NICOLAS NIERENBERG, WALTER R. TSCHINKEL, AND VICTORIA J. TSCHINKEL*

Early Climate Change Consensus at the National Academy: The Origins and Making of *Changing Climate*

ABSTRACT

The 1983 National Academy of Sciences report entitled *Changing Climate,* authored by a committee of physical and social scientists chaired by William Nierenberg, was an early comprehensive review of the effects of human-caused increases in the levels of atmospheric CO_2. Study of the events surrounding the committee's creation, deliberations, and subsequent report demonstrates that the conclusions of the report were the consensus of the entire committee and in line with the scientific consensus of the time. This result contraverts a 2008 paper in which Naomi Oreskes, Erik M. Conway, and Matthew Shindell asserted that the report contradicted a growing consensus about climate change, and that Nierenberg for political reasons deliberately altered the summary and conclusions of the report in a way that played down the concerns of the other physical scientists on the committee. Examining the production of the report and contextualizing it in contemporaneous scientific and political discussion, we instead show how it was a multi-year effort with work divided among the various members of the committee according to their expertise. The synthesis and conclusions were expressly a joint statement of the committee and were consistent

*Nicolas Nierenberg, Nierenberg Foundation, 9494 La Jolla Farms Rd., La Jolla, CA 92037; nicolasnierenberg@gmail.com. Walter R. Tschinkel, Department of Biological Science, Florida State University, Tallahassee, FL 32306-4370; tschinkel@bio.fsu.edu. Victoria J. Tschinkel, 1561 Marion Ave., Tallahassee, FL 32314-5948; vtschinkel@vtschinkel.com.

The following abbreviations are used: CDAC, Carbon Dioxide Assessment Committee; CRB, Climate Research Board; DOE, Department of Energy; EPA, Environmental Protection Agency; NAS, National Academy of Sciences; NCAR, National Center for Atmospheric Research; NRC, National Research Council; OCS, 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," *HSNS* 38, no. 1 (2008): 109–52; OSTP, Office of Science and Technology Policy; Scripps, Scripps Institution of Oceanography; WAIS, West Antarctic Ice Shelf; WAN, papers of William A. Nierenberg, MC13, Scripps Institution of Oceanography Archives, La Jolla, CA.

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with other assessments of that time expressing deep concern over the potential issues while stopping short of recommending major policy changes due to the uncertainties, and to a lack of good alternatives.

KEY WORDS: global warming, climate, controversy, debate, National Academy

The late 1970s and early 1980s were a transitional time for scientific research into the effects of CO_2 on climate. Before the late '70s substantial research had been conducted from a purely scientific perspective, largely independent of politics. In 1978 the Carter administration's desire to use domestic coal to solve the energy crisis brought the CO_2 issue into the political arena for the first time. It also increased attention from various scientific groups resulting in the publication of several influential papers. As a result this period has become the focus of some attention by historians.[1]

Two reports could be viewed as marking the beginning of this era. The first was written by a group of physicists from the JASON defense advisory panel (a select group of scientists who met annually, usually to work on defense-related issues, named for the hero of Greek myth), with Gordon MacDonald as the lead author.[2] The second was an ad hoc National Academy of Sciences (NAS) report authored by Jule G. Charney et al.[3] Of the two, the Charney study is probably the better known. Many references to the period describe one or both of these as having called for policy action, potentially setting them apart from previous work in the field.[4]

Largely as a result of these reports, the Energy Security Act of 1980 called for a comprehensive NAS study of the issue. Accordingly, the NAS formed the Carbon Dioxide Assessment Committee (CDAC), which in 1983 produced

1. M. Oppenheimer and A. Petsonk, "Article 2 of the UNFCCC: Historical Origins, Recent Interpretations," *Climatic Change* 73 (2005): 195–226, on 196–97.

2. G. F. MacDonald, H. Abarbanel, P. Carruthers, J. Chamberlain, H. Foley, W. Munk, W. Nierenberg, O. Rothaus, M. Ruderman, J. Vesecky, and F. Zachariasen, *The Long Term Impact of Atmospheric Carbon Dioxide on Climate,* JASON Technical Report, JSR-78-07, 1979.

3. Jule G. Charney, Akio Arakawa, D. James Baker, Bert Bolin, Robert E. Dickinson, Richard M. Goody, Cecil E. Leith, Henry M. Stommel, and Carl I. Wunsch, *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).

4. See also Rafe Pomerance, "The Dangers from Climate Warming: A Public Awakening," *EPA Journal* 16 (1986): 15–16; and Thomas C. Peterson, William M. Connolley, and John Fleck, "The Myth of the 1970's Global Cooling Scientific Consensus," *Bulletin of the American Meteorological Society* 89 (2008): 1325–37.

a report called *Changing Climate.* It provided a thorough assessment of the problem but did not recommend immediate changes to the U.S. energy program.

In a 2008 paper, Naomi Oreskes, Erik M. Conway and Matthew Shindell (OCS) reexamined this period.[5] In that paper, they concluded that in 1980 a consensus was emerging in the scientific community for action to limit the increase of CO_2 in the atmosphere. They argue that, in a process they call "deconstruction," William Nierenberg, as the chairman of the CDAC, deliberately altered the summary and conclusions of the report in a way that played down the concerns of the other physical scientists on the committee, while emphasizing the views of the economists. He did so, they claim, because he felt that CO_2 was not a significant problem and that humans would easily adapt to future climate changes. They also imply that political considerations and influences played a role in his actions.

We find that the evidence does not support their conclusion that, under the influence of the committee's chairman, some of the most distinguished scientists of the time signed their names to a report with which they did not agree. In addition the evidence of an emerging consensus for policy action in 1980 is weak. It is certainly not well supported by either the JASON or Charney report. Although an EPA report issued around the same time made projections more alarming than those of *Changing Climate,* it likewise concluded that no short-term changes to U.S. energy policy were possible. *Changing Climate* was almost certainly the most comprehensive look at the climate change issue at the date of its publication. Its conclusions were the consensus view of the committee; in no way did it contradict an emerging scientific consensus, and it contributed to the core scientific and policy views of the period.

ACCELERATED SCIENTIFIC CONCERN OVER CO_2

In the late 1970s the U.S. economy was struggling, and energy policy was a key issue. The country had suffered from an oil embargo earlier that decade, and concern was great that future economic growth was too dependent on an unreliable stream of oil from the Middle East.[6] Nuclear power, which had been

5. OCS.

6. See "Nation: Ugly Mood Developing on the Hill," *Time Magazine*, 17 Sep 1979; http://www.time.com/time/magazine/article/0,9171,920644,00.html (last accessed 20 Jan 2010).

seen as a solution by many, had suffered serious setbacks as a result of general environmental concerns as well as fear of a nuclear disaster. In 1979 the Three Mile Island accident pushed nuclear power to the bottom of national energy priorities.[7]

President Carter's solution for the energy crisis depended "on conservation, on the direct use of coal, on development of solar power and synthetic fuels, and enhanced production of American oil and natural gas."[8] In July 1979 Carter proposed an $88 billion plan to produce synthetic liquid fuel from coal and shale-oil reserves.[9] As a result the Energy Security Act, that William Proxmire had introduced in April of 1979 was amended to include the Synthetic Fuels Corporation Act of 1979.

Although the idea of synthetic fuel as a solution to the energy crisis had been around since the Nixon administration, these actions by the White House and Congress took it well out of the concept stage, and scientists were worried. Concern over atmospheric CO_2 had been growing through the 1970s, and many felt that a move to use the country's vast reserves of coal and shale oil would greatly increase the threat.

Gordon MacDonald, a geophysicist who had been working on climate-related issues for much of his career, wrote Phillip Handler, president of the NAS:

> The Congress through a variety of laws has mandated the Executive to examine long-term issues such as carbon dioxide in developing its energy programs . . . the Executive has failed to consider adequately this problem. I would hope that before we as a nation commit very large expenditures on the subsidy of synthetic fuels, we attempt to understand the longer term consequence of such a commitment.[10]

George Woodwell, director of the Ecosystems Center at the Marine Biological Laboratory in Woods Hole as well as a founder and member of several

7. Jimmy Carter, "President's Commission on the Accident at Three Mile Island Remarks Announcing Actions in Response to the Commission's Report," 7 Dec 1979, the American Presidency Project, http://www.presidency.ucsb.edu/ws/?pid=31788 (last accessed 30 Mar 2010); Roger E. Kasperson, Gerald Berk, David Pijawka, Alan B. Sharaf, and James Wood, "Public Opposition to Nuclear Energy: Retrospect and Prospect," *Science, Technology, and Human Values* 5, no. 31 (1980): 11–23; http://www.jstor.org/stable/689009 (last accessed 24 Nov 2009).

8. Ibid.

9. See Robert T. Grieves and Jay Branegan, "Portrait of a Federal Fiasco," *Time Magazine*, 14 May 1984; http://www.time.com/time/magazine/article/0,9171,955274,00.html (last accessed 20 Jan 2010).

10. Gordon MacDonald to Phillip Handler, 7 Aug 1979, WAN, Box 88, Folder 5.

environmental organizations, wrote to Ruth Clusen, the Assistant Secretary for the Environment: "You have suggested that a 1–2 million barrel per day program of synthetic fuels development would add about 1% more carbon dioxide than all other global sources in 1990. . . . You have suggested that this increment is insignificant . . . I would suggest that there is very little basis in an analysis of that problem for supporting a rationale that allows any further aggravation of that problem."[11]

Of course scientists had known about potential problems from burning fossil fuels for a long time. Theories about the possible effects of carbon dioxide on climate dated back to the latter part of the 19th century. Excellent summaries of this history have appeared in many places, including Annex 1 of *Changing Climate*. By the mid-1970s these principles were well understood, but models were still subject to great uncertainties as reflected in the studies authored during the period. For example, a major NAS study on the issue, chaired by Roger Revelle in 1977, had come to the conclusion that the threat of a corresponding increase in world temperatures of over 6°C would lead to a curtailment in the growth of fossil fuel use long before the scenario could fully unfold. Specific recommendations were made for research to reduce uncertainties and fill gaps in scientific knowledge regarding the physical and biological processes involved.[12]

In the late 1970s the JASON and Charney reports were two of the most influential on the topic of CO_2. In the spring of 1977 a group from JASON, to start, convened a two-day meeting in Boulder, Colorado, at the National Center for Atmospheric Research (NCAR).[13] Several members had decided that they were interested in climate-related topics, so a group led by MacDonald and Henry Abarbanel began doing research in this area. Regular members of the group included William Nierenberg, Ed Frieman, and Freeman Dyson.

11. Founder of the Environmental Defense Fund, the Natural Resources Defense Council, and the World Resources Institute, as well as the Chairman of the World Wildlife Fund, National Research Council (U.S.); W. Nierenberg et al., *Changing Climate: Report of the Carbon Dioxide Assessment Committee*, Board on Atmospheric Sciences and Climate, Commission on Physical Sciences, Mathematics, and Resources, National Research Council (Washington, DC: National Academy Press, 1983), 495–96; G. M. Woodwell to Ruth Clausen, 23 Oct 1979, WAN, Box 88, Folder 7.

12. Roger Revelle and Walter H. Munk, *Energy and Climate* (Washington, DC: National Academy of Sciences, 1977).

13. MacDonald et al., *Long Term Impact* (ref. 2).

This first climate study had no sponsor and arose out of the group's general interest. Unlike much of the other work produced by JASON, the climate reports were not classified.[14]

The primary conclusions of the report were that a doubling of CO_2 would produce a 2–3°C increase in global temperature. Temperatures at the poles were predicted to increase by 10°C, whereas those at the equator would only increase by 0.7°C. Although the scientific conclusions of the JASON report were dramatic, the JASON committee was not ready to make policy recommendations. They concluded, in a summary typical of papers from the period, that:

> There are numerous uncertainties about the direction and magnitude of anticipated changes. The benefits and costs of these changes to society will depend on the timing and magnitude of the changes and the appropriateness of human responses. Significant uncertainties exist . . . The uncertainties are great enough to suggest that now is not the proper moment to undertake far-reaching actions designed to mitigate potential effects of increasing CO_2.[15]

When increasing CO_2 was introduced into the models, the result was significant warming, but how well did the models reflect reality? Of course all models are simplifications of the actual planet. In the case of the JASON group, they were even simpler than those that had typically been built by climate specialists. They were therefore easier to build and understand, but by simplifying the problem, the investigators might have left out some important factors.[16] Although they felt their results were reasonable, they were certainly not conclusive. Mainstream climate scientists from outside of JASON were certainly not convinced by the group's "little physics project." They felt that climate was their territory and that the JASON model was, in fact, too simple.[17]

In addition to the scientific uncertainty, how harmful would warming really be? In 2010 the conventional wisdom is that such changes have virtually universally negative consequences, but in 1979 opinions among scientists differed. The final paragraph of the abstract from the JASON report demonstrates the

14. Ann Finkbeiner, *The Jasons: The Secret History of Science's Postwar Elite* (New York: Penguin, 2006), 134.

15. MacDonald et al., *Long Term Impact* (ref. 2).

16. Ibid., iii.

17. Finkbeiner, *Jasons* (ref. 14), 136.

far different mindset of that time, with a reference to the idea that warming might generally be a good thing, although the outcome was unpredictable.

> The warming of the climate will not necessarily lead to improved living conditions everywhere. Changes in sea level, in agricultural productivity, and in water availability can be anticipated, but the dimensions of their economic, political or social consequences can not.[18]

In 1979 MacDonald wrote an article for the *Washington Post* arguing that subsidizing synthetic fuels, as proposed by the Carter administration, would be a mistake.[19] He pointed out that synthetic fuels would produce even more CO_2 than the current U.S. mix of fossil fuels. The article drew the attention of U.S. Senator Abraham Ribicoff (D-CT), who had recently been warned about the issue by West German Chancellor Helmut Schmidt. Ribicoff made a request to Phillip Handler for further scientific guidance on the issue.[20] Because of the perceived urgency, National Research Council (NRC) Chairman R. M. White contacted Charney directly. Charney, a professor of meteorology at MIT and a member of the NAS Climate Research Board (CRB), arranged an ad hoc study at the end of the CRB summer meeting.

Because of the limited time available, Charney decided to address one specific question. How well could the climatic consequences of increasing atmospheric carbon dioxide be estimated. In a now famous statement, the committee concluded that the atmosphere would warm significantly but that the oceans might delay the change, and in any event the social consequences were outside their expertise.

> To summarize, we have tried but have been unable to find any overlooked or underestimated physical effects that could reduce the currently estimated global warmings [sic] due to a doubling of atmospheric CO_2 to negligible proportions or reverse them altogether. However, we believe it quite possible that the capacity of the intermediate waters of the oceans to absorb heat could delay the estimated warming by several decades. It appears that the warming will eventually occur, and the associated regional climatic changes so important to the assessment

18. MacDonald et al., *Long Term Impact* (ref. 2), iii. OCS, without citing evidence, attribute a portion of this quote to William Nierenberg (OCS, 117). It seems more likely, given that this is the concluding paragraph of the abstract, that it was written by the first author, Gordon MacDonald.

19. Pomerance, "Dangers from Climate Warming" (ref. 4), 16.

20. Ibid.

of socioeconomic consequences may well be significant, but unfortunately the latter cannot yet be adequately projected.[21]

THE GROUNDWORK FOR *CHANGING CLIMATE*

Some have found a call to action in the introduction to the report by Verner Suomi, chairman of the CRB. He pointed out that the group had no reason to doubt that changes were coming and that the results would "not be negligible." In addition a "wait-and-see policy might mean waiting until it was too late."[22] Although these remarks clearly express concern, they fall far short of proposing even general policy actions. Expressions of concern were certainly nothing new in studies of the CO_2 issue.

At a CRB meeting on October 18, chaired by Suomi, the board expressed dissatisfaction that, despite Handler's disclaimers on the letter of transmittal accompanying the report, the Charney study had been viewed as a formal statement of position by the CRB.[23] Although they appreciated the effort that the study group had made in such a short time, they were dissatisfied with the "hurried and irregular procedures employed in its organization. . . . *It was agreed* that future efforts . . . would be organized in a more orderly fashion and with fuller participation by the Board members and by other concerned National Research Council (NRC) groups."[24] Uneasiness with the results of the Charney report was not limited to the CRB. Many climate scientists felt that the models were incomplete and that their ability to model the real world was uncertain.[25]

Meanwhile the White House Office of Science and Technology Policy (OSTP) had asked the NRC for a follow-on study. They noted that they had not yet had a chance to examine the Charney report, but they already knew they wanted more information. In particular, how far off was the problem?[26]

The Carter White House was clearly concerned that the CO_2 issue could stall the energy legislation that was key to its economic strategy. Synthetic fuel

21. Charney et al., *Carbon Dioxide and Climate* (ref. 3), 3.

22. Ibid., vii, viii.

23. Draft Record of Actions, Fifth session of the CRB, 12 Nov 1979, WAN, Box 89, Folder 7.

24. Ibid.

25. Spencer R. Weart, *The Discovery of Global Warming* (rev. ed., Cambridge, MA: Harvard University Press, 2008), 100.

26. Richard Meserve to Verner Suomi, 5 Oct 1979, WAN, Box 89, Folder 8.

was an important component of that legislation, and now some scientists were saying that it could be a bad idea because of its long-term effect on the atmosphere. The following April, Frank Press, the head of the OSTP, reacted angrily to a draft of a Council on Environmental Quality report that he felt greatly overemphasized the dangers and underplayed the uncertainty. "At this moment of great national trauma with respect to energy, inflation, and foreign affairs, I believe it is a serious disservice to the public to raise widespread concern about an issue with hazards, that are, at the moment, so speculative and uncertain."[27] Even though Press received this draft in April of 1980, the Council on Environmental Quality report was subsequently revised and was not published until January of 1981, well after the passage of the Energy Security Act.

The Environmental Protection Agency (EPA) did not yet view CO_2 as a major issue. In an article published in 1980 titled "Coal and Ecology," Barbara Blum, the EPA Deputy Administrator, discussed the importance of coal to the energy future of the country. "The policy of the Carter Administration is to burn three times more coal by the year 1995." She then discussed the need to do so as cleanly as oil was burned. Although the article mentioned issues like water quality and acid rain, it never mentioned CO_2.[28]

Although in 1979 and 1980 a scientific consensus was emerging that increasing CO_2 would probably cause the atmosphere to grow significantly warmer, no consensus had formed about the level of uncertainty, the seriousness of the problem, or what if anything should be done about it. The JASON and Charney reports, although influential, specifically refrained from commenting on the potential economic and social impacts of climate change, and neither called for any type of policy response. Individuals such as MacDonald and Woodwell were communicating their specific concerns about the potential harm of a synthetic fuels program, but among the larger group at the NAS, and within the Carter administration, the feeling was still that the uncertainties were very large and the social impacts unknown.[29] These conclusions agree with historians' accounts of this period. In *The Discovery of Global Warming*, Spencer Weart concludes, "Nobody of consequence proposed to regulate CO_2 emissions or make any other significant policy changes to deal directly with greenhouse gases. Academy reports and other scientific pronouncements advised that any such action would be premature given the lack of scientific consensus."[30]

27. Frank Press to Gus Speth, 14 Apr 1980, WAN, Box 89, Folder 9.
28. Barbara Blum, "Coal and Ecology," *EPA Journal* (Sep 1980), 4–5.
29. Our views contradict OCS on this issue; see OCS, 116.
30. Weart, *Discovery of Global Warming* (ref. 25), 103.

The CRB viewed the request from the OSTP as a way to implement the broader study of the CO_2 issue that they were looking for. At the same meeting of the CRB where the Charney report was discussed, Suomi, the chairman of the CRB, asked Nierenberg to head up an ad hoc panel focused on CO_2 that could respond to the OSTP and recommend a future plan of action.[31]

CHOOSING A COMMITTEE CHAIR

William Nierenberg received his Ph.D. in physics in 1947 from Columbia University, where he also participated in the Manhattan Project. From 1950 to 1965 he was a professor of physics at the University of California, Berkeley, where he produced over a hundred publications in theoretical and experimental areas related to low-energy nuclear physics. Nierenberg was elected to the National Academy of Sciences in 1971 as a result of this work. He was also a member of the American Physical Society, the National Academy of Engineering, and the National Academy of Arts and Sciences.

During a sabbatical in 1953 and 1954 he served as the first director of Columbia's Hudson Laboratories. His work there renewed his interest in geophysics and oceanography that had been interrupted by World War II. Between 1960 and 1962, he took leave from Berkeley to serve as Assistant Secretary General of the North Atlantic Treaty Organization (NATO) in charge of scientific affairs, where he oversaw many international studies on physics and advanced defense technologies.[32]

In 1965 Nierenberg was appointed director of the Scripps Institution of Oceanography, where he became its longest serving director and oversaw a five-fold increase in its budget. His predecessors, Harald U. Sverdrup and Roger Revelle, had been oceanographers, and his appointment was a reflection of the "growing breadth and national importance of oceanography."[33] A particular qualification was his background in geophysics and "a familiarity with prominent oceanographers like Roger Revelle, Freeman Gilbert Columbus Iselin, and Walter Munk."[34] Among many other achievements, Nierenberg was

31. Draft Record of Actions, 12 Nov 1979, WAN (ref. 23).

32. Deborah Day, William Aaron Nierenberg's Biography, 1 Jan 1997, UC San Diego Biographies, http://escholarship.org/uc/item/68k0253c (last accessed 20 Jan 2010).

33. Russ Davis and William Happer, "William Aaron Nierenberg," *Proceedings of the American Philosophical Society* 151 (2007): 264.

34. Ibid., 264.

credited with accelerating the climate program at Scripps and with steadfast support of Charles Keeling's ongoing difficulties in getting funding for his carbon-dioxide time series.[35]

Nierenberg joined JASON in 1962 (he eventually served as the group's chairman in the early 1980s). The 1977 report discussed in a previous section was JASON's first related to climate, and they continued to do research on this subject into the 1980s.[36] Nierenberg also served in numerous other leadership roles for government, advising on matters of earth and space sciences as well as science policy. These roles spanned both Democratic and Republican administrations and brought him into frequent contact with members of Congress.[37]

OCS provide two theories explaining Nierenberg's selection as chair of the CDAC committee. The first is that his role on the Reagan transition team made him a natural choice, and the second that his conservative politics would have influenced the CRB decision.[38] The first argument is clearly not applicable, given that Reagan's election took place a year after Nierenberg's selection to head the ad hoc committee of the CRB that led to the CDAC. Even at the time of Nierenberg's formal appointment as the head of the CDAC by the CRB, Reagan's election was two months in the future. His conservative politics also seem unlikely to have led to his appointment, because at that time Carter was the President and the Democratic Party controlled both houses of Congress. The NAS does tend to be scientifically conservative, but the OCS reference was to conservative politics, not science.

In summary, Nierenberg was a distinguished scientist who was currently the director of a major institution with a strong track record on climate. He had served two separate administrations including the current one in a science advisory capacity, and he was well known in Congress. He had served as

35. Ibid., 265. See also Charles D. Keeling, "Rewards and Penalties of Monitoring the Earth," *Annual Review of Energy and the Environment* 23 (1998): 25–82.

36. Finkbeiner, *Jasons* (ref. 14), 134–41. OCS do not credit Nierenberg with being a co-author on the JASON report other than the reference to his being responsible for the statement about living conditions being improved (see ref. 18).

37. Nierenberg was the first chairman of the National Advisory Committee on Oceans and Atmosphere from 1972 to 1977. He was a member of the National Science Board between 1972 and 1978. Nierenberg was on the advisory council of the National Aeronautics and Space Administration and served as its first chairman from 1978 to 1982. He served as a presidential advisor as part of the President's Science Advisory Committee for President Ford in 1975 and during 1976–1978 for President Carter as a member of the OSTP.

38. OCS, 122.

chairman of important advisory committees. He had worked on climate research directly as a member of JASON and had been a member of the NAS Climate Research Board for several years. He was an ideal choice to head a major project on climate for the NAS.

As we saw, at the time of Nierenberg's appointment, scientists were aware of the potential dangers from the buildup of CO_2 but generally felt that more research was called for. Nierenberg's views on the CO_2 issue appear to have been aligned with this worldview. He had been a coauthor of the JASON report led by Gordon MacDonald. He was a member of the CRB and had just been appointed to its international panel. As we have described, Verner Suomi, who had written the introduction to the Charney report, appointed him to head up the committee's efforts on CO_2. Our conclusion that Nierenberg was in the mainstream contradicts the position taken by OCS, who concluded that Nierenberg "equally clearly rejected his colleagues' emerging consensus about it [CO_2]."[39] As evidence of their claim, OCS provided the following quotation: "There were lots of 'man-induced perturbations' in the environment, he [Nierenberg] suggested, and CO_2 was 'not particularly different from others that have been dealt with'."[40] However, this quotation is used out of context, as it was taken from a memo which discussed the issue of climate *forecasting*, not with the consequences that increasing CO_2 could have for society.[41]

In a similar vein, OCS also refer to a 1978 letter from Nierenberg addressing the CRB's summer review of the National Climate Plan.[42] In their main text OCS incorrectly identify this letter as referring to the Charney report, although their footnote is correct. In context, Nierenberg's comment in that letter about adaptability had nothing to do with CO_2. Ultimately, the claim that Nierenberg felt that the CO_2 issue was not important because "humans were capable

39. OCS, 118.

40. Ibid.

41. The following is a more complete quotation. "A large number of brilliant people have worked on this problem [forecasting] for a long time. Other than some limited prediction capability for a few months, it can be said that there have been no real advances made. The effect of CO_2 changes is not particularly different from others that have been dealt with (this is implicitly recognized in the Comprehensive Plan)." William A. Nierenberg, "Draft, August 11, 1978, Review of the May 1978 Comprehensive Plan for CO_2 Effects Research and Assessment," WAN, Box 172, Folder 6.

42. OCS, 118. The National Climate Plan was established by the National Climate Act in 1978. It was intended to give focus to the government's overall climate programs. The CRB was providing input to this plan. See Weart, *Discovery of Global Warming* (ref. 25), 104–105.

of adapting to whatever changes ensued"[43] is simply not supported by the evidence presented by OCS and is contradicted by Nierenberg's public statements and by his preface to *Changing Climate*:

> There is a broad class of problems that have no "solution" in the sense of an agreed course of action that would be expected to make the problem go away. These problems can also be so important that they should not be avoided or ignored until the fog lifts. . . . Increasing atmospheric CO_2 and its climatic consequences constitute such a problem.[44]

THE CDAC AND ITS WORK

Late in 1979 Congress's wishes became clearer; it wanted more than just an assessment of the scientific issues involved with CO_2. A letter from Senator Ribicoff to Handler discussed the included language of proposed legislation in conjunction with action on synthetic fuels. "The study shall also assess the effects of various possible levels of atmospheric carbon dioxide accumulation on climate and the economic, physical, and social impacts of such climate changes."[45] In January 1980, perhaps in response to this proposed legislation and to get ahead of the situation, Frank Press wrote to Handler with a similar message. "There are broad areas of policy involving research priorities as affected by the potential CO_2 problem, the economic and political impacts of climatic change induced by CO_2 build-up. . . . Is there any interest in the Academy towards pursuing these broader questions?"[46]

The NAS had already been expecting to consider the scientific issues surrounding CO_2 much more broadly than had the relatively narrow JASON and Charney studies. Now they were being asked to look at the economic and social impacts. To do so, the NAS needed to ask social scientists to aid in the effort. The CRB asked Thomas Schelling, a Harvard economist, to head an ad hoc committee, including Revelle, Woodwell, Nierenberg, and Joseph Smagorinsky, a climate scientist from MIT, to address the economic and societal impacts of the projected CO_2 increase. The ad hoc committee published its results in the form of a letter in the spring of 1980, concluding:

43. OCS, 119.
44. W. Nierenberg et al., *Changing Climate* (ref. 11), xiii.
45. Senator Abraham Ribicoff to Phillip Handler, 30 Oct 1979, WAN, Box 89, Folder 8.
46. Frank Press, Director OSTP, to Phillip Handler, 2 Jan 1980, WAN, Box 89, Folder 9.

> To sum up, carbon dioxide will pose exceedingly difficult and divisive policy questions for all the world's nations individually and collectively. We do not know enough to address most of these questions right now. We believe we can learn faster than the problem can develop.[47]

In June 1980 the Energy Security Act was passed by Congress and signed into law by President Carter. Part of that legislation established the Synthetic Fuels Corporation. The goals were ambitious: "This bill establishes a corporation to encourage production of 2 million barrels a day of synthetic fuels by the year 1992, by converting coal to synthetic oil and gas and by extracting oil from shale and from tar sands and by other means."[48] Although other portions of the act provided funds for conservation and alternative energy, the synthetic-fuel provision would entail a major acceleration in the use of domestic fossil fuels.

To help satisfy Ribicoff and others who were concerned with the CO_2 issue, the legislation also required from the NAS, through the OSTP, "a comprehensive study of the projected impact on the level of carbon dioxide in the atmosphere, of fossil fuel combustion, coal-conversion, and related synthetic fuels activities authorized in this Act, and other sources. Such study should also include an assessment of the economic, physical, climatic and social effects of such impacts."[49] Because of the extraordinary scope of this request, the legislation gave the NAS three years to complete it. In the end this goal was nearly met despite a long delay in the allocation of the required funds.

Once the legislation had passed, Nierenberg, as head of the ad hoc CRB panel, arranged for a meeting with the Chairmen of Assemblies and Commissions of the National Research Council to discuss how they would approach this research. At that meeting, they agreed that the CRB "should be clearly in charge of designing and managing the project."[50]

At the next meeting of the CRB in September of 1980, the committee decided that a relatively small, focused group would be needed to respond to the mandate. This new group was the CDAC. They "would be assisted by a larger group of invited experts and consultants approved by the Board." Another decision was

47. Ad Hoc Study Panel on Economic and Social Aspects of Carbon Dioxide Increase, SIO Archives MC6A, Box 71, Folder 10.

48. Jimmy Carter, "Remarks on Signing 2.952 Into Law," http://www.presidency.ucsb.edu/ws/index.php?pid=44684&st=Energy+Security+Act&st1 (last accessed 20 Apr 2009).

49. W. Nierenberg et al., *Changing Climate* (ref. 11), annex 3.

50. Draft Record of Actions, Seventh session of the CRB, 26 Sep 1980, WAN, Box 88, Folder 14.

that preliminary funding should be sought and a more complete plan developed by the end of the year. In executive session, the CRB formally appointed Nierenberg to chair the CDAC, and agreed on a list of nominees. Several of the nominees had been members of the Schelling ad hoc committee.

The evidence contradicts the assertion by OCS that the choice to include economists on the committee was Nierenberg's. They argue, indeed, "it is a striking feature of the CO_2 assessment committee that its members included two economists." It was hardly striking as the directive to focus on the social impacts was from Congress, and Schelling, an economist, had just chaired a study of the issue for the NAS.[51]

In *The Discovery of Global Warming,* Spencer Weart described the CDAC as having produced its report in 1983 after a "sustained effort to work out a consensus," implying that the committee spent much of the three years trying to find a common view about the issue. [52] In fact, delays in funding held up the committee by almost a full year, while research and writing of the various scientific chapters, as well as time for peer review, took up much of the rest of the time. As discussed below, neither the minutes of the committee meetings nor other materials reflect any particular difficulty in forming a consensus view.

At the first meeting of the CDAC, in October of 1980, Richard Meserve assured the committee that "although the Office had not initiated the action leading to the legislative mandate, they supported it fully."[53] Despite this assurance, funding for the project was not forthcoming. The OSTP had provided an initial $250,000, but the funds required for the full study were not authorized during the remaining months of the Carter administration. This was likely a result of the administration's general indifference to the issue, combined with the confusion of the transition process. The committee met as a whole twice more in the fall of 1980 and formed a plan for the study, but most work was suspended at that point due to the lack of funding. The only part of the program that continued was an ad hoc committee headed by Smagorinsky. That group was following up on the Charney report and was conducting an "assessment of the climate modeling aspects of the problem."[54]

51. Quote from OCS, 123. What may be more interesting is that Nierenberg supported the participation of Woodwell on the committee because of his focus on the effects of land processes on the CO_2 issue, something that Nierenberg felt the marine geochemists underestimated.

52. Weart, *Discovery of Global Warming* (ref. 25), 141.

53. Draft Record of Actions, CRB Meeting, 4–5 Jun 1981, WAN, Box 89, Folder 5.

54. Ibid.

During the first half of 1981, Nierenberg and John Perry, the committee's executive secretary, wrote letters and lobbied both the OSTP and Congress to provide the needed funds. Meanwhile at the Department of Energy (DOE), David Slade, who was the chairman of the Interagency Committee for Carbon Dioxide and Climate, saw the study as part of a strategy to increase U.S. influence in the international CO_2 research effort. The World Meteorological Organization (WMO) had just issued an update after a meeting in Villach, Austria.[55] Slade felt that the work to date and the proposed work were unbalanced and overemphasized the carbon cycle over other aspects of the problem.[56]

On June 26, 1981, with support from both Congress and the DOE, the OSTP approved funding for the CDAC study. The form of the report was specified in the funding proposal. An overview would be followed by individual sections focused on each of the individual studies made by the committee.[57]

THE DELIBERATIONS OF THE CDAC

Because most of the committee members had just worked together on the Schelling report, they knew each other well. During the initial three meetings in 1980, before the almost year-long hiatus, much of the time was spent reviewing the previous NAS and JASON reports and discussing recent scientific updates. The rest was spent discussing what format the eventual report should take.[58]

OCS state that Nierenberg "repeatedly tried to bring forward suggestions" that warming was not a serious concern. Our review of all the available minutes and correspondence related to the CDAC does not support that statement. The evidence presented by OCS consists of a single discussion at "one early meeting" regarding a scientific theory proposed by the "chair of

55. The Villach meeting had typical results for the period. It projected potentially serious impacts, but "[b]ecause of uncertainties in present knowledge it is premature to develop a management plan to control CO_2 levels in the atmosphere." More research was needed.

56. David Slade to William Nierenberg, 2 Mar 1981, WAN, Box 89, Folder 3.

57. "Proposal for Support of the National Research Council's Comprehensive Carbon Dioxide Assessment," Jul 1981, WAN, Box 91, Folder 9.

58. Draft Record of Actions, First meeting of the CDAC, 14 Oct 1980, WAN, Box 89, Folder 11.

the Oceanography Department at Woods Hole" regarding ocean circulation.[59] In a further attempt to show that Nierenberg was pushing a particular view, OCS state: "But Nierenberg's principal tactic was to rely on the arguments provided by the two economists."[60] They provide no reference for this assertion, and we have found nothing reflecting it in any of the source materials.

Returning to the committee's progress, by the end of 1980 its members had decided on an outline. Some of the technical issues would be investigated directly by the committee, and others would be addressed by other NRC units. The only one of these technical reports actually initiated at that time was the report undertaken by the Smagorinsky subcommittee, which was reviewing and updating the Charney report.[61] When the CDAC resumed meeting in the fall of 1981, they further refined the report on the basis of the accepted proposal from the OSTP. Topics were assigned to the different members, and Nierenberg took on the task of formulating specific recommendations.[62] This organization reflected the way the committee viewed the issue. An integrated group could handle a single topic like climate sensitivity to an increase in CO_2, but the scope of the proposed CDAC report was very diverse, and the various topics needed to be assigned to specialists. One interesting result was that the entire issue of societal impacts was handed to Schelling as a committee of one, perhaps because most of the CDAC had worked together on this issue as part of Schelling's predecessor committee. The chapter eventually written by Schelling largely echoes that committee's conclusions.

59. OCS, 125. This discussion actually occurred at the second meeting of the CDAC on December 4, 1980. In fact Nierenberg called the theory "speculative" and said that most oceanographers did not agree with it. OCS attribute the statement "most oceanographers don't agree" to the rest of the committee as a kind of rejection of Nierenberg, but the minutes make clear that this was Nierenberg's opinion. Draft Record of Actions, Second meeting of the CDAC, 4–15 Dec 1980, WAN, Box 90, Folder 1.

60. OCS, 125.

61. Draft Record of Actions, Eighth session of the CRB, 13 Feb 1981, WAN, Box 89, Folder 2.

62. Draft Record of Actions, CDAC Meeting, 28–29 Sep 1981, WAN, Box 90 Folder 7, William Nordhaus and consultant, "Fossil Fuel Production/Use and Consequent CO_2 Inputs"; Leonard Machta, Woodwell, plus new member, "Airborne Concentrations of CO_2 and Other Trace Gases"; Smagorinsky, Climate Research Committee, "Effects of Increased CO_2 on Climate"; Paul Wagonner, "CO_2 Influences on Agriculture"; Revelle and ad hoc group of experts, "Environmental and Other Nonclimatic Implications (West Antarctic Ice Sheet, Arctic Pack Ice, Hydrology, etc.)"; Smagorinsky, Climate Research Council, and possibly new ad hoc panel, "Strategies for Monitoring and Possible Early Detection of CO_2 Effects"; and Schelling, "The Context of CO_2/Climate Change and Concurrent Demographic, Economic, and Societal Changes."

During 1981 Nierenberg, Revelle, and Perry also worked together to update the outline of the report. It would include an Executive Summary, which would integrate the scientific chapters into a highly quotable and compressed format, and "an analysis of the report in terms of the questions posed by Congress."[63] In this outline a separate recommendations chapter was no longer present, subsumed by the Executive Summary.[64]

In February and March 1982, Revelle then chaired two informal workshops at Scripps. The first considered the issues of sea-level rise and the West Antarctic Ice Shelf (WAIS), and the second covered the subject of hydrology. Experts from around the country participated in these two-day sessions. The results of these workshops formed much of the basis for the Revelle chapters in *Changing Climate.*[65]

At the sea-level-rise meeting, a large part of the discussion focused on how much the melting of the WAIS was likely to contribute to sea-level rise. Revelle opened the meeting by observing, "If it were to disintegrate over several hundred years, we could no doubt adapt. But if significant rises in sea level took place in a 100 years or less there could be a real problem." In the end, the panel concluded that collapse of the WAIS was not a realistic short-term issue. "Using extreme arguments and assuming the most rapid conceivable ice movements," John Perry noted in his summary after the meeting, "the conclusion was that due to physical constraints of ice movement it is highly unlikely that the WAIS could disappear in less than 200 years, and that starting from scratch the time required could be of the order of a thousand years."[66] They went on to establish a set of priorities for research needed before the WAIS could be fully understood.

The hydrology meeting, in turn, covered the results and shortcomings of climate models. "The inability of the models . . . to generate extreme events, variability, and regional climate" called for much more work to reduce the granularity of results. The attendees concluded that, given time, rain-fed

63. John Perry to the CDAC, "Discussion between the Chairman and the Staff," 7 Dec 1981, WAN, Box 90, Folder 9.

64. In the final document, this was called a Synthesis, and an additional Executive Summary was added with one-paragraph conclusions, presumably because the Synthesis was too long to be quotable.

65. A chapter on water supplies in the western United States, written by Revelle and Waggoner, was added prior to publication.

66. John Perry to participants in the West Antarctic Ice Sheet and Global Sea Level Workshop, "Notes on the Workshop," 9 Mar 1982, WAN, Box 90, Folder 11.

agriculture could adapt. The bigger potential issue was with irrigated agriculture: "It appears that the availability of water is of greater concern . . . than changes in temperature, that in rainfall areas we can probably adapt, and that even in arid areas, where the adjustment may be much more critical, some adaptation can take place."[67]

At the hydrology meeting, Revelle was asked whether a CO_2 increase was inevitable. He responded "that since using all the recoverable oil and gas will not double the atmospheric CO_2, we would have to go to coal to reach a doubling." He added that most of the energy needs of the future could be met by wood (which would, in effect, be a way of recycling the CO_2).[68]

Thus for some of the most serious topics, sea level rise and agriculture, Revelle was developing a view of concern but not alarm. In addition he clearly felt that technological solutions would be found that would limit the problem over time. This attitude was further reflected in his remarks at the next CRB meeting, where he filled in for Nierenberg in reporting the progress of the CDAC. In general terms the "effort was 'going great guns'. . . . At present the biosphere is probably a sink [for CO_2]. . . . This implies a net increase in global photosynthesis." Although lack of water could be an issue, "the effects of CO_2 on rain-fed agriculture are likely to be beneficial. Certainly the adaptability of agriculture has been amply demonstrated," but a continuing area of concern was "the frequency of extreme events which did not seem to be adequately treated in present models."[69]

William Nordhaus, for his part, was in the process of determining that future CO_2 emissions might not be as high as previous estimates. Nordhaus was a professor of economics at Yale University and had been a member of Carter's Council of Economic Advisors from 1977 to 1979.[70] In the spring of 1982 Nordhaus proposed a new approach for projecting future CO_2 emissions. In searching the literature he had not found any long-term models of energy use. He noted that the most common current method was simply to assume a constant rate of change to current fossil fuel use. For example, Ralph Rotty, who was a researcher at the Institute for Energy Analysis at Oak Ridge along with Alvin Weinberg, published an influential paper in 1976 titled "How Long

67. Ibid.
68. Ibid.
69. Draft Record of Actions, Meeting of the CRB, 19 May 1982, 18, WAN, Box 89, Folder 7.
70. W. Nierenberg et al., *Changing Climate* (ref. 11), 494.

Is Coal's Future?" that made projections using an exponential growth rate in fossil fuel production.[71] Alternatively, some models simply assumed that fossil fuel use would diminish in the future because of either resource constraints or concern over rising CO_2 levels.

Nordhaus suggested that scenarios be created "in which three subjective values (low, medium, and high) are assigned to each of many variables." These included items like population growth, economic growth, elasticity of substitution between carbon and noncarbon fuels, etc. The models, run with various combinations of these parameters, could assign probabilities to various levels of CO_2 output. The sensitivity of the result to changes in the variables could also be determined. The models could be used to predict the effects of tax policies. By looking at the various assumptions and adding in the effect of carbon taxes, Nordhaus and colleagues could estimate how high taxes would have to be to affect energy use and substitution of nonfossil fuels significantly.[72] Jesse Ausubel, who was assisting Nordhaus with the model, said that "His [Ausubel's] hunch was that it will show CO_2 growth for 40–50 years, but then with increasing rate of CO_2 there will be mix changes. . . . tax policy will be a way of handling environmental costs."[73]

In the final report Nordhaus did "show a considerably slower emissions rate and carbon dioxide buildup than many of the earlier studies. . . . Atmospheric concentrations in the average case are expected to hit nominal doubling (600 ppm) around the year 2070."[74]

At the March meeting of the CDAC, Ausubel also reported on the current state of the Schelling chapter. It would be built around four topics: first, general stresses and factors related to the type of population and economic growth that would produce so much CO_2; second, the major engineering issues related to

71. Ralph M. Rotty and Alvin M. Weinberg, "How Long Is Coal's Future," *Climatic Change* 1 (1977): 45–57. They projected increases in the rate of fossil fuel rate indefinitely at the rate of 4.3% per year.

72. CDAC, Fifth session, 25–26 Mar 1982, WAN, Box 90, Folder 4.

73. Ibid.

74. W. Nierenberg et al., *Changing Climate* (ref. 11), 91. The commonly used 4.3% growth rate showed doubling by 2035. These estimates made by Nordhaus in 1983 turned out to be in line with the current consensus as reflected in S. Solomon, D. Quinn, M. Manning, Z. Chen, M. Marquis, K. B. Averyt, M. Tignor, and H. L. Miller, eds., *Climate Change 2007: The Physical Science Basis,* Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change (Cambridge: Cambridge University Press, 2007), 790 (fig. 10.20(a)).

sea-level rise, intended to determine "what is protectable and what is not"; third, the relationship between climate change and migration; fourth, what policy options were available and how they could be implemented.[75] Schelling had asked for feedback on "troublesome" issues like migration, but the request does not appear to have elicited any direct comments.[76] Leonard Machta was concerned that this chapter would "raise more questions (with Congress) than it would settle."[77] He was "reassured on being told that while it is necessary to look at policy options no recommendations were to be made."[78] As a whole the group agreed "that talk about mitigation is premature . . . that what is wanted is to look at the options, to undertake exploratory talks, not cost out mitigation."[79]

The carbon cycle was an area of some disagreement. George Woodwell felt that deforestation was a relatively major contributor to the increasing CO_2 level. P. G. Brewer, Leonard Machta, and others felt that Woodwell's deforestation estimates were too high in relation to measured values in other areas and that in fact terrestrial biota could be a net sink. They felt that the amount of CO_2 emissions from fossil fuel were well understood, as was the atmospheric concentration. If Woodwell was correct, then the amount of CO_2 that was staying in the atmosphere, the airborne fraction, was much lower than the conventional estimate of about 0.6.[80] Also unclear was where the carbon would be going. A lower airborne fraction would mean that, all other things being equal, atmospheric CO_2 levels would grow more slowly, but forest management would be a more important issue. Woodwell estimated current terrestrial biotic release at 1.8–4.7 gigatons (Gt) of carbon annually, primarily as the result of deforestation.[81] At a meeting in April of 1982, "Nierenberg to make the best of it noted, and Woodwell agreed, that the Committee could say in any event that reforestation would make an enormous difference even though one does not know where the zero balance is."[82] In a compromise the final report estimated "the current net release of carbon from the biosphere . . . as about

75. CDAC, 25–26 Mar 1982, WAN (ref. 72).

76. Ibid.

77. Ibid.

78. Ibid.

79. Ibid.

80. The airborne fraction is defined as the ratio of the annual increase in atmospheric CO_2 to the CO_2 emissions from annual fossil fuel and cement manufacture combined. It is, in fact, currently assumed to be 0.6; Solomon et al., *Climate Change 2007* (ref. 74), 139.

81. W. Nierenberg et al., *Changing Climate* (ref. 11), 234.

82. Record of Actions, CDAC, 6 Apr 1982, WAN, Box 90, Folder 4.

2 gigatons. . . . This is the upper limit that can be accommodated by atmosphere-ocean models."[83]

During the development of the report small shifts had taken place in the chapters and the authors. Because of the various views of the members, the carbon-cycle chapter was a composite; Brewer, Machta, Woodwell, and Revelle wrote different portions. The climate-modeling chapter was largely a reprint of the report of the Smagorinsky subcommittee of the Climate Research Council[84] with additional material added by Smagorinsky, plus a new section on non-CO_2 trace gases written by Machta.[85] Detection and monitoring were the work of a subcommittee chaired by Gunter Weller.

The question for Weller's group was identification of the appropriate signals revealing CO_2-induced climate change. They were also asked to determine whether the current signal was strong enough to indicate that climatic changes due to CO_2 were already happening. They determined that a set of variables would be more reliable than any one variable such as atmospheric temperature. Unfortunately the temperature record was the only one "which is now about lengthy enough so that it can be (and has been) analyzed."[86] In looking at the available data, they determined, "In view of the relatively large and inadequately explained fluctuations over the last century, we do not believe that the overall pattern of variations in hemispheric or global mean temperature or associated changes in other climatic variables yet confirms the occurrence of temperature changes attributable to increasing atmospheric CO_2 concentration."[87]

The Weller panel also made an empirical estimate of climate sensitivity from the available record. "If the preindustrial CO_2 concentration was near 300 ppm, the sensitivity of climate to CO_2 (expressed as projected temperature increase

83. W. Nierenberg et al., *Changing Climate* (ref. 11), 187. Even the 2-gigaton figure seriously overestimated contributions from the terrestrial biota, which in fact were of the opposite sign. According to the Intergovernmental Panel on Climate Change (IPCC) 2007, the biota was a net sink of 1.7 gigatons of carbon in the 1980s, increasing to a sink of 2.6 gigtaons in the 1990s. Eventually Woodwell had to admit that he had been wrong (Weart, *Discovery of Global Warming* (ref. 25), 111).

84. This report had been published separately as *Carbon Dioxide and Climate: A Second Assessment* (Washington, DC: National Academy Press, 1982).

85. Ausubel believes this was the first significant review of other greenhouse gases (personal communication).

86. M. C. MacCracken to Weller Panel, "Comments on John Tunney letter to William Nierenberg," 5 Jul 1983, WAN, Box 86, Folder 7.

87. W. Nierenberg et al., *Changing Climate* (ref. 11), 292.

for a doubling of CO_2 concentration) might be as large as suggested by the upper half of the range of the study of the CO_2/Climate review panel (1982), i.e., up to perhaps 4.5°C. However, if the preindustrial concentration was well below 300 ppm, and other forcing factors did not intervene, the sensitivity must be below about 3°C to avoid inconsistency with the available record."[88] This estimate meant that climate sensitivity might indeed be in the lower part of the range, given that Machta had estimated that preindustrial concentrations were perhaps as low as 265 ppm.[89]

A reference to this conclusion can be found in the synopsis and executive summary of *Changing Climate,* which state that, on the basis of the available climate record, warming at the lower end of the 1.5° to 4.5 C range was more probable. OCS claim that Nierenberg inserted this conclusion into the synthesis without evidence,[90] but it is clearly a direct conclusion of the Weller panel.[91]

FINAL VERSION OF THE CDAC REPORT AND IMMEDIATE REACTIONS

By the spring of 1982 the CDAC had settled on the format and tone of the report, although much specific work remained to be done. The issues were serious, but the committee had concluded that the most catastrophic scenarios were not probable, at least in the near term.[92] A great deal of research was required to settle the uncertainties, but committee members believed that significant policy actions would be premature. In the spring of 1983, the drafts of the various chapters were completed, and Ausubel had begun work on the synthesis chapter, which was designed to summarize the joint conclusions of

88. Ibid., 293–94.

89. Ibid., 186.

90. OCS, 144.

91. The Weller conclusion was reasonable given the available evidence, and it is in line with current consensus views regarding observational constraints. See Solomon et al., *Climate Change 2007* (ref. 74), § 9.6.4, p. 726 regarding observational constraints for climate sensitivity. "Results from studies of observed climate change . . . indicate that ECS [Equilibrium Climate Sensitivity] is very likely larger than 1.5°C with a most likely value between 2°C and 3°C." The overall IPCC assessment of a most likely 3°C sensitivity is based on models as well as observation.

92. The range implied by "near term" varies by topic. For example, dramatic sea level increases by 2100 resulting from the collapse of the WAIS were unlikely while agricultural impacts were only projected for the next thirty years.

the committee. At a two-day meeting in March of 1983, the group assembled for a final time to go over the synthesis before the report was submitted for review in April.

A typical scientific paper might be reviewed by only two or three individuals, but because of the broad scope and importance of *Changing Climate,* the CDAC and the Board on Atmospheric Science and Climate (BASC, successor to the CRB) sought a large group of reviewers. In addition to the membership of the board sixteen individual scientists, experts in fields relevant to the various chapters, were asked to offer reviews. Almost all of these reviews were anonymous, and most of the reviewers provided comments on the synthesis, as well as on the specific chapters in their areas of expertise. In July of 1983 Ausubel distributed the reviews to the committee. In an accompanying memo, he noted that the individual authors or groups would respond directly to technical comments on their chapters. He went on to say that the reviewers' comments might directly or indirectly affect the synthesis. The committee was asked to submit suggested changes to him, and he indicated that a response would be sent in writing to the independent NRC Review Committee.[93]

Copies of the anonymous reviews forwarded to the committee by Ausubel can be found in the Scripps archives, but we were unable to locate the exact draft given to the reviewers or the response to the reviews sent to the NRC Review Committee. Unfortunately, therefore, although substantial revisions were made between the drafts that are available and the final version, we were not able to determine the extent to which individual comments were incorporated.

Because of the number of people who responded, the reviews represent an interesting cross section of viewpoints of the time. The majority of the reviewers were positive about the synthesis and considered it clear and well balanced. The following are typical remarks from these reviews. "I find the chapter well written, making its points with clarity and precision." "In summary, I think the report is a good one. Even in a world three feet deep in CO_2 reports, it pulls together a lot on possible impacts of CO_2 increase." "In general it is a very impressive and scholarly document. I think it fills an important need and will be extremely valuable."[94]

Some reviewers, however, took exception. "[The report] does not, in my view, provide the focused and critical assessment that is needed and expected from the NRC's first comprehensive look at the CO_2 question since the 1977

93. Jesse Ausubel to the CDAC, "CDAC Report Review," 18 Jul 1983, WAN, Box 86, Folder 91.
94. Anonymous reviewers, attachment to CDAC Report Review.

publication of *Energy and Climate*," one reviewer commented, continuing, "The present draft is a disappointing sequel to the corresponding chapter of that earlier study, and fails to do justice to the advances in understanding." Perhaps surprisingly from the perspective of 2010, this reviewer felt that the synthesis had missed the chance to "diffuse the generally extreme visions of CO_2-induced changes that have been advocated by the press, Congress, and several scientific publications." The reviewer pointed out that "in reading the Report . . . CO_2 is (1) unlikely to significantly help or hurt agriculture over the next generation; (2) unlikely to cause dramatic changes in available water run-off over the next century; and (3) unlikely to present insuperable problems of sea-level rise over the next several centuries. . . . Perversely, however, these tremendously significant results are nowhere in the present Chapter 1 (synthesis) pulled together and presented as a potentially radical change in our understanding of the CO_2 issue."[95]

Alvin Weinberg, who was one of the anonymous reviewers, came away with the opposite conclusion.[96] "Thus the 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 adiabatically through many small decisions taken by tillers of the soil and keepers of the irrigation systems." Weinberg wanted to solve the climate problem with technology, specifically nuclear power. He wanted a much stronger statement on this topic:

> I would therefore implore the Committee to consider including in its final recommendations a prominently displayed paragraph along these lines . . . of the non-fossil options the only ones that appear to have the capacity during the next 75 years of seriously reducing the growth of CO_2 are conservation and nuclear power. Progress in conservation has been substantial. By contrast, the nuclear option . . . has fallen into deep trouble.[97]

Weinberg's opinion was not surprising, because he had a history of pushing the nuclear option. In 1982 at a DOE conference he said, "One could imagine 3,000 reactors in 2030 displacing a significant fraction of the projected fossil fuel energy demand for the globe."[98] Weinberg had built his career around

95. Ibid.

96. Weinberg's letter appears as one of the anonymous reviews; in addition, he apparently felt so strongly that he sent a copy over his signature directly to Nierenberg. WAN, Box 86, Folder BASC/CO2, Jul–Aug 1983.

97. Alvin Weinberg, Comments on NRC Draft Report of the CDAC, WAN (ibid.).

98. John Perry to CDAC, 27 Sep 1982, WAN, Box 91, Folder 1.

nuclear power, and he saw the CO_2 issue as the way to restart interest in developing this technology. Even though several committee members were advocates of nuclear power, including Revelle and Nierenberg,[99] the committee did not choose to discuss this option directly in the report.[100]

On the basis of this feedback, the committee would reasonably have assumed that their tone was about right. All but two of the outside reviewers felt that the report had generally struck a good balance and reflected the state of knowledge at the time. Of the two dissenters, one felt that they had missed the chance to play down the seriousness of the CO_2 issue and the other that they had played down the dangers of inaction and that they should have used the opportunity to advocate nuclear power.

Changing Climate was published three months later, in October of 1983. Some fanfare surrounded the publication—a dinner in Washington and extensive press coverage. The *New York Times* ran the executive summary of the report essentially verbatim the day after publication.

In an interesting temporal conjunction, two days before *Changing Climate* was released, the EPA report issued *Can We Delay a Greenhouse Warming?*[101] The report has been described as drawing scientific conclusions similar to those of *Changing Climate* but with much more of a tone of alarm.[102] A more accurate description would be that the EPA contained a lot less science than the NAS report, and that its scientific conclusions were very different. The EPA report predicted much higher CO_2 levels, temperatures, and sea-level rise at every future time period. However, although the tone of the report was alarming, largely because of these differing forecasts, its policy conclusions were essentially the same: that no current, reasonable policy options would significantly

99. Myanna Lahsen, "Experiences of Modernity in the Greenhouse: A Cultural Analysis of a Physicist 'Trio' Supporting the Backlash against Global Warming," *Global Environmental Change* 18 (2008): 204–19, on 211.

100. Out of more than twenty peer reviews OCS refer only to Weinberg's. OCS, 145–48. They comment that they were unable to find a record of a response to Weinberg's peer review, concluding therefore that the CDAC ignored Weinberg. However, we were unable to find a record of responses to any of the anonymous reviewers, so it is unlikely that this is a correct conclusion. In an e-mail to Naomi Oreskes in November 2007 John Perry wrote, "My recollection of the Weinberg review is somewhere between fuzzy and nonexistent, but I can't imagine that I would have buried such probing comments." John Perry to Naomi Oreskes, e-mail, 27 Nov 2007, used with permission of John Perry.

101. Steven Seidel and Dale Keyes, *Can We Delay a Greenhouse Warming?: The Effectiveness and Feasibility of Options to Slow a Build-up of Carbon Dioxide in the Atmosphere* (Washington, DC: Strategic Studies Staff, Office of Policy Analysis, U.S. Environmental Protection Agency, 1983).

102. Weart, *Discovery of Global Warming* (ref. 25), 141.

delay or reduce the amount of warming. The only policy that would be effective was a complete ban on the use of coal and shale oil starting in 2000, but the authors concluded that such a ban was economically and politically infeasible.[103] As a result, they recommended that the U.S. accelerate and expand research on adapting to a warmer climate, while narrowing the uncertainties surrounding climate sensitivity.[104]

Joseph Smagorinsky, author of the climate-modeling chapter for *Changing Climate*, was highly critical of the EPA report. Speaking at Youngstown University, he said, "Evidently the EPA was hell-bent on coming up with spectacular numbers. . . . It's bad enough when an individual does this kind of thing, but when a federal agency does it . . ."[105] The Reagan administration reacted negatively to the EPA report and pointed to *Changing Climate* to show that the EPA report was overblown.[106] The projections in *Changing Climate* have turned out to be much closer to the 2007 scientific consensus than have those in the EPA report, so at least in this case, the Reagan administration was right on the science, if perhaps for the wrong reason.[107]

ASSESSING *CHANGING CLIMATE*

Changing Climate was nearly 500 pages long and included a ninety-page synthesis, which was written by Ausubel with some editing by Nierenberg. In the preface, Nierenberg laid out the role of the synthesis and explained that it represented the collective views of the committee:

> The CO_2 issue is so diverse in its intellectual components that no individual can be considered an expert on the entire problem. For this reason, as noted above,

103. Although they projected that such a ban would only lower the temperature increase to 3.5°C from 5°C.

104. Looking at their baseline scenario, the EPA predicted that CO_2 levels would reach twice preindustrial levels by 2060. Because of these estimates of CO_2 increase, along with a somewhat higher estimate of climate sensitivity, they forecast that a 2°C temperature increase would occur by around 2040; for 2100 they forecast a more than 5°C increase. They also projected that sea level would rise between 48 to 380 cm by 2100. Seidel and Keyes, *Can We Delay a Greenhouse Warming?* (ref. 101), iv, 1–7. All of these predictions were significantly higher than those in *Changing Climate*.

105. "Climate Change Report Criticized," Youngstown, Ohio, *Vindicator,* 25 Oct 1983, WAN, Box 86, Folder 11.

106. Weart, *Discovery of Global Warming* (ref. 25), 141.

107. Solomon et al., *Climate Change 2007* (ref. 74), tables SPM.3 and SPM.4.

> the CDAC prepared or commissioned separately authored and separately peer-reviewed papers in each area and made no attempt to force unanimity of style or of views. For the same reason, the committee members felt themselves incapable of judging and endorsing as a group the details of each paper's analysis and findings. Each paper should therefore be viewed primarily as the product of its individual members and other reviewers but not as enjoying the unanimity of conclusions possible on a more homogeneous and less controversial topic. The committee's work did, however, reveal a large core of views, findings, conclusions, and recommendations on a more general level, which all members could wholeheartedly and responsibly endorse. These are presented in the synthesis of the report. Despite the existence of some areas of continuing controversy, such as the carbon cycle, no major dissents emerged with respect to the contents of this assessment.[108]

The synthesis was organized into four sections. The first was the projections for emissions, atmospheric changes, climatic changes, and impacts; the second a discussion of the committee's views on the seriousness of these effects; the third a catalog of potential responses; and the fourth a set of recommendations. Each of these sections cut across elements of the eight separately authored reports.

In Nierenberg's own words the conclusions were "conservative" and called for "concern, but not panic."[109] The report called for a vigorous and expanded research effort in a number of areas including alternative fuels, but like the JASON, Charney, Schelling, and EPA reports, it stopped short of recommending energy-policy changes in the near term.

The overall conservative tone of the report was not surprising given the results of the various chapters. William D. Nordhaus and Gary Yohe concluded that CO_2 emissions were likely to be lower than previous estimates. The Weller panel did not believe that evidence of climatic change was yet significant and concluded that the current evidence might support a sensitivity in the lower part of the modeled range. Revelle forecasted sea-level rise that was below previous estimates and had determined that the WAIS was unlikely to disappear for several hundred years. Waggoner saw no negative effect on U.S. agriculture in the foreseeable future, except for a possible reduction in water for irrigation. Schelling pointed out that the forecasts over these time frames were highly uncertain and that the world might be a very different place, with very different

108. W. Nierenberg et al., *Changing Climate* (ref. 11), xv. The carbon cycle comment clearly refers to the issue of biotic uptake or release of CO_2.

109. Ibid., xiii.

problems, in fifty or a hundred years. And, ironically, if Woodwell was right and deforestation had been a big factor in atmospheric CO_2 growth, future CO_2 levels would be *lower.*[110] All of these conclusions tended to push forward into the future the time at which critical decisions must be made, allowing more time for research and for better options to appear.

The report did not recommend an immediate change of fuel use patterns away from fossil fuels, apparently troubling some modern observers. This point must be viewed in light of two factors. First, by the time the report finally came out, Carter's Synthetic Fuels program, which had been its immediate impetus, was dead. The United States was therefore not moving rapidly toward the much more carbon-intensive policy of focusing on domestic coal. Second, as even the more alarming EPA report had concluded, no immediate realistic options seemed available. What the committee recommended instead was research and development that could create those options in the future. In what appears to be an overlooked recommendation, they also suggested that control of non-CO_2 greenhouse gases might be a much more viable place to start.[111]

These conclusions did not mean that the committee believed that no problem existed. Throughout the report, the authors expressed their uneasiness about the seriousness of the issue. In many cases they were much more specific than prior reports, looking at changes in sea level and agriculture and the potential conflicts that could occur.

> we are deeply concerned about environmental changes of this magnitude; man-made emissions of greenhouse gases promise to impose a warming of unusual dimensions on a global climate that is already unusually warm. We may get into trouble in ways that we have barely imagined.[112]

In 2010, the reader might have difficulty remembering where the battles were being fought in 1983. In an e-mail to Naomi Oreskes regarding a draft of OCS, John Perry wrote, "It seems to me that you are evaluating a report of 1983 in

110. If deforestation were causing a substantial amount of the growth in atmospheric CO_2, then the nondeforestation portion of the airborne fraction would be lower. For any given level of deforestation and emissions, the atmospheric CO_2 levels would therefore be lower in the future, because a smaller proportion would stay in the atmosphere. See W. Nierenberg et al., *Changing Climate* (ref. 11), Executive Summary, par. 4, p. 1.

111. Ibid., 4. We note that this topic became a focus at the 2009 Copenhagen conference. "Climate Negotiators Eye the 'Forgotten 50%' of Greenhouse Gas Pollutants," *Los Angeles Times*, http://www.latimes.com/news/nation-and-world/la-fg-climate-emissions14-2009dec14,0,4164470.story (last accessed 25 Mar 2010).

112. W. Nierenberg et al., *Changing Climate* (ref. 11), Executive Summary, 3.

terms of the received wisdom of 2007. . . . One must also remember the political context of the day. . . . Nierenberg's strong support for a broad and vigorous research program was very welcome, and I don't recall any bitter complaints about his softness on immediate policy actions."[113]

We find that *Changing Climate* was in the mainstream and that it reflected the views of all the panel members. OCS take the position that Nierenberg personally wrote the synthesis in a way that was in conflict with the established consensus. "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*."[114] We fail to see what aspect of the then-current consensus the report challenged. The report's scientific conclusions, although greatly expanded, were in line with the earlier JASON and Charney reports. At the same time, its socioeconomic conclusions were similar to those of the prior NAS committee, which had been chaired by the economist Schelling.

OCS further claim that conflict divided the committee and that Nierenberg sided with the economists, causing the synthesis to deny the seriousness of the CO_2 issue. Specifically they make four arguments: first, that the conclusions of the various chapters were in conflict with each other and that, in particular, the chapters written by the economists differed from those written by the scientists; second, that the synthesis reflected mainly the views of the economists; third, that it did not accurately represent the contents of the scientific chapters; and, fourth, that, in areas of uncertainty, the synopsis inevitably took the most sanguine view. We address each of these in turn.

Each of the chapters of the report focused on a particular topic and did not draw general conclusions, so their conclusions could hardly be in either agreement or conflict. OCS used the issue of timing as an example. They attempt to contrast the welfare and policy chapter authored by Schelling with the chapters written by the physical scientists. "Schelling's discussion was framed," they suggest, "by the underlying presumption that the changes under consideration were 'beyond the lifetimes of contemporary decision-makers'."[115] They go on to say, "However, the physical scientists did not think that the anticipated changes were beyond the lifetimes of contemporary decision makers." This statement about the beliefs of the physical scientists is made without citation, and we were unable to find its basis. For example, Revelle's predictions of sea-level rise were focused on the end of the following century or, in the case of the WAIS, well beyond.

113. John Perry to Naomi Oreskes, e-mail, 27 Nov 2007.
114. OCS, 113 (emphasis added).
115. Ibid., 142.

The idea that the synthesis mainly presented the views of the economists is not correct. We see no evidence that the chapters written by the economists were given any special weight in the executive summary. Of the twenty-one numbered paragraphs in the executive summary, the first thirteen are completely scientific in nature.[116] These are expressed with very little uncertainty.

In any event, when the conclusions are taken together, they do not seem particularly controversial, taking into consideration that they were written in 1983 and not 2010. For example, Revelle's discussion of the possible collapse of the WAIS is approximately three pages long in Chapter 8, and a summary of the topic takes up a full page in the synthesis. OCS take the position that Revelle was predicting an immediate threat from the collapse of the WAIS and that this prediction was not reflected in the synthesis.[117] In fact Revelle only viewed that collapse as a long-term possibility, and his overall prediction of sea-level rise was lower than those of earlier forecasts. "We arrive at a probable rise in sea level during the next 100 years of about 70 cm," he wrote. "But a much larger rate of rise is not unlikely during the following several centuries because of events in Antarctica."[118] This 70-cm figure was much lower than the 144 to 217 cm estimated in earlier studies.[119]

Far from playing down this problem, the synthesis actually goes beyond the Revelle chapter to mention that even 70 cm could produce significant negative effects.

> As explained by Revelle, melting of land ice and thermal expansion of the ocean might lead to a rise of about 70 cm in global sea level over the next 100 years, continuing thereafter. Many shoreline problems (for example, coastal erosion, storm surges, and salinity of groundwater) are sensitive to sea-level changes on the order of decimeters, and 70 cm, though modest-sounding on a calm day at the seashore, could effect a variety of unwelcome changes.[120]

116. Paragraphs 2 and 3 refer to Chapter 2, which was written by Nordhaus and Yohe, but they deal with causes and projections of CO_2 growth.

117. OCS, 144. Although OCS say they are discussing the synthesis their reference is to the executive summary.

118. W. Nierenberg et al., *Changing Climate* (ref. 11), 440. OCS erroneously states that the 70 cm was projected to be from thermal expansion alone. See OCS, 138.

119. A statement that OCS attribute to Roger Revelle that sea level rise from melting ice "could one day cause 'salt water to flow in the streets of New York and London'" was actually made by the author of the referenced *Time* magazine article, not Revelle. OCS, 114.

120. W. Nierenberg et al., *Changing Climate* (ref. 11), 48. The 70-cm figure still was higher than current Intergovernmental Panel on Climate Change estimates as presented in Solomon et al., *Climate Change 2007* (ref. 74), 182 (fig. 10.33).

OCS also claim that in areas of uncertainty the synthesis "took the most sanguine view." Their examples were CO_2 levels, deforestation, weather modification, and temperature, but in each case the synthesis reflects the contents of the relevant chapters. Projections of CO_2 levels were taken directly from Nordhaus and Yohe. Deforestation was from Woodwell's portion of Chapter 3. Weather modification was hardly discussed but was taken from Schelling's chapter.

We have therefore found no evidence supporting the assertions by OCS that the synthesis favored the economists' view and contradicted an emerging consensus. Instead the synthesis follows the conclusions of the underlying papers and the consensus of the committee, as was affirmed in the November 2007 e-mail from John Perry to Naomi Oreskes:

> You assert that the members of the committee did not concur with the Synthesis (which by the way was written by Jesse Ausubel with some guidance and editing by Nierenberg). However, in the Preface Nierenberg specifically states that the Committee members "wholeheartedly and responsibly endorse" the general conclusions, and that there are "no major dissents with respect to the contents of this assessment." To the best of my knowledge, this statement is correct. Certainly I would never have permitted the report to go forward if any member had raised explicit objections. It's possible, of course, that Nierenberg simply cowed the group into submission by the force of his terrifying personality—but people of this caliber are not easily cowed![121]

Contemporaneous evidence that the report was in the mainstream, and was considered a positive contribution, can also be found in this note written in November of 1983 from Perry to Nierenberg. "Bill, we must have slipped up somewhere in the CO_2 paper. Everyone likes the report, everyone loves us, there's no interesting hate mail, nobody's picking a fight with us—no fun at all."[122]

Changing Climate did not challenge any incipient scientific consensus but rather brought together a diverse set of expertise in one place for the first time, a point that Thomas Malone explained in the foreword to the report.[123] Whether or not "an emerging consensus" was present in 1983, as OCS claim, is open to debate. In any case, a reasonable claim in retrospect is that *Changing Climate* played a role in forming the modern consensus.

121. John Perry to Naomi Oreskes, e-mail, 27 Nov 2007.
122. John Perry to William Nierenberg, 11 Nov 1983, WAN, Box 86, Folder 12.
123. W. Nierenberg et al., *Changing Climate* (ref. 11), xi.