analysis and in its conclusions," he wrote, that he hardly knew where to begin. It flew in the face of virtually every other scientific analysis of the issue, yet presented almost no evidence to support its radically divergent conclusions.

"[T]he whole report conveys an impression of 'let's cool it'—the CO_2 issue is very unlikely to be a show-stopper; at most it will be dealt with . . . through many small decisions taken by tillers of the soil and keepers of the irrigation system."[126] These conclusions were at variance with almost every comparable study, Weinberg noted, yet the authors had presented virtually no evidence to support them. They had simply *asserted* that markets would provide technological solutions, without much pain or dislocation.

The committee's conclusions "may be correct," Weinberg allowed, but he doubted it, in part because the report contained its own refutations. "[T]here are quite plausible contingencies, some delineated in the report itself, which, if taken seriously, would vitiate the main conclusions of the report." The most important of these was whether discussing a mean value of 3°C warming for doubling of CO_2 gave an adequate impression of the risks. Talking means was just a convenience—one had to consider upper bounds, too. Charney had placed the likely upper boundary at 4.5°C, a figure that had been reaffirmed by the Smagorinsky committee.[127] This was half again as much as the mean and was based solely on CO_2. When other greenhouse gases such as methane were added to the equation, model results produced values as high as 6°C.

"Do the committee members really believe that adaptation is that painless at $\Delta T = 6°C$?" Weinberg demanded.[128] The report noted this possibility, yet

125. Larry Armi, personal communication, 8 and 17 Oct 2007. By his own account, Armi, a member of the Smagorinsky committee, was "too junior" to speak up over the contradictions between the Nierenberg and Smagorinsky reports.

126. Alvin Weinberg, Comments on NRC draft *Report of the Carbon Dioxide Assessment Committee*, SIO WAN, MC 13, Box 86, Folder BASC/CO_2, Jul–Aug 1983.

127. Smagorinsky et al., *Carbon Dioxide* (ref. 63), 7.

128. Weinberg, Comments on NRC draft (ref. 126), 2.

then ignored it.[129] He compared the potential for catastrophe to one he had many times pondered: all-out nuclear war. "About this we are properly worried; about a much-aggravated CO_2 problem which has perhaps the same probability, we are preaching complacency."[130]

Weinberg also took issue with the proposed remedies: improvements in irrigated agriculture, and migration. Agricultural improvements would no doubt occur, but could they be put in place fast enough and on a sufficient scale, particularly in poor countries? The report provided no evidence. As for migration, Weinberg wrote, "does the Committee really believe that the United States or Western Europe or Canada would accept the huge influx of refugees from poor countries that have suffered a drastic shift in rainfall pattern? I can't for the life of me see how historic migrations, which generally have taken place when political boundaries were far more permeable than they are now, can tell us anything about migrations seventy-five to one hundred years from now when large areas lose their capacity to support people. Surely there will be times of trouble then." It was one thing for Ice Age Neanderthals to freely migrate in unpopulated Europe, quite another for tens of millions of Bangladeshis to clamor for admission to England. And it was another kind of problem altogether if the American breadbasket migrated into Canada. Schelling had argued that the world today was a very different place than it had been seventy-five years ago, yet Nierenberg was using migrations that had happened 120,000 years ago to reassure us about potential future ones. This was inconsistent at best. In any case, the report provided no analysis of the migration demands that might develop or the capabilities that might be needed.

Irrigation was the other "panacea," crucial to the recasting of the issue not as a CO_2 problem, but as a water problem. How realistic was this? "If . . . the upper range of possible outcomes prevails, the required irrigation becomes prodigious. I am not at all reassured that adaptation is so easy," Weinberg wrote. The required water projects would be enormous, and extremely expensive, something that perhaps the U.S. could handle, but that poor countries clearly could not. "[F]or an official Academy report to imply that these are easily handled, business-as-usual, developments simply makes no sense to me, and certainly is not supported by the analysis in the report."[131] Carbon taxes were dismissed

129. Authors' note: 6°C would deglaciate the entire Earth, leaving an equilibrium sea level about 200 meters higher than at present. By this standard, the 6 m that was preoccupying Roger Revelle pales into insignificance.

130. Weinberg, Comments on NRC draft (ref. 126), 2.

131. Ibid., 3.

as too difficult to implement, so why were stupendous water projects considered eminently reasonable?

Weinberg concluded that the whole report was disingenuous, because it recommended a great deal more scientific work, but if warming were inevitable and adaptation the only reasonable response, then why bother with further scientific work? "Why devote so much attention to understanding and measuring if one really believes adaptation is inevitable? All the effort should go into figuring out how to adapt, even putting measures for adaptation into place."[132] Yet there was no recommendation to that effect.

The U.S National Academy of Sciences, like most scientific societies and journals, requires authors of papers and reports to respond to peer review comments. Yet there is no evidence, either in the archival record or in the report itself, that the Nierenberg report was altered in response to Weinberg's complaints. How was it possible for Weinberg's review to be ignored? One senior scientist recently answered this way: "Academy review was much more lax in those days."[133]

CONCLUSION

Some scientists involved in the 1983 report have suggested that it was the best that could be done at the time in light of the very real scientific uncertainties and difficult political context. Others have suggested that it was indeed good for science, as the U.S. government, including the Reagan administration, became convinced of the need for serious, sustained study, both of the physical and social scientific aspects of global change. No doubt this is true. Our focus here, however, is not on the general contextual issues surrounding the report, but on the specific question of the construction and deconstruction of scientific knowledge claims. William Nierenberg's 1983 report did not construct scientific knowledge in any sense that either scientists or scholars of science would readily recognize. Rather, it reframed existing scientific work so as to cast its results as socially and politically inconsequential, at least anytime in the foreseeable future. The message on global warming, as Alvin Weinberg said, was "cool it."

132. Ibid., 4.

133. Edward Frieman, personal communication, 16 Mar 2007, UCSD Faculty Club, La Jolla, CA.

Making slightly strange bedfellows with skeptical critics of climate science, many science studies scholars have focused attention on the way in which the threat of climate change helped scientists justify more money for their research.[134] Yet the Nierenberg report did not primarily do that. Although it recommended more scientific research in a general way (particularly monitoring), it did not identify gaps in climate science and recommend research activities to fill them. Nor did Nierenberg attempt to deny the legitimacy of the existing science. Rather, he accepted the scientific facts while adopting a conceptual framework in which those facts were irrelevant. The essence of the report is the reframing of climate change as something that policymakers and politicians should ignore, which in the United States at least, for the next two decades, they largely did.[135]

Our analysis thus complements the existing science studies literature on climate science, much of which has focused on the heterogeneous social construction of the scientific claims involved. Sheila Jasanoff and Brian Wynne, for example, have attended to the complex relationships between lay and expert understandings of climate science and their unfolding in policy realms, as well as to the blurriness of the boundaries between them.[136] Craig Trumbo, Allan Mazur and Jinling Lee, and Moti Nissani have in various ways called attention to mass media coverage, particularly how scientific claims may be framed and reframed as social problems.[137] David Hart and David Victor have examined the role of elite oceanographers and atmospheric scientists in the construction of

134. For example, Hart and Victor, "Scientific Elites" (ref. 10); Weart, *Discovery* (ref. 8).

135. This recommendation arguably became a self-fulfilling prophecy, as we are now "committed" to 2°C of temperature increase based on our activities to date, and scholars are virtually all in agreement that some degree of adaptation will be unavoidable. Indeed, arguably the citizens of Tuvalu, and even perhaps New Orleans, have already been forced to adapt.

136. Sheila Jasanoff and Brian Wynne, "Science and Decision Making," in *Human Choice and Climate Change*, ed. S. Rayner and E. L. Malone, vol. 1 of *The Societal Framework* (Columbus, OH: Battelle Press, 1998); see also Sheila Jasanoff, *The Fifth Branch: Science Advisors as Policy Makers* (Cambridge, MA: Harvard University Press, 1990); and Sheila Jasanoff, *Science at the Bar: Law, Science, and Technology in America* (Cambridge, MA: Harvard University Press, 1995).

137. Craig Trumbo, "Constructing Climate Change: Claims and Frames in U.S. News Coverage of an Environmental Issue," *Public Understanding of Science* 5 (1996): 269–83; Allan Mazur and Jinling Lee, "Sounding the Global Alarm: Environmental Issues in the U.S. National News," *Social Studies of Science* 23 (1993): 681–720; Allan Mazur, "Global Environmental Change in the News," *International Sociology* 13 (1998): 457–72; Moti Nissani, "Media Coverage of the Greenhouse Effect," *Population and Environment: A Journal of Interdisciplinary Studies* 21:1 (1999): 27–43; Sheldon Ungar, "The Rise and (Relative) Decline of Global Warming as a Social Problem," *Sociological Quarterly* 33 (1992): 483–501.

the discipline of climate science since the International Geophysical Year, stressing the ways in which these scientists both built pioneering research programs and also redirected a large group of non-elite scientists to the task of monitoring, which in turn made it possible for policy-relevant claims to be made in the 1970s.[138] Jeroen P. Van der Sluijs and his colleagues have considered how climate scientists achieved closure of visible disputes about climate sensitivity to CO_2 and other greenhouse gases.[139] And Dale Jamieson has focused on scientists' failed attempts to manage and control uncertainty in the face of the diverse social dimensions, failures that challenge the presumptions that uncertainty is a purely epistemological problem.[140]

All of these approaches, disparate as they may be, underscore how the permeable boundary between science and politics warrants a reflexive attitude toward science as a socially situated practice of knowledge production, a point with which we fully concur. What we wish to do here is add an additional dimension for consideration. Many of these studies seem to assume, at least to some degree, that scientists are more or less unified in the primary goal of knowledge *production*.

The actions of William Nierenberg belie that assumption. Nierenberg did not engage his scientific colleagues over the technical basis of their scientific views. He did not produce new or competing claims about how the Earth would respond to increased CO_2. In short, he did not try to construct knowledge about the Earth. Rather, while accepting his colleagues' technical conclusions, he dismissed the interferences that they (and others) had drawn from those conclusions, substituting an alternative framework that insisted that those inferences were wrong. Rather than constructing knowledge, William Nierenberg de-constructed it.

We thus find ourselves in concord with the recent work of Myanna Lahsen, who has written in detail about politically conservative scientists, including Nierenberg, who in various ways challenged climate science throughout the

138. Hart and Victor, "Scientific Elites" (ref. 10), 643–80.

139. Jeroen P. van der Sluijs and Josée C.M. van Eijndhoven, "Closure of Disputes in Assessments of Climate Change in the Netherlands," *Environmental Management* 22 (1998): 597–609; Jeroen P. van der Sluijs et al., "Anchoring Devices in Science for Policy: The Case of Consensus around Climate Sensitivity," *Social Studies of Science* 29 (2004): 291–323.

140. Dale Jamieson, "Scientific Uncertainty and the Political Process," *Annals of the American Academy of Political and Social Science* 545 (1996): 35–43, on 39; see also Simon Shackley and Brian Wynne, "Representing Uncertainty in Global Climate Change Science and Policy: Boundary-Ordering Devices and Authority," *Science, Technology, and Human Values* 21 (1996): 276–80.

1990s, and of Aaron McCright and Riley Dunlap, who have noted that in response to scientific efforts to establish climate change as a "problem," a counter movement of "non-problematicity" arose.[141] Particularly from 1990 onward, conservatives outside of the scientific community used this approach to oppose U.S. participation in the U.N. Framework Convention on Climate Change and delay regulatory action. Journalists have also written on this movement, but few have paid much attention to the central role that *scientists* played, a role that we suggest here, and argue more fully elsewhere, can be traced back to William Nierenberg.[142]

Focusing on Nierenberg's reframing of climate change as a "non-problem" brings to light key elements of the history of the climate change controversy that are not revealed by a strict focus on knowledge production—particularly the use of scientific and political authority to reinterpret problematic knowledge constructions. Nierenberg's position as a distinguished scientist—a prominent physicist, a member of the Academy, and a director of a leading oceanographic institution—was crucial to his capacity to reframe the question in the way he did. His prominence in the Academy and membership on the Climate Research Board enabled him to position himself as a logical leader on the issue. And his broad academic connections placed him in contact with the economic perspectives that informed his reframing, while his detailed knowledge of climate science helped to enable his strategy and render it plausible to both scientific colleagues, and others.[143]

141. Aaron M. McCright and Riley E. Dunlap, "Challenging Global Warming as a Social Problem: An Analysis of the Conservative Movement's Counter-Claims," *Social Problems* 47 (2000): 499–522, on 501. A counter-movement is defined by McCright and Dunlap as a movement that calls for *inaction*, seeking to preserve current institutions, values, etc. See also Lahsen, "Climate Rhetoric" (ref. 6), 358–69, 372–73, 379; Myanna H. Lahsen, "Technocracy, Democracy and U.S. Climate Science Politics: The Need for Demarcations," *Science, Technology, and Human Values*, 30 (2005): 137–69; Myanna H. Lahsen, "The Detection and Attribution of Conspiracies: The Controversy Over Chapter 8," in *Paranoia Within Reason: A Casebook on Conspiracy as Explanation*, ed. George E. Marcus (Chicago: University of Chicago Press, 1999); Myanna H. Lahsen, "Seductive Simulations: Uncertainty Distribution Around Climate Models," *Social Studies of Science* 35 (2005): 895–922.

142. Oreskes and Conway, "Challenging Knowledge" (ref. 1). See also Ross Gelbspan, *The Heat is On: The Climate Crisis, the Cover Up* (New York: Perseus Books, 1997, 1998); Ross Gelbspan, *Boiling Point: How Politicians, Big Oil and Coal, Journalists and Activists are Fueling the Climate Crisis—And What We Can Do to Avert Disaster* (New York: Basic Books, 2004). Gelbspan tends to view the scientific community as largely blameless, and to lay all the troubles at the feet of the fossil fuel industry.

143. Ibid. Elsewhere we discuss how his political connections, forged in the Cold War, were also key to his ability to make his position influential. Oreskes and Conway, *Challenging Knowledge* (ref. 1).

Historians and sociologists have focused much attention on the stabilization of certified knowledge, and thus, in turn, have been moved to consider the various strategies through which agonists may attempt to block that stabilization. But William Nierenberg did not attempt to block consensus on particular scientific claims about climate. Instead, he found a way to challenge the inferences that colleagues were drawing from those claims, and to reframe the existing knowledge as "not a problem."

And this was just the beginning of Nierenberg's involvement in the climate change controversy. The following year, Nierenberg would become a co-founder of the George C. Marshall Institute, which in the late 1980s and throughout the 1990s was a major source of claims that climate scientists were in error: that global warming was not actually occurring, or if it was it was simply natural variability. In the early episode of reframing discussed in this paper, William Nierenberg denied that global warming would be a problem; in later years, he would deny that it was happening at all.[144] Reframing would prove to be a first step in what was to become a long-term project in the de-construction of politically inconvenient scientific knowledge of anthropogenic climate change.

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144. Lahsen, "Climate Rhetoric" (ref. 6); Lahsen, "Technocracy, Democracy and U.S. Climate Science Politics (ref. 141); Lahsen, "Detection and Attribution of Conspiracies" (ref. 141); Lahsen, "Seductive Simulations (ref. 141); Lahsen, "Experiences of Modernity (ref. 1); also Oreskes and Conway, "Challenging Knowledge" (ref. 1).