A SIERRA CLUB BOOK

ARE WE ENTERING THE GREENHOUSE CENTURY?

GLOBAL WARMING

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Mediarology

Was the "Summer of '88" a real example of climate change in the making or simply a media event? I am constantly asked that question in some form by journalists, business leaders, politicians, and people I happen to sit next to on airplanes, and I heard it asked of others literally hundreds of times since the summer of 1988. The daily newspaper, newsweekly, and broadcast stories on the heat, drought, forest fires, and super hurricane Gilbert were overwhelming factors in bringing climate-change issues to the public consciousness. The term *greenhouse effect,* which had been so difficult to squeeze into the public vocabulary over the preceding fifteen years, had entered the media jargon even before 50% of Yellowstone National Park was blackened by tremendous uncontrollable fires in late summer. How did a relatively obscure scientific concept, such as the trapping of thermal infrared radiant energy in the lower atmosphere—that is, the greenhouse effect—move from the professionals to the press so rapidly?

The rise in public consciousness engendered by the Summer of '88 actually began in the early seventies, when stories on human impact on the global climate first started appearing. Of course, much of the talk then was about cooling from human-generated dust, but the greenhouse effect still got substantial play. Neither theory, though, gathered much serious public attention. In 1976 I testified to Congress in support of the creation of a National

Climate Program that was to consolidate fragmented government-agency efforts into a coherent national program.[1] Part of my testimony dealt with the potential of global warming from increasing carbon dioxide. "Are you saying," asked one congressman rhetorically, "that there might be some global environmental limits to the use of our nearly unlimited supplies of cheap coal?" While I acknowledged the alacrity with which he picked up this subtlety, privately I was stunned to realize that people of his stature had not heard of the greenhouse effect. And even up to 1988, this was still possible!

Since 1976, testimony on the issue has taken place before Congress, in parliamentary proceedings in Europe or Australia, at the World Climate Conference in 1979 (sponsored by the United Nations agency the World Meteorological Organization), and at numerous national and international meetings. And, gratifyingly, media coverage of the issue, at least that by the well-established science writers, has become increasingly sophisticated. Of course, even these journalists had to make some compromises to get the next century's news into print today. Many, for example, needed to call me or other scientists in the field on the heels of an unusual heat wave, a drought, a hurricane, even a cold wave, or some other "weather peg" on which they could hang the greenhouse issue. After a hurricane, for instance, a journalist might provide a few obligatory paragraphs about the millions of dollars' worth of destruction and the 150-mile-an-hour winds before slipping gradually into a decent feature article about the overall prospect of climate change and its implications for hurricane intensity, the environment, and society. Many good science writers would call for a quote or two about the weather peg, as an excuse to write an article about the more important longer-term issues that are difficult to get in to the news media without some dramatic and immediate hook. With the hundreds of such stories plus the government forums that had occurred since 1970, by 1987 I assumed that the greenhouse effect was pretty much of a household term. As things turned out, there was a long way to go.

Throughout 1987, the climate committee of the AAAS sponsored a series of meetings. First a special panel was convened to analyze and then to write a book about how the United States might cope with the greenhouse effect in the area of water resources. I was on it (its conclusions were mentioned several times in Chapter 6). One of our panelists was a distinguished political

scientist, and her initial contribution suggested that climate change was unlikely to become a part of the national, let alone international, political agenda. The issues involved simply changed too slowly; they were long-term issues with relatively little immediacy, no focused constituency, no single identifiable villain, and no clear and visible proof that could be trumpeted in terms familiar to most people. In other words, to get attention our issue had to be "soon, serious and certain."[2] Without these characteristics, we were told, no issue could be high on the agenda. Surveys and other professional judgments bolstered this hard-nosed conclusion born of political experience.

In May 1988, I attended a meeting in Washington sponsored by the economist-oriented think tank Resources for the Future (RFF). In one debate session, several economists argued that the government would never express concern about, let alone take action on, the greenhouse effect until scientists could provide detailed forecasts of the winners and the losers and the wheres and the whens. Another tack would be to create popular movies or television shows with known media stars who would embrace the subject sufficiently to push it through the public's rather narrow sensors. I wasn't so cynical and expressed the opinion that real-world events would have much more impact on the public consciousness than a few clever scientists or cute stars hyping the issue on the tube. A long heat wave, complete with brownouts in the Northeast, or a month of wildfire smoke streaming across California, I speculated, would do more to raise public consciousness than any of us could possibly do through high-profile public relations. What happened in the few months after the RFF meeting was beyond my wildest expectations of how events could, through the media, totally transform the public awareness.

THE SUMMER OF '88. May and June in the United States saw rare and anomalous weather. Normally, the jet stream, a five-mile-high river of air, steers storms directly across the midsection of the United States as it meanders from an average position roughly around southern California across the Midwest into New England. But that spring, the jet stream split in two: a weak branch went into southern California and across northern Mexico (keeping cool Pacific air in southern California) and a strong branch looped up into Canada, diverting the storms away from

the U.S. breadbasket. Given the high intensity of sunlight in May and June and the relative absence of clouds and rain, drought and heat began to build at an alarming rate across the country. Since northern California, many parts of the West, and the Missouri River basin were already drier than normal when the spring of 1988 began, this rare climate anomaly (a persistently split springtime jet stream) moved toward catastrophe. Newly planted crops withered, Mississippi River levels dropped so low that thousands of barges were halted until the Army Corps of Engineers could dredge narrow channels, and salt water moved up the Mississippi toward New Orleans. The hundreds of greenhouse effect stories that had been dribbling out of the media for years seemed suddenly to combine, producing a spontaneous media conclusion all over America: The greenhouse effect has finally arrived!

No longer was a weather peg needed to get a climate-change story on the front page; such a story was the lead almost every day for weeks in every form of media. Cracked earth, withering plants, stranded barges, and record high 100° + F (38° + C) temperatures flashing on outdoor bank building thermometers were commonplace visual images in the media accounts. An international gathering in Toronto at the end of June attracted so many reporters that extra press rooms had to be added to handle the hordes of descending journalists. To be fair, the Canadian organizers had already built unique elements into this conference: a mixture of scientists, government-agency civil servants, elected political officials, diplomats, and even a few prime ministers. The purpose was to discuss policy implications of global warming. Initially, there were hopes that there might be some press interest since the subject had been increasingly covered in the past few years. But by late June 1988, the almost daily bombardment of climate news had turned this conference into a media mecca.

A U.S. Senate energy committee hearing on global warming a couple of weeks earlier had gathered even more momentum. I had been invited to testify, but declined because of a schedule conflict.[3] In doing so, I missed one of the most effective speeches in raising the public's consciousness about the greenhouse effect, by Dr. James Hansen of NASA's Goddard Institute for Space Studies in New York.[4]

Jim Hansen, an atmospheric physicist, spent most of his early career in the rather arcane business of calculating how light (or

other forms of radiant energy) was propagated, scattered, and absorbed by planetary atmospheres containing gases and particles. I spent 1970–1972 at the Goddard Institute for Space Studies as a visitor and postdoctoral fellow, and had almost daily lunches with Jim, where I discussed the new emerging field of human impacts on climate. He, in turn, would tell me of complicated mathematical techniques for making highly accurate radiative-transfer calculations.[5]

Many years after I had left this small, quiet research outpost of NASA, both Jim's interest in climate and his administrative responsibility had grown, and he had assembled a multidisciplinary group of atmospheric modelers, radiative-transfer specialists, geologists, biologists, and others to work on the greenhouse effect. This was GISS, a small satellite branch of NASA's large Goddard Space Flight Center in Greenbelt, Maryland. GISS was constantly struggling for money. It had received some early funding help from the Environmental Protection Agency but never had much prominence or support within NASA. But by the mideighties Jim had testified a number of times before Congress, and several juicy quotes from him had made it into the national media. He began to develop the reputation as an ardent believer in the seriousness of the greenhouse effect, and had calculations from his institute team to back up his strong words.

Then, one fateful day in June 1988 (the 23rd—a day selected months earlier by Senator Tim Wirth, since it was the anniversary of the hottest day ever recorded in D.C.), Hansen appeared before the Senate Energy Committee, the television lights glaring and the pens of the world's journalists poised. That day he gave the greenhouse-effect buffs a big shot in the arm but himself a load of trouble. He was "99%" sure that the warming of the 1980s evident in his calculations was not a chance event, he said. It is time we stop the "waffling" around about uncertainty and started taking the greenhouse effect pretty seriously, he went on. Immediately that "99%" was everywhere. Journalists loved it. Environmentalists were ecstatic. Many meteorologists were upset. Jim appeared on a dozen or more national television programs, was quoted in a front-page story in the *New York Times,* and even showed up on David Brinkley's Sunday television program sporting a large pair of dice on which he painted some faces to represent more warm years.

A lightning rod attracts lightning. In science, if you enhance

your visibility through the media, you're almost certainly going to stir the passions of some of the quiet workers who have virtually no opportunity for such notoriety. A few of those workers—people who had been toiling to resolve the uncertainty bit by bit without the world watching—began to look for ways to shoot down Jim Hansen's now famous "99%" assertion as to the likelihood that the greenhouse effect had been detected in the climatic record. Natural scientific skepticism, aversion to public discussion of still uncertain science, a misunderstanding of the policy-making process and the media, and yes, plain jealousy all contributed to the scrutiny Hansen was about to receive from different quarters. Newspaper accounts soon appeared quoting opposing scientists. Patrick Michaels, a University of Virginia climatologist, found that a thermometer in an obscure island in the south Atlantic called St. Helena had been moved down a mountain slope in the 1970s, thereby giving the false impression of a recent rapid climatic warming. In some stories, this data error got equal billing with the thousands of valid thermometers Hansen and Lebedeff had averaged. (Later on, Hansen made a correction and concluded that this error changed his global record by less than a tenth of a degree.)

In order to validate one of the GISS model predictions, Hansen had worked with his colleague Sergei Lebedeff to try to reconstruct a record of the earth's temperatures over the past hundred years. This issue is not straightforward. Not only must researchers assemble millions of data bits from thousands of widely scattered thermometers around the globe, but they must rely on people to read them, and that means human error. Further, some thermometers, as noted, have had cities grow up around them, while others have been moved out to airports and up or down mountains. Although Hansen and Lebedeff did attempt to correct for these variables, the computed planetary temperature changes over the last century contained significant elements of uncertainty. (In late summer 1988, an entire meeting was devoted to critical analysis of the Hansen-Lebedeff curve.)

Furthermore, when Jim stated that there was only a 1% chance that the record year of 1987 could have been so warm by accident, he implied that every year of the climatic record was independent from every other year. In other words, each year of climate has no memory of what has come before. Analyses of long-

term climatic trends suggest that this assumption is not necessarily a good one, and that trends can persist for decades if one accounts for the temperature memory in the oceans.[6] This meant that the statistical method the GISS scientists used was, technically speaking, questionable for drawing a quantitative conclusion such as the "99%" confidence one. Despite these qualifications, I fully agree with Jim that it is much more likely than not that the greenhouse gas buildups in this century have contributed to the observed global warming. It is simply impossible to assign formal probabilities without making intuitive assumptions.

Incidentally, Phil Jones and Tom Wigley in England produced a similar but not quite as dramatic graph of the global temperature trend that agrees the world has warmed up by more than 0.5° C (1° F) this century. Jones and Wigley used different techniques to average their thermometers, and included many more measurements from the oceans as well. Both records were compared in Figure 4. The English group's warming is about 0.6° C and the Hansen record about 0.8° C over the same 100-year time period. Considering the difference in techniques and the additional records, this is good agreement. Furthermore, Tom Karl of the U.S. government's National Climate Data Center in Asheville, North Carolina, compared these records with a very detailed study that made use of thousands of additional thermometers in the United States for this century that helped eliminate the biases owing to urban heating. What he found was that both the GISS and the CRU records captured the warming in the U.S. stations up to the middle of the twentieth century and also captured a slight cooling thereafter. (The slight cooling in the United States in the last several decades proves nothing about the greenhouse effect since the greenhouse effect is a global, not a local, phenomenon.) Karl found that for the United States, Jones and Wigley had a century-long trend with about 0.15° C too much warming,[7] whereas the Hansen record had about 0.38° C too much warming. If you apply these correction factors from the United States to the global trends of the English or NASA scientists, both would come out with a global trend value of nearly 0.5° C warming over the past 100 years. And that correction makes the radical assumption that the entire world would experience as severe an urban-heat-island bias as the United States, which I think is unlikely. At a recent meeting at the National Academy of Sciences,

Tom Karl assured the assembled group that he was not trying to suggest that there had been no global warming during this century, nor that the magnitude of change projected by these famous trend curves in Figure 4 are incorrect. He argued that the simple averaging of thermometers, with their observer errors and other complicating factors, is a very tricky business and to get precise quantitative answers is not easy. Unfortunately, that message has often been lost in the media, obscured by a highly polarized account of Hansen versus his doubting or cautious critics.

Personally, I think Jim would have been better off not using the "99%" figure since the problem with the urban heat islands and the technical arguments that are possible over the assumption that each year was independent from the others gave ammunition to his detractors, regardless of whether they were motivated by an honest search for the truth, political attempts to damn Hansen's credibility, or simple jealousy. In December 1988, Jim Hansen and I appeared simultaneously at a press conference held in Washington at a well-attended meeting sponsored by the Climate Institute. At the end, one of the reporters asked Jim the inevitable question: "Dr. Hansen, after all the criticism you have received, do you still stand by the 99% statement?" I presumed that whatever Jim said I would be getting the question next. Jim answered that he never should have said it, but since he believed in the statement, he still stood by it. Then it was my turn. I argued that I wouldn't have used the 99% number because it simply had no meaning for me. I noted that there were too many assumptions we could not verify (such as the independence of one year's temperature from the next) and things we didn't know (how to correct for urban bias effect) to be able to assign meaningful probabilities. Nevertheless, I said I completely agreed with Jim that there was a very good chance that the world had warmed up at least half a degree Celsius in the past century and that it was very likely as well that much of this was caused by the buildup of greenhouse gases. The fact that I believed the enhanced greenhouse effect was already present in the observed records was not the same as proving it, which was why I preferred to use verbal rather than numerical descriptions of its likelihood. Another reporter then asked, "But isn't the greenhouse effect just a theory? Why do you sound so confident?" Yes, it is a theory, I said, but

so is gravity. If you wish to revalidate that theory, then please stick your neck into my guillotine and we'll conduct another test. I don't think the greenhouse effect is quite yet as solid a theory as gravity, I continued, but the point is not whether the greenhouse effect is a "theory" but rather whether we can validate the theory. I told him that I believed the greenhouse effect to be a well-validated theory, based not on the performance of the planet in the last hundred years but on other evidence such as the climates of Mars, Earth, and Venus, the workings of the seasonal cycle, hundreds of physical measurements of the properties of atmospheric gases, and millions of spacecraft measurements of the energy balance of the earth.

Two days later, when I came home I found a headline from the local newspaper posted on the bulletin board: "Boulder scientist says 'greenhouse effect' not just a theory." This Associated Press story correctly quoted my wisecrack about the guillotine, but did not contain the extra few sentences about the importance of *validation,* and not the word *theory.* Two of my colleagues told me they thought I had been excessive in that statement, until I explained what I had said in total. I could only wonder how many others would never bother to ask. Such experiences are typical of those involved in the public discussion of complicated issues in which polarized opinions abound. Scientists who go public with their work, particularly if it could potentially affect policy, often meet with a mixed reception from their own colleagues. Let me recount some of my earliest experiences with this problem.

In the early 1970s, the northern hemisphere appeared to have been cooling at an alarming rate. There was frequent talk of a new ice age. Books and documentaries appeared, hypothesizing a snowblitz or sporting titles such as *The Cooling.* Even the CIA got into the act, sponsoring several meetings and writing a controversial report warning of threats to American security from the potential collapse of Third World governments in the wake of climate change. (I described one such meeting at the White House in 1974 in *The Genesis Strategy.*) I believed then (and now, too) that climatic variability could be a major security threat to nations, particularly those chronically short of food and without adequate financial and agricultural reserves to deal with the bouts of bad weather that nature might select. Some scientists had an additional concern that the climate could be altered as an in-

advertent by-product of economic and population growth. Industrial and agricultural dust and smoke drifting in the skies were being blamed for blocking out sunlight, thereby cooling the climate—a view made popular by Reid Bryson of the University of Wisconsin. Such logic often seemed plausible to those who looked skyward through polluted air at a dimmed, orange sun. But by 1972 a number of us began to think it wasn't only the visible junk in the sky we should worry about, but also an ever-growing burden of invisible gases, which might turn out to be the more sinister climate threat.

In Baltimore in December 1972 I gave a talk on the issue of human weather control to the annual convention of the American Association for the Advancement of Science (AAAS). AAAS meetings are internationally known because they bring together research scientists and policy makers to discuss the societal implications of new knowledge. In addition, AAAS meetings attract hundreds of journalists; by contrast, most ordinary scientific meetings are unlikely to attract even half a dozen media people. For me, an upstart twenty-seven-year-old postdoctoral fellow, my invitation to speak at the meeting was an exciting opportunity to share the podium with scientific leaders typically twice my age. After speaking for half an hour or so on how various kinds of human activities could change the climate, I concluded that, unfortunately, only a relatively few people were aware of the possibilities. I then quipped: "Nowadays, everybody is doing something with the weather, but nobody is talking about it."

At the front of the audience a distinguished-looking gentleman was taking notes: he turned out to be the dean of all science writers, Walter Sullivan of the *New York Times*. Since journalists love one-liners—especially if they boil down complicated issues into a quick phrase, or reflect or create controversy, the next day's *New York Times* featured a story on weather control that closed with my reverse Mark Twain quip.[8] From then on, for better and for worse, my opinions were no longer my own property.

Meanwhile, in Boulder, Colorado, at the National Center for Atmospheric Research (NCAR), most scientists were busy doing what the vast majority of good scientists do well: working quietly and carefully to advance knowledge by learning small bits about their discipline or inventing new tools or techniques to help facilitate such learning. Scientists toil for years in some specialty

trying to uncover some of nature's mysteries. Then they publish their findings. Over time, those publications contribute to the most precious intangible a scientist ever owns—his or her scientific reputation. The unwritten rules in science decree that recognition is supposed to be based on years of careful work backed up by scores of publications appearing in the most strictly peer-reviewed scientific journals dealing with narrowly defined topics. Published deeds that stand the tests of time are supposed to build one's recognition, not clever phrases that capture the public's—or worse, the media's—attention. These unprinted rules were still in full force in the early 1970s. Therefore, it shouldn't have been surprising that when I returned to Colorado I found a clipping of the *New York Times* story prominently displayed on the door of the weather map room (the most conspicuous spot imaginable). Stamped on it in large letters, right next to the quote from me, was an anonymous peer review, unusually clear and to the point: "BULLSHIT." Over the next few weeks, things went even further downhill.

NCAR employs a "press intelligence service," which is a company that reads all the newspapers and magazines readily available. Any time the service finds the name National Center for Atmospheric Research, it clips the article and sends it to NCAR. NCAR's public information office then compiles a month's worth of clippings, photocopies them, and distributes them to hundreds of scientists around the country. It was interesting to watch what happened in the wake of Walter Sullivan's well-balanced article. Within days of its original appearance, better newspapers picked up the story with Sullivan's byline and the *New York Times* copyright. Some dailies shortened the article and nearly all of them had different headlines. (Headlines are rarely written by the journalist who writes the article, I later learned, as on many occasions journalists have called me to apologize in advance for the sensational or inaccurate headlines that their papers were about to put over their story.) As the Sullivan article continued to work its way into smaller and smaller markets, parts got cut out and eventually some papers dropped the *New York Times* copyright and even the Sullivan byline! The worst offender, I recall, was a short condensation under a local reporter's byline, with a headline about "water" modification rather than "weather" modification. That article also appeared on the map room door.

this time with two friendly peer comments written in bright blue ink. What happened to the Sullivan story as it journeyed from city to city and eventually into the journalistic backwaters of America was akin to what happens in the children's game of "telephone": a group of kids sit in a circle; one whispers a word or phrase in the ear of his neighbor who then whispers it to his neighbor and so on, until the last child repeats what he thinks his neighbor just told him. How often have children giggled wildly when the one who originated the phrase tells out loud what he actually said. But this was no game; this involved in forming the public.

I learned several lessons from this experience. First, that no one should believe all the details of newspaper stories—hardly an original discovery. However, one can usually trust the broader outline of such stories, such as what the issues are, and, if they are controversial, why, and who the people are who are involved with these issues. Secondly, I decided there are probably two safe ways for a scientist to deal with the press: not at all, or a lot. If you deal with the media at all and choose the latter way, you minimize the risk of damaging your reputation from a few bad stories since there is a greater chance for more good ones as well. Ultimately, however, I resolved to write a book about climate change myself rather than expect the media to carry my message about this complex, uncertainty-riddled topic to the public.

My first book, *The Genesis Strategy: Climate and Global Survival*, came off the presses and was announced and distributed in February 1976—appropriately enough, at an AAAS meeting in Boston. (By then, the core of science journalists who routinely attend this annual meeting had been exposed to several years of climate talk and were now much more sophisticated about climate issues. The quality and number of stories written by them—not by general assignment writers unfamiliar with scientific content and methods—dramatically increased.) The book hit about the same time that a number of nasty climatic events were unfolding that helped to increase public exposure to issues of climate change. But the greenhouse effect hardly became a household phrase. Indeed, in *The Genesis Strategy* I focused more on the need to plan for large (but precedented) natural variability of climate. However, the book also clearly argues that climate change from human activities may pose an ultimate limit to growth in the long

term and that therefore we must attempt to control environmental pollution.

In the decade that followed, scientific consensus grew for the theory that the injection of invisible greenhouse gases was likely to cause unambiguous global-scale warming by around 1990 and create unprecedented climatic change sometime in the twenty-first century. Assessments by teams of scientists from the U.S. National Academy of Sciences, the United Nations, the U.S. Congress, the Canadian Meteorological Society, the Australian government, the European community, business, and other institutions increasingly took place to discuss the climate issues; and increasingly, news of global atmospheric change was moving up from the back pages of the science section of newspapers and magazines, particularly when bad weather (like heat waves) struck. But until 1988, acute awareness of the greenhouse effect and global warming was still largely confined to academic, congressional, environmental, or industrial professionals.

Despite very sound physical principles that drove a few dozen scientists around the world to boldly predict ten to fifteen years ago (when most people thought the earth was cooling) that a global warming trend would become clear by 1990, the issue was not raised to prominence in the public consciousness or *on the political agenda. For one thing, the public had long been* subjected to doom-saying placard-carriers warning of various disasters that never came to pass. Moreover, global atmospheric change had few dramatic, familiar metaphors—at least until 1986 when the ozone hole mysteriously appeared over Antarctica, threatening additional skin cancers and untold ecological damage. Then, in 1988, nature did more for the notoriety of global warming in fifteen weeks than any of us or the sympathetic journalists and politicians were able to do in the previous fifteen years.

Journalists have a tradition, quite appropriate I believe in political reporting, of providing balance in a story. This tradition stems from the need to give both sides of the political spectrum equal exposure. However, balance is not a very good doctrine when applied to science reporting. It does not help the public to understand the nature of complex technical questions to balance an extreme position of a scientist or advocate at one end of the spectrum against an extreme position of a scientist

or advocate on the other end. Technical issues often have more than two sides, which means that polarized reporting can create a false dichotomy. The public, and the politicians who must ultimately make policy, need to know not only what the members of the community think, but also what the broad spectrum of responsible and knowledgeable opinion is on an issue. If only the irreconcilable debates of implacable expert enemies are reported, the typical public reaction (and probably those of politicians as well) will be, "Well, if the experts don't know what's going on, how can I decide?" The next reaction would probably be, "You folks go back and study some more, and when you have more certainty come and tell us so we can decide how to act." Instead, knowledge of a consensus over how much we already know or a rough estimate of how long it will take to learn a great deal more could very well lead to public and political reactions quite different from those created by a noisy, angry, and dichotomized debate.[9]

In the June 1988 Toronto meeting, for instance, I was asked to hold a press conference. A reporter from a U.S. newsweekly kept pressing me on whether I differed from Jim Hansen and his 99% statement. I said that I felt the 1980s, having had several record warm years, provided increasingly strong circumstantial evidence that a greenhouse signal was emerging. I even said that I believed the issue would be settled over the next decade or two, but that as a scientist I'd have to admit that some uncertainty still remained. "Then you disagree with Hansen," said the reporter. I then said, "I'm not going to get into a false dichotomy debate with Jim Hansen when we both agree that the physical basis of the greenhouse effect is very strong, that greenhouse gases have increased, and that global warming is occurring on the planet. True, I choose to state this problem differently than he has, but focusing only on that difference is not going to give the right impression if it is presented in the name of 'balance' as an opinion opposite to Jim." Several reporters asked other questions. Then, toward the end, my persistent friend from the newsweekly said, "I take it, then, that you don't agree with Hansen that the greenhouse effect has been detected with 99% certainty."

Despite a few such frustrations, the Toronto meeting had a major impact. It got excellent press coverage in the United States, Canada, and other parts of the world. The participants called

for radical and specific policies to reduce carbon dioxide emissions by 20% in the year 2005. That stirred quite an internal debate (described later) but was effective in capturing press attention. On the closing day of the plenary session, the meeting chair (the then Canadian ambassador to the United Nations, Stephen Lewis) asked me for my view of how the meeting's strong conclusions would be received in the United States. There were basically two categories of conclusions he wanted me to address: (1) reducing emissions of greenhouse gases through energy-conservation programs or the development of nonfossil energy systems; and (2) recognizing the special problems of Third World countries who desperately lacked economic development and needed extra help if they were to develop with less polluting technologies. To answer his question, I considered the probable U.S. reaction of five separate groups: the scientists, the bureaucrats, the politicians, the media, and the public. I offer my response below as an overview of current thinking among these groups in the United States.

Most scientists probably appreciate the attention their fields are getting, but many consider statements suggesting concrete policies premature as long as major uncertainties remain. Unfortunately, too many scientists still regard a decision to react to possible change in the face of uncertainty as a scientific judgment, when in fact it is a value question, requiring that we weigh the risk of unabated change against the risk of possible wasted investment.

Most bureaucrats' reactions are also relatively easy to identify and describe. The bureaucratic culture thrives on information. Those who live and work in it subsist on a daily diet of cost-benefit studies, flowchart assessments, and so forth. While environmental protection officials would largely welcome the possibility of more regulatory activities that would expand their influence, they would probably be uneasy with the conference's recommendation for action until further cost-benefit studies showed the impact of the proposed specific reductions in greenhouse gas emission on various countries and various economic sectors.

Politicians, on the other hand, care much less about information than about perception—the perceptions of their constituents. If they feel the public is alarmed about an issue such as climate change, they will scramble over each other to look as if they are

making hard decisions and lending leadership to implement concrete actions for dealing with the situation.

The public's perceptions, of course, are primarily shaped by the next category, the media, which thrive on the four Ds: drama, disaster, debate, and dichotomy. The summer of 1988—not even a month old at the end of June, when the Toronto meeting took place—had already provided enough of these to bring the media on board the climate problem wholeheartedly. Keeping their interest up without overstating the relatively weak connection between the greenhouse effect and one season of weather was no easy chore.

What, then, was to be the public reaction? The public, of course, has long been subjected to placard-carrying advocates warning of one impending disaster or another, the forestalling of which supposedly demand immediate attention—and resources. Although I thought people had all heard of global warming, I had long feared what my political science colleagues on the AAAS water-supply panel had documented: that a problem like climate change simply lacked sufficient familiarity and credibility to capture the public's attention, let alone its imagination. Moreover, faced with a steady diet of dichotomy and debate in the media—with one scientist saying warming while others said cooling—it was not surprising that it had been difficult to mobilize public attention on the issue over the past decade. But the Summer of '88 had reversed all that. In the United States, public perception of the problem was becoming so acute, I told the Toronto assembly, that U.S. politicians would soon be proposing dramatic actions, such as the 20% CO_2 emissions reductions being proposed in Toronto (indeed, before the end of 1988, several major bills based in part on the Toronto recommendations had been introduced in the Congress). However, there was a risk of severe credibility loss for climatology if nature rolled a cold, wet summer or two soon, and this was quite possible given all the faces on the climate dice, even if they are loaded.

So far, my answers to Ambassador Lewis's question had been mostly positive: I thought the United States would be able to react positively to the conference's recommendations—as long as we didn't get too much cold, wet weather in the next few years. But I had so far dealt only with the first category of conclusions—U.S. emissions reduction. It was much tougher to be optimistic about the second: encouraging developed countries to help devel-

oping countries recognize and meet their special development needs in an environmentally sustainable way.

Only a few studies have been done to help the bureaucrats determine cost-effective ways in which Third World countries could develop in a reasonably pollution-free manner. The media have primarily focused on climate emergencies at home, and with the exception of floods in Bangladesh and hurricane destruction in the Caribbean and Mexico, in 1988 there was little coverage of the every-day crises in food, health, shelter, and employment in many Third World countries. Can we ask China not to develop the abundant coal resources she is counting on to improve her economy (as we ourselves did in the early days of the Industrial Revolution) simply because the increased use of this highly polluting stuff will make our world warmer? We could only ask the Chinese (or the Indians, or the Brazilians, who are chopping down their forests) to change their development plans if we provided alternative, more efficient, and less polluting technologies or helped those populous, debt-ridden countries develop less energy-intensive economies. But it will be a political struggle even to pursue these objectives faster at home, and we will have no moral authority to advise the Third World to make sacrifices until we make some at home. Before we can expect our political leadership to agree to transfer substantial resources to the Chinese, we will need to generate a great deal more media attention and work to foster public understanding of the global development issue. Therefore, I concluded, while we may have experienced an unprecedented jump in climate-change consciousness that might encourage action to curb some of our own emissions, we are not yet at the level of perception and understanding needed to address the greenhouse-effect problem on the global scale.

As the summer of 1988 wore on, more hearings were held, more crops withered, and by late August fires had become the new media event. Yellowstone National Park, perhaps America's most important symbol of nature protected from the ravages of civilization, was ablaze. Day after day, flames over Yellowstone were seen on televisions in living rooms across America. Firefighters, old monuments, buildings, fleeing wildlife, and obscuring smoke drifted across the tube. Before long, senators, governors, and cabinet secretaries appeared at the scene with their sleeves rolled up looking at charts while smoke billowed behind them. We must

question the park service's policy of "let burn," they typically remarked, and work harder to put these fires out before they spread. The media dutifully covered their seemingly grave concern and a number of good feature stories appeared on the issue,[10] but unfortunately there was little widespread reporting of the underlying basis of the conflagration. The great Yellowstone fire of 1988 was as much a manmade phenomenon as one of nature. True, intense drought dried the woods and underbrush to such a tinder that any spark, whether from lightning or an irresponsible campfire, could ignite it. Strong winds would blow such fires out of control, even if hundreds or thousands of firefighters were waiting to contain a small plot that was deliberately being allowed to burn. What lay behind all this, however, was the fact that Smokey the Bear had been too successful.

The fairly recent let-burn policy of the park service was based on the ecological principle that natural environments had evolved with fire, and that fire was part of—in fact, beneficial to—the health of forests. Some seeds cannot even be released from their pine cones until heated. The ash from burning underbrush, dead leaves, and trunks fertilizes the soil, generally reducing its acidity. Acid soil is both bad for many species of trees and promotes the growth of moss, which further acidifies the soil. Eventually, enough moss can kill trees, turning the land into bogs.[11] Fire is a disturbance that allows ecosystems to remain dynamic. Thus, fire is one of nature's ways of rejuvenating forests by turning over the nutrient stock and maintaining the vigor of the ecosystems. However, when people move into forested areas and build houses or recreation centers, they lose sight of long-term ecological considerations. Their primary goal is to protect their property. That fact, coupled with the mid-twentieth-century view that human beings know better than nature, helped create the fire-suppression mentality that has hovered over forest lands in this century. We must not forget either that timber is a big business and that burned trees cannot be logged and turned into lumber.

All these pressures were combined with one of the most successful public relations campaigns ever mounted to prevent the natural rate of burning. Many of us remember the stern-faced Smokey the Bear pointing his finger at us from out of the small black-and-white television sets of the 1950s and 1960s and saying, "Remember, only you can prevent forest fires." Unfortunately,

Smokey forgot to say one thing: if fires don't occur often enough, then dead and accumulating plant matter will increase the fuel loading on the surface of the woods to such an extent that when the inevitable drought occurs not only will a fire occur but it will be one of such intense heat and scale that it could actually damage the ecosystem. This, apparently, is what happened in Yellowstone. Fuel levels built up over decades of intense fire suppression. Such a catastrophic fire was inevitable, a number of foresters said at a recent meeting. It was only a matter of time. Drought and heat are bound to occur sometime—and they did in the summer of 1988. The foresters were hardly surprised when these conditions conspired. Nor were they surprised, they said rather cynically, that when the fire hit the politicians went straight to the press to warn us sternly that we must work harder to suppress fires so this would never happen again. Such short-term political expediency, uncritically reported through the media, is likely to drive us even further in the wrong direction based on a misinterpretation of the underlying cause of the intensity of the Yellowstone disaster. Of course, too much political attention to short-term solutions is hardly news. A few anecdotes may make the short-term bias of society more tangible.

In July of 1980, in the middle of a heat wave in the southcentral states, I received an exciting invitation from NBC News: fly to Washington and appear live on "Meet the Press." The Sunday morning on which the program was to be aired was not a feasible date for me, so I asked if we could prerecord it. An hour later brought the affirmative response. That evening, I had dinner with the moderator; the program was to be taped later that night. It was a pleasant evening, enhanced by the elegant style of wining and dining. We discussed the media, and I told the moderator of the frustration of having several lengthy interviews of mine cut or discarded altogether unless there was a strong weather peg. He sympathized and recounted similar stories of his own. Has that ever happened with "Meet the Press," I asked. "No," he replied. "It would take an assassination of the president to bump this program," he said. Off we went to the studio to tape the show.

The taping went well, the producer expressed satisfaction with the program, the director said the tape was technically good, and we all left happy. The program was then advertised in the newspapers for the upcoming Sunday. However, that Saturday night

the phone rang. It was the producer. "Remember what I said about an assassination? Well, it's essentially happened. Four Democratic congressmen have just announced a 'dump Carter' movement, and my executive producer insists that we put them on live this Sunday morning. We're going to put your program on the following week."

What then happened to the first-ever embargoed "Meet the Press"? A week later, they weren't sure whether they were going to run it on the next Sunday—the Democratic Convention was coming up. Two weeks later, a major flood struck in drought-ravaged Texas. NBC decided it couldn't do a weather program that makes no mention of this highly visible weather event—once again they failed to run the program, and so it went.

One final anecdote illustrates our society's inevitable emphasis on short-term priorities. In July 1988, Democratic presidential candidate Michael Dukakis visited Colorado. Senator Tim Wirth called to invite several Colorado environmental specialists to join him for a 6 A.M. expedition to the Rocky Mountain National Park, where the candidate was scheduled to tour the park and give an address outlining his environmental views. The senator had arranged a breakfast briefing where a number of us would have a few minutes to explain our particular viewpoints to Dukakis. At the breakfast, three of us briefed the candidate: one on wilderness, another on urban air pollution and transportation problems in the West, and me, on global warming. I had six minutes at the end to deal with the next sixty years.

After breakfast, two buses loaded up, carrying an entourage of some 100 campaign staffers, national press people, secret service agents, and state officials in a police-escorted drive through the Rocky Mountain National Park to the beautiful Bear Lake location. At one point along the narrow pathway around the lake, the Colorado senator began explaining to the candidate how an eroded gully could not be repaired as long as the current administration continued to sequester funds from park receipts rather than spend them on trail repair. Some seventy-five members of the press elbowed past each other in an effort to zero in on this conversation, with technicians, producers, camera operators, boom holders, and others all racing to the scene—trampling flowers, stomping trees, cracking branches, and scaring wildlife, all to hear a rather short but important political discussion on preserving the park environment!

In the scenic beauty of the national park, the candidate nicely summarized what he had heard that morning and added several additional interesting twists of his own—proposing, for example, an environmental summit between the U.S. president and Soviet leader Gorbachev to deal with the greenhouse effect, ozone, and acid rain. After his short speech, the scores of press began to question the candidate. There were nine questions, of which only one was even on the environment, to draw attention to which was the announced purpose for the trip to Rocky Mountain National Park. And that question came from a European journalist, not an American. The Americans asked questions about the candidate's political rivals (especially Jesse Jackson) and Dukakis's views on taxes. The commercial broadcast networks as well as the cable news services all covered the event, but the twenty-second sound bites that made the evening news programs primarily dealt with the issues of taxes or Dukakis versus Jackson. I heard virtually no discussion of the environment, except in one passing reference to that as the purpose of the trip.

The relevance of these stories to the greenhouse-effect issue is obvious: short-term, immediate issues always take precedence in the public view over long-term, slowly building questions. Indeed, in more despairing moments I often wonder if the combination of short-term electoral politics with the high ratings value of short-term news stories will prove to be an insurmountable obstacle that will virtually banish the possibility of fostering long-term social consciousness and sparking political action on global warming. The situation is not always that bleak, however, for sometimes nature rolls snake eyes, and all of a sudden the strengths of the media become an advantage thanks to a dramatic short-term event. I refer to the Summer of '88. This media event did more for climate consciousness in fifteen weeks than all the "Nightlines," "Novas," *Newsweeks,* I and other scientists were able to achieve in the preceding fifteen years. Whether this was enough to precipitate remedial action remains to be seen. And, overly dramatic coverage of the Summer of '88 and overstrong association with the greenhouse effect inevitably created a backlash.

The hype and hoopla of the Summer of '88, overblown as it sometimes was, inevitably created an angry response from some scientists. Several Op-Ed pieces appeared from scientists, decrying the irresponsibility of the media hypsters and public scientists for misleading the public and politicians about the seriousness

or immediacy of the greenhouse effect. For example, a young statistician at the Woods Hole Institute of Oceanography, Andrew Solow, had an Op-Ed piece in the *New York Times*—"Greenhouse Effects: Hot Air in Lieu of Evidence." The piece was sarcastic in style (certainly not typical of scientific literature). Solow argued that "existing data show no evidence of the greenhouse effect. Many people will be surprised to hear that this is more or less the view expressed in scientific journals, where articles are subject to peer review. Unsubstantiated or misleading statements appear in such journals only when the review process fails. Congressional testimony and interviews in the press are not subject to peer review, and that is how unsubstantiated and misleading statements come to dominate public discussions."[12] I was, frankly, annoyed at that latter remark, since I dislike innuendo and sweeping generalities. I prefer critics to state forthrightly who is the perpetrator of such misleading statements, and who use some quotations to let us judge for ourselves whether they are in fact misleading. Without such specifics this kind of criticism simply sets up a strawman to knock it down. Later on Solow attacked those of us (and I plead guilty to his charge) who argue for policy response to the greenhouse effect: "Some will say that if we wait until we are sure about climate change, it will be too late to do anything about it. This argument applies equally to an invasion of aliens from space. More seriously, this argument neglects the costs of overreaction now." Articles such as this, and others (like the Op-Ed piece by S. Fred Singer that appeared in the *Wall Street Journal* and that of Patrick Michaels in the *Washington Post*)[13] created a substantial wake of media articles that essentially put the greenhouse effect on trial in their headlines.

On the morning of February 21, 1989 two Congressional hearings were held in which I was asked to testify about the latest scientific controversies challenging the existence of the greenhouse effect (the controversies were a direct result of the Op-Ed pieces and the media response to them). I noted, quoting Solow's crack about aliens from space, that "of course, what Solow neglects to make explicit is that waiting for more evidence before acting is his value position. He also fails (which is surprising for a statistician) to suggest anything about the different probabilities between an invasion of aliens from space and rapid future climate change. In my estimation, space invaders are no more likely than the col-

lision of the earth with a large asteroid—which would be a catastrophe! The odds of such a collision are presently assessed at something like one chance in 10,000,000 each year. But the odds of rapid, potentially unprecedented—even catastrophic—climate change are certainly in the first decimal place of probability, and I believe a likelihood of several degrees warming by fifty years is a better-than-even bet. In any case, the cost of overreaction is a legitimate issue but so is the cost of underreaction. The policy process is not advanced when such issues are ridiculed with hollow rhetoric. Instead, scientists should be asked to provide what they are technically competent to offer: estimates of specific consequences of greenhouse gas buildups and their likelihoods of occurrence. Any statements beyond that are the personal opinions of those scientists. Although I believe that scientists, like all citizens, are entitled to opinions on how to deal with those probabilities and consequences, they must always be scrupulous to point out that such opinions are personal value judgments."[14]

The real cause célèbre of the hearings was not the Solow piece but rather a much lengthier and more reasoned critique from Patrick Michaels, who appeared simultaneously with me in front of Indiana Congressman Phil Sharp's House Subcommittee on Energy and Power. In the *Washington Post,* Michaels had charged James Hansen in particular, but other media scientists by implication, with misleading the public on the nature of the greenhouse evidence. In particular, he cited Jerome Namias of the Scripps Institution of Oceanography, "the dean of American climatologists," as "saying there is no way one can scientifically defend any statement linking causation of last summer's drought to the greenhouse effect. Then in the December 23 issue of *Science,* Kevin Trenberth of the National Center for Atmospheric Research and his co-authors convincingly argued that the drought of '88 was caused by warm ocean temperatures in the tropical Pacific, which have since dropped to near record low values." Michaels went on to attack Hansen's June 1988 congressional testimony and cited instead the recent work of Tom Karl, "who arguably knows more about regional climate variations than anyone in the world," to the effect that "there may have been no global warming to speak of during the last century." Pat concluded somewhat heroically: "Karl's findings surprised none of us who merely toil with the data. But it should be a major shock to those who are using those

figures for policy purposes. Is it irresponsible to point this out in public?"

At the February congressional hearings I read out loud some of these quotes and tried to put them in perspective. With regard to the Namias/Trenberth view that the drought was caused by factors other than the greenhouse effect, I cited a recent letter to the *New York Times* from Jim Hansen: "As I testified to the Senate during the 1988 heat wave, the greenhouse effect cannot be blamed for specific drought, but it alters the probabilities. Our climate model, tested by simulations of climate on other planets and past climates on earth, indicates that the greenhouse effect is now becoming large enough to compete with natural variability." It was fun for me then to read a passage from Trenberth and colleagues' *Science* article: "Climate simulations indicate that a doubling of carbon dioxide concentrations could increase the frequency of summer droughts over North America. Thus, the greenhouse effect may tilt the balance such that the conditions for droughts and heat waves are more likely, but it cannot be blamed for an individual drought." That statement is clearly reasonable and, contrary to the impression in the media set off by the Op-Ed pieces, is quite consistent with those of Jim Hansen, myself, and most other scientists that I'm aware of who speak on these issues.[15] "Trial-by-media of the greenhouse effect was thus a nonscientific issue from the very beginning," I told the Congress. "Nevertheless many Op-Ed pieces continue to appear from scientists and others criticizing the 'hysteria' being generated by some (usually unnamed) public scientists on this issue."

Next I turned to Tom Karl and the urban heat island issue by noting that on several occasions Tom had personally told me of his frustration with frequent media phone calls asking him to give the "other side" of the greenhouse effect when in fact he does not represent another side of the greenhouse effect, but is simply trying to maintain quantitative reliability in the world's temperature record. He told Richard Kerr of *Science* magazine in an interview that "the long-term global warming is something on the order of 0.4° C during the past century. Is the bias 0.05° or 0.2°? The chances that it is the same size as the warming are pretty remote. It's a matter of adjusting the rate of rise, not questioning the rise itself."[16] In other words, a man cited so often as the destroyer of the warming trend actually believes in it, and

is simply trying to get the numbers right as any good scientist should do.

Finally, I discussed with the congressmen two rapid-fire articles that had appeared in the *New York Times* and had gathered much attention. Both were written by the well-respected and careful science writer, Philip Shabecoff. The first one reported on a 100-year record of annual temperatures and rainfall in the United States prepared by NOAA scientists, including Tom Karl. Well-balanced, the article quoted the authors as saying this limited record did not imply that the greenhouse effect was a false issue. Nevertheless, the headline (not written by the science writer) did the damage: "U.S. Data Since 1895 Failed To Show Warming Trend." Shabecoff's next article appeared a week later, on February 4, 1989. It was entitled "Global Warmth in '88 Is Found To Set a Record." In it Phil discusses the latest conclusions from Phil Jones and Tom Wigley at the University of East Anglia: that 1988 is the new record warm year, followed in order by 1987, 1983, 1981, 1980, and 1986.[17] While urban heating could perhaps help to explain some of the century-long warming trend, it obviously could no more explain the rapid warming of the 1980s than the greenhouse effect could explain the rapid onset of drought in 1988 compared to 1987.

The congressional representatives could not understand how global warmth could be setting a record when the United States was showing no warming trend. I answered by analogy, showing two slides of the United States on which the states that were won by Democrats in the 1976 and 1984 presidential elections were colored in (I colored in the Democratic states because it was less work). In the 1976 election, in which Jimmy Carter beat Gerald Ford in a close contest, roughly half the states were won by the Democratic victor. It would be foolish, I argued, to try to guess who won the 1976 election by simply picking two states at random. On the other hand, the 1984 election in which Ronald Reagan won by a landslide—with only the District of Columbia and Minnesota going to the Democratic challenger—is a different story. It would be relatively easy to guess the winner of that election by looking at only a limited sample of the country. The relevance of this to global warming, I went on, is that the United States contains less than 2% of the area of the world. Therefore, for a very small climate trend—as with a close election—it is

ludicrous to expect to make large-scale inferences from a very small sample. On the other hand, if the warming trend were 2° C instead of the current 0.5° C, then you would expect—as with a landslide election—that even a small sampling (of states or of the world area represented by the United States) could give you a fairly accurate indication of larger-scale trends. In other words, the larger the climatic signal the less likely that small regions like the United States would be exceptions. That metaphor certainly seemed clear to the congressmen in the room.

I concluded my testimony by pointing out that scientific concern for unprecedented climate change in the next century is not based on the performance of the planet in the past century. Indeed, at the level of global temperature change to date (about 0.5° C), the noise of natural climate variability is simply too large to be able to clearly detect a greenhouse effect signal of sufficient clarity to verify precisely how sensitive the climate has been to increasing greenhouse gases. (But even if not particularly relevant for detecting greenhouse signals, the detailed patterns of climate variability this century are worth careful analysis to determine whether they were due to unaccounted-for influences like human dust or solar activity, or alternatively to some internal manifestation of the complicated dynamics of a chaotic climatic system.)[18] Rather, the case for rapid future climate change is based on the millions of satellite and laboratory observations that validate heat trapping as a theory essentially beyond any reasonable doubt. If we couple that validation with the certainty that global greenhouse gases have increased substantially and are very likely to continue if economic and population growth maintains its expected trends, we see a substantial probability of growing climate change; and in my opinion it is a better-than-even-money bet that large global change is likely. In their Op-Ed pieces, Pat Michaels and Fred Singer also acknowledged that they were not questioning the heat-trapping properties of greenhouse gases, but were focusing instead on uncertainties having to do with the nature of twentieth-century climate variability. I agree that these are legitimate scientific issues and that until they are resolved there will be substantial questions about the detailed nature of climatic change. But these uncertainties do not call into question the relatively high probability for unprecedented climate change into the next century. Neither can the world's thermometers resolve

the greenhouse-signal-detection debate without another ten to twenty years of data. And, I repeated to the Congress, waiting for that extra degree of certainty is not a risk-free proposition, since it could force us to adapt to a much larger dose of change than if we took preventive actions now.

The role of the media in questioning the greenhouse effect because of the largely irrelevant weather phenomena that created the drought of 1988 was, of course, a two-edged sword. On the one hand, because of media attention in 1988, the issue is now firmly in the front of public consciousness. On the other hand, a debate in 1989 that is largely irrelevant to the scientific validity of the greenhouse effect was turned into a trial-by-media of a phenomenon whose existence is proved essentially beyond doubt. My fear is that a cold and wet summer in the next few years, which certainly could occur since even loaded climatic dice have many faces, will be misinterpreted as evidence of the nonexistence of a greenhouse effect.[19]

There is, of course, a generic underlying concern reflected in these remarks: that democracy works only to the extent that the citizenry is informed about the complex nature of the issues their leaders must deal with. To be sure, without the media, people would be much more poorly informed than they are. But if the most popular media largely stick with the four Ds—drama, debate, disaster, and dichotomy—the public will be much more poorly informed than it could be and the policy-making process will be that much poorer.

I do not assign to journalism sole responsibility for the public's general lack of understanding of the many complex issues of the day. This is more a symptom of the problem than a cause. The media executives produce a product that their experience tells them will either sell to the public or attract advertisers. At the root of my worry is the need for all of us, the citizens, to develop an intense interest in how our world works and how we might improve it. We need to be more skeptical about what we hear without being too cynical. We need to learn to question while still recognizing that uncertainty is not automatically a reason for inaction. The problems with most press coverage, I believe, reflect the demands of their audiences for entertainment or brevity more than a conscious conspiracy of journalists to reduce the complexity of modern life to slogans that will fit on bumper

stickers. We need to build curiosity and interest about the future at home, and to teach young people how to think and question in schools. Teachers (or any experts) shouldn't be portrayed as the owners of truth, but rather as guides to the vast fields of knowledge and as models of how to observe, question, and synthesize ideas. If the public is receptive, the media organizations—whose rewards are usually driven by audience sizes—will respond quickly to the tastes of the public, just as politicians respond to the polls they constantly examine.

OZONE DEPLETION. It would be inappropriate to leave the discussion of the Summer of '88 without mentioning another issue that went a long way toward raising public consciousness about global atmospheric problems. In this case, the media played an important and mostly responsible role, partly because the high degree of polarization they reported honestly reflected differing perceptions and values of two rather diametrically opposed debating camps. The problem I refer to is ozone depletion and, more recently, the ozone hole.[20]

Ozone is a trace gas in the atmosphere critical to life on earth. It consists of a molecule of three oxygen atoms (O_3) and is very chemically reactive in the earth's atmosphere. Too much ozone in the lower atmosphere helps to create an eye-burning, lung-damaging, and plant-harming component of urban smog, whereas too little ozone in the stratosphere overhead (16 to 48 kilometers, or 10 to 30 miles) allows extra ultraviolet (UV) radiation from the sun to reach the earth's surface. Ozone has filtered out such UV for perhaps as much as a billon years since the buildup of oxygen in the atmosphere. UV damages DNA molecules and is known to reduce the effectiveness of the immune system and cause skin cancer in humans. Each 1% reduction in the total amount of ozone increases UV reaching the surface by about 2% or more, which, in turn, is thought to increase the risk of skin cancer, particularly in fair skinned people or those who spend considerable time in the sun.[21] UV may also be harmful to ecosystems at many levels in the food chain. Some think that enhanced UV reaching the ocean surface could reduce the productivity of phytoplankton (which make DMS, see Chapters 4 and 5) upon which krill (small shrimplike creatures) feed.[22] These, in turn, are

the food upon which larger creatures such as whales are ultimately dependent.

Pollutants produced by human activities eventually make their way up to the stratosphere, and several of them, including chemicals manufactured by major industries, could be remarkably effective in reducing the ozone shield. Unfortunately, the most direct way to prevent this would be to put those manufacturing enterprises out of business. Thus, ozone depletion in the stratosphere has all of the elements of a good news story: dread consequences, dramatic potential confrontations between environmentalists and industrialists, and dichotomous debates about what is known, what is speculated, and what should be done. Because the ozone-depletion controversy often arises along with the greenhouse-effect issue (for example, both got equal headline space on the October 19, 1987 cover of *Time* magazine) and because the ozone issue can serve as an analogy to climate change with respect to policy making and the media, it seems worthwhile to spend a few pages now briefly reviewing the debate.

Ozone has been known for many decades to exist in the stratosphere. Its occurrence was explained for quite awhile by a simple set of chemical reactions that predicted its existence and its approximate amount at each height in the atmosphere. But the amounts of O_3 this first theory predicted were much too large. Thus, scientists searched for additional natural phenomena that might aid in the destruction of some ozone to bring the simple theory in line with what was known to exist overhead. One idea proposed "catalytic reactions," whereby repeated encounters of ozone molecules with a single molecule of some catalyst could destroy thousands of ozone molecules. Thus, seemingly minuscule amounts of a trace gas could have a substantial impact on ozone. By 1970 it was suggested that hydrogen oxide generated from high-altitude water vapor could be part of such a catalytic cycle. This soon led to a major controversy since proof of this theory would mean that water-vapor emissions from a proposed fleet of high-altitude supersonic transports (SSTs) might pose a threat to the ozone layer. Industrial and environmental chemists began to square off over the plausibility of this hypothesis almost two decades ago. But concern about the potential damage from water-vapor emissions was quickly replaced when Paul Crutzen in Europe and Harold Johnston at Berkeley independently discov-

ered that oxides of nitrogen in the stratosphere can destroy ozone by very active catalytic reactions. These oxides of nitrogen can exist naturally, being produced by the action of cosmic rays hitting molecules of nitrogen in the air or by the injection into the stratosphere of relatively stable, biologically produced gases such as nitrous oxide (common laughing gas). Over a decade or so, this gas percolates slowly through the lower atmosphere to the stratosphere. Some nitrogen oxides are also an inevitable by-product of combustion since high-temperature burning in the presence of air (with 78% of the volume of air occupied by nitrogen) inevitably results in the production of nitrogen oxides. The obvious source for combustion at stratospheric elevations would be a fleet of high-flying SSTs.

In the early 1970s, a major political debate took place about whether the federal government should sponsor the development of an American SST, which would be primarily centered at the Boeing Corporation in Seattle. The debate led to an ironic reversal of the normal ideological polarization. Liberal members of Congress, who typically vote for spending programs that increase employment, generally opposed SST federal development on the environmental grounds that the tailpipe emissions of a large fleet of these aircraft could damage ozone and enhance ultraviolet levels at the earth's surface. Normally conservative senators and representatives, ever mindful of federal budgetary expansion, nonetheless largely supported the venture. Even without environmental considerations the vote would have been close, but most analysts think that the presence of the ozone-depletion complication tipped the balance slightly in favor of the ultimate vote that killed the U.S. project. The Anglo-French Concorde SST project proceeded despite the ozone debate. In fact, many British scientists and industrialists were hostile to U.S. environmentalists for years over this issue.

It is useful to recall the political context in the early 1970s. The United States had been embroiled in a half dozen years of a stalemated and unpopular war that had led to tremendous internal dissension. A war-weary nation built consensus quickly and moved with remarkable speed to take political action on concerns over deteriorating environmental conditions that were poisoning lakes and damaging the health of communities. But understanding the tangible air and water pollution crises that were

visible—and sometimes painful—to the naked eyes of those in polluted areas was one thing; comprehending an invisible, hypothetical threat to the global ozone layer was another, requiring a stunning transition. The media carried the debate and it was well known at the time. However, the publicity surrounding the SST/ozone issue would prove to be merely a warm-up for a much larger controversy: the hypothesized ozone-destroying properties of the chlorofluorocarbons (CFCs), a major group of industrial chemicals used as refrigerants, foam-blowing agents, propellants for sprays cans, and in other applications that required a very inert gas.

In 1974, Mario Molina and F. Sherwood Rowland, two chemists from the University of California at Irvine, made a landmark discovery.[23] They theorized that CFCs, because they are so inert in the lower atmosphere, would not react with other atmospheric chemicals and thus would eventually reach the stratosphere. There they would diffuse above the ozone layer and be subjected to increased levels of the sun's ultraviolet light, which would break them up into their constituent molecules—chlorine, fluorine, and carbon (ergo the name chlorofluorocarbons). The most common CFC is known as Freon, Dupont's trade name for the chemical. Molina and Rowland went further, suggesting that chlorine, for example, would combine with oxygen to form a chlorine-oxide molecule in the stratosphere that could, through catalytic reactions, eat up many thousands of times its own weight in ozone. Their early estimates suggested that industrial production capacity over the next several decades could eventually lead to a destruction of as much as 20% of the ozone layer.

Battle lines were quickly drawn. The industry labeled the theory as speculative without a shred of real-world evidence. Environmentalists countered by saying that to tolerate such a frivolity as spray cans was unthinkable in the face of even the slightest chance that their products could damage the integrity of the global ozone layer. Charges and countercharges were traded in front of congressional committees, on the pages of newspapers, and in the broadcast media. At one stage, some of my colleagues at NCAR reported that a preliminary chemical analysis of stratospheric air suggested some of the reaction products expected in the Rowland-Molina theory hadn't been observed. Industry immediately jumped on the news, publishing full-page ads in the

New York Times and elsewhere calling the industry's sober judgment justified and the environmental concern premature. That this finding was given so much weight was ironic since several weeks later the scientists involved discovered a mistake in their work. Its correction led to measurements that actually supported a CFC-ozone destruction theory.

The media, of course, drank it all in and for awhile in 1974 it was hard to find a program or read a newspaper that did not mention the controversy. The administration, deeply embroiled in the Watergate scandal, wasted little energy defending industry or taking major sides on the issue. Many members of the federal government were thus free to reach their own judgments as to the issue's relative importance and the appropriateness of dramatic regulations that environmentalists were calling for. Despite the fact that the chemistry predicting the amounts of ozone destruction was extremely complicated, rendering any estimate highly tentative at best, the United States nonetheless chose to regulate the industry. In 1976, a Food and Drug Administration official said, "It's a simple case of negligible benefit measured against possible catastrophic risk for both individual citizens and for society. Our course of action seems clear beyond doubt."[24]

Many national and international assessment groups were convened and quite a number of scientists, economists, and others spent weeks crisscrossing the skies (mostly in lower-flying subsonic aircraft) on their way to meetings on the science and policy implications of the CFC-ozone controversy. The National Academy of Sciences convened a committee on the impacts of stratospheric change (CISC). The initial publication of the CISC report was delayed because some of it was leaked to the press, which only served to heighten interest. This in turn made the academy committee, which consisted of thirteen physical scientists, more cautious than ever—they didn't want to be burned by unforeseen scientific developments that could either kill the theory or rapidly accelerate it. As a result, the CISC report (while strongly supporting the hypothesis that CFC emissions could deplete the ozone layer) included a curious and, I believe, inappropriate recommendation *against* the decision to "regulate at this time." Instead, the committee suggested that two more years be devoted to studies aimed at reducing uncertainties in the ozone-depletion theory.

The committee completely disregarded the difficult position

this recommendation would put regulatory agencies in. Regulatory processes can be lengthy, on the order of years to perhaps a decade, so a delay of two years before even starting the process could result in a regulatory delay much longer than even the committee might have expected or approved. Furthermore, the industry immediately cited the prestige of the Academy to imply that there was no need for emission controls, certainly not for the two-year period. Russell Peterson, former Republican governor of Delaware, was then the chairman of the President's Council on Environmental Quality. He complained that the academy committee was out of line in deciding how much uncertainty was enough for policy and thereby assuming the role of policy maker. Instead, he argued, it should have stuck to the scientific issues, leaving the value judgments of whether or not current uncertainties justified regulatory action to those responsible for those judgments—government officials. The CISC's recommendation underscores once again the need for experts to carefully avoid mixing facts and values, an all-too-common pitfall when dealing with complex technical issues.

Nevertheless, by 1977 the Environmental Protection Agency, the Food and Drug Administration, and the Consumer Product and Safety Commission jointly announced U.S. emission controls prohibiting nonessential uses of CFCs in aerosol spray cans. This was the first time a substance suspected of causing global harm had been regulated before the effects had been demonstrated fully. (The earlier ban on atmospheric testing of nuclear weapons was implemented well after widespread contamination from radioactive fallout had occurred and evidence already existed that radioactivity could cause health damage.) Thus, a precedent was set that would prove important in the future for the protection of the ozone layer and that could have major implications as well for global control of other atmospheric gases, such as carbon dioxide.

By late 1982, about twenty countries had taken some form of action to control CFC emissions. Several factors helped make this regulatory action possible: largely proenvironmental public attitudes in the 1970s, strong media attention to the issue, the presence of a strong regulatory authority, limited influence of CFC production on the national economy, and a reasonable availability of substitutes, including hand pumps for many aerosol

applications. But not all countries faced the CFC ban with the enthusiasm the United States showed. The Soviet Union, Great Britain, and Japan were much more skeptical. For example, in 1979 I attended the first World Climate Conference in Geneva. It was a major international event featuring such important issues as the greenhouse effect, drought, soil erosion, and the role of climate change in global development. Sir Basil John Mason, then director-general of the British Meteorological Office and the major British meteorological figure, gave a keynote address. He noted that in the early 1970s initial predictions of stratospheric ozone decrease from supersonic transports were as large as 10% or 20%, but that now, in 1979, improved chemistry in the models showed that a fleet of SSTs—Concordes, which fly at lower altitudes than the hypothesized U.S. SSTs, as he neglected to mention—would, according to the latest chemical calculations, actually *increase* the ozone a couple of percent. "A rather expensive way to maintain the ozone layer," he joked. The atmosphere is resilient, he said; it will make fools out of those who do not understand its complexity. This lesson, which he drew from the SST-ozone controversy, he then applied to the greenhouse effect, suggesting that perhaps nature has some strong negative feedback mechanisms that will make this problem go away too.[25]

I was annoyed. I squirmed in my seat as questioner after questioner was recognized from the podium and the time for questions was frittered away. Finally, with seconds left, I was recognized. "Professor Mason," I said, "I agree that there is much uncertainty in the chemistry of the stratosphere and the climate response to CO_2. Indeed, perhaps our calculations are way off, as you showed for the SSTs. But time probably didn't permit you to show that the very same chemistry that made the SST-ozone depletions rise from an early 1970s estimate of a 10% decrease to a late 1970s estimate of about a 4% increase also made the CFC estimates of ozone depletion from CFC buildup grow from an 8% drop predicted around 1977 to nearly a 20% decrease by 1979 calculations. I agree that uncertainty means that the numbers we project could be wrong. But the sword of uncertainty cuts two ways. The lesson isn't necessarily that the atmosphere is resilient, but that when we modify it with disturbances that are comparable in magnitude to the chemical composition or energy flows in nature we are bound to experience some significant changes."

Happily, Sir Basil and I have since had several cordial interactions. Unfortunately, that is more than could be said for some members of the British entourage, for, as discussed later, some U.K. representatives strongly oppose the notion of environmental regulation of industry before absolute scientific certainty has been established.

By the early 1980s, the CFC-ozone problem faded from public view, probably because the initial regulatory action gave the impression that the problem had been solved. Furthermore, the Reagan administration's antienvironmental stance in its earliest years hardly encouraged regulators or environmentalists to try to expand the chlorofluorocarbon ban beyond spray cans or to pressure the other countries that were doing very little. The loss of American leadership in environmental issues in the early 1980s was noted recently at a *Time* magazine environment conference, where a number of international political, industry, and scientific leaders came together to discuss the revival of the environment as an issue in 1988. At that conference, French environmental official Brice Lalonde remarked, "Through the late 1970s, lots of things we learned about the environment came from the United States. And [in the] late seventies, it stops, and the lead [switched to] Scandinavia, Germany, and the Netherlands." To this Tennessee Democrat Senator Albert Gore quickly responded, "January of 1981, to be precise."[26]

Despite the fading of the U.S. environmental leadership in the early 1980s, there was a small movement toward international action to protect the ozone layer from human disruption. In fact, under the auspices of the United Nations Environment Program, a convention on a global framework for protection of the ozone layer was organized. Still, the wheels of international diplomatic progress turned very slowly relative to the tastes of environmental activists. For endless years, it seemed to them, the chlorofluorocarbon industry had been growing rapidly, emissions had been and still were increasing, and the ozone layer was falling increasingly under threat. Then, in 1985 nature finally stood up and took center stage—with much help from the media. A gaping hole opened up in the ozone layer over Antarctica, providing impetus to what I believe is a major swing of the political pendulum back toward environmental concern. Ironically, publicity associated with the ozone hole arose and the enhanced pressure to regulate

emissions of CFCs was exerted while the Reagan administration was preoccupied with the Iran-contra scandal. Once again, middle-level government officials at regulatory agencies and other places, many of whom had virtually been in hiding during the early years of the administration's antiregulation crusade, reasserted themselves and began working quietly to protect the global atmosphere.

It's ironic that with all the modern satellite technology in the possession of NASA and other U.S. agencies, the opening of a hole in the ozone over the South Pole went undetected for years. It wasn't that the satellites were failing; rather, the scientists who wrote the computer programs that diagnosed vast volumes of satellite data were specifically asking the computers to reject measurements that diverged sharply from normal conditions. In other words, every time a high or low value came in, the computer program rejected it and called it to no one's attention. Incredibly, the phenomenon went undetected by the high-technology branch of science for nearly a decade. Instead, it was discovered by British scientists plotting by hand their own records of how much UV radiation was reaching the earth's surface at their station on the coast of Antarctica.[27] They detected a steady decrease in the southern springtime amount of ozone from the mid-1970s to the mid-1980s, a phenomenon that no one expected and no one could explain. Immediately, U.S. space scientists reprogrammed their data-analysis programs to allow all values, and there in beautiful living colors for all to see (ideal for television) were maps showing a deep hole in the ozone growing in intensity and length over the Antarctic continent and nearby oceans.

Cries for regulation came up again from the environmental community, predictably met by industry denials that the theory had been conclusively demonstrated. Environmentalists claimed that the ozone hole proved they had been right a decade ago, that you don't fool around with Mother Nature or you get nasty surprises. Industry retorted that *it* was right a decade ago when it charged that the atmospheric chemists really hadn't understood what they were doing or they would have been able to predict the ozone hole. In any case, industry argued, the scientists still had no idea whether the hole was caused by CFC buildup or was entirely natural.

In the mid-1980s, a major set of scientific expeditions to the

Antarctic were mounted to find out what was going on. This time, camera crews from all over the world accompanied the scientists, and a press conference led by Boulder NOAA scientist Susan Solomon was even held on the south polar continent. The first year's data were suggestive that chlorofluorocarbons were connected to the ozone hole but were not conclusive. But by 1988 the chemical fingerprints measured in the cold Antarctic stratosphere as the sun rose over that frozen continent clearly showed the presence of chlorine compounds concurrent with a decrease in ozone. The reason the hole occurred there, catching everybody by surprise, was that the stratosphere over Antarctica is nature's coldest spot. There it is so cold that sulfuric and nitric acids exist in the atmosphere in a frozen state as thin clouds, and the ice particles of these clouds are ideal surfaces for the catalytic reactions that destroy ozone. No one had predicted the hole because people were using "homogeneous chemistry" in their calculations—that is, chemistry based on reactions among gases without solid particles in the mix.

Amazingly, even before the CFC-ozone-hole link had been strongly established, a stunning political-environmental achievement was won: in the fall of 1987, the Montreal Protocol on Substances That Deplete the Ozone Layer was approved. This protocol established international regulations on chemicals that can both destroy the stratospheric ozone and exacerbate the greenhouse effect—since CFCs are projected to account for as much as 25% of the greenhouse effect's increase in the next century if they are not controlled. Against these potential dangers, the negotiators had weighed the social, environmental, and economic costs of regulating chemicals upon which the livelihoods of many workers and investors rested. Activities in food processing, plastics, transportation, electronics, cosmetics, fire prevention, and health care would all be affected. The resulting regulations were based on state-of-the-art scientific theory with the acknowledgment that major, let alone certain, evidence of both ozone depletion and ecological or human damages from ozone depletion or climatic change was not in hand. Although existence of the ozone hole had been known and widely publicized prior to and during the Montreal deliberations, no one—including Sherry Rowland, who first pointed out the CFC-ozone connection—was willing to claim that there was any clear connection between the

ozone hole and chlorofluorocarbons. That connection would only be brought out many months after the protocol had been signed. As noted by the chief U.S. negotiator, Ambassador Richard Benedick, "The accord was . . . unique in the astonishing rapidity with which it was achieved, considering the complexities involved: thirteen years from the first scientific hypothesis in 1974, and only nine months of actual diplomatic negotiations in four formal sessions beginning in December 1986."

How did Richard Benedick, a career diplomat in the U.S. State Department, become the mild-mannered hero of the ozone treaty? I first met Ambassador Benedick when we appeared on a joint panel discussing atmospheric change before the members of the fifty-eight universities forming the nonprofit corporation that runs my laboratory. Benedick was just beginning his involvement with environmental treaties when this meeting took place. The primary thrust of his brief comments to the audience was that we scientists should not overstate our knowledge or minimize our uncertainties lest we lose the credibility that is so vital to achieving pollutant-control policy action. I recall thinking that this was yet another Reagan administration official using platitudes about uncertainty as an excuse to delay concrete pollution-control actions. How wrong that snap assessment would ultimately be.

For the next year, Richard Benedick spent considerable time with scientists and industry and environmental advocates, all of whom would influence him greatly. In particular, Robert Watson, an important official in NASA's attempt to understand the stratosphere in general and the ozone hole in particular, had many opportunities to explain to Benedick the science behind the belief of many world-class atmospheric scientists that regulating CFCs was not premature, despite the remaining uncertainties. Others, such as environmentalists Michael Oppenheimer of the Environmental Defense Fund and Rafe Pomerance of the World Resources Institute, also spoke repeatedly with Ambassador Benedick, pointing out some of the potential risks of delaying action. Indeed, at one stage, Watson, Benedick, and a few other Americans traveled to numerous foreign capitals to provide scientific and other information about the issue. Their tireless efforts to discuss the questions with the officials and industry representatives of various countries helped to change many minds about

the importance of pressing on with an international treaty. At one stage, the British government complained to the U.S. State Department that it was encouraging U.S. environmentalists to lobby inappropriately against British foreign policy. Indeed, Ambassador Benedick has provided a thorough and eye-opening account of the complex and politically intricate nature of the negotiation process in a monograph.[28] Here I will briefly touch on some of the highlights of that year of negotiations.

The chief antagonists in the negotiations were the United States and the European Economic Community (EEC). Despite their political alliances and common economic and historical connections, the United States and the EEC, which together accounted for more than 80% of the world's output of CFCs, differed substantially about the issue of regulation. For example, after the United States banned chlorofluorocarbons as spray-can propellants in 1978, the EEC grabbed a much greater share of the market. However, European public opinion cut considerably the growth rate in domestic chlorofluorocarbon consumption in Europe, so EEC producers adjusted by becoming the principal nations to export CFCs to other parts of the world. By 1985, the year before the negotiations began, the EEC countries dominated world production for two principal CFCs, whereas the U.S. production was a third lower than it had been ten years earlier and its share of the market was about half that of the Europeans. The United States resented the Europeans' general refusal to impose the kind of controls we had had in the 1970s. On the other hand, the Europeans, especially the British and French, had not forgotten their anger over the 1970–1971 Concord SST controversy and were suspicious that this was simply a ploy of the U.S.—using an environmental scare to weaken a European industrial market superiority. Furthermore, they suspected that if we were so gung-ho on regulating CFCs, we must have ready substitutes waiting in the wings, which meant we would profit from regulation. So, in Richard Benedick's words, the European industry had as its "primary objective to preserve their expanded markets and to avoid the costs of switching to alternative products for as long as possible." The Europeans thought the Americans "had been panicked into enacting the aerosol ban," continued Benedick, "and therefore had only themselves to blame for any market losses. Indeed, they hoped that delaying agreement on international con-

trols would provoke impatient environmentalists in the United States to demand a second round of unilateral regulations, which could further consolidate Europe's competitive edge."

However, as the negotiations wore on, it became clear that the European block was by no means united. Germany, a principal manufacturer of CFCs, had a strong domestic environmental constituency and was not nearly as implacably opposed to regulations as Britain or France. Furthermore, Belgium, the Netherlands, and the Scandinavian countries had already been leaning toward strong environmental control. Greece, Spain, Ireland, and Portugal, on the other hand, virtually ignored the entire proceeding. Europeans have long tried to smooth over historical animosities, and the European common market has gone a long way toward that goal. Therefore, the EEC members agreed to achieve internal consensus before and during negotiations, which, as Ambassador Benedick put it, "tended to make it a difficult and inflexible negotiating partner." The central executive of the EEC demanded to be the only spokesperson for all members at formal negotiations but was frustrated when individual European nations ignored this idea.

Bitter behind-the-scenes fights were apparent to more than the EEC negotiators. The "Council of Ministers"—that is, the environmental ministers of the EEC countries who had the authority to determine a negotiating position—only met (and still only meet) twice a year. Therefore, there were times when EEC negotiators, while waiting for the ministers to meet in their next scheduled get-together, simply could not negotiate directly. This situation, along with the demand of EEC unanimity, appeared for a time to be an insurmountable obstacle to rapid treaty approval. By chance, the EEC presidency, which automatically changes hands every half year from one member country to another, proved to be one of the most important steps leading to the eventual reversal of the EEC hard line on the chlorofluorocarbon regulations. A British delegate had been president up until January 1987, when a Belgian replaced the British official, at which point the past, present, and future presidents all met together in closed-door meetings to determine the EEC position. But when the presidency revolved again in July, the British had rotated out of this inner circle, which now consisted of representatives from Belgium, Denmark, and Germany—the EEC countries that favored

stringent controls. As Richard Benedick noted, "It is interesting to speculate how much this serendipitous constellation, in the right place at the right time, influenced the final EEC acceptance in Montreal of considerably stronger measures than it had originally favored." In the final chapter of the protocol, which called for 50% reductions in CFC production by the turn of the century, most EEC countries, including Germany, France, Italy, the Netherlands, Belgium, and Denmark, sent high-level representatives to sign the treaty. There was one exception: the United Kingdom, whose representative was a midlevel environmental official.

The impression so far is that the United States led the fight for strong regulations with little or no internal dissent. That would be a misapprehension, for there was indeed substantial internal administration opposition, reportedly from the departments of commerce and interior, to any international regulations dealing with environmental questions marked by large uncertainty. But by the time these forces became organized, the publicity surrounding progress at Montreal had reached Congress and the public. According to Richard Benedick, secret orders from President Reagan to continue the process and see it completed overwhelmed rear-guard action from some administration conservatives. This action was consistent with the substantial reversal in antienvironmental attitudes of the Reagan administration in its final few years, a trend that I hope will grow in the Bush administration.

Even though the United States had signed the treaty and was the principal protagonist in its eventual adoption, almost immediately after the 1987 Antarctic expedition, there was substantial complaint from the environmental community that a 50% CFC ban simply would not be enough if the ozone hole was to be checked. The treaty does permit a reexamination of the appropriateness of the ban, with the possibility of further strengthening it, depending upon new scientific information. Indeed, disturbing new scientific information surfaced in the months following the Montreal signing. Antarctic ozone expeditions had led to a widespread consensus that the ozone hole was caused by chlorofluorocarbons. Furthermore, a number of estimates have suggested that we need at least a 90% ban—a percentage originally proposed by U.S. environmentalists but rejected by the

other countries participating in the protocol negotiations as too radical—if we are to prevent the ozone hole from expanding further. Even more disturbing was the conclusion of a U.S.-led scientific committee that not only was a springtime ozone hole in Antarctica proof of the seriousness of chlorofluorocarbon buildup, but also that the best calculation of *global* ozone trends suggested a depletion of several percent in the past decade.[29] When taken together with the growing consensus that the ozone hole is a creation of CFC buildup, this finding will result in substantial pressure on most nations of the world to ratify the present Montreal Protocol, and will substantially strengthen the protocol's regulatory grip on these potentially dangerous chemical emissions. (October 1988 saw some amelioration in the ozone hole, but this apparently was caused by a periodic atmospheric variation that kept the stratosphere over Antarctica relatively warm and thus not as packed with clouds as in 1986 or 1987 or 1989.)

Does this Montreal process, whose complexity has been only briefly outlined here, lend any hope for a global treaty on emissions of greenhouse gases, especially CO_2? Opinions are divided on this issue, with skeptics pointing out that half the chlorofluorocarbon uses are frivolous—in spray cans or foam cups, for example. Moreover, substitutes should be readily available for these applications—and indeed it is rumored that Dupont is about to manufacture some. Nevertheless, it may not be that easy to get an agreement for a total phase-out of CFCs on a global basis, despite the fact that this single action would substantially reduce two global atmospheric threats, ozone depletion and global warming. For example, at the *Time* magazine meeting mentioned earlier, when World Resources Institute president Gus Speth and U.S. Senator Albert Gore suggested a phase-out of chlorofluorocarbons by 95% at the end of the century, the Soviet representative, somewhat reluctantly, added a cautionary note: "I agree completely with Senator Gore that CFC production must be banned," said Vladimir Sakharov, "but I'm afraid that for us that it's not feasible in the near future. I'm afraid very much that it would be extremely difficult, if not impossible, to go further than the Montreal Protocol in the near future. I wouldn't specify what does it mean in this case 'near future,' but five years."

"Why?" Gore asked.

"Because for us it's not only a political question but an eco-

nomic question. Our economy is not as flexible as yours . . . and when we discuss in our country the Montreal Protocol, we had very hard times with the industry, with the chemical people."

Gore asked if the initial Soviet resistance to the Montreal Protocol was due to the Soviets' desire to build semiconductors, to which Sakharov replied, "like practically in every country, spray cans, refrigeration and semiconductors as well. And, you know, we have to change mentality in many things. Even in this particular building [NCAR] where we are now, downstairs they are using CFCs for cooling of semiconductors [he was referring to NCAR's supercomputer] and it is done by people who know very well the dangers of it."

Gore acknowledged that the resistance to further bans was legitimate, "but that doesn't mean it shouldn't be proposed and pushed. And maybe with *glasnost* it will be a groundswell of support," Gore said.

"Absolutely. Because, me, myself, I am an environmentalist," said Vladimir Sakharov, "so I'm quite on your side. And we're going to push our industry." Sakharov went on to say that if the Soviets were to achieve environmental improvement, they would need to replace their aging technology with new technologies, presumably from the West.

Even though a phase-out of CFCs would be the single biggest step toward stabilizing the global atmosphere, this exchange shows that it may not be simple to achieve, despite the desire on all sides and excellent media coverage of the event—after all, this meeting itself was held by a major media representative, *Time* magazine, which made the transcript just quoted.

Skeptics would argue that substitutes for fossil fuel burning are even more difficult to introduce than those for CFCs, and that a "law of the atmosphere" will be much tougher to achieve. Furthermore, the pessimists contend, the consequences of ozone depletion are clearly dangerous and seem uniformly negative—that is, serious, soon, and certain—, whereas the climate changes associated with the greenhouse effect cannot as easily be proved all bad.

But Ambassador Benedick is less pessimistic. He feels that many lessons from Montreal are immediately applicable to the process of developing a law of the atmosphere. He cites four important components. First, it is important to build scientific consensus

and to impress this consensus on government policy makers through close collaboration in which each side comes to understand the working context of the other. This collaboration, he asserts, "contributed to the irresistible logic of the American position on ozone, and greatly strengthened the persuasiveness of U.S. negotiators; in contrast, the European commission based its tactics largely on self-serving contentions and data provided by industry, which, ultimately, proved less convincing in the international arena" than the less biased materials provided by independent scientists. Second, the political will of nations can only follow if public opinion is adequately informed. The role of individual scientists at national academies in building a credible case and in helping to translate and disseminate information through the media is critical. International organizations such as the United Nations Environment Program (UNEP) have been engaged in major educational efforts. UNEP's director, Moustafa Tolba, and the head of its New York office, Noel Brown, were generous in their efforts to promote the issue. Similar efforts will be needed if greenhouse gas emissions are to become publicly familiar, Benedick believes. (Of course, no public education program can easily make climate change appear as obvious and dangerous as skin cancer, as the pessimists have pointed out.) Third, the success in the ozone protocol suggests that a treaty on the greenhouse effect may require a multiple-step process. Initially, a framework convention would be called, primarily as an expression of various nations' commitment to act. That would be administratively (not necessarily politically) easy, for a Vienna Convention had already been signed. It set up the framework for the subsequent Montreal Protocol. Konrad von Moltke, of the Conservation Foundation, has suggested that all we need to do is add the words "greenhouse gases" after the phrases in the Vienna convention on controlling ozone-depleting substances. This would be much easier than making any new conventions, he has argued.[30] This updated convention would then be followed by individual protocols with specific actions.

All along, international scientific assessments of the highest credibility would be needed. However, Benedick cautions, climate change, unlike ozone, has many aspects—energy production and demand, population, deforestation, agriculture, coastal zones, biological diversity, and so on—that he agrees with the pessimists

that it will be difficult to address them all in a protocol treaty. It is for this reason that he recommends a step-by-step approach.

Finally, the most important component of the Montreal Protocol is its dynamic and flexible capacity to modify itself as new scientific, economic, or technical assessments are made. The treaty has provisions both for calling emergency meetings in the case of fast-breaking developments and for differentiating the regulatory impact on developed and developing countries. In short, the treaty's sensitivity to special situations ultimately brought about the compromise necessary to get it approved. The media, on balance, did a fine job of carrying the technical ozone debate to the public, but they have spent much less ink or air time on these complicated diplomatic maneuverings.

The greenhouse effect and ozone depletion teach the same lesson: we cannot continue to use the atmosphere as a sewer without expecting substantial and potentially irreversible global environmental disruption. The problem scientists face in motivating people to make immediate economic investments to minimize this disruption involves the tough tradeoff between the need to give clear and dramatic statements versus the ethical requirement to tell the whole story, complete with both uncertainties and caveats. This is the double ethical bind I described in the preface. On September 7, 1988, with the Summer of '88 still fully in American consciousness, the ABC news program "Nightline" broadcast a segment dedicated to the greenhouse effect. I was contacted as a possible guest but was later told my views were "too moderate." Some of the exchange between "Nightline" moderator Ted Koppel and the environmental activist Michael Oppenheimer, of the Environmental Defense Fund, helps to make this dilemma quite explicit:

KOPPEL: Dr. Oppenheimer, I'd love to be able to say to you that I think the American public can get energized over some perceived threat forty years down the road, but I don't believe it. Do you?

OPPENHEIMER: Well, I think that they can. This summer has provided us with a vivid example of the kinds of changes that are in store for us if we don't move to limit the greenhouse gases—record heat, record drought, record smog levels, forest fires

putting our forests up in flames. It doesn't matter whether the summer was due to the greenhouse effect, and in fact, we'll never know whether it was or wasn't. . . .

KOPPEL: Well, forgive me, but I think it does matter. If—if it was not caused, if it is simply a coincidence . . . that's one thing. If it is the consequence of the greenhouse effect and you can draw some kind of correlation there, then presumably we can present evidence to the American public and say, "Look, it may happen in forty years, it may happen in sixty years, but folks, there's disaster that far down the road and there are some really bad things that are going to happen ten years from now, or five years from now."

Koppel then introduced Alan Hecht, Director of the National Climate Program office, who has long been concerned with issues of climate change. However, true to the need for dichotomy, Hecht was brought on as a "balance" to Oppenheimer and indeed pointed out substantially more uncertainties than Oppenheimer had. Despite that, a few minutes later in the program, Koppel came back to the magnitude of the risk.

KOPPEL: Dr. Oppenheimer . . . I mean—there are not many opportunities when someone with your background and expertise, or Dr. Hecht, has a chance to talk to several million Americans at the same time and say, "Hey, dummies, wake up. What we are doing here is causing a problem." Now you can either say it in such a way that people sort of doze off while you're saying it, or you can say it in such a way as to convey a sense of alarm.

Even the normally outspoken Mike Oppenheimer was unable (to his credit) to overcome his scientist's instinct and take Koppel's invitation as an opportunity to deliver a "certain disaster if we don't act" speech. He led off his reply with a caveat.

This exchange once again reflects the difficulties scientists have in trying to communicate the seriousness they feel about rapidly increasing buildups of atmospheric pollution. There is a tension between the scientific culture of caution and reticence and the media's penchant for the drama, dread, and debate that keeps

the show lively and the audience tuned in. I often close public lectures with a slide of a license plate from Missouri displaying the slogan "Show-Me State." While this is certainly a practical piece of folk wisdom, in applying it to environmental matters we will be forced to experiment with our own planet if we must prove potential threats beyond a reasonable doubt before we take action. To me, a rational society anticipates problems, just as a rational person not shackled with poverty buys insurance. How much insurance is appropriate for any person, enterprise, nation, or world is itself a difficult question, one that is addressed in the next chapter. In this discussion of the media's role in dealing with hypothetical or long-term threats, I want to end by suggesting that there is a growing mismatch between the complex nature of reality and the way such problems are usually reported in the popular media or perceived by the public. Self-governance demands an informed public and a knowledgeable political leadership. If information is distorted by an overemphasis of extreme opposing views, then the policy-making process will not be rooted in the level of understanding appropriate to the reality of the issues. Scientists must learn to communicate clearly without fear of using familiar language and metaphors. (At a minimum, I wish they would be less critical of those who do!) For example, when a scientist or statistician suggests there is "no evidence" of a greenhouse gas-induced signal in twentieth century temperature records because of his implicit assumption that "evidence" begins only after 95% or 99% statistical significance is established, this is not a responsible public statement. In my opinion, few nonstatisticians can be expected to know the assumptions professionals implicitly make. Any specialist making public comments is obliged to use language familiar to the audience and to anticipate their level of technical comprehension before making pronouncements, particularly on controversial issues or those with public policy overtones. The media, on the other hand, must come to report the spectrum of scientific opinions without concentrating on the most contentious and extreme opposing views, as interesting as these may be. Mainstream views may not be as exciting, sell as many newspapers, or increase advertising revenue on television, but if the media are to carry out their essential role as the independent guardians of truth in a democracy, then they need to give us more of the *whole* truth, not simply a selection of the most dramatic or visually fascinating bits.

CHAPTER 7

1. Subcommittee on the Environment and the Atmosphere of the Committee on Science and Technology, U.S. House of Representatives, 94th Congress, 2nd sess., 1976, vol. 78. *The National Climate Program Act,* Hearings, May 18–20, 25–27, 1976 (Washington, D.C.: U.S. Government Printing Office).
2. H. A. Ingram, H. J. Cortner, and M. K. Landy, "The Political Agenda," chap. 18 in *Climate Change,* Waggoner.
3. My schedule conflict was a previous agreement to discuss and debate the strategic defense initiative and "nuclear winter" with the controversial physicist Edward Teller in front of a group of students in Flagstaff, Arizona. Interestingly, when Teller arrived and the organizers joined us for lunch, I found it difficult to engage him in discussions about "star wars" or any other defense issues because all he wanted to talk to me about was the greenhouse effect and how to deal with it.
4. J. E. Hansen, "The Greenhouse Effect: Impacts on Current Global Temperature and Regional Heat Waves," prepared statement (U.S. Congress, Senate, Committee on Energy and Natural Resources, *The Greenhouse Effect and Global Climate Change,* Hearing, June 23, 1988, 100th Cong., 1st sess., 1988, pt. 2, 42–49).
5. Indeed, if I may indulge in a fond recollection, Jim helped me in a very early and controversial paper I published with my institute mentor, S. I. Rasool. Jim suggested an effective yet accurate and simple method for making a radiative-transfer calculation, which I then used with Rasool—a method originally brought to widespread scientific attention by Carl Sagan and James Pollack (C. Sagan and J. B. Pollack, "Anisotrophic Nonconservative Scattering and the Clouds of Venus," *Journal of Geophysical Research* 72 [1967]: 469).
6. The record that Hansen showed to Congress can be found in Hansen and Lebedeff, "Global Surface Air Temperatures." The concern about the correlation of global temperature anomalies over many years is discussed in T. R. Karl, "Multi-year Fluctuations of Temperature and Precipitation: The Gray Area of Climate Change," *Climatic Change* 12 (1988): 179–98.
7. Karl and Jones, "Urban Bias."
8. W. Sullivan, "Goals for U.S. Urged on Weather Control," *New York Times* (December 29, 1972): 50.
9. S. H. Schneider, "What Makes a Good Science Story?" panel discussion, in *Scientists and Journalists: Reporting Science as News,* eds. S. M. Friedman, S. Dunwoody, and C. L. Rogers (New York: Free Press, 1986), 103–15.
10. One good story that appeared was by A. H. Malcolm ("In Ashes of Burned Forests, a Rare Chance to Study Nature's Recovery," *New York Times* [September 27, 1988]: C-1).
11. Klinger, "Successional Change."
12. A. Solow, "Pseudo Scientific Hot Air; The Data on Climate Are Inconclusive," *New York Times* (December 28, 1988): A-15. In connection with the problem of scientists with narrow perspectives who communicate to broad audiences, physicist and policy analyst John Ahearne commented: "When an expert does not communicate effectively, it usually stems from inability or unwillingness. Failure to communicate well also can be connected, however, with an overestimation or overvaluation of one's own expertise. People who are well informed about science and engineering in general but not about the specific policy questions in dispute should not be called experts, but often they believe they are experts. In contrast to those who are aware of all the complexities bearing on the issues at hand, the less informed often take a paternalistic or maternalistic attitude toward the general public. Sometimes they express the belief that controversy would disappear if only the public were better educated—if only, that is, the public became as well informed as they believe themselves to be. Sometimes they act as though the solution to conflict is simply for the public to trust them and what they claim." (J. F. Ahearne, "Addressing Public Concerns in Science," *Physics Today* [September 1988]): 36–42.)
13. S. Singer, "Fact and Fancy on Greenhouse Earth," *Wall Street Journal* (August 30, 1988); P. Michaels, "The Greenhouse Climate of Fear," *Washington Post* (January 8, 1989).

14. S. H. Schneider, "Global Warming: Scientific Reality or Political Hype?" (U.S. Congress, House, Committee on Energy and Commerce, Subcommittee on Energy and Power, Hearing, February 21, 1989, *Global Warming,* 101st Cong., 1st sess., in press).
15. J. Hansen, letter to *New York Times* (January 11, 1989); Trenberth, Branstator, and Arkin, "Origins."
16. Kerr, "Global Warming Is Real."
17. P. Shabecoff, "Global Warmth in '88 Is Found To Set a Record," *New York Times* (February 4, 1989): 1. Phil Jones told me by letter that Shabecoff did indeed correctly report Jones and Wigley's latest results.
18. Schneider, "Science and Policy." See also endnote 17, chapter 4.
19. S. H. Schneider, "The Greenhouse Effect and the U.S. Summer of 1988: Cause and Effect or a Media Event?—An Editorial," *Climatic Change* 13 (1988): 113–16.
20. For an excellent account of the early phase of the public and private ozone debate (before the ozone hole was discovered), see L. Dotto and H. Schiff, *The Ozone War* (Garden City, N.Y.: Doubleday, 1978).
21. For a recent survey of atmospheric chemistry, see McElroy and Salawitch, "Changing Composition." For a discussion of health and environmental effects see J. G. Titus, ed., *Effects of Changes in Stratospheric Ozone and Global Climate, Volume I: Overview* (Washington, D.C.: United Nations Environment Programme and U.S. Environmental Protection Agency, 1986).
22. R. C. Worrest, "The Effect of Solar UV-B Radiation on Aquatic Systems: An Overview," in *Effects of Changes,* Titus, 175–91.
23. M. J. Molina and F. S. Rowland, "Stratospheric Sink for Chlorofluoromethanes: Chlorine Atom Catalysed Destruction of Ozone," *Nature* 249 (1974): 810–12.
24. Dotto and Schiff, *Ozone War.*
25. Sir B. J. Mason, in *Proceedings of the World Climate Conference,* World Climate Conference (Geneva: World Meteorological Organization, 1979); Sir B. J. Mason, "Has the Weather Gone Mad?" *New Republic Magazine* (July 30, 1977): 23.
26. This *Time* magazine conference resulted in the 1988 *Time* cover story in which the earth was named "Planet of the Year" (*Time,* January 2, 1989). A transcript made by *Time* of the conference's climate change working group is the source of a number of quotes throughout this book.
27. J. C. Farman, B. G. Gardiner, and J. D. Shanklin, "Large Losses of Total Ozone in Antarctica Reveal Seasonal ClO_x/NO_x Interaction," *Nature* 315 (1985): 207–10.
28. This quote from Richard Benedick and the many others that appear in the remainder of this chapter were taken from a manuscript he prepared for the Conservation Foundation on the history of the problem: R. Benedick, *Ozone Protocol: A New Global Diplomacy* (Washington, D.C.: Georgetown University Institute for the Study of Diplomacy and The Conservation Foundation, in press).
29. R. Kerr, "Stratospheric Ozone Is Decreasing," *Science* 239 (1988): 1489–91; K. E. Trenberth, "Executive Summary of the Ozone Trends Panel Report," *Environment* 30 (July/August 1988): 25–26.
30. K. von Moltke, "International Agreement to Stabilize Climate: Lessons from the Montreal Protocol—An Editorial," *Climatic Change* (in press).