CHAPTER 1

Doubt Is Our Product

On May 9, 1979, a group of tobacco industry executives gathered to hear about an important new program. They had been invited by Colin H. Stokes, the former chairman of R. J. Reynolds, a company famous for pioneering marketing, including the first cigarette advertisements on radio and television ("I'd walk a mile for a Camel"). In later years, Reynolds would be found guilty of violating federal law by appealing to children with the character Joe Camel (which the Federal Trade Commission compared to Mickey Mouse), but the executives had not come to hear about products or marketing. They had come to hear about science. The star of the evening was not Stokes, but an elderly, balding, bespectacled physicist named Frederick Seitz.

Seitz was one of America's most distinguished scientists. A wunderkind who had helped to build the atomic bomb, Seitz had spent his career at the highest levels of American science: a science advisor to NATO in the 1950s, president of the National Academy of Sciences in the 1960s, president of the Rockefeller University—America's leading biomedical research institution—in the 1970s. In 1979, Seitz had just retired, and he was there to talk about one last job: a new program, which he would run on behalf of R. J. Reynolds, to fund biomedical research at major universities, hospitals, and research institutes across the country.

The focus of the new program was degenerative diseases—cancer, heart disease, emphysema, diabetes—the leading causes of death in the United States. And the project was huge: $45 million would be spent over the next six years. The money would fund research at Harvard, the universities of

Connecticut, California, Colorado, Pennsylvania, and Washington, the Sloan-Kettering Institute, and, not surprisingly, the Rockefeller University.[1] A typical grant was $500,000 per year for six years—a very large amount of money for scientific research in those days.[2] The program would support twenty-six different research programs, plus six young investigators on "RJR research scholarships," in the areas of chronic degenerative disease, basic immunology, the effect of "lifestyle modes" on disease.[3]

Seitz's role was to choose which projects to fund, to supervise and monitor the research, and to report progress to R. J. Reynolds. To determine the project criteria—what types of projects to fund—he enlisted the help of two other prominent colleagues: James A. Shannon and Maclyn McCarty.

Shannon was a physician who pioneered the use of the antimalaria drug Atabrine during World War II. Atabrine was effective, but had lousy side effects; Shannon figured out how to deliver the drug without the sickening side effects, and then administered the program that delivered it to millions of troops throughout the South Pacific, saving thousands from sickness and death.[4] Later, as director of the National Institutes of Health from 1955 to 1968, he transformed the NIH by convincing Congress to allow them to offer grants to university and hospital researchers. Before that, NIH funds were spent internally; very little money was available to American hospitals and universities for biomedical research. Shannon's external grant program was wildly popular and successful, and so it grew—and grew. Eventually it produced the gargantuan granting system that is the core of the NIH today, propelling the United States to leadership in biomedical research. Yet, for all this, Shannon never won a Nobel Prize, a National Medal of Science, or even a Lasker Award—often said to be biology's next best thing to the Nobel Prize.

Maclyn McCarty similarly had a fabulously successful career without being fabulously recognized. Many people have heard of James Watson and Francis Crick, who won the Nobel Prize for deciphering the double helix structure of DNA, but Watson and Crick did not prove that DNA carried the genetic information in cells. That crucial first step had been done a decade earlier, in 1944, by three bacteriologists at the Rockefeller University—Oswald Avery, Maclyn McCarty, and Colin MacLeod. In an experiment with pneumonia bacteria, they showed that benign bacteria could be made virulent by injecting them with DNA from virulent strands. You could change the nature of an organism by altering its DNA—something we take for granted now, but a revolutionary idea in the 1940s.

Perhaps because Avery was a quiet man who didn't trumpet his discovery, or perhaps because World War II made it difficult to get attention for any discovery without immediate military relevance, Avery, McCarty, and McLeod got relatively little notice for their experiment. Still, all three had distinguished scientific careers and in 1994 McCarty won the Lasker Award. But in 1979, McCarty was definitely underappreciated.

So it is perhaps not surprising that when Shannon and McCarty helped Seitz to develop their criteria for judging proposals, they sought projects that took a different perspective from the mainstream, individuals with unusual or offbeat ideas, and young investigators in their "formative stages" who lacked federal support.[5] One funded study examined the impact of stress, therapeutic drugs, and food additives (like saccharin) on the immune system. Another explored the relation between "the emotional framework and the state of . . . the immune system . . . in a family of depressed patients." A third asked whether the "psychological attitude of a patient can play a significant role in determining the course of a disease."[6] Projects explored the genetic and dietary causes of atherosclerosis, possible viral causes of cancer, and details of drug metabolism and interactions.

Two scientists in particular caught Seitz's personal attention. One was Martin J. Cline, a professor at UCLA who was studying the lung's natural defense mechanisms and was on the verge of creating the first transgenic organism.[7] Another was Stanley B. Prusiner, the discoverer of prions—the folded proteins that cause mad cow disease—for which he later won the Nobel Prize in Physiology or Medicine.[8]

All of the chosen studies addressed legitimate scientific questions, some that mainstream medicine had neglected—like the role of emotions and stress in somatic disease. All the investigators were credentialed researchers at respected institutions.[9] Some of the work they were doing was pathbreaking. But was the purpose simply to advance science? Not exactly.

Various R. J. Reynolds documents discuss the purpose of Seitz's program. Some suggest that supporting research was an "obligation of corporate citizenship." Others note the company's desire to "contribute to the prevention and cure of diseases for which tobacco products have been blamed." Still others suggest that by using science to refute the case against tobacco, the industry could "remove the government's excuse" for imposing punitive taxes.[10] (In 1978, smokers paid over a billion and a half dollars in cigarette excise taxes in the United States and abroad—taxes that had been raised in part in response to the scientific evidence of its harms.)

But the principal goal, stressed by Stokes to his advisory board that day

in May and repeated in scores of industry documents, was to develop "an extensive body of scientifically, well-grounded data useful in defending the industry against attacks."[11] No doubt some scientists declined the offer of industry funding, but others accepted it, presumably feeling that so long as they were able to do science, it didn't really matter who paid for it. If any shareholders were to ask why company funds were being used to support basic (as opposed to applied) science, they could be told that the expenditure was "fully justified on the basis of the support it provides for defending the tobacco industry against fundamental attacks on its business."[12] The goal was to fight science with science—or at least with the gaps and uncertainties in existing science, and with scientific research that could be used to deflect attention from the main event. Like the magician who waves his right hand to distract attention from what he is doing with his left, the tobacco industry would fund distracting research.

In a presentation to R. J. Reynolds' International Advisory Board, and reviewed by RJR's in-house legal counsel, Stokes explained it this way: The charges that tobacco was linked to lung cancer, hardening of the arteries, and carbon monoxide poisoning were unfounded. "Reynolds and other cigarette makers have reacted to these scientifically unproven claims by intensifying our funding of objective research into these matters."[13] This research was needed because the case against tobacco was far from proven.

"Science really knows little about the causes or development mechanisms of chronic degenerative diseases imputed to cigarettes," Stokes went on, "including lung cancer, emphysema, and cardiovascular disorders." Many of the attacks against smoking were based on studies that were either "incomplete or . . . relied on dubious methods or hypotheses and faulty interpretations." The new program would supply new data, new hypotheses, and new interpretations to develop "a strong body of scientific data or opinion in defense of the product."[14] Above all, it would supply witnesses.

By the late 1970s, scores of lawsuits had been filed claiming personal injury from smoking cigarettes, but the industry had successfully defended itself by using scientists as expert witnesses to testify that the smoking-cancer link was not unequivocal. They could do this by discussing research that focused on other "causes or development mechanisms of chronic degenerative diseases imputed to cigarettes."[15] The testimony would be particularly convincing if it were their *own* research. Experts could supply reasonable doubt, and who better to serve as an expert than an actual scientist?

The strategy had worked in the past, so there was no reason to think it would not continue to work in the future. "Due to favorable scientific testimony," Stokes boasted, "no plaintiff has ever collected a penny from any tobacco company in lawsuits claiming that smoking causes lung cancer or cardiovascular illness—even though one hundred and seventeen such cases have been brought since 1954."[16]

In later years, this would change, but in 1979 it was still true. No one had collected a penny from the tobacco industry, even though scientists had been certain of the tobacco-cancer link since the 1950s (and many had been convinced before that).[17] Every project Reynolds funded could potentially produce such a witness who could testify to causes of illness other than smoking. Prusiner's work, for example, suggested a disease mechanism that had nothing to do with external causes. A prion, Seitz explained, could "take over in such a way that it over-produces its own species of protein and . . . destroys the cell," in "the manner in which certain genes . . . can be stimulated to over-produce cell division and lead to cancer."[18] Cancer might just be cells gone wild.

Cline's research suggested the possibility of preventing cancer by strengthening the cell's natural defenses, which in turn suggested that cancer might just be a (natural) failure of those defenses. Many of the studies explored other causes of disease—stress, genetic inheritance, and the like—an entirely legitimate topic, but one that could also help distract attention from the industry's central problem: the overwhelming evidence that tobacco killed people. Tobacco caused cancer: that was a fact, and the industry knew it. So they looked for some way to deflect attention from it. Indeed, they had known it since the early 1950s, when the industry first began to use science to fight science, when the modern era of fighting facts began. Let us return, for a moment, to 1953.

DECEMBER 15, 1953, was a fateful day. A few months earlier, researchers at the Sloan-Kettering Institute in New York City had demonstrated that cigarette tar painted on the skin of mice caused fatal cancers.[19] This work had attracted an enormous amount of press attention: the *New York Times* and *Life* magazine had both covered it, and *Reader's Digest*—the most widely read publication in the world—ran a piece entitled "Cancer by the Carton."[20] Perhaps the journalists and editors were impressed by the scientific paper's dramatic concluding sentences: "Such studies, in view of the corollary clinical data relating smoking to various types of cancer, appear urgent. They may not only result in furthering our knowledge of carcinogens, but in promoting some practical aspects of cancer prevention."

These findings shouldn't have been a surprise. German scientists had shown in the 1930s that cigarette smoking caused lung cancer, and the Nazi government had run major antismoking campaigns; Adolf Hitler forbade smoking in his presence. However, the German scientific work was tainted by its Nazi associations, and to some extent ignored, if not actually suppressed, after the war; it had taken some time to be rediscovered and independently confirmed.[21] Now, however, American researchers—not Nazis—were calling the matter "urgent," and the news media were reporting it.[22] "Cancer by the carton" was not a slogan the tobacco industry would embrace.

The tobacco industry was thrown into panic. One industry memo noted that their salesmen were "frantically alarmed."[23] So industry executives made a fateful decision, one that would later become the basis on which a federal judge would find the industry guilty of conspiracy to commit fraud—a massive and ongoing fraud to deceive the American public about the health effects of smoking.[24] The decision was to hire a public relations firm to challenge the scientific evidence that smoking could kill you.

On that December morning, the presidents of four of America's largest tobacco companies—American Tobacco, Benson and Hedges, Philip Morris, and U.S. Tobacco—met at the venerable Plaza Hotel in New York City. The French Renaissance chateau-style building—in which unaccompanied ladies were not permitted in its famous Oak Room bar—was a fitting place for the task at hand: the protection of one of America's oldest and most powerful industries. The man they had come to meet was equally powerful: John Hill, founder and CEO of one of America's largest and most effective public relations firms, Hill and Knowlton.

The four company presidents—as well as the CEOs of R. J. Reynolds and Brown and Williamson—had agreed to cooperate on a public relations program to defend their product.[25] They would work together to convince the public that there was "no sound scientific basis for the charges," and that the recent reports were simply "sensational accusations" made by publicity-seeking scientists hoping to attract more funds for their research.[26] They would not sit idly by while their product was vilified; instead, they would create a Tobacco Industry Committee for Public Information to supply a "positive" and "entirely 'pro-cigarette'" message to counter the anti-cigarette scientific one. As the U.S. Department of Justice would later put it, they decided "to deceive the American public about the health effects of smoking."[27]

At first, the companies didn't think they needed to fund new scientific research, thinking it would be sufficient to "disseminate information on hand." John Hill disagreed, "emphatically warn[ing] . . . that they should . . .

sponsor additional research," and that this would be a long-term project.[28] He also suggested including the word "research" in the title of their new committee, because a pro-cigarette message would need science to back it up.[29] At the end of the day, Hill concluded, "scientific doubts must remain."[30] It would be his job to ensure it.

Over the next half century, the industry did what Hill and Knowlton advised. They created the "Tobacco Industry Research Committee" to challenge the mounting scientific evidence of the harms of tobacco. They funded alternative research to cast doubt on the tobacco-cancer link.[31] They conducted polls to gauge public opinion and used the results to guide campaigns to sway it. They distributed pamphlets and booklets to doctors, the media, policy makers, and the general public insisting there was no cause for alarm.

The industry's position was that there was "no proof" that tobacco was bad, and they fostered that position by manufacturing a "debate," convincing the mass media that responsible journalists had an obligation to present "both sides" of it. Representatives of the Tobacco Industry Research Committee met with staff at *Time, Newsweek, U.S. News and World Report, BusinessWeek, Life,* and *Reader's Digest,* including men and women at the very top of the American media industry. In the summer of 1954, industry spokesmen met with Arthur Hays Sulzberger, publisher of the *New York Times*; Helen Rogers Reid, chairwoman of the *New York Herald Tribune*; Jack Howard, president of Scripps Howard Newspapers; Roy Larsen, president of Luce Publications (owners of *Time* and *Life*); and William Randolph Hearst Jr. Their purpose was to "explain" the industry's commitment to "a long-range . . . research program devoted primarily to the public interest"—which was needed since the science was so unsettled—and to stress to the media their responsibility to provide a "balanced presentation of all the facts" to ensure the public was not needlessly frightened.[32]

The industry did not leave it to journalists to seek out "all the facts." They made sure they got them. The so-called balance campaign involved aggressive dissemination and promotion to editors and publishers of "information" that supported the industry's position. But if the science was firm, how could they do that? *Was* the science firm?

The answer is yes, but. A scientific discovery is not an event; it's a process, and often it takes time for the full picture to come into clear focus. By the late 1950s, mounting experimental and epidemiological data linked tobacco with cancer—which is why the industry took action to oppose it. In private, executives acknowledged this evidence.[33] In hindsight it is fair

to say—and science historians *have* said—that the link was already established beyond a reasonable doubt. Certainly no one could honestly say that science showed that smoking was safe.

But science involves many details, many of which remained unclear, such as why some smokers get lung cancer and others do not (a question that remains incompletely answered today). So some scientists remained skeptical. One of them was Dr. Clarence Cook Little.

C. C. Little was a renowned geneticist, a member of the U.S. National Academy of Sciences and former president of the University of Michigan.[34] But he was also well outside the mainstream of scientific thinking. In the 1930s, Little had been a strong supporter of eugenics—the idea that society should actively improve its gene pool by encouraging breeding by the "fit" and discouraging or preventing breeding by the "unfit." His views were not particularly unusual in the 1920s—they were shared by many scientists and politicians including President Theodore Roosevelt—but nearly everyone abandoned eugenics in the '40s when the Nazis made manifest where that sort of thinking could lead. Little, however, remained convinced that essentially all human traits were genetically based, including vulnerability to cancer. For him, the cause of cancer was genetic weakness, not smoking.

In 1954, the tobacco industry hired Little to head the Tobacco Industry Research Committee and spearhead the effort to foster the impression of debate, primarily by promoting the work of scientists whose views might be useful to the industry. One of these scientists was Wilhelm C. Hueper, chief of the Environmental Cancer Section at the National Cancer Institute. Hueper had been a frequent expert witness in asbestos litigation where he sometimes had to respond to accusations that a plaintiff's illnesses were caused not by asbestos, but by smoking. Perhaps for this reason, Hueper prepared a talk questioning the tobacco-cancer link for a meeting in São Paulo, Brazil. When the Tobacco Industry Research Committee learned about it, they contacted Hueper, who agreed to allow them to promote his work. Hill and Knowlton prepared and delivered a press release, with copies of Hueper's talk, to newspapers offices, wire services, and science and editorial writers around the country. They later reported that "as a result of the distribution [of the press release] in the U.S.A., stories questioning a link between smoking and cancer were given wide attention, both in headlines and stories."[35] *U.S. News and World Report* practically gushed, "Cigarettes are now gaining support from new studies at the National Cancer Institute."[36]

Little's committee prepared a booklet, *A Scientific Perspective on the Cigarette Controversy*, which was sent to 176,800 American doctors.[37] Fifteen thousand additional copies were sent to editors, reporters, columnists, and members of Congress. A poll conducted two years later showed that "neither the press nor the public seems to be reacting with any noticeable fear or alarm to the recent attacks."[38]

The industry made its case in part by cherry-picking data and focusing on unexplained or anomalous details. No one in 1954 would have claimed that everything that needed to be known about smoking and cancer was known, and the industry exploited this normal scientific honesty to spin unreasonable doubt. One Hill and Knowlton document, for example, prepared shortly after John Hill's meeting with the executives, enumerated fifteen scientific questions related to the hazards of tobacco.[39] Experiments showed that laboratory mice got skin cancer when painted with tobacco tar, but not when left in smoke-filled chambers. Why? Why do cancer rates vary greatly between cities even when smoking rates are similar? Do other environmental changes, such as increased air pollution, correlate with lung cancer? Why is the recent rise in lung cancer greatest in men, even though the rise in cigarette use was greatest in women? If smoking causes lung cancer, why aren't cancers of the lips, tongue, or throat on the rise? Why does Britain have a lung cancer rate four times higher than the United States? Does climate affect cancer? Do the casings placed on American cigarettes (but not British ones) somehow serve as an antidote to the deleterious effect of tobacco? How much is the increase in cancer simply due to longer life expectancy and improved accuracy in diagnosis?[40]

None of the questions was illegitimate, but they were all disingenuous, because the answers were known: Cancer rates vary between cities and countries because smoking is not the only cause of cancer. The greater rise in cancer in men is the result of latency—lung cancer appears ten, twenty, or thirty years after a person begins to smoke—so women, who had only recently begun to smoke heavily, would get cancer in due course (which they did). Improved diagnosis explained some of the observed increase, but not all: lung cancer was an exceptionally rare disease before the invention of the mass-marketed cigarette. And so on.

When posed to journalists, however, the loaded questions did the trick: they convinced people who didn't know otherwise that there was still a lot of doubt about the whole matter. The industry had realized that you could create the impression of controversy simply by asking questions, even if you actually knew the answers and they didn't help your case.[41] And so the industry began to transmogrify emerging scientific consensus into raging scientific "debate."[42]

The appeal to journalistic balance (as well as perhaps the industry's large advertising budget) evidently resonated with writers and editors, perhaps because of the influence of the Fairness Doctrine. Under this doctrine, established in 1949 (in conjunction with the rise of television), broadcast journalists were required to dedicate airtime to controversial issues of public concern in a balanced manner.[43] (The logic was that broadcasts licenses were a scarce resource, and therefore a public trust.) While the doctrine did not formally apply to print journalism, many writers and editors seem to have applied it to the tobacco question, because throughout the 1950s and well into the 1960s, newspapers and magazines presented the smoking issue as a great debate rather than as a scientific problem in which evidence was rapidly accumulating, a clear picture was coming into focus, and the trajectory of knowledge was clearly against tobacco's safety.[44] Balance was interpreted, it seems, as giving equal weight to both sides, rather than giving *accurate* weight to both sides.

Even the great Edward R. Murrow fell victim to these tactics. In 1956, Hill and Knowlton reported on a conference held with Murrow, his staff, and their producer, Fred Friendly:

> The Murrow staff emphasized the intention to present a coldly objective program with every effort made to tell the story as it stands today, with *special effort toward a balanced perspective and concrete steps to show that the facts still are not established and must be sought by scientific means such as the research activities the Tobacco Industry Research Committee will support.*[45]

Balance. Cold objectivity. These were Murrow's trademarks—along with his dangling cigarette—and the tobacco industry exploited them both. Murrow's later death from lung cancer was both tragic and ironic, for during World War II Murrow had been an articulate opponent of meretricious balance in reporting. As David Halberstam has put it, Murrow was not ashamed to take the side of democracy, and felt no need to try to get the Nazi perspective or consider how isolationists felt. There was no need to "balance Hitler against Churchill."[46]

Yet Murrow fell prey to the tobacco industry's insistence that their self-interested views should be balanced against independent science. Perhaps, being a smoker, he was reluctant to admit that his daily habit was deadly and reassured to hear that the allegations were unproven. Roger Ferger, publisher of the *Cincinnati Enquirer*, evidently felt that way, as he wrote a bread-and-butter note for his copy of the *Scientific Perspective* pamphlet: "I have been a smoker for some forty-five years and I am still a pretty healthy specimen."[47] It was certainly comforting to be told that the jury was still out.

Editors, however, might eventually be expected to notice if the only support for industry claims came from obscure conferences in Brazil. No doubt realizing this, the industry sought links with mainstream medicine, funding research projects at leading medical schools related to cancer pathology, diagnosis, and distribution, and potentially related diseases such as coronary heart disease. In 1955, the industry established a fellowship program to support research by medical degree candidates: seventy-seven of seventy-nine medical schools agreed to participate.[48] (Industry documents don't tell which two declined; perhaps they were affiliated with religious denominations that eschewed smoking.) The industry also sought to develop good relations with members of the National Cancer Institute and American Heart Association by inviting their representatives to board meetings.[49] Building on his success, in 1957 the Tobacco Industry Research Committee published 350,000 copies of a new pamphlet, *Smoking and Health*, mostly sent to doctors and dentists.[50]

By the end of the 1950s, the tobacco industry had successfully developed ties with doctors, medical school faculty, and public health authorities across the country. In 1962, when U.S. Surgeon General Luther L. Terry established an Advisory Committee on Smoking and Health, the tobacco industry made nominations, submitted information, and ensured that Dr. Little "established lines of communication" with the committee.[51] To ensure that the panel was "democratically" constituted, the surgeon general invited nominations from the tobacco industry, as well as from the Federal Trade Commission (who would become involved if restrictions were placed on tobacco advertising). To ensure that the panel was unbiased, he excluded anyone who had publicly expressed a prior opinion. One hundred and fifty names were put forward, and the tobacco industry was permitted to veto anyone they considered unsuitable.[52]

Despite these concessions, the 1964 report was not favorable to the tobacco industry.[53] Historian Allan Brandt recounts how half the members of the panel were smokers, and by the time their report was ready, most of them had quit.[54] For those close to the science, this was no surprise, because the evidence against smoking had been steadily mounting. In 1957, the U.S. Public Health Service had concluded that smoking was "the principal etiological factor in the increased incidence of lung cancer."[55] In 1959, leading researchers had declared in the peer-reviewed scientific literature that the evidence linking cigarettes and cancer was "beyond dispute."[56] That same year, the American Cancer Society had issued a formal statement declaring that "cigarette smoking is the major causative factor in lung cancer."[57] In 1962, the Royal College of Physicians of London had declared that "cigarette smoking is a cause of cancer and bronchitis and probably contributes to . . . coronary heart disease," a finding that was prominently reported in *Reader's Digest* and *Scientific American*. Perhaps most revealingly, the tobacco industry's own scientists had come to the same conclusion.

As University of California professor Stanton Glantz and his colleagues have shown in their exhaustive reading of tobacco industry documents, by the early 1960s the industry's own scientists had concluded not only that smoking caused cancer, but also that nicotine was addictive (a conclusion that mainstream scientists came to only in the 1980s, and the industry would continue to deny well into the 1990s).[58] In the 1950s, manufacturers had advertised some brands as "better for your health," implicitly acknowledging health concerns.[59] In the early 1960s, Brown and Williamson's in-house scientists conducted their own experiments demonstrating that tobacco smoke caused cancer in laboratory animals, as well as experiments showing the addictive properties of nicotine. In 1963, the vice president of Brown and Williamson concluded, presumably with reluctance, "We are, then, in the business of selling nicotine, an addictive drug." Two years later, the head of research and development for Brown and Williamson noted that industry scientists were "unanimous in their opinion that smoke is . . . carcinogenic."[60] Some companies began secretly working on a "safe" cigarette, even while the industry as a whole was publicly denying that one was needed.

It's one thing for scientists to report something in peer-reviewed journals, however, and another for the country's doctor in chief to announce it publicly, loud and clear. The 1964 surgeon general's report, *Smoking and Health*, did just that. Based on review of more than seven thousand scientific studies and testimony of over one hundred and fifty consultants, the landmark report was written by a committee—in this case selected from nominations provided by the U.S. Food and Drug Administration, the Federal Trade Commission, the American Medical Association, and the Tobacco

Institute—but its conclusions were unanimous.[61] Lung cancer in the twentieth century had reached epidemic proportions, and the principal cause was not air pollution, radioactivity, or exposure to asbestos. It was tobacco smoking. Smokers were ten to twenty times more likely to get lung cancer than nonsmokers. They were also more likely to suffer from emphysema, bronchitis, and heart disease. The more a person smoked, the worse the effects.

Terry realized that the report's release would be explosive, so when he gathered two hundred reporters into the State Department for a two-hour briefing, the auditorium doors were locked for security.[62] The report was released on a Saturday to minimize impact on the stock market, but it was still a bombshell. Nearly half of all adult Americans smoked—many men had picked up the habit while serving their country during World War II or in Korea—and the surgeon general was telling them that this pleasurable habit, at worst a mild vice, was killing them. The government not only allowed this killing, but promoted and profited from it: the federal government subsidized tobacco farming, and tobacco sales were an enormous source of both federal and state tax revenues. To argue that tobacco killed people was to suggest that our own government both sanctioned and profited from the sale of a deadly product. In hindsight, calling it the biggest news story of 1964 seems insufficient; it was one of the biggest news stories of the era.[63] One tobacco industry PR director concluded that the cigarette business was now in a "grave crisis."[64] They did not sit idly by.

Immediately, they redoubled their effort to challenge the science. They changed the name of the Tobacco Industry Research Council to the Council for Tobacco Research (losing the word "industry" entirely), and severed their relations with Hill and Knowlton. They resolved that the new organization would be wholly dedicated to health research, and not to "industry technical or commercial studies."[65] They "refined" the approval and review process for grants, intensifying their search for "experts" who would affirm their views.

Given the evidence produced in their own laboratories, the industry might have concluded that the "debate" game was up. The PR director for Brown and Williamson suggested that perhaps the time had come to back off "assurances, denial of harm, and similar claims."[66] Others suggested identifying the hazardous components in cigarette smoke and trying to remove them, or adopting voluntary warning labels.[67] In 1978, the Liggitt Group—makers of L&Ms, Larks, and Chesterfields—filed a patent application for a technique to reduce the "tumorigenicity" of tobacco. (Tumorigenicity is the tendency of something to generate tumors, so this was an implicit acknowledgement that tobacco did indeed cause tumors, as one newspaper realized.)[68]

The cigarette manufacturers did not give up. Rather, they resolved to fight harder. "A steady expansion in our program of scientific research into tobacco use and health has convinced us of the need for more permanent organizational machinery," one press release concluded. The industry had already given more than $7 million in research funds to 155 scientists at more than one hundred American medical schools, hospitals, and laboratories; now it would give even more.[69] When Congress held hearings in 1965 on bills to require health warnings on tobacco packages and advertisements, the tobacco industry responded with "a parade of dissenting doctors," and a "cancer specialist [who warned] against going off 'half cocked' in the controversy."[70]

Sometimes further research muddies scientific waters, as additional complications are uncovered or previously unrecognized factors are acknowledged. Not so with smoking. When a new surgeon general reviewed the evidence in 1967, the conclusions were even starker.[71] Two thousand more scientific studies pointed emphatically to three results, enumerated on the report's first page: One, smokers lived sicker and died sooner than their nonsmoking counterparts. Two, a substantial portion of these early deaths would not have occurred if these people had never smoked. Three, were it not for smoking "practically none" of the early deaths from lung cancer would have occurred. Smoking killed people. It was as simple as that. Nothing had been learned since 1964 that brought into question the conclusions of the earlier report.[72]

How did the industry respond to this? More denial. "There is no scientific evidence that cigarette smoking causes lung cancer and other disease," Brown and Williamson insisted.[73]

In 1969, when the Federal Communications Commission voted to ban cigarette advertising from television and radio, Clarence Little insisted that there was "no demonstrated causal relationship between smoking or [*sic*] any disease."[74] Publicly, the industry supported the advertising ban, because under the Fairness Doctrine health groups were getting free antismoking advertisements on television, and these were having an effect.[75] Privately, however, the Tobacco Research Council sent materials to the liquor industry suggesting that it would be the next target.[76] In fact, the FCC had disavowed any such intentions, declaring in their own press release, "Our action is limited to the unique situation and product; we . . . expressly disclaim any intention to so proceed against other product[s]."[77] But the tobacco industry sought to foster the anxiety that controlling tobacco advertising was the first step down a slippery slope to controlling advertising of all sensitive products.

Despite industry fears, the U.S. Congress did not ban or even limit sales of tobacco, but it did require warning labels. The American people now knew that smoking was dangerous. And the danger wasn't just cancer. A host of ailments had been clearly linked to smoking: bronchitis, emphysema, coronary heart disease, hardening of the arteries, low birth weight in infants, and many more. As the 1960s came to a close, the numbers of Americans who smoked had declined significantly. By 1969, the number of adult Americans who smoked was down to 37 percent. By 1979 it would fall to 33 percent—among doctors it would fall to 21 percent—and the *New York Times* would finally stop quoting tobacco industry spokesmen to provide "balance."[78]

While smoking had declined, industry profits had not. In 1969, R. J. Reynolds reported net revenues of $2.25 billion. Despite the mounting political pressure to control tobacco sales and discourage tobacco use, Reynolds's directors reported records for sales, revenues, and earnings, and the continuation of its seventy-year record of uninterrupted dividends to its stockholders. "Tobacco," they concluded, "remains a good business."[79] Protecting that business—against regulation, punitive taxes, FDA control, and, especially, lawsuits—became a growing concern.[80]

Although 125 lawsuits related to health impairment were filed against the tobacco industry between 1954 and 1979, only nine went to trial, and none were settled in favor of the plaintiffs.[81] Still, industry lawyers were increasingly concerned, in part because their insistence that the debate was still open was contradicted not just by academic science, but by their own internal company documents. To cite just one example: in 1978, the minutes from a British American Tobacco Company research conference concluded that the tobacco-cancer link "has long ceased to be an area for scientific controversy."[82] (Brown and Williamson lawyers recommended the destruction or removal of documents that spoke to this point.)[83]

How could the industry possibly defend itself when the vast majority of independent experts agreed that tobacco was harmful, and their own documents showed that they knew this? The answer was to continue to market doubt, and to do so by recruiting ever more prominent scientists to help.

Collectively the industry had already spent over $50 million on biomedical research. Individual tobacco companies had invested millions more—

bringing the total to over $70 million. By the mid-1980s, that figure had exceeded $100 million. One industry document happily reported that "this expenditure exceeds that given for research by any other source except the federal government."[84] Another noted that grants had been distributed to 640 investigators in 250 hospitals, medical schools, and research institutions.[85] The American Cancer Society and American Lung Association in 1981 devoted just under $300,000 to research; that same year, the tobacco industry gave $6.3 million.[86] It was time to do even more.

In the 1950s, the tobacco industry had enlisted geneticist C. C. Little—a member of the U.S. National Academy of Sciences—to lend credibility to their position. This time they went one step better: they enlisted Dr. Frederick Seitz—the balding man introduced to Reynolds executives in 1979—a former *president* of the Academy.[87]

Seitz was part of the generation of bright young men whose lives were transformed by the Manhattan Project, catapulted into positions of power and influence on the basis of brainpower. Before World War II, physics was a fairly obscure discipline; nobody expected to become rich, famous, or powerful through a career in physics. But the atomic bomb changed all that, as hundreds of physicists were recruited by the U.S. government to build the most powerful weapon ever known. After the war, many of these physicists were recruited to build major academic departments at elite universities, where they frequently also served as consultants to the U.S. government on all kinds of issues—not just weapons.

Seitz's link to the atomic bomb was even closer than most. A solid-state physicist, he had trained under Eugene Wigner at Princeton, the man who, along with colleague Leo Szilard, convinced Albert Einstein to send his famous letter to Franklin Roosevelt urging him to build the atomic bomb. Later, Wigner won the Nobel Prize for work in nuclear physics; Seitz was Wigner's best and most famous student.

From 1939 to 1945, Seitz had worked on a variety of projects related to the war effort, including ballistics, armor penetration, metal corrosion, radar, and the atomic bomb. He also managed to complete a textbook published in 1940, *The Modern Theory of Solids*—widely acknowledged as the definitive textbook of its day on solid-state physics—and a second volume, *The Physics of Metals*, in 1943. He also found time to consult for the DuPont Corporation.

In 1959, Seitz became science advisor to NATO and from there moved into the highest echelons of American science and policy. From 1962 to 1969, he served as president of the National Academy of Sciences and as ex officio member of the U.S. President's Science Advisory Committee. In 1973, he received the National Medal of Science from President Richard Nixon. As Academy president, he developed an interest in biology, and in 1968 became president of the Rockefeller University—American's preeminent biomedical research center. In 1979 he went to work for R. J. Reynolds.

It's obvious why R. J. Reynolds would have wanted a man of Seitz's credentials on their team, but why would Seitz want to work for R. J. Reynolds?[88] Speaking to the industry executives in 1979, Seitz stressed the debt of gratitude he felt to Reynolds for the funding they had supplied his institution. Rockefeller was one of the universities that the tobacco industry had long funded, and Seitz put it this way:

> About a year ago, when my period as President of the Rockefeller University was nearing its end, [I was] asked if I would be willing to serve as advisor to the Board of Directors of R. J. Reynolds Industries, as it developed its program on the support of biomedical research related to degenerative diseases in man—a program which would enlarge upon the work supported through the consortium of tobacco industries. Since . . . R. J. Reynolds had provided very generous support for the biomedical work at The Rockefeller University, I was more than glad to accept.[89]

Reynolds *had* been generous to Rockefeller. In 1975 they had established the R. J. Reynolds Fund for the Biomedical Sciences and Clinical Research, with a grant of $500,000 per year for five years, with an additional $300,000 in year one to endow the R. J. Reynolds Industries Postdoctoral Fellowship "to make possible permanent recognition of RJR's assistance."[90]

There was a bit more to it than gratitude. Seitz also harbored an enormous grudge against the scientific community that he once led. Over the years, Seitz had come to view the scientific community as fickle, even irrational. As president of the National Academy, he had become "keenly aware how quickly, and irrationally, the mood of the membership of an organization can change. I could become highly unpopular almost overnight because of some seemingly trivial issue."[91]

Seitz was particularly unpopular for his support of the Vietnam War, which increasingly isolated him from colleagues on the President's Science Advisory Committee, who by the early 1970s had concluded not only that the war was a morass, but that they, like the rest of America, had been lied to about its progress.[92] As the 1970s drew to a close, Seitz also parted company with scientific colleagues on questions of nuclear preparedness. The scientific community generally supported arms limitations talks and treaties, and rejected as impossible the idea of achieving permanent technology superiority. Seitz, on the other hand, was committed to a muscular military strengthened by the most technologically advanced weaponry. He never rejected the idea of achieving American political superiority through superior weaponry, an idea that most colleagues had abandoned, but which would continue to crop up and cause conflict in the 1980s.

Above all, Seitz, like his mentor Eugene Wigner (a Hungarian refugee), was ardently anti-Communist. (Wigner in later years lent his support to Reverend Sun Myung Moon's Unification Church, evidently feeling that any enemy of Communism was his friend.)[93] Seitz's support for aggressive weapons programs was a reflection of this anti-Communism, but the feeling went further. As president of the Academy, Seitz had been a strong supporter of Taiwan, developing exchange programs with Taiwanese scientists as a counterbalance to the influence of "red" China. Exchange programs with Taiwanese scientists was an idea that most colleagues found reasonable enough, but in later years Seitz's anti-Communism would seem to lose a sense of proportion, as he increasingly defended anything that private enterprise did, and attacked anything with the scent of Socialism.[94]

Seitz justified his increasing social and intellectual isolation by blaming others. American science had become "rigid," he insisted, his colleagues dogmatic and closed-minded. The growing competition for federal funds stifled creativity, and discouraged work that didn't fall into clean disciplinary categories. This, perhaps, was the most important basis for his connection with the tobacco industry, as he explained in a presentation to Reynolds's International Advisory Board: "From time to time, [there are] exceptional cases where the ever-growing rigidity of the support provided by the federal government excludes the support of an important program in the hands of a distinguished and imaginative investigator."[95] Seitz would welcome the role of being the arbiter of who these distinguished and imaginative investigators were, and his judgment was not necessarily bad. Witness his support for Stanley Prusiner.

Seitz, however, did not simply want to support creative science. He was also angry at what he saw as an increasingly antiscience and antitechnology attitude in American life. He accepted the industry argument that attacks on the use of tobacco were "irrational," and that "independent" science was needed to "sift truth from fiction" (although independent from whom was never made clear).[96] Seitz saw irrationality everywhere, from the attack on tobacco to the "attempt to lay much of the blame for cancer upon industrialization."[97] After all, the natural environment was hardly carcinogen-free, he noted, and even "the oxygen in the air we breathe . . . plays a role in radiation-induced cancer."[98] (Oxygen, like most elements, has a radioactive version—oxygen-15—although it is not naturally occurring.)[99]

Seitz believed passionately in science and technology, both as the cause of modern health and wealth and the only means for future improvements, and it infuriated him that others didn't see it his way. In his memoir, he confidently proclaimed his faith in technology, insisting that "technology is continuously devising procedures to protect our health and safety and the natural beauty and resources of our world."[100]

While in his own mind a staunch defender of democracy, Seitz had an uneasy time with the masses. Environmentalists, he felt, were Luddites who wanted to reverse progress. His academic colleagues were ingrates who failed to appreciate what science and technology had done for them. Democracy as a whole had an uncertain relation to science, Seitz noted, and higher culture in general. Popular culture was a morass—Seitz despised Hollywood—and he wondered with more than a trace of bitterness whether the "culminating struggle to create free and open societies" would culminate in the "triumph of the ordinary." Seitz did not help build the atomic bomb to make the world safe for action-adventure films.[101]

These attitudes all help to explain how and why Seitz would have been willing to work for the tobacco industry. And there is one more important piece of the puzzle. Like C. C. Little before him, Seitz was something of a genetic determinist (perhaps because he was loath to admit that environmental hazards related to technology might cause serious health harms, or perhaps because he just saw the science that way). In his memoir, he attributed the early death of his friend William Webster Hansen, co-inventor of the klystron (important in the development of radar) to "a genetic defect leading to emphysema," but this interpretation is highly unlikely.[102]

Medical experts believe that emphysema is almost invariably caused by environmental assaults. The Aetna insurance company concludes that up to 90 percent of cases are caused by smoking and most of the rest to other airborne toxins; only 1 percent of cases are attributable to a rare genetic defect.[103] Hansen's case was strange, because he died so young—only thirty-nine—so perhaps he did have a genetic defect, but his disease could also have been caused by inhaling the beryllium he used in his research.[104]

Beryllium is well-known to be extraordinarily toxic; in later years the U.S. federal government would compensate workers exposed to beryllium in the nation's nuclear weapons programs.[105] Seitz clearly had trouble accepting that Hansen's exposure to beryllium could have been the cause of his early death.[106]

Given these various views—hawkish, superior, technophilic, and communophobic—Seitz may well have felt more comfortable in the company of conservative men from the tobacco industry (who perhaps shared his political views) than with his mostly liberal academic colleagues (who generally did not). Over the years, he had spent a good deal of time in corporate America, first as a physicist at General Electric in the 1930s, and then, for thirty-five years during his academic career, as a consultant to DuPont. He was also a member of the Bohemian Grove, an exclusive men's club in San Francisco, which in those days counted among its members Secretary of Defense Caspar Weinberger, as well as many executives of California banks, oil companies, and military-industrial contractors. (One former president of Caltech recalls that he joined Bohemian Grove because the trustees of his institution insisted it was important, but as a liberal and a Jew he never felt comfortable there.)[107]

Seitz no doubt also enjoyed the perks he received while working for the tobacco industry, such as flying to Bermuda with his wife when the Reynolds Advisory Committee met there in November 1979, as well as the heady feeling of distributing money to researchers that he had handpicked.[108] Given his views that genetic weakness was the crux of disease susceptibility, and that modern science had become narrow-minded, Seitz may well have honestly believed that tobacco was being unfairly attacked, and that Reynolds money could do some real good. But we know from tobacco industry documents that the criteria by which he chose projects for funding were not purely scientific.

By May 1979, Seitz had made commitments for over $43.4 million in research grants. During this time, he corresponded frequently with H. C. Roemer—R. J. Reynolds's legal counsel—discussing with him which particular projects they planned to fund and why; all press releases regarding the research program had to be cleared by the legal department.[109] It's not normal for granting agencies to consult legal counsel on each and every grant they make, so this connection alone might suggest a criterion related to legal liability. But we don't have to speculate, because industry documents tell us so: "Support [for scientific research] over the years has produced a number of authorities upon whom the industry could draw for expert testimony in court suits and hearings by governmental bodies."[110] The industry wasn't just generating reasonable doubt; it was creating friendly witnesses—witnesses that could be called on in the future.

One of these witnesses was Martin J. Cline, who had earlier caught Seitz's attention. Cline was one of the most famous biomedical researchers in the United States. Chief of the Division of Hematology-Oncology at UCLA's medical school, he had created the world's first transgenic organism: a genetically modified mouse. In 1980, however, he was censured by UCLA and the National Institutes of Health for an unapproved human experiment injecting bone marrow cells that had been altered with recombinant DNA into two patients with a hereditary blood disorder.[111] Cline was found to have misrepresented the nature of the experiment to hospital authorities, telling them that the experiment did not involve recombinant DNA.[112] He later admitted that he had performed the experiments, but claimed that he did it because he believed it would work. Cline lost nearly $200,000 in research grants and was forced to resign his position as division chief, although he was permitted to stay on as a professor of medicine.[113]

Many years later—in 1997—Cline was deposed in the case of *Norma R. Broin et al. v. Philip Morris*.[114] (Broin was a nonsmoking flight attendant who contracted lung cancer at the age of thirty-two, and sued—along with her husband and twenty-five other flight attendants—charging that their illnesses were caused by secondhand smoke in airline cabins, and the tobacco industry had suppressed information about its hazards.)[115] In the deposition Cline acknowledged that he had been a witness in two previous trials, one in which he testified that a plaintiff's cancer was not caused by exposure to toxic fumes, and another in which he testified that a plaintiff's leukemia was not caused by exposure to radiation. He had also served as a paid consultant in a previous tobacco litigation case, had given seminars to a law firm representing the tobacco industry, and had served on a so-called Scientific Advisory Board for R. J. Reynolds. (The scientists that Seitz supported were also sometimes called upon as an advisory group, attending periodic meetings to offer "advice and criticism." One letter suggested that they might also act as an advocacy group—although this was later struck out.)[116]

When asked point blank in the Norma Broin case, "Does cigarette smoking cause lung cancer?" attorneys for Philip Morris objected to the "form of the question."[117] When asked, "Does direct cigarette smoking cause lung cancer?" the attorneys objected on the grounds that the question was "irrelevant and immaterial." When finally instructed to answer, Cline was evasive.

> Cline: Well, if by "cause" you mean a population base or epidemiologic risk factor, then cigarette smoking is related to certain types of lung cancer. If you mean: In a particular individual is the cigarette smoking the cause of his or her cancer? Then . . . it is difficult to say "yes" or "no." There is no evidence.[118]

When asked if a three-pack-a-day habit might be a *contributory* factor to the lung cancer of someone who'd smoked for twenty years, Cline again answered no, you "could not say [that] with certainty . . . I can envision many scenarios where it [smoking] had nothing to do with it." When asked if he was paid for the research he did on behalf of the tobacco industry, he acknowledged that the tobacco industry had supplied $300,000 per year over ten years—$3 million—but it wasn't "pay," it was a "gift."[119]

What Cline said about cancer was technically true: current science does not allow us to say with certainty that any one particular person's lung cancer—no matter how much she smoked—was caused by smoking. There are always other possibilities. The science *does* tell us that a person with a twenty-year, three-pack-a-day habit who has lung cancer most probably got that cancer from smoking, because other causes of lung cancer are very rare. If there's no evidence that the woman in question was ever exposed to asbestos or radon, or smoked cigars or pipes, or had prolonged occupational exposure to arsenic, chromium, or nickel, then we could say that her lung cancer was almost certainly caused by her heavy smoking. But we couldn't say it for sure. In scientific research, there is always doubt. In a lawsuit we ask, Is it *reasonable* doubt? Ultimately, juries began to say no, but it took a long time, in large part because of witnesses like Martin Cline, witnesses that the industry had cultivated by supporting their research. Reynolds supported scientists, and when the need arose they were available to support Reynolds.

Stanley Prusiner would have been an even better witness for the industry—his work on prions was groundbreaking and his reputation untarnished—and his name did appear on a list of potential witnesses in the 2004 landmark federal case against the tobacco industry: *U.S. vs. Philip Morris et al.*[120] (He evidently did not testify; available documents do not indicate why.) The industry was finally found guilty under the RICO Act (Racketeer Influenced and Corrupt Organizations).[121] In 2006, U.S.

district judge Gladys Kessler found that the tobacco industry had "devised and executed a scheme to defraud consumers and potential consumers" about the hazards of cigarettes, hazards that their own internal company documents proved they had known about since the 1950s.[122]

But it took a long time—just about half a century—to get to that point. Along the way the tobacco industry won many of the suits that were brought against it. Juries, of course, were much more likely to believe scientific experts than industry executives—especially scientists who appeared to be independent—and neither Cline nor Prusiner ever worked "directly" for the tobacco industry; many of the funds were channelled through law firms.[123] External research could also help bolster the industry's position that the public should decide for themselves. "We believe any proof developed should be presented fully and objectively to the public and that the public should then be allowed to make its own decisions based on the evidence," they had argued, seemingly reasonably.[124] The problem was that public had no way to know that this "evidence" was part of an industry campaign designed to confuse. It was, in fact, part of a criminal conspiracy to commit fraud.

Cline and Prusiner were reputable scientists, so one might ask, Didn't they have a right to be heard? In later years Seitz and his colleagues would often make this claim, insisting that they deserved equal time, and their ability to invoke the Fairness Doctrine to obtain time and space for their views in the mainstream media was crucial to the impact of their efforts. Did they deserve equal time?

The simple answer is no. While the idea of equal time for opposing opinions makes sense in a two-party political system, it does not work for science, because science is not about opinion. It is about evidence. It is about claims that can be, and have been, tested through scientific research—experiments, experience, and observation—research that is then subject to *critical review by a jury of scientific peers.* Claims that have not gone through that process—or have gone through it and failed—are not scientific, and do not deserve equal time in a scientific debate.

A scientific hypothesis is like a prosecutor's indictment; it's just the beginning of a long process. The jury must decide not on the elegance of the indictment, but on the volume, strength, and coherence of the evidence to support it. We rightly demand that a prosecutor provide evidence—abundant, good, solid, consistent evidence—and that the evidence stands up to the scrutiny of a jury of peers, who can take as much time as they need.

Science is pretty much the same. A conclusion becomes established not when a clever person proposes it, or even a group of people begin to discuss it, but when the jury of peers—the community of researchers—reviews the evidence and concludes that it is sufficient to accept the claim. By the 1960s, the scientific community had done that with respect to tobacco. In contrast, the tobacco industry was never able to support its claims with evidence, which is why they had to resort to obfuscation. Even after decades and tens of millions of dollars spent, the research they funded failed to supply evidence that smoking was really OK. But then, that was never really the point of it anyway.

THE TOBACCO INDUSTRY was found guilty under the RICO statute in part because of what the Hill and Knowlton documents showed: that the tobacco industry knew the dangers of smoking as early as 1953 and conspired to suppress this knowledge. They conspired to fight the facts, and to merchandise doubt.

But it took a long time for those facts to emerge, and the doubt to be dispelled. For many years, the American people did continue to think that there was reasonable doubt about the harms of smoking (and some still do). While hazard labels were strengthened, it was not until the 1990s that the industry began to lose cases in courts. And although the FDA sought to regulate tobacco as an addictive drug in the early 1990s, it was not until 2009 that the U.S. Congress finally gave them the authority to do so.[125]

One reason the industry's campaigns were successful is that not everyone who smokes gets cancer. In fact, most people who smoke will not get lung cancer. They may suffer chronic bronchitis, emphysema, heart disease, or stroke, and they may suffer cancer of the mouth, uterus, cervix, liver, kidney, bladder, or stomach. They may develop leukemia, suffer a miscarriage, or go blind. The children of women who smoke are much more likely to be low birth weight babies than the children of women who don't, and to suffer high rates of sudden infant death syndrome. Today, the World Health Organization finds that smoking is the known or probable cause of twenty-five different diseases, that it is responsible for five million deaths worldwide every year, and that half of these deaths occur in middle age.[126] By the 1990s, most Americans knew that smoking was generally harmful, but as many as 30 percent could not tie that harm to specific disease. Even many doctors do not know the full extent of tobacco harms, and nearly a quarter of poll respondents still doubt that smoking is harmful at all.[127]

Industry doubt-mongering worked in part because most of us don't really understand what it means to say something is a cause. We think it means that if A causes B, then if you do A, you will get B. If smoking causes cancer, then if you smoke, you will get cancer. But life is more complicated than that. In science, something can be a *statistical* cause, in the sense that that if you smoke, you are much more *likely* to get cancer. Something can also be a cause in the everyday sense of being an occasion for something—as in "the cause of the quarrel was jealousy."[128] Jealousy does not always cause quarrels, but it very often does. Smoking does not kill everyone who smokes, but it does kill about half of them.

Doubt-mongering also works because we think science is about facts—cold, hard, definite facts. If someone tells us that things are uncertain, we think that means that the science is muddled. This is a mistake. There are always uncertainties in any live science, because science is a process of discovery. Scientists do not sit still once a question is answered; they immediately formulate the next one. If you ask them what they are doing, they won't tell you about the work they finished last week or last year, and certainly not what they did last decade. They will tell you about the new and uncertain things they are working on *now*. Yes, we know that smoking causes cancer, but we still don't fully understand the mechanism by which that happens. Yes, we know that smokers die early, but if a particular smoker dies early, we may not be able to say with certainty how much smoking contributed to that early death. And so on.

Doubt is crucial to science—in the version we call curiosity or healthy skepticism, it drives science forward—but it also makes science vulnerable to misrepresentation, because it is easy to take uncertainties out of context and create the impression that *everything* is unresolved. This was the tobacco industry's key insight: that you could use *normal* scientific uncertainty to undermine the status of actual scientific knowledge. As in jujitsu, you could use science against itself. "Doubt is our product," ran the infamous memo written by one tobacco industry executive in 1969, "since it is the best means of competing with the 'body of fact' that exists in the minds of the general public."[129] The industry defended its primary product—tobacco—by manufacturing something else: doubt about its harm. "No proof" became a mantra that they would use again in the 1990s when attention turned to secondhand smoke. It also became the mantra of nearly every campaign in the last quarter of the century to fight facts.

For tobacco is not the end of our story. It is just the beginning. In the years to come various groups and individuals began to challenge scientific

evidence that threatened their commercial interests or ideological beliefs. Many of these campaigns involved the strategies developed by the tobacco industry, and some of them involved the same people. One of these people was Frederick Seitz.

As the industry campaign to defend tobacco was reaching the end of its course—and the claim that smoking's harms were unproven became harder to say with a straight face—Seitz moved on to other things. One of these was to found the George C. Marshall Institute, created to challenge scientists' conclusions in a whole new arena—strategic defense. When that debate was over, they would turn to the environment. Seitz had railed about scientific colleagues who made "simplified, dramatic statements" to capture public attention, rather than remaining "sober," yet in the later years of his life, he would do exactly that when discussing the ozone hole, global warming, and other environmental threats.[130]

The tobacco road would lead through Star Wars, nuclear winter, acid rain, and the ozone hole, all the way to global warming. Seitz and his colleagues would fight the facts and merchandise doubt all the way.

CHAPTER 3

Sowing the Seeds of Doubt: Acid Rain

WHILE THE DEBATE OVER STRATEGIC defense and nuclear winter was playing out, another rather different issue had come to the fore: acid rain. While the science of nuclear winter was entirely different from that of acid rain, some of the same people would be involved in both debates. And as in the debate over tobacco, opponents of regulating the pollution that caused acid rain would argue that the science was too uncertain to justify action.

The story begins in 1955, when the U.S. Department of Agriculture established the Hubbard Brook Experimental Forest in central New Hampshire. Experimental forest might seem like an oxymoron—forests are natural; experiments are man-made—but the idea was the same as what scientists do in laboratories: take an object or question and investigate it intensively. In this case the object was the "watershed ecosystem"—the forest, the diverse plants and animals associated with it, and the water flowing through it.

Hydrological studies at Hubbard Brook had been pioneered by a U.S. Forest Service scientist named Robert S. Pierce, who teamed up with F. Herbert Bormann, a biology professor at Dartmouth College, and two bright young assistant professors, biologist Gene E. Likens and geologist Noye M. Johnson. In 1963, Bormann, Likens, Johnson, and Pierce established the Hubbard Brook Ecosystem Study. That same year they discovered acid rain in North America.[1]

"Discovered" is perhaps too strong a word, because naturally acidic rain—caused by volcanoes or other natural phenomena—had been known since

the Renaissance, and man-made acid rain had been recognized since the nineteenth century in areas close to industrial pollution in the British Midlands and central Germany.[2] But Hubbard Brook was located in the White Mountains of New Hampshire, a refuge where New Yorkers and Bostonians sought shelter from the haste and waste of urban centers, far from any major cities or factories. Yet its rain had a measured pH of 4 or less (neutral pH is 7, ordinary rain is around 5); one sample measured 2.85—about the same as lemon juice, acidic enough to burn a cut. Acid rain in this remote a setting was new, and worrisome.

The Hubbard Brook work came at a crucial time, coinciding with a shift in American thinking about environmentalism. In the first half of the twentieth century, conservationists such as Theodore Roosevelt, John D. Rockefeller, John Muir, and Gifford Pinchot sought to preserve and protect America's beautiful and wild places, in part by creating special areas—like Yosemite, Yellowstone, and the Grand Tetons—set aside from daily use and development. "Preservationist" environmentalism was broadly popular and bipartisan; Roosevelt was a progressive Republican, Rockefeller a captain of industry. Preservationism was mostly driven by aesthetics and moral values, and by the desire for restorative recreation. It did not depend on science. Preservationists were often interested in science—particularly the natural historical kind, like geology, zoology, and botany—but they did not need science to make their case.

For decades, preservationist environmentalism remained bipartisan. When the Wilderness Act of 1964 designated over nine million acres of American lands as "areas where man himself is a visitor and does not remain," it passed the U.S. Senate by a vote of 73–12, and the House of Representatives by a vote of 373–1.[3] Richard Nixon, a president not generally recalled as a visionary environmentalist, created the Environmental Protection Agency and signed into law several signature pieces of environmental legislation: the Clean Air Act Extension, the Clean Water Act, the Endangered Species Act, and the National Environmental Policy Act. Things were changing, though, and within a few years, Ronald Reagan would begin to shift the Republican Party away from both environmental preservation and environmental regulation, a position that would separate the party from its historic environmentalism, and put it on a collision course with science.

Bills like the Clean Air Act reflected a shift in focus from land preservation to pollution prevention through science-based government regulation, and from local to global. These were profound shifts. *Silent*

Spring—Rachel Carson's alarm bell over the impacts of the pesticide DDT—led Americans to realize that local pollution could have global impacts. Private actions that seemed reasonable—like a farmer spraying his crops to control pests—could have unreasonable public impacts. Pollution was not simply a matter of evil industries dumping toxic sludge in the night: people with good intentions might unintentionally do harm. Economic activity yielded collateral damage. Recognizing this meant acknowledging that the role of the government might need to change in ways that would inevitably affect economic activity.

Collateral damage was what acid rain was all about. Sulfur and nitrogen emissions from electrical utilities, cars, and factories could mix with rain, snow, and clouds in the atmosphere, travel long distances, and affect lakes, rivers, soils, and wildlife far from the source of the pollution. At least, this is what the Hubbard Brook work seemed to show. Throughout the mid to late 1960s and into the 1970s, the Hubbard Brook scientists studied the phenomenon in great detail, writing numerous scientific articles and reports. Then, in 1974, Gene Likens took the lead on a paper submitted to *Science*, declaring unequivocally: "Acid rain or snow is falling on most of the northeastern United States."[4] The phenomenon appeared to have reached Hubbard Brook about twenty years before, they explained, and was associated with the introduction of tall smokestacks in the Midwest.[5] The government would have to take acid rain into account when it set rules and regulations for air pollution.

Chemical analysis showed that most of the acidity was due to dissolved sulfate and the rest mostly to dissolved nitrate, by-products of burning coal and oil. Yet fossil fuels had been burned enthusiastically since the mid-nineteenth century, so why had this problem only arisen of late? The answer was the unintended consequence of the introduction of devices to remove particles from smoke and to reduce local air pollution.

In industrial England, particulate pollution was so bad it killed people—famously in the great smog of London in 1952—and dramatic steps had been taken to reduce it by using taller smokestacks to disperse the pollution more widely, and by installing particle removers, or "scrubbers," at power plants. However, scientific work subsequently showed that the offending particles also neutralize acid, so that removing them inadvertently increased the acidity of the remaining pollution. Particles also tend to settle back to Earth fairly quickly, so while tall smokestacks had successfully decreased local pollution, they had increased regional pollution, transforming local soot into regional acid rain.[6]

But was acid rain a problem? As we will see in later chapters, studies of global warming and the ozone hole involved predicting damage before it was detected. It was the prediction that motivated people to check for damage; research was intended in part to test the prediction, and in part to stimulate action before it was too late to stop—so too, here. It was too soon to tell whether or not widespread and serious ecological damage was occurring, but the potential effects were troubling. They included leaching of nutrients from soils and plant foliage, acidification of lakes and rivers, damage to wildlife, and corrosion of buildings and other structures. Still, if the point were to prevent damage before it happened, then such arguments were necessarily speculative. A careful scientist would be in a bit of a bind: wanting to prevent damage, but not being able to *prove* that damage was coming.

So scientists looked for early warning signs, and they found them. Studies in Sweden suggested that acid precipitation was reducing forest growth. Studies in the United States and elsewhere documented the damaging effects of acidity on plant growth, leaf tissue development, and pollen germination. In Sweden, Canada, and Norway, acidification of lakes and rivers was correlated with increased fish mortality.

Many of the details had been published in very specialized journals (which few journalists or congressional staffers routinely read) or in government reports. The Swedish results were, not unreasonably, mostly published in Swedish.[7] This technical difficulty had also been true for the damage from DDT, much of which had been documented in government reports, which Rachel Carson gathered together in *Silent Spring*, and for the risks of taking the birth control pill, which were first documented in specialized ophthalmology journals when otherwise healthy young women developed mysterious blood clots.[8] This is a characteristic pattern in science: first there is scattered evidence of a phenomenon, published in specialist journals or reports, and then someone begins to connect the dots.

Likens and his colleagues were connecting the dots, and so was Swedish meteorologist Bert Bolin—who would later help to create the Intergovernmental Panel on Climate Change. In 1971, Bolin led a panel on behalf of the Swedish government in preparation for a United Nations Conference on the Human Environment, cochaired by Svante Odén, one of the first Europeans to document the impacts of acid rain on soils.[9] Their report, *Air pollution across national boundaries: The impact on the environment of sulfur in air and precipitation, Sweden's case study for the United Nations conference on the human environment*, laid out the essentials. It explained the evidence of acid rain, the chemistry of how it formed, the physics of how it dispersed, and the effects it had or was likely to have on human health, plant life, soils, lakes and rivers (and the fish in them), and buildings and other structures. (Among other things, the report illustrated corrosion from acid rain of a set of nickel door handles.)[10]

Although the exact magnitude of the acid rain effects was uncertain, their existence and gravity was not, and the Swedes warned against discounting the effects just because they weren't immediate, or fully documented. Although occurring gradually, the effects were serious, and potentially irreversible. However, the situation was not all bleak, because the cause was known, and so was the remedy. "A reduction in the total emissions both in Sweden and in adjacent countries is required."[11]

In science, this sort of clear demonstration of a phenomenon should inspire fellow scientists to learn more. It did. Over the next ten years, scientists around the globe worked to document acid rain, understand its dimensions, and communicate its significance. In 1975, the U.S. Department of Agriculture sponsored the first International Symposium on Acid Precipitation and the Forest Ecosystem.[12] In 1976, the International Association for Great Lakes Research held a symposium, cosponsored by the U.S. Environmental Protection Agency and Environment Canada, on the effects of acid rain on lakes.[13] That same year, Canadian scientists documented the extinction of fish species in acidified lakes in the nickel-mining district of Sudbury, Ontario.[14]

As acid precipitation came to be seen as a global problem, scientists working on it were increasingly able to get their papers published in high-profile journals. In 1976, two Norwegian scientists reported in *Nature* on massive fish kills associated with pH shock, caused by a sudden influx of spring meltwaters from acidic snow and ice.[15] Gene Likens summarized these results in *Chemical and Engineering News*—the official magazine of the American Chemical Society—explaining that acid rain and snow were having "a far-reaching environmental impact." This included sharp declines of fish in lakes and streams, damage to trees and other plants, and corrosion of buildings, and maybe damage to human health.[16]

A few years later, skeptics would argue that the science was not yet really firm, but Likens's summary shows otherwise. However, the way *Chemical and Engineering News* framed it also shows that resistance to the scientific evidence was already beginning to emerge. Likens's argument was clear—acid rain was happening, it was caused by pollution, and it was killing fish and trees and possibly harming people—but in a caption that sat

above the article's title, the editors wrote, "The acidity of rain and snow falling on parts of the U.S. and Europe has been rising—for reasons that are still not entirely clear and with consequences that have yet to be well evaluated."[17]

Were the reasons not entirely clear? It depended on what you meant by *entirely*. Science is hard—why so many kids hate it in school—and nothing is ever *entirely* clear. There are always more questions to be asked, which is why expert consensus is so significant—a point we will return to later in this book. For acid rain, the consensus of experts was that anthropogenic sulfur was implicated, but exactly how that sulfur moved through the atmosphere and exactly how much damage it could do was still being worked out. On the other hand, negative effects on fish and forests were clear, so why did *Chemical and Engineering News* suggest otherwise?

Herbert Bormann, at this point teaching at Yale, thought that ambiguity arose from confusing different types of uncertainty. There was no question that acid rain was real. Rainfall in the northeastern United States was many times more acidic than it used to be. The uncertainty was about the *precise* nature of its cause: tall smokestacks—dispersing sulfur higher in the atmosphere—or just increased use of fossil fuels overall?[18] Moreover, while the broad picture was emerging, many details were still to be sorted out, some of them quite important. Chief among these was the question: did we know *for sure* that the sulfur was anthropogenic—made by man—rather than natural? This question would recur in debates over ozone and global warming, so it's worth understanding how it was answered here.

Bolin and his Swedish colleagues had made "mass balance arguments": they considered how much sulfur could be supplied by the three largest known sources—pollution, volcanoes, and sea spray—and compared this with how much sulfur was falling as acid rain. Since there are no active volcanoes in northern Europe, and sea spray doesn't travel very far, they deduced that most of the acid rain in northern Europe had to come from air pollution. Still, this was an indirect argument. To really prove the point, you'd want to show that the actual sulfur in actual acid rain came from a known pollution source. Fortunately there was a way to do this—using isotopes.

Scientists love isotopes—atoms of the same element with different atomic weights, like carbon-12 and carbon-14—because they are exceptionally useful. If they are radioactive and decay over time—like carbon-14—they can be used to determine the age of objects, like fossils and archeological relics. If they are stable, like carbon-13—or sulfur-34—they can be used to figure out where the carbon or sulfur has come from.[19] Different sources of sulfur have different amounts of sulfur-34, so you can use the sulfur isotope content as a "fingerprint" or "signature" of a particular source, either natural or man-made. In 1978, Canadian scientists showed that the isotopic signature of sulfur in acid rain in Sudbury was identical to the sulfur in the nickel minerals being mined there. In later years, some skeptics would argue that the acid in acid rain came from volcanoes (they would say the same about fluorine and ozone depletion, and about CO_2 and global warming), but the isotope analysis showed that *couldn't* be true.[20] In any case there are no active volcanoes in Ontario.

Meanwhile, Noye Johnson—the geologist in the original Hubbard Brook team—and his colleagues had made a crucial discovery. The acid rain story contained an anomaly: rain at Hubbard Brook was acidic, but the pH of the local streams was mostly normal. Why didn't the acidity affect the local streams? Johnson and his colleagues now explained why: the acidic rain was neutralized as it moved through soils. Acid precipitation fell onto the forest floor, where it reacted with minerals in the soils. These reactions stripped the soils of essential nutrients—particularly calcium—and simultaneously buffered the acidity of the water. The buffered water then percolated into local streams. This explained why the pH of the streams was largely unaffected even while the soils were being damaged and overall stream chemistry being changed. The results were reported first in *Science*; then Johnson took the lead on a more detailed paper that would become the third most cited scientific paper ever written on acid rain—published in the elite journal *Geochimica and Cosmochimica Acta*.[21]

The basic science of acid rain was now understood. Scientists had been working steadily on the question for nearly twenty-five years, demonstrating the existence of acid rain, its causes, and its effects on soils, streams, and forests. Major articles had been published in the world's most prominent scientific journals, as well as in many specialist journals and government reports. In 1979, when Likens and his colleagues summarized the arguments for the general scientific reader in *Scientific American*, the magazine's editors did not cast doubt or raise uncertainties. In a summary below the article's title, the editors encapsulated: "In recent decades, the acidity of rain and snow has increased sharply over wide areas. The principal cause is the release of sulfur and nitrogen by the burning of fossil fuels."[22] Not a maybe, possibly, or probably in sight.

Scientific American is often viewed as the place where well-established science is explained to the general public. If so, then we can say that 1979

was the year in which the American people were told about acid rain. As if to seal the case, an eight-year Norwegian study designed to integrate all the evidence related to acid precipitation was reviewed in *Nature* in the summer of 1981. The message? "It has now been established beyond doubt that the precipitation in southern Scandinavia has become more acidic as a result of long-distance transport of air pollution."[23] If this were a court of law, the jury would now have ruled the defendant guilty beyond a reasonable doubt. But science is not a courtroom, and environmental problems involve far more than science. Acid rain had become the first global environmental problem, and with that came global challenges.

Political Action and the U.S.-Canadian Rift

In 1979, the United Nations Economic Commission for Europe passed the Convention on Long-range Transboundary Pollution. Based on the Declaration of the U.N. Conference on the Human Environment—the one for which Bert Bolin's report had been prepared—the convention insisted that all nations have responsibility to "ensure that activities within their jurisdiction or control do not cause damage to the environment of other states or of areas beyond the limits of national jurisdiction."[24] Henceforth, it would be illegal to dump your pollution on someone else, whether you did it with trucks or with smokestacks.

The 1979 convention committed its signatories to control any emissions into the air that could harm human health, property, or the natural environment. Article 7 specifically focused on sulfur, with its impacts on agriculture, forestry, materials, aquatic and other natural ecosystems, and visibility. When the signatories met again in 1985, they set firm limits on sulfur emissions, mandating 30 percent reductions.[25]

Meanwhile, the United States and Canada had started their own bilateral negotiations, and in July 1979, the two countries issued a Joint Statement of Intent to move toward a formal agreement. The statement outlined eight general principles, including prevention and reduction of transboundary air pollution, and development of strategies to limit emissions. The overall goal was "a meaningful agreement that will make a real contribution to reducing air pollution and acid rain."[26]

While negotiations were proceeding behind the scenes, a confluence of scientists, environmentalists, and political leaders convened in Canada in November for an "Action Seminar on Acid Precipitation." The U.S.

government was represented by Gus Speth, chairman of President Carter's Council on Environmental Quality. Speth thought the way forward was clear. Some years before, he noted, industry leaders had objected to emissions reductions, arguing that tall smokestacks could remedy the problem by dispersing the pollutants high in the atmosphere where they would "finally come down in harmless traces." One electric utility, he recalled, had been particularly shrill, taking out newspaper and magazine ads blasting "irresponsible environmentalists" who insisted on absurdly strict emission standards at the expense of jobs and the economy.[27]

Those "irresponsible" environmentalists had been right: the emissions had *not* come down in harmless traces, but as acid rain. This could have been avoided had the power companies done the right thing and controlled pollution at the source, rather than attempting to get around air quality standards by building taller smokestacks and attacking environmentalists. Still, Speth was optimistic, because "both at home and internationally, we are beginning to address the acid rain problem with the seriousness it deserves."[28]

The Carter administration tried to. As Environment Canada concluded that more than half the acid rain falling in Canada was coming from U.S. sources, President Carter signed the Acid Precipitation Act of 1980, which established the National Acid Precipitation Assessment Program (NAPAP), a comprehensive ten-year research, monitoring, and assessment program to determine the effects of sulfur and nitrogen oxides on the environment and human health.[29]

Carter also created the federal Acid Rain Coordinating Committee and began negotiations with the Canadian federal government for scientific and political cooperation on acid rain. Canada and the United States signed a Memorandum of Intent concerning transboundary air pollution, committing both nations to enforcing air pollution control laws and establishing a series of technical working groups to evaluate the scientific basis for a new, stronger treaty to stop acid rain.

Then the political winds in America changed.

Skepticism in the Reagan White House

In 1980, Ronald Reagan came to power in the United States on a platform of reducing regulation, decreasing the reach of the federal government, and unleashing the power of private enterprise. Government, the new president insisted, was not the solution but the problem. Reagan was charismatic, his demeanor relaxed and genial, and his worldview put his administration on a collision course with the scientists working on acid rain.

The new administration did not oppose NAPAP.[30] Diverse groups and constituencies agreed that it made sense to reduce the scientific uncertainties, particularly if the cost of mitigation would be high. But as events unfolded, the administration's position began to diverge from the scientific community, and strongly.

In 1983, the technical working groups established under the 1980 Memorandum of Intent affirmed that acid rain caused by sulfur emissions was real and causing serious damage. The solution was to reduce these emissions—the necessary technology already existed—and if reductions were not made, damage would increase.[31] At the last minute, however, the U.S. representatives seemingly backpedaled. When the working group results were summarized, the U.S. versions were much weaker than the Canadians expected.

The Canadian government asked the Royal Society of Canada to review the documents compiled by the working groups. Chaired by F. Kenneth Hare, a distinguished meteorologist and provost at the University of Toronto, the review panel included two scientists from the United States, one from Sweden, and one from Denmark. They also consulted with other several other experts, including Bert Bolin.

The panel began by noting a common problem among scientists: the tendency to emphasize uncertainties rather than settled knowledge. Scientists do this because it's necessary for inquiry—the research frontier can't be identified by focusing on what you already know—but it's not very helpful when trying to create public policy. The panel wished that the working group scientists had begun with a "clear statement of what is known." This, in their view, included three crucial facts: one, that detrimental acidification of large areas of the continent had been occurring for decades; two, that acid deposition could be quantitatively related to anthropogenic emissions through long-range atmospheric transport; and three, that emissions and pollutants were crossing the U.S.-Canadian border in both directions, so both countries had a stake in preventing them.[32] "The evidence supporting these conclusions is persuasive, and, in the opinion of most Panel members, overwhelms residual uncertainties in our knowledge . . . The existence of a severe problem of environmental acidification . . . is not in doubt."[33] But the U.S. summaries seemed to suggest otherwise.

The reports of the technical working groups revealed overall broad agreement, especially on big picture issues: "The facts about acid deposition are actually much clearer than in other environmental *causes célèbres*," Hare's panel concluded, but when these facts had been gathered, something peculiar had happened.[34] There had been numerous "changes in scientific content" as the report went through successive drafts, changes that made the summaries more ambiguous than the reports themselves.[35] Moreover, while most of the report was "agreed text," the U.S. and Canadian groups had submitted different versions of the conclusions. The U.S. version saw far greater uncertainty than the Canadian one. It did *not* accept that cause and effect had been established, on the grounds that the relative importance of different contributing factors had not been quantified, and potentially offsetting processes had not been fully investigated.

This was like saying that we know that both cigarettes and asbestos cause lung cancer, but we can't say either is proven, because we don't know exactly how much cancer is caused by one and how much by the other, and we don't know whether eating vegetables might prevent those cancers. The Canadian group fell short of accusing the United States of tampering with the evidence, but they certainly implied it. In the panel's words: "The U.S. version of the text cannot be reconciled with the evidence as presented in the *agreed* text."[36] The following year, Environment Canada put it this way: "In each country independent peer review experts have indicated the need for action based on what we now know."[37] But that was not how the U.S. government saw it, and in January 1984 Congress rejected a joint pollution control program. What had happened?

SCIENCE IS NEVER FINISHED, so the relevant policy question is always whether the available evidence is *persuasive*, and whether the established facts outweigh the residual uncertainties. This is a judgment call. Chris Bernabo, who worked at the White House Council on Environmental Quality at the time and served as research director for the Interagency Task Force on Acid Precipitation, suggests that because so much more was at stake for Canada—70 percent of their economy at the time came from forests and fish or tourism related to them—it was only natural that they would interpret the evidence as more dire than their U.S. counterparts would.[38] Pollution went across the border in both directions, but by far the larger share came from the United States, which would therefore bear most of the burden of cleanup. As Bernabo puts it, for any problem, the degree of scientific certainty demanded is proportional to the cost of doing something about it. So the United States was more resistant to accepting the evidence and demanded a high level of certainty.[39]

No doubt this is true, but it doesn't quite explain the gap between the science and the summaries. Scientists are supposed to summarize science, and let the chips fall where they may. However, the summaries were not written by the scientists who had done the research. They were written, at least in part, by interagency panels—groups of scientists from U.S. government agencies, including the Department of Energy and the EPA, with relevant (or roughly relevant) expertise.[40] Government scientists are usually conscientious individuals who strive to be objective, but sometimes they come under political pressure. Even when they don't, they often can't help but be mindful of the positions of their bosses. And the position of the U.S. boss was clear. Gene Likens recalls that both agencies were very reticent "to do anything that would jeopardize their positions in the Reagan White House."[41] Richard Ayres, chairman of the National Clean Air Coalition, who worked to ensure passage of the acid rain control amendments to the Clean Air Act, recently put it more bluntly: "This was during the Reagan years, when acid rain was almost as verboten [to acknowledge] as global warming under George W. Bush."[42]

Getting a Third Opinion

In 1982, while the technical working groups were at their task, the White House Office of Science and Technology Policy (OSTP), under the direction of physicist George Keyworth, commissioned its own panel to review the evidence on acid rain. The National Academy of Sciences had already reviewed the available evidence the previous year—so some wondered why the OSTP needed yet another report.[43] The *New York Times* reported that most observers assumed that the review of the joint U.S.-Canadian work would be done by a joint panel of the National Academy and the Canadian Royal Society, and called it "unusual" that the administration would bypass the Academy and use "an outside group," picked by the White House.[44] The *Washington Post* noted that the president was "certainly entitled to appoint his own panel of experts," but he had done it in a manner that was "far from reassuring."[45]

The *Post* was right about presidential prerogative—the president may of course ask anyone he likes for information—and there are plenty of occasions where scientists agree that more information is needed.[46] But that wasn't the case here. In 1981, the Academy had stated unequivocally that there was "clear evidence of serious hazard to human health and the biosphere," and that continuing business as usual would be "extremely risky from a long-term economic standpoint as well as from the standpoint of biosphere protection." And they concluded that the situation was "disturbing enough to merit prompt tightening" of emissions standards, perhaps by as much as 50 percent.[47] A major EPA report the following year agreed. The *Wall Street Journal* reported on the EPA study under the headline ACID RAIN IS CAUSED MOSTLY BY POLLUTION AT COAL-FIRED MIDWEST PLANTS, STUDY SAYS, and quoted an EPA spokesman explaining how the twelve-hundred-page report had been compiled over two years by forty-six industry, government, and university scientists to produce a "scientifically unimpeachable assessment."[48]

The administration's outright rejection of the conclusions of the nation's most distinguished and qualified experts caused considerable consternation in scientific and regulatory circles. But what is particularly striking to our story is that the man they asked to assemble and chair the panel was someone we have already met—a man who had never worked on acid rain, but was well-known to the Reagan White House—Marshall Institute cofounder and SDI defender William A. Nierenberg.

Nierenberg already had ties to the Reagan White House. When Reagan was elected in the autumn of 1980, Nierenberg had been approached as a candidate for the position of president's science advisor. It was a position any scientist would covet and Nierenberg did, soliciting supporting letters from numerous colleagues.[49] Nierenberg was also interviewed by National Security Advisor Henry Kissinger for a special position as a liaison between his office and the science advisor.[50]

Ultimately, the nod for science advisor went to Keyworth, and the special position didn't materialize. Nierenberg was offered a job as the head of the National Science Foundation, which he turned down. However, he served the administration in several other ways. He was invited to be a member of Reagan's Transition Advisory Group on Science and Technology (to make suggestions for scientists to serve in high-level positions), and he served as a member of the Townes Commission to select a launching platform for the MX mobile ballistic missile. In March 1982, Nierenberg received a personal note from "Ron" thanking him for this work, and that November, a nomination to the National Science Board—a prestigious position that Nierenberg had asked numerous friends and acquaintances to suggest him for, including the Republican mayor of San Diego (and later governor of California), Pete Wilson.[51]

When the United States began to run into conflict with Canada over acid rain, Nierenberg was putting the finishing touches on a major report of the National Academy of Sciences on the impact of carbon dioxide on climate, arguably the first comprehensive scientific assessment on the subject. Its conclusions were fully in line with the position of the administration—that no action was needed other than more scientific research—and the administration used it publicly to counter work being done at the time by the EPA with a graver outlook.[52] So it is perhaps unsurprising that when the administration needed someone to grapple with acid rain, they turned to Bill Nierenberg.

Like his fellow physicists Frederick Seitz and Robert Jastrow, Nierenberg was a child of the atomic age, a man for whom the global anxieties and national challenges of the Cold War had offered remarkable personal opportunities. Raised in the Bronx by immigrant parents, Nierenberg had attended the prestigious Townsend-Harris High School (as did Robert Jastrow) and the City College of New York, where he studied physics, won a coveted fellowship to spend a year in Paris, and returned to New York in 1939 fluent in French and fearful of fascism.

In September 1942 he entered Columbia University for his Ph.D. He soon found himself working on isotope separation: how to isolate fissionable uranium for the atomic bomb. After graduating, he taught nuclear physics at the University of California, Berkeley, and in 1953 he became director of Columbia University's Hudson Laboratory, created to continue scientific projects begun on behalf of the U.S. Navy during World War II, particularly underwater acoustic surveillance of submarines. He subsequently held a series of positions at the interface between science and politics, including succeeding Seitz as NATO's assistant secretary general for scientific affairs. In 1965, Nierenberg became director of the Scripps Institution of Oceanography in La Jolla, California, an institution busy at the time applying scientific knowledge to national security problems, particularly in research linked to underwater surveillance of Soviet submarines and targeting submarine-launched ballistic missiles.[53]

Like Seitz and Teller, Nierenberg hated environmentalists, whom he viewed as Luddites (particularly for their opposition to nuclear power), and like Seitz and Teller he was an unapologetic hawk. He had been a fierce defender of the Vietnam War. Three decades later he still harbored bitterness toward academic colleagues who had failed to defend military-sponsored work, as well as residual anger over the disruption and violence that left-wing students had brought to campuses in the 1960s. Recalling an incident in which students at the University of California, San Diego, had threatened to march onto the nearby Scripps campus in protest of military-sponsored work, Nierenberg became visibly upset. Moreover, he insisted that the students were mistaken—because there was no classified work being done at Scripps. But this was untrue; many Scripps scientists—including Nierenberg—had security clearances to work on secret military projects and had done so for years, even decades.[54]

Nierenberg was a man of strong will and even stronger opinions—a good talker but not always a good listener. Some colleagues said that the old adage about famous physicists definitely applied to him: he was sometimes in error but never in doubt. And he was fiercely competitive, often debating until his adversaries simply gave up. Still, Nierenberg was a highly respected scientist and administrator, and if at times he was overconfident, it wasn't without justification: even his detractors thought he was brilliant. He had a way of keeping a conversation going, because he knew so much. He was an authority, but an accessible one. He pushed you around, but somehow you didn't mind. He was interesting to be around. He could even be fun. Perhaps in part for these reasons, when he asked you to serve on a committee, you'd most likely say yes. One of the people who said yes to Nierenberg was Sherwood Rowland.

In 1982, Rowland was already pretty famous. In the early 1970s he had realized that certain common chemicals—the so-called chlorinated fluorocarbons, or CFCs, used in hairsprays and refrigerants—could damage the Earth's protective ozone layer. In the mid-1980s, a giant ozone hole was discovered, and in the 1990s, Rowland, together with colleagues Mario Molina and Paul Crutzen, would be awarded the chemistry Nobel Prize for this work. After that, he would never lack for people eager to hear what he had to say, and usually to agree with it.

But in the early 1980s, when Nierenberg asked him to serve on the acid rain panel, Rowland worried that he would be lonely. He was fairly certain acid rain was a real problem, but he wondered if the rest of the panel felt the same way. Things got off to an inauspicious start at the first meeting, where Nierenberg had arranged for a briefing by Dr. Lester Machta, an expert on radioactive fallout. Rowland had encountered Machta in the 1950s, when radioactive strontium had been detected in the baby teeth of children in St. Louis. Scientific work showed that it came from the U.S. weapons testing site in Nevada, but for a long time the official position was to blame *Soviet* fallout. Machta had been a spokesman for that view. The prospects for an unbiased acid rain panel didn't look good.

But Rowland soon found that he was not alone. Nierenberg's panel also included Gene Likens, and after the Machta presentation, Rowland, Likens, and a few others discovered over dinner that they were in general agreement about acid rain. As Likens recalled, the food was extremely good, too.[55] Rowland felt that things were going to be all right. Events turned out to be more complicated.

The Nierenberg Acid Rain Peer Review Panel

Nierenberg's panel was charged with reviewing the output of the technical working groups that had been impaneled under the U.S.-Canada bilateral agreement. They concluded that it was "basically sound and thorough," and they affirmed that acid rain was serious and sufficiently documented to warrant policy action now.

Nierenberg's panel summarized:

> Large portions of eastern North America are currently being stressed by wet deposition of acids, by dry-deposition of acid-forming substances, and by other air pollutants . . . The principal agent altering the biosphere acidity is traceable to man-made sulfur dioxide (SO_2) emission . . . The panel recommends that cost effective steps to reduce emissions begin now even though the resulting ecological benefits cannot yet be quantified.[56]

Of course, there were still details to be worked out, but these might take "ten, twenty, or fifty years" to resolve, and that was too long to wait.[57] There was no need to wait to dot every scientific *i* and cross every technical *t*, because you had enough information to begin to act now. This was a pretty strong conclusion. It would have been even stronger, but for political interference.

Bill Nierenberg had boasted about how six of the nine of the members of his acid rain panel were members either of the National Academy of Sciences or the National Academy of Engineering. He had also boasted that he had handpicked all the members—that is, all but one.[58] That one was S. Fred Singer, who had been suggested to Nierenberg by the White House

Office of Science and Technology, and would contribute an appendix suggesting that, despite the conclusions of the Executive Summary, we really *didn't* know enough to move forward with emissions controls.

Why was Singer on this committee?

Like Jastrow, Seitz, and Nierenberg, Fred Singer was a physicist who owed his career to the Cold War. While a graduate student at Princeton during World War II, he had worked for the navy on underwater mine design; after the war, he moved to the Applied Physics Laboratory at Johns Hopkins University, where he pursued upper atmosphere rocketry research. And also like Jastrow, Seitz, and Nierenberg, Singer rapidly moved into administrative positions at the interface between science, government, and the military. In the early 1950s he served as a scientific liaison officer for the naval attaché in London, and later as the first director of the U.S. National Weather Satellite Center, an organization that drew on military rocketry and expertise to develop civilian weather prediction. However, despite his bona fides, Singer's relations with his colleagues were sometimes testy. Some colleagues think Singer's attitude problems began in the mid-1950s, when scientists were making plans for what would become the International Geophysical Year (IGY)—an international collaborative effort to collect synoptic geophysical data around the globe.

An illustration of a satellite orbiting the globe would later become the official symbol of IGY, but in the mid-1950s it was unclear whether satellites were even feasible, and whether scientists might have to make do with rockets that penetrated the upper atmosphere without going into orbit. Singer, who had been using rockets to study cosmic rays and the Earth's magnetic field, became a strong advocate for a satellite. As NASA historian Homer Newell recounts it, Singer's outspokenness generated friction in part because of his aggressive demeanor, and in part because he acted as if the idea of using satellites for scientific research was his alone. Scientists working with the navy and air force had been trying to determine if a satellite was feasible, but because of security restrictions they couldn't discuss it openly. Their calculations suggested that Singer's proposal was overly optimistic; it could be done, but not as readily as Singer said.[59] In the end, the International Geophysical Year did include geophysical instrumentation of satellites, but Singer felt he'd been insufficiently credited, and continued to antagonize colleagues by implying that he had invented the satellite concept.[60]

Shortly after the IGY incident, Singer moved to the National Weather Satellite Center. This center had been organized as part of the Weather Bureau, rather than as part of the space program, setting up further conflict between Singer and scientific colleagues at NASA who thought all satellites should be overseen by the space agency.[61] In the years that followed, Singer moved away from science and into government and policy.[62] In the 1970s, he served in the Nixon administration as deputy assistant secretary in the Department of the Interior under Walter J. Hickel, and then as deputy assistant administrator at the EPA. So Singer and Nierenberg had much in common—both physicists, both conservative politically, both with a history of working at the interface between science and government. Indeed, the commonalities went perhaps even deeper. Born in Vienna in 1924—the *s* stood for Siegfried—Singer had personally witnessed the threat of looming fascism, just as Nierenberg had during his year in France in 1939. However, there was one interesting difference. Throughout the 1960s, Singer had been an environmentalist.

In a book published in 1970 (and reprinted in 1975), based on a symposium held by the American Association for the Advancement of Science (AAAS) on "Global Effects of Environmental Pollution," Singer made clear that he shared the view later famously credited to Roger Revelle: that human activities had reached a tipping point. Our actions were no longer trivial; we were capable of changing fundamental processes on a planetary scale. Numerous emerging problems—acid rain, global warming, the effects of DDT—made this clear.

Like most of his colleagues, Singer believed there was a need for more science, but in 1970 he argued that one cannot always wait to act until matters are proven beyond a shadow of a doubt. Singer cited the famous essay "The Tragedy of the Commons," in which biologist Garrett Hardin argued that individuals acting in their rational self-interest may undermine the common good, and warned against assuming that technology would save us from ourselves. "If we ignore the present warning signs and wait for an ecological disaster to strike, it will probably be too late," Singer noted. He imagined what it must have been like to be Noah, surrounded by "complacent compatriots," saying, " 'Don't worry about the rising waters, Noah; our advanced technology will surely discover a substitute for breathing.' If it was wisdom that enabled Noah to believe in the 'never-yet-happened,' we could use some of that wisdom now," Singer concluded.[63]

Singer made a similar argument in a book on population control published in 1971, in which he framed the debate about population as a clash between neo-Malthusians, who focused on the limits of resources, and Cornucopians, who believed that resources are created by human ingenuity and are therefore unlimited. In 1971, Singer did not take sides, but stressed that the Cornucopian view hinged on the availability of energy: if population increases and one has to work harder to obtain available resources, then "per capita energy consumption must necessarily increase."[64] Energy was key; the other crucial issue was protecting the quality of life. "Environmental quality is not a luxury; it is an absolute necessity of life,"[65] Singer wrote, and so it was "incumbent upon us . . . to learn how to reduce the environmental impact of population growth: by conservation of resources; by re-use and re-cycling; by a better distribution of people which reduces the extreme concentrations in metropolitan centers; but above all by choosing life styles which permit 'growth' of a type that makes a minimum impact on the ecology of the earth's biosphere."[66]

Somewhere between 1970 and 1980, however, Singer's views changed. He began to worry more about the cost of environmental protection, and to feel that it might not be worth the gain. He also adopted the position he previously attributed to Noah's detractors: that something *would* happen to save us. That something would be technological innovation fostered in a free market. Singer would come down on the Cornucopian side.[67]

In 1978 Singer developed an argument for cost-benefit analysis as a way to think about environmental problems in a report for the Mitre Corporation—a private group that did extensive consulting to the government on energy and security issues. "In the next decade," he wrote, ". . . the nation will spend at least 428 billion dollars to reach and maintain certain legal air and water standards. To know whether these costs are in any sense justified, one must carry out a cost-benefit analysis. This has not been done."[68]

In the years to come economists would grapple with how to value species conservation, clean air and water, beautiful views, pristine landscapes. The problem then, as it largely remains today, is that it is easier to calculate the cost of a pollution control device than the value of the environment it is intended to protect: who can calculate the benefit of a blue sky? Meanwhile Singer did his own analysis, focusing on the fairly well-known costs of emissions control, and glossing over the admittedly harder-to-quantify benefits of clean air and water. In doing so, he radically changed his views. "The public policy conclusion from our analysis is that where a choice exists, one should always choose a lower national cost, i.e. a conservative approach to air pollution control, which will not inflict as much economic damage on the poorer segment of the population."[69] Singer had emphasized the potential cost to those who could afford it least—a point with which many liberals would concur—but if you left off

his final phrase, you had a view that many free market conservatives, as well as polluting industries, found very attractive.[70]

When Nierenberg had been finding scientists to serve the Reagan administration, Singer had sent Nierenberg his CV.[71] He stressed that he was a longtime Republican and member of the Republican National Committee, with close ties to George H. W. Bush and Virginia Republican senator John Warner. Above all, he had "the right political-economic philosophy to mesh with the Reagan administration."[72]

Singer also sent Nierenberg two articles he had written on oil markets, which showed how he had moved away from his earlier environmentalism to embrace a market-based approach. The gist of Singer's argument was supply and demand: if the price of oil went up, supply would increase—either directly, due to more exploration or more efficient refining, or indirectly, as the price of other fuels, such as nuclear, became competitive—so there was no need for government intervention. The "oil industry is making . . . major adjustments in response to market forces, without specific government help or advice," Singer wrote. To increase supply, one simply needed to deregulate the natural gas industry, license nuclear power plants more quickly, and expand oil drilling in Alaska and offshore. In other words, just unleash the power of the marketplace by decreasing government regulation and restriction of economic activity.[73] In an article published in the *Wall Street Journal* in February 1981, Singer predicted that by the 1990s, the world would be using "less than half of the oil it uses today," and by 2000 the U.S. "oil dependence on the Middle East" would "become a thing of the past."[74] Too bad he wasn't right.

Singer had high ambitions, suggesting himself to run either NASA or NOAA—the National Oceanic and Atmospheric Administration. He was also interested in the Department of Energy, the Department of the Interior, and the EPA, where he thought he could serve in the number two position, or having an impact at the State or Treasury Department, or even "a greater impact on government operations . . . from OMB."[75] Singer was offered the number two spot at NOAA, which he turned down on the grounds that it would not permit him to accomplish "any substantial policy initiatives." However, if the administration had a future opening where he could exert some policy influence, such as on a presidential commission, he remained interested.[76] In 1982, the opportunity to influence policy arose.

When the White House asked Nierenberg to chair the Acid Rain Peer

Review Panel, Nierenberg sent a detailed list of proposed members, with various options including "a foreigner, if wanted." (The foreigner was Svante Odén—one of the original discoverers of acid rain—but he was not wanted.) The White House accepted most of the people on Nierenberg's list, but rejected Gordon MacDonald, a geophysicist and former advisor to Richard Nixon who had warned about global warming in 1964, and who Nierenberg had labeled "A must!"[77] They also rejected biologist George Woodwell, the ecologist we met in chapter 2 considering the biological impacts of nuclear winter, who Nierenberg described as "deeply concerned about environmental degradation and active in environmental protection issues."[78] And despite plenty of names still left over, they added one of their own: Fred Singer.[79]

Besides being the only member proposed by the White House, Singer was also the only member without a regular, full-time academic appointment. He was affiliated with the conservative Heritage Foundation in Washington, D.C., which advocated unrestricted offshore oil development, transfer of federal lands to private hands, reductions in air-quality standards, and faster licensing of nuclear power plants.[80] (Heritage continues to oppose environmental regulation: in 2009, their Web site featured the article "Five Reasons Why the EPA Should Not Attempt to Deal with Global Warming.")[81]

Nierenberg did not propose Singer, but he did know Singer's views on acid rain. In January 1982, Gordon MacDonald had made a presentation to the State Department on acid rain, and in a three-page letter to Nierenberg two weeks later, Singer raised numerous doubts about it. While most studies focused on sulfur, MacDonald had called attention to NO_X—oxides of nitrogen, mostly from automobiles, that can also contribute to atmospheric acidity—suggesting that tighter emissions standards for cars might be needed. Without exactly saying that MacDonald was wrong (and later research would show that he wasn't), Singer insisted that the problem was very complex, it was premature to suggest remedies, and in any case technological solutions might obviate the need for emissions controls.[82] This was pretty much the same tack he took on the acid rain panel.

When Nierenberg's panel convened in January 1983, they began by discussing what their procedure would be.[83] The panel agreed that any conflicting or dissenting views would be included in the report; there was no discussion of any appendices.[84] In June, the White House Office of Science and Technology Policy asked the panel for an interim report and summary of research recommendations. The OSTP then prepared a press

release.[85] The word was out on the street that the report would be a strong one, and the *Wall Street Journal* reported on June 28: REAGAN-APPOINTED PANEL URGES BIG CUT IN SULFUR EMISSIONS TO CONTROL ACID RAIN.[86] They were right.

The draft version of the press release, which was admittedly long at nearly five full single-spaced pages, pulled no punches. It began by noting that the United States and Canada together emitted more than 25 million tons of sulfur dioxide per year, and then stated: "The incomplete present scientific knowledge sometimes prevents the kinds of certainty which scientists would prefer, but there are many indicators which, taken collectively, lead us to our finding that the phenomena of acid deposition are real and constitute a problem for which solutions should be sought."[87] It was a little verbose, but the point was clear. Lakes were acidifying, fish were dying, forests were being damaged, and the time had come to act.[88] "Steps should be taken now which will result in meaningful reductions in the emission of sulfur compounds."[89]

The strongest part of the press release was perhaps the two paragraphs on the fourth page that dealt with long-term damage. The first noted that the damage being discussed might not be irreversible in an absolute sense, but that it was legitimate to use that term when discussing damage that could take more than a few decades to repair. The second paragraph dealt with the most worrisome issue: that soil damage might set off a cascade of effects at the base of the food chain. "The prospect of such an occurrence is grave."[90]

However, when the draft came back to Bill Nierenberg from the White House, these two paragraphs had been struck out, and someone at the OSTP—probably senior policy analyst Tom Pestorius, the committee's official liaison with the OSTP—had placed a set of numbers in the margins suggesting that the remaining paragraphs be presented in a different order. Rather than start with the fact of the 25 million tons of SO_2 emissions per year, the White House wanted to start with a statement that earlier actions taken under the Clean Air Act were a "prudent first step," and then proceed to the discussion about incomplete scientific knowledge. In other words, the White House version would *not* begin by stressing the problem—massive sulfur emissions that caused acid rain—but by stressing that pollution was already partially controlled, and then moving straight on to the uncertainties that might be taken to suggest that further controls were not justified.

A second document, "Overall Recommendation of the Acid Rain Review

Panel," also came back to Nierenberg with suggested revisions. "Enclosed is a draft substitute first paragraph written by Fred Singer," with Singer's initials on the document. Singer's version again began differently from the panel's: "Acid Deposition (A.D.) is a serious problem, but not a life-threatening one. It is at once a scientific problem, a technological problem, as well as an institutional problem." The summary then made three enumerated points. The first sentences of each read as follows:

> 1) Scientifically we are not certain of all the causes of A.D. . . .
>
> 2) Control technologies are still costly and unreliable . . .
>
> 3) Institutionally, the Clean Air Act, and successive amendments, have [*sic*] wrestled with the problem of setting air standards to protect human health and property.[91]

Singer suggested that he was proposing a reasonable middle ground. "We would recommend a middle course: Removing a meaningful percentage of pollutants by a least-cost approach and observing the results, before proceeding with a more costly program."[92] This might have been a reasonable recommendation. It might even have been correct. But it was not what the peer review panel had said.

So now there were two different versions of the problem. One, written by the panel, acknowledged the uncertainties but insisted that the weight of evidence justified significant action. The other, written by Singer (perhaps with help from the White House), suggested that the problem was not so grave, and that the best thing was to make only small adjustments and see if they helped before considering anything more serious. These were not the same view at all. Which one would prevail?

Throughout the panel deliberations, Singer highlighted uncertainties in the science and emphasized the costs of emissions controls. On more than one occasion, he presented views that echoed those promoted or circulated by the electric power industry. One of these was the suggestion that forests in Germany were not actually in decline—or if they were, it wasn't because of acid rain—a view promoted by Chauncey Starr, a nuclear physicist at the Electric Power Research Institute (EPRI). In a letter to Keyworth in August, copied to Nierenberg, Starr had insisted that the panel review should contain a "comprehensive societal benefit/risk/cost analysis," because "public anxiety" was being unnecessarily inflamed.[93] What was really needed was more research.[94] Starr continued the argument in additional letters to

Nierenberg; in November, Singer presented a set of arguments that largely paralleled Starr's points. He also circulated a paper produced by the so-called National Council of the Paper Industry for Air and Stream Improvement arguing that acid rain had not been shown to affect tree growth, and a set of papers arguing for market-based, rather than regulatory, approaches to clean air (even though it was well outside the charge of the committee to consider, much less propose, solutions).[95] Perhaps to suggest that other forms of pollution were more serious than acid rain, he circulated a paper outlining crop damage from ground-level ozone.[96]

When Nierenberg circulated a draft of research recommendations in August 1983, Singer added several comments consistent with the idea that the problem might be overstated and the cost of fixing it too high. Where the report said that a pressing need was to understand the ecological consequences, Singer changed this to "ecological and economic consequences." In a discussion of emissions data, Singer added, "A better characterization of natural sources is required."[97]

That the science was uncertain, that more research was needed, that the economic consequences of controlling acid rain would be too great, and that acid rain might be caused by natural sources: these claims were all part of the position taken by the electrical utility industry. As *Time* magazine put it, the utility industry was "vociferously opposed to any emission control program without further research into the causes of acid rain," and insisted that "installing scrubbers could break the economic backbone of the Midwest."[98]

But the cause of acid rain *was* known, and it was *not* natural. Singer found himself out on a limb among his scientific colleagues. Rowland and Likens's memory is that no one supported Singer's views, which were in any case seen as irrelevant to the panel's charge to summarize the *science*. No one, that is, except Tom Pestorius from the White House Office of Science and Technology Policy. In April 1983, Pestorius had forwarded to the committee some "unsolicited" material from a representative of the Edison Electric Institute—a utility group—which Gene Likens dismissed as "uncritical propaganda" from a man with a "track record for obfuscating the obvious and for generating 'red herrings' . . . pleasing to his employer."[99]

Someone on the panel also circulated a document produced by a private consulting firm criticizing earlier National Academy work on acid rain. The consultants' report asserted that the scientific arguments for adverse effects from acid rain were "speculative" and "oversimplified," the conclusions "premature" and "unbalanced," and also added that some crops might benefit from acid rain.[100] While the record doesn't say who circulated this report to the panel, its complaint that "relative costs and benefits of available options are not considered" certainly resonated with Fred Singer's views. But economic analysis was neither within the charge nor the expertise of the Academy scientists, so they were being criticized for not doing something they had not been asked to do.

A few weeks later, Singer sent a set of materials to John Robertson, a major at the West Point who was serving as the committee's executive secretary. Writing on Heritage Foundation letterhead, Singer asked Robertson to distribute to the panel a long document that "set forth the Administration's *general* perspective and policy on global issues."[101] These included the claims that "although important 'global' problems do exist, recent . . . projections . . . are less alarming than most previous studies." Moreover, these problems "all seem amenable to solution . . . and promising new approaches and technologies are emerging." Above all, the administration wished to stress the "importance of the market place for achieving environmental quality goals." A primary goal of U.S. policy in the 1980s would be to "improve the functioning of the market place by removing trade barriers and . . . in particular to expand food, minerals and energy availability over the long term."[102]

Whether or not these claims were true and the policy goals reasonable was irrelevant—or should have been irrelevant—to the panel. Their job was to summarize and critique the science of the U.S.-Canada technical working groups. That is what it means to do a scientific peer review. The White House's perspectives were irrelevant to that task, but Fred Singer didn't see it that way.

Gene Likens recalls one particularly frustrating moment, when he blurted out, "Fred, you're saying that lakes aren't valuable. They *are* economically valuable. Let me give you an example. Let's say every bacterium is worth $1. There are 10^4–10^6 bacteria [ten thousand to a million] in every milliliter of water. You do the math." Singer replied, "Well, I just don't believe a bacterium is worth a dollar," and Likens retorted, "Well, prove that it isn't." Twenty-six years later, Likens recalled, "It was the only time I ever shut him up."[103]

Singer was effectively insisting that if the scientists couldn't *prove* the value of things (like bacteria), then they had no value. It was a foolish argument, and no one on the committee accepted it, not even Bill Nierenberg.[104] "If we went by absolute science," Nierenberg noted at another juncture, "there would be nothing to do."[105] When the panel's report came out in the summer of 1984, Nierenberg summarized its gist: "Even in the absence of precise scientific knowledge, you just know in your heart that you can't throw 25 million tons a year of sulfates into the Northeast and not expect some . . . consequences."[106]

Having failed to sway his fellow panelists, Singer tried another tack. In September 1983, civil engineer William Ackermann, the panel's vice-chair, had presented the committee's interim conclusions to the House of Representatives Committee on Science and Technology.[107] Singer wrote a six-page letter to the committee chair taking issue with Ackermann's testimony, which he claimed was unsupported by sufficient data. He argued that evidence of damage was lacking, or limited, that a good deal of soil acidification is natural, that only certain kinds of soils were susceptible to acid damage, and that acidification might in some cases be beneficial. Some of Singer's claims—for example, that some soils are naturally acidic—were true, but irrelevant. Others were misleading, insofar as he was the only member of the committee who held the opinion that the evidence of potential soil damage was "insufficient."[108] Whether or not the House Committee chairman believed Singer's claims, his letter certainly would have had at least one effect: to make it appear that the committee was divided and there was real and serious scientific disagreement. The committee *was* divided, but it was divided 8–1, with the dissenter appointed by the Reagan White House.

Singer was supposed to be writing the final chapter of the report, on the feasibility of estimating the economic benefits of controlling acid pollution. It was to be an investigation of how you *might* try to place a dollar value on nature—and what would be lost if you didn't.[109] Somehow, along the way, it turned into the claim that if you did nothing, it cost you nothing. Singer was continuing to equate the value of nature to zero. This was not something the others would accept, so the panel had three choices: keep working until they came to agreement, delete the chapter altogether, or relegate it to an appendix.

As the panel neared completion of their report, this issue remained unresolved. When the report finally appeared, the third solution had been chosen. While the rest of the report was jointly authored—the norm for National Academy and other peer review panel reports—Singer's appendix was all his own. It began with a strange claim: that the benefits as well as the costs of doing nothing were zero. This was patently at odds with the rest of the report, which stressed repeatedly the ecological costs of acid deposition.

If the panelists were correct, then the cost-benefit question at stake was how much money should be spent on pollution abatement to avoid or minimize these ecological costs. Singer ignored this, considering cost only in terms of the cost of pollution control—ignoring the cost of ecological damage. Moreover, one *could* calculate the cost of ecological damage and the value of avoiding it: in 1979, the White House Council on Environmental Quality had done just that, and placed the value of air quality improvements since the passage of the Clean Air Act at $21.4 billion—*a year.*[110]

Singer also presumed that the costs were mostly accrued in the present, but the benefits in the future, and therefore the latter had to be discounted in order to make them commensurate with the former. (That is to say, a dollar in the future is not worth as much to you as a dollar now, so you "discount" its value in your planning and decision making. How much you discount it depends in part on inflation, but also in part on how much you value the future.) Discounting would later become a huge issue in assessing the costs and benefits of stopping global warming, as long-term risks can be quickly written off with a sufficiently high discount rate.[111]

Was Singer doing that here? Not quite. He acknowledged that the choice of discount rate was "important," but then changed the subject to argue that because there are many sources of pollution you could spend a great deal of money addressing one source without any immediate benefit.[112] In principle this was true, but it was not what the scientists had said about acid rain. They had concluded that there was one dominant cause—sulfur dioxide—and that cutting it by 25 percent would yield rapid benefits. Singer also asserted that because pollution control often was applied only to new sources—think of automobiles—this also made it very hard to achieve quick results. True again, but the analogy to cars was faulty, because while it was very hard to put new pollution control devices on old cars, the available technology to control sulfur at power plants could be easily applied to old plants as well as new ones. Singer himself acknowledged that there was a strong argument in favor of applying new regulations to both old and new sources, lest you create a perverse incentive to stick to obsolete technologies. How regulations worked depended upon how policy makers designed them, and that was a matter of political power and will, rather than a law of nature.

Singer's appendix did not actually include the analysis he insisted was needed. When he reached the point of actually making the analysis, he demurred, arguing that both the costs and the benefits were extremely difficult to quantify, and simply jumped to his preferred conclusion: that the most practical approach would be a market-based one. Using transferable emissions rights, the government would determine the maximum allowable pollution, and then grant or sell the right to pollute to parties who could then use, sell, or trade those rights.[113]

In later years, emissions trading *would* be used to reduce acid pollution—and today many people are looking to such a system to control the greenhouse gases that cause global warming. Yet economists (and ordinary people) know that markets do not always work.[114] Indeed, many economists would say that pollution is a prime example of market failure: its collateral damage is a hidden cost not reflected in the price of a given good or service. Milton Friedman—the modern guru of free market capitalism—had a name for such costs (albeit an innocuous one): he called them "neighborhood effects."[115]

Friedman tended to dismiss the significance of neighborhood effects, suggesting that the evils of expanded government power to prevent them generally outweigh any plausible benefit. "It is hard to know when neighborhood effects are sufficiently large to justify particular costs in overcoming them and even harder to distribute the costs in an appropriate fashion," he wrote in his classic work, *Capitalism and Freedom.*[116] So in the vast majority of cases it would be better to let the market sort things out—and this is pretty much what Singer concluded about acid rain. Without any analysis of the details or an example of a successful market-based pollution control scheme, he simply asserted that a system of transferable emissions rights "would guarantee that the market will work in such a way as to achieve the lowest-cost methods of removing pollution."[117] For a man who worried enormously about scientific uncertainties, he was remarkably untroubled by economic ones.

Singer's final sentence was a question: "Will a reduction in emissions produce proportionate reductions in deposition and in the environmental impacts believed to be associated with acid rain?"[118] In posing the question, he left the reader with the impression that the answer, perhaps, was no. So a report that was otherwise clear on the reality and severity of acid rain now ended with doubt.

Singer's appendix left the reader with an impression very different from what the rest of the text had said. Yet it was very similar to what Reagan officials had been saying for some time. In 1980, David A. Stockman, director of the powerful Office of Management and Budget, asked in a speech to the National Association of Manufacturers, "How much are the fish worth in these 170 lakes that account for 4 percent of the lake area of New

York? And does it make sense to spend billions of dollars controlling emissions from sources in Ohio?" On another occasion, Stockman put the cost of eliminating acid rain at $6,000 for every fish saved.[119] The acid rain panel report was supposed to be a scientific peer review, but Singer had placed within it a policy view consistent with that of the Reagan administration, but seemingly at odds with the science that had been reviewed.

The full report was sent to the White House in early April, just as a key House of Representatives subcommittee was considering legislation to control acid rain. Secretary of State George Shultz had reassured the Canadians that he and EPA director William Ruckelshaus held acid rain to be a high priority, but the Canadians were worried.[120] Canadian government spokesman Allan MacEachen noted that they believed there was enough evidence to justify abatement measures, but the U.S. view was "that the scientific conclusions are not clear."[121] The Canadians were right on both counts. In May the House subcommittee voted 10–9 against the legislation, effectively killing congressional action on the issue. The panel report was finally released to the public on the last day of August.[122]

Press coverage was extensive and critical. "Prove it," was how *Newsweek* later characterized the Reagan administration position, neglecting to point out that scientists had, in fact, proved it.[123] "Who'll stop acid rain? Not Ronald Reagan," said the *New Republic.*[124] *Nature* concluded that "Canada must act alone."[125]

The business press, however, began to pick up on Singer's theme. *Fortune* ran an article by a researcher at the Hudson Institute, a progrowth think tank founded by Cold Warrior Herman Kahn. "Maybe acid rain isn't the villain" asserted that it "could eventually cost Americans about $100 billion . . . to achieve a major reduction in sulfur dioxide emissions. Before committing to any program of this magnitude, we should want to be more certain that acid rain is in fact a major threat."[126] The article didn't just misrepresent the state of the science, it misrepresented its history, too. "It's not surprising that there should be sharp disagreements about acid rain. The rain has been studied only for about six years." (You'd think think tank researchers could do arithmetic: the elapsed time between 1963 and 1984 did not come to six years.) The *Wall Street Journal* ran a piece on its editorial page by a consultant for Edison Electric named Alan Katzenstein entitled "Acidity is not a major factor," questioning the scientific evidence and suggesting that the real "villain in the acid-rain story" might be aluminum.[127] One forest ecologist responded in a letter to the editor: "Katzenstein made several assertions about the research findings [and] all of them are incorrect!"[128] Who was Katzenstein? An ecologist? A chemist? A biologist? No, he was a business consultant who previously had worked for the tobacco industry.[129]

Many of these pieces were published before the panel's final report was actually released; some of them were based on the interim findings published the previous summer. So maybe it didn't really matter whether or not the report had been delayed. Why *had* it been delayed? If the report was sent to the White House in April, why was it not released until August?

Manipulating Peer Review

On August 18, Maine senator George Mitchell and New Hampshire congressman Norman D'Amours issued statements saying that the report had been suppressed by the White House. Both the *New York Times* and the *Los Angeles Times* covered the story. "The 78-page study . . . directly rebuts the Reagan Administration position that pollution controls should not be ordered until further studies are conducted," the *Los Angeles Times* concluded. The administration shelved the report, D'Amours was quoted as saying, to avoid giving legislators "the ammunition we needed to push acid rain controls through Congress." The *New York Times*, however, quoted an OSTP spokesman explaining that the final report had not been received until mid-July, and quoted Nierenberg explaining, "We were making changes right up to mid-July."[130] That was true. But they were not changes that the panel had authorized.

A few weeks later, *Science* magazine suggested that the congressional vote might have been different had the Nierenberg report been released beforehand. *Science* quoted one panel member saying that "paragraphs were re-ordered and material added . . . that changed the tone of the original summary. The net effect [was] that the new summary weakens the panel's message that the federal government should take action now."[131] A Canadian paper repeated the charge: "The U.S. Administration suppressed a report that told it to cut acidic air pollution during a crucial congressional vote."[132]

The historical record shows that something irregular had indeed occurred. Changes *were* made after the report was finished, at least some of them were made without the agreement of the full panel, and they did weaken the message.

The report had been more or less completed by March, when Nierenberg sent a draft to the panelists, asking for final comments; in April, the final version was ready. Somehow proceedings were delayed, and a plan developed to present it to EPA administrator William Ruckelshaus on June 27.[133] After the article in *Science*, Nierenberg wrote to the journal to protest that, contrary to the article, the panel's report had *not* been changed since June. Who was right? After all, it was Nierenberg's panel; surely he knew what had gone on.

The historical record supports *Science* magazine. Documents show that the panel report *was* forwarded to the White House in April, it was ready to be released in June, and it was not actually released until August (albeit with a July date). The record also shows that changes had been made to the text. In fact, it shows that *two* sets of changes were made—one set in the spring, and a second set in the summer. Fred Singer had played a role in these changes—and so had Bill Nierenberg.

On May 21, Tom Pestorius sent a telecopy of the Executive Summary to Nierenberg. The first paragraph was completely different: a strong statement about the reality of acid rain was replaced with a historical introduction as to how and why the panel came to be. The original opening paragraph, which began, "Large portions of eastern North America are currently being stressed by . . . deposition of acids . . . [and t]he principal agent altering the biosphere is acidity traceable to sulfur dioxide," had been buried as the penultimate paragraph.[134]

The changes made in the summer were even more serious, and when the panel realized what had happened, they protested loudly. A red flag was raised in September by panel member Kenneth Rahn, an atmospheric chemist who had studied pollution dispersion. Rahn thought the claim that northeastern acid rain definitely came from midwestern pollution *was* a bit premature—that it might be best to do more research before implementing policy solutions, and he had testified in Congress to this effect—so no one would have considered him an alarmist.[135] But he now sent a very alarmed, three-page, single-spaced letter to the panel members, detailing what he had learned.

The penultimate draft of the report had been compiled in February, and this "was the last that most of us saw," Rahn recalled. "We read it over for a last time and sent any remarks back, and from this a final version was constructed. The principal change in the final version was Fred Singer's Chapter VIII, which was made into the signed appendix 5."[136] This account was consistent with other documents; a letter sent from John Robertson to the panel in February referred to the report as "the 'almost final' draft."[137]

John Robertson had been handling all the compiling of the various sections and was responsible for making the editorial changes suggested by the panelists. In a memo on February 24, 1984, he had summarized for the panel the major changes he had made, based on their input. There were only five, and most just dealt with organization and style. Only one was substantive: adding a recommendation to include control of nitrogen oxide emissions. However, Robertson did remind the panel of one "unresolved" issue: "the form, placement, and content of [Singer's] chapter VIII." Two panelists did not want it to have the status of a chapter, but would accept it as a signed appendix; four would accept it as a chapter but only if "the conclusions . . . are removed."[138]

In fact, the report had been almost ready nearly a year before. In March 1983—eleven months earlier—Robertson had written to the panel asking for their comments on a draft of what was expected to be the final report. "I have compared the draft issued to you in November with the enclosed 'Final Report,'" Robertson wrote. "All changes are attached. Rahn, Rowland and Ruderman should be in a position to finish all writing within the next two weeks . . . I would like to have all input by 21 March."[139] This version included Singer's contribution as a chapter. But the March 1983 version did not prove final. In July 1983, a draft of the report, with revisions, was sent to the panel, but somehow this too failed to become the final version. As noted above, in August, Singer sent Nierenberg a round of suggested corrections on the draft; Singer also sent panel members various memos and materials suggesting that acid rain might not be as serious as they believed. Another half year elapsed before the committee agreed on a final report. Among other things, Singer's chapter had been moved to the appendix, after all.

According to Robertson, only three copies of the final report were made: one each for him, Nierenberg, and the OSTP. That report was finished in March 1984, and sent to the OSTP in the first week of April. "We all know that the March report was regarded by us all as the final one," Rahn reminded his colleagues.[140]

Why was Singer's chapter converted into an appendix? It would appear that the OSTP was hoping to avoid the obligation of getting the committee to sign off on what Singer had done—something they had already refused to do. That was one of two major changes. Rahn explained the other. "By sometime in May, OSTP had decided to request that the Executive Summary be changed; they proposed this to the Chairman and followed up with a draft version of a revised Executive Summary which contained the types of changes they wanted the panel to consider. There followed several exchanges of new versions between the Chairman and OSTP, and at one point, Dr. Keyworth became personally involved." The OSTP had told Nierenberg what changes it wanted, and Nierenberg had made them.

So two parts—the Executive Summary, and Singer's appendix—had *not* been approved by all panel members. Most of the members didn't even know that the summary had been changed. But Rahn had now read them side-by-side, and he sent them to the rest of the panel to compare.

In Rahn's opinion, nothing important had been added or deleted, but changes in order, in adjectives, and in tone had changed the tenor of the report, so the reader was left with a very different impression. "The new message carries a softer message than the old one did. All parties who have carefully read both versions agree on this point, and in fact OSTP freely admits that their goal was to soften the tone." The structure of the summary had been changed, too. Whereas the original closely followed the report, beginning with the policy recommendation to act to control SO_2 emissions, the new one left that recommendation to the end. In so doing, "the remarks on policy which OSTP found most unjustified are thereby diminished in stature."[141]

It's a frequently asked question in scientific circles whether scientists should make policy recommendations about complex issues. The OSTP is the Office of Science and Technology *Policy*, so it was perhaps reasonable for them to suggest adjustments to the report's policy recommendations. Or was it? No, because this was a peer review panel. Their charge was to review, summarize, and critique what the technical working groups had done, and this included summarizing *their* policy recommendations. Peer review is a crucial part of science. For the OSTP to alter those recommendations was to interfere with scientific process. The report released by the OSTP on August 31 was simply not the report the panel had authorized.

"In short, our report has been altered since we last saw it," Rahn concluded. " 'Tampered with' may not be too strong . . . In light of the changes made, which I judge to be substantial, I suspect that we would not have approved it if we had been given the chance."[142]

Other panelists drew the same conclusion. "I am very distressed to learn that the Executive Summary for our Report from the Acid Rain Peer Review Panel has been rewritten and changed from the version our Panel prepared and authorized last spring," Gene Likens wrote to Nierenberg. "These revisions were done without informing the members of our Panel and without gaining their approval . . . My understanding is that these unapproved changes in the Executive Summary originated within the White House/ OSTP. Frankly, I find such meddling to be less than honest and extremely distasteful." Likens was clearly angry, but he held his temper, ending with a straightforward question: "Is there some explanation for what happened?"[143]

Panel member Mal Ruderman was also deeply disturbed, particularly because the *Science* article gave the impression that he had participated in the tampering. In a letter to Nierenberg, Ruderman wrote, "I am extremely upset by the description in *Science* of what happened to our Executive Summary between April and June." Ruderman *had* seen a version in June with certain proposed changes, but Nierenberg had not explained to him that this version had *already* been altered in May, with the text rearranged, and that the proposed changes in June were *additional* ones. Moreover, Ruderman now reminded Nierenberg that he (Ruderman) had *rejected* the proposed changes. "Some of the suggested changes altered the meaning of certain sentences and I did my best to change them back to conform to what our Committee had agreed on . . . I feel strongly that my role in all of this was defending against substantial changes in the Executive Summary given to me [and] I am counting on you to set the record straight on all of this to our Committee and to *Science*. It is a matter of great importance to me."[144]

Rahn made a similar point. The press coverage made it seem as if the whole committee had participated in the alterations, noting an article in the *New York Times* in August quoting "an OSTP spokesman as saying that the authors 'were making changes right up to mid-July,' which is very misleading."[145]

Nierenberg responded by suggesting that he too had been misled, or at least confused. "I received communications from [Kenneth] Rahn and Gene L[ikens] which partially confused me," he replied to Ruderman. "Your letter cleared up what was my major confusion . . . I did not realize till just now (any more than you did) that there had been prior rearrangement of the text . . . I am not sure what should be done. We could ask *Science* to publish the original summary. I am reasonably sure they would. We could also ask them to publish both at the same time and let the readers judge." He added a P.S. "I also have [now] compared the various versions and agree that there was considerable rearrangement."[146]

It is common practice for the head of a panel to meet, at the start of a review, with the office or agency that is commissioning the report to receive the charge. It is also accepted practice for the committee to meet with government officials to present the finished report, but it's highly irregular for a government official to *change* a report without the committee's knowledge and permission. If the White House had done that, it's hard to believe that Nierenberg would have been so calm about it. He should have been outraged.

Moreover, the record does not support the idea that Nierenberg had no idea what the White House was up to. When word got back to the OSTP that Ruderman was asking for the record to be set straight, Pestorius laid the blame squarely on Nierenberg's shoulders: "Bill told me that 'Mal is out here with me working on the Executive Summary.' "[147]

Ruderman was not content with Nierenberg's explanation, and in November he wrote to Nierenberg again. "I think that there still exists a need to explain to the Acid Rain committee members just what did happen between the original submission of our report to the OSTP (April?) and the receipt of an amended telecopied Executive Summary by you from Tom Pestorius in late July."[148] While Nierenberg had offered to set the record straight, there is no evidence in the published record or in his own files that he did.

When asked recently about Nierenberg's role, Gene Likens said simply, "He was the one talking to politicians in power. He pushed it through . . . Nierenberg was definitely responsible for the changes." Some of the panelists sought advice from colleagues at the National Academy about what to do, but to no avail. Likens recalls again, "We went to our sources, but ours weren't as powerful as Nierenberg's."[149]

Historical documents confirm Likens's account. In Bill Nierenberg's files, there is a second copy of the telecopied Executive Summary from May 21, but this time dated, by hand, 7/10/84—and the note next to the date reads: "Changes wanted by Keyworth." Nierenberg *had* changed the Executive Summary, and it was the science advisor to the president who had asked him to do so.[150]

Republicans in general were pleased with Nierenberg's work. In July, he received a letter from the Republican congressman from Lansing, Michigan. "I am delighted that you were chosen for this task," the congressman wrote.[151] In September, Nierenberg received an autographed photograph of President Reagan.[152] In 1984, Nierenberg sent Attorney General Ed Meese a copy of a crossword puzzle he had completed in which one of the answers was "Meese" (the clue was "Reagan aide"). In 1985, Nierenberg was considered once again for the position of science advisor to the president. One referee described him as "a strong, loyal, and vocal supporter of the Administration's policies . . . a [real] team player."[153]

THERE WOULD BE no legislation addressing acid rain during the remainder of the Reagan years. The administration would continue to insist that the problem was too expensive to fix—a billion-dollar solution to a million-dollar problem. There would, however, be plenty of further scientific research. William Ruckelshaus, the EPA administrator who had banned DDT in the Nixon years and was viewed by most people as an honest broker, appeared on ABC news in August 1984 to explain the administration's position. He was asked by conservative commentator George Will, "Isn't the evidence now in on acid rain?" Ruckelshaus replied, "Well, no it's not . . . We don't know what's causing it."[154]

"We don't know what's causing it" became the official position of the Reagan administration, despite twenty-one years of scientific work that demonstrated otherwise. "We don't know" was the mantra of the tobacco industry in staving off regulation of tobacco long after scientists had proven its harms, too. But no one seemed to notice this similarity, and the doubt message was picked up by the media, which increasingly covered acid rain as an unsettled question. We've already noted how *Fortune* ran an article insisting that the "standard scientific view of acid rain's effects may be simply wrong." (At least they acknowledged that there was a standard scientific view.) Echoing Fred Singer, the author, William Brown, associated with the Cato Institute, asserted that it "could eventually cost Americans about $100 billion in today's dollars to achieve a major reduction in sulfur dioxide." Given this enormous cost, "we should want to be more certain that acid rain is in fact a major threat to the country's environment."[155] The fact that all the relevant scientific panels *had* concluded that it was a major threat was ignored.

Likens tried to set the record straight with an article in *Environmental Science and Technology* entitled "Red Herrings in Acid Rain Research."[156] But in a pattern that was becoming familiar, the scientific facts were published in a place where few ordinary people would see them, whereas the unscientific claims—that acid rain was not a problem, that it would cost hundreds of billions to fix—were published in mass circulation outlets. It was not a level playing field.

And it wasn't just *Fortune* that misrepresented the science and the situation. *BusinessWeek* attacked the EPA as "activist" for trying to take action on acid rain—in effect, for doing its job.[157] *Consumers' Research Magazine* (which despite its name was a journal that consistently took probusiness positions) demanded to know: "Acid Rain: How Great a Threat?"[158] William Brown reprised his earlier article in *Fortune* with a new piece in 1986, "Hysteria about Acid Rain."[159] A few months later *Fortune* repeated his claim yet again, insisting that "delay makes sense because we still have a lot to learn about acid rain."[160] The *Futurist* joined the chorus, insisting that "the jury is still out on acid rain."[161]

Conflict, it is sometimes said, makes good copy, and when a lonely scientist took up the right-wing charge that acid rain might not be a serious problem, the press were quick to pick up his claims. Edward Krug was a soil scientist at the Connecticut Agricultural Research Station who began to argue that a good deal of soil acidification in northeastern forests was natural or associated with land use changes.[162] Krug called his argument a "a new perspective," but it wasn't new at all; natural acidification had been considered and found to be inadequate to explain the observations.[163] Still, Krug's argument was presented in *Policy Review*, published by the Hoover Institution,[164] and taken up by *Reason* magazine, which insisted that new evidence showed that "acid rain was not a problem."[165] He even appeared on *60 Minutes*, where he claimed that NAPAP had shown that acid rain simply wasn't a serious problem—a claim that almost no one else associated with NAPAP agreed with.[166]

As the World Wide Web developed in the 1990s, many sites began to quote Krug as having demonstrated that acid rain was not the crisis that environmentalists made it out to be. Many of these sites are still live today.[167] One complains that Krug was cited in the mainstream media only nine times between 1980 and 1993, while Gene Likens was cited thirty-nine times. (Given their relative standing in the scientific community and the depth and breadth of their acid rain research, this figure suggests that the mainstream media were biasing their coverage in *favor* of Krug.)[168] Print media kept up the drumbeat, as *Fortune* continued in the 1990s to claim that acid rain was "a relatively minor problem on which it would be absurd to spend billions of tax dollars."[169] Fred Singer, citing his own contributions to the 1983 Nierenberg report, claimed in *Regulation*—the journal of the Cato Institute—that avoiding premature action on acid rain had saved from $5 billion to $10 billion per year.[170]

Many people became confused, thinking that the acid rain issue was unsettled, that scientists had no consensus. When a group of NAPAP scientists met in 1990 at Hilton Head, South Carolina, National Public Radio reported that there was a "general consensus among the scientific community that acid rain is . . . complicated."[171] And while we are embarrassed to admit it, in the early 1990s one of us (N.O.) used Krug's arguments in an introductory earth science class at Dartmouth College to teach "both sides" of the acid rain "debate."

Meanwhile the Reagan administration, having gotten some but perhaps not all of what they wanted from Bill Nierenberg, commissioned yet another report. This one was led by Columbia University geochemist Laurence Kulp, who was well-known for his conservative religious views; colleagues at Columbia referred to him as a "theochemist" for his efforts to reconcile geological evidence with Christian belief."[172] Kulp's report concluded that acid rain was not as great a threat as many believed, a conclusion that most scientists described, in the words of the *New York Times*, as "inaccurate and misleading." With echoes of the 1984 Nierenberg report, the *Times* reported that several scientists "suggested that the [Executive] summary had been tailored . . . in the belief that policy-makers and journalists would read it [the summary] and not the report itself."[173]

It would take six years and a new administration to pass legislation to control acidic emissions. In 1990, under the administration of George H. W. Bush, amendments to the Clean Air Act established an emissions trading—or "cap and trade"—system to control acid rain. The system resulted in a 54 percent decline in sulfur dioxide levels between 1990 and 2007, while the inflation-adjusted price of electricity *declined* during the same period.[174] In 2003, the EPA reported to Congress that the overall cost of air pollution control during the previous ten years was between $8 billion and $9 billion, while the benefits were estimated from $101 billion to $119 billion—more than ten times as great.[175] Singer's "billion-dollar solution to a million-dollar problem" was just plain wrong.

The energy industry had often accused environmentalists of scaremongering, yet this is just what they had done with their claims of economic devastation. Protecting the environment didn't produce economic devastation. It didn't lead to massive job losses. It didn't cost hundreds of billions of dollars. It didn't even cause the price of electricity to rise. And the science was correct all along. As Mohamed El-Ashry of the World Resources Institute was quoted in *Newsweek*, "When we waited for more research on acid rain, we ended up realizing that everything we knew 10 years earlier was true."[176]

But even if the scientists got the science right, perhaps Republican policy *was* right to focus on market mechanisms to control pollution. Cap and trade to control sulfate emissions was widely considered a success and is now the leading model for controlling the greenhouse gases that cause global warming. Perhaps Singer was right to push for a market-based solution to acid rain. Perhaps, except that scientists close to the issue have reservations as to whether cap and trade has really worked.

Well after acid rain was off the headlines, Gene Likens and his colleagues continued to work at Hubbard Brook. By 1999, they had concluded that the problem had not been solved. "Acid rain still exists," Likens wrote in the *Proceedings of the American Philosophical Society*, "and its ecological effects have not gone away." Indeed, matters had gotten worse, as additional stresses such as global warming were making the forests "even more vulnerable to these anthropogenic inputs of strong acids from the atmosphere."[177] The net result was that "the forest has stopped growing."[178]

Over the next ten years, Likens and his colleagues pursued the question of net forest health. In 2009 they spoke out frankly. "Since 1982, the forest has not accumulated biomass. In fact, since 1997, the accumulation . . . has been significantly negative."[179] The forest was shrinking, "under siege" from multiple onslaughts of climate change, alien species invasion, disease, mercury and salt pollution, landscape fragmentation, and continued acid rain. The sugar maple—beloved by both Canadians and New Englanders—"is dying . . . [and s]cientific research suggests that by 2076, the 300th birthday of the United States, sugar maples will be extinct in large areas of the northern forest."[180] First on their list of threats to forest sustainability is acid rain, which "remains a major problem . . . as emissions were not fully controlled" by the Clean Air Act Amendments.[181] The cap and trade system simply did not do enough. Not only did it not eliminate acid rain, it did not even reduce it sufficiently to stabilize the situation. Forest decline has continued.

Likens and his colleagues do not rule out the continued use of market-based mechanisms to help save the forests, but they also note that some issues "require national and even global regulation."[182] But the real issue in either a cap and trade system or its alternative—setting pollution limits through command and control—is *where* you set the cap, and whether or not you have a mechanism to adjust it (either up or down) if future information suggests you should. The ongoing scientific work shows that, among other things, the Clean Air Amendments set the caps too high, perhaps in part because the arguments made by Fred Singer and his allies—and then taken up by the Reagan administration and much of the media—suggested that since we weren't entirely sure about the problem and its severity, it would be foolish to take excessively dramatic action. And so we didn't. We took modest steps, and then did nothing to strengthen them as time went on, even as the science increasingly indicated that we needed to. We went on faith that the market would do its "magic."

Magical thinking still informs the position of many who oppose environmental regulation. As recently as 2007, the George Marshall Institute continued to insist that the damages associated with acid rain were always "largely hypothetical," and that "further scientific investigation revealed that most of them were not in fact occurring."[183] The Institute cited no studies to support this extraordinary claim.

Moreover, there is reason to believe that a straight-out command and control approach might have better results than cap and trade in one important respect: research shows that regulation is an effective means to stimulate technological innovation. That is to say, if you want the market to do its magic—if you want businesses to provide the goods and services that people need—the best way to do that, at least in terms of pollution prevention, appears, paradoxically, to be to mandate it.

David Hounshell is one of America's leading historians of technology. Recently he and his colleagues at Carnegie Mellon University have turned their attention to the question of regulation and technological innovation. In an article published in 2005, "Regulation as the Mother of Innovation," based on the Ph.D. research of Hounshell's student, Margaret Taylor, they examined the question of what drives innovation in environmental control technology. It is well established that the lack of immediate financial benefits leads companies to underinvest in R & D, and this general problem is particularly severe when it comes to pollution control. Because pollution prevention is a public good—not well reflected in the market price of goods and services—the incentives for private investment are weak. Competitive forces just don't provide enough justification for the long-term investment required; there is a lack of driving demand. However, when government establishes a regulation, it creates demand. If companies know they have to meet a firm regulation with a definite deadline, they respond—and innovate. The net result may even be cost savings for the companies, as obsolete technologies are replaced with state-of-the art ones, yet the companies would not have bothered to make the change had they not been forced to.

Of course, regulation is not the only possible government action. Governments can invest directly in R & D, provide tax credits and subsidies, or facilitate knowledge transfer. Many economists prefer these alternatives to straight-out regulation, thinking they provide companies greater flexibility, increasing the likelihood that resources will be allocated in appropriate ways and the desired goals actually met. But Hounshell and his colleagues show that this presumption may be wrong. The empirical evidence shows that regulation may be the most effective means, because clear and stringent regulation provides a strong and continuous stimulus for invention.[184] Necessity *is* the mother of invention, and regulatory compliance is a powerful form of necessity.

If the U.S. government had established a strong regulatory regime on acid emissions, then the industry might have done more to innovate. And if technological advancement had made it easier and cheaper to control emissions, then industrial resistance to tightening the caps as time went on would have lessened, and it might well have been easier to tighten the regulations over time, giving the forests the protection that science showed they really needed.

This is admittedly speculative. We will never know what would have happened had a different approach been taken. However, one thing we do know for sure is that doubt-mongering about acid rain—like doubt-mongering about tobacco—led to delay, and that was a lesson that many people took to heart. In the years that followed, the same strategy would be applied again, and again, and again—and in several cases by the same people. Only next time around, they would not merely deny the gravity of the problem; they would deny that there was any problem at all. In the future, they wouldn't just tamper with the peer review process; they would reject the science itself.

CHAPTER 4

Constructing a Counternarrative: The Fight over the Ozone Hole

AT THE SAME TIME AS ACID RAIN was being politicized, another, possibly even more worrisome problem had come to light: the ozone hole. The idea that human activities might be damaging the Earth's protective ozone layer first entered the public mind in 1970. Awareness began with the American attempt to develop a commercial airliner that could fly faster than the speed of sound. The "supersonic transport," or SST, would fly inside the stratospheric ozone layer, and scientists worried that its emissions might do damage. While the SST did not turn out to be a serious threat, concern over it led to the realization that chemicals called chlorofluorocarbons were.

In 1969, MIT commissioned a major study of human environmental impact. "Man's Impact on the Global Environment: Report of the Study of Critical Environmental Problems" (mercifully abbreviated SCEP) was released a year later, and contained the first major statement on the state of the stratosphere and the probable impact of the SST.[1] A panel chaired by William Kellogg of the National Center for Atmospheric Research (NCAR) in Boulder, Colorado, took on the question. Water vapor is the second-largest combustion product of jet engines, after carbon dioxide, and like carbon dioxide, it's a greenhouse gas, so the scientists worried that water vapor from engine exhaust could cause climate change. Water vapor also makes clouds, which in turn affect weather. The scientists concluded that although stratospheric water vapor concentration would increase—by as much as 60 percent with a large SST fleet—it probably would not appreciably change

CHAPTER 7

Denial Rides Again: The Revisionist Attack on Rachel Carson

RACHEL CARSON IS AN AMERICAN HERO—the courageous woman who in the early 1960s called our attention to the harms of indiscriminate pesticide use. In *Silent Spring*, a beautiful book about a dreadful topic, Carson explained how pesticides were accumulating in the food chain, damaging the natural environment, and threatening even the symbol of American freedom: the bald eagle. Although the pesticide industry tried to paint her as a hysterical female, her work was affirmed by the President's Science Advisory Committee, and in 1972, the EPA concluded that the scientific evidence was sufficient to warrant the banning of the pesticide DDT in America.

Most historians, we included, consider this a success story. A serious problem was brought to public attention by an articulate spokesperson, and, acting on the advice of acknowledged experts, our government took appropriate action. Moreover, the banning of DDT, which took place under a Republican administration, had widespread public and bipartisan political support.[1] The policy allowed for exceptions, including the sale of DDT to the World Health Organization for use in countries with endemic malaria, and for public health emergencies here at home. It was sensible policy, based on solid science.

Fast-forward to 2007. The Internet is flooded with the assertion that Carson was a mass murderer, worse than Hitler. Carson killed more people than the Nazis. She had blood on her hands, posthumously. Why? Because *Silent Spring* led to the banning of DDT, without which millions of Africans died of malaria. The Competitive Enterprise Institute—whom we encoun-

tered in previous chapters defending tobacco and doubting the reality of global warming—now tells us that "Rachel was wrong." "Millions of people around the world suffer the painful and often deadly effects of malaria because one person sounded a false alarm," their site asserts. "That person is Rachel Carson."[2]

Other conservative and Libertarian think tanks sound a similar cry. The American Enterprise Institute argues that DDT was "probably the single most valuable chemical ever synthesized to prevent disease," but was unnecessarily banned because of hysteria generated by Carson's influence.[3] The Cato Institute tells us that DDT is making a comeback.[4] And the Heartland Institute posts an article defending DDT by Bonner Cohen, the man who created EPA Watch for Philip Morris back in the mid-1990s.[5] (Heartland also has extensive, continuing programs to challenge climate science.)[6]

The stories we've told so far in this book involve the creation of doubt and the spread of disinformation by individuals and groups attempting to prevent regulation of tobacco, CFCs, pollution from coal-fired power plants, and greenhouse gases. They involve fighting facts that demonstrate the harms that these products and pollutants induce in order to stave off regulation. At first, the Carson case seems slightly different from these earlier ones, because by 2007 DDT had been banned in the United States for more than thirty years. This horse was long out of the barn, so why try to reopen a thirty-year-old debate?

Sometimes reopening an old debate can serve present purposes. In the 1950s, the tobacco industry realized that they could protect their product by casting doubt on the science and insisting the dangers of smoking were unproven. In the 1990s, they realized that if you could convince people that science in general was unreliable, then you didn't have to argue the merits of any particular case, particularly one—like the defense of secondhand smoke—that had no scientific merit. In the demonizing of Rachel Carson, free marketeers realized that if you could convince people that an example of successful government regulation wasn't, in fact, successful—that it was actually a mistake—you could strengthen the argument against regulation in general.

Silent Spring *and the President's Science Advisory Committee*

DDT was invented in 1873, but got little attention until 1940, when Swiss chemist Paul Müller, working for a Swiss chemical firm, resynthesized it.

Field trials demonstrated its efficacy against numerous pests, including mosquitoes and lice, leading to the realization that DDT could be used to stop the spread of deadly insect-borne diseases like malaria and typhus.[7] The timing was fortunate, because supplies of the pesticide conventionally used against lice—pyrethrum, derived from chrysanthemums—were in short supply and wartime demand was great. In the latter part of World War II, DDT was widely used in Italian and African campaigns, as well as in some parts of the Pacific. Military strategists credited it with saving many lives.[8]

DDT seemed to be a miracle chemical. It killed insects immediately and almost entirely, yet seemed to have no adverse effects on the troops. It was easy to use: soldiers could apply it to their skin and clothing, or it could be mixed with oils and sprayed from airplanes. And it was cheap. In 1948 Müller was awarded the Nobel Prize for Physiology or Medicine for the value of DDT in disease control.[9]

After the war, DDT use expanded, particularly in agriculture. DDT was clearly less immediately toxic than the arsenic-based pesticides that had been previously widely used, and spraying from airplanes was much less expensive than the older methods of disease eradication, such as draining swamps, eliminating sources of open water near buildings, and clearing brush.[10] Across America, pest control districts switched to spraying. State and local governments began using it too, and even ordinary homeowners. Farmers began to use DDT as the U.S. government sold surplus warplanes cheaply and farmers turned them into crop dusters.[11]

Everyone believed that DDT was safe. One documentary from the period shows schoolchildren happily eating their lunches at picnic benches as DDT is sprayed around them.[12] But adverse effects were starting to be noticed. Among the first to recognize damage were biologists at the U.S. Fish and Wildlife Service, where Carson, a biologist, had worked. As she began to investigate, she found that there were numerous case reports of damage to birds and fish after DDT application. There was also some circumstantial evidence that DDT and other widely used pesticides might be doing harm to humans, too. But as with the early evidence of acid rain, most of these descriptions had been published in obscure places, in reports of the Fish and Wildlife Service or specialized journals of wildlife biology. Few people knew about any of this until Carson began to write about it.

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CARSON WAS AN eloquent writer who had already achieved success and the respect of the scientific community with her earlier book, *The Sea Around Us*. As *Silent Spring* neared completion, it was serialized in the *New Yorker*, so by the time it was published in 1962, its basic message was already out: DDT, the supposed miracle chemical, was no miracle at all.

Carson documented at great length both the anecdotal and systematic scientific evidence that DDT and other pesticides were doing great harm.[13] She reported on death to fish in regions that had been sprayed for pest control, on birds dying on college campuses and in suburban neighborhoods, and on spraying campaigns in Michigan and Illinois that had destroyed squirrel populations and the pets of people unfortunate enough to have been outside during the spraying or that had gone out soon after. The pesticides destroyed beneficial species, too. Spraying DDT in New Brunswick to save evergreens from a budworm infestation destroyed the bugs upon which local salmon relied, and the fish starved. DDT also killed useful insects, vital to pollinating flowers and food crops.

Silent Spring wasn't just about DDT—it was about the indiscriminate use of pesticides in general—but DDT was a particular focus for Carson, as it was for her biology colleagues, because of the evidence of bioaccumulation. Other pesticides broke down quickly in the natural environment, but DDT was very persistent, accumulating up the food chain. Because it was so long lasting, it continued to be concentrated in the tissues of the animals and insects that it didn't kill—long after spraying campaigns were over—so when those animals were eaten, the effects rippled through the ecosystem. One of its most alarming effects—interference in the reproductive systems of eagles and falcons—occurred not by direct exposure, but by those predators eating small rodents that had eaten things with DDT in or on them.

Precisely because DDT was so effective, it unbalanced ecosystems. During spraying to prevent the spread of Dutch elm disease by beetles, DDT accelerated the beetles' spread by destroying the natural predators that previously helped to keep those beetles in check.[14] Spraying in the Helena National Forest to protect trees from budworms caused an outbreak of the spider mite, which further damaged the trees. (It also hurt birds that depended on the forest's insect population.)[15] Carson remarked that populations recovered in one portion of the region because it was only sprayed once in a single year; other parts of the region had experienced continual spraying, and populations in those areas didn't recover.

What about people? The two other most commonly sprayed insecticides, aldrin and dieldrin, were already known to be toxic to humans and other mammals at high doses; so it was reasonable to suppose that DDT might show similar effects. Laboratory rats fed DDT had smaller litters and higher infant mortality than control subjects. Even if DDT were perfectly safe to people in the short run, it might not be in the long run.

Historians have suggested that *Silent Spring* was to environmentalism what *Uncle Tom's Cabin* was to abolitionism: the spark for a new public consciousness.[16] Yet almost as soon as *Silent Spring* came out, the pesticide industry went on the attack. They called Carson hysterical and emotional. They claimed that the science behind her work was anecdotal, unproven, inadequate, and wrong. They threatened Carson's publisher with lawsuits.[17]

Of course, not all scientists agreed with Carson, particularly chemists, who tended to believe pesticides were safe if used properly, and food scientists who appreciated the value of DDT in improving agricultural productivity. One of these skeptics was Emil Mrak, chancellor of the University of California, Davis, who testified to the U.S. Congress that Carson's conclusion that pesticides were "affecting biological systems in nature and may eventually affect human health [was] contrary to the present body of scientific knowledge."[18] Most biologists did not agree with Mrak, however, and the personal attacks on Carson backfired. The publicity and furor caused sales of *Silent Spring* to skyrocket, while the obvious sexism of calling a highly trained biologist and world-class writer "hysterical"—in the age of rising feminist consciousness—led many to rally to her defense. Even President John Kennedy spoke in reverent tones of "Miss Carson's work."[19]

But what about the science? *Silent Spring* was well written, but did Carson have the science right? To answer that question, President Kennedy turned to the leading group of scientific experts in America at that time—the President's Science Advisory Committee (PSAC, pronounced pea-sack). Established in the 1950s, and mostly populated by physicists, PSAC had mainly considered issues related to nuclear weapons and warfare, but in 1962 the president asked his advisors to guide him on DDT.

In the early 1960s, few systematic studies of the cumulative environmental effects of DDT had been done, in part because DDT had been used primarily as a military technology under exigent conditions.[20] Some government scientists had warned of DDT's hazards, but their studies were mostly classified or buried in government file cabinets; few people knew of their findings. After the war, safety considerations were largely brushed aside as DDT was lionized and Müller awarded the Nobel Prize.[21] In any case, pesticide regulation in the United States was based on assuring efficacy and controlling residues on food, not on environmental impact. Food production in the postwar United States was a great success story—American farmers were producing more food than ever at lower and lower prices—so if DDT had played a role in that as well, it showed how successful the chemical was.

So PSAC had a difficult charge: to contrast the obvious, rapid benefits of pesticide use in disease control and food production with the subtle, long-term, poorly understood risks to humans and nature. They also had to sort out a multitude of acknowledged scientific uncertainties. These gray areas included the gap between data on acute exposure (whose risks were not disputed) and chronic effects; a lack of information on synergistic impacts; the worry that existing data underreported adverse effects (because doctors weren't trained to recognize low-level pesticide poisoning and rarely did); and the familiar problem of extrapolating from experiments on lab rats to people.[22] They also had to address the difficulties of predicting long-term effects based on the few existing clinical studies.[23]

Despite these difficulties, PSAC came to a clear conclusion: it was time for immediate action to restrain pesticide use. The evidence of damage to wildlife was clear and compelling, even in cases of "programs carried out exactly as planned," and these harms would sooner or later spread to humans.[24] "Precisely because pesticide chemicals are designed to kill or metabolically upset some living target organism, they are potentially dangerous to other living organisms," the panel concluded logically enough. "The hazards resulting from their use dictate rapid strengthening of interim measures until such time as we have realized a comprehensive program for controlling environmental pollution."[25]

In the years to come, the U.S. government developed just such a program, as bipartisan majorities in Congress passed the Clean Air and Clean Water Acts and established a number of agencies, such as the National Institute for Environmental Health Sciences, to address environmental issues. This effort culminated in 1970 in the establishment of the U.S. Environmental Protection Agency. In 1972—ten years after the publication of *Silent Spring* and at least three more national-level science assessments—the Environmental Protection Agency under President Richard Nixon banned the use of DDT in the United States.[26] There was no rush to judgment against DDT: it took three presidencies to enact the ban. Science was not the cause of that policy—political will was—but the scientific facts supported it.

The Kennedy PSAC report, *Use of Pesticides: A Report of the President's Science Advisory Committee*, is notable in hindsight as much for what it did not do as for what it did. The scientists did not claim that the hazards of persistent pesticides were "proven," "demonstrated," "certain," or even well understood; they simply concluded that the weight of evidence was sufficient to warrant policy action to control DDT. Environmental concerns other than pesticides might be more serious, they acknowledged, but that was no reason to deflect or distract attention from the issue with which they were charged. They did not dismiss alternatives to pesticide use, such as biological pest control, and they did not accuse Carson of harboring a hidden agenda. Nor did they let a lack of scientific understanding of the mechanisms of pesticide damage stop them from accepting the empirical evidence of it. Most important, while calling for more study, they didn't stall or hedge; they called for action.

The committee placed the burden of proof—or at least a substantial weight of it—on those who argued that persistent pesticides were safe, and explicitly invoked the standard of reasonable doubt. The legal phrase "reasonable doubt" suggests that they were guided by existing legal frameworks, such as the landmark federal Food, Drug, and Cosmetic Act (1938), which placed the burden of proof on manufacturers to demonstrate the safety of their products, and the Miller Amendment to that act (1954), which extended the act's reach to pesticides.[27] Manufacturers had not demonstrated the safety of DDT, and reasonable people now had reason to doubt it.[28]

Both science and democracy worked as they were supposed to. Independent scientific experts summarized the evidence. Polls showed that the public supported strong legislation to protect the environment.[29] Gordon MacDonald, a member of President Nixon's Council on Environmental Quality, recalled that Nixon supported the creation of the EPA not because he was a visionary environmentalist, but because he knew that the environment would be an important issue in the 1972 presidential election.[30] Our leaders acted in concert with both science and the will of the people.

Does the story end there? No, for as we began to explain above, Carson has now become the victim of a shrill revisionist attack. "Rachel was wrong," claims the Web site of the Competitive Enterprise Institute.[31] "Fifty million dead," claims another."[32] "More deaths likely," insists a third.[33] Why? Because malaria has not been eradicated, and it would have been, these critics insist, had the United States not succumbed to environmental hysteria. There was no good scientific evidence to support the DDT ban, they say, and DDT was the only effective means to kill the mosquitoes that carry the malarial parasite.[34] Banning it was "the worst crime of the century."[35]

In his bestselling book, *The Skeptical Environmentalist*, Danish economist Bjørn Lomborg (listed by *Time* as one of the one hundred most influential people in 2004) echoed the accusation that Carson's argument was more emotional than rational, insisting that more lives were saved by disease control and improved food supply than were ever lost to DDT. Thomas Sowell, a conservative writer associated with the Hoover Institution, insists "there has not been a mass murderer executed in the past half-century who has been responsible for as many deaths of human beings as the sainted Rachel Carson."[36] Others have compared Carson to Stalin and Hitler.[37]

One might ignore these venomous claims except that they have been repeated in mainstream newspapers. In 2007, the *San Francisco Examiner* ran an op-ed piece alleging that "Carson was wrong, and millions of people continue to pay the price."[38] The *Wall Street Journal* argued that Carson's work led to the attitude that "environmental controls were more important than the lives of human beings."[39] The *New York Times* has run several articles and op-ed pieces doubting the wisdom of U.S. action on DDT.[40] "What the World Needs Now Is DDT" ran the title of a Sunday *New York Times Magazine* piece in 2004. "No one concerned about the environmental damage of DDT set out to kill African children," the article began, but their deaths happened all the same. "*Silent Spring* is now killing African children because of its persistence in the public mind."[41]

One of the anti-Carson voices at the *New York Times* is the "science" columnist John Tierney, who in 2007 argued that *Silent Spring* was a "hodgepodge of science and junk science" and that the person who actually got the science right in the 1960s was I. L. Baldwin, a professor of agricultural bacteriology at the University of Wisconsin. No one listened to him, Tierney insisted, because Baldwin didn't scare people. His calm demeanor was no match for Carson's "rhetoric," which "still drowns out real science."[42]

Is Tierney right? Was Carson wrong? What does real science—and real history—tell us? It tells us that Carson—and the President's Science Advisory Committee and the U.S Environmental Protection Agency and President Richard Nixon—were not wrong about DDT.

After DDT's demonstrated successes in World War II, the United States and the World Health Assembly launched a Global Malaria Eradication Campaign (1955–1969). It was not based on large outdoor spraying campaigns—the principal target of Carson's indictment—but primarily on indoor spraying of household walls and surfaces with DDT (and dieldrin). The U.S. Centers for Disease Control summarizes the results: "The campaign did not achieve its stated objective." Endemic malaria was eliminated in developed nations, mainly in Europe and Australia, and sharply reduced in India and parts of Latin America, but the campaign failed in many less developed areas, especially sub-Saharan Africa. It was halted in 1969—four years before the U.S. DDT ban—so whatever happened could not have been the result of the U.S. ban. What did happen?[43]

Malaria eradication failed in less developed nations because spraying alone didn't work. Spraying *along with* good nutrition, reduction of insect breeding grounds, education, and health care did work, which explains why malaria was eradicated in developed nations like Italy and Australia, but not in sub-Saharan Africa. Like nearly all public health initiatives, the program needed people's cooperation and understanding.

Indoor Residual Spraying—the central technique used—worked by leaving insecticide on the walls and ceilings of dwellings. This meant that people needed not to wash, paint, or replaster their walls, and many people didn't understand this, as it contradicted most other public health directives. Others just didn't like the idea, as it seemed to instruct them to have dirty homes. But the most important reason that eradication was only partially successful was that mosquitoes were developing resistance. In the United States, DDT use peaked in 1959—thirteen years before the ban—because it was already starting to fail.

BUGS AND BACTERIA offer the best evidence we have of natural selection. When an insecticide wipes out part of a population, the ones that survive pass on their genes to their offspring, and it is only a matter of time before the population adapts to the insecticide-laden environment. Insect generations last a few days to a few months, so they evolve with enormous speed—far faster than slow-breeding species like humans and most animals. So they show the effects of natural selection in a time frame that we can directly observe—sometimes in as little as a few years.

Insect resistance to DDT was first recognized in 1947, just a few years after DDT's wartime triumphs. Mosquito control workers in Fort Lauderdale reported that "the normal application of a 5 percent DDT solution had no discernible effect on salt marsh mosquitoes . . . the miraculous 'magic dust' had lost its efficacy against the hordes of salt marsh mosquitoes along Florida's east coast."[44] Resistance increased rapidly during the 1950s, and soon many pest control districts were abandoning DDT for other alternatives.

Sadly, most of the resistance that insects developed to DDT came from agricultural use, not from disease control. There *is* a tragedy in this story, but it is not the one that the Competitive Enterprise Institute thinks it is. It is that the attempt to grow food cheaply, especially in the United States, was largely responsible for the development of insect resistance. The failure of DDT in disease control is in part the result of its excess use in agriculture. Here's why.

The most efficient way to use pesticides against disease is through application to the insides of buildings—the Indoor Residual Spraying on which the World Health Organization largely relied. DDT is particularly potent in this use, as an application can last up to a year. Most important, it doesn't produce resistance very quickly, because most insects don't wind up in buildings and therefore aren't subjected to the poison. Indoor Residual Spraying just affects the small percentage of the population that make it indoors, where they are likely to bite people and transmit disease, so the selection pressure on the insect population isn't very high. It's a very sensible strategy.

However, when pesticides are sprayed over large agricultural areas, they kill a large fraction of the total insect population, ensuring that the hardy survivors breed only with other hardy survivors; the very next generation may display resistance. The more extensive the agricultural use, the more likely bugs are to evolve resistance rapidly, and the less effective the pesticide is likely to be when you need it for disease control.

We now know that agricultural spraying produced insect immunity in only seven to ten years. This isn't merely hindsight: Rachel Carson discussed insect resistance in *Silent Spring*.[45] DDT was also widely used for agriculture in countries where it was being used for disease control, so it became ineffective for disease control much sooner than it might otherwise have. In the 1950s, we already knew that insects evolved very rapidly, but our political institutions evolved much more slowly than the bugs did.

Events proved that DDT alone was not sufficient to eradicate malaria, but was DDT necessary? Was it essential in the regions where malaria was controlled? The answer here is no, too. Most people have forgotten that in the nineteenth century malaria was endemic in the United States—and a major anxiety for settlers in places like Arkansas, Alabama, and Mississippi.[46] Even California struggled with malaria.

By the 1930s mosquito control districts throughout the nation had largely brought malaria under control by drainage, removal of breeding sites, and pesticides other than DDT.[47] Malaria infection in Florida, for example, declined every year after 1935, even though DDT was yet to be introduced.[48] Urbanization played a role, too, as more Americans lived away from mosquito breeding grounds. After World War II, DDT became an additional tool in the arsenal, helping to eradicate the remaining cases—by then few and far between.

Another case is worth mentioning: the Panama Canal. Led by Ferdinand de Lesseps (who had also led the construction of the Suez Canal) the canal project was started by a French company in 1882, but faltered in part because of the impact of yellow fever and malaria. By 1889, more than twenty-two thousand workers had been felled by these two diseases, and the construction effort collapsed.

In 1904, the U.S. government took over and the new American leadership appointed a medical officer to the post of chief sanitation officer, William Crawford Gorgas. Gorgas believed what was then a radical hypothesis: that these diseases were carried by insects. He drained swamps and wetlands, removed standing pools of water from around buildings, and sent teams of men to destroy mosquito larvae with oil and to fumigate the buildings. He also equipped the buildings, especially the workers' dormitories, with screens. Between 1906 and the completion of the canal in 1914, there was only a single case of yellow fever, and the death rate in the population declined from 16.21 per thousand in 1906 to 2.58 per thousand in December 1909.[49] Yellow fever was completely eradicated—thirty-one years before Müller's discovery of DDT's insecticidal properties. While malaria proved more recalcitrant, it too was controlled in many regions by similar techniques. The lesson of history is clear: DDT alone did not eradicate insect-borne diseases, and those diseases have been controlled in places with little or no use of DDT.[50]

WHEN THE UNITED STATES took action against DDT in 1971, EPA administrator William Ruckelshaus made clear that the new ban would not apply outside the United States. (How could it? EPA had no authority over other countries.) Ruckelshaus stressed that U.S. manufacturers were free to continue to manufacture and sell the product for disease control overseas, and that his agency would "not presume to regulate the felt necessities of other countries."[51] Whatever subsequently happened in Africa, it was hardly Rachel Carson's fault—or William Ruckelshaus's.

As for Baldwin—the scientist whom John Tierney claims got the science right—the work that Tierney quotes wasn't a piece of scientific research at

all, it was a *book review*: a review of *Silent Spring*. Baldwin acknowledged that *Silent Spring* was "superbly written and beautifully illustrated," and constituted "an exhaustive study of the facts bearing on the problem."[52] He also allowed that Carson's approach "will undoubtedly result in wider recognition of the fact that [pesticides] are poisons and in a more careful and rigorous control of every step in the pathway that pesticides must travel . . . There are serious hazards involved in the use of pesticides."[53]

So what was Baldwin's complaint? That the book was impassioned, rather than balanced, and read as if written by a prosecutor. That was true: Carson *was* trying to make a case. But above all, Baldwin complained that Carson had written the wrong book. He wanted to read a progress story about how the development of chemicals—pesticides included—constituted a "chemical revolution . . . that has most intimately affected every aspect of our daily life." He wanted a book that recounted how technology had made life better, emphasizing that "the span of our life has been greatly extended; our clothes are composed of fibers unknown 20 years ago; our machinery and household utensils are made of new and strange materials."[54] He wanted to be told about the benefits that science and technology had brought us, not their frightening unintended consequences. Perhaps John Tierney felt the same way.

Like virtually all of Carson's critics who followed, Baldwin insisted that pesticides were the key to the productivity of modern agriculture, and that greater use of pesticides was the key to wiping out world hunger (although most social scientists disagree, pointing out that there is plenty of food in the world; the problem we face is one of unequal distribution). Rather than answer Carson's points and address her evidence, Baldwin changed the subject: focusing on the good that modern technology has brought, and refusing to address her central argument about ecosystem harms. Contrary to Tierney's claim, Baldwin conceded the science. Like virtually all of Carson's critics—including Tierney—his faith in technology and anthropocentrism caused him to miss Carson's most important point.

In 1962, evidence of human deaths from DDT was scant. Carson acknowledged this. While she suggested that DDT was likely to cause cancer, she never claimed that large numbers of people had been killed by it. What she emphasized was the overwhelming evidence of harm to *ecosystems*, harm that she believed would sooner or later reach us. Carson's argument was that any war on nature was one that we were bound to lose. Fish and birds were killed, while fast-evolving insects came back stronger than ever. Finally—and perhaps above all—it was a mistake to assume that the only harms that counted were *physical*. Even if DDT caused not one human death, humans would be affected: our world would be impoverished if spring came and no birds sang.

If DDT's defenders have exaggerated its benefits, have its detractors exaggerated the harms? If DDT rarely harms people and sometimes helps, why not reintroduce it? Isn't Bjørn Lomborg right at least that DDT saved more lives than it cost?

The argument is a red herring. DDT was not banned on the basis of harm to humans; it was banned on the basis of harms to the environment. The scientific evidence of those harms was not only affirmed by PSAC and the EPA; it has been reaffirmed by numerous studies in areas where DDT and its metabolite, DDE, persist.[55] DDT kills birds, fish, and beneficial insects, and continues to do so long after spraying has stopped. Even today, birds in the Catalina Islands show signs of DDT poisoning, probably from eating fish that have ingested materials from the sea floor laced with residual DDT, left over from its manufacture in California decades ago.[56]

What about humans? Tierney argues that when DDT was banned "there wasn't evidence that it was carcinogenic." This is true. But since then we have learned a great deal about the risks of pesticides, and there is now strong scientific evidence that many pesticides carry serious risks to humans. (Recall that *Silent Spring* was not just about DDT; it was about pesticides in general.) Since 1971, the cancer-causing properties of diverse pesticides have been demonstrated by numerous peer-reviewed scientific studies, both in animal models and exposed humans.[57] We have also learned much more about the manner in which DDT does, in fact, harm humans.

A recent review in the *Lancet*—the world's leading medical journal—concluded that when used at levels required for mosquito control, DDT causes significant human impacts, particularly on reproductive health. (This is not surprising, given that some of the earliest evidence against DDT was that it interfered with reproduction in birds and rats.) Abundant scientific evidence reveals DDT's impact on child development, including preterm birth, low birth weight, and possible birth defects. High concentrations of DDT in breast milk are correlated with shortened duration of lactation and early weaning—itself highly correlated with infant and childhood mortality. The *Lancet* authors conclude that any saving of lives from malaria might well be abrogated by infant and early childhood mortality caused by DDT.[58] Some lives might have been saved by continued use of DDT, but others would have been lost.

And what about cancer? A few years ago, medical researchers realized that there was a shocking flaw in previous studies that investigated DDT exposure and breast cancer. Most of them were done *after* DDT use was already on the decline, or even after the ban, so the women being studied had probably been exposed only to low levels (if at all), and exposed later in life when the body is less vulnerable. To really know whether or not DDT had an effect, you'd need to study women who'd been exposed to DDT early in life, at a time when environmental exposures were high.

In a remarkable piece of medical detective work, Dr. Barbara A. Cohn and her colleagues identified women who had been part of medical study of pregnant women in the 1960s, and therefore might have been exposed as children or teenagers when DDT use was widespread in the 1940s and '50s. These women had given blood samples at the time, samples that could now be reanalyzed for DDT and its metabolites. In 2000–2001, they measured DDT-related compounds in these samples and compared them with breast cancer rates. The average age at the time of the original study was twenty-six; these women were now in their fifties and sixties—an age by which breast cancer might reasonably be expected to appear. The results showed a *fivefold increase* in breast cancer risk among women with high levels of serum DDT or its metabolites.[59] DDT *does* cause cancer, it *does* affect human health, and it *does* cost human lives. Rachel Carson was not wrong.

Admittedly, some public health experts think that DDT could play a useful role in malaria control in some places in the world today, but it never was the miracle cure that Lomborg, Sowell, Cohen, and Tierney have made it out to be. There is no scientific evidence to support the claim that millions of lives have been needlessly lost, and there is substantial scientific evidence that a good deal of harm—both to humans and the other species we share this planet with—has been avoided.

So what is going on here? Are these folks just confused? Misinformed? Ignorant? Even hysterical? Would that it were so.

We've seen how some people have fought the facts about the hazards of tobacco, acid rain, ozone depletion, secondhand smoke, and global warming. Their denials seemed plausible, at least to some, because they involved matters that were still under scientific investigation, where many of the details were uncertain even if the big picture was becoming clear. But the construction of a revisionist history of DDT gives the game away, because it came so long after the science was settled, far too long to argue that scientists had not come to agreement, that there was still a real scientific debate. The game here, as before, was to defend an extreme free market ideology. But in this case, they didn't just deny the facts of science. They denied the facts of history.

Denial as Political Strategy

Each of the stories we've told so far involved a handful of actors attempting to prevent regulation of specific products. But the twenty-first-century attack on Carson had nothing to do with preventing regulation; the regulation was long established. Nor was it an effort to overturn that regulation. It was well understood in American science, government, and agriculture that DDT was no longer effective in the United States. So why does DDT matter? Why attack a woman who has been dead for nearly half a century?

We saw in chapter 3 that as the acid rain story was emerging in the 1960s, the American environmental movement was changing its orientation away from an aesthetic environmentalism toward legal regulation. Carson's voice was fundamental to that reorientation. After all, what was the value of a national park if no birds sang in it? If Carson was wrong, then the shift in orientation might have been wrong, too. The contemporary environmental movement could be shown to have been based on a fallacy, and the need for government intervention in the marketplace would be refuted.

We see this narrative first emerging from someone we have already met: Dixy Lee Ray. In *Trashing the Planet*, Ray sang the praises of DDT and constructed a set of "facts" that have circulated ever since. She told a story of how DDT was wrongly abandoned in Sri Lanka, where "public health statistics . . . testify to the effectiveness of the spraying program." It began like this:

> In 1948, before the use of DDT there were 2.8 million cases of malaria [in Sri Lanka]. By 1963, there were only 17. Low levels of infection continued until the late 1960s, when the attacks on DDT in the U.S. convinced officials to suspend spraying. In 1968 there were one million cases of malaria. In 1969, the number reached 2.5 million, back to the pre-DDT levels. Moreover by 1972, the largely unsubstantiated charges against DDT in the United States had a worldwide effect.[60]

Is this account true? Partly—the part up to 1963. Between 1948 and 1963, DDT worked, and malaria cases dropped dramatically. Although resistance was seen as early as 1958, eradication appeared to be working overall. In 1963, the small handful of new cases should have made it controllable; indeed, malaria should have been on the path to eradication in Sri Lanka. But then Ray started to leave out key facts.

In 1968, malaria flared up again, and DDT couldn't control it. Still, the Sri Lankans persisted, using even more DDT over larger areas at more frequent intervals. Still, it didn't work. In its 1976 study of pesticide resistance, the World Health Organization's Expert Committee reported:

> In Sri Lanka a revised programme started in March 1975 that had been planned in the light of the limited financial resources available . . . The use of DDT at 1 g/m² at 4 monthly intervals with particular attention to improved coverage did not result in any significant difference in malaria prevalence as compared with an area with normal (lesser) coverage, and no improvement was obtained either by using DDT at the rate of 2 g/m² at 4 monthly intervals.[61]

Finally they switched to malathion, a more expensive agent, but one that the region's insect population hadn't yet adapted to. This brought the malaria rate down again, although not to the extremely low levels seen in 1963.[62]

So Sri Lanka didn't stop using DDT because of what the United States did, or for any other reason. DDT stopped working, but they kept using it anyway. We can surmise why: since DDT had appeared to work at first, officials were reluctant to give it up, even as malaria became resurgent. It took a long time for people to admit defeat—to accept that tiny mosquitoes were in their own way stronger than us. As a WHO committee concluded in 1976, "It is finally becoming acknowledged that resistance is probably the biggest single obstacle in the struggle against vector-borne disease and is mainly responsible for preventing successful malaria eradication in many countries."[63]

Resistance is never mentioned in Ray's account, an especially notable omission given that she was a zoologist. In a particularly egregious example of the pot calling the kettle black, Ray accused both environmentalists and William Ruckelshaus of giving credibility to pseudoscience, by creating "an atmosphere in which scientific evidence can be pushed aside by emotion, hysteria, and political pressure."[64] But it was she, not Ruckelshaus, who was spreading hysteria.

Ray had not accused Rachel Carson of mass murder, but others soon did. We met Steve Milloy in chapter 5, as he founded The Advancement of Sound Science Coalition on behalf of Philip Morris in 1993 to defend a product that really *had* caused millions of deaths. Soon thereafter, he began to spread the "millions of deaths" claim about DDT. According to his 1997 annual report, he began working with J. Gordon Edwards, an entomologist at San Jose State University, to help him publish an account of the DDT controversy.[65] Edwards's account finally appeared in 2004 in the *Journal of American Physicians and Surgeons*, published by the Association of American Physicians and Surgeons. This is a Libertarian political group that shares a board member with the Oregon Institute of Science and Medicine—a group that had also promoted skepticism about global warming. Edwards contended that "the worldwide effect of the U.S. ban has been millions of preventable deaths."[66] While suggesting that "the term genocide is used in other contexts to describe such numbers of casualties," he never mentioned the fact of pesticide resistance—a striking omission for an entomologist.

Milloy continued the antiscientific crusade in his post-TASSC career, and continues it to this day. "It might be easy for some to dismiss the past 43 years of eco-hysteria over DDT with a simple 'never mind,'" Milloy asserted recently, "except for the blood of millions of people dripping from the hands of the WWF, Greenpeace, Rachel Carson, Environmental Defense Fund, and other junk science–fueled opponents of DDT."[67] Milloy is well-known for his attacks on science related to all kinds of environmental issues, including global warming (which he calls a "swindle"), acid rain (which he notes helps slow global warming—although he doesn't believe in global warming anyway), and the ozone hole (which he considers to be of no real significance).[68] Milloy's current project is junkscience.com, but, as we saw in chapter 5 "junk science" was a term invented by the tobacco industry to discredit science it didn't like. Junkscience.com was originally established in a partnership with the Cato Institute, which, after Milloy's continued tobacco funding came to light, severed its ties.[69]

The disinformation campaign continues on the Web, supported by organizations and institutes that are by now familiar. After Rush Limbaugh parroted the "Rachel was wrong" attack, the Competitive Enterprise Institute promoted him for the Nobel Peace Prize.[70] The Competitive Enterprise Institute shares philosophical ground with the American Enterprise Institute, which promoted the work of the late fiction writer Michael Crichton. His 2004 novel, *State of Fear*, portrayed global warming as a liberal hoax meant to bring down Western capitalism.[71] Crichton also took on the DDT issue, as one character in the novel insists, "Banning DDT killed more people than Hitler . . . It was so safe you could eat it."[72]

The "Rachel was wrong" chorus is echoed particularly loudly at the Heartland Institute, a group dedicated to "free-market solutions to social and economic problems."[73] Their Web site insists that "some one million African, Asian, and Latin American lives could be saved annually" had DDT not been banned by the U.S. Environmental Protection Agency.[74]

The Heartland Institute is known among climate scientists for persistent questioning of climate science, for its promotion of "experts" who have done little, if any, peer-reviewed climate research, and for its sponsorship of a conference in New York City in 2008 alleging that the scientific community's work on global warming is a fake.[75] But Heartland's activities are far more extensive, and reach back into the 1990s when they, too, were working with Philip Morris.

In 1993, Richard C. Rue, a project director for the Heartland Institute, wrote to Roy E. Marden, manager of Industrial Affairs for Philip Morris Management, to solicit continued support. Rue enclosed a copy of an op-ed piece, evidently an excerpt from a forthcoming book, written by Joseph Bast, the Institute president and CEO.[76] He recounted other recent Institute activities, boasting of distributing almost nine thousand copies of a special publication of the Chemical Manufacturers Association, of which eight thousand were sent to "state legislators and constitutional officers and other public opinion leaders."[77]

Philip Morris also used Heartland to distribute reports that they (Philip Morris) had commissioned. In April 1997, Roy Marden wrote to Thomas Borelli (who we met in chapter 5) discussing a task force report they had prepared in conjunction with the Association for Private Enterprise Education. Marden wrote:

> . . . the Heartland Institute, an Illinois-based policy group with whom we work, [will] publish a 24-page summary of the report/paper as a policy study. This will be released late next week, with a distribution of at least 3000 (half journalists, the remainder to state Constitutional officers and business types). Heartland would be willing to do a full run of 10,000 (which would include every state legislator and Member of Congress) if they can get the

> funding for the 7000 differential. I am getting faxed later what this will cost . . . and I think we should consider this.[78]

Heartland Institute officials also met with members of Congress on behalf of the tobacco industry, organized "off-the record" briefings, wrote and placed op-ed pieces, and organized radio interviews and letters to editors.[79]

In 1997, Philip Morris paid $50,000 to the Heartland Institute to support its activities, but this was just the tip of the iceberg of a network of support to supposedly independent and nonpartisan think tanks. The stunning extent of Philip Morris's reach is encapsulated in a ten-page document from 1997 listing policy payments that were made to various organizations. Besides the $50,000 to the Heartland Institute, there was $200,000 for TASSC, $125,000 for the Competitive Enterprise Institute, $100,000 for the American Enterprise Institute, and scores more.[80] Payments were for as little as $1,000 or as much as $300,000, and many went to groups with no evident interest in the tobacco issue, such as the Ludwig von Mises Institute or Americans for Affordable Electricity. Numerous other documents attest to activities designed to undermine the Clinton health care reform plan.[81] Often financial contributions were referred to in company documents as "philanthropy," and because these organizations were all nonprofit and nonpartisan, the donations were all tax deductible.[82]

The following image is the first page of this ten-page document listing the "policy" organizations to which the Philip Morris Corporation contributed. Note how nearly all of these were described as having a focus in either "Individual Liberties," "Regulatory Issues," or both, and how the Cato Institute, the American Enterprise Institute, and the Competitive Enterprise Institute—all of whom have questioned the scientific evidence of global warming—each received six-figure contributions. Note also the funding to the American Civil Liberties Union. Additional pages document contributions to the Frontiers of Freedom Institute, the Acton Institute, the Alexis de Tocqueville Institute, and the Independent Institute; to seemingly grass-roots organizations—the Citizens Against Government Waste, the Independent Women's Forum, and the Institute for Youth Development—and to university groups such as the George Mason Law and Economics Center and the University of Kansas Law and Organizational Economics Center.

Primary Focus	Secondary Focus	Multiple Focus	Oganization	Amount Paid in '97	Amt. Charged to BAC	Amt. Charge to Pol/Civ	Amt. Charged to USA BAC	Amt. Charged to Admin
Commercial Speech	Individual Liberties		Libertad, Inc.	100,000.00	-	-	-	100,000.00
Commercial Speech			Freedom of Expression Foundation	20,000.00	20,000.00	-	20,000.00	-
Individual Liberties	Regulatory Issues	X	American Enterprise Institute	100,000.00	-	-	-	100,000.00
Individual Liberties	Regulatory Issues	X	Cato Institute	175,000.00	150,000.00	100,000.00	50,000.00	25,000.00
Individual Liberties	Regulatory Issues	X	Competitive Enterprise Institute	125,000.00	125,000.00	100,000.00	25,000.00	-
Individual Liberties	Regulatory Issues	X	ETV of South Carolina	300,000.00	150,000.00	150,000.00	-	150,000.00
Individual Liberties	Regulatory Issues	X	Free Congress Foundation	300,000.00	300,000.00	300,000.00	-	-
Privacy			American Civil Liberties Union Foundation	75,000.00	75,000.00	37,500.00	37,500.00	-
Privacy	Individual Liberties		Defenders of Property Rights	45,000.00	45,000.00	-	45,000.00	-
Public Opinion Research			Roper Center for Public Opinion Research	5,000.00	5,000.00	-	5,000.00	-
Regulatory Issues	Tax	X	Manhattan Institute	20,000.00	20,000.00	10,000.00	10,000.00	-
Regulatory Issues		X	Hudson Institute	25,000.00	25,000.00	25,000.00	-	-
Sound Science	Regulatory Issues		TASCC	200,000.00	-	-	-	200,000.00
Sound Science	Regulatory Issues		Reason Foundation	20,000.00	20,000.00	10,000.00	10,000.00	-
Tax	Divestment		Municipal Treasurers Association of the US and Canada	2,500.00	2,500.00	-	2,500.00	-
Tax	Regulatory Issues	X	Heartland Institute	50,000.00	50,000.00	25,000.00	25,000.00	-

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Source: BN: 2078848138, Legacy Tobacco Documents Library

The Orwellian Problem

The network of right-wing foundations, the corporations that fund them, and the journalists who echo their claims have created a tremendous problem for American science. A recent academic study found that of the fifty-six "environmentally skeptical" books published in the 1990s, 92 percent were linked to these right-wing foundations (only thirteen were published in the 1980s, and 100 percent were linked to the foundations).[83] Scientists have faced an ongoing misrepresentation of scientific evidence and historical facts that brands them as public enemies—even mass murderers—on the basis of phony facts.

There is a deep irony here. One of the great heroes of the anti-Communist political right wing—indeed one of the clearest, most reasoned voices against the risks of oppressive government, in general—was George Orwell, whose famous *1984* portrayed a government that manufactured fake histories to support its political program.[84] Orwell coined the term "memory hole" to denote a system that destroyed inconvenient facts, and "Newspeak" for a language designed to constrain thought within politically acceptable bounds.

All of us who were children in the Cold War learned in school how the Soviet Union routinely engaged in historical cleansing, erasing real events and real people from their official histories and even official photographs. The right-wing defenders of American liberty have now done the same. The painstaking work of scientists, the reasoned deliberations of the President's Science Advisory Committee, and the bipartisan American agreement to ban DDT have been flushed down the memory hole, along with the well-documented and easily found (but extremely inconvenient) fact that the most important reason that DDT failed to eliminate malaria was because insects *evolved*. That is the truth—a truth that those with blind faith in free markets and blind trust in technology simply refuse to see.

The rhetoric of "sound science" is similarly Orwellian. Real science—done by scientists and published in scientific journals—is dismissed as "junk," while misrepresentations and inventions are offered in its place. Orwell's Newspeak contained no science at all, as the very concept of science had been erased from his dystopia. And not surprisingly, for if science is about studying the world as it actually is—rather than as we wish it to be—then science will always have the potential to unsettle the status quo. As an independent source of authority and knowledge, science has

always had the capacity to challenge ruling powers' ability to control people by controlling their beliefs. Indeed, it has the power to challenge anyone who wishes to preserve, protect, or defend the status quo.

Lately science has shown us that contemporary industrial civilization is not sustainable. Maintaining our standard of living *will* require finding new ways to produce our energy and less ecologically damaging ways to produce our food. Science has shown us that Rachel Carson was not wrong.

This is the crux of the issue, the crux of our story. For the shift in the American environmental movement from aesthetic environmentalism to regulatory environmentalism wasn't just a change in political strategy. It was the manifestation of a crucial realization: that unrestricted commercial activity was doing damage—real, lasting, pervasive damage. It was the realization that pollution was global, not just local, and the solution to pollution was *not* dilution. This shift began with the understanding that DDT remained in the environment long after its purpose was served. And it grew as acid rain and the ozone hole demonstrated that pollution traveled hundreds or even thousands of kilometers from its source, doing damage to people who did not benefit from the economic activity that produced it. It reached a crescendo when global warming showed that even the most seemingly innocuous by-product of industrial civilization—CO_2, the stuff on which plants depend—could produce a very different planet.

To acknowledge this was to acknowledge the soft underbelly of free market capitalism: that free enterprise can bring real costs—profound costs—that the free market does not reflect. Economists have a term for these costs—a less reassuring one than Friedman's "neighborhood effects." They are "negative externalities": negative because they aren't beneficial and external because they fall outside the market system. Those who find this hard to accept attack the messenger, which is science.

We all expect to pay for the things we buy—to pay a fair cost for goods and services from which we expect to reap benefits—but external costs are unhinged from benefits, often imposed on people who did not choose the good or service, and did not benefit from their use. They are imposed on people who did not benefit from the economic activity that produced them. DDT imposed enormous external costs through the destruction of ecosystems; acid rain, secondhand smoke, the ozone hole, and global warming did the same. This is the common thread that ties these diverse issues together: they were all market failures. They are instances where serious damage was done and the free market seemed unable to account for it, much less prevent it. Government intervention was required. This is why free market ideologues and old Cold Warriors joined together to fight them. Accepting that by-products of industrial civilization were irreparably damaging the global environment was to accept the reality of market failure. It was to acknowledge the limits of free market capitalism.

ORWELL UNDERSTOOD THAT those in power will always seek to control history, because whoever controls the past controls the present. So our Cold Warriors—Fred Seitz and Fred Singer, Robert Jastrow and Bill Nierenberg, and later Dixy Lee Ray, too, who had dedicated their lives to fighting Soviet Communism, joined forces with the self-appointed defenders of the free market to blame the messenger, to undermine science, to deny the truth, and to market doubt. People who began their careers as fact finders ended them as fact fighters. Evidently accepting that their ends justified their means, they embraced the tactics of their enemy, the very things they had hated Soviet Communism for: its lies, its deceit, its denial of the very realities it had created.

Why would any scientist participate in such a fraud? We've seen that Steve Milloy and John Tierney, the Competitive Enterprise Institute and the Heartland Institute, were late entries in this tournament, echoing arguments that had been first constructed by scientists. Our story began in the 1950s, when the tobacco industry first enlisted scientists to aid its cause, and deepened in the 1970s when Frederick Seitz joined forces with tobacco, and then with Robert Jastrow and Bill Nierenberg to defend the Strategic Defense Initiative. It continued in the early 1980s as Fred Singer planted the idea that acid rain wasn't worth worrying about, and Nierenberg worked with the Reagan White House to adjust the Executive Summary of his Acid Rain Peer Review Panel. It continued still further, and turned more personal, in the 1990s as the Marshall Institute, with help from Singer and Ray, challenged the evidence of ozone depletion and global warming and personally attacked distinguished scientists like Sherwood Rowland and Ben Santer.

Why did this group of Cold Warriors turn against the very science to which they had previously dedicated their lives? Because they felt—as did Lt. General Daniel O. Graham (one of the original members of Team B and chief advocate of weapons in space) when he invoked the preamble to the U.S. Constitution—they were working to "secure the blessings of liberty."[85] If science was being used against those blessings—in ways that

challenged the freedom of free enterprise—then they would fight it as they would fight any enemy. For indeed, science *was* starting to show that certain kinds of liberties are not sustainable—like the liberty to pollute. Science was showing that Isaiah Berlin was right: liberty for wolves does indeed mean death to lambs.