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Orchestrating Environmental Research and Assessment[1]

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Abstract. When pressing national environmental problems must be solved, and serve as the justification for large infusions of public funds, mechanisms must be found to assure that the requisite research and assessment are performed. Large, managed programs seem to offer a way to direct energies in the needed directions, but individual creativity and intellectual curiosity must also be fostered through investigator-initiated studies. Research results cannot be achieved to meet imposed deadlines, and assessment in the face of uncertainty must be given due attention. This paper introduces five subsequent papers, four of which present perspectives on the National Acid Precipitation Assessment Program, as a model for coordinated research and assessment programs, and one that presents plans for a national and international research effort on biodiversity.

Key words: acid deposition; acid precipitation; acid rain; biodiversity; environmental assessment; environmental research; global change; NAPAP; pollution.

As the world's environmental problems mount, the importance of supporting ecological and related research that addresses societal needs is becoming increasingly recognized. The Ecological Society of America (ESA), responding to this challenge and opportunity, launched its Sustainable Biosphere Initiative (SBI) (Lubchenco et al. 1991), calling for an enhanced program of research, education, and outreach directed towards the problems of global change, biological diversity, and the maintenance of sustainable ecological systems. The details of how that initiative was to be developed and implemented were left to the further deliberations of other committees and workshops, to be coordinated through a carefully planned program (Risser et al. 1991). That program has already had success in expanding SBI to the international level (Huntley et al., *in press*), and has won the endorsement of other professional societies, agencies, and legislators.

The orchestration of large-scale coordinated research and assessment programs, however, raises difficult questions, and a range of strong feelings and opinions. Lubchenco et al. (1991) emphasize the importance of individual investigator-based studies, as the wellspring of creativity and scientific innovation; and any reading of the history of scientific progress provides substantial support for this point of view. Yet, when pressing national problems must be solved, and serve as the justification for large infusions of public funds, ways must be found to ensure that research is performed that leads to the solution of those problems and builds the knowledge base for addressing future problems. Such research must involve long-term and exploratory studies that develop basic principles, as well as the application of those principles to the solution of near-term problems. A critical challenge facing our society is to find ways to achieve the necessary balance. The most immediate need is the coordination of global-change research, currently the mandate in the United States of the Committee on Earth and Environmental Sciences (CEES) of the Federal Coordinating Council for Science, Engineering, and Technology (FCCSET); but regional air and water pollution, biological diversity, sustainable use, and other real and potential problems demand equal attention. The considerable support that has become evident for a proposed National Institutes for the Environment (NIE) (Committee for the NIE 1990), and the ongoing National Academy of Sciences committee to examine the federal coordination and support of environmental research in the United States, are reflections of the recognition of the urgency of the problems.

As we struggle to design federal research and assessment programs to meet the needs identified in the SBI, it is helpful to look at models that already exist, and identify their strengths and weaknesses. The most obvious candidate is the National Acid Precipitation Assessment Program (NAPAP), a decade-long effort involving thousands of researchers and more than half a billion dollars in federal funds. NAPAP has had a roller-coaster history, and garnered mixed reviews. Though its justification was to provide critical data, analyses, and assessments to inform the debate on the reauthorization of the Clean Air Act, a combination

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of missed deadlines and legislative impatience resulted in the key bills being passed before the NAPAP final reports could be issued. The Oversight Review Board (ORB), of which I was a member, in its report to the Joint Chairs (ORB 1991), found substantive pluses and minuses in the record of NAPAP, and fundamental lessons for improvement in the design of future programs.

Even among the members of the ORB there was a wide diversity of views on the degree of success represented by NAPAP; that diversity is only a microcosm of the spectrum of views in the scientific community at large. To attempt to represent that spectrum, I invited a set of perspectives from some of those who, in my opinion, had something to communicate to the readers of *Ecological Applications*. Of course, not everyone who was invited was able to accept my invitation to contribute, given the unreasonable deadlines I had to impose; and there are others with equally valid perspectives who could have been invited. Nonetheless, the views of Milton Russell, the Chair of the ORB; Ellis Cowling, one of the most distinguished of acid rain researchers, and the liaison between the ORB and the NAPAP effort; and noted ecologists Orie Loucks and David Schindler, two leading experts on acid rain, should be of considerable interest to our readers.

These authors of the first four papers that follow were asked to provide personal perspectives, with special attention to lessons learned that could be applied to future programs, such as global-change research and assessment. The range of views presented is extremely broad, and uncensored. Among others whose views are not represented, probably the most articulate and outspoken has been Gene Likens, one of the leading and most knowledgeable acid deposition researchers in the world, and a former President of the ESA. Likens (1992) has strongly criticized NAPAP, and argued that it is a poor model for future efforts. In this view, science cannot be forced to provide answers to policy questions on a fixed timetable, and this "fatally flaws" efforts to create such programs: "The NAPAP effort was 'big' science on a forced march, with platoons of consultants, civil servants and scientists stepping to the cadence of a federal bureaucracy" (Likens 1992). In support of this view, the NAPAP ORB (ORB 1991:26) concedes that "science is an on-going process that does not fit neatly into a policy-driven timetable and may be antithetical to it."

A second, and major, criticism of NAPAP relates to the inadequacy of the review process:

> *Indeed, the NAPAP effort has added to our understanding about acid rain and air pollution. . . . But the NAPAP reports were not unlike student term papers on an assigned topic where the grades can range from A (=excellent) to F (=failure). Yet unlike publication in peer-reviewed journals, there was no real option in the NAPAP effort to "fail" poor efforts and reject their publication (i.e., grade of F). The customary quality review process for science—publication in peer-reviewed journals—was largely by-passed because of time constraints, sheer volume and "need" for "answers." The result was voluminous interim reports combined with compact "sound bites" and easily digested headlines for the news media . . . appearing just in time for the closed-door congressional debates on clean-air legislation* (Likens 1992)

Additional criticisms can and have been directed at NAPAP, its specific conclusions, and the way they were represented (e.g., Flynn 1991, Likens 1992). Likens states that his comments ". . . should not be interpreted as critical of the NAPAP scientists or their science, instead [he] is critical of the organization and process whereby the scientific assessment was done" (Likens 1992). On the other hand, others have found much to praise in aspects of NAPAP's performance. Evaluation of NAPAP, however, is not the point of the series of articles that follows; rather, my objective in inviting these papers was to explore how the NAPAP model could be improved to address future national and international environmental problems. The Oversight Review Board began this process, viewing one of its primary mandates as providing suggestions for improvement (ORB 1991; Russell 1992 [this issue]). These included:

a) "Match institutional remedies to problems." The size of the program, and the timetable on which results are needed, determine whether a large coordinated program is feasible or appropriate: ". . . in the ORB's judgment, there should not be many NAPAPs in the nation's future."

b) "Obtain and maintain political commitment." I would also emphasize that scientific judgments must be insulated from agency pressure to modify findings to conform with agency positions.

c) "Take steps to assure continuity."

d) "Give assessment primacy."

e) "Provide for independent external programmatic oversight."

f) "Understand the role of science and how to use it."

g) "Take special care with communication."

Likens (1992) presented his own list of recommendations for "an assessment program [his underlining] for large multifaceted environmental programs such as global climate change." Many of these recommendations are in substantial agreement with the ORB report, though Likens (*personal communication*) has been strongly critical of other aspects of that report, which he feels was not sufficiently searching in identifying NAPAP's failings and weaknesses. Likens recommends that

(1) focus should be on an assessment of environmental risks, as well as benefits of mitigation, with annual, or similarly regular, reports to the primary legislative body of the governmental entity (e.g., U.S. Congress). [NAPAP reported infrequently and to a consortium of federal agencies.] The reports should be prepared for policymakers in an understandable manner, with uncertainties clearly stated.

The ORB did not go as far as to suggest direct reports to Congress. It did, however, agree that

success also is fostered if formal interim assessments are made at regular intervals, not just final assessments at the end, [and that] the process of reading those findings must . . . be clear and unambiguous if the findings are to be accepted as credible. Obviously, this includes presenting uncertainties clearly and informing and educating the media and the public on what they are and what they mean.

Likens (1992) goes on to recommend that

(2) A partnership . . . between policy makers and scientists should be forged in which the best answers at the time are used to develop policy;
(3) there must be a strong science program advisor to the executive officer of the governmental entity (e.g., to the U.S. President);
(4) there must be a strong, practicing scientist to direct government-sponsored assessment programs, with authority to lead the program and to be evaluated by the scientific leadership on the basis of that leadership . . . ;
(5) the program should be led by ideas and questions relative to assessment needs, not by funding and top-down bureaucracy;
(6) a significant percentage (e.g., 15–20%) of the funds for an assessment program should be devoted to competitive, investigator-initiated, innovative research . . .

The ORB, agreeing with this general recommendation, concluded that

setting aside a small percentage of funds from the policy program for competitive, investigator-initiated grants would likely be a good idea. This could open new sources of inquiry, assure that broader participation was not precluded, and provide a real measure of interchange and ownership among scientists not otherwise directly engaged in the program.

Indeed, both the ORB and Likens documents operate on the premise that the best science has been and is likely to be done outside orchestrated programs, except through the above mechanisms, and that it is the assessment function for which such orchestration is needed. Likens' final point, an essential one, is that

(7) a significant amount of the research results must be published in a timely fashion in peer-reviewed journals to establish scientific credibility.

Acid deposition, air pollution, and global change are not the only urgent problems on the nation's agenda. The problem of maintaining biological diversity has emerged, through the efforts of scientists and concerned citizens, as one of those for which research and action is most critically needed (Ehrlich and Wilson 1991). To that end, the International Union of Biological Sciences (IUBS) recently organized a planning workshop to develop an international research program in biodiversity, under the leadership of the distinguished biologist, Otto Solbrig. The objectives of this workshop were entirely consonant with those laid out in the SBI report (Lubchenco et al. 1991). In focusing on research rather than assessment, it provides a stark contrast with NAPAP. Indeed, in calling for efforts on research, education, and management, the SBI recognized the distinct needs in each area, and their mutual interdependence. As the ORB report (ORB 1991) emphasizes, science can "inform" the decision-making process, but is not a substitute for it. For research programs, with different mandates and different timetables, a different set of criteria apply than those given for assessment programs. Solbrig, in his invited companion to the NAPAP papers, provides the recommendations of the IUBS workshop, focusing properly on the research component.

Increasingly, ecologists will be called upon to participate in policy-relevant research, and research-relevant policymaking. The development of sustainable systems, the most important new initiative called for in the SBI, is the next challenge to be faced. *Ecological Applications* will continue to be a forum for informed opinion by those most knowledgeable about work at the science–policy interface. The papers in this issue, by five distinguished and thoughtful scholars, comprise a worthy beginning.

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