
**Global Warming Research:
Learning from NAPAP's
Mistakes**

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By now nearly everyone has heard about global warming. Predictions are that the atmospheric concentration of carbon dioxide (CO₂) from the burning of gasoline, coal, and other fossil fuels will double by the middle of the next century, causing the average temperature of the Earth to rise by about 3° to 8° Fahrenheit. While this may be good news to winter-weary Northerners, the potential ecological and climate impacts of such a temperature rise could be devastating. Radically altered precipitation patterns, coastal inundation from sea-level rise, and the extinction of plant and animal species are among the concerns of many who have studied the situation. It is no wonder that global climate change has become the most important environmental issue on the world agenda, and the key focus of international policy deliberations.

At the same time there are many uncertainties. The predictions could be wrong; two decades ago, for example, it appeared the Earth was cooling. If warming does occur, it may not be nearly as severe as predicted, nor as quick to happen. That could leave ample time for people and ecosystems to adapt to any changes. But the uncertainties cut both ways. What if climate change occurs faster than ex-

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pected, or if impacts prove more severe? The results could be catastrophic.

Actions to address the problem would require worldwide reductions in the amount of CO₂ and other so-called "greenhouse gases" (mainly methane, chlorofluorocarbons, and nitrous oxide) which, like CO₂, trap heat in the atmosphere. European nations, especially the Scandinavian countries, have called for immediate reductions in such emissions to stabilize the buildup of greenhouse gases in the atmosphere. Reductions would be achieved primarily by a decrease in fossil-fuel consumption that would come through taxes on fuels or their carbon content, or through reduced demand for fuels as a result of improvements in energy efficiency. Such proposals figured prominently in deliberations for the June 1992 United Nations Conference on Environment and Development.

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To date, however, the Bush administration has rejected such initiatives, seeing them as hasty and wasteful of resources needed for more pressing matters. Until the global-climate problem is better understood, the administration contends, the United States should not be stampeded into taking actions that could have ruinous consequences for the economy, especially at a time when the economy is already hard-hit by a recession. The administration does endorse the notion of "no regrets" options that lower CO₂ emissions for reasons unrelated to climate change (e.g., via energy efficiency improvements), and strongly supports the need for more research. Indeed, the U.S. government now boasts a \$1.2 billion dollar annual research program on global-climate change.

The question is whether this research will make any difference, or will help us make wis-

er decisions in the future. At the present time the answer appears to be "no."

Déjà Vu All Over Again

The current situation on global-climate research bears a distinct resemblance to the situation roughly a decade ago with acid rain. In the early 1980s international calls from Canada and northern Europe for reductions in the sulfur dioxide emissions believed responsible for acid rain largely fell on deaf ears. The U.S. economy was pinched by a recession which had especially hurt the industrial Midwest. Instead of immediate action, the Reagan administration favored a program of research to understand the effects of acid rain and ways of controlling it. For a full decade the National Acid Precipitation Assessment Program — commonly known as NAPAP — labored at its task.

But as recent post-mortems of NAPAP have confirmed, the 10-year, half-billion-dollar program to guide U.S. policy on acid-rain control proved largely irrelevant when the time came for action on the new Clean Air Act Amendments in 1990. While NAPAP's scientific accomplishments were praised, the inter-agency program created by Congress to inform public policy had failed in its primary mission — providing policy-relevant information in a timely manner.

Now, the United States appears to be headed down the same ill-fated path in dealing with the more difficult and far-reaching environmental issues associated with global warming. While the resources devoted to global-climate change research are laudable, this program appears to be following the precedent set by NAPAP; i.e., the current research agenda will produce a lot of good science, but be largely irrelevant to the policy decisions that the United States and other nations face over the next decade.

How can a multi-billion dollar research program involving some of the best scientific

minds fail to help produce better public policy? Answers to this question may be found by examining the lessons learned from NAPAP and the current plans for global-climate change research.

The NAPAP Legacy

In the early 1970s, a number of U.S. scientists gained national attention by asserting that lakes in the Adirondacks Park region of New York were dying from "acid rain." A number of environmental groups echoed the warning that acid deposition was damaging forests and lakes across the northeastern United States and Canada. Congressional leaders reacted to public concern by demanding reductions in emissions of sulfur and nitrogen oxides from the midwestern coal-fired power plants believed culpable. However, the cost of these emission reductions were predicted to be high, leading to steep increases in utility bills for some consumers and severe unemployment in the midwestern high sulfur coal industry — a region already hard hit by economic recession.

Pointing to the scientific and technical uncertainties surrounding the acid-rain debate, Congress passed the Acid Precipitation Act of 1980, creating a ten-year research program, NAPAP, to get better scientific answers that would inform public policy. Congress delayed acid-rain legislation for nearly ten years, in part waiting for NAPAP's results. In 1990, the final results were announced. But the findings of NAPAP's 27 technical reports and three volume integrated assessment were, by then, incidental to the debate. Even before the reports were finalized, Congress had all but completed action on the 1990 Clean Air Act Amendments, including sulfur dioxide emission controls similar to those called for in the early 1980s.

What NAPAP reported, however, was that many of the early scientific claims were exaggerated, that fewer lakes were being acidified than originally thought, and that reducing U.S.

emissions of sulfur dioxide (SO₂) by about 30 percent yielded essentially the same long-term environmental benefits to aquatic systems as the more costly reductions of 40 to 50 percent that had been proposed. Although NAPAP was silent or equivocal about many other effects of acid rain, the tone of the final assessment was that acid-rain was not the environmental catastrophe widely portrayed a decade earlier.

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Yet, these results never made it into the political debate. Congress had already decided on a 40 percent (10 million ton per year) reduction in SO₂ emissions based essentially on pre-NAPAP estimates of what was needed. Why was NAPAP ignored? There are a number of explanations, but several factors were critical.

Reporting Delays

First of all, NAPAP's results were not reported in a timely, understandable, or credible manner. NAPAP's first interim assessment, due in 1985, was delayed a full two years, largely because of a change in directors. This delay, combined with accusations by NAPAP scientists of attempts by the new director to downplay the significance of scientific findings in the interim assessment, seriously damaged NAPAP's credibility. The original plan to publish additional interim assessments in 1987 and 1989 gave way to a hurried attempt simply to finish on time under the leadership of a new director.

Technical Jargon

Further, NAPAP's reports were largely unintelligible to Congress and the public. The interim assessment was a four-volume compila-

tion of scientific findings, not the coherent, policy-relevant assessment of acid-rain damages that had been awaited. The final assessment, which attempted to address some of the earlier shortcomings, received only a cursory congressional committee hearing, shortly before the new Clean Air Act Amendments passed into law. NAPAP had failed to communicate what Congress wanted to know.

A major shortcoming of NAPAP was its failure to pose the critical policy-related questions at the outset.

Nothing was said, for example, about the impacts of acid-rain controls on regional coal-mining employment or customers' electric bills. Nor were the benefits of acid-rain controls — of major interest to Congress — reported in ways that members of Congress or the public could understand. For example, NAPAP's integrated assessment, which was supposed to be policy-oriented, summarized the impact of acid-rain on soils by writing that, a "reduction of sulfur deposition by 50 percent over 10 years would cause a slight increase in base saturation of some shallow sensitive soils with low cation exchange capacity, but most soils would not be affected."

Misdirected Research Efforts

How did things get to such a state? A major shortcoming of NAPAP was its failure to pose the critical policy-related questions at the outset, and then direct the overall research agenda to answer these questions in a timely fashion. Instead, independent government agencies, driven by different missions, motivations, and expertise, largely pushed their own scientific research agendas. No serious effort was made to define policy-related priorities, and to then shape an appropriate set of projects and timetables to answer in a meaningful way

the most critical questions of concern to Congress. A small policy and assessment program begun during the early years of NAPAP was dismantled halfway through the program in a move to keep NAPAP science-oriented. Thus, despite the recognition by some NAPAP participants that policy needs were important, it was the general quest for scientific knowledge rather than the policy needs of Congress that dominated the thinking and programs of NAPAP.

Major resources, for instance, were committed to developing a complex atmospheric transport model that could allow researchers to identify the sources of an acid aerosol falling on a particular region of the country at a particular hour of the day. While this research clearly advanced the state of science of atmospheric transport modeling and chemistry, and resolved some of the technical debates among researchers, the substantial resources devoted to that endeavor did little to improve upon the existing policy options available to Congress, or to enhance understanding of how best to control acid deposition. Indeed, NAPAP's pursuit of the best scientific models may have inhibited the timely application of other less sophisticated policy analysis methods that were available.

While atmospheric transport modeling received major attention, other important areas were neglected. Studies of the effects of nitrogen oxides on forests and soil systems, for instance, were not begun until halfway through the program, with findings only now beginning to emerge. As a result, some scientists contend that NAPAP may have significantly underestimated the emission reductions needed to protect against some of the long-term effects of acid deposition. Similar criticisms have been leveled at NAPAP's failure to adequately study other areas such as effects on construction materials and human health.

The lack of policy relevance was augmented by the fact that the research was managed and reviewed largely by researchers interested in

pursuing scientific research, not applied, policy-related studies. NAPAP scientists produced a fundamentally different approach to research than was needed for a policy-oriented program. For example, aquatic scientists initially focused on the detailed mechanisms of lake acidification, not on how many lakes were actually acidified, or how many would be improved by acid-rain controls — issues critical to the policy process. Only late in the game did NAPAP begin to address policy-related issues for aquatic and forest systems. In other areas, such as materials effects, health effects, and economic damage assessments, policy-driven studies barely got started.

Funding Shortfalls and Politics

The Reagan administration put little new money into NAPAP in the early years. Instead, it directed EPA and the other agencies to reprogram existing funds. Predictably, many program managers and agencies responded by altering descriptions of their on-going research projects to emphasize the links to acid deposition, even where the link was flimsy. NAPAP, with no independent budget authority, no strong control over agency agendas, and no real willingness to make waves, merely turned a blind eye to this strategy. Thus, little of NAPAP's resources were new or newly targeted on policy-related acid rain research. Not until roughly half-way through the program did NAPAP's funding increase to address a few policy-relevant issues. But that effort proved too little, too late.

Politics also mattered. Congress could not agree on what action to take and so compromised on a program of research. The Reagan administration wanted to delay action. If policy-relevant research were performed early in the process, the results might have been used to force legislation that the administration did not want. The obvious solution was to emphasize scientific uncertainties, call for more scientific research, and criticize findings that did not reflect the best possible science. This tactic wins

the acclaim of the scientific community — after all, who can be opposed to good science?

Thus, despite Congress' explicit intent that NAPAP be policy-oriented, NAPAP grew increasingly detached from the policy process. Over time, Congress learned that NAPAP had little to say that was of interest. By the late 1980s, acid rain again attracted public concern. Given NAPAP's lackluster performance, Congress and environmentalists were determined to resist any further delaying tactics, such as hand-wringing over scientific uncertainties or appeals for better science.

An acid-rain bill became likely when a new president, having campaigned as a friend of the environment, endorsed acid-rain controls and sent a bill to Congress. By that time there was little interest in new scientific findings. Most of the quibbling was over who should pay for acid-rain controls, how to reduce costs through "market mechanisms," and how quickly to implement controls. The question of the extent of needed controls — a quintessentially scientific question — was decided with virtually no input from NAPAP. Thus, to this day it remains unclear whether the ceiling on SO₂ emissions now established for the United States provides too much or too little protection against the effects of acid rain.

The Global Climate Change Research Program

The Bush administration's position on global-climate change closely resembles the Reagan administration's stance on acid-rain control a decade ago. Rejecting environmentalists' call for immediate action to abate greenhouse gas emissions on the grounds that there is insufficient scientific evidence, the president has initiated a multi-year, multi-billion dollar research program to address the issue.

The federal program is coordinated through the Committee on Earth and Environmental Sciences (CEES) within the Office of Science

and Technology Policy (OSTP) of the White House. Some 18 federal agencies are represented in CEES, with nine agencies receiving the bulk of the research funding. The National Aeronautics and Space Administration (NASA), with two-thirds of the total funding, is the largest player, followed by the National Science Foundation (NSF), with 10 percent of the total climate-change research budget. The remaining 25 percent is shared among the Smithsonian Institution plus a half-dozen federal agencies, including the Environmental Protection Agency (EPA), and the Departments of Agriculture, Commerce, Defense, Energy, and Interior. Most of these agencies participated in NAPAP.

A research program that begins to deliver answers 10 or 20 years from now will be of little value to near-term policy decisions.

Although the federal climate-change research program totals more than \$1 billion, no more than half of this represents new money. To a large extent, program managers have again repackaged their current projects to emphasize their relevance to global-climate change. Thus, most of the climate-change research represented in the current U.S. program is motivated by scientific curiosity or the desire to support existing programs, not policy issues.

NASA, for example, is using space probes to measure properties of the earth's atmosphere to better understand cloud physics and other phenomena affecting the global-energy balance. Oceanographers at the Department of Commerce's National Oceanic and Atmospheric Administration (NOAA) are pursuing studies of ocean currents that affect global energy transport. And in several U.S. agencies atmospheric scientists are developing more refined mathematical models of atmospheric transport and

chemistry for use in the general circulation models (GCMs) used to predict world climate.

Eventually, much of this research will help to provide a better understanding of how the climate system works and how human activity affects it. But the time scales for such payoffs are likely to be at least 10 to 20 years, perhaps longer. In the meanwhile, these science-oriented research activities will be of little value in informing policy decisions confronting the United States and other countries in the near term.

Over the next several years, for example, key policy choices will require judgments about how serious the impacts of climate change might be, how costly it might be for society to adapt, and how much "insurance" society should buy through reduced emissions. Timing is critical because, unlike the pollutants that cause acid rain, the greenhouse gases responsible for global warming are not readily washed out of the atmosphere. Indeed, once emitted, most greenhouse gases have very long lifetimes, on the order of 50 to 100 years or more. Thus, a research program that begins to deliver answers 10 or 20 years from now will be of little value to near-term policy decisions.

In addition to the problem of research timing, there is also the problem of coverage. In particular, few resources are being devoted to understanding the human implications of global-climate change, or to strategies for mitigating warming or adapting to future climate change. While some research in these areas is being pursued by EPA, NSF, and others, there is no government-wide coordination comparable in depth or breadth to the billion-dollar science research program. While an interagency Mitigation and Adaptation Research Strategies (MARS) program does exist as part of the U.S. climate program, the MARS program has yet to be empowered to pursue any policy-related research. The MARS program's present activity is limited largely to a survey of other government agency projects related to mitigating or adapting to climate change.

Yet, many of the most pressing issues before Congress require a better understanding of the economic and social consequences of taking actions now to avoid the possible consequences of future global warming. The federal program is not structured to elucidate such issues and direct resources to priority areas. Rather, there is a patchwork of projects lacking in overall direction and unlikely to produce the best information for U.S. policymakers who must draft legislation and negotiate treaties over the next decade.

In short, the global-climate change research program has been "NAPAPed." Unless things change, it is easy to predict the outcome five to ten years from now: some interesting research will have been completed, but the program will not have adequately informed the important policy decisions that will have been made, nor will it be producing results in a form to support pending policy decisions. Congress will have found that the climate change research program could not answer its most pressing questions, and will have learned to ignore it.

At some point, public and international pressure is likely to build to the point where either Congress or the White House will take action. Even if there are results of policy significance, they will likely be found too late to influence the policy process, or will not be communicated effectively enough to make a difference. After spending billions of dollars on climate-change research, Congress will make its decisions with little more scientific input than is available today.

What Needs to Be Done

The global climate research program needs to be focused on posing and answering key policy-relevant questions over the next decade and beyond. A billion dollar research program scattered across a dozen government agencies and hundreds of individual investigators is far too complicated. What is needed is effective

management tools that can provide the program with greater focus and improved ability to address both the policy and fundamental science issues. Two needs are critical. The first is to articulate the program objectives and adopt a management plan for achieving them. The second is to establish an independent review and priority-setting function.

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What precisely should the federal government's global climate change research program accomplish? Who is responsible and by what dates? Without specifying the goals of the inquiry, and the tasks needed to get there, it is impossible to say whether current funding is hopelessly small or far too generous. Without specifying responsibilities and schedules, there is no guidance for overall program management or ability to evaluate its progress. Does the current program address all of the important issues? It is impossible to say, since the goals of the program are not well specified. Will the inputs that different research groups need from one another be provided on time and in a usable form? Again, one cannot tell since timetables are not defined and data requirements remain vague.

The second critical need is to define program priorities, interactions, needed outputs, and the time frames in which they are required. This could be accomplished by redirecting only a tiny portion of the billion dollar program budget in a way that would vastly increase the program's policy relevance. Comprehensive integrated assessments offer the most effective means of achieving this objective. Integrated assessments are needed because the issues are complicated, multi-disciplinary, and highly interactive. Several parallel assessments are needed because the issues are too complicated

and too important to entrust to a single group. As elaborated later, such assessments should be carried out by multi-disciplinary teams commissioned by the Executive branch, Congress, and the private sector.

To maintain their objectivity and critical perspective, the assessments should be independently funded activities, able to conduct studies and publish findings without delay. Ideally, such assessments should have been started already, before major funding was initiated. But later is still better than never.

The Role for Integrated Assessments

The first policy-relevant question an assessment might pose is, "How important is global warming?" Though many estimates have been offered, a definitive answer to that question appears to be decades away. The current generation of general circulation models (GCMs) have many deficiencies. Thus, the ability to *predict* climate change accurately on a global or regional scale will require significantly improved understanding of the ocean-atmosphere-biosphere system, plus a new generation of computers able to perform the massive calculations required. Similarly, the ability to *measure* global warming that is unambiguously due to human activity will require at least another decade of careful measurements to discount the influence of natural variations in climate (as inferred from historical records).

Given these uncertainties, a key function of an integrated assessment is to survey the state of current knowledge regarding climate change, and to reach scientifically informed judgments about what is known and unknown, what the key uncertainties are, and where new research could aid the policy process most effectively. In doing this, special attention must be paid to the links among research projects in various disciplines, and to the magnitude and relative