

## The Denial of Global Warming

**M**ANY AMERICANS HAVE THE impression that global warming is something that scientists have only recently realized was important. In 2004, *Discover* magazine ran an article on the top science stories of the year, one of which was the emergence of a scientific consensus over the reality of global warming. *National Geographic* similarly declared 2004 the year that global warming “got respect.”<sup>1</sup>

Many scientists felt that respect was overdue: as early as 1995, the leading international organization on climate, the Intergovernmental Panel on Climate Change (IPCC), had concluded that human activities were affecting global climate. By 2001, IPCC’s Third Assessment Report stated that the evidence was strong and getting stronger, and in 2007, the Fourth Assessment called global warming “unequivocal.”<sup>2</sup> Major scientific organizations and prominent scientists around the globe have repeatedly ratified the IPCC conclusion.<sup>3</sup> Today, all but a tiny handful of climate scientists are convinced that Earth’s climate is heating up, and that human activities are the dominant cause.

Yet many Americans remained skeptical. A public opinion poll reported in *Time* magazine in 2006 found that just over half (56 percent) of Americans thought that average global temperatures had risen—despite the fact that virtually all climate scientists thought so.<sup>4</sup> An ABC News poll that year reported that 85 percent of Americans believed that global warming was occurring, but more than half did not think that the science was settled; 64 percent of Americans perceived “a lot of disagreement among scientists.” The Pew Center for the People and the Press gave the number

believing that there is “solid evidence the Earth is warming” as 71 percent in 2008, but in 2009, the answer to that same question was only 57 percent.<sup>5</sup>

The doubts and confusion of the American people are particularly peculiar when put into historical perspective, for scientific research on carbon dioxide and climate has been going on for 150 years. In the mid-nineteenth century, Irish experimentalist John Tyndall first established that CO<sub>2</sub> is a greenhouse gas—meaning that it traps heat and keeps it from escaping to outer space. He understood this as a fact about our planet, with no particular social or political implications. This changed in the early twentieth century, when Swedish geochemist Svante Arrhenius realized that CO<sub>2</sub> released to the atmosphere by burning fossil fuels could alter the Earth’s climate, and British engineer Guy Callendar compiled the first empirical evidence that the “greenhouse effect” might already be detectable. In the 1960s, American scientists started to warn our political leaders that this could be a real problem, and at least some of them—including Lyndon Johnson—heard the message. Yet they failed to act on it.<sup>6</sup>

There are many reasons why the United States has failed to act on global warming, but at least one is the confusion raised by Bill Nierenberg, Fred Seitz, and Fred Singer.

### *1979: A Seminal Year for Climate*

In 1965, the President’s Science Advisory Committee asked Roger Revelle, then director of the Scripps Institution of Oceanography, to write a summary of the potential impacts of carbon dioxide–induced warming. Revelle had been interested in global climate for some time, and in the late 1950s had obtained funding for his colleague, chemist Charles David Keeling, to measure CO<sub>2</sub> systematically. (This work would produce the Keeling curve—showing CO<sub>2</sub>’s steady increase over time—for which Keeling would win the National Medal of Science and be made famous by Al Gore in *An Inconvenient Truth*.) Revelle knew that there was a lot about the problem that wasn’t well understood, so he focused his essay on the impact he considered most certain: sea level rise.<sup>7</sup> He also made a forecast: “By the year 2000 there will be about 25% more CO<sub>2</sub> in our atmosphere than at present [and] this will modify the heat balance of the atmosphere to such an extent that marked changes in climate . . . could occur.”<sup>8</sup>

The report made it to the Office of the President, and Lyndon Johnson mentioned it in a Special Message to Congress later that year: "This generation has altered the composition of the atmosphere on a global scale through . . . a steady increase in carbon dioxide from the burning of fossil fuels."<sup>9</sup> But with the war in Vietnam going badly, civil rights workers being murdered in Mississippi, and the surgeon general declaring that smoking was hazardous to your health, Johnson had more pressing things to worry about. Nor was it easy to get Richard Nixon's focus a few years later. Nixon undertook a number of important environmentally oriented reforms, including creating the Environmental Protection Agency, but during his administration climate concerns were focused on the SST project and the potential climate impact of its water vapor emissions, not CO<sub>2</sub>.

Yet, while CO<sub>2</sub> didn't get much attention in the 1970s, climate did, as drought-related famines in Africa and Asia drew attention to the vulnerability of world food supplies. The Soviet Union had a series of crop failures that forced the humiliated nation to buy grain on the world market, and six African nations south of the Sahel (the semi-arid region south of the Sahara) suffered a devastating drought that continued through much of the 1970s.<sup>10</sup> These famines didn't just hurt poor Africans and Asians; they also caused skyrocketing food prices worldwide.

The famines were also noticed by the Jasons, a committee of elite scientists, mostly physicists, first gathered in the early 1960s to advise the U.S. government on national security issues.<sup>11</sup> The Jasons have long been an independent, self-confident group, and especially in its early days the committee members often told the government what they thought it needed to know. But the Jasons also respond to requests, and in 1977, the Department of Energy asked them to review the DOE research programs related to CO<sub>2</sub>. The Jasons decided to look at carbon dioxide and climate.

Their report began with a recognition of the acute sensitivity of agriculture, and thus society in general, to even small changes in climate: "The Sahelian drought and the Soviet grain failure . . . illustrate the fragility of the world's crop producing capacity, particularly in those marginal areas where small alterations in temperature and precipitation can bring about major changes in total productivity."<sup>12</sup>

Over two summers they developed a climate model, which showed that doubling the carbon dioxide concentration of the atmosphere from its preindustrial level (about 270 ppm) would result in "an increase of average surface temperature of 2.4 C." Perhaps more worrying than the average

temperature increase was the prospect of “polar amplification”—that warming would be greater, maybe a lot greater, at the poles. In their model, the poles warmed by 10°C to 12°C—a colossal amount.<sup>13</sup>

None of this was new. Professional climate modelers had already published papers that said pretty much the same thing, and in 1977, Robert M. White, the head of the National Oceanic and Atmospheric Administration (and later head of the National Academy of Engineering) had headed a committee for the National Research Council that warned of the serious impacts of unimpeded climate change: “We now understand that industrial wastes, such as carbon dioxide released during the burning of fossil fuels, can have consequences for climate that pose a considerable threat to future society . . . The scientific problems are formidable, the technological problems, unprecedented, and the potential economic and social impacts, ominous.”<sup>14</sup>

But what matters in science is not the same as what matters in politics, and while the Jason study found nothing new, the fact that it *was* a Jason study “stimulated some excitement in White House circles.”<sup>15</sup> Still, the Jasons were mostly physicists, not climate scientists. They included a couple of geophysicists, one of whom had a long-standing interest in climate, but none claimed climate as their central area of active research. So Frank Press, President Carter’s science advisor, asked the National Academy of Sciences president Philip Handler to empanel a review of the Jason study. Handler turned to MIT professor Jule Charney.

One of the founders of modern numerical atmospheric modeling, and perhaps the most revered meteorologist in America, Charney assembled a panel of eight other scientists at the Academy’s summer study facility in Woods Hole, Massachusetts. Charney also decided to go a bit beyond reviewing what the Jasons had done, inviting two leading climate modelers—Syukuro Manabe from the Geophysical Fluid Dynamics Laboratory and James E. Hansen at the Goddard Institute for Space Studies—to present the results of their new three-dimensional climate models. These were the state of the art—with a lot more detail and complexity than the Jason model—yet their results were basically the same. The key question in climate modeling is “sensitivity”—how sensitive the climate is to changing levels of CO<sub>2</sub>. If you double, triple, or even quadruple CO<sub>2</sub>, what average global temperature change would you expect? The state-of-the-art answer, for the convenient case of doubling CO<sub>2</sub>, was “near 3 C with a probable error of 1.5 C.”<sup>16</sup> That meant that total warming might be as little as 1.5°C or as much as 4°C, but either way, there was warming, and the most likely

value was about 3°C. If you more than doubled CO<sub>2</sub>, you'd probably get more than 3°C of warming.

There were, however, natural processes that might act as a brake on warming. The panel spent some time thinking about such “negative feedbacks,” but concluded they wouldn't prevent a substantial warming. “We have examined with care all known negative feedback mechanisms, such as increase in low or middle cloud amount, and have concluded that the oversimplifications and inaccuracies in the models are not likely to have vitiated the principal conclusions that there will be appreciable warming.”<sup>17</sup> The devil was not in the details. It was in the main story. CO<sub>2</sub> was a greenhouse gas. It trapped heat. So if you increased CO<sub>2</sub>, the Earth would warm up. It wasn't quite that simple—clouds, winds, and ocean circulation did complicate matters—but those complications were “second-order effects”—things that make a difference in the second decimal place, but not the first. The report concluded, “If carbon dioxide continues to increase, the study group finds no reason to doubt that climate changes will result and no reason to believe that these changes will be negligible.”<sup>18</sup>

How soon would these changes occur? Charney's group couldn't say, in part because that depended on how the oceans absorbed heat. The climate models had “swamp oceans,” meaning they provided moisture to the atmosphere but did not hold or transport heat, so they weren't realistic. What would happen in real life? Everyone understood that the oceans have a huge “thermal inertia”—meaning that they take a very long time to heat up. Exactly how long depended in part on how well mixed they are, because the more well mixed the oceans are, the more heat would be distributed into the deep waters, and the slower the warming of the atmosphere would be. Scientists use the word “sink” to describe processes that remove components from natural systems; the oceans are almost literally a heat sink, as heat in effect sinks to the bottom of the sea.

The available evidence suggested that ocean mixing was sufficient to delay the Earth's atmospheric warming for several decades.<sup>19</sup> Greenhouse gases would start to alter the atmosphere immediately—they already had—but it would take decades before the effects would be pronounced enough for people to really see and feel. This had very serious consequences: it meant that you might not be able to prove that warming was under way, even though it really was, and by the time you could prove it, it would be too late to stop it.

One Jason recalls being asked by colleagues, “When you go to Washington and tell them that the CO<sub>2</sub> will double in 50 years and will have major impacts on the planet, what do they say?” His reply? “They . . . ask

me to come back in forty-nine years.”<sup>20</sup> But in forty-nine years it would be too late. We would be, as scientists would later say, “committed” to the warming—although “sentenced” might have been a better word.

Verner E. Suomi, chairman of the National Academy’s Climate Research Board, tried to explain this crucial point in his foreword to the Charney report: “The ocean, the great and ponderous flywheel of the global climate system, may be expected to slow the course of observable climatic change. A wait-and-see policy may mean waiting until it is too late.”<sup>21</sup> Suomi realized that this conclusion might be “disturbing to policymakers.”<sup>22</sup> He was right.

*Organizing Delay:  
The Second and Third Academy Assessments*

Before Charney’s study had even been published, the White House Office of Science and Technology started asking the National Academy of Sciences for more information.<sup>23</sup> There was a host of questions about anthropogenic climate warming that Charney hadn’t asked, let alone tried to answer. Prominent among them was quantification of the time frame. When would measurable change occur? “Decades” was a pretty loose estimate. What specific effects would follow? Policy makers wanted answers.

The next Academy study to address the anthropogenic warming problem produced only a letter, not a full scientific assessment, but it was nonetheless influential. Chaired by economist Thomas Schelling, famous for his work in game theory (and for which he would later win the Nobel Prize in Economics), the committee included Roger Revelle, Bill Nierenberg, and McGeorge Bundy, the national security advisor to presidents Kennedy and Johnson. Their letter report was submitted in April 1980.

Schelling focused on what warming would mean socially and politically, an aspect of the problem that was scarcely studied, much less understood. So his letter to the Academy focused on uncertainties, although he stressed not just the social scientific uncertainties, but the physical scientific ones as well. Because there were enormous uncertainties about both climate change and its potential costs, policy makers should do nothing yet, he argued, but fund more research. Moreover, Schelling wasn’t certain that all the effects of warming would be bad. “The credible range of effects is extremely broad,” he wrote. “By the middle of the next century, we may

have a climate almost as different from today's as today's is from the peak of the last major glaciation. At the other extreme, we may only experience noticeable but not necessarily unfavorable effects around mid-century or later."<sup>24</sup> No one really knew.

Climate change wouldn't produce *new* kinds of climate, Schelling argued, but would simply change the distribution of climatic zones on Earth. This suggested an idea that climate skeptics would echo for the next three decades: that we could continue to burn fossil fuels without restriction and deal with the consequences through migration and adaptation. Schelling noted that past human migrations "to and throughout the new world subjected large numbers of people—together with their livestock, food crops, and culture—to drastically changed climate."<sup>25</sup>

Schelling acknowledged that these historic migrations occurred in eras with few or no national boundaries, very *unlike* the present, but he nevertheless suggested that adaptation would be the best response. We had time—Charney's group had said so—and during that time the cost of fossil fuel would probably go up, and so usage would go down. The slowing rate of fossil fuel use "will make adaptation to climate change easier and may permit more absorption of carbon into non-atmospheric sinks. It will also permit conversion to alternative energy sources at a lower cumulative carbon dioxide concentration, and it is likely that the sooner we begin the transition from fossil fuels the easier the transition will be." All this, he suggested, would happen naturally as market forces kicked in, so there was no need for regulation now.

Considering all the other uncertainties that Schelling emphasized, his faith in the free market could have been viewed as surprising, and his predictions have turned out to be entirely wrong: fossil fuel use has risen dramatically over the past three decades even as global warming has accelerated. But if his prediction were true, then there would be no need for government action. So this panel of worthies did not recommend a program of emissions reduction that might be phased in over time, despite their own acknowledgment that the sooner we began the transition the easier it would be. Instead, they counseled research:

In view of the uncertainties, controversies, and complex linkages surrounding the carbon dioxide issue, and the possibility that some of the greatest uncertainties will be reduced within the decade, it seems to most of us that *the near-term emphasis should be*

*on research, with as low a political profile as possible . . . We do not know enough to address most of these questions right now. We believe that we can learn faster than the problem can develop.*<sup>26</sup>

At least one scientist close to this work wasn't sure this prescription was right. John Perry, the chief staff officer for the Academy's Climate Research Board—and a meteorologist in his own right—was following the arguments closely, and penned an article for the journal *Climatic Change*. Its title gave away its argument, "Energy and Climate: Today's Problem, Not Tomorrow's."<sup>27</sup>

Everyone was focusing on doubling CO<sub>2</sub> in their models and analyses, but Perry sagely pointed out that this was just a convenient point of comparison. "Physically, a doubling of carbon dioxide is no magic threshold," he noted. "If we have good reason to believe that a 100 percent increase in carbon dioxide will produce significant impacts on climate, then we must have equally good reason to suspect that even the small increase we have already produced may have subtly altered our climate," he concluded. "Climate change is not a matter for the next century; we are most probably doing it right now."<sup>28</sup> Schelling's group had expressed the hope that we could "learn faster than the problem can develop." Perry countered, "The problem is already upon us; we must learn very quickly indeed."<sup>29</sup> Perry would be proven right, but Schelling's view would prevail politically. Indeed, it provided the kernel of the emerging skeptics' argument, and the eventual basis for the Reagan administration to push the problem off the political agenda entirely.

Congress was also looking into climate change. The 1978 National Climate Act had established a national climate research program, and Connecticut senator Abraham Ribicoff was planning to introduce an amendment to fund a closer look at CO<sub>2</sub>. It's a cliché that scientists always say that more research is needed, but Ribicoff concluded that more research *was* needed.<sup>30</sup> President Jimmy Carter was proposing a major effort to increase U.S. energy independence by developing "synfuels"—liquid fuels made from coal, oil shales, and tar sands—and scientific experts had warned that this could accelerate CO<sub>2</sub> accumulation. Ribicoff's amendment authorized the National Academy of Sciences to undertake a comprehensive study of CO<sub>2</sub> and climate.<sup>31</sup> While the formal charge to the new committee was not formulated until June of the following year, a committee was already in place by October 1980, with Bill Nierenberg as its chair.

Nierenberg seems to have done a certain amount of groundwork, if not



actual lobbying, for the job. In August 1979, as the Charney group was compiling its conclusions, John Perry had already been pondering the follow-up. Following normal Academy patterns, Perry suggested to members of the Climate Research Board that the new committee should not undertake new research, but simply review the adequacy and conclusions of existing work.<sup>32</sup> Nierenberg disagreed and argued for a much broader view. He thought the Academy should undertake a comprehensive, integrated assessment of all aspects of the problem, and that the members of the committee should be chosen with more than the usual care.<sup>33</sup> They were. They included Tom Schelling, and another who would support his views, Yale economist William Nordhaus.

Most National Academy reports are written collectively, reviewed by all the committee members, and then reviewed again by outside reviewers. Changes are made by the authors of the various sections and by the chairperson, and the report is accepted and signed by all the authors. An Executive Summary, or synthesis, sometimes written by the chairperson, sometimes by Academy staff, is also reviewed to ensure that it accurately reflects the contents of the study. That didn't happen here. The Carbon Dioxide Assessment Committee—chaired by Bill Nierenberg—could not agree on an integrated assessment, so they settled for chapters that were individually authored and signed. The result, *Changing Climate: Report of the Carbon Dioxide Assessment Committee*, was really two reports—five chapters detailing the likelihood of anthropogenic climate change written by natural scientists, and two chapters on emissions and climate impacts by economists—which presented very different impressions of the problem. The synthesis sided with the economists, not the natural scientists.

The chapters written by the natural scientists were broadly consistent with what other natural scientists had already said. No one challenged the basic claim that warming would occur, with serious physical and biological ramifications. Revelle's chapter on sea level rise warned of the possible disintegration of the West Antarctic Ice Sheet, which "would release about 2 million km<sup>3</sup> of ice before the remaining half of the ice sheet began to float. The resulting worldwide rise in sea level would be between 5 and 6 m[eters]."<sup>34</sup> The likely result: "The oceans would flood all existing port facilities and other low-lying coastal structures, extensive sections of the heavily farmed and densely populated river deltas of the world, major portions of the state[s] of Florida and Louisiana, and large areas of many of the world's major cities."<sup>35</sup>

How quickly could such a disaster occur? Total disintegration of that ice sheet would take a long time, perhaps two hundred to five hundred years, but smaller effects might begin much sooner. If temperature increases of 2°C to 3°C were achieved by midcentury, thermal expansion alone would produce seventy centimeters of sea level rise, to which one could add another two meters by 2050 or so if the ice sheet began to fail. Whether fast or slow, “disintegration of the West Antarctic Ice Sheet would have . . . far-reaching consequences.”<sup>36</sup>

Other chapters addressed the impacts on climate, water availability, marine ecosystems, and more. The physical scientists allowed that many details were unclear—more research *was* needed—but they broadly agreed that the issue was very serious. When the chapters were boiled down to their essence, the overall conclusion was the same as before: CO<sub>2</sub> had increased due to human activities, CO<sub>2</sub> will continue to increase unless changes are made, and these increases will affect weather, agriculture, and ecosystems. None of the physical scientists suggested that accumulating CO<sub>2</sub> was not a problem, or that we should simply wait and see.

But that’s precisely what the economists’ chapters, as well as the synthesis, argued. The report’s first chapter, written by Nordhaus, National Research Council staff member Jesse Ausubel, and a consultant named Gary Yohe (an economics professor at Wesleyan University), focused on future energy use and carbon dioxide emissions. The long and detailed chapter began by acknowledging the “widespread agreement that anthropogenic carbon dioxide emissions have been rising steadily, primarily driven by the combustion of fossil fuels.” Their focus, however, was not so much on what was known, but on what was not known: the “enormous uncertainty” beyond 2000, and the “even greater uncertainty” about the “social and economic impacts of possible future trajectories of carbon dioxide.”<sup>37</sup>

Using a probabilistic scenario analysis, they projected atmospheric CO<sub>2</sub> levels to 2100, using various assumptions regarding energy use, costs, and increased economic efficiencies. The range of possible outcomes was large, but they considered the most likely scenario to be CO<sub>2</sub> doubling by 2065.<sup>38</sup> The economists acknowledged the “substantial probability that doubling will occur much more quickly,” including a 27 percent chance that it would occur by 2050, and admitted that it was “unwise to dismiss the possibility that a doubling may occur in the first half of the twenty-first century.” Yet they did just that.

What could be done to stop climate change? According to Nordhaus,

not much. The most effective action would be to impose a large permanent carbon tax, but that would be hard to implement and enforce.

A significant reduction in the concentration of CO<sub>2</sub> will require very stringent policies, such as hefty taxes on fossil fuels . . . The strategies suggested later [in the report] by Schelling—climate modification or simply adaptation to a high CO<sub>2</sub> and high temperature world—are likely to be more economical ways of adjusting . . . Whether the imponderable side effects on society—on coastlines and agriculture, on life in high latitudes, on human health, and simply the unforeseen—will in the end prove more costly than a stringent abatement of greenhouse gases, we do not now know.<sup>39</sup>

Rather than confront their own caveat that changes might happen much sooner than their model predicted—and thus be much more costly than prevention—the economists assumed that serious changes were so far off as to be essentially discountable.

Schelling picked up the thread of this argument in the final chapter of the report, where the economists' reframing of the climate question became explicit. Natural scientists were not worried about climate change per se—because scientists knew climate was naturally variable—but about rapid, unidirectional change forced by carbon dioxide. Such change would seriously challenge ecosystems that couldn't adapt in only a few decades, as well as human infrastructure. But Schelling rejected this view, insisting that the real issue *was* climate change and that the impact of carbon dioxide needed to be assessed together with "other climate-changing activities," such as dust, land use changes, and natural variability. It was wrong to single out CO<sub>2</sub> for special consideration.

Common sense might suggest that if carbon dioxide is the cause of climate change, then controlling it is the obvious solution, but Schelling rejected this view, too. He insisted that it was a mistake to assume a "preference for . . . dealing with causes rather than symptoms . . . It would be wrong to commit ourselves to the principle that if fossil fuels and carbon dioxide are where the problem arises, that must also be where the solution lies."<sup>40</sup> It might be best just to treat the symptoms through deliberate weather modification or to adapt.

Schelling's attempt to ignore the cause of global warming was pretty

peculiar. It was equivalent to arguing that medical researchers shouldn't try to cure cancer, because that would be too expensive, and in any case people in the future might decide that dying from cancer is not so bad. But it was based on an ordinary economic principle—the same principle invoked by Fred Singer when discussing acid rain—namely, discounting. A dollar today is worth more to us than a dollar tomorrow and a lot more than a dollar a century from now, so we can “discount” faraway costs. This is what Schelling was doing, presuming that the changes under consideration were “beyond the lifetimes of contemporary decision-makers.”<sup>41</sup> Not only did we not know how much energy future populations would use, and therefore how much CO<sub>2</sub> they would produce, we didn't know how they would live, how mobile they would be, what technologies they would have at their disposal, or even what climates they might prefer.

Schelling had a point: if changes were a century away, then it would be impossible to predict how troubling they would be. Perhaps by 2100 everyone would be living indoors, with agriculture pursued in controlled hydroponic environments. The rub was that most of the physical scientists on the panel did *not* think that trouble was more than a century away. Most of them thought that significant changes were much closer, and that carbon dioxide *was* the problem.

So Nierenberg's committee had produced a report with two quite different views: the physical scientists viewed accumulating CO<sub>2</sub> as a serious problem; the economists argued that it wasn't. And the latter view framed the report—providing its first and last chapters. A fair synthesis might have laid out the conflicting views and tried to reconcile them or at least account for the differences. But this synthesis didn't. It followed the position advocated by Nordhaus and Schelling. It did not disagree with the scientific facts as laid out by Charney, the Jaxons, and all the other physical scientists who had looked at the question, but it rejected the interpretation of those facts as a problem. “Viewed in terms of energy, global pollution, and worldwide environmental damage, the ‘CO<sub>2</sub> problem’ appears intractable,” the synthesis explained, but “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.”<sup>42</sup>

Some climatic effects—like serious sea level rise—might make some areas of the world uninhabitable, but this could be addressed through migration. Nierenberg stressed that people had often migrated in the past, and when they did, they often had to adapt to new climates. “Not only have

people moved” Nierenberg noted, “but they have taken with them their horses, dogs, children, technologies, crops, livestock, and hobbies. It is extraordinary how adaptable people can be.”<sup>43</sup> Thus Nierenberg’s argument was the same as Schelling’s had been in 1980: research, not policy action, was necessary, and that research should take the lowest possible political profile. Vern Suomi had admonished that a “wait and see” attitude was likely to be untenable, but that’s exactly what Nierenberg’s committee recommended.

The fact is, historical mass migrations had been accompanied by massive suffering, and typically people moved under duress and threat of violence. So Nierenberg’s cavalier tone, and suggestion that these migrations were essentially benign, flew in the face of historical evidence. At least one reviewer recognized this. Alvin Weinberg, a physicist who had led the Oak Ridge National Laboratory for nearly twenty years, wrote a scathing eight-page critique. Weinberg was one of the first physicists to recognize the potential severity of global warming, arguing in 1974 that climate impacts might limit our use of fossil fuels before they were even close to running out.<sup>44</sup> This perspective meshed with his advocacy of nuclear power, which he believed was the only energy source that could enable better living conditions for all humanity, an opinion he and Nierenberg shared. But Weinberg was outraged by what he read in Nierenberg’s report.

The report was “so seriously flawed in its underlying analysis and in its conclusions,” Weinberg wrote, that he hardly knew where to begin. The report flew in the face of virtually every other scientific analysis of the issue, yet presented almost no evidence to support its radical recommendation to do nothing. Improvements in irrigated agriculture would no doubt occur, but could they be put in place fast enough and on a sufficient scale, particularly in poor countries? The report provided no evidence. As for migration, “does the Committee really believe that the United States or Western Europe or Canada would accept the huge influx of refugees from poor countries that have suffered a drastic shift in rainfall pattern?” Weinberg demanded. “I can’t for the life of me see how historic migrations, which generally have taken place when political boundaries were far more permeable than they are now, can tell us anything about migrations 75 to 100 years from now when large areas lose their capacity to support people. Surely there will be times of trouble then.”<sup>45</sup>

Weinberg wasn’t alone in realizing that the claims made in the synthesis were not supported by the analysis presented in the body of the report. Two other reviewers made the same point, although with less passion.<sup>46</sup>

Yet these reviewers were also ignored. How was it possible for the reviewers' comments to be ignored, and for a report to be issued in which the synthesis was at odds with the report it claimed to synthesize and in which major claims were unsupported by evidence? One senior scientist many years later answered this way: "Academy review was much more lax in those days." But why didn't anyone object after the report was released? This same scientist: "We knew it was garbage so we just ignored it."<sup>47</sup>

But the Nierenberg report didn't go out with the morning trash. It was used by the White House to counter scientific work being done by the Environmental Protection Agency. The EPA prepared two reports of its own, both of which concluded that global warming would be serious, and that the nation should take immediate action to reduce coal use.<sup>48</sup> When the EPA reports came out, White House Science Advisor George Keyworth used Nierenberg's report to refute them. In his monthly report for October prepared for Ed Meese, Keyworth wrote, "The Science Advisor has discredited the EPA reports . . . and cited the NAS report as the best current assessment of the CO<sub>2</sub> issue. The press seems to have discounted the EPA alarmism and has taken the conservative NAS position as the wisest."<sup>49</sup>

Keyworth was right. The press would indeed take the "conservative" position. A *New York Times* reporter put it this way: "The Academy found that since there is no politically or economically realistic way of heading off the greenhouse effect, strategies must be prepared to adapt to a 'high temperature world.'"<sup>50</sup> But the Academy hadn't *found* that; the committee had *asserted* it. And it wasn't the Academy; it was Bill Nierenberg and a handful of economists.

Was it just coincidence—a meeting of minds—that Nierenberg gave the White House just what it wanted? The historical record suggests not. In meetings with the Climate Research Board, Energy Department officials had told Academy members that they "did not approve of . . . speculative, alarmist, 'wolf-crying' scenarios."<sup>51</sup> They simply wanted "guidance on the on-going research program."<sup>52</sup> Tom Pestorius, the senior policy analyst at the White House Office of Science and Technology who was a White House liaison to the Acid Rain Peer Review, was involved here, too. There was no need for alarm, he told John Perry, who reported this back to Nierenberg's committee, because "technology will ultimately be the answer to the problems of providing energy and protecting the environment."<sup>53</sup>

Nierenberg's CO<sub>2</sub> and climate report pioneered all the major themes behind later efforts to block greenhouse gas regulation, save one. Nieren-

berg didn't deny the legitimacy of climate science. He simply ignored it in favor of the claims made by economists: that treating symptoms rather than causes would be less expensive, that new technology would solve the problems that might appear so long as government didn't interfere, and that if technology couldn't solve all the problems, we could just migrate. In the two decades to come, these claims would be heard again and again.

But just as Alvin Weinberg hadn't bought these arguments, not all economists did, either. A handful of economists in the late 1960s had realized that free market economics, focused as it was on consumption growth, was inherently destructive to the natural environment and to the ecosystems on which we all depend. The Earth doesn't have infinite resources, and, as we saw in chapter 3 with acid rain, it doesn't have an infinite ability to withstand pollution. Nierenberg hadn't put any of *these* economists on his panel. So just as Nierenberg had built his Executive Summary around a one-sided view of climate change, he'd built it around a one-sided view of economics.

Nierenberg gave the administration everything it wanted: a report that presented a united front rather than the real differences of opinion between the social and physical scientists, insisted that no action was needed now, and concluded that technology would solve any problems that did, in the future, emerge. The government did not need to do anything—except fund research.

*Meeting the “Greenhouse Effect”  
with the “White House Effect”*

Two crucial developments during the presidential campaign year of 1988 changed climate science forever. The first was the creation of the Intergovernmental Panel on Climate Change. The second was the announcement by climate modeler James E. Hansen, director of the Goddard Institute for Space Studies, that anthropogenic global warming had begun. An organized campaign of denial began the following year, and soon ensnared the entire climate science community.

In November 1987 Colorado senator Tim Wirth had sponsored a hearing on climate in which Hansen had testified, but it had been widely ignored by the nation's media establishment.<sup>54</sup> A drought was setting in across the United States, however, and by the following summer, the nation was in crisis. The year 1988 proved to be one of the hottest and driest in U.S. history. As 40 percent of the nation's counties were affected, and as

crops failed, livestock died, and food prices rose, people were beginning to wonder if perhaps global warming was not so far off after all. Popular and media interest in climate soared. In June, Wirth tried again. Senator J. Bennett Johnston of Louisiana delivered the opening statement of the hearing:

Today, as we experience 101°[F] temperatures in Washington, DC, and the soil moisture across the midwest is ruining the soybean crops, the corn crops, the cotton crops, when we're having emergency meetings of the Members of the Congress in order to figure out how to deal with this emergency, then the words of Dr. Manabe and other witnesses who told us about the greenhouse effect are becoming not just concern, but alarm.<sup>55</sup>

Hansen was the star of the show. He testified about some new research at the Goddard Institute for Space Studies, showing that there had been a warming of just about half a degree Celsius—or one degree Fahrenheit—relative to the 1950–1980 average. The probability that this could be explained by natural events was only 1 percent. “The global warming is now large enough that we can ascribe with a high degree of confidence a cause and effect relationship to the greenhouse effect,” Hansen told the committee.<sup>56</sup>

His team had also modeled the increase of carbon dioxide and other trace gases according to three “emissions scenarios.” The scenarios were not intended to be predictions of the actual course of human carbon emissions; they were what-if scenarios bracketing likely rates of future emissions and their consequences. One scenario imagined rapid reduction of fossil fuel use after 2000, which reduced future warming. The other two—more realistic scenarios—raised the Earth's global mean temperature rapidly. Within twenty years, it would be higher than at any time since the warmest previous interglacial period then known, which ended about 120,000 years ago.<sup>57</sup>

This time, major newspapers across the country covered the hearings. The *New York Times* put Hansen's testimony on the front page; suddenly he was the leading advocate for doing something about the global warming.<sup>58</sup> Some colleagues, uncomfortable with all the media attention—and maybe a bit jealous, too—attacked Hansen for going too far, thinking he had discounted the significant uncertainties that still remained. On the other hand, Hansen had captured attention as no one else had. Moreover,



most of the scientific community *did* believe that one could not endlessly raise atmospheric concentrations of greenhouse gases without a climatic response. It was basic physics. Still, Hansen's claim of detection was unexpected, and seemed perhaps premature.<sup>59</sup>

DURING THE FIVE-YEAR INTERREGNUM between the release of the Nierenberg report and Hansen's powerful testimony, atmospheric scientists had been busy with other things. They had discovered the Antarctic ozone hole, investigated it, and explained its cause. They had also demonstrated the existence of global ozone depletion through the work of the Ozone Trends Panel. Certain scientists, including NASA's Bob Watson, began to think that something like the Ozone Trends Panel was needed for global warming, too. This became the Intergovernmental Panel on Climate Change.

Bert Bolin, the man who had first warned about acid rain in Europe, thought that Hansen's temperature data hadn't been "scrutinized well enough," and accepted the task.<sup>60</sup> He divided the panel into three working groups. The first would produce a report reflecting the state of climate science. The second would assess the potential environmental and socio-economic impacts. The third would formulate a set of possible responses. The scientists set themselves a deadline of 1990 for their first assessment: a very short time given their intent to involve more than three hundred scientists from twenty-five nations.<sup>61</sup>

The political pressure generated by the June hearings also caused presidential candidate, and sitting vice president, George H. W. Bush to promise to counter the "greenhouse effect with the White House effect" by bringing the power of the presidency to bear on the problem.<sup>62</sup> After his inauguration as forty-first president of the United States in January 1989, he sent his secretary of state, James Baker, to the first IPCC meeting, and had the Federal Coordinating Council for Science, Engineering, and Technology's Committee on Earth Sciences outline a proposed U.S. Global Climate Change Research initiative for the fiscal year 1990 budget.<sup>63</sup> It was welcomed in the U.S. Senate, where the Committee on Commerce, Science, and Transportation had prepared a bill proposing the same thing: the National Global Change Research Act of 1989.<sup>64</sup> The United States, it seemed, was preparing to deal with anthropogenic climate change. As Gus Speth later recalled, "We thought we were on track to make real changes."<sup>65</sup> He underestimated the challenge.

*Blaming the Sun*

In 1984 Bill Nierenberg retired as director of the Scripps Institution of Oceanography, and joined the Board of Directors of the George C. Marshall Institute. As we saw earlier, Robert Jastrow had established the Institute to defend President Reagan's Strategic Defense Initiative against attack by other scientists. But by 1989, the enemy that justified SDI was rapidly disappearing. The Warsaw Pact had fallen apart, the Soviet Union itself was disintegrating, and the end of the Cold War was in sight. The Institute might have disbanded—its *raison d'être* disappeared—but instead, the old Cold Warriors decided to fight on. The new enemy? Environmental “alarmists.” In 1989—the very year the Berlin Wall fell—the Marshall Institute issued its first report attacking climate science. Within a few years, they would be attacking climate scientists as well.

Their initial strategy wasn't to deny the fact of global warming, but to blame it on the Sun. They circulated an unpublished “white paper,” generated by Jastrow, Seitz, and Nierenberg and published as a small book the following year, entitled “Global Warming: What Does the Science Tell Us?”<sup>66</sup> Echoing the tobacco industry strategy, they claimed that the report would set the record straight on global warming. The Institute's Washington office staff contacted the White House to request the opportunity to present it. Nierenberg gave the briefing himself, to members of the Office of Cabinet Affairs, the Office of Policy Development, the Council of Economic Advisers, and the Office of Management and Budget.<sup>67</sup>

The briefing had a big impact, stopping the positive momentum that had been building in the Bush administration. “I was impressed with the report,” said one member of the cabinet affairs office. “Everyone has read it. Everyone takes it seriously.” Another ruminated, “It is well worth listening to. They are eminent scientists. I was impressed.”<sup>68</sup> White House chief of staff John Sununu—a nuclear engineer by training—was particularly taken. Stanford University's Stephen Schneider lamented, “Sununu is holding the report up like a cross to a vampire, fending off greenhouse warming.”<sup>69</sup> Meanwhile, no one had invited Bert Bolin to the White House. Perhaps he hadn't known to ask to be invited.

The central claim of the Marshall Institute report was that the warming that Hansen and others had found didn't track the historical increase in CO<sub>2</sub>. The majority of the warming had been prior to 1940—prior to

the majority of the carbon dioxide emissions. Then there was a cooling trend through 1975, and a return to warming. Since the warming didn't parallel the increase in CO<sub>2</sub>, it must have been caused, they claimed, by the Sun.<sup>70</sup>

Drawing on sunspot and carbon-14 data from tree rings, they argued that the Sun had entered a period of higher energy output during the nineteenth century, and that this solar output increase (of about 0.3 percent) was responsible for the climate warming to date. They also contended that the data showed a two-hundred-year cycle, so the warming trend was almost over, and things would soon begin to cool off. "If the correlation between solar activity and global temperatures also continues, a trend toward a cooler planet can also be expected in the 21st century as a result of natural forces of climate change."<sup>71</sup>

Had there been cooling between 1940 and 1975? Yes, but the Marshall report misrepresented it. The Institute's source for their diagram was an article by Hansen's team, so it looked eminently credible.<sup>72</sup> It looked like they were relying on peer-reviewed science. But Jastrow, Nierenberg, and Seitz had cherry-picked the data—using only one diagram out of six that were relevant. They had shown their readers only the top piece of figure 5 (see next page). What Hansen and his group had done was to explore the role of various "forcings"—the different causes of climate change. One was greenhouse gases, a second was volcanoes, and the third was the Sun. Hansen's team had done what scientists are supposed to do—objectively considered all the known possible causes.

Then they asked, What cause or combination of causes best explains the observations? The answer was all of the above. "CO<sub>2</sub> + volcanoes + Sun" fit the observational record best. The Sun did make a difference, but greenhouse gases did, too. The observed climate of the twentieth century was a product of all three forcings, but since Jastrow, Seitz, and Nierenberg had shown their readers only the top portion of Hansen's figure, they'd made it appear as if only the Sun mattered. The warming prior to 1940 probably was the effect of a nineteenth-century increase in solar output, but not the increase that had started in the mid-1970s. There hadn't been any solar output increase in the mid-twentieth century, so only CO<sub>2</sub> explained the recent warming.

There was an even larger problem with the Marshall analysis that climate modeler Steven Schneider pointed out. If Jastrow and company were right that the climate was extremely sensitive to small changes in solar

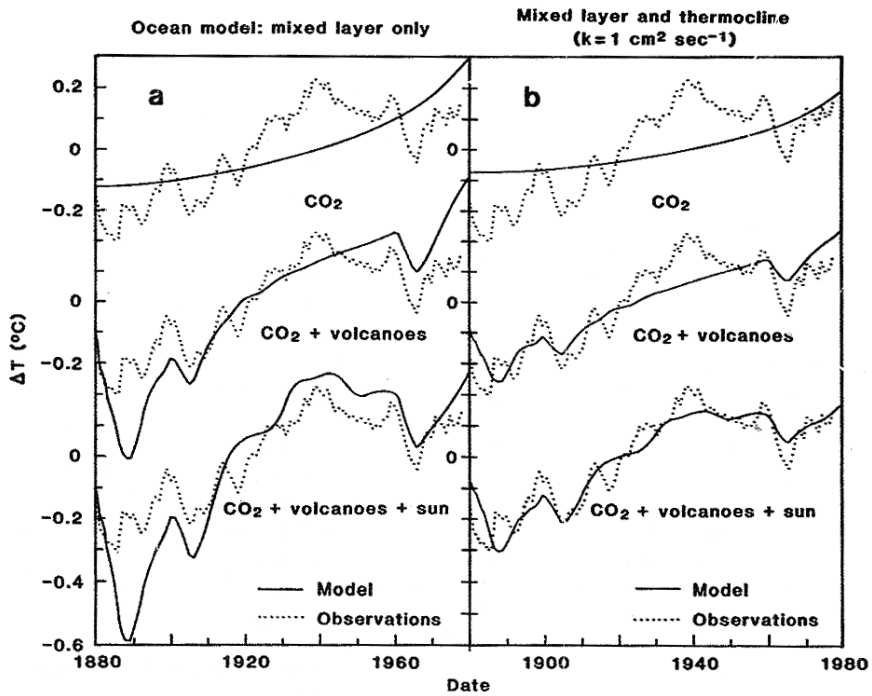


Fig. 5, Global temperature trend obtained from climate model with sensitivity  $2.8^{\circ}\text{C}$  for doubled  $\text{CO}_2$ . The results in (a) are based on a 100-m mixed-layer ocean for heat capacity; those in (b) include diffusion of heat into the thermocline to 1000 m. The forcings by  $\text{CO}_2$ , volcanoes, and the sun are based on Broecker (25), Lamb (27), and Hoyt (48). Mean  $\Delta T$  is zero for observations and model.

This set of charts was part of an article by James E. Hansen at the Goddard Institute for Space Studies, showing (left side) model results for an “Earth” with only very shallow oceans exchanging heat with the atmosphere, and (right side) oceans with much deeper mixing of heat. Hansen’s team argued that the bottom right image best reflected the behavior of the real Earth—with ocean mixing to 1,000 meter depth, solar irradiance, volcanic dust and aerosols, and  $\text{CO}_2$  all playing roles. The Marshall Institute’s version included only the top left portion of the diagram, leaving the impression that  $\text{CO}_2$  didn’t matter. From J. Hansen et al., “Climate Impact of Increasing Atmospheric Carbon Dioxide,” *Science* (28 August 1981): 963. Reprinted with permission from AAAS.

output, then it meant that the climate would also be extremely sensitive to small changes in greenhouse gases. Schneider argued,

If only a few tenths of a percent change in solar energy were responsible for the [observed] .5 C long trend in climate over the past century, then this would suggest a planet that is relatively sensitive to small energy inputs. The Marshall Institute simply can't have it both ways: they can't argue on the one hand that small changes in solar energy output can cause large temperature changes, but that comparable changes in the energy input from greenhouse gases will not also produce comparable large signals. Either the system is sensitive to large scale radiative forcing or it is not.<sup>73</sup>

Sensitivity cuts both ways. And as physicists, Jastrow, Seitz, and Nierenberg would of course have known this.

The Intergovernmental Panel on Climate Change published its first assessment of the state of climate science in May 1990. It reiterated the result that was by now familiar to anyone who had been following the issue: unrestricted fossil fuel use would produce a “rate of increase of global mean temperature during the next century of about .3°C per decade; this is greater than that seen over the past 10,000 years.”<sup>74</sup> Global warming from greenhouse gases would produce changes unlike what humans had ever seen before.

The IPCC explicitly addressed—and rejected—the Marshall Institute argument for blaming the Sun. The upper limits on solar variability, they explained, are “small compared with greenhouse forcing and even if such a change occurred over the next few decades, it would be swamped by the enhanced greenhouse effect.”<sup>75</sup>

But the IPCC's refutation didn't matter to the Marshall Institute. In 1991, they reiterated their argument in a longer version, and in October 1992 Bill Nierenberg took it on the road to the World Petroleum Congress in Buenos Aires, where he launched a full frontal attack on the IPCC. Nierenberg insisted that global temperatures would increase at most by 1°C by the end of the twenty-first century, based on a straight linear projection of twentieth-century warming. Bert Bolin confronted him directly, pointing out that greenhouse gas emissions were increasing *exponentially*, not linearly. Add to this the time lag induced by the oceans—which Jule Charney had warned about a decade earlier—and warming would accelerate over time.

In his memoir, Bolin called Nierenberg's conclusion "simply wrong."<sup>76</sup> A less polite man would have said something far worse. If Nierenberg had been a journalist, one might suppose he was just confused. But Nierenberg was no journalist; one longtime associate at Scripps once said she never knew a man who was more careful in choosing what he worked on and how he worked on it.<sup>77</sup> Meanwhile, the Cato Institute distributed an uncorrected version of the graph printed in the original Marshall Institute white paper—the one that showed only the top part of Hansen's graph.<sup>78</sup> Given all the efforts the climate scientists had made to set the record straight, it's not plausible that this was simply a mistake.

Moreover, they were proud of the results. In a February 1991 letter to the vice president of the American Petroleum Institute, Robert Jastrow crowed, "It is generally considered in the scientific community that the Marshall report was responsible for the Administration's opposition to carbon taxes and restrictions on fossil fuel consumption." Quoting *New Scientist* magazine, he reported that the Marshall Institute "is still the controlling influence in the White House."<sup>79</sup>

Fred Singer would push their efforts one step further.

### *The Attack on Roger Revelle*

While Jastrow, Seitz, and Nierenberg were broadcasting their "blame the Sun" claim, Fred Singer was preparing to attack climate science in a different way: by claiming that Roger Revelle had changed his mind about global warming. In addition to his role in helping to launch the Keeling Curve, Revelle had played another crucial role in the history of climate science, as mentor to Al Gore. Gore had studied with Revelle in the 1960s at Harvard, and it was well-known that Gore's concern about climate change stemmed from his tutelage under Revelle. If Revelle no longer considered global warming worrisome, this would be news indeed. It would also embarrass Gore, who was running his 1992 presidential campaign on environmental themes.

On February 19, 1990, the eighty-one-year-old Revelle had presented a paper entitled "What Can We Do About Climate Change?" at the American Association for the Advancement of Science meeting in New Orleans. Research and observations over the next ten to twenty years "should give us a much better idea of the likely magnitude of atmospheric and oceanic warming during the twenty-first century," he noted.<sup>80</sup> In the meantime,

there were six approaches that could be taken to reduce future warming: emphasizing natural gas over coal and oil, conservation, substitution of non-fossil energy sources, carbon sequestration by stimulating phytoplankton production, increasing atmospheric reflection through artificial intervention, and expanding forests. Revelle had lately developed an interest in the possibility that high-latitude (or “boreal”) forests might expand as the Earth warmed, removing carbon dioxide from the atmosphere and preventing some of the warming. He thought this expansion might remove 2.7 billion tons of carbon per year, roughly half the total contributed by fossil fuel combustion each year.<sup>81</sup> This wouldn’t be negligible—it might even be the negative feedback that the Charney panel had looked for but never found—and he thought more research *was* needed.

Revelle’s discussion of mitigation strategies—conservation, nuclear power, boreal forests, etc.—would have made no sense if he didn’t think there was something to mitigate against. Read in full, his talk clearly demonstrates that he believed the prudent step was to begin to switch to nuclear power and natural gas and improve energy conservation, while continuing research. Like all good scientists, Revelle was careful not to overstate his claims. He knew as well as anyone that there were still important uncertainties, and perhaps because he was intrigued by the prospect that boreal forests might delay warming significantly, he’d started his talk with this potentially ambiguous statement: “There is a good but by no means certain chance that the world’s average climate will become significantly warmer during the next century.”<sup>82</sup>

That gave Fred Singer the opening he needed. Singer approached Revelle after the talk about collaborating on an article for the *Washington Post*. The historical record doesn’t tell us exactly what the article was supposed to be about, and had Revelle stayed healthy, he might have left a fuller record. But on his way back to La Jolla, Revelle suffered a massive heart attack. He went straight from the airport to the hospital, where he underwent a triple-bypass operation.

Revelle didn’t recover quickly. After finally returning home in March, he was forced back to the hospital for an emergency hernia operation. Then he contracted a severe infection and spent another six weeks in the hospital. When he finally returned home in May, he was so weak that his personal secretary, Christa Beran, and Justin Lancaster, a graduate student with whom Revelle was teaching, arranged to limit his appointments to under a half hour.<sup>83</sup> Famous for his energy, Revelle was now falling asleep while dictating letters. He was not well.

The title of the paper that Singer would later publish, with Revelle as coauthor, was “What To Do about Greenhouse Warming: Look Before You Leap,” but, given the state of his health, it’s not clear how closely Revelle was able to look at the various drafts that Singer sent him, or how closely he checked that Singer had made the changes he suggested. Revelle had never been good at saying no to people; one of Revelle’s closest colleagues, oceanographer Walter Munk, admits that “Roger often leapt before he looked.”<sup>84</sup>

What we do know from Revelle’s papers at Scripps Institution of Oceanography is that Singer sent three drafts of their proposed article during March, while Revelle was still in the hospital. We also know that something about the paper clearly bothered Revelle. Christa Beran later recalled that whenever Singer sent him a draft, Revelle buried it under piles of paper on his desk. When Singer called, Beran would dig up the draft and put it on top, and Revelle would bury it again. Beran wondered why, and Revelle, she recalled later in a legal affidavit, told her, “Some people don’t think Fred Singer is a very good scientist.”<sup>85</sup>

Singer had made himself an unpopular figure in the scientific mainstream by attacking fellow scientists over acid rain and ozone, so perhaps after having said yes to Singer at the AAAS meeting in New Orleans, Revelle was regretting it, hoping that if he ignored the paper, it would go away. But Singer was not one to go away.

While Singer was trying to get Revelle to review the drafts, he published an article on his own in the journal *Environmental Science and Technology*, with essentially the same title, “What To Do about Greenhouse Warming.” Singer echoed the Marshall Institute’s arguments, implying that scientists just didn’t know what had caused the warming of the twentieth century. “There is major uncertainty and disagreement about whether this increase [in CO<sub>2</sub>] has caused a change in the climate during the past 100 years; observations simply don’t fit the theory,” he insisted. Of course there was disagreement—the Marshall Institute had generated it—but not among climate scientists. The IPCC had clearly stated that the unrestricted fossil fuel use would produce a “rate of increase of global mean temperature during the next century of about .3 C per decade; this is greater than that seen over the past 10,000 years.”<sup>86</sup> Singer rejected this, asserting instead that “the scientific base for [greenhouse warming] includes some facts, lots of uncertainty, and just plain ignorance.” He concluded emphatically, “*The scientific base for a greenhouse warming is too uncertain to justify drastic action at this time.*”<sup>87</sup> This, of course, was precisely what he had said about



acid rain. And ozone depletion. It was easy to see why many working scientists didn't like Fred Singer. He routinely rejected their conclusions, suggesting that he knew better than they did.

In February 1991, Singer visited Scripps. In one multihour meeting, Singer and Revelle went over the paper, which was already set in galleys. There was at least one point of contention between the two, and it was a big one: what *was* the climate sensitivity to carbon dioxide? The galleys that Singer gave to Revelle to review asserted, "Assume what we regard as the most likely outcome: A modest average warming in the next century of less than one degree Celsius, well below the normal year to year variation."<sup>88</sup>

This was completely inconsistent with what the Jasons had said, what Charney's panel had said, and what the IPCC had said. No one in the climate community was asserting that the climate change from increased greenhouses gases would be no different from normal year-to-year variation. In fact, the IPCC had said just the opposite. Revelle apparently crossed out "less than one degree" and wrote in the margin next to it: "one to three degrees."<sup>89</sup>

This might not seem like a big difference, but it was. One to three degrees fell within the mainstream view, and clearly *outside* the range of the natural climate variability of the past few hundred years. This was the key point: would warming lead us into a new man-made climate regime, unlike anything we had seen before? Revelle (and thousands of climate scientists) said yes; Singer said no.

Singer finessed the disagreement by dropping numbers altogether. The sentence as published read, "Assume what we regard as the most likely outcome: A modest average warming in the next century well below the normal year to year variation."<sup>90</sup> The paper contradicted what Revelle had written in the margin, and asserted that there was no likelihood of significant warming. What little change would occur would be not noticeably different from natural variation. Singer had prevailed, and it looked as if Revelle had agreed.

The paper was published later that year in *Cosmos*, the journal of the elite Washington Cosmos Club, founded in 1878 (and that only opened its doors to women in 1988 when forced to by the threat of an antidiscrimination suit). Revelle was listed as second author.<sup>91</sup> There was also a third author: Chauncey Starr, the physicist we met in chapter 3 casting doubt on the reality of acid rain, and in chapter 5 arguing for radiation hormesis—that radiation is good for you.<sup>92</sup>

Did Roger Revelle agree to this final version? We will never know for

sure, because in July, Revelle suffered a fatal heart attack, but it's hard to believe that he would have—at least, not if he were in good health and clear of mind—and no one close to him did believe that he had.

Scientists already knew from paleoclimate data that the lowest possible climate sensitivity to doubled CO<sub>2</sub> was 1.5°C. We knew from the geological record that CO<sub>2</sub> levels had varied in the past, and temperatures had varied in a manner consistent with an overall sensitivity of not less than 1.5°C for CO<sub>2</sub> doubling. Revelle—a geologist by training—knew this very well. He had cotaught a course at Scripps with Justin Lancaster that included discussion of this natural climate variation.

Lancaster later recalled that Revelle was embarrassed when the *Cosmos* paper was published.<sup>93</sup> But *Cosmos* wasn't a scientific journal—it wasn't peer reviewed—and it didn't have a very high circulation. Few scientists would have seen the article, much less paid much attention to it, so even had he been in good health, Revelle might well have just let it drop. Perhaps he would have thought it was “garbage” and just ignored it.

But as the 1992 election campaign got under way, the *Cosmos* article was not ignored. It was used to attack Senator Al Gore. The first salvo seems to have fired by Gregg Easterbrook in the July issue of the *New Republic*, and reiterated in August in the *Independent*. Criticizing Gore's new book, *Earth in the Balance*, Easterbrook sniffed indignantly that Gore had failed to mention that “before his death last year, Revelle published a paper that concludes, ‘the scientific base for a greenhouse warming is too uncertain to justify drastic action at this time.’”<sup>94</sup>

Those were *Singer's* words, not Revelle's. Singer had used them in his stand-alone 1990 paper, and again in 1991, in a book chapter questioning the existence of global warming and attacking the Intergovernmental Panel on Climate Change.<sup>95</sup> Revelle had said nothing like that in his AAAS talk. Moreover, it's customary in both academic and journalistic circles to credit the lead author of a paper. That, of course, was Fred Singer. Easterbrook might just as well have said he was quoting Chauncey Starr. Either Easterbrook was being sloppy or he was exploiting the Revelle connection for political purposes. After all, it was Revelle, not Singer or Starr, who was Gore's mentor.

Easterbrook's attack was picked up by conservative columnist George Will, who repeated it almost verbatim in a September 1992 column. “Gore knows that his former mentor at Harvard, Roger Revelle, who died last year, concluded: “The scientific base for greenhouse warming is too uncertain to justify drastic action at this time. There is little risk in delaying pol-

icy responses.’”<sup>96</sup> From there, it became part of the only vice-presidential debate of the campaign. Retired admiral James B. Stockdale, the running mate of Ross Perot, attacked Gore with the claim, again using the statement that had originated in Singer’s 1990 article.<sup>97</sup>

The use of Revelle’s name to attack Al Gore infuriated the Revelle family, as well as his colleagues at Scripps. Revelle’s daughter, Carolyn Hufbauer, protested Will’s attack in an op-ed published just before the vice-presidential debate, September 13.<sup>98</sup> Two of Revelle’s closest colleagues at Scripps, oceanographer Walter Munk and physicist Edward Frieman, agreed with Hufbauer that Revelle’s views were being misrepresented. They wrote a letter to *Cosmos*, but the journal declined to publish it, so they published it in the journal *Oceanography*, along with the text of Revelle’s AAAS paper.<sup>99</sup> (Yet again, unscientific claims were being circulated broadly, but the scientists’ refutation of them was published where only fellow scientists would see it.)

Munk and Frieman explained that the *Cosmos* paper hadn’t been written by Revelle at all. “S. Fred Singer wrote the paper,” they explained, suggesting that “as a courtesy, [Singer] added Roger as a co-author based upon his willingness to review the manuscript and advise on aspects relating to sea-level rise.”<sup>100</sup>

More than a decade later, Munk was still angry about what he referred to as “Singer’s betrayal of Roger.”<sup>101</sup> But the person who fought longest and hardest to defend Revelle’s legacy—and paid the highest price—was Justin Lancaster. In that last year of Revelle’s life, Lancaster had seen him on nearly a daily basis. The two had taught a class together, and they shared a commitment to addressing policy questions. (This was something that most of the scientists at Scripps weren’t actually interested in; they just wanted to do pure science.) Lancaster felt he knew Revelle’s views as well as anyone.

Lancaster and his thesis advisor, Dave Keeling, wrote a letter to the *New Republic* challenging the Easterbrook article, but it was never published. For a second time, scientists close to Revelle were attempting to refute the misrepresentation, but their attempts to set the record straight were rejected by the journals that had published the misrepresentation in the first place. So Lancaster did what Munk and Frieman had done. He turned to the scientific community, who he figured did care about the truth. At the time, Lancaster was serving on the editorial board for a volume titled *A Global Warming Forum*, and Singer intended to republish the *Cosmos* piece there. Lancaster tried to get Singer to remove Revelle’s name from it, but Singer refused. A struggle among Singer, Lancaster, and the volume’s editorial staff ensued as Lancaster tried to remove Revelle’s name from the article; when the volume

was finally published in 1993, it contained a footnote on the first page pointing readers to Revelle's AAAS paper, now published in *Oceanography*.<sup>102</sup>

In October, Harvard held a memorial symposium for Revelle, the same month that the vice-presidential debate placed the *Cosmos* dispute in the national light. Originally the organizers had planned to have Singer present the now-infamous paper, but they'd also invited Walter Munk and the Revelle family. Given their objections to the piece, the organizers removed Singer from the program, hoping to prevent a confrontation. But it didn't work. Singer went anyway.

Walter Munk and Justin Lancaster complained about the *Cosmos* article, Munk apparently in his introductory remarks, and Lancaster in a statement that read in part: "Revelle did not write the *Cosmos* article and was reluctant to join it. Pressured rather unfairly at a very weak moment while recovering from heart surgery, Revelle finally gave in to the lead author." The chairman of the symposium allowed Singer to respond. Singer denied having pressured Revelle, insisting that the *Cosmos* paper was based on Revelle's AAAS paper, and he attacked Munk and Lancaster for their "politically inspired misrepresentations."<sup>103</sup>

Singer neglected to mention that the key sentence of the *Cosmos* paper—the one that had been loudly quoted in the press—came from his own 1990 paper. But Singer wasn't content with having made a scene at a symposium that was supposed to be celebrating Revelle's life and work. As Lancaster continued to publicly dispute Revelle's coauthorship of the paper, Singer filed a libel lawsuit against him. Lancaster had little money and fewer resources, but he tried to fight Singer, insisting that the facts were on his side. The only other person who could corroborate Lancaster's account, Revelle's secretary, Christa Beran, did. It wasn't enough. Singer's pockets were deeper than Lancaster's, and in 1994, Lancaster accepted a settlement that forced him to retract his claim that Revelle hadn't really been a coauthor, put him under a ten-year gag order, and sealed all the court documents.<sup>104</sup> (In 2007, he spoke to us. He now also has a Web site.)<sup>105</sup>

What did Roger Revelle really believe about global warming in 1991? We have looked closely at the records in Revelle's papers at Scripps, and can find only one other statement of his thoughts at the time. It's a short, apparently unpublished, introduction to a November 1990 meeting on climate variability. Revelle wrote:

There is good reason to expect that because of the increase of greenhouse gases in the atmosphere there will be a climate

warming. How big that warming will be is . . . very difficult to say. Probably somewhere between 2 and 5 degrees centigrade at the latitudes of the United States, probably a greater change in average temperature at higher latitudes and a lesser change at lower latitudes . . . Whatever climate change there is will have a profound effect on some aspects of water resources.<sup>106</sup>

The documentary record clearly shows that Roger Revelle did not change his mind. He believed that global warming was coming and it would have serious impacts on water resources. This, of course, is precisely what his colleagues said then and continue to say today. He also believed that the best way to address it was to shift our energy sources. Nowhere did he ever suggest that he considered that a “drastic” action. It seems to us, in fact, that he considered it pretty darn obvious.

The rest of the world did too, as leaders of governments and NGOs made plans to convene in Rio de Janeiro for the U.N. Earth Summit. In June 1992, 108 heads of state, 2,400 representatives of nongovernmental organizations, and more than 10,000 on-site journalists converged in Rio, along with 17,000 other individuals who would convene in a parallel NGO forum, to address the problem of anthropogenic climate change. Yet it was unclear whether President Bush would even attend. At the last minute, President George H. W. Bush flew to Rio de Janeiro to sign the U.N. Framework Convention on Climate Change, which committed its signatories to preventing “dangerous anthropogenic interference in the climate system.”<sup>107</sup> President Bush then pledged to translate the written document into “concrete action to protect the planet.”<sup>108</sup> By March 1994, 192 countries had signed on to the Framework Convention, and it came into force.

Like the Vienna Convention for the Protection of the Ozone Layer, the U.N. Framework Convention on Climate Change had no real teeth: it set no binding limits on emissions. It was an agreement in principle. Real limits would be determined later, in a protocol that would be eventually signed in Kyoto, Japan. And with the threat that real limitations would soon be enforced, the merchants of doubt redoubled their efforts.

### *Doubling Down on Denial*

Despite the best efforts of Jastrow, Seitz, Nierenberg, and Singer to create doubt, the scientific debate over the detection of global warming was

reaching closure. By 1992, Hansen's 1988 claim that warming was detectable no longer seemed bold. It seemed prescient. The only remaining issue really was whether we could prove that the warming was caused by human activities. As scientists had acknowledged many times, there are many causes of climate change, so the key question was how to sort out these various causes. Now that warming had been *detected*, could it be definitively *attributed* to humans?

"Detection and attribution studies" work by considering how warming caused by greenhouse gases might be different from warming caused by the Sun—or other natural forces. They use statistical tests to compare climate model output with real-life data. These studies were the most threatening to the so-called skeptics because they spoke directly to the issue of *causality*: to the social question of whether or not humans were to blame, and to the regulatory question of whether or not greenhouse gases need to be controlled. As these studies began to appear in the peer-reviewed literature, it's not surprising that Singer and his colleagues tried to undermine them. Having taken on the patriarch of climate change research, they went after one of its rising young stars: Benjamin Santer of the Program for Climate Model Diagnosis and Intercomparison at the Lawrence Livermore National Laboratory.

Santer had done his Ph.D. work in the 1980s at the University of East Anglia, England, where he had compared climate model results to observational data, using so-called Monte Carlo methods to make a rigorous statistical analysis.<sup>109</sup> Until this point, model comparisons had been mostly done qualitatively. Scientists looked at maps of model output and compared them to maps of real-life observations to identify similarities and differences. Santer and his Ph.D. supervisor, Tom Wigley (the director of the Climatic Research Unit at East Anglia, U.K.), thought statistical analysis offered more to climate science than such qualitative comparisons. Besides, other parameters—detailed patterns of surface pressure, precipitation, and humidity—might actually provide better tests of the models than global mean temperature. Depending on the driving force—greenhouse gases, volcanic dust, or the Sun—you'd expect different changes in some of these parameters.<sup>110</sup>

After finishing his thesis with Wigley, Santer was invited to the Max Planck Institute for Meteorology in Hamburg. One of the Institute directors was Klaus Hasselmann, a physicist who spent much of his spare time working on unification theory: the effort to merge the four known funda-

mental forces in the universe into a single field at extremely high energies like those that theoretically existed at the universe's first few moments of existence. This was pretty far from climate science, but Hasselmann had also made a number of major scientific contributions to climate questions. One of those was a paper in 1979 that proposed a new detection and attribution technique called "optimal fingerprinting."<sup>111</sup> The idea was derived from signal processing theory, and the paper was so technical, so elegant, and so laden with dense tensor field mathematics, that Santer at first didn't get it. Santer recalls it as "a thing of beauty. It was many years ahead of its time. I was just too dumb to understand it."<sup>112</sup>

Hasselmann's key insight was that climate scientists faced the same basic problem as communications engineers: how to detect a weak signal—the thing you're interested in—amid lots of noise that you don't care about. In climate science, the noise is caused by phenomena that are internal to the climate system, such as El Niño. The "signal" is something caused by things that are external to the Earth's natural climate system: the Sun, volcanic dust, or man-made greenhouse gases. Engineers had worked for a century to develop mathematical techniques to sort out signals from noise, but they were largely unknown to climate scientists. They also aren't simple to master.

Santer got started, but progress was slow. The results of his Ph.D. thesis hadn't been all that encouraging, either. He and Wigley had shown that some of the models used for the IPCC's first assessment had large errors in surface pressures; it was what scientists call a "negative result." Still, it was important to point out such errors, and based on this and his preliminary work with Hasselmann, he was offered a position at the Climate Model Intercomparison project at the Lawrence Livermore National Laboratory, in California. The program's founder, Lawrence Gates, believed that if models were to be used for policy purposes—and they obviously would be if climate policy were to be based partly on model forecasts—it was important to evaluate them to see whether they were reliable or not. Gates pioneered the idea of "benchmark experiments"—getting climate model centers around the world to perform exactly the same calculation with their models—to permit scientists to rule out differences in model design as an explanation for the differences in model performance. (Model benchmarking was a radical idea at the time; now it is standard procedure.) Gates also argued for making the results of these experiments widely available, so that model diagnosis became an activity of the entire climate science community, not just the responsibility of the modelers

themselves—who might not be entirely objective. The lab, in other words, was trying to make modeling more rigorous, more objective, and more transparent.

Santer had the good fortune to arrive at the lab not only in the middle of one of the first major model intercomparison projects, but also at a time when Livermore colleagues Karl Taylor and Joyce Penner were performing an innovative set of climate model experiments that considered not only greenhouse gases, which cause warming, but also sulfate aerosol particles, which generally cause cooling. The Taylor and Penner experiments clearly showed that human influences on climate were complex: changes in CO<sub>2</sub> and sulfate aerosols had distinctly different climate fingerprints.

Fingerprinting proved to be a powerful tool for studying cause-and-effect relationships. Up to that point, much of the scientific argument about the causes of climate change had gone like this: if greenhouse gases increased, then you would expect temperatures to increase, too. They had. So the prediction had come true—textbook scientific method. The problem with the textbook method, however, is that it's logically fallacious. Just because a prediction comes true doesn't mean the hypothesis that generated it is correct. Other causes could produce the same effect. To prove that greenhouse gases had caused climate change, you'd have to find some aspect of it that was *different* than if the cause were the Sun or volcanoes. You needed a pattern that was unique.

We saw in chapter 4 that V. Ramanathan, a prominent atmospheric scientist, had suggested one: the vertical structure of temperature.<sup>113</sup> If warming were caused by the Sun, then you'd expect the whole atmosphere to warm up. If warming were caused by greenhouse gases, however, the effect on the atmosphere would be different, and distinctive. Greenhouse gases trap heat in the lower atmosphere (so it warms up), while the reduced heat flow into the upper atmosphere causes it to cool. Collaborating with colleagues at the Max Planck Institute, and six other research institutions around the world, Santer started to look at the vertical variation of temperature.<sup>114</sup> Before they'd finished the work, Santer was asked to become the convening lead author for "Detection of Climate Change and Attribution of Causes," chapter 8 of the second IPCC assessment.

Nowadays, there's a lot of prestige associated with the IPCC, since they shared the 2007 Nobel Peace Prize, but back in 1994 most scientists considered it a distraction from their "real" work—doing basic research—and Bert Bolin was having a difficult time finding someone to take the lead on the detection and attribution chapter. In the spring of 1994, after some of



the other chapters were already started, Tim Barnett of the Scripps Institution of Oceanography called Santer to ask if he'd be willing to do it. Barnett had been one of two lead authors of the equivalent chapter in the First Assessment, and he convinced Santer that it would be a feather in his cap. Santer signed on.

The job of the convening lead author (this position is now called the coordinating lead author) is to produce an assessment of some aspect of climate science based on "the best scientific and technical information available."<sup>115</sup> This involves working together with other "lead authors" and "contributing authors" to agree on the structure and scope of the future chapter. Individual scientists are then assigned the task of drafting different sections of the chapter. Once all sections are drafted, the convening lead author and the lead authors attempt to hammer out a complete draft that's acceptable to the entire group. Santer's chapter ultimately had four lead authors, including his old mentor at East Anglia, Tom Wigley, Tim Barnett, and thirty-two additional contributing authors—in other words, thirty-six of the world's top climate scientists.<sup>116</sup>

The chapter 8 author group met in Livermore, California, in August 1994 to identify the key scientific areas that needed to be addressed. There were a total of twenty participants (from the United States, Canada, the United Kingdom, Germany, and Kenya). After this initial meeting, most of the author group's discussion took place by e-mail. Then, in October through November, Santer attended the first of three so-called drafting sessions, involving the lead authors and the convening lead authors of all chapters of the IPCC Working Group I Report.

The first drafting session convened in Sigtuna, Sweden, and Santer encountered his first challenge: a disagreement over whether the chapter should include a discussion of model and observational uncertainties. Since the topic was covered in other chapters, some authors thought it would be redundant to do it here, but Santer didn't think readers would search other chapters to find it, and in any case his panel would have no control over what was said in those chapters. Santer prevailed, and the published version contained about six pages of discussion of model and observational uncertainties.

Shortly after the Sigtuna meeting, chapter 8 went through an initial round of peer review. The "zeroth" draft was sent out to roughly twenty scientific experts in detection and attribution work, to all scientific contributors to the chapter, and to the lead authors of all other chapters of the report. After updating their chapters in response to the peer review comments, the

IPCC lead authors met for a second drafting session in March 1995 in the British seaside resort of Brighton. In May, a complete draft of the entire IPCC Working Group I Report, as well the Summary for Policymakers, was submitted for full “country review” by the governments participating in the IPCC. The governments chose reviewers—a mixture of scientists and laypeople—who were supposed to provide comments to the lead authors prior to the third drafting session, in Asheville, North Carolina, in July, but because Santer had been chosen so late as the convening lead author, this schedule didn’t quite work out for his group. Santer arrived in Asheville having yet to receive the government reviewers’ comments.

At the Asheville meeting, Santer presented the results of his fingerprint study of changes in the vertical structure of atmospheric temperatures, which by this point had been submitted to *Nature*.<sup>117</sup> One scientist present at the meeting reported that Santer’s presentation electrified the audience; it was “mind-boggling to a lot of the scientists there.”<sup>118</sup> It looked like Santer and his colleagues might just have proved the human impact on climate.

After Asheville, all chapters were revised in response to the country review—all, that is, except chapter 8, because Santer was still awaiting the comments. The final stage in the process was the IPCC plenary meeting, scheduled to start in Madrid on November 27. In October, drafts of the Working Group I Report and the Summary for Policymakers had been sent to all the government delegates to the Madrid meeting. When Santer arrived at the Madrid meeting, he was handed a sheaf of comments—including comments from the U.S. government—that he had never seen before.

Meanwhile, sometime in September, a draft of the entire Working Group I Report was leaked. The central message of chapter 8, that the anthropogenic fingerprint had been found, drew widespread attention.<sup>119</sup> “In an important shift of scientific judgment, experts advising the world’s governments on climate change are saying for the first time that human activity is a likely cause of the warming of the global atmosphere,” the *New York Times* declared on its front page. This, of course, wasn’t quite right. Scientists had been saying for a long time that human activity was a *likely* cause of warming. They were now saying that it was *demonstrated*. The *New York Times* didn’t get it. But the skeptics did, and they went on the attack.

Two weeks before the plenary session in Madrid, the Republican majority in the U.S. Congress launched a preemptive strike. In a set of hearings held in November, they repeatedly questioned the scientific basis for concern. The star witness was another well-known contrarian, Patrick J.

Michaels, who had completed his Ph.D. at the University of Wisconsin–Madison in 1979, building models relating climate change to crop yields. In 1980, he was appointed state climatologist of Virginia by Republican governor John Dalton (although many years later Michaels was forced to forego that title when it was shown that Dalton had acted without legal authority).<sup>120</sup> In the 1980s, Michaels had published scientific work on the climate sensitivity of various crops and ecosystems, but by the early 1990s, he was mainly known not for mainstream science, but his contrarian views.<sup>121</sup> He had joined Fred Singer in publicly attacking the mainstream view of ozone depletion in a series of columns in the *Washington Times*.<sup>122</sup> He produced a quarterly newsletter called the *World Climate Review*, funded at least in part by fossil fuel interests, and used it as a platform to attack mainstream climate science. The *Review* was circulated free to members of the Society for Environmental Journalism, ensuring that its claims got wide attention.<sup>123</sup> In the early 1990s, he had worked as a consultant to the Western Fuels Association—a coal mining industry group—to promote the idea that burning fossil fuels was good, because it would lead to higher crop yields as increased atmospheric CO<sub>2</sub> led to increased photosynthesis and therefore increased agricultural productivity.<sup>124</sup>

In the Republican hearings, Michaels was presented as an expert who somehow knew more than all the scientists working within the IPCC umbrella. His own personal analysis of the difference between a model prediction of greenhouse gas–induced warming and atmospheric temperatures derived from NOAA’s weather satellites showed, he claimed, that the IPCC climate models had heavily overpredicted global warming and could not be trusted. He complained in the hearing that while he’d made many critical comments on the various chapters of the IPCC report, his comments had been ignored, resulting “in not one discernable change in the text of the IPCC drafts.”<sup>125</sup>

Congressman George E. Brown Jr. of California asked Jerry Mahlman, director of NOAA’s Geophysical Fluid Dynamics Laboratory (GFDL), to respond to Michaels’s claims. The particular model study that Michaels attacked was the work of GFDL scientist Syukuro Manabe—probably the world’s most respected climate modeler—the man who, along with Jim Hansen, had presented his work to the Charney committee back in 1979. Mahlman explained that Michaels’s analysis contained an elementary flaw. Manabe’s study was designed to investigate the impact of CO<sub>2</sub> on climate, and had deliberately omitted other factors—including volcanic dust. However, there had been a set of large volcanic eruptions in the early 1990s,

most famously Mt. Pinatubo in 1992. The satellite measurements obviously did incorporate these other real world phenomena, so naturally, they'd be different from the model results.

"The bottom line," Mahlman concluded, "is that there is no logical basis for a direct comparison of this GFDL model experiment with that of [satellite] data sets or any other data set."<sup>126</sup> A legitimate comparison between models and observations could only be carried out when the models and observations examined the same things. It was obvious why the IPCC had ignored Michaels's complaints.

The hearing wasn't very successful at getting press attention, receiving no notice from the *New York Times*, the *Washington Post*, or even the *Washington Times*. Among major newspapers, only the *Boston Globe* seems to have bothered covering it. It wasn't exactly news by late 1995 that the Republican congressional leadership opposed environmental protection; there had been discussion that year of repealing the Clean Water Act, one of the cornerstones of American environmental improvement. But the lack of press attention didn't matter; the hearing had the desired effect of reinforcing the Republican majority's do-nothing attitude. Writing to Fred Seitz after the hearing, Nierenberg said, "I doubt that Congress will do anything foolish. I can also tell you that at least one high-level corporate advisor is advising boards that the issue is politically dead. Happy holiday."<sup>127</sup>

Santer presented the findings in chapter 8 on November 27, 1995, the first day of the plenary session (and the same day Nierenberg proclaimed the issue politically dead in his letter to Seitz). The chapter was immediately opposed by the Saudi Arabian and Kuwaiti delegates. In the words of the *New York Times's* reporter, these oil-rich states "made common cause with American industry lobbyists to try to weaken the conclusions emerging from Chapter 8."<sup>128</sup> The lone Kenyan delegate, Santer remembers, "thought there should not be a detection and attribution chapter at all."<sup>129</sup> Then the chairman of a fossil fuel industry group, the Global Climate Coalition, and automobile industry representatives monopolized the rest of the afternoon.<sup>130</sup> Finally the IPCC chairman, Britain's Sir John Houghton, closed the discussion and appointed an ad hoc drafting group to work out the disagreements and to address all of the late government comments. The working group included the lead authors, and delegates from the United States, Britain, Australia, Canada, New Zealand, the Netherlands, Saudi Arabia, Kuwait, and the lone Kenyan.

A portion of the ad hoc group hammered out an acceptable language. Steve Schneider convinced the Kenyan that there really was a scientific

basis for the chapter's central conclusion that anthropogenic climate change had been detected.<sup>131</sup> But the Saudis never sent a representative to the ad hoc sessions, and when Santer presented the revised draft, the Saudi head delegate protested all over again. A bit of a shouting match ensued, and Houghton had to intervene, effectively tabling the issue while the working group finished negotiating the Summary for Policymakers. There the entire issue boiled down to a single sentence, in fact a single *adjective*, drawn from Santer's chapter: "The balance of evidence suggests that there is a [blank] human influence on global climate."<sup>132</sup>

What should the adjective be? Santer and Wigley wanted "appreciable." This was unacceptable to the Saudi delegate, but it was too strong for Bert Bolin, too. One participant recalls the group trying about twenty-eight different words before Bolin suggested "discernible." That clicked, and the outcome of the Madrid meeting was this sentence: "The balance of evidence suggests that there is a discernible human influence on global climate."<sup>133</sup> This line would be quoted repeatedly in the years to come.

With the Summary for Policymakers settled, the individual chapters had to be revised in the light of all the late review comments, and Houghton instructed the lead authors to make the necessary changes after the meeting.<sup>134</sup> Santer went from Madrid to the Hadley Center in Bracknell, England, where he made the changes in long-distance collaboration with Wigley and Barnett. The most significant of these changes was structural. The draft chapter 8 had summary statements at both beginning and end of the chapter, but none of the other chapters did. They only had summaries at the beginning. Therefore, Santer had been instructed to remove the summary statement at the end of the chapter so that it would have the same structure as the rest of the chapters. That, Santer remembered years later, was a fateful decision, as critics would later attack him for "removing material."<sup>135</sup>

Then Fred Singer launched an attack. In a letter to *Science* on February 2, 1996, four months before formal release of the Working Group I Report, Singer presented a litany of complaints. The Summary for Policymakers, he claimed, ignored satellite data that showed "no warming at all, but actually a slight cooling." On this basis he claimed that the climate models, which all showed warming, were wrong. The IPCC had violated one of its "major rules" by including the fingerprinting work, because "the research had not yet, to my knowledge, appeared in the peer-reviewed literature." The panel had also ignored an "authoritative U.S. government report" that had found the twenty-first-century warming might be as little as 0.5°C, making global warming a nonproblem. (Singer didn't cite the report.)

Finally, he concluded, “The mystery is why some insist in making it into a problem, a crisis, or a catastrophe—‘the greatest global challenge facing mankind.’”<sup>136</sup>

Tom Wigley responded to Singer’s criticisms in March. Rejecting the “no warming” claim entirely, he simply stated, “This is not supported by the data; the trend from 1946 to 1995 is .3 C. As shown in chapter 8 of the full report (figure 8.4) there is no inconsistency between the observed temperature record and model simulations.” There were some differences between measurements made with satellites and measurements made with “radiosondes”—instruments on balloons, with radios attached to transmit the results—but climate scientists didn’t expect them to perfectly track each other; the reasons were explained in both chapters 3 and 8. “There are good physical reasons to expect differences between these two climate indicators,” Wigley noted, because they were in different places measuring somewhat different things.

The claim that the pattern recognition studies violated the IPCC’s rules was wrong on two counts. First, Wigley explained, the IPCC allowed use of material from outside the peer-reviewed journals as long as it was accessible to reviewers. This was to ensure the report was “up to date” when published. Moreover, the specific work Singer referred to “on the increasing correlation between the expected greenhouse-aerosol pattern and observed temperature changes, *is in the peer-reviewed literature.*”<sup>137</sup>

Moreover, Singer was again creating a straw man. “Singer refers to the [Summary for Policymakers] as saying that global warming is ‘the greatest global challenge facing mankind,’” Wigley and his coauthors wrote. “We do not know the origin of this statement—it does not appear in any of the IPCC documents. Further, it is the sort of extreme statement that most involved with the IPCC would not support.”<sup>138</sup>

Wigley was right. The IPCC had not described global warming as the “greatest global challenge facing mankind.” The words Singer attributed to the IPCC don’t appear in either the Working Group I Report or in its Summary for Policymakers. Singer was putting words into other people’s mouths—and then using those words to discredit them.

The IPCC had in fact bent over backward *not* to use alarmist terms. Bert Bolin had deliberately imposed a policy of extreme conservatism of language; witness his rejection of “appreciable” in favor of “discernible.” The opposition of the Saudi and Kuwaiti delegations had ensured only least common denominator statements. Everyone involved had seen how the

process led to a conservative estimation of the threat. What was Singer's response to this refutation of his allegations? He provided the missing citation for his claim that there would be only a 0.5°C warming in the twenty-first century.<sup>139</sup>

The IPCC had contracted with Cambridge University Press to publish the Working Group I Report, scheduled to appear in the United States in June 1996. In May, Santer and Wigley presented their chapter at a briefing in the Rayburn House Office Building on Capitol Hill, organized by the American Meteorological Society and the U.S. Global Change Research Program. The two scientists were now challenged by William O'Keefe of the American Petroleum Institute and by Donald Pearlman, an industry lobbyist and registered foreign agent of several oil-producing nations.<sup>140</sup> O'Keefe and Pearlman accused them of "secretly altering the IPCC report, suppressing dissent by other scientists, and eliminating references to scientific uncertainties."<sup>141</sup>

"Who made these changes to the chapter? Who authorized these changes? Why were they made?" Pearlman demanded. "Pearlman got up and in my face, turned beet red and [started] screaming at me," Santer recalls. AMS officer Anthony Socci "finally separated us, but Pearlman kept following me around."<sup>142</sup> Santer explained that he'd been required by IPCC procedures to make the changes in response to the government comments and discussions at Madrid, and the chapter had never been out of his control, but the truth did not satisfy the opposition.<sup>143</sup>

The Global Climate Coalition meanwhile had circulated a report entitled "The IPCC: Institutionalized Scientific Cleansing" to reporters, members of Congress, and some scientists. By chance, anthropologist Myanna Lahsen interviewed Nierenberg about his "skepticism" about global warming two weeks before the Working Group I Report was published, and found that he had a copy of the coalition report. He had evidently accepted its veracity, even though there was no way to compare its claims against the real chapter 8 (since the latter had not yet been released). He quoted its claims to Lahsen, telling her that the revisions had "just altered the whole meaning of the document. Without permission of the authors." Moreover, he claimed, "Anything that would imply the current status of knowledge is so poor that you can't do anything is struck out."<sup>144</sup> That was hardly true; Santer's panel had included six pages of discussion of uncertainty in the final text. But Bill Nierenberg knew all about altering scientific reports for political reasons, so perhaps he followed the adage that the best defense is

offense. Or perhaps he was guilty of “mirror imaging,” as Team B had accused the CIA of in 1976: assuming that his opponents thought and operated the way he did.

Then Fred Seitz took the attack to the national media. In a letter published in the *Wall Street Journal* on June 12, 1996, he accused Ben Santer of fraud. “In my more than 60 years as a member of the American scientific community, including my services as president of the National Academy of Sciences and the American Physical Society, I have never witnessed a more disturbing corruption of the peer-review process than the events that led to this IPCC report.” Seitz repeated the Global Climate Coalition’s charges that unauthorized changes to chapter 8 had been made after its acceptance in Madrid. “Few of these changes were merely cosmetic; nearly all worked to remove hints of the skepticism with which many scientists regard claims that human activities are having a major impact on climate in general and on global warming in particular,” Seitz claimed. If the IPCC couldn’t follow its own procedures, he concluded, it should be abandoned and governments should look for “more reliable sources of advice to governments on this important question.”<sup>145</sup> Presumably, he meant the George C. Marshall Institute, of which he was still chairman of the board.

Santer immediately drafted a letter to the *Journal*, which forty of the other IPCC lead authors signed. Santer explained what had happened, how he had been instructed by Houghton to make the changes, and why the changes were late in coming. At first the *Journal* wouldn’t publish it. After three tries, Santer finally got a call from the *Journal*’s letters editor, and the letter was finally published on June 25. Santer’s reply had been heavily edited, and the names of the forty other cosigners deleted.

What the *Journal* allowed Santer to say was that he had been required to make the changes “in response to written review comments received in October and November 1995 from governments, individual scientists, and non-government organizations during plenary sessions of the Madrid meeting.” This was peer review—the very process that Seitz, as a research scientist, had been a part of all his life. Only it was extended to include comments and queries from governments and NGOs as well as scientific experts. But the changes didn’t affect the bottom line conclusion.

Santer also pointed out that Seitz wasn’t a climate scientist, hadn’t been involved in creating the IPCC report, hadn’t attended the Madrid meeting, and hadn’t seen the hundreds of review comments to which Santer had to respond. In other words, his claims were just hearsay.<sup>146</sup>

Bert Bolin and Sir John Houghton also responded with a long letter de-



fending Santer and the IPCC process. “Frederick Seitz’s article is completely without foundation,” they replied unequivocally. “It makes serious allegations about the Intergovernmental Panel on Climate Change and about the scientists who have contributed to its work which have no basis in fact. Mr. Seitz does not state the source of his material, and we note for the record that he did not check his facts either with the IPCC officers or with any of the scientists involved.”<sup>147</sup>

Well, that’s what they’d wanted it to say, but the *Journal* edited that statement out, too, along with three more paragraphs explaining the drafting process in some detail. The *Journal* allowed them to say only that:

... [in] accordance with IPCC Procedures, the changes to the draft of Chapter 8 were under the full scientific control of its convening Lead Author, Benjamin Santer. No one could have been more thorough and honest in undertaking that task. As the responsible officers of the IPCC, we are completely satisfied that the changes incorporated in the revised version were made with the sole purpose of producing the best possible and most clearly explained assessment of the science and were not in any way motivated by any political or other considerations.<sup>148</sup>

We know how the *Journal* edited the letters because Seitz’s attack and the *Journal*’s weakening of the response so offended the officials of the American Meteorological Society and of the University Corporation for Atmospheric Research that their boards agreed to publish an “Open Letter to Ben Santer” in the *Bulletin of the American Meteorological Society*, where they republished the letters in their entirety, showing how the *Journal* had edited them. They voiced their support of Santer and the effort it had taken all the authors to put the report together, and categorically rejected Seitz’s attack as having “no place in the scientific debate about issues related to global change.”<sup>149</sup> They began, finally, to realize what they were up against.

[There] appear[ed] to be a concerted and systematic effort by some individuals to undermine and discredit the scientific process that has led many scientists working on understanding climate to conclude that there is a very real possibility that humans are modifying Earth’s climate on a global scale. Rather than carrying out a legitimate scientific debate through the peer-reviewed

literature, they are waging in the public media a vocal campaign against scientific results with which they disagree.<sup>150</sup>

But the attack was far from over. On July 11, the *Wall Street Journal* published three more letters reprising the charges, one from Fred Seitz, one from Fred Singer, and one from Hugh Ellsaesser. (Ellsaesser was a retired geophysicist at the Lawrence Livermore National Laboratory who previously had questioned the evidence of the ozone hole. He served in the mid-1990s on the Marshall Institute's Scientific Advisory Board, and in 1995 wrote a report for the Heartland Institute on *The Misuse of Science in Environmental Management*.) Singer and Seitz simply repeated the charges they'd already made, but Singer also took the opportunity to turn the IPCC's caution against it. The IPCC had bent over backward to be judicious, arguing at length to choose just the right, reasonable adjective—"discernible." Singer dismissed the IPCC conclusion as "feeble," at the same time insisting illogically that it was being used to frighten politicians into believing that a climate catastrophe is about to happen.<sup>151</sup>

Santer and Bolin responded a second time to the attacks in letters the *Journal* published July 23—prompting another attack by Singer.<sup>152</sup> This time, the *Journal* wouldn't publish it, and Singer circulated it by e-mail instead. Santer responded by e-mail, too. There was, Singer maintained, no "evidence for a current warming trend." According to Singer, chapter 8 had been based primarily on Santer's "unpublished work," and the panel should have included as a lead author "Professor Patrick J. Michaels, who, at the time, had published the only refereed paper on the subject" of climate fingerprinting. And he repeated the charge of "scientific cleansing." Santer rejected all of Singer's charges. Chapter 8 was based on more than 130 references, not just Santer's two papers. The claim that Michaels had published the only "refereed paper on the subject" of pattern-based recognition before mid-1995 was incorrect: Hasselmann's theoretical paper on the subject was published in 1979, and Tim Barnett and Mike Schlesinger had published a "real-world" fingerprint study as early as 1987. Michaels had been invited to be a contributing author to chapter 8 but had refused. Finally, Santer noted, chapter 8 contained several paragraphs discussing Michaels's paper, but when Wigley had approached Michaels for comments, "Prof. Michaels did not respond."<sup>153</sup>

Singer's claims were not only false, but had been *shown* to be false. Still, he wasn't finished repeating them. Now he would claim that Fred Seitz was the real victim of the whole affair.

In November, Singer penned an article for the *Washington Times* entitled “Global Warming Disinformation?” By this time, the IPCC report had been published and available for months, so Singer could have seen for himself that chapter 8 contained six pages of discussions of model and observational uncertainties, as Santer had insisted it should all along. Still, Singer repeated the claim that chapter 8 had been edited to remove uncertainties, and then asserted that “Seitz, one of the nation’s most respected scientists, was attacked for factually reporting the revisions made by the IPCC leadership, which clearly affected the sense of the report!”<sup>154</sup>

Joined by Bill Nierenberg, Patrick Michaels, and a new ally—MIT meteorologist Richard Lindzen—Singer then attacked the AMS/UCAR Open Letter. After repeating the refuted charges of “substantial and substantive” deletions of uncertainty, Singer cast the deletions as a conspiracy that Santer was now trying to cover up. “Santer . . . has not been forthcoming in revealing who instructed him to make such revisions and who approved them after they were made. He has, however, told others privately that he was asked [prevailed upon?] to do so by IPCC co-chairman John Houghton.” To Singer and his co-authors on the letter, this was evidence of political meddling in the chapter. He continued, “You may not have seen the 15 November [1995] letter from the State Department instructing Dr. Houghton to ‘prevail upon’ chapter authors ‘to modify their texts in an appropriate manner following discussion in Madrid.’”<sup>155</sup> Singer’s presentation of it as some sort of clandestine conspiracy was absurd: Bolin and Houghton had already identified themselves months before as the source of Santer’s instructions.

In her 1999 analysis, Myanna Lahsen pinned Singer’s efforts to “envelop the IPCC in an aura of secrecy and unaccountability” to a common American conservative rhetoric of political suppression.<sup>156</sup> As we have seen in previous chapters, if anyone was meddling in the scientific assessment and peer review process, it was the political right wing, not the left. It wasn’t the Sierra Club that tried to pressure the National Academy of Sciences over the 1983 Carbon Dioxide Assessment; it was officials from the Department of Energy under Ronald Reagan. It wasn’t Environmental Defense that worked with Bill Nierenberg to alter the Executive Summary of the 1983 Acid Rain Peer Review Panel; it was the White House Office of Science and Technology Policy. And it was the *Wall Street Journal* spreading the attack on Santer and the IPCC, not *Mother Jones*.

The over-the-top attacks on Santer began to have consequences for Nierenberg. In April, Nierenberg had invited Tom Wigley to a conference he wanted to hold at Scripps that November on the costs and benefits of

global warming, but Wigley smelled a rat. “I have decided to withdraw from your November meeting,” he wrote. “The reason for this is the letter you co-signed which appeared in BAMS [*Bulletin of the American Meteorological Society*]. I have no desire to cooperate with anyone who endorses such an unmitigated collection of distortions and misinformation.”<sup>157</sup>

Nierenberg tried flattery to keep Wigley on board. “The personally difficult part for me is that your work, Klaus [Hasselmann’s] work, and [Bill] Nordhaus’ work have had the most influence (and still do) on my thinking.” He lamented the rift that was developing in the climate science community over the ongoing public attacks, but then followed Singer’s lead in imputing conspiracy, this time in the scientific journals. “I remind you in this instance of something that touched you personally about which I only had the slightest information from the gossip columns and some hallway talk. I was told that you faced great opposition in getting your *Nature* paper published. That great pressure was put on you.”<sup>158</sup>

Wigley evidently had no idea what *Nature* paper Nierenberg was talking about. “It seems that you have not only NOT been influenced by, but actually disagree with (or are unaware of) the vast bulk of my scientific work: in particular the work on detection, which the BAMS letter you co-signed has unfairly, unjustifiably, unscientifically and incorrectly criticized.” Wigley also rejected the imputation that *Nature* had pressured him. “To which paper are you referring? I have published 22 papers in that particular journal. No matter which, you shouldn’t take any notice of what you hear in ‘gossip columns and hallway talk.’” He concluded, “So Bill, what I said in my previous email stands. Your 17 April ‘response’ gives me no reason to change my mind—just the opposite. The BAMS letter makes it quite clear that you think my IPCC detection work with Ben Santer was distorted for political motives. I am surprised, therefore, that you would even want a person like me to attend a meeting of yours. I still think you are being duplicitous, and I still suspect your motives.”<sup>159</sup>

Wigley wasn’t the only one to begin to understand what Nierenberg was really up to. Klaus Hasselmann also wrote to Nierenberg: “I have followed the attacks on Ben Santer during the last year and found them to be grossly unfair and clearly politically motivated. In a letter I wrote to the *Wall Street Journal* (which was not published, with many other similar letters) I pointed out that it was ridiculous to imply that the conclusions of Chapter 8 had been willfully or unintentionally altered against the will of the Madrid delegates.”<sup>160</sup> Hasselmann was still willing to come to a meeting about the costs and benefits of global warming—a subject that interested him greatly—but

he wouldn't come to a meeting with a political agenda. "In view of the pronounced political colouring of the BAMS letter I am not convinced at this point that the concerns of Tom Wigley are not justified."<sup>161</sup>

Perhaps after so many years as Svengali, Bill Nierenberg did not realize that this time he had gone too far. Nierenberg, despite his intellect, really didn't seem to understand that by participating in this assault on Ben Santer, he was attacking the entire community of climate modelers. By signing on to Singer's letter, he marked himself in their eyes as a political actor, not a scientific one. Nierenberg's comment that he feared the polarization of the community was both perceptive and blinkered; the climate science community was most definitely becoming polarized, but it was due to his own actions, and those of a small network of doubt-mongers.

WE MIGHT DISMISS this whole story as just infighting within the scientific community, except that the Marshall Institute claims were taken seriously in the Bush White House and published in the *Wall Street Journal*, where they would have been read by millions of educated people. Members of Congress also took them seriously. Proposing a bill to reduce climate research funding by more than a third in 1995, Congressman Dana Rohrabacher called it "trendy science that is propped up by liberal/left politics rather than good science."<sup>162</sup> In July 2003, Senator James Inhofe called global warming "the greatest hoax ever perpetrated on the American people."<sup>163</sup> As late as 2007, Vice President Richard Cheney commented in a television interview, "Where there does not appear to be a consensus, where it begins to break down, is the extent to which that's part of a normal cycle versus the extent to which it's caused by man, greenhouse gases, et cetera"—exactly the question Santer had answered a decade before.<sup>164</sup> How did such a small group come to have such a powerful voice?

We take it for granted that great individuals—Gandhi, Kennedy, Martin Luther King—can have great positive impacts on the world. But we are loath to believe the same about negative impacts—unless the individuals are obvious monsters like Hitler or Stalin. But small numbers of people can have large, negative impacts, especially if they are organized, determined, and have access to power.

Seitz, Jastrow, Nierenberg, and Singer had access to power—all the way to the White House—by virtue of their positions as physicists who had won the Cold War. They used this power to support their political agenda, even though it meant attacking science and their fellow scientists, evidently

believing that their larger end justified their means. Perhaps this, too, was part of their professional legacy. During the Manhattan Project, and throughout the Cold War, for security reasons many scientists had to hide the true nature of their work. All weapons projects were secret, but so were many other projects that dealt with rocketry, missile launching and targeting, navigation, underwater acoustics, marine geology, bathymetry, seismology, weather modification; the list goes on and on.<sup>165</sup> These secret projects frequently had “cover stories” that scientists could share with colleagues, friends, and families, and sometimes the cover stories were true in part. But they weren’t the whole truth, and sometimes they weren’t true at all. After the Cold War, most scientists were relieved to be freed of the burdens of secrecy and misrepresentation, but Seitz, Singer, and Nierenberg continued to act as if the Cold War had not ended.

Whatever the reasons and justifications of our protagonists, there’s another crucial element to our story. It’s how the mass media became complicit, as a wide spectrum of the media—not just obviously right-wing newspapers like the *Washington Times*, but mainstream outlets, too—felt obligated to treat these issues as scientific controversies. Journalists were constantly pressured to grant the professional deniers equal status—and equal time and newsprint space—and they did. Eugene Linden, once an environment reporter for *Time* magazine, commented in his book *Winds of Change* that “members of the media found themselves hounded by experts who conflated scientific diffidence with scientific uncertainty, and who wrote outraged letters to the editor when a report didn’t include their dissent.”<sup>166</sup> Editors evidently succumbed to this pressure, and reporting on climate in the United States became biased *toward* the skeptics and deniers because of it.

We’ve noted how the notion of balance was enshrined in the Fairness Doctrine, and it may make sense for political news in a two-party system (although not in a multiparty system). But it doesn’t reflect the way science works. In an active scientific debate, there can be many sides. But once a scientific issue is closed, there’s only one “side.” Imagine providing “balance” to the issue of whether the Earth orbits the Sun, whether continents move, or whether DNA carries genetic information. These matters were long ago settled in scientists’ minds. Nobody can publish an article in a scientific journal claiming the Sun orbits the Earth, and for the same reason, you can’t publish an article in a peer-reviewed journal claiming there’s no global warming. Probably well-informed professional science journalists wouldn’t publish it either. But ordinary journalists repeatedly did.

In 2004, one of us showed that scientists had a consensus about the reality of global warming and its human causes—and had since the mid-1990s. Yet throughout this time period, the mass media presented global warming and its cause as a major debate. By coincidence, another study also published in 2004 analyzed media stories about global warming from 1988 to 2002. Max and Jules Boykoff found that “balanced” articles—ones that gave equal time to the majority view among climate scientists as well as to deniers of global warming—represented nearly 53 percent of media stories. Another 35 percent of articles presented the correct majority position among climate scientists, while still giving space to the deniers.<sup>167</sup> The authors conclude that this “balanced” coverage is a form of “informational bias,” that the ideal of balance leads journalists to give minority views more credence than they deserve.

This divergence between the state of the science and how it was presented in the major media helped make it easy for our government to do nothing about global warming. Gus Speth had thought in 1988 that there was real momentum toward taking action. By the mid-1990s, that policy momentum had not just fizzled; it had evaporated. In July 1997, three months before the Kyoto Protocol was finalized, U.S. senators Robert Byrd and Charles Hagel introduced a resolution blocking its adoption.<sup>168</sup> Byrd-Hagel passed the Senate by a vote of 97–0. Scientifically, global warming was an established fact. Politically, global warming was dead.