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Keeping Climate Research Relevant

The federal government must avoid repeating the mistakes it made in studying acid rain.

Recent post-mortems of the National Acid Precipitation Assessment Program (NAPAP) confirmed what Congress and other key parties to the acid rain debate already knew: that the 10-year, half-billion-dollar interagency program to guide U.S. policy on acid rain control proved largely irrelevant to the effort to forge the new Clean Air Act last fall. Although NAPAP won praise for its scientific accomplishments, the program failed in its primary mission—to provide policy-relevant information in a timely manner. Now, government attempts to deal with the more difficult and far-reaching environmental issues associated with global warming appear to be headed down the same ill-fated path.

Global climate change has become the most important environmental issue on the world agenda. The potentially enormous human and ecological implications of global warming—rising sea levels, altered precipitation patterns, and damage to natural ecosystems—have generated concern. But the equally large uncertainties about the timing and magnitude of possible effects, coupled with questions about the costs and impacts of possible abatement measures, have thus far kept policymakers from agreeing on what to do.

European nations, especially the Scandinavian countries, have called for immediate, large-scale reductions in emissions of carbon dioxide and other "greenhouse" gases. The Bush administration has rejected such initiatives 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. The administration does, however, subscribe to the need for more research, boasting a billion-dollar annual federal research program.

Although the government's decision to commit resources to research on global climate change is laudable, the structure of the program and the research

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agenda indicate that this program may follow the precedent set by NAPAP. It is likely to produce a lot of good science but to remain largely irrelevant to the policy decisions that the United States and other nations face over the next decade.

How can a multibillion-dollar research program involving some of the best scientific minds be so predictably irrelevant? The lessons learned from NAPAP and the current plans for R&D on global climate change offer some answers.

What went wrong with NAPAP?

In the early 1970s, a number of U.S. scientists gained national attention by asserting that lakes in Adirondack Park were dying from "acid rain." Pollutants such as sulfur dioxide and nitrogen dioxide were mixing with water vapor in the atmosphere and returning to earth in the form of acidic rain, snow, or fog. A growing 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 the public outcry by demanding large reductions in emissions of sulfur oxides and nitrogen oxides from midwestern coal-fired power plants. However, the cost of reducing emissions was predicted to be high, leading to steep increases in utility bills for some consumers and severe unemployment in the high-sulfur coal industry. Pointing to the scientific and technical uncertainties surrounding the acid rain debate, the Reagan administration endorsed the Acid Precipitation Act of 1980, creating a 10-year research program—NAPAP.

Congress delayed acid rain legislation for nearly 10 years, in part waiting for NAPAP's results. In 1990, the program's 27 technical reports and three-volume Integrated Assessment were released. By that time, however, Congress had nearly completed action on the 1990 Clean Air Act Amendments, including acid rain emission controls similar to those called for in the early 1980s. Guided primarily by pre-NAPAP studies, Congress decided on a 40 percent reduction in total sulfur dioxide emissions—a decrease of about 10 million tons per year.

Part of what NAPAP reported, however, was that many of the early scientific claims about aquatic damage were exaggerated, that fewer lakes were being acidified than originally thought, and that reducing sulfur dioxide emissions 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 suggested that acid rain was not the environmental catastrophe widely portrayed a decade earlier. Yet these results never made it into the political debate.

One reason that NAPAP's findings were ignored is that they were not reported in a timely, credible, or understandable 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 that the new director had attempted to downplay the significance of acid rain effects in the interim assessment, significantly damaged NAPAP's credibility. NAPAP's original plan to publish additional interim assessments in 1987 and 1989 gave way to a hurried attempt to redirect the effort and finish on time.

Further, NAPAP's reports were largely unintelligible to Congress. The interim assessment was essentially a four-volume compilation of scientific findings, not the long-awaited, policy-relevant assessment of acid rain damages. 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 were passed into law. Again, NAPAP simply failed to communicate what Congress wanted to know.

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 could understand. NAPAP's final 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." Oh.

A major failure of NAPAP is that no serious effort was made to define policy-related research priorities and to then shape an appropriate set of projects and timetables to answer these questions in a meaningful way. Instead, independent government agencies,

driven by different missions, motivations, and expertise, largely pushed their own scientific research agendas. 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, the general quest for scientific knowledge rather than the specific policy needs of Congress dominated the thinking and programs of NAPAP.

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Major resources, for example, were committed to developing a complex atmospheric transport model that could allow someone to identify the sources of an acid aerosol falling on a particular region of the country at a particular hour of the day. Although this research clearly advanced the state of the science of atmospheric transport modeling and chemistry and resolved some longstanding technical debates among researchers, the substantial resources devoted to this endeavor did little to refine the existing policy options available to Congress.

On the other hand, 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 materials and human health.

The NAPAP effort was poorly focused for several reasons. First, the Reagan administration put little new money into NAPAP in the early years. Instead, it directed the Environmental Protection Agency and other NAPAP agencies to reprogram existing funds. Many program managers and agencies responded by altering descriptions of existing 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, turned a blind eye to this strategy. Thus, few of NAPAP's resources were new or newly targeted on policy-related acid rain research. Not until roughly halfway through the program did NAPAP's funding increase to address some of the key policy-relevant issues. But that effort proved too little, too late.

A second key reason for NAPAP's lack of policy relevance is that 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.

Third, NAPAP reflected the political ambivalence of its creators. 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 had been performed early in the process, the results might have been used to force legislation that the administration didn't 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. And 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. By the late 1980s, when acid rain again attracted public concern, 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 proposal to Congress as part of a new Clean Air Act. Most of the quibbling was over who should pay, 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.

Following in NAPAP's footsteps

The Bush administration's global climate change research program is "déjà vu all over again." The president has rejected environmentalists' call for immediate action to abate greenhouse-gas emissions on the grounds that we don't yet know enough. Instead, he has initiated a multiyear, multibillion-dollar research program. Although the federal government is expected to fund $1 billion in climate-change research for fiscal year 1992, no more than half of this represents new money for research. To a large extent, program managers have once again repackaged their existing projects to emphasize their relevance to global climate change.

Agencies throughout the government have created plans and research objectives to define the scope of their research on global climate change. Boasts of significant R&D activities on global climate can now be heard from nearly a dozen federal agencies, including the National Aeronautics and Space Administration, the Environmental Protection Agency, the National Science Foundation, and the Departments of Agriculture, Commerce, Energy, and the Interior. Most of these agencies participated in NAPAP. In addition, new interagency groups have been created for global climate research. The Committee on Earth and Environmental Sciences, involving representatives of 18 federal departments and agencies, coordinates the effort.

Most of this research, however, is motivated by scientific inquiry or existing programs, not policy issues. NASA, for example, with two-thirds of the global change research budget, 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. NOAA oceanographers are pursuing studies of deep 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 application in the general circulation models used to predict the world's climate.

Much of this research will ultimately help to gain a better understanding of how the climate system works and how human activity affects it. But, as with acid rain, there has been no attempt to ensure that the overall research agenda is addressing the most important problems, sequencing research programs appropriately, using the most cost-effective methods, and avoiding unimportant issues. Most important, there is no individual or agency responsible for seeing that the program produces timely results that are relevant to the most pressing policy issues confronting Congress and the president. A research program that begins to deliver answers 10 or 20 years from now will be of little value to the policy decisions facing us 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. Many of the most pressing issues before Congress require a better understanding of the economic and social costs of taking actions now to avoid the possible future consequences. But so far, few resources have been devoted to studying the impact of global warming on humans or the implications of strategies to mitigate or adapt to possible changes.

It is easy to predict the program's outcome 5 to 10 years from now. Some good 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. At some point, public pressure is likely to build to the point where either Congress or the White House takes action. But by then, Congress will have found that the climate-change research program could not answer its most pressing questions and will have learned to ignore it. Even if the program's results have significant implications for public policy, they will be found too late to influence the process or will not be communicated effectively or credibly enough to make a difference. In short, after having spent billions of dollars to study global climate change, Congress will make its decisions with little more scientific input than is available today.

Implementing integrated assessments

A billion-dollar research program scattered among a dozen government agencies and hundreds of in-

dividual investigators is far too complicated for loose management or benign neglect. What the global climate change research program lacks is effective management tools for focusing research and improving its ability to address policy as well as fundamental science issues. By redirecting a tiny proportion of the program's billion-dollar budget to these ends, Congress could vastly increase the timeliness and relevance of its findings.

The most immediate priority is to determine the feasibility, costs, and human implications of proposed policy measures.

The first priority is to articulate the program's objectives and adopt a management plan for achieving them. What precisely is the federal government's global climate change research program to accomplish? And by when? Is the current level of funding adequate to support these goals? Does the current program address all of the issues identified as important? Who is responsible for what research and by what dates? Will the inputs that different research groups need from one another be provided on time and in a usable form?

Once these questions have been answered, a mechanism must be established for reviewing results and reevaluating research priorities. We believe that a series of comprehensive, integrated assessments offers the most effective means of achieving this end. Integrated assessments are needed because the issues are multidisciplinary and highly interactive; multiple assessments are needed because the issues are too contentious and too important to entrust to a single group. Ideally, such assessments should have been started already, before major funding was initiated. But late is still better than never.

The primary functions of an integrated assessment are to survey the state of current knowledge regarding climate change and to reach scientifically informed judgments about what we know and don't know, what the key uncertainties are, and where new research could aid the policy process most effectively. Special attention must be paid to the links among research projects in various disciplines and to the magnitude and relative importance of uncertainties that characterize aspects of the problem. A key task is to determine the value of new information. Thus, the assessments strive to keep research focused upon identifying and reducing key uncertainties, especially as they change over time.

As they evolve, the assessments may also focus on issues related to research methods. For example, should ground-based or space-based platforms be used to survey changes in temperature and concentrations of gases at different strata in the atmosphere? The assessments may even venture to compare the relative merits of conducting certain types of research through government labs, universities, or industry.

Integrated assessments must also serve as a bridge between the scientific community and the policy community. Policymakers need to understand the limitations of what science can determine, and scientists must understand what the policy community really needs. By evolving in steps, the assessment process can facilitate convergence between what is desired and what is possible, within various time and resource constraints. The goals at the outset are to ask the right questions, influence the research agenda, and help ensure that appropriate priorities are established. As the assessments proceed, the goal is to ensure that the technical and often heavily qualified language of science is translated into terms that policymakers and lay audiences can understand and use to make more informed decisions. The need for more effective communication will require concerted attention if climate-change research is to fulfill its goal of informing public policy.

Finally, it is reasonable to ask: If integrated assessments are such a good idea, why hasn't one been started already? Can it really be true that no one in the federal program has done this yet? Certainly there have been a number of important efforts in the past few years to synthesize current knowledge about global warming. Recent studies by the Office of Technology Assessment, the Intergovernmental Panel on Climate Change, and the Panel on Policy Implications of Greenhouse Warming of the National Academies of Sciences and Engineering and the Institute of Medicine have been the most comprehensive. Research ef-

forts by the Environmental Protection Agency and the Department of Energy also are trying to integrate several pieces of the problem and develop policy guidance. The Committee on Earth and Environmental Sciences has begun to coordinate interagency efforts and to define the objectives of U.S. research. Workshops on policy-related research priorities are beginning to emerge.

Such efforts, while laudable and important, have not yet been directed toward managing the entire U.S. research program. These high-level study groups have not systematically addressed the issue of policy-relevant research priorities, nor have individual agencies or interagency working groups taken it upon themselves to initiate such an assessment. Executive branch agencies are unlikely to address these issues critically: Why risk "rocking the boat" by questioning the relevance of current research or suggesting new priorities—particularly for other agencies? Certainly such behavior is not typical of government researchers, nor is it normally rewarded. Thus, new leadership is needed to get the assessment activity started.

Tackling research priorities

The first policy-relevant question an integrated 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 used for global climate projections have many deficiencies. The ability to predict climate change accurately on a global or regional scale thus 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 the extent of global warming attributable to human activity will require at least another decade of careful measurements to discount the influence of natural variations in climate.

To improve the scientific capability to measure and predict climate change, an integrated assessment must identify the most critical areas of uncertainty. For example, what are the highest priorities for data collection in investigating ocean-atmosphere interactions, the ways these change over time, the role of clouds, and the effects of climate change on people and ecosystems in different regions of the world? It must determine how various research projects will contribute to answering these key questions: For example, how exactly will the millions of megabytes of data being collected by NASA and other agencies be used? Do data collection efforts serve a clear purpose tied to an identified need? How will the new global climate models being developed be used to make better decisions, and when? Will the models and data developed by different groups of scientists be compatible with one another? What measures can be taken to avoid large gaps and incompatibilities across related projects?

At the same time that the assessment helps focus long-term research and reduce key uncertainties, it also must ensure that the global climate change research program informs the decisions that confront policymakers in the very near future. The time required to gain a fundamentally better scientific understanding of the climate system is likely to be long (barring any major surprises or breakthroughs, which certainly cannot be ruled out). In the meantime, the most immediate priority is to determine the feasibility, costs, and human implications of proposed policy measures to mitigate global warming.

Applied research to evaluate mitigation options that might be undertaken in the coming decade is especially important for two reasons. One is that domestic and international pressures may force Congress to draft legislation and negotiate treaties long before scientists have arrived at a better understanding of climate-change effects. If so, policy-oriented research (including substantial efforts in the social sciences) can help identify the most feasible and cost-effective short- and medium-term options. A second reason is that if longer-term mitigation strategies become necessary, their success may well depend on actions taken now. Policy-focused research can help identify the steps needed to lay the groundwork for developing new technologies, for example, or altering patterns of energy demand.

Mitigation strategies include measures to reduce greenhouse-gas emissions, such as controls on the use of fossil fuels and chlorofluorocarbons (CFCs); land-use policies, such as those related to agriculture and deforestation; population control; and so-called geoengineering measures to alter the earth's radiative balance, either indirectly, such as by reforestation, or

directly, such as by seeding the upper atmosphere with fine particles. To date, relatively little research has been undertaken in many of these areas. The bulk of research on reducing greenhouse-gas emissions has focused on carbon dioxide. Other important greenhouse gases, such as methane and nitrous oxide, have received comparatively little attention. CFCs have been studied primarily because of their role in stratospheric ozone depletion, but the warming potential of CFCs and their substitutes also requires attention.

The government should undertake an in-house assessment in parallel to those conducted by nongovernmental groups.

An initial task for the integrated assessments would be to review all current research related to potential mitigation strategies for all greenhouse gases. This would cover research on all areas of energy utilization and many non-energy activities. The next task would be to recommend where this research should be expanded, integrated, or redirected to address the anticipated needs of policymakers. For instance, more research is needed to determine the true cost of energy efficiency improvements. A well-focused assessment can help deliver such policy-relevant information more quickly.

A related priority for integrated assessments is research on adaptation strategies, which entail measures such as moving housing settlements inland in response to rising sea levels, developing new agricultural crops suitable for a drier or warmer climate, improving weather forecasting systems to anticipate storms or tornadoes, and a host of other actions that will be necessary if the climate does change significantly. In many ways, this is a tougher problem than developing mitigation strategies, since it requires the capability to anticipate climate changes at the regional level—a capability we do not currently have. Nonetheless, research in this area may identify some robust strategies for which near-term policy actions, such as those related to coastal zone management, water systems infrastructure, agricultural development, and other land-use issues, may be desirable. Estimates of the costs of adaptation strategies need to be refined as well.

All this is not to suggest that long-term basic and applied research to understand Earth's climate system should be curtailed. Indeed, such long-term research will undoubtedly continue to consume the largest share of the federal research program. Nor is it necessary that all research have a clearly defined mission. Given the enormous uncertainties involved, adequate support for unfettered basic research driven by scientific curiosity remains a healthy and essential component of any national program. Integrated assessments can, however, help to ensure that more research is ultimately of value to policy decisions. The assessments can help find holes, identify needed links, and sort out priorities among current projects. In short, they can help increase the odds that the shortcomings of NAPAP won't be visited on global climate research.

Along parallel tracks

To be credible and effective, the assessment process must be undertaken simultaneously by several groups of researchers. Integrated assessments are too important to be left to a single group. Just as several research teams are pursuing the development of large-scale global climate models, multiple teams are needed to guide the wide-ranging efforts on global climate research.

The assessments should proceed along two parallel tracks. One track would draw assessment teams from nongovernmental organizations, such as universities or nonprofit research groups, and be funded jointly by public and private sponsors. The advantages of nongovernmental groups include the absence of direct political pressure, the freshness of an independent perspective, the freedom to recruit the most qualified people, and the ability to publish findings without undue delay. The primary disadvantage is that those undertaking the assessments have little say over the agencies whose work they hope to influence.

The government should undertake an in-house assessment in parallel to those conducted by nongovernmental groups. The primary advantage of an in-house assessment is that government researchers would automatically participate; they could not readily ignore the assessment or its findings. Although it is

difficult to ensure that an in-house program can maintain the independent, critical perspective essential for effective program management, the existence of independent assessments outside government can help keep attention focused on the issues and provide a benchmark for evaluating the government effort.

The nongovernmental assessments, conducted by multidisciplinary teams of experts in the natural sciences, social sciences, engineering, and public policy would establish links with other researchers in academia, government, and industry and report their findings periodically. One such effort already is under way at Carnegie Mellon University, with core funding from the Electric Power Research Institute and the National Science Foundation. The major foundations as well as individual government agencies or interagency groups such as the Committee on Earth and Environmental Sciences could take the lead in fostering such programs.

The leaders of foundations, corporations, public-interest groups, and government agencies also should exert their personal influence to support integrated assessments and stress the need for such assessments to senior government officials. Congressional committees, private-sector groups, and professional societies such as the American Association for the Advancement of Science can provide forums for presenting and comparing the results of integrated assessments and establishing their credibility. Periodic conferences, publications, or public hearings will give the assessments visibility in scientific and policy circles.

Either the executive branch or Congress will have to take the initiative to sponsor the in-house governmental assessment. In the executive branch, the science advisor, the EPA administrator, the White House chief of staff, or the president himself are among those who could take the lead. Similarly, key senators or House committee chairmen could sponsor legislation to mobilize government and nongovernment assessments and monitor their progress.

If the lessons of NAPAP are borne in mind, a government assessment program for global climate change should be of significant value. Several actions could help overcome the institutional barriers that thwarted NAPAP. The assessment director could be confirmed by Congress and given a regular reporting schedule that would underscore the importance of the activity. A well-balanced oversight board of prominent scientists, business leaders, and political figures can help lend the political clout, scientific influence, and intellectual independence needed to make the assessment effective. The assessment program must also have an independent budget and staffing authority.

The National Academy of Sciences could also participate in defining the initial scope and organizational framework of the assessment program and evaluating its progress periodically. The imprimatur of an Academy recommendation would give the assessment added weight and visibility.

No assessment, whether inside or outside the government, can be assured of success in moving climate-change research toward policy-oriented objectives. Some agencies will see an assessment as intruding on their turf and attempt to ignore it, resist it, or scuttle the effort. Ultimately, success will depend on the merit of the findings and recommendations, the credibility and impartiality of the assessment teams, and the willingness of decisionmakers in the public and private sectors to endorse the process. Nonetheless, the political incentives and visibility created by an interested Congress, coupled with the external pressures of independently funded assessments outside of government, can significantly increase the likelihood of a successful effort.

The commitment of policymakers and research managers to support this effort is essential to its success; the ball is in their court. Does the White House really want global climate change research to inform public policy? Do members of Congress, agency heads, and private-sector leaders really want research to be productive and more relevant? The means for improvement are clear—as are the consequences of pretending there is no problem.

Recommended reading

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