

Naomi Oreskes: Betraying the Public Trust

“When the definitive history of human society and global climate is written, both the Nierenberg report and your paper¹ will be seen much more favorably than Naomi's hatchet job.”

John Perry – Executive Secretary of the Carbon Dioxide Assessment Committee, National Academy of Sciences.

Note

This article was written in June 2015 at the request of the New York Times, which was running a profile of Naomi Oreskes. A link to this article appears in that story. The authors² of this article are solely responsible for the opinions expressed.

Introduction

In 2008 Oreskes, Conway, and Shindell published “From Chicken Little to Dr. Pangloss: William Nierenberg, Global Warming, and the Social Deconstruction of Scientific Knowledge”³⁴ (hereafter "Chicken Little"). In 2010 Oreskes and Conway also published *Merchants of Doubt* a non peered reviewed book which repeated much of Chicken Little as a chapter and included a chapter called “Sowing the seeds of doubt: Acid Rain.” A review of the evidence shows that these papers are so contaminated by intellectual dishonesty and fabrication that they could be most charitably considered fiction. Oreskes et al simply chose a narrative and then used cherry picking, misdirection, omission, and outright falsehoods in order to fit the story to the narrative.

¹ This reference is to our peer-reviewed paper see footnote 4.

² Nicolas Nierenberg, Victoria Tschinkel and Walter Tschinkel. Nicolas Nierenberg can be contacted at nicolasnierenberg (at) gmail (dot) com. A short biography of the authors can be found at the end of this article.

³Naomi Oreskes, Erik M. Conway, and Matthew Shindell, “From Chicken Little to Dr. Pangloss: William Nierenberg, Global Warming, and the Social Deconstruction of Scientific Knowledge.” *Historical Studies in the Natural Sciences* 38: 109–152.

⁴ Our peer-reviewed paper, which corrects the record, is Nierenberg, Nicolas, Walter R. Tschinkel, and Victoria J. Tschinkel. "Early climate change consensus at the National Academy: The origins and making of changing climate." *Historical Studies in the Natural Sciences*, Vol. 40, No. 3 (Summer 2010), pp. 318-349

Compounding the issue are Oreskes' popular publications and lectures that often expand on these false narratives in ways that even contradict her own work.

Naomi Oreskes asks the public to trust scientists.⁵ But when a Professor of the History of Science at Harvard is unconstrained by truth this can only erode the very public trust that she asks for. Of course Oreskes is no longer working as a scientist and history is open to interpretation, but as Daniel Patrick Moynihan famously said "You are entitled to your own opinions. You're not entitled to your own facts."

Below we refer to our own peer-reviewed paper on this subject as well as other documents and references.

Chicken Little

The core premise of "Chicken Little" is simple: Oreskes et al. allege that Dr. William Nierenberg did not believe that CO₂-induced global warming was a serious issue that must be addressed. As a result, they claim, Nierenberg used his role as chairman of the Carbon Dioxide Assessment Committee (CDAC) to cause the 1983 National Research Council report *Changing Climate* to conform to that view. They further claim that the report was completely out of step with the consensus of the time, including the views of the other scientific members of the CDAC. Nierenberg allegedly achieved this result by handpicking the panel and by writing the Executive Summary and the Synthesis in a way that essentially ignored the scientific conclusions contained in the body of the report. They also imply that Nierenberg was further motivated by a desire to appease the political leaders of the time.

In fact, after a careful review of Oreskes et al., the sources cited by the authors, and sources which should have been cited by the authors, we find their version of events to be unsubstantiated, and completely out of step with the facts. The National Academy of Sciences (NAS) committee that produced *Changing Climate*, the CDAC, was made up of some of the most distinguished scientists of the period.⁶ Different authors wrote different chapters of the report, and were largely responsible for the contents of their chapters. The Executive Summary and the Synthesis represented the joint findings and conclusions of all the members of the committee, as was clearly stated in the report's

⁵ See for example

http://www.ted.com/talks/naomi_oreskes_why_we_should_believe_in_science/transcript?language=en

⁶The members of the CDAC were William A. Nierenberg (Chairman), Scripps Institution of Oceanography; Peter G. Brewer, Woods Hole Oceanographic Institute/National Science Foundation; Lester Machta, National Oceanic and Atmospheric Administration; William D. Nordhaus, Yale University; Roger R. Revelle, University of California, San Diego; Thomas C. Schelling, Harvard University; Joseph Smagorinsky, Princeton University; Paul E. Waggoner, Connecticut Agricultural Experiment Station; George M. Woodwell, Marine Biological Laboratory, Woods Hole.

introduction. Abundant evidence, including the report itself, supports all these points, and Oreskes et al. present no evidence to indicate that Nierenberg unduly influenced the committee, that he had a hidden agenda as chairman, or that the general scientific community considered the report out of the mainstream.

In 2010 As a result of our own review of the same set of source materials from the Scripps Institution of Oceanography (SIO) archives, we published a peer-reviewed paper that completely contradicted Oreskes et al.'s results. Our paper appeared in the same journal that published "Chicken Little". It is remarkable that a journal would publish two papers on as narrow a topic as a climate change report from 1983. Normally they would have only have been willing to publish a short rebuttal or a letter. We can only assume that when presented with the errors and omissions present in "Chicken Little" they were as disturbed as we were. During this period Oreskes continued to promote her narrative in the public press with what were deliberately false statements about this period and about Nierenberg.⁷

Our peer reviewed paper⁸ and the various corrections printed by publications are theoretically an acceptable way to correct the history. But they do not address what we believe to be the serious misconduct of the "Chicken Little" authors. They also have not prevented Oreskes from continuing to publish and discuss the same narrative in the non peer-reviewed literature and in her lectures. As far as we know she has never acknowledged our paper or informed her readers or listeners that such a paper exists. Readers of academic research expect that they will be presented with work that is based on the best facts available to the researchers. Scholars are also expected to report on relevant papers that have appeared in the literature. Of course there is always a matter of choice about which facts and papers are important. But in this case numerous facts were altered and ignored, and the resulting history is unrecognizable. We believe that this goes well beyond any gray area of acceptable academic conduct.

Oreskes chose to publish popular versions of this false narrative in both the press and on television in the UK. In the UK newspapers and television are subject to review for truthfulness. After review by Ofcom the Sunday Times was required to print a correction of an article written by Oreskes, and the BBC chose to completely retract the

⁷ Many of these statements were direct contradictions to even her own peer reviewed work. It seems likely that this paper and the subsequent popular writing was a build-up to Oreskes' book *Merchants of Doubt* where the "Chicken Little" paper appears almost unaltered as a chapter. That book also includes a misleading chapter on the Acid Rain committee chaired by Dr. Nierenberg, and attempts to create the appearance of a long-term anti-environment conspiracy by Dr. Nierenberg and others. We assume Oreskes receives direct compensation from sales of that book, and the recent movie version as well as her lectures on this topic.

⁸ Nierenberg, Nicolas, Walter R. Tschinkel, and Victoria J. Tschinkel. "Early climate change consensus at the National Academy: The origins and making of changing climate." *Historical Studies in the Natural Sciences*, Vol. 40, No. 3 (Summer 2010), pp. 318-349

segment dealing with William Nierenberg in the “Climate Wars.” Oreskes had been an advisor to the producers on the BBC segment. The BBC wrote “In response to a previous complaint, the programme had been edited to address the erroneous impression that Dr. Nierenberg had been a Reagan appointee, but not with complete success. The residual inaccuracy...served to reinforce the impression that the coincidence between the committee’s conclusions and the Presidential agenda had been the result of political motivation on Dr. Nierenberg’s part, rather than a reflection of his and the committee’s scientific assessment of the issues. While it might have been legitimate to raise this as a possibility, the programme went further than the evidence warranted in suggesting that it was the case. The programme team were reminded of the need for accuracy when commenting on causation in an historical context. The programme will not be repeated in its present form.”⁹

Others have also taken a look at the details of Oreskes’ work. William Connolley, who is a published author in the history of climate science, writes a popular blog on climate change, which generally takes climate skeptics (or “septics” as he calls them) to task. Nevertheless he wrote a stinging critique of “Chicken Little” on his blog. His critique can be found here <http://scienceblogs.com/stoat/2008/11/10/nierneberg-concluded-oreskes-i/>.

Our critique of the Chicken Little paper is necessarily long and detailed. A simple way to understand the issue is to read what John Perry had to say after our paper was published in 2010. Perry was the executive secretary for the CDAC committee and witnessed first hand the events that transpired during the committee’s deliberations. Oreskes clearly thought Perry was a credible source as she quoted Perry in several places in Chicken Little and sent it to him for review. However she appears to have treated his review like she has treated all the other evidence. She included the parts (often in misleading fashion) that made her case, and withheld the rest, which clearly did not. We suppose that she might have felt that Perry was wrong somehow, but to not include his views was a deliberate fraud on the reader.

Dear Nico,

I eagerly speed-read the paper. I think you're accurate on all counts.

Your dad was appointed primarily on the basis of his broad expertise and accomplishments, as you note. His generally conservative views and acceptability to Administrations of both left-ish and right-is complexions were quite possibly considered a plus -- but he wasn't picked as a hit man. The Reagan administration probably wished they did have hit man in place. But if one hypothesizes that he was a tool of the Reagan Right, why did an OSTP political appointee put the arm on little me? I was invited to OSTP one afternoon, and a well-dressed and manicured chap named Markham rather crudely leaned on me to ensure that the report would go easy on fossil

⁹ Ecu_apr_sep10.pdf accessed from the BBC web site.

fuels. "This Administration has decided that our energy will come from coal and oil," he said! If my Chairman was in their pocket, why bother with me?

When the definitive history of human society and global climate is written, both the Nierenberg report and your paper will be seen much more favorably than Naomi's hatchet job.

Thanks again, and best regards,

John¹⁰

Tactics of Oreskes et al

The fundamental strategy of Oreskes et al is to decide on a narrative prior doing research or writing a paper. Then the facts are twisted, ignored or even altered to fit the narrative. Specifically Oreskes et al. use four tactics: (1) they make claims without evidence, (2) they do not admit to or cite readily available countervailing evidence, (3) they edit quotations or remove context in ways that completely change the original meaning, and (4) they alter chronological sequences. In the case of "Chicken Little" they completely mischaracterize the origins of the 1983 National Research Council report *Changing Climate*, the procedures used by the Committee and the National Research Council, and the peer review process used by the National Research Council prior to its release.¹¹ Each of these tactics constitutes an act of intellectual dishonesty or possibly outright academic fraud.

Below, we address each of Oreskes et al.'s major points. In order to facilitate a reader's initial review of our claims, we include the Executive Summary of *Changing Climate* as Appendix I. The Executive Summary was printed almost verbatim in the *New York Times* shortly after the full report was published in 1983.

Setting the Stage for an Attack on William Nierenberg Nierenberg, his Career and Qualifications

Oreskes et al. imply that Nierenberg was selected as Chairman of the Carbon Dioxide Assessment Committee (CDAC) for reasons other than his capability as a scientist and a strong reputation for fairly synthesizing complex scientific topics. They

¹⁰ Email from John Perry to Nicolas Nierenberg July 6th 2010

¹¹ National Research Council (U.S.). Carbon Dioxide Assessment Committee, *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, D.C.: National Academy Press, 1983.

do so by misrepresenting his scientific credentials and by reference to his “conservative politics.” They attempt to diminish Nierenberg’s credibility as a scientific chairman by omitting most of the salient features of his career, by leaving out important information and using facts carelessly, and by suggesting he was no longer a working scientist after 1953. “He subsequently held a series of positions at the interface between science and politics.” They depict Nierenberg as moving directly from his 1947 Ph.D. at Columbia University to “teaching physics” at the University of California, Berkeley, then leaving to take the position as director of the Hudson Laboratory in 1953.¹² This sequence is incomplete and misleading. A more complete and accurate summary makes it clear that throughout most of his career he was an active scientist with over one hundred peer-reviewed papers and expertise in many of the relevant areas. Nierenberg continued as a professor of physics at Berkeley until 1965 when he became the director of the Scripps Institution of Oceanography. Nierenberg had been involved in climate and atmospheric issues for a number of years as a bipartisan advisor. He had served as chair of the new presidential advisory committee, the National Advisory Committee on Oceans and Atmosphere, to oversee a national program on oceanography. Nierenberg chaired this committee from 1972 to 1977 and spoke forcefully in support of the National Oceanic and Atmospheric Administration. He had also served the Ford White House during 1975–1976 as a member of the President’s Science Advisory Committee and during 1976–1978 as a member of the Office of Science and Technology Policy under President Carter. He chaired and served on the National Aeronautics and Space Administration’s Advisory Council and was its first chairman from 1978 to 1982. He was also concurrently serving on the National Research Council’s Climate Research Board. We can deduce that his chairmanship abilities were well known at the time, and he was certainly technically prepared to be the chairman of the CDAC.

A more complete account of his career can be found in Appendix II.

This brief review of Nierenberg’s career and expertise demonstrates that he was, contrary to the claims and innuendo in “Chicken Little,” a more than reasonable choice, but Oreskes et al. were apparently unsatisfied with Nierenberg’s background as an explanation. When she asked John Perry his response was that “he had no recollection of how Nierenberg was chosen.” Most historians would have considered this response a dead end, but Oreskes et al. instead asked Perry to speculate. Perry suggested Nierenberg’s stature and “well known conservative politics.”¹³ Of course this is part of a carefully selected subset of what Perry told Oreskes about Nierenberg and the committee, as we will show in more detail later.

Creation of a Myth to Explain Nierenberg's Selection as Chairman of the CDAC

In fact if politics were a factor at all, being a conservative would have made Nierenberg a risky choice for the Academy to chair the committee at that time. The

¹²Oreskes et al., 117. Nierenberg’s true history was trivial to find as there were numerous memorials published when he passed away in 2000.

¹³Ibid., 122.

authors fail to note that in mid-1980, Jimmy Carter (a Democrat) was president, and both houses of Congress (which specifically requested this report from the Academy) had Democratic majorities. Oreskes et al. do note that Nierenberg was in place as chairman of the committee in October 1980, but then observe that “in 1981 Nierenberg joined the transition team of the new Reagan administration . . . Academy leaders likely viewed this as an asset, too.”¹⁴ Well they may have, but it certainly had nothing to do with his selection in the first place. In order to twist the reader's mind around to their concept the reader would have to believe that, in mid-1980, the academy leaders knew that Reagan would be elected later that year and that Nierenberg would be on his transition team, making him ideal as Chairman of the CDAC!

Subsequently Oreskes has continued to push the claim that Nierenberg was in the political service of the Reagan administration. In a bylined article in the Sunday Times “Jason and the secret climate change war”, published September 7th 2008, Oreskes writes “The US was by far the biggest producer of greenhouse gases in the world. If the president wasn’t careful, global warming could become a stick to beat America with. So Reagan commissioned a third report about global warming from Bill Nierenberg.” This statement was completely at odds with the facts, and even contradicted her peer-reviewed paper. As shown above, Nierenberg was appointed by the National Academy, well prior to Reagan’s election, and the report was a request of the U.S. Congress, which at the time was controlled by the Democratic Party. Similar statements were included in BBC programming that involved Oreskes. Both the Sunday Times, and the BBC published retractions after we contacted them.

Oreskes et al. Attempts to Portray Nierenberg as One who had an Early and Entrenched Belief that CO₂ was not a Serious Issue

Oreskes et al. purport to establish that Nierenberg did not consider CO₂ to be a serious issue. Nierenberg was a very public figure who wrote numerous articles and speeches during the relevant periods. Instead of relying on these, Oreskes et al. try to support their view of what might have been Nierenberg's position at the time by referring to two 1979 reports¹⁵. “The origins of his alternative view can be traced back to the year of the JASON and Charney reports,”¹⁶ but as we will show the connection they make to the JASON report is spurious, and they make no relevant connection to the Charney report.

The lead author of the 1979 JASON report was Gordon MacDonald.¹⁷ This report is interpreted in Oreskes et al. to have clearly laid out the scientific concerns over the

¹⁴Ibid., 122.

¹⁵ Oreskes et al. also use articles and events of 1989-90, but these are irrelevant to their argument that Nierenberg had biased views in 1981-83 when *Changing Climate* was being produced.

¹⁶Ibid., 118.

¹⁷Gordon MacDonald et al., “The Long Term Impact of Atmospheric Carbon Dioxide on

growth of CO₂ in the atmosphere¹⁸ and Oreskes et al. use this report as an example of the "emerging scientific consensus" on CO₂. However, they neglect to mention that Nierenberg was a coauthor of this very same report.

Later, Oreskes et al. return to that same report for a different purpose. They object to the sentence, "The warming of the climate will not necessarily lead to improved living conditions everywhere," which they say "the evidence suggests" is attributable to Nierenberg.¹⁹ Aside from failing to cite any evidence connecting Nierenberg to it, this statement is taken out of context. The cited sentence appears in the abstract of the JASON report, but it can also be found in the body of the paper. The full quotation is:

Viewed in the longer perspective and noting past historical events, changes in climate would not lead to improved living conditions everywhere. Any change would produce stress and possibly disaster in some parts of the world since so many aspects of society have adapted, with very large investments in their infra-structures, to the climate of the past few decades.²⁰

Clearly, the intended point was that the naïve view that warm is good was incorrect and that higher temperature would probably lead to a whole set of negative consequences.

In search of any direct evidence on Nierenberg's early views, the authors lift the following from a review of a Department of Energy Report: "There were lots of 'man-induced perturbations' in the environment, he (Nierenberg) suggested, and CO₂ was 'not particularly different from others that have been dealt with'."²¹ By stringing together two sentence fragments, and not providing the subject of the four-page document, they lead the reader to believe that Nierenberg was referring to the consequences of CO₂ for society. In fact, the title of the document tells the story. It is a very early (1978) review of the "Comprehensive Plan for CO₂ Effects Research and Assessment." The comments are specifically related to the problems of forecasting. Here is the more complete quotation:

A large number of brilliant people have worked on this problem 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 CO₂ changes is not particularly different from others that have been dealt with (this is implicitly recognized in the Comprehensive Plan.)²²

Climate," JASON Technical Report JSR -78-07, prepared for the U.S. Department of Energy, 1979.

¹⁸Oreskes et al., 115.

¹⁹Ibid., 117.

²⁰Macdonald et al., 25.

²¹Oreskes et al., 118.

²²William A. Nierenberg, "Draft, August 11, 1978, Review of the May 1978 Comprehensive Plan for CO₂ effects research and assessment," SIO WAN, MC 13 Box

So Oreskes et al. turned a comment on the state of climate modeling in general, and modeling the effects of increased CO₂, into an apparent comment on the problems society will face as a result of CO₂ increases.

The authors go on to select another couple of fragments out of comments sent to John Perry by Nierenberg regarding a draft Summer Study CRB report. They incorrectly identify this communication as a letter commenting on the Charney report.²³

Reviewing a draft of the Charney report in 1979 [Nierenberg] 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.”²⁴

(Oreskes et al. use the dismissive phrase, “oft-repeated position,” without citing any additional examples.) Taken in context, the comments they quote had to do with effective communication, not any broad-based philosophical viewpoint. And as we stated it had nothing to do with the Charney report. The full comment without the paraphrasing is;

The third bullet is again too strongly overstated for a well-balanced Academy document. Man has survived extreme climate changes in the past and will do so in the future. I don’t believe it is a question of survival—it could be more felicitously worded as the degree of adaptability to climate change.

Other comments in the same document support the view that Nierenberg was concentrating on the details of the document. “A very small point on page 9, third line of the second paragraph—I would like to see the ‘s’ dropped from ‘flows’. I like the subjunctive.”²⁵

In our view the first paragraph of his preface to the CDAC report is most probably the best representation of Nierenberg’s views in 1983, when the report in question was published.

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

172, Folder: “Review of the May 1978.”

²³The date of the letter, together with the page numbers and subjects referenced, make clear that it referred to a different document. We suppose that Oreskes et al. wanted to show that Nierenberg disagreed with the views in the Charney report.

²⁴Oreskes et al., 118.

²⁵William Nierenberg to John Perry, August 14, 1979.

avoided or ignored until the fog lifts Increasing atmospheric CO₂ and its climatic consequences constitute such a problem.²⁶

The Consensus on Climate Change

In order to make their argument that Nierenberg was a heretic at the time of the Academy report, Oreskes et al seek to create a generally accepted view of climate change existing in 1983. Oreskes et al. write, “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*” [emphasis ours].²⁷ We believe that defining what the consensus was as early as 1983 is difficult and perhaps too subjective a task. However, even if the reader accepts their definition of consensus, and they propose at least two, *Changing Climate* in no way contradicted their conclusion.

Referring to two 1979 reports, the JASON (MacDonald et al., of which Nierenberg was a co-author) and the National Research Council Charney report²⁸, Oreskes et al. write “A consensus view had emerged: global warming would happen and its impact would not be negligible.”²⁹ Later they add, “And if it was momentous the implication was that something had to be done to stop it.”³⁰ Their premise is that these reports concluded that the implications of climate change were so serious that they recommended immediate action. However, they do not correctly portray the policy recommendations, as the preface of the JASON report makes clear:

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 CO₂.³¹

²⁶*Changing Climate* (preface), 3.

²⁷*Ibid.*, 123.

²⁸Jule 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).

²⁹Oreskes et al., 112.

³⁰*Ibid.* 119.

³¹Gordon MacDonald et al., *The Long-term Impacts of Increasing Atmospheric Carbon Dioxide Levels*, Ballinger, 1982, ISBN 088410902X, 9780884109020, xviii.

Clearly, the JASON report was not pressing for immediate action. Oreskes et al. claim not only that the Charney report was pressing for action but also that it potentially moved forward the time frame in which it predicted effects would be felt. “In the proposal for the Charney report, the Academy had noted that effects might be felt sooner,”³² but here is what the report actually concluded:

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 CO₂ 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 of socioeconomic consequences may well be significant, but unfortunately the latter cannot yet be adequately projected.³³

The Charney report thus predicted that warming would happen, and that the changes might actually occur farther in the future than previously predicted because of the effects of the ocean. This is the *opposite* of what Oreskes et al. attributed to the report³⁴

We conclude that there is no support for the statement by Oreskes et al. that “Climate scientists had been suggesting that the government had to do something about greenhouse gasses...”³⁵

Oreskes et al. also proposed a more specific 1980 scientific consensus based on three claims: first that CO₂ was a greenhouse gas and was increasing, second that doubling was likely in the next half-century or so,³⁶ and third that doubling would lead to a temperature increase of 2–3°C. A plain reading of the CDAC report shows that it was in complete agreement with this view, not opposed to it. The Executive Summary of *Changing Climate* states,

2. The current increase is primarily attributable to burning of coal, oil, and gas; future increases will similarly be determined primarily by fossil fuel

³²Oreskes et al., 120.

³³The Charney report, 3.

³⁴Later (on page 121) Oreskes et al. include the conclusion by the Charney panel that the oceans might slow the warming process; as we have noted, Oreskes et al. are not always consistent.

³⁵Oreskes et al., 119

³⁶Neither the JASON nor the Charney report attempted to forecast future CO₂ levels; they computed climatic effects on the basis of a given increase.

combustion. Deforestation and land use changes have probably been important factors in atmosphere CO₂ increase over the past 100 years (Chapters 2, 3).³⁷

The report speaks for itself and is almost completely in agreement with these three theoretical areas of consensus. The only difference is in the estimate of when carbon dioxide would double. The CDAC report, more accurately in retrospect, put the likely projected doubling into the second half of the twenty-first century. Oreskes et al. provide no examples of how the CDAC report's conclusions differ from their theoretical three-point consensus, and they never return to this topic, leaving it as a bare assertion.

The Executive Summary of the CDAC report also contains a much stronger statement about potential consequences than either the JASON or the Charney report.

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.³⁸

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

Oreskes et al. Seek to Establish that Nierenberg Stacked the Academy Committee in order to Predetermine its Outcome

It is central to Oreskes et al.'s argument that the appearance of economists on the committee was Nierenberg's doing because the authors particularly dislike the conclusions of the economists. Oreskes et al. imply that Nierenberg somehow stacked the committee, writing that, "When the opportunity came to constitute the committee he included more than biology, geology, and oceanography. He included economics too." They go on to argue, "It is a striking feature of the CO₂ assessment committee that its members included two economists"³⁹. We wonder whether Oreskes et al. even read the history of the creation of the committee. In fact, Oreskes et al. make a grievous omission with respect to the committee's charge, which is contained in an act of Congress, conveniently reprinted in Annex 3 of the *Climate Change* report: "Such study should also include an assessment of the economic, physical, climatic, and social effects of such impacts."⁴⁰ This directive makes clear that Congress, not Nierenberg, wished the

³⁷*Changing Climate*, executive summary, 1.

³⁸*Changing Climate* (Executive Summary), 3.

³⁹*Ibid* 123.

⁴⁰*Changing Climate*, 492.

committee to include social scientists (as have many subsequent comprehensive reports, such as the current IPCC report). The charge to the committee removes any scrap of evidence that the inclusion of economists was Nierenberg's attempt to stack the membership.

National Research Council Committee membership is very carefully vetted by the supervising board, which must approve any reports. The Foreword to the final report, by Thomas Malone, says

A number of considerations went into the design of the Committee and the selection of additional experts to contribute to its work. Competence was sought in each of the major subject areas of the question as well as experience with assessment of long-range issues.⁴¹

Oreskes et al. Attempt to Argue that Nierenberg Inappropriately Steered the Committee's Deliberations

Oreskes et al. allege that Nierenberg “repeatedly (emphasis ours) tried to bring forward suggestions” that warming was not a serious concern. The supporting evidence consists of a single discussion at “an early meeting” regarding a scientific theory proposed by the “chair of the Oceanography Department at Woods Hole” regarding ocean circulation.⁴² Clearly regardless of the subject, a single scientific discussion, during the second of four meetings, does not constitute a “repeated attempt” to downplay the seriousness of global warming. In a further attempt to show that Nierenberg was personally pushing a particular view, the authors then suggest a sort of Machiavellian proxy scheme: “But Nierenberg’s principal tactic was to rely on the arguments provided by the two economists.”⁴³ They provide no evidence to back this assertion, but simply go on to outline some of the comments made by the economists at committee meetings.

Oreskes et al. Assert that Nierenberg Wrote an Executive Summary and Synthesis which was not in Keeping with the Body of the Report

Oreskes et al. note that the Executive Summary and Synthesis were based on conclusions “which all members could wholeheartedly endorse.” Yet strangely they immediately state, “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.”⁴⁴ Because the report is actually a series of separately peer-reviewed papers authored by the various members of the committee, each with different expertise, the differences among chapters are hardly surprising. The six physical science

⁴¹*Changing Climate*, x, xi.

⁴²Oreskes et al., 125 [Note: there is no single “Oceanography Department at WHOI].

⁴³*Ibid* 125.

⁴⁴*Ibid* 138.

chapters were focused on various physical effects that could result from the predicted increases in CO₂. The second chapter written by Nordhaus and Yohe was focused on predictions of future CO₂ levels. Schelling's chapter was more generally focused on societal impacts and choices for dealing with these threats.

Changing Climate included six physical-science chapters. Although Oreskes et al. briefly mention the other chapters, they focus entirely on Revelle's chapter on sea level rise, and mischaracterize the science from this chapter in an attempt to show that Revelle was predicting an immediate threat from the collapse of the west Antarctic ice sheet. Oreskes et al. paraphrase Revelle, "Disintegration of the West Antarctic Ice Sheet would have... far reaching consequences' Revelle concluded." What Revelle actually wrote was "Disintegration of the West Antarctic Ice Sheet would have such far reaching consequences that both the possibility of its occurrence and the rate at which disintegration might proceed should be carefully researched." 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. So through selective editing, Oreskes et al., first remove the uncertainty of outcome that Revelle expressed and, second, characterize his call for more research as a call for policy action.

Oreskes et al. continue with "Even without that, thermal expansion alone would produce 70 cm of rise—a not insignificant figure."⁴⁵ In fact, what Revelle wrote was "adding this estimate for ocean warming to our estimate for melting in Greenland and Antarctica we arrive at a probable rise in sea level of about 70 cm." Clearly Revelle's estimate of 70 cm was for total sea level rise, not just thermal expansion. Oreskes et al. may simply have made an error, but more probably they do not want the reader to understand that this figure included Revelle's prediction of losses from Greenland and Antarctica. We also note that the 70 cm figure was vastly lower than the 144 to 217 cm estimated in earlier studies.⁴⁶ Viewed in the context of the time, therefore, Revelle's chapter would have been viewed as less alarming.⁴⁷

The comment "not an insignificant figure" for the predicted sea level rise is unreferenced. Revelle's chapter does not include it, but a comment like that can be found in the Synthesis.

As explained by Revelle, melting of land ice and thermal expansion of the ocean may 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.

⁴⁵ Ibid 138.

⁴⁶ Ibid 135.

⁴⁷ We also note that with twenty-five years of additional scientific work Revelle's estimate would now be viewed as somewhat high.

The Synthesis is therefore the only place where the potential damaging effects of even a 70-cm rise in sea level are explained, again *the opposite* of the argument that Oreskes et al. make throughout their paper that the Synthesis watered down the science.

Oreskes et al.'s statement that the chapters differed from the Synthesis and Executive Summary is also untenable—it implies that the various committee members, all highly distinguished and influential scientists, agreed to a consensus synthesis that differed from their views. Despite the critical role in their core argument, Oreskes et al. produce no evidence to support this idea. Instead they failed to include contrary evidence that they had in hand. The available evidence instead indicates that these were indeed the views of the entire committee. Here is the relevant paragraph from Nierenberg's Preface to the report:

The CO₂ issue is so diverse in its intellectual components that no individual may be considered an expert on the entire problem. For this reason, as noted above, the CDAC prepared or commissioned separately authored and separately peer-reviewed papers in each area, with 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. Thus, each paper should be viewed primarily as the product of its individual members and other reviewers but not enjoying the unanimity of conclusions possible in a more homogeneous and less controversial topic. However, the Committee's work did 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, there are no major dissents with respect to the contents of this assessment.⁴⁸

Oreskes et al. include a brief paraphrased portion of this paragraph but immediately dismiss it. They then use personal correspondence between Oreskes and John Perry to support their claim that the Synthesis did not represent the consensus, but they do so by omitting the fact that Perry unambiguously told them that the synthesis *did* represent the consensus view. In November 2007, John Perry wrote to 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

⁴⁸*Changing Climate*, xv.

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!⁴⁹

In a remarkable example of not including adverse evidence, the only portion of this note from John Perry that Oreskes et al. include in their paper was a footnote that Jesse Ausubel drafted the Synthesis and Nierenberg edited it.⁵⁰ The implication is that the Synthesis only supported the views of Nierenberg with Ausubel as the scribe. Because this quotation comes from a private correspondence between Perry and Oreskes, a reader of “Chicken Little” would have no way to know that the authors deliberately omitted Perry’s view that the Executive Summary and Synthesis represented the consensus of the committee.

Because the correspondence with Perry constitutes a particularly flagrant example of omitting contrary evidence, we cite more extensively from Perry’s letter to Oreskes below. Information about and from Perry was used extensively throughout “Chicken Little”, and as executive secretary for the committee he clearly was in a position to have observed firsthand what happened. Perry shared these observations with Oreskes both before the paper was written and after reading a draft. After reading a draft, Perry wrote an extensive email to Oreskes essentially disagreeing with her entire approach. We include portions of that email here.

On pp. 54-55, your presentation of Revelle’s conclusions is a bit unbalanced. You quote his estimates of the impacts of WAIS disintegration, but fail to note his later references to Bentley’s work, which placed upper limits on the speed with which the WAIS could deliver ice to the ocean. At the bottom of p. 444 of the report, Revelle concludes that the resulting rise in sea level would be at most 2m/100 years. Indeed, if one took Bentley’s “preferred” estimate, the rate would be 1.1 m/100 years, which “is about the mean rate for the last 15,000 years. It seems to me that point 12 of the Synthesis is thoroughly consistent with Revelle’s paper, and it’s misleading to imply otherwise.

On p. 55, you observe that none of the scientists advised a wait and see and adapt policy. Well, of course not. Their papers were in the nature of scientific reviews. There was never any intention that the individual papers would include policy recommendations. Indeed, this would have been inappropriate in individually authored contributions to an NRC report. Recommendations would have to appear only in sections of the

⁴⁹E-mail from John Perry to Naomi Oreskes, November 2007.

⁵⁰Oreskes et al., 137 (footnote)

report agreed upon by all members of the authoring group. Now, I haven't re-read the report in its entirety for a very long time, but a quick scan of the concluding paragraphs of some of the individual chapters simply does not support the notion that there was a yawning gap between the committee's views and the elements of the overview and synthesis. If this is an important component of your argument, then I think it requires much stronger documentation based on what was actually said in the report – not what you think should have been said.

On p. 58, after quoting accurately Nordhaus and Yohe's conclusion that there are unlikely to be any "easy ways" to constrain atmospheric buildup of carbon dioxide, you state that 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." I can't find that assumption in their text.

Your discussion of Schelling's paper (pp. 58 ff) grossly oversimplifies his complex, sophisticated and nuanced argument. I think you should read his paper again with an open mind.

Your thesis in this paper is that Nierenberg "deconstructed" a scientific consensus that existed at the time, replacing a definition of the issue as a fundamentally scientific problem with a new (and by inference inappropriate) framing as a broader social science problem. I have several problems with your approach:

First, the Nierenberg report does not take issue with any purely scientific findings – the magnitude and origins of carbon dioxide increase, the range of climatic effects, the implications for ice and sea level, etc.

Second, scientists did not generally frame the issue at the time in terms of immediate policy action on constraining fossil fuel use. If you look at the SMIC and SCEP reports, the NAS "Understanding Climate Change" report, and the World Climate Conference papers, you will see anthropogenic climate change as but one of a range of issues relating to climate variability and fluctuations. Indeed, Canada's Patrick McTaggart-Cowan promoted a program on "Living with Climate Change" around this time. As I noted before, the thrust for urgent mitigation came later. Nierenberg didn't deconstruct an action-oriented consensus framework, simply because it didn't exist at the time.

Third, the broadened multi-disciplinary framework that Nierenberg's report constructed is in fact the appropriate framework for the issue. Everything rests on the science, of course – but it's impossible to think rationally about dealing with the implications of that science without taking into account the human society that will experience impacts,

employ its resources for mitigation and adaptation, and maintain a society stable enough to do the job.

In responding to Dr. Perry's email Oreskes thanked him and said his comments would be taken into account. We can find no evidence that this happened. Even his specific comment about Nordhaus and Yohe was ignored.

It is certainly possible that Dr. Oreskes, after reviewing all the evidence, didn't agree with these statements by Perry. But at no point in "Chicken Little" is it ever mentioned that Perry completely disagreed with their conclusions in every important aspect. Why was Perry credible enough to believe on some things and not others? Why should his views be hidden from the reader? We also note that Perry urged Oreskes to contact Jesse Ausubel who also had assisted the committee, but she did not do so. Certainly a historian should not have suppressed this evidence simply because it was inconvenient to the theory.

Oreskes et al. argue that although the Synthesis included the relevant science it "rejected the interpretation of those facts as a problem."⁵¹ To make this point about the Synthesis they choose to quote from the Executive Summary (we realize that jumping between the Executive Summary and the Synthesis is confusing, but we must follow the logic of "Chicken Little").

Viewed in terms of energy, global pollution, and worldwide environmental damage the CO₂ 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.⁵²

In another act of careful editing Oreskes et al. leave out the last sentence:

It is important to be flexible both in definition of the issue, which is really more climate change than CO₂, and in maintaining a variety of alternative options for response.⁵³

By doing so, they omit the point of the paragraph and conclude, "In short Nierenberg reframed the issue as just one of many changes and challenges facing human society."⁵⁴ Even if this paragraph could be attributed to Nierenberg rather than to the

⁵¹Ibid, 142.

⁵²Ibid, 143.

⁵³*Changing Climate* (Executive Summary), 3.

⁵⁴Oreskes et al., 143.

committee as a whole—and there is no reason to believe it can—we do not see how, when not carefully edited, it “reframes the issue” or rejects CO₂ growth as a problem.

Oreskes et al. claim “it was the economist’s view that the report would place front and center”⁵⁵ but never establish a conflict of views between the scientists and the economists, who were discussing different aspects of the same issue. There is 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 nature⁵⁶. In fact the estimate of climate sensitivity to CO₂ growth in *Changing Climate* is very similar to current IPCC estimates, warming in the 1.5–4.5°C range commensurate with CO₂ doubling, polar amplification, sea-level rise, an ice-free arctic, etc. These are expressed with very little uncertainty.

Two paragraphs refer solely to the analysis by Schelling, but are hardly “front and center,” and the final paragraphs of the Executive Summary refer to the Synthesis along with other chapters. In any event, when the conclusions are taken together, they do not seem particularly controversial, given that they were written in 1983 and not 2008. We had a difficult time finding the unreasonable positions of the economists. Surely economists would be expected to suggest that cost-benefit analysis is a useful tool for approaching the issue of timing actions.

In the course of criticizing the Synthesis, Oreskes et al. continue to paraphrase misleadingly. The following engineered quote actually spans two paragraphs. They begin “As for international agreements to curb 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 CO₂ emissions. By curbing our CO₂ output, we make little contribution to the solution and do not know whether we will receive any benefits... [A] CO₂ 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.⁵⁷

The second portion of the quote, beginning with “[A],” actually begins, “Given the need for a widespread long term commitment.” Omitting this passage seems designed to obscure the committee’s desire for such a commitment. The “[and]” replaced “and while such an outcome is possible.” This omission has the effect of altering the committee’s view of the potential to achieve such a treaty. Finally Oreskes et al. leave

⁵⁵Ibid, 126.

⁵⁶Paragraphs 2 and 3 refer to chapter 2, which was written by Nordhaus and Yohe, but they deal with causes and projections of CO₂ growth.

⁵⁷Oreskes et al., 143

out “the nuclear test ban treaty being the most prominent.”⁵⁸ The original paragraph expresses that this type of treaty would be desirable, difficult to achieve, but not impossible. It provides an example of success from the past. Oreskes et al., through careful editing and paraphrasing, completely change the meaning. Having set up this misleading reference they criticize the committee’s “pessimism” in a footnote.

Oreskes et al. claim “Nierenberg gave the administration everything it wanted ...and concluded that technology would solve the problem with no need for government intervention.”⁵⁹ The citation supporting this statement is of a speech made by Nierenberg on the subject of the report. In reviewing that speech we find no reference to government intervention.⁶⁰ We also find no reference to technology’s solving the problem. The Executive Summary and the Synthesis discuss government intervention, but nowhere do they say that it will not be necessary. They discuss technology, of course, but not as an alternative to government intervention.

Also difficult to see is how the report gave the Reagan administration “everything it wanted.” Oreskes et al. had argued that the administration wanted to cut funding for climate research.

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... Friends of the Earth accused the administration of trying to eliminate science that had disagreed with their ideology... It was cutting research that ‘might disprove its conceptions’.⁶¹

Changing Climate called for continued and expanded research in a number of areas. Major sections of the Synthesis were devoted to areas where future research would be most beneficial. If the administration wanted permission to cut climate funding, it did not find it in this report.

In 2015, the reader might have difficulty remembering where the battles were being fought in 1983. In his e-mail to Oreskes, John Perry said, “It seems to me that you are evaluating a report of 1983 in 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.”⁶²

⁵⁸*Changing Climate*, 70

⁵⁹*Ibid*, 137.

⁶⁰ W.A. Nierenberg, Opening Statements, CO2 Dinner Symposium, 19 Oct 1983, SIO WAN, Box 87

⁶¹Oreskes et al., 136. Note also that many credit Nierenberg with saving Keeling’s research funding. See, for example, the NAS memorial to Nierenberg.

⁶²John Perry to Naomi Oreskes, November 2007.

Oreskes et al. Claim That, Where Uncertainty Existed, the Synthesis Consistently Took the More Optimistic View on the Effects of CO₂

Oreskes et al. allege that

At junctures where an important uncertainty was broached the synthesis took the most sanguine view: that CO₂ 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 CO₂ was likely to be at the low end of earlier estimates.⁶³

These objections on topics related to the forecasts are a bit bizarre, because they have turned out to be fairly accurate and are congruent with the current IPCC forecasts made twenty-five years later.⁶⁴ The first three claims are made without citation or other evidence and for good reason. First, the Synthesis simply and plainly summarizes the content of the second chapter of *Changing Climate*, “Future Carbon Dioxide Emissions from Fossil Fuels.” Second, projections of deforestation are actually not discussed in the Synthesis (but are implicit in the forecasts of CO₂ concentrations.) Third, weather modification is not mentioned in the Executive Summary and appears only in passing on page 60 of an 86-page Synthesis: “From study of CO₂ we know that, in principle, modification of climate and weather is feasible; the question is what kinds of advances in climate and weather modification will emerge over the coming century.”⁶⁵

The final claim, that the Synthesis took a sanguine view of the temperature forecast, is a more interesting error. Oreskes et al. actually cite the Executive Summary even though they say that they are referring to the Synthesis. The Executive Summary includes the statement that a CO₂ doubling “would cause a global surface air warming of between 1.5°C and 4.5° C. The climate record of the past hundred years and our estimates of CO₂ changes over that period suggest that values in the lower half of this range are more probable.”⁶⁶ Oreskes et al. are very critical of the last sentence:

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.⁶⁷

⁶³Ibid, 144 (We assume that the reference to CO₂ “use” was an error and actually meant CO₂ emissions.)

⁶⁴ IPCC, 2007: Climate Change 2007: The Physical Science Basis. Cambridge University Press, Cambridge United Kingdom and New York, NY, USA, 791.

⁶⁵*Changing Climate*, 60.

⁶⁶Ibid, 2.

⁶⁷Oreskes et al., 144.

With the same type of omission that we have seen earlier, Oreskes et al. failed to note that the paragraph referred to both Chapter 4 and Chapter 5. (And a more complete review would also show that Chapter 4 was not simply a set of excerpts from the Smagorinsky panel.) On the second page of Chapter 5, the following statements were underlined for emphasis in the original report.

If the preindustrial CO₂ concentration was near 300 ppm, the sensitivity of climate to CO₂ (expressed as projected temperature increase for a doubling of CO₂ concentration) might be as large as suggested by the upper half of the range of the study of the CO₂/Climate review panel (1982), i.e. up to perhaps 4.5 degrees C; if the preindustrial CO₂ concentration was well below 300 ppm and other forcing factors did not intervene, however, the sensitivity must be below about 3 degrees C to avoid inconsistency with the available record.”⁶⁸

In section 3.4 of the report appears the statement that “a WMO-sponsored Meeting of Experts in June 1983 concluded the most likely mid-nineteenth-century concentration was between 260 and 280 ppm...”⁶⁹ The statement that Oreskes et al. say “flew in the face” of scientific evidence was actually a quite logical inference from this set of conclusions. In any event they were incorrect to say that “no evidence” supported this conclusion.

This pattern continues with the following statement:

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.”⁷⁰

This quote is actually from the Executive Summary, and Oreskes et al. fail to include the remainder of the paragraph: “More rapid rates could occur subsequently, if the West Antarctic Ice Sheet should begin to disintegrate.”⁷¹ Oreskes et al. imply that the statement did not mention the ice sheet, but only their own “editing” removed it. The Synthesis, which they do not quote from, includes the following: “Of even greater uncertainty is the potential disintegration of the West Antarctic Ice Sheet . . . This could cause a further sea-level rise of 5 to 6m in the next several hundred years.”⁷²

The current IPCC forecast made 25 years later is generally below a 70-cm increase from 1983, and it does not forecast west Antarctic ice sheet disintegration this

⁶⁸*Changing Climate*, 294.

⁶⁹*Ibid*, 242.

⁷⁰Oreskes et al., 144.

⁷¹*Changing Climate*, 2.

⁷²*Ibid*, 42.

century. Oreskes et al. criticize a 1983 report for being too conservative when it was actually slightly too aggressive as compared to the IPCC Report.⁷³

The Failure of Oreskes et al. to Produce Evidence That There was Contemporaneous Concern with the Report

Given their assertion that the CDAC report contradicted the "emerging consensus" and that it misrepresented the views of a majority of the committee members, we would expect that Oreskes et al. could have produced a series of critical responses to the report. Where is the outrage from the other committee members and members of the research community? The answer is that none was forthcoming, and the obvious reason is that the assertion is completely incorrect.

The executive summary of the report was published on the front page of the New York Times, and it received significant coverage from newspapers throughout the country. Oreskes et al. did not produce any adverse coverage. They also didn't produce any critical response from environmental organizations, or anyone else for that matter.

In the complete absence of such evidence, Oreskes et al. engage in an extreme example of cherry picking and exclusion of evidence. More than twenty peer reviews of *Changing Climate* were available in the Scripps Archive, but they only use material from one critical review by Alvin Weinberg and never mention the others. Of the other reviews, one felt that the committee had overemphasized the problem. Other than that the reviews were generally quite favorable and thought the report struck a good tone. The *Changing Climate* authors and Perry would have reasonably believed that their conclusions reflected mainstream scientific thinking of the time. Oreskes et al. go on for four pages discussing the Weinberg review, but even then they seriously distort it.

They write, "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."^{74, 75} They assert Weinberg "noted that this

⁷³IPCC 2007 Physical Science Basis, 752.

⁷⁴Oreskes et al., 38.

⁷⁵An aside: Oreskes et al. attribute this statement of academy policy to a private communication with Larry Armi, currently a scientist with the Scripps Institution of Oceanography. Why they felt that Armi was the best source of this academy policy is unclear, except that they wanted to include the quote "by his own account Armi was 'too junior' to speak up." Actually, Armi was a coauthor of the second assessment with Smagorinsky, and he never read *Changing Climate* (personal communication with Nicolas Nierenberg, June 2008). The only conceivable purpose of including this quotation is to mislead the reader into thinking that Armi was critical of a report that he never read.

report failed to pass that bar.” This assertion is another fabrication by Oreskes et al., because in fact, Weinberg made no comment on either of these issues at all. Weinberg had a history of pushing the nuclear option and wanted to solve the climate problem with technology, specifically nuclear power.⁷⁶ 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 CO₂ are conservation and nuclear power. Progress in conservation has been substantial. By contrast, the nuclear option...has fallen into deep trouble.⁷⁷

We find it unlikely that Nierenberg, who himself was supportive of nuclear power⁷⁸, could have gained the support of the committee members to follow Weinberg’s suggestion and make this a key recommendation of the report. This view is actually backed up by Weinberg’s letter. “Perhaps the Committee regarded a forthright discussion of the nuclear issue as too controversial.”⁷⁹

Oreskes et al. also claim that Weinberg’s input was ignored but present no evidence either way.⁸⁰ The final version of the report differed substantially from the review copy that Weinberg received, but whether that was a result of his comments has been impossible for us to determine.

Having set up this straw man, Oreskes et al. now ask themselves how these comments could have been “ignored,” and they quote Edward Frieman as saying that standards were lax in those days. Oreskes et al. knew that Frieman was not involved in *Changing Climate*. Instead they had in their hands the following from John Perry “My recollection of the Weinberg review is somewhere between fuzzy and non-existent, but I can’t believe I would have buried such probing comments.”⁸¹ So they chose to use the comments of someone who had no knowledge of the process and to omit the comments of a person who was responsible for the process at the time.

⁷⁶ *New York Times*, Obituary Alvin M. Weinberg, October 21, 2006

⁷⁷ Alvin Weinberg, Comments on NRC draft Report of the Carbon Dioxide Assessment Committee, SIO WAN, MC₁₃, Box 86 Folder BASC/CO₂, Jul–Aug 1983.

⁷⁸ Myanna Lahsen, “Experiences of Modernity in the Greenhouse: A Cultural Analysis of a ‘Trio’ Supporting the Backlash against Global Warming,” *Global Environmental Change* 18:211.

⁷⁹ Weinberg comments on NRC draft report, 5.

⁸⁰ *Ibid*, 148.

⁸¹ E-mail from John Perry to Naomi Oreskes, November 2007.

Conclusion

Oreskes et al. make the extraordinary claim that William Nierenberg was chosen by the Reagan administration to hijack a report of the National Academy of sciences in order to play down the risks of carbon dioxide. They use claims without evidence, manufactured evidence, and extreme cherry picking in order to support this claim. Contemporaneous response, the previous peer-reviewed literature (generally unreferenced by them), and the report itself are in direct contradiction to this claim. They were also directly contradicted by their own sources, which they fail to report.

It is a very serious charge that a distinguished member of the National Academy would misuse his role as the chairman of an important report in order to achieve a policy result. For scholars to make that claim without a shred of supporting evidence is, in our opinion, indefensible. In the United States libel laws do not apply once someone is deceased so Oreskes et al. knew they were safe from any type of legal action. But we believe had this defense not existed they would have been quite vulnerable to an action for defamation.

Merchants of Doubt Acid Rain (Chapter 3)

Because *Merchants of Doubt* was not peer reviewed there has been no effective way to respond to this misleading chapter covering William Nierenberg's chairmanship of the Acid Rain committee.⁸² This convoluted chapter completely reverses the story of that committee. Oreskes titles the chapter "sowing the seeds of doubt." In fact the acid rain committee was an example of "telling truth to power." The committee, with Nierenberg as its chair, publicly and repeatedly called for immediate and significant action on acid rain in direct contradiction to Reagan Administration policy. This advice wasn't taken by either the President, or the Democratically controlled Congress, but this was hardly Nierenberg or the rest of the committees' fault. Strangely the true story can be gleaned from much of the material included in the chapter but it is buried and obfuscated in so many ways that the reader is left with the opposite impression.

The Acid Rain committee unequivocally called for policy action. These conclusions were published in an interim report in July 1983, and again in the final report in July 1984. In both cases they received extensive national press coverage.

Large portions of eastern North America are currently being stressed by wet deposition of acids, by dry deposition of acid-forming substances, and by other air pollutants such as ozone metals and organics. Annual wet deposition of acidity in the northeastern United States and portions of Canada is at least 10 times that of remote areas. Acid deposition has altered the chemistry and biology of aquatic and terrestrial ecosystems of eastern North America. The principal agent altering the biosphere acidity is traceable to man-made sulfur dioxide (SO₂) emission. The Clean Air Act of 1970 has reduced the emission of SO₂ considerably, and may continue to do so. Nevertheless, the ecological problems that clearly result from man-made acid emissions are sufficiently well substantiated that additional reductions are required to prevent even more consequential environmental effects. The panel recommends that cost effective steps to reduce emissions begin now even though the resulting ecological benefits cannot yet be quantified.

There exist large uncertainties in every aspect of acid deposition—emission, transport, transformation, and eventual deposition, interaction with the biosphere, and economic consequences. Nevertheless when all the converging partial indicators are considered, it becomes clear that acid deposition is a

⁸² Oreskes, Naomi, and Erik M. Conway. *Merchants of doubt: how a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. Bloomsbury Publishing USA, 2010. (MOD)

problem for which solutions should be sought now, and further remedial steps taken.⁸³

One might wonder how this was sowing the seeds of doubt on acid rain. In fact, as can plainly be seen, it didn't. Yet in forty pages Oreskes manages to obscure the fact that these conclusions were published and misleads the reader on the chronology as it related to national decisions on policy.

Misrepresentation of Nierenberg's Background

Much as in "Chicken Little" Nierenberg's history is distorted in an apparent attempt to downplay his credibility, and to impugn him in various ways. Once again a period where he published over a 100 peer reviewed papers on low energy physics is described as "positions at the interface between science and politics." In this chapter some new twists were added.

In 1965 Nierenberg became director of the Scripps Institution of Oceanography...an institution busy at the time applying scientific knowledge to national security problems particularly in research linked to underwater surveillance of Soviet submarines and targeting submarine-launched ballistic missiles.⁸⁴

This is, of course, a ridiculous characterization of one of the top oceanographic institutions in the world. This is the home of Roger Revelle, Walter Munk and Charles Keeling. In order to paint William Nierenberg as a cold warrior Oreskes recast Scripps as a research arm of the DOD. More astoundingly, evidence to the contrary is readily found in the memorial of Nierenberg written by Charles Townes and Walter Munk that Oreskes herself cited.⁸⁵

Oreskes goes on to say "Nierenberg hated environmentalists, whom he viewed as Ludites."⁸⁶ This is an outrageous statement considering the fact that his daughter Victoria Tschinkel was the head of the Florida Department of Environmental regulation under Governor Bob Graham. In addition there are numerous counter examples including the fact that Nierenberg campaigned on television for the California Coastal Initiative and that Nierenberg saved the funding for Keeling's CO₂ research. But to make matters worse it makes little sense in the context of the chapter. Oreskes points out that Nierenberg was pushing for George Woodwell and Gordon MacDonald to be on the acid

⁸³ William Nierenberg, chairman, *Report of the Acid Rain Peer Review Panel*, Executive Summary July 1984 (Washington, D.C.: White House Office of Science and Technology Policy, 1984), This also appeared in the panels interim report in July 1983.

⁸⁴ Ibid, 79

⁸⁵ Oreskes is very familiar with SIO and knows that military research was a small part of the overall program.

⁸⁶ Ibid, 79

rain panel. Why would Nierenberg be doing that if he “hated environmentalists?”⁸⁷ A statement like that requires strong evidence and as usual Oreskes presents none.

He did disagree with environmental groups that opposed nuclear power and in a letter from 1980 to the Audubon society he explained why.⁸⁸

The principal alternative for the foreseeable future for nuclear power is the steady or increased use of fossil fuels. These are almost always accompanied by sulfurous emissions which our government recognizes as being practically uncontrollable...The acid rain problem is serious now and becoming more so all the time. The northeast is particularly plagued by this curse which has an extremely deleterious effect on the fresh water system and severe consequences for the forests.⁸⁹

Nierenberg is introduced as “a man who had never worked on acid rain, but was well-known to the White House--Marshall Institute cofounder and SDI defender William A. Nierenberg.” Of course neither the Marshall Institute or SDI existed in 1982 when Nierenberg was asked to chair the committee, and the claim that Nierenberg had never worked on acid rain was again ludicrous. He had been the Chairman of the National Advisory Committee on Oceans and Atmosphere from 1971 to 1977 and was a member of the Climate Research Board of the National Academy of Sciences prior to and during the period covered in this chapter. During that period acid rain was one of the major issues in atmospheric research.⁹⁰

Contradictions on Fred Singer

We are mostly concerned with Oreskes’ writing about Nierenberg, but some of the convoluted discussion of Fred Singer is worth at least briefly mentioning as it reflects on her tactics. Oreskes spends an extraordinary amount of the chapter discussing Singer and his role on the committee despite the fact that in the end his contribution was in an appendix of the final report, and that she is completely unable to document how he had any other effect on the report.

She claims that Singer didn’t care about the environmental damage of acid rain in a convoluted paragraph where she uses benefits and costs interchangeably.

“Singer ignored this, considering cost only in terms of the cost of pollution control-ignoring the cost of ecological damage.” This would be a terrible thing if it were true, but in the very next paragraph Oreskes complains “Singer also presumed that costs

⁸⁷ Elsewhere Oreskes had already established their credentials see for example Woodwell on MOD page 49, Macdonald on MOD page 86.

⁸⁸ Oreskes certainly had access to this letter as it was in the same files as the Acid Rain committee materials.

⁸⁹ Letter to Russell Peterson President of the National Audubon Society from William Nierenberg September 25th 1980

⁹⁰ All of this is covered in the memorial biography that Oreskes cites.

were mostly accrued in the present, but the benefits in the future, and therefore the latter had to be discounted.” So in the space of about two sentences Oreskes has gone from saying that Singer didn’t think there were any benefits to reducing acid rain to complaining that he wanted to discount them.⁹¹

Eventually Oreskes’ biggest criticism is that Singer recommended “transferable emissions rights, the government would determine the maximum allowable pollution, and then grant or sell the right to pollute to parties who could then use, sell or trade those rights.” Strangely she admits that this is indeed how the acid rain problem was eventually solved, but somehow Singer recommending it was perverse or right wing. Oreskes also calls this a “free market” approach saying, “economists (and ordinary people) know that markets do not always work.”⁹² However government issued emission rights are not a free market approach and they are designed exactly to solve one of the cases when free markets don’t work.⁹³

A passing slander of Lester Machta

In introducing some of the panel members Oreskes attempts to smear Lester Machta. Machta had a long and distinguished career and was a Fellow of the American Association for the Advancement of Science, the American Meteorological Society, the American Geophysical Union, and the Royal Meteorological Society. He won numerous awards and prizes for his “work on atmospheric problems pertaining to the protection of the environment.”⁹⁴ But Oreskes dismisses him for wrong thinking.

Rowland had encountered Machta in the 1950s, when radioactive strontium had been detected in the baby teeth of children in St. Louis. Scientific work showed that it came from the U.S. weapons testing site in Nevada, but for a long time the official position was to blame Soviet fallout. Machta had been a spokesman for that view. The prospects for an unbiased acid rain panel didn’t look good.⁹⁵

The claim about fallout is made without citation. Modern science actually says the opposite. Due to characteristics of the testing at the Nevada test site fallout was largely regional in states immediately to the East, and the fallout in St. Louis was mainly the result of high altitude fallout from weapons testing in the Soviet Union and to a lesser

⁹¹ MOD 92.

⁹² Ibid, 93.

⁹³ They are often called a “market-based approach” which is certainly not the same thing as “free market.” A central authority chooses the level of acceptable pollution and enforces it with laws and regulations. See for example http://en.wikipedia.org/wiki/Emissions_trading, and http://en.wikipedia.org/wiki/Free_market.

⁹⁴ <http://www.arl.noaa.gov/machta.php> accessed June 3rd 2015.

⁹⁵ Ibid, 81.

extent by the U.S. in the Pacific.⁹⁶ Machta is dismissed for bias for blaming the Soviets even though he was right. We suppose that blaming the Soviets constitutes an example of wrong thinking to Oreskes that would carry over to all issues including acid rain.⁹⁷

The Interim Report

On June 27th 1983 the Office of Science and Technology Policy (OSTP) released a preliminary report from the acid rain committee where the headline was the call for action to reduce acid rain. Nierenberg was quoted nationally. A reader of *Merchants of Doubt* would be hard pressed to understand that this event occurred. Oreskes mentions a story in the *Wall Street Journal* in passing but using a kind of literary sleight of hand buries the lead and then goes off on what is in fact a misleading topic. She discusses at length changes that Fred Singer had proposed to the press release. "So now there were two versions of the problem. One, written by the panel, acknowledged the uncertainties but insisted that the weight of evidence justified significant action. The other, written by Singer (perhaps with help from the White House), suggested that the problem was not so grave... These were not the same view at all. Which one would prevail?" With this very dramatic conclusion Oreskes leaves the topic of the preliminary report here. Given her overall tone the reader probably imagines that it is the altered version that had "prevailed."⁹⁸ But in fact Oreskes knows perfectly well that the original version is what was issued as a press release by the OSTP. Singer's comments were never adopted as is obvious from the record.

Oreskes applauds language that was actually in the interim report. But incredibly she obscures the truth by referring to it as a draft. All of the language that she praises was, in fact, included. But a reader of *Merchants of Doubt* would never know that.

The draft version of the press release, which was admittedly long at nearly five full single-spaced pages, pulled no punches. It began by noting that the United States and Canada together emitted more than 25 million tons of sulfur dioxide per year, and then stated: "The incomplete present scientific knowledge sometimes prevents the kinds of certainty which scientists would prefer, but there are many indicators which, taken collectively, lead us to our finding that the phenomena of acid deposition are real and constitute a problem for which solutions should be sought."⁹⁹

⁹⁶ Simon, Steven L., André Bouville, and Harold L. Beck. "The geographic distribution of radionuclide deposition across the continental US from atmospheric nuclear testing." *Journal of Environmental Radioactivity* 74.1 (2004): 91-105.

⁹⁷ The smear of Machta seems to us gratuitous since we don't believe he ever appears again in the chapter.

⁹⁸ Ibid, 88.

⁹⁹ Ibid, 87.

From the New York Times account June 28th 1983 “A report from a panel of scientists prepared for the White House Science Office concludes that action should be taken immediately to curb acid rain, despite continuing uncertainty. The recommendation made public today, is contrary to the Reagan Administration’s current position...It is the first recommendation by an official Government panel calling for action and not just more study to deal with acid rain, according to William A. Nierenberg, chairman of the panel of nine scientists who prepared it.”¹⁰⁰

Oreskes then fails to even mention a very important event. On September 15th 1983 Nierenberg and the other committee members went to the White House to brief Ronald Reagan and Bill Ruckelshaus on their interim conclusions and recommendations. This meeting was scheduled for a full hour with each of the committee members being allotted five to ten minutes to cover various topics.¹⁰¹ Unfortunately subsequent to that meeting, despite the committee advice, the Reagan administration decided that they would not take action, but would instead maintain a policy of continued research. Although they may not have fully realized it at the time the purpose of the committee had effectively ended.

Upon reflecting on these scientific findings and recommendations, President Reagan chose to ignore his own panels’ recommendation for immediate action to reduce sulfur emissions and instead chose to highlight the areas of scientific uncertainty.¹⁰²

After skipping over the OSTP release of the preliminary report, the meeting at the White House, and Reagan’s decision not to act Oreskes spends about five pages mostly discussing Fred Singer. Oreskes then suddenly jumps to 1984 without informing the reader. “The full report was sent to the White House in early April, just as a key House of Representatives subcommittee was considering legislation to control acid rain...In May the House subcommittee voted 10-9 against the legislation effectively killing congressional action on the issue. The panel report was finally released to the public on the last day of August.”¹⁰³

Oreskes tries to connect the May House vote to the final report somehow. “Science magazine suggested that the congressional vote might have been different had the Nierenberg report been released beforehand.” Of course once again Oreskes is being misleading. Science magazine itself didn’t make that suggestion; rather they quoted a congressional Democrat in the fall of 1984 making that suggestion. And it is hard to see why the Democratic majority in the House needed any more from the acid rain committee

¹⁰⁰ Accessed from <http://www.nytimes.com/1983/06/28/us/panel-of-scientists-bids-us-act-now-to-curb-acid-rain.html>

¹⁰¹ Facsimile of Meeting agenda September 12 1983 Nierenberg files.

¹⁰² Alm, Leslie R. *Crossing borders, crossing boundaries: The role of scientists in the US acid rain debate*. Greenwood Publishing Group, 2000. 68

¹⁰³ MOD 94

in order to vote in favor. They had already received the interim report as well as testimony in the fall of 1983 on the committee's conclusions that action was necessary.

In fact as has been well documented the bill failed mainly due to a lack of support from environmentalists who wanted "more tonnage at any cost."

The best example of this was the fate of the Waxman-Sikorski Bill. This piece of legislation (H.R. 3400) appeared to have some prospects of adoption once the reports of the Interagency Task Force, the OSTP panel, and the NAS were released during 1983 and 1984. However, environmentalists and other supporters of more aggressive acid rain policy failed to mobilize successfully behind the initiative.¹⁰⁴

Oreskes goes on to say that "Press coverage was extensive and critical." Yet this is a completely baseless assertion. In May the report hadn't been issued yet and the citations refer to newspaper articles from 1985, 1983, and 1983 respectively. None of the articles she references had anything to do with the committee's report.¹⁰⁵

Oreskes spends considerable time discussing edits to the executive summary that took place in 1984. But while this is perhaps an interesting example of White House interference and Nierenberg's indifference it omits two critical things. First by the spring of 1984 the committee knew that their advice had been ignored by both the White House and the Congress. Nierenberg and the committee had naturally lost interest. Second while the summary had been expanded and rearranged no content was removed from the summary, the report's recommendation for action and the content of the body of the report were never modified. Perhaps to a sharp eye the tone was changed, but it didn't affect the headlines.

This is taken from the beginning of the New York Times story from September 1984 "A panel of experts commissioned by President Reagan's Office of Science and Technology today recommended an immediate start on "cost-effective" steps to reduce the pollution that causes acid rain. The recommendation runs counter to current Administration policy...A spokesman for the White House...said the report...was not new, having been contained in a preliminary report last year."¹⁰⁶

In order to confuse the reader Oreskes focuses on what is in a historical context a tempest in a teapot, and ignores the fact that the key recommendations were perfectly clear and once again covered by the national media.

¹⁰⁴ Gould, Roy. "ACID RAIN AS A POLITICAL PROBLEM." *The Acid Rain Controversy* (1988): 121. (Perhaps if Oreskes had reviewed the literature rather than just relying on clippings in Nierenberg's files she would have known this. Of course it isn't at all clear that she would have mentioned it anyway.)

¹⁰⁵ *ibid*, 94.

¹⁰⁶ Accessed from <http://www.nytimes.com/1984/09/05/us/panel-asks-action-on-acid-rain-now.html>

Oreskes claims that the Reagan administration was pleased with the result. But this again ignores the evidence. Nierenberg wrote an angry letter to George Keyworth in September 1984.

I am greeted by a huge number of press clippings on acid rain on my return. Several of them cited Bruce Abell that (sic) our Committee went beyond our terms of reference in making the recommendations that we did. This is simply and flatly not true. If you will remember in June 1983 we used your personal office to hold a press conference sponsored by OSTP releasing our recommendations—which have remained unchanged ever since. Those recommendations were reviewed and approved by your office. The original terms of reference had been augmented by John Marcum—presumably with your approval. I hope you have a good week in China.¹⁰⁷

William Nierenberg was never again appointed to any committees or national position by the Reagan administration.

In summary the acid rain committee, chaired by Nierenberg, followed the science, produced a comprehensive assessment of the issue, and recommended action against administration policy. Through the magic of creative writing Oreskes turned that into “Sowing the Seeds of Doubt: Acid Rain.”

¹⁰⁷ Letter from Nierenberg to Keyworth September 24th 1984.

Epilogue

"You are a slow learner, Winston."

"How can I help it? How can I help but see what is in front of my eyes? Two and two are four."

"Sometimes, Winston. Sometimes they are five. Sometimes they are three.

Sometimes they are all of them at once. You must try harder. It is not easy to become sane."

— George Orwell, *1984*

About the Authors

Nicolas Nierenberg is the CEO of Persyst Development Corporation a medical device manufacturer; he is also currently a member of the California Board of the Nature Conservancy and the Director's Council of the Scripps Institution of Oceanography. Victoria Tschinkel is Vice Chairman of 1000 Friends of Florida, a member of the board of the National Fish and Wildlife Conservation, a member of the board of Phillips 66, former Director of the Nature Conservancy in Florida, and former Florida Secretary of Environmental Regulation. Walter Tschinkel is an R. O. Lawton Distinguished Professor in the Department of Biological Science at Florida State University in Tallahassee.

Appendix I. Executive Summary of *Changing Climate*

Appendix 1 - "Changing Climate"

Executive Summary

1. Carbon dioxide (CO₂) is one of the gases of the atmosphere important in determining the Earth's climate. In the last generation the CO₂ concentration in the atmosphere has increased from 315 parts per million (ppm) by volume to over 340 ppmv. (Chapters 3, 4)

2. The current increase is primarily attributable to burning of coal, oil, and gas; future increases will similarly be determined primarily by fossil fuel combustion. Deforestation and land use changes have probably been important factors in atmospheric CO₂ increase over the past 100 years. (Chapters 2, 3)

3. Projections of future fossil fuel use and atmospheric concentrations of CO₂ embody large uncertainties that are to a considerable extent irreducible. The dominant sources of uncertainty stem from our inability to predict future economic and technological developments that will determine the global demand for energy and the attractiveness of fossil fuels. We think it most likely that atmospheric CO₂ concentration will pass 600 ppm (the nominal doubling of the recent level) in the third quarter of the next century. We also estimate that there is about a 1-in-20 chance that doubling will occur before 2035. (Chapters 2, 3)

4. If deforestation has been a large net source of CO₂ in recent decades, then the models that we are using to project future atmospheric concentrations are seriously flawed; the fraction of man-made CO₂ remaining airborne must then be lower, and CO₂ increase will probably occur more slowly than it otherwise would. (Chapter 3)

5. Estimates of effects of increasing CO₂ on climate also embody significant uncertainties, stemming from fundamental gaps in our understanding of physical processes, notably the processes that determine cloudiness and the long-term interactions between atmosphere and ocean. (Chapter 4)

6. Several other gases besides CO₂ that can affect the climate appear to be increasing as a result of human activities; if we project

increases in all these gases, climate changes can be expected significantly earlier than if we consider CO₂ alone. (Chapter 4)

7. From climate model simulations of increased CO₂ we conclude with considerable confidence that there would be global mean temperature increase. With much less confidence we infer other more specific regional climate changes, including relatively greater polar temperature increase and summer dryness in middle latitudes (e.g., the latitudes of the United States). (Chapter 4)

8. Results of most numerical model experiments suggest that a doubling of CO₂, if maintained indefinitely, would cause a global surface air warming of between 1.5°C and 4.5°C. The climate record of the past hundred years and our estimates of CO₂ changes over that period suggest that values in the lower half of this range are more probable. (Chapters 4, 5)

9. By itself, CO₂ increase should have beneficial effects on photosynthesis and water-use efficiency of agricultural plants, especially when other factors are not already limiting growth. (Chapters 3, 6)

10. Analysis of the effects of a warmer and drier climate on rain-fed agriculture in the United States suggests that over the next couple of decades negative effects of climate change and positive effects from CO₂ fertilization both will be modest and will approximately balance. The outlook is more troubling for agriculture in lands dependent on irrigation. Longer-term impacts are highly uncertain and will depend strongly on the outcome of future agricultural research, development, and technology. (Chapter 6)

11. Changes in temperature and rainfall may be amplified as changes in the annual discharge of rivers. For example, a 2°C warming could severely reduce the quantity and quality of water resources in the western United States. (Chapter 7)

12. (a) If a global warming of about 3 or 4°C were to occur over the next hundred years, it is likely that there would be a global sea-level rise of about 70 cm, in comparison with the rise of about 15 cm over the last century. More rapid rates could occur subsequently, if the West Antarctic Ice Sheet should begin to disintegrate. (Chapter 8)

(b) Such a warming might also bring about changes in Arctic ice cover, with perhaps a disappearance of the summer ice pack and associated changes in high-latitude weather and climate. (Annex 1)

13. Because of their large uncertainties and significant implications, it is important to confirm the various predictions of climate changes at the earliest possible time and to achieve greater precision. This can best be done through carefully designed monitoring programs of long duration emphasizing the ensemble of variables believed to influence climate or to reflect strongly the effect of CO₂. (Chapter 5)

14. The social and economic implications of even the most carefully constructed and detailed scenarios of CO₂ increase and climatic consequences are largely unpredictable. However, a number of inferences seem clear:

(a) Rapid climate change will take its place among the numerous other changes that will influence the course of society, and these other changes may largely determine whether the climatic impacts of greenhouse gases are a serious problem.

(b) As a human experience, climate change is far from novel; large numbers of people now live in almost all climatic zones and move easily between them.

(c) Nevertheless, 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, like release of methane from marine sediments, or not yet discovered.

(d) Climate changes, their benefits and damages, and the benefits and damages of the actions that bring them about will fall unequally on the world's people and nations. Because of real or perceived inequities, climate change could well be a divisive rather than a unifying factor in world affairs. (Chapter 9)

15. Viewed in terms of energy, global pollution, and worldwide environmental damage, the "CO₂ 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. It is important to be flexible both in definition of the issue, which is really more climate change than CO₂, and in maintaining a variety of alternative options for response. (Chapter 9)

16. Given the extent and character of the uncertainty in each segment of the argument--emissions, concentrations, climatic effects, environmental and societal impacts--a balanced program of research, both basic and applied, is called for, with appropriate attention to more significant uncertainties and potentially more serious problems. (Chapter 1)

17. Even very forceful policies adopted soon with regard to energy and land use are unlikely to prevent some modification of climate as a result of human activities. Thus, it is prudent to undertake applied research and development--and to consider some adjustments--in regard to activities, like irrigated agriculture, that are vulnerable to climate change. (Chapters 1, 9)

18. Assessment of the CO₂ issue should be regarded as an iterative process that emphasizes carry over of learning from one effort to the next. (Chapter 1)

19. Successful response to widespread environmental change will be facilitated by the existence of an international network of scientists

conversant with the issues and of broad international consensus on facts and their reliability. Sound international research and assessment efforts can turn up new solutions and lubricate the processes of change and adaptation. (Chapter 1)

20. With respect to specific recommendations on research, development, or use of different energy systems, the Committee offers three levels of recommendations. These are based on the general view that, if other things are equal, policy should lean away from the injection of greenhouse gases into the atmosphere.

(a) Research and development should give some priority to the enhancement of long-term energy options that are not based on combustion of fossil fuels. (Chapters 1, 2, 9)

(b) We do not believe, however, that the evidence at hand about CO₂-induced climate change would support steps to change current fuel-use patterns away from fossil fuels. Such steps may be necessary or desirable at some time in the future, and we should certainly think carefully about costs and benefits of such steps; but the very near future would be better spent improving our knowledge (including knowledge of energy and other processes leading to creation of greenhouse gases) than in changing fuel mix or use. (Chapters 1, 2, 9)

(c) It is possible that steps to control costly climate change should start with non-CO₂ greenhouse gases. While our studies focused chiefly on CO₂, fragmentary evidence suggests that non-CO₂ greenhouse gases may be as important a set of determinants as CO₂ itself. While the costs of climate change from non-CO₂ gases would be the same as those from CO₂, the control of emissions of some non-CO₂ gases may be more easily achieved. (Chapters 1, 2, 4, 9)

21. Finally, we wish to emphasize that the CO₂ issue interacts with many other issues, and it can be seen as a healthy stimulus for acquiring knowledge and skills useful in the treatment of numerous other important problems. (Chapter 1)

Appendix II.

A synopsis of William A. Nierenberg's career

In 1948, Nierenberg was Assistant Professor of Physics at the University of Michigan. From 1950 to 1965, he was Associate and then Professor of Physics at the University of California, Berkeley, where he had a very large and productive low-energy nuclear-physics laboratory; graduating 40 Ph.D.'s and publishing about 100 papers. During that period he took a one-year leave to serve as the director of the Columbia University Hudson Laboratory. Much later (1960–1962) he took leave once again 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.

In 1965 Nierenberg was offered the chance to run what most feel is the most prestigious oceanographic institution in the world, the Scripps Institution of Oceanography (SIO). His was not a random selection. His background in sonar research and other naval-related science, his knowledge of and interest in geophysics dating from his college times, and his many friendships in the community made him intellectually eager to go. By then he had also developed the many necessary international relationships. As SIO's longest-serving director, he quadrupled the institution's funding and developed a fleet of five modern research vessels.

Nierenberg gained national recognition for his achievements and contributions to science and earned a great deal of influence. In 1965, he was elected to the American Academy of Sciences and in 1979 to the Council of the National Academy of Sciences. He was also elected to the National Academy of Engineering and the National Academy of Arts and Sciences. He served on a large number of prestigious committees, primarily after he returned from NATO. Most relevant here, in 1971 he was appointed chairman of the National Academy of Sciences National Advisory Committee on Oceans and Atmosphere and served on this committee until 1977. He also served on various panels of the Presidents' Science Advisory Committee. He was a member of the National Science Board from 1972 to 1978 and again from 1982 to 1988.

Nierenberg was a consultant to the National Security Agency and served on many military-related panels. In 1976 he was appointed one of two senior consultants to the then newly formed White House Office of Science and Technology Policy (OSTP). He was a member of the National Aeronautics and Space Administration's Advisory Council from 1978 to 1982 and served as its first chairman. He was Chairman of the OSTP Acid Rain Peer Review Panel, whose report *Acid Rain* was published in 1984, and defended its recommendations for legislation capping SO₂ emissions to the Director of Management and Budget, David Stockman, with President Ronald Reagan looking on.