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Global Warming

Are We Entering the Greenhouse Century?

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Epilogue

The Global Warming Debate Heats Up

Since the first appearance of *Global Warming* in September 1989 there has been vociferous debate among scientists, economists, industrialists, and environmentalists about how serious the global warming issue is. This sometimes acrimonious exchange has spread confusion among the public and politicians alike about the credibility of greenhouse effect warnings and has, at least temporarily, delayed action on the problem. Therefore, I think it is worthwhile to summarize briefly what has happened since the initial publication of *Global Warming,* and why the ensuing debate hasn't changed the fundamental conclusions of the original edition. Indeed, while many scientific questions remain open to dispute, some of the bitter criticisms denying the immediacy of the problem are becoming recognized for what they are: attempts by special interest groups or ideologues to delay global actions that might adversely affect them.

Eighteen years ago, when I first began to address the public policy implications of still uncertain climatic forecasts, a controversial and well-known scientist took me aside: "You'll be able to judge the magnitude of your impact on society," he warned, "by the position and vehemence of your critics." Getting people's attention is one thing, however; getting action on public policy is another.

Critics maintain four principal objections to the likelihood of global warming. First, the scientific basis for projecting future climate change is so uncertain that no responsible scientists would dare propose immediate policy responses. Second, those who suggest that a hundred years or more of unprecedented climate change (what I have called the Greenhouse Century) is being built into the future are just "environmental activists" whose ideological agenda aims to destroy the free market system. On the other hand, those who argue that there are unlikely to be any significant effects are, by contrast, thoughtful senior scientists protecting the public's interests. Third, decade-to-decade temperature changes over the past 100 years aren't consistent with climate model predictions of the effects of increasing greenhouse gases; thus the model projections are probably exaggerated. And fourth, it's too expensive to do anything about global warming anyway. The only thing to do now is wait and see what happens—after all, the changes probably won't be great and may be beneficial even if they occur. So say the most strident critics.

The debate became so intense that in 1990 the U.S. National Academy of Sciences invited the principal scientific opponents of activism on global warming to debate the scientific "establishment" that has consistently reaffirmed its confidence in its own 1.5°C to 4.5°C warming estimates for the middle of the next century. President Bush's Chief of Staff, John Sununu, cited these oppositionist critics to justify a go-slow approach to joining international efforts to regulate emissions of atmospheric pollutants that have the potential to cause unprecedented climate change in the twenty-first century. As long as the U.S. refuses to limit its emissions, Great Britain, France, Canada, Japan, and other nations are also unlikely to act. Thus, the U.S. refusal to agree to specific curbs on some of its emissions is having a major impact on the shape of the Greenhouse Century.

The debate has strayed far from reason and civility on occasion. *Forbes* magazine, for example, placed an ad on the back page of the *New York Times* (February 7, 1990), praising itself for courageous journalism in debunking global warming as "Hype Not Heat" and belittling the issue with the headline, "No Guts, No Story." "Global warming effects," *Forbes* said, "would be at worst minimal." Editorial cartoons in support of warming controls

advocates escalated the media circus. One showed Sununu as a devil whispering in the President's right ear, "To hell with the future, let's go for short-term profit." Bush is flicking an angel, William Reilly, the Environmental Protection Agency administrator, off his left ear as Reilly reminds him of his campaign promise to be the "Environmental President." When debate degenerates to such inanity, it's no wonder the public and most politicians are confused about what the real problem is, let alone what to do about it.

The irony is that very little has changed scientifically during the past several years. Nevertheless, when new technical data surfaces that apparently reduces the magnitude of the problem, it is welcomed by global warming opponents as a major finding. New data that reinforces global warming concerns is ignored by the critics, but promoted by environmentalists. Sides are taken without putting new information in perspective, with the result that confusion grows. Unfortunately, while such a polarized debate makes entertaining op-ed page reading and grabs ratings on TV, it clarifies little of the real scientific controversy or of the broad consensus on basic issues within the scientific community.

Typical projections of global warming possibilities (see Figure 7, page 105) into the twenty-first century have been drawn by a group of scientists convened by the International Council of Scientific Unions. They show warming from a moderate half degree Celsius (.9°F) up to a catastrophic 5°C (9°F) or greater before the end of the next century. I do not hesitate to call the latter figure catastrophic because it is the magnitude of warming that occurred between about 15,000 and 5,000 years ago: from the end of the last ice age to our present interglacial epoch. It took nature some 5,000 to 10,000 years to accomplish that warming, which was accompanied by an approximately 100-metre (330-foot) rise in sea level, long distance migration of forest species, radically altered habitats, extinction and evolution of species, and other major environmental changes.

Critics of immediate policy responses to global warming are quick to point out the uncertainties that could reduce the average projections of climate models (such as the middle line on Figure 7). Indeed, most climate modellers include similar caveats in their papers. Many critics, including the authors of a report

for the Marshall Institute[1] (a Washington-based think tank best known for its advocacy of President Reagan's "Star Wars" Strategic Defense Initiative), hardly mention that the sword of uncertainty has two edges: that is, the same inexactitude in physical or biological processes that makes it possible for the present generation of models to overestimate future warming effects is just as likely to cause the models to underestimate change. [I wrote a letter putting this Marshall Institute report in perspective at the request of Alan Hecht, a deputy administrator in the U.S. Environmental Protection Agency. Since that letter has already been widely circulated (after someone obtained it under the U.S. Freedom of Information Act), I reproduce it here in the notes.][2]

The public policy dilemma is how to respond in the absence of conclusive evidence of the effects of global warming. It is my opinion that the scientific community will not be able to provide definitive information over the next decade or so about the precise timing and magnitude of century-long climate changes, especially if research efforts remain at current levels. Policy makers must decide how much information is "enough" to act on and what measures to take to deal with the plausible range of environmental changes. Unfortunately, the probability of such changes cannot be precisely estimated by analytical methods. Rather, we must rely on the intuition of experts, which is why obfuscating media debate impedes policy development.

Fortunately, making intuitive scientific judgments is the purpose of such deliberative bodies as the National Research Council of the U.S. National Academy of Sciences (NAS) and the International Council of Scientific Unions. NAS, for example, regularly convenes a range of experts to estimate the probabilities of various scenarios of change. The deliberations of these panels are removed from the cacophony of media debates that typically highlight only the extreme opposite positions. Half a dozen such assessments[3] over the past ten years have all reaffirmed the plausibility of unprecedented climate change building into the next fifty to a hundred years. In 1990 a United Nations-sponsored group of several hundred international scientists, the Intergovernmental Panel on Climate Change (IPCC), also reaffirmed that plausibility.[4]

I've mentioned that the National Academy of Sciences has regularly produced studies from a broad cross-section of the scien-

tific community of possible climate changes from greenhouse gas build-up. Since these assessments have been used by both politicians and conservationists to justify serious consideration of policy actions, some critics have tried to discredit the National Research Council (NRC), which compiles such assessments for NAS.

One such critic, Richard Lindzen, a meteorologist at the Massachusetts Institute of Technology, is himself a member of the National Academy. Nearly 20 years ago Lindzen criticized the Department of Transportation's Climatic Impact Assessment Program to evaluate the environmental impact of high-flying supersonic transports (SSTs). In 1989 he went so far as to try to discredit the Academy consensus on global warming by impugning the credentials of its committees:

> The National Academy consists of about 1,500 members who are elected on the basis of their scientific accomplishments. Election to the Academy is considered a high honour. When, however, the Academy responds to inquiry, it does so through committees of the National Research Council. Oddly enough, there's only a minuscule representation of Academy members on these committees (approximately 6%). Thus, the connection of so-called National Academy Reports to the Academy is itself tenuous and uncertain. In principle, these reports are reviewed by academicians, but the selection of these reviewers is fairly arbitrarily handled by the NRC staff and the presidents of the Academies of Science and Engineering and the Institute of Medicine.[5]

In other words, to Lindzen the Academy committees are suspect because many of their members have not met the standards for election to the Academy—high academic standards to be sure. But Academy membership is largely based on disciplinary specialization and outstanding contributions in a narrow field, rather than on an individual's capacity for multidisciplinary synthesis. But the latter quality is also essential for integrative assessments of complex subjects such as climate change. That is one reason why the Academy casts its net widely in selecting committee members.

Lindzen went on to suggest that his intuitive understanding of how the atmosphere worked led him to believe that its "response to doubling of carbon dioxide may readily be ⅛ to ¼ — or even less—of what is suggested" by the National Research

Council consensus of 1.5°C to 4.5°C warming if CO_2 doubled. Lindzen based this "⅛ to ¼" statement upon his intuitive scientific judgment and offered no calculations in the peer-reviewed literature to back it up. Nevertheless, opinion page articles proliferated following Lindzen's statements, and my telephone was busy with reporters and others seeking my response to his assertions that global warming was a vastly overblown environmental scare.

Finally, the December 25, 1989 issue of *Forbes* magazine featured an article whose scientific objectivity was emblazoned colourfully on the cover: "Global Warming Panic: A Classical Case of Overreaction."[6] Excerpts appeared in newspaper opinion pages for weeks following. The piece was by Warren Brookes, an economic journalist who combines sharply worded anti-global warming scientific and policy opinions with *ad hominem* attacks. It's not surprising that such stuff is written, but it is surprising to me that it appears on the opinion pages of respectable newspapers without any attempt to put this nonscientist's scientific arguments in the perspective of the broad consensus. What newspapers tend to do for "balance" is print the contrary views of comparably extreme advocates of radical environmental policies or *laissez-faire* economics. The result is increased confusion and further loss of objectivity and perspective, as I observed in Chapter 7 on "Mediarology."

What Brookes does is to cite the many uncertainties surrounding global warming projections—usually attributing the caveats to the critics, whose impeccable credentials he flaunts. Then he selectively quotes from "environmental activists" (like Jim Hansen or me). But more important than its *ad hominem* attacks, the *Forbes* article does not indicate that most of the scientific uncertainties mentioned are as likely to make our estimates for the future worse as they are to make it better. Unknown phenomena are as likely to cause increases as decreases in current estimates of future changes. Furthermore, Brookes does not refer to the substantial amount of information that validates climate models, including their capacity to reproduce the large seasonal cycle of surface temperature and to reproduce many significantly different climates from glaciological history. He overlooks the fact that ice ages and interglacial warm periods have seen 5°C (9°F) temperature changes marked by 25% increases in CO_2 at the warm times

and 25% decreases in the cold times (see Figure 2, page 40). These strong circumstantial pieces of evidence are what motivate me, and most other scientists involved in National Research Council or IPCC studies, to believe there is a substantial probability of unprecedented warming building into the next century.

Brookes, along with other critics, also points out the lack of significant warming in the lower forty-eight United States during the past century. "That news alone," Brookes asserts, "should have cooled off the global warming movement."[7] What he and others who raise this issue ignore is that while the U.S. warmed in the west and cooled in the east, if Alaska were included in their estimates (which it was not), the U.S. as a whole would have warmed up more than 0.3°C, close to the global average. What the data actually shows is that natural climate variability is significant on a regional basis. For example, had one chosen to observe north central Asia, one would have noticed a 40-year warming trend vastly in excess of the rest of the globe. It is as deceptive to suggest that the Asian trend proves the world is warming up faster than the models project, as it is to allege that the absence of an appreciable temperature trend in the lower forty-eight United States proves that the greenhouse effect does not apply to the world—or even to the U.S. This kind of scientific silliness couldn't survive in serious debate, yet it is reprinted in news stories and opinion pieces.

As mentioned before, because of public doubt aroused by the *Forbes* story and other commentaries, the National Academy of Sciences and National Academy of Engineering organized debates to inform their most recently convened panel of the critics' views. Lindzen and others were present at the first debate, along with the "establishment scientists": James Hansen, Director of NASA's Goddard Institute for Space Studies; V. Ramanathan, a climate expert from the University of Chicago; and Jerry Mahlman, Director of the National Oceanic and Atmospheric Administration's Geophysical Fluid Dynamics Laboratory in Princeton. Also attending was my NCAR colleague, Kevin Trenberth, frequently cited as a critic of global temperature trend data.

Trenberth started the discussion, showing how difficult it is to estimate the world's average surface temperature. He noted that ocean temperatures inferred from ships' records in the pre-1900s period were problematic, since they were obtained from

thermometers placed in buckets dropped over the side of the boat. Some buckets were made of leather, some of wood; some measurements were taken on the windward side, some to the leeward side of the ship, all of which would affect the readings. He also showed that the fraction of the oceans covered by ship tracks before 1900 was 10% to 20% at best. One of the Academy panel members asked him if he felt there was any utility at all in the ocean temperature data before 1900 for the purpose of global trend analysis. "Not much," he said. In fact, he commented, the corrections due to faulty measurement techniques are typically larger than the inferred climatic trends, which is why no one pays serious attention to them.

Nevertheless, Brookes, Lindzen and other critics have prominently cited M.I.T. meteorologist Reginald Newell's study suggesting that ocean temperatures were as warm in the 1850s as in the 1950s; thus they argue that no global warming has taken place over the past 150 years. But as noted by Trenberth, pre-1890 thermometer data is not usually regarded as credible in scientific assessments of global trends; coverage is not global and the measurements themselves are unreliable. These reservations did not stand out in the critics' citation of Newell's analysis.

I asked all the assembled scientists at this Academy debate my favourite polling question: "What is your estimate of the probability that the next century will see a global warming of 2°C (3.6°F) or more?" All the atmospheric scientists present, including Lindzen, agreed that there would be warming. He, however, felt the most likely extent of warming was between 0.5°C to 1°C rather than 2°C to 4°C as in typical Academy assessments—and the IPCC estimate. Atmospheric scientists Hansen, Mahlman, Trenberth and Ramanathan all agreed that 2°C was certainly a reasonable number for the twenty-first century. They assigned the occurrence of a 2°C warming a probability between 60% and 90%–95%. Lindzen was the only exception; his probability for 2°C warming was 25%—the lowest estimate I've yet heard from any knowledgeable atmospheric scientist. Indeed, several people in the audience—aware of his assertions that global warming is likely to be "⅛ to ¼" of what the Academy presently estimates—expressed surprise to hear that his estimate of the probability of unprecedented warming in the next century differs from mine by less than a factor of 2.5 (25% for him versus 60% for me). One science

writer observing the Academy debate for a major newspaper said to me later, "This 'great debate' is a phoney; you guys really disagree scientifically much less fundamentally than most people think." I was glad he discovered it.

Perhaps the strangest aspect of the National Academy debate occurred the morning before the debate itself. Warren Brookes, writing an editorial in the *Washington Times* entitled "Greenhouse Showdown or Show Trial?"[8] blasted the upcoming session by challenging the roster of debaters and the credentials of the Academy committee members who called for the debate. "Only one of the three panelists asked to prepare papers is from the dissenting side," said Brookes. That, of course, was not true; in addition to Lindzen there was Trenberth, and retired Yale University forester William Reifsnyder, all of whom had been mentioned as critics. Moreover, Robert Jastrow of Dartmouth and William Nierenberg of Scripps Institute of Oceanography, co-authors of the Marshall Institute critique of global warming, refused to attend the meeting, though invited. Nierenberg, however, apologized for his absence by letter and said he was certain that his views would be adequately represented by the three critics present.

Nevertheless Brookes went on: "The most serious 'offenders' to be tried at today's meeting are three of the nation's most prominent senior scientists who won't even be there, Robert Jastrow of Dartmouth, founder of the Goddard Institute for Space Studies at NASA, Frederick Seitz, past president of NAS and William Nierenberg, former director of Scripps Institution (*sic*) of Oceanography." Brookes accused the Academy of being "eager to trash" Jastrow, Seitz and Nierenberg, authors of the Marshall Institute report that many believe was used by White House Chief of Staff John Sununu to prevent E.P.A. Administrator William Reilly from convincing President Bush to join in an international commitment to reduce greenhouse gas pollution. Brookes concluded by accusing some in the Academy of coming "dangerously close to using Lysenkoist tactics to anyone who dares to dissent from the prevailing political/scientific wisdom." [Lysenko was the Soviet biologist who faked genetic research results to match Stalinist ideology.]

Interestingly these personal attacks were not reciprocated by those accused of Lysenkoism. Perhaps they did not feel the ideological antagonism expressed by Brookes in *Forbes:* "As Marxism

is giving way to markets, the political 'Greens' seem determined to put the world economy back into the red, using the greenhouse effect to stop unfettered market-based economic expansion." To me, this says that *Forbes* wasn't publishing a courageous exposé about science, as its newspaper ads boast ("No Guts, No Story"), but rather a defence of its ideology in the guise of science journalism. Everyone is entitled to an ideology, and, to his credit, Brookes admits his in this one sentence, even if he buries it in the middle of a six-page article. What does bother me is his cavalier way of taking selected bits of science out of context to cut and paste them into a slick-sounding article in support of a blatantly ideological viewpoint—a criticism that applies to the writings of some environmental activists as well.

Scientists share responsibility with the media for often failing to communicate complex issues clearly to the public. What the general public, as well as politicians and bureaucrats, do not recognize is that most scientists spend their time arguing about what they don't know. Scientists generally consider discussions of accepted ideas boring and a waste of time. This is because the scientific method operates by constant questioning, particularly of issues not yet well substantiated. But if the public and its representatives do not understand the process of scientific inquiry, then they will have difficulty interpreting the "duelling scientists" debates, let alone deciding whether debaters are honest or ideologically driven.

We scientists simply have to spend more time differentiating accepted information from what is reliably believed to be true and, most important, from what is highly speculative. The public version of the global warming debate rarely separates those components clearly, thereby leaving the false impression that the scientific community overall is in intellectual disarray, when, in fact, the IPCC and the National Research Council's consensus of 1.5° to 4.5°C warming in the next century still reflects the best estimate of a wide range of knowledgeable scientists. This estimate includes recent United Kingdom studies that halved the "best guess" from over 5°C to around 2.5°C.[9] Perhaps some new discovery will push the best guess higher again next week, but meanwhile the 1.5° to 4.5°C consensus warming range endures. Unless we communicate what we do know along with what we don't know the public policy process is subverted in confusing debate that

inadequately represents the true nature of informed opinion.

It is difficult for the media to do what I sometimes wish they would: back off the concept of "balance" in favour of the concept of "perspective." If an issue is complicated, it simply is not enough to play off "all sides," particularly if the opinions of the majority of the experts—the people who create the consensus—are left out. Moreover, that consensus information must be expressed in terms of its probability. Very few scientists would say they believe the future climate *will* warm up from 1.5°C to 4.5°C; rather, most believe that to be reasonably probable. Conveying issues in probabilistic terms with the range of views in perspective is necessary if scientific opinion is to be communicated accurately, rather than as a misleading debate among feuding scientists, or occasionally as a travesty perpetrated by polemicists and ideologues.

Let's return to the details of the debate. Another criticism of global warming projections has been the imperfect match between the warming of the earth and the smooth increase in greenhouse gases over the past hundred years (see Figure 4, page 85). It has been alleged that since most warming in the twentieth century took place up to the 1940s, followed by a cooling at the time the global greenhouse gases were increasing at their fastest, the decade-by-decade temperature trends in the twentieth century cannot therefore be attributed to greenhouse gas build-up.

At first reading that sounds like a valid criticism; but there are several flaws in the argument. First, nature always fluctuates. Several tenths of a degree Celsius warmings and coolings over decades are part of the natural record and, indeed, are normal. Scientists call these fluctuations "climatic noise." These are not predictable as far as anyone can tell, since they appear to be caused largely by the internal redistribution of energy among the principal reservoirs: the atmosphere, oceans, ice, and land surfaces. Therefore, natural fluctuations could partially explain the sharp warming up to the 1940s, the Northern Hemisphere's cooling to 1975, and possibly even the spectacularly rapid re-warming of the 1980s—the warmest decade in the 100-year instrumental record. Secondly, we do not know precisely what other potential climatic forcings (that is, processes that could force the climate to change) have been doing over the past 100 years. These include energy output from the sun, atmospheric particles from

volcanic eruptions, or particles generated by human activities—or as University of Wisconsin climatologist Reid Bryson likes to put it, the "human volcano."

This "forcings" problem is akin to a criminal investigation in which the whereabouts of only one suspect is known and the activities of the other possible suspects were not carefully observed. In this case, the "crime" is the 100-year 0.5°C (0.9°F) warming trend and the leading "suspect" is the known greenhouse gas increase. Unfortunately, since we do not have quantitatively accurate ways of knowing precisely what the other potential forcings may have been (that is, the unwatched "other suspects"), we can't rule out some possible role for them. Some scientists, such as James Hansen and myself, have led efforts to estimate what volcanic or solar forcings may have contributed to twentieth-century temperature trends.[10] Indeed, such estimates improve the match of our computer model simulations to observed twentieth-century temperature trends. However, as all of us have admitted, without more quantitatively reliable information these exercises can do nothing more than sketch out plausible rather than definitive results.

Incidentally, my own simulation result, which used temperature trend data up to the early 1980s, suggested the best fit to the data was such that a CO_2 doubling would cause only about 1.5°C to 2°C global warming—the lower end of the 1.5° C to 4.5°C range cited in most U.S. National Academy of Science reports or by the ICPP. However, it was noted at the time that this could not be taken as strong evidence that CO_2 doubling would result in such a moderate climate warming because we know that the twentieth century record is marked by natural noise, unmeasured alternative forcings and uncertainties in how much temperature change actually occurred. Most scientists still agree that without ten to twenty more years of thermometer, solar, air pollution, and volcanic observations it's difficult to pin down anything quantitatively to very high reliability, say 99% confidence.

Arguing that climate models have been unable to predict a detailed sequence of decade-by-decade temperature fluctuations has been a favourite tactic of some global warming critics.[11] But this is akin to arguing that because we can't predict the individual rolls of a pair of dice, we also can't predict the odds of getting any two faces on any roll. All gamblers know better. Though we

know the statistics for rolling a particular number, we certainly wouldn't be expected to know the sequence of numbers on successive rolls, even for slightly loaded dice. In short, those who argue that an absence of an exact match on a decade-by-decade basis between observed temperature fluctuations and greenhouse gas build-up demonstrates that greenhouse effect sensitivity of models is wrong are simply off in their logic. Such agreement should not be expected in detail as long as a large degree of climatic noise continues to make up much of the decade-by-decade temperature record and as long as we lack precise data on other non-greenhouse gas forcings.

Fortunately, we are now measuring the sun, volcanos and pollution-generated particles, and can thus account better for their effects. In other words, we finally are checking up on the "other suspects." Thus, as greenhouse gases continue to build up in the future, if greenhouse warming does not take place at roughly the predicted rate during the 1990s and into the twenty-first century, then indeed it will be possible to argue on the basis of some direct evidence that the effect predicted by today's models is off line. Personally, I'll be surprised if there is a major error that overrides the 1.5°C to 4.5°C warming projections.

Let me next address the final, and perhaps the most important, criticism made against action to slow global warming; that immediate policy steps to cut CO_2 emissions are too expensive. The *Forbes* newspaper ad suggests, for example, that if we cut CO_2 emissions the U.S. will be bankrupt and the Third World impoverished. Indeed, as I discuss frequently in *Global Warming*, there is substantial Third World opposition to the prospect that they may be deprived of their own industrial revolutions, and of the economic growth experienced in the Victorian period by the then-developing countries using cheap and dirty coal. Since developing Third World countries such as India and China have abundant coal supplies, they would like to use them as low-cost means to industrialization. In 1990, however, these countries have between them some two billion people, whereas in the nineteenth century the entire world didn't have two billion people. The global impact of the developing countries' use of coal to produce even a quarter of our current industrial standard of living would be greater than was ours. Needless to say, such arguments are not met sympathetically in China or India.

It is sensible, I believe, to argue that developing countries need not repeat the western experience of industrialization with smog-choked cities, acid rain, and inefficient power production, given that modern technology has many better solutions. Unfortunately, developing countries typically respond that high-tech, efficient machinery is more expensive than the traditional options available to them. This dilemma makes obvious the need for a bargain. Countries with technology and capital must provide resources to developing countries, which in return must keep population growth under control and work toward industrial development with the lowest polluting, most efficient technologies, even if they cost more initially.

There have been international efforts afoot to draft an agreement requiring the developed nations to decrease their carbon dioxide emissions by, say, 20% by the year 2000, and to cut the projected emissions growth rates in developing countries. This has been strongly opposed by the United States, echoed by Japan, the U.K., the U.S.S.R., and some other countries. The Japanese are unhappy since they're already twice as energy efficient as the U.S. They claim it would cost them much more to cut carbon dioxide by 20% than the U.S., since our very inefficiency gives us more opportunity to cut cheaply.

Still less efficient developing countries could produce far less growth in carbon dioxide pollution by using efficient, modern technologies instead of older, cheaper ones. This sets up a possibility for creative international management that might not only eliminate Third World opposition to global emissions limitations, but also could encourage competition among these nations to be the venue for future emissions cuts funded by developed countries; cuts that in turn could buy the developed nation out of its reduction requirement by funding even larger CO_2 reductions in now-energy inefficient developing countries.

I recently discussed this idea with a Japanese economist, Yoichi Kaya, an energy analyst from the University of Tokyo. He tried to assess what a significant cut in CO_2 emissions might imply for world and Japanese economic growth. Kaya concluded that for energy-efficient countries (like Japan) substantial cuts could severely lower economic growth—unless ways were found to increase the rate at which the economy becomes less dependent on energy growth. (This is what environmentally-oriented energy

analysts already argue is essential for environmentally sustainable development; see discussion in Chapter 8.) He also showed that developing countries' economic growth would require efficient energy or else they would pollute severely.

I met with Professor Kaya in Tokyo in November 1989, while the Noordwijk Conference, an international environmental meeting in the Netherlands, was producing daily headlines about how the U.S., Japan, and the United Kingdom were balking at specific emissions cuts. Professor Kaya and I agreed that equal percentage emissions limitations for each nation may make international politics simpler, but that from a global point of view, reducing emissions by a fixed fraction for all nations may be neither the most cost effective plan, nor the fairest. Unequal fractional cuts may sound unfair to some nations, but what determines fairness, we felt, is not how much CO_2 emission each cuts at home, but *who pays* for all the cuts. To be sure, the rich nations should pay a disproportionate share of the costs since they have been responsible for over half of the CO_2 pollution to date. But energy-efficient countries like Japan, Germany, and Italy may not be the logical first places to look for big CO_2 emissions savings. However, we agreed it would not be politically easy to negotiate a system where some nations pay and other nations cut their emissions (or limit emissions growth rates in the case of Third World nations) by greater amounts. But from the point of view of the minimum global investment for the maximum amount of global pollution reduction, such a strategy probably is most efficient and consideration of the issue should be part of the international negotiating process.

In other words, supposing each developed country had to reduce its CO_2 impact on the world by something equivalent to 20% of its present production. Let's say, continuing the Japanese example, that their quota is to cut 100 million tons of CO_2 annually. Why not structure an international agreement so that the Japanese need to be responsible either for reducing 100 million tons of CO_2 from their own industries, or else paying to cut 150 million tons in another country (or some combination of both)? The obvious candidate is China, since Japanese investment in China for efficient energy production would both reduce acid rain over Japan as well as global CO_2 build-up. It is likely to be much cheaper per unit of CO_2 saved for the Japanese to improve

Chinese energy efficiency, since China is starting out so inefficient, than for Japan to improve its own efficiency. At the same time the Chinese would receive extra development assistance, the Japanese would get less acid rain and could buy out of their emissions reduction quota without having to cut emissions at home. Moreover, the Japanese would be creating friendship and markets for their products in the future, whereas the Chinese would be getting more efficient machines with lower long-term operating costs, thereby improving their economy and competitive posture into the twenty-first century. In other words, everybody wins. But first, we need a world emissions agreement that provides incentives for such bargaining and trading to take place. That agreement is what the U.S., the U.K., the U.S.S.R., and Japan were balking at in 1989.

Other similar ideas include issuing "tradeable (or leaseable) permits," giving everyone in the world the right to emit a certain amount of CO_2 or some other greenhouse gas.[12] These permits could be traded for cash, food, energy efficient products, etc. As of the signing of the global agreement, all nations would have equal per capita CO_2 emissions rights. That implies a certain amount of total national emissions. In the future a country could sell or trade these rights or exercise them for development, or increase their population and thereby limit their future emissions per capita. In other words, a fixed per capita emissions right that goes into effect at the signing date would dramatically reduce Third World suspicions that they were being singled out to bear the immediate burden of emission controls.

Critics of emissions reductions cite the supposed annual costs of global warming reduction at tens of billions of dollars—too much to be worth the benefits of climate change abatement. But they often neglect the additional non-greenhouse-effect benefits of emissions reductions: reduced acid rain; reduced air pollution; reduced balance of payments deficits; and lower long-term operating costs of efficient equipment, which reduces the energy costs of manufactured products and enhances competitiveness. Critics who simply cite the potential capital costs of CO_2 reduction write newspaper stories about how many billions or trillions it will cost and scare people away from action. But they often present a very unbalanced view of the distribution of benefits that come with greenhouse gas abatement. Unfortunately, it is

very difficult to communicate these benefits in window sticker length headlines or in sound bites on the evening news, which is often all the time this complicated story gets.

For example, John Sununu, defending his role in persuading President Bush not to agree to specific CO_2 emissions reductions at Noordwijk, said on TV in 1990: "There's a little tendency by some of the faceless bureaucrats on the environmental side to try and create a policy in this country that cuts off our use of coal, oil and natural gas. I don't think America wants not to be able to use their automobiles."[13] I agree we don't want to cut "off our use of coal, oil and natural gas"; nor do we want to abandon our cars. But, Mr. Sununu, who does? Not one proposal from any bureaucrat, National Academy of Sciences committee or even environmental group I know of ever proposed such an absurd policy. Rather, most talk about giving up (or heavily taxing) petrol guzzling cars and switching to less polluting, more energy-efficient equipment, regardless of the fuel used. But some studies have suggested that switching to less polluting energy systems could cost "$800 billion, under optimistic scenarios of available fuel substitutes and increasing energy efficiency, to $3.6 trillion under pessimistic scenarios . . . between now and 2100." This quote from the February 1990 "Economic Report of the President" to Congress was based on the initial results of the first wave of economic model simulations.

Because of the controversy over the reliability of such models, the National Academy of Sciences ran a debate (following the climate debate mentioned earlier) among some economic forecasters and their critics. What emerged was very revealing. First, over 110 years (1990 to 2100) even a trillion dollars in CO_2 reduction costs, which sounds very expensive, is less than $10 billion each year—only a few per-cent of the current annual defence budget. Moreover, Robert Williams, an energy technology specialist from Princeton University, pointed out that the so-called "optimistic scenario" of $800 billion in costs to cut CO_2 emissions was based on very *pessimistic* assumptions about the rapidly decreasing costs of renewable energy systems such as solar, wind, or biomass power. Furthermore, with the exception of one effort by Yale University economist William Nordhaus, none of the other modelling simulations attempted to estimate the direct environmental benefits of our supposed trillion dollar investment

in CO_2 emission controls. It is unconscionable that some critics of global warming action would cite these dubious cost estimates without so much as mentioning the potential benefits of slowing CO_2 emissions. Nordhaus, though, by balancing costs and benefits in his model runs, argued that cutting annual CO_2 emissions by as little as 10% or as much as 47% would actually produce benefits greater than the costs.[14] His model, however, was admittedly crude, laden with unprovable assumptions, and unable by itself to provide quantitatively reliable information for making policy choices.

Cross-examination of the economists by Academy committee members also revealed that their economic models had not been tested to see how they performed in predicting the economic consequences of historical events such as the 1973 OPEC oil price rise. I was shocked that such tests had not been done, and dismayed that some administration officials were actually citing these premature, unvalidated economic model results for costs of emissions controls as an alleged rational basis for making national policy.[15]

In summary, then, the greenhouse effect, the heat-trapping properties of the atmosphere and its gases and particles, is well understood and well validated as a scientific principle. Indeed, it is as good a theory as there is in the atmospheric sciences. Moreover, in late 1989, A. Raval and V. Ramanathan at the University of Chicago, used satellite observations to study the important water vapour greenhouse feedback mechanism, a process that is central to most models' estimates of some 3°C plus or minus 1.5°C equilibrium warming from a doubling of CO_2. They conclude, "The greenhouse effect is found to increase significantly with sea surface temperature. The rate of increase gives compelling evidence for the positive feedback between surface temperature, water vapour and the greenhouse effect; the magnitude of the feedback is consistent with that predicted by climate models."[16] *In other words, the heat-trapping capacity of the atmosphere is well understood and well measured on earth, and much of the sometimes polemical debate in the media over the greenhouse effect has little basis in reality.* This empirical confirmation of the natural greenhouse effect, which is consistent with the greenhouse effect of climate models, stands in stark contrast to the theoretical arguments of some critics. They believe that their untested conceptions

of temperature–water vapour processes in parts of the tropics will reduce present model estimates of global warming by a factor of four or so.

It is well known that the 25% increase in CO_2 documented since the industrial revolution, the 100% increase in methane since the industrial revolution, and the introduction of man-made chemicals such as chlorofluorocarbons (also responsible for stratospheric ozone depletion) since the 1950s should have trapped about two extra watts of radiant energy over every square metre of earth. That much is accepted by most climatological specialists. Less well accepted, however, is how to translate those two watts of heat into "x" degrees of surface temperature change, since this involves assumptions about how that heat will be distributed among surface temperature rises, evaporation increases, cloudiness changes, ice changes, and so forth. The factor of two to three uncertainty in global temperature rise projections cited in the National Research Council's reports reflects a legitimate estimate of uncertainty held by most in the scientific community. Indeed, recent attempts by a British group to mimic the effects of cloud droplets halved their model's sensitivity to doubled CO_2, but the results remained well within the often-cited 1.5°C to 4.5°C range. However, the authors of the study wisely pointed out that "although the revised cloud scheme is more detailed, it is not necessarily more accurate than the less sophisticated scheme."[17] I have never seen this forthright and important reservation quoted by global warming critics who cite the British work as a reason to lower our concern by 50%. Nor, in the spring of 1990, was a NASA group's satellite estimate of global temperature change for the 1980s properly cited in the media as confirming, rather than questioning, instrumental records of global warming.[18] Finally, as explained in the original edition of *Global Warming*, predicting detailed regional distribution of climatic anomalies—that is, where and when it will be wetter and drier, whether floods will occur in the spring in California or forest fires in Siberia in August—is highly speculative, although plausible scenarios can be given.

While climatic models are far from fully verified for future simulations, the present seasonal and ancient climatic simulations, along with satellite observations of atmospheric heat trapping, are strong evidence that state-of-the-art climatic models already

have considerable predictive skills. An awareness of what models are and what they can and can't do is probably the best we can ask of the public and its representatives. Then the tough policy problem is how to apply society's values about risk taking in choosing to face the future, given the possible outcomes that climatic models foretell.

The global warming debate takes in both science and politics. But it is essential for the public to understand that disagreements over what to do about the prospect of global warming (a political value issue) are far greater than over the approximate probability that unprecedented climate change is being built into a Greenhouse Century (a scientific debate). Nothing that has happened since the first publication of *Global Warming* has changed the strong consensus among scientists that climatic changes unprecedented in the 10,000-year era of human civilization are a good bet to happen. The more we debate and the longer we delay slowing down the greenhouse gas emissions, the greater the magnitude of climatic change that we and the rest of life on Earth will have to cope with. We are still marching relentlessly into the Greenhouse Century.

Epilogue

1. George C. Marshall Institute, *Scientific Perspectives on the Greenhouse Problem* (Washington D.C.: George C. Marshall Institute, 1989): 37 pp.
2. S. H. Schneider, 1989 personal correspondence to Alan Hecht as follows:

September 1, 1989

Dr. Alan Hecht
Office of International Activities
Environmental Protection Agency

Dear Alan:
Congratulations on your new job. I predict it will be stimulating both for you and the people you interact with. As you requested, here is my brief analysis of the recent Marshall Institute report (Scientific Perspectives on the Greenhouse Problem, 1989, George C. Marshall Institute, Washington, D.C.)

The Marshall Institute report is really three reports in one. The first part is a fairly standard review of the greenhouse science problem. It is typical of that in most National Academy of Science reports, and reflects the caveats normally given in the presentations of most of the climatic modelers that talk or write on this issue. There is an impression in the Marshall report that somehow these caveats have been underrepresented by some members of the community, but this is not strongly argued. In any case, up to page 14, I have few disputes with the narrative, other than its constant focus on uncertainty and its omission of the primary reasons that climate modelers are concerned for the future: validation of the models' performance against actual climatic changes. There is insufficient mention of model validation in the Marshall report. For example, the authors do not mention the excellent performance of most General Circulation Models in simulating the very large seasonal cycle of surface temperature, or the successes in simulation of neighboring planets, Mars and Venus, each of which has radically different greenhouse properties than earth, nor is there mention of the daily variability validation studies that have recently been published. While these validations do not remove the uncertainties focused on in the first 14 pages, the absence of the mention of the substantial degree of model validation of climate sensitivity suggests a bias.

On page 15 begin what I believe to be the major problems with this report. First of all, the authors note, citing my *Science* article (S. H. Schneider, *Science 243*, 771, 1989), that the global temperature response to CO_2 doubling is in the range of 1° to 5°C. They correctly point out that a temperature drop of roughly 1°C relative to today's temperatures occurred in the Little Ice Age. That episode occurred between about 1400 and 1850 in various parts of the world at various times and differing degrees. Thus, a natural cooling of 1°C would, the Marshall authors argue, completely compensate for the lower end of any greenhouse warming if such a Little Ice Age-like event recurred in the next century. Indeed, this is correct, but it fails to mention that the upper range of the CO_2 warming limit, (i.e., 5°C), would totally swamp any natural fluctuations of the type that have occurred in the past 10,000 years, the time since the end of the last ice age. Moreover, natural centuries-long fluctuations on the order of 1° *warming* (e.g., the so-called Medieval Optimum around 1,000 years ago) are just as likely to occur, and could *add* to any anthropogenic greenhouse warming in the next century. Indeed, what is absent in the remainder of the Marshall document is any statement, even intuitive ones, about the relative probabilities of the greenhouse warming being at the low end of the range and comparable natural cooling occurring relative to the probabilities that more middle or upper severity warming scenarios could occur. (e.g., see *Current Issues in Atmospheric Change*, National Acade-

my of Sciences, 1987 for statements of the high probability of warming greater than 1°C in the 21st century.)

In section four the authors delve into the very complicated and long debated controversial issues of potential solar effects on climate. They cite the fact that sunspots have been relatively absent for certain century long periods, which have typical recurrence intervals of hundreds of years. The coincidence between the last such minimum, the "Maunder Minimum" between 1650 and 1700, and the "Little Ice Age" is cited. But what is not cited by the Marshall authors is the fact that before and after this 50 year long minimum the so-called Little Ice Age was in full force in many parts of the world. Furthermore, the coolest periods during this time did not necessarily occur in all places at the same time nor was it fully coincident with Maunder Minimum. In addition, a 10,000 year look at tree ring evidence shows many other such minima, but does not reveal consistent correlation of these sunspots minima with glacial advance periods, and therefore, no firm correlation can be established. This is why this area of research is still controversial, a point not adequately stressed in the Marshall document. Finally, the magnitude of natural, several century long global climate fluctuations during the past 10,000 year inter-glacial period is on the order of 1°C, which is at the low end of the 1° to 5°C range the Marshall authors concede as the consensus estimate for the next century.

Therefore, the higher probabilities are that whatever nature does on the sun or earth to cause natural fluctuations is likely to be swamped by manmade influence of greenhouse gases sometime between now and the first few decades of the 21st century. These relative probabilities get no prominent attention. Rather the lower probability scenarios are what the Marshall Institute authors focus on. Moreover, they cite recent evidence that some stars have been observed to have energy output changes on the order of a few tenths of a percent to explain recent global warming trends. By no means can this be cited to explain with high confidence the observed approximately 0.5°C warming trend of the past century. It is equally likely, as not mentioned by the Marshall Institution report, that a solar constant *decrease* from our sun could have been in progress during the past 100 years, and therefore, any greenhouse warming that has taken place could have been masked by such an event. Since the solar constant has not been well observed, except in the past decade, both scenarios are equally likely. Furthermore, if only a few tenths of a percent change in solar energy were responsible for the 0.5°C century long trend in climate over the past century, then this would suggest a planet that is relatively sensitive to small energy inputs. The Marshall Institute simply can't have it both ways: they can't argue on the one hand that small changes in solar energy output can cause large temperature changes, but that comparable changes in the energy input from greenhouse gases will not also produce comparable large signals. Either the system is sensitive to large scale radiative forcing or it is not, another factor not mentioned by the Marshall authors. Although they argue that a 33-year running average of sunspot numbers superficially resembles the temperature record of the past 100 years, they do wisely admit that this could well be a coincidence.

The Marshall authors go on to claim that "scientists' concerns for offering sound advice on the greenhouse problem have tended to rely on the observed temperature increase of 0.5°C since 1880 as their best evidence that the greenhouse effect is already here and that steps should be taken now to cope with its full development in the next century." (Marshall Institute, page 28.)

However, only very few people have made such a claim. Most scientists I know argue that it is not the performance of the planet in the past century, which only in the last decade is at the margin of the noise level of natural climate fluctuations, that motivates their concerns for the next century. Rather, our concerns are grounded in the very well-validated understanding of how radiative trapping by important trace gases like carbon dioxide, methane and chlorofluorocarbons can heat the surface. Further concern is based on the (less well) validated modeling sensitivity studies which suggest that something between 1° and 5°C warming is the most probable prediction for the next century, (e.g., NAS, 1987). Indeed, it will take another decade or two of observations, with a presumed continuing of the record heat of the 1980s, to establish to a high degree of confidence that the greenhouse forcing of the past cen-

tury is finally unambiguously detected. But, if we follow the Marshall Institute's advice and wait for such certainty before implementing actions we will then have to adapt to a much larger dose of change than if we attempt to slow it down now. That decision, of course, is a *value judgement*, not a decision that can be made based solely on any scientific method.

Herein is my principal objection to the Marshall report, and in fact, an objection so serious as to make me doubt the capability of the authors to provide a balanced scientific view. They claim "current forecasts of the manmade greenhouse effect do not appear to be sufficiently accurate to be used as a basis for sound national policy decisions." (Marshall Institute, page 23.) Furthermore, they argue that "it is our judgement that if a prudent investment is made in computing power, observing programs and added manpower, answers that have a usable degree of reliability can be provided to policy makers within three to five years." Thus, they recommend no current policy response to the present debate, other than more research. What I find so objectionable in these statements, particularly the former one, is that it is not a scientific judgement, but their value judgement, which they do not explicitly claim as their personal views of how to respond to the range of uncertainties. The second statement is technically inaccurate, I believe, in that virtually all knowledgeable scientists would never claim that we will have a strong consensus of atmospheric researchers as to the reliability of the regional distribution of time evolving climate changes in three to five years, regardless of the level of research effort. If left to its own devices, the climate community is likely to take twenty-five years to provide such information. If the kinds of "prudent investments" that the Marshall Institute authors do wisely call for were implemented, then it is my opinion, and one I believe is widely shared in the scientific community, that we could *accelerate* substantially the rate of building such consensus. But, a decade or so would still be needed before which high resolution, coupled with models of atmosphere, ocean, sea ice, land hydrology, ecosystems, and chemistry could be adequately run, tested and validated to provide credible regional forecasts. Of course, it is better to produce such forecasts in ten years than in twenty-five and the costs are small relative to the potential risks. Thus, I agree with the advice to invest in such research. But to claim answers will be clear within three to five years and therefore, policy should wait for them, is in my opinion a scientifically erroneous judgment as well as a value judgment with which I personally disagree strongly. People may legitimately disagree about values, but scientists must always make this explicit, and the absence of this is what has me so disturbed about the Marshall Report.

Finally, the authors provide no cost/benefit analysis of the risks or benefits of alternative actions now versus the risks and benefits of delay. In the absence of such studies, or even the citation to the few such studies that exist, the Marshall report's primary conclusion to delay policy actions has little merit.

Alan, I hope this is useful to you and that you can use it to good purpose.

Warm regards and best wishes in your new position.

Sincerely,
Stephen H. Schneider
Head
Interdisciplinary Climate Systems

3. National Research Council, *Carbon Dioxide and Climate: A Scientific Assessment* (Washington, D.C.: National Academy Press, 1979): 11 pp; National Research Council, *Changing Climate: Report of the Carbon Dioxide Assessment Committee* (Washington D.C.: National Academy Press, 1983): 496 pp; National Academy of Sciences, *Current Issues in Atmospheric Change*, (Washington D.C.: National Academy Press, 1987): 39 pp., are examples of National Academy reports in which the well-travelled 1.5°C–4.5°C warming range is cited. A current study by the National Research Council's Committee on Science, Engineering and Public Policy is likely to reaffirm that range, in full awareness of the current debate.
4. Intergovernmental Panel on Climate Change (IPCC), *Scientific Assessment of Climate Change*, 2nd Draft, March 1990 (Geneva: World Meteorological Organization).
5. R. S. Lindzen, "Greenhouse Warming: Science Versus Consensus" (unpublished – but widely circulated – manuscript, dated October 10, 1989). A less *ad hominem* ver-

sion was published as: R. S. Lindzen, "Some Coolness Concerning Global Warming," *Bulletin of the American Meteorological Society* 77 (1990): 288–299. In the first version Lindzen asserted that the NAS consensus overestimated global warming by a factor of four to eight. The published version some months later suggested a more moderate factor of two to five overestimation.

6. W. T. Brookes, "The Global Warming Panic," *Forbes*, (25 December 1989): 96–102.
7. W. T. Brookes, *op. cit.*
8. W. T. Brookes, "Greenhouse Showdown or Show Trial?," *Washington Times (January* 24, 1990): F2.
9. J. F. B. Mitchell, C. A. Senior and W. J. Ingram, "CO_2 and Climate: A Missing Feedback?" *Nature* 341 (1989): 132–134.
10. J. Hansen, D. Johnson, A. Lacis, S. Lebedeff, P. Lee, D. Rind and G. Russell, "Climate Impact of Increasing Atmospheric Carbon Dioxide," *Science* 213 (1981): 957–966; R. L. Gilliland and S. H. Schneider, "Volcanic, CO_2 and Solar Forcing of Northern and Southern Hemisphere Surface Air Temperatures," *Nature* 310 (1984): 38–41. See also, T. M. L. Wigley and S. C. B. Raper, "Natural variability of the Climate System and Detection of the Greenhouse Effect," *Nature 344* (1990): 324–327.
11. H. W. Ellsaeser, "The Climatic Effect of CO_2: A Different View," *Atmospheric Environment* 18 (1984): 431–434.
12. M. Grubb, "The Greenhouse Effect: Negotiating Targets," Energy and Environmental Program (London: Royal Institute of International Affairs, 1989): 56 pp.
13. S. Engelberg, "Sununu Says He Revised Speech on Warming," *New York Times* (February 5, 1990): A15.
14. W. D. Nordhaus, "To Slow or Not To Slow: The Economics of the Greenhouse Effect," manuscript submitted to the Panel on Policy Implications of Greenhouse Warming; Committee on Science, Engineering and Public Policy (Washington D.C.: National Research Council, 1990).
15. To understand why one prominent economic model is very questionable in projecting economic costs of CO_2 emissions cuts (usually stated as a loss of gross national product) we need to explain briefly how it works. In essence, the econometric model used to project future GNP performance as a function of energy prices is calibrated by statistical methods (called regression equations) that match GNP change over time with energy prices over time. For example, following the OPEC oil price rise of 1973, GNP responded by decreasing for several years. Then, as economies adjusted to the higher prices and energy efficiency was encouraged by the price shock, GNP grew once again even though total energy consumption changed very little for the next half dozen years. Clearly, the immediate transient response of the GNP to energy price changes will be different from the medium term adjustment, which should again be different over the very long term. Yet, an econometric model used to project GNP response to CO_2 controls (which were dealt with as an equivalent energy tax) was run over 110 years into the future based on curve matching that included data on short term, transient conditions. Seen in this light it is not surprising that such models would predict large GNP losses 100 years into the future when energy prices go up—it is built into the model by regression equations that include transient situations and then apply them over the very long term. It is also no wonder such models were severely criticized at the U.S. National Academy of Sciences debate. More on this debate can be found in Williams, R. H., 1990 (Draft). "Will Constraining Fossil Fuel Carbon Dioxide Emissions Really Cost So Much?" A Critique of: Manne, A. S. and R. Richels, 1990 (Draft), "Global CO_2 Emissions Reduction—the Impacts of Rising Energy Costs," February 1990.
16. A. Raval and V. Ramanathan, "Observational Determination of the Greenhouse Effect," *Nature* 341 (1989): 758.
17. J. F. B. Mitchell et al., *Nature.*
18. In 1990 two NASA satellite experts [Spencer, R. W. and J. R. Christy, "Precise Monitoring of Global Temperature Trends from Satellites," *Science* 247 (1990): 1558–1562] stated the following:

> Passive microwave radiometry from satellites provides more precise atmospheric temperature information than that obtained from the relatively sparse

> distribution of thermometers over the earth's surface. Accurate global atmospheric temperature estimates are needed for detection of possible greenhouse warming, evaluation of computer models of climate change, and for understanding important factors in the climate system. Analysis of the first ten years (1979 to 1988) of satellite measurements of lower atmospheric temperature changes reveals a monthly precision of 0.01°C, large temperature variability on time scales from weeks to several years, but no obvious trend for the ten-year period. The warmest years, in descending order, were 1987, 1988, 1983, and 1980. The years 1984, 1985, and 1986 were the coolest.

The usually warm early 1980s were followed by a few cooler years that were coincident with the explosive volcanic eruption of El Chichon. This was followed a few years later by the warmest years in the instrumental record, 1987 and 1988 (see Fig. 4, p. 85). Whether the cool year-volcano correlation was coincidence or cause and effect is not certain. What is important is that the instrumental record for the 1980s correlated well with the satellite remote-sensing technique for mid-tropospheric temperature of Spencer and Christy. Yet, most of the media stories following the publication of this paper ignored the correlation between the thermometer and satellite records, and instead extrapolated out of context a sentence of the authors to the effect that no global warming trend was evident in the ten years of their analysis. Of course, no responsible scientist would ever claim that a global warming signal could be detected above background noise from ten years of data! Nevertheless, a frequent media interpretation of this study was that there was "no global warming trend"—clearly a serious misinterpretation of the facts given the one-hundred-year record (on Fig. 4).