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A VIEW OF NAPAP FROM NORTH OF THE BORDER[1,2]

D. W. SCHINDLER
Departments of Zoology and Botany, CW 312 Biological Sciences, University of Alberta, Edmonton, Alberta, Canada T6G 2E9

Abstract. Despite widespread political interference with programs and confusion of science and policy, the NAPAP program has produced a number of sound, if not outstanding, publications documenting the effects of acidic deposition. NAPAP's outstanding strengths in aquatic science are in paleoecology and spatial surveys of chemistry. NAPAP has severe shortcomings in documentation of temporal trends, in deducing biological responses to acidification by organisms other than fish, in considering the effects of nitrogen deposition, and in considering results from countries other than the USA. Summaries of the NAPAP program in 1987 and 1990 underrepresent the extent of damage caused by acidification, as documented elsewhere in NAPAP's publications and by the peer-reviewed literature at large. Overall, it represents a mediocre return for a large amount of investment, and is a poor model for future large, multidisciplinary science projects.

Key words: acid precipitation; acid rain policy; NAPAP.

INTRODUCTION

It is premature for me or anyone else to attempt an overall assessment of NAPAP, the National Acid Precipitation Assessment Program. I have only received some of the published State of Science and Technology and Regional Case Study volumes, and haven't had time to read more than a few of them. Many of NAPAP's studies are still not finished. However, I have reviewed many draft manuscripts either directly for the reports or for journals, and have seen a number of recent publications. I have also had periodic contact with many of the aquatic investigators for over a decade. There is some very good science in NAPAP, but not 570 000 000 dollars worth. On the basis of what I have seen, I hesitate to recommend it either as a blueprint for future mega-scale studies, or as a model multidisciplinary study.

NAPAP provides a remarkable case history. It includes political interference with the course of science, obfuscation of scientific conclusions, and delays in the release of first-class science that did not support political agendas. First-class American scientists who would not conform with NAPAP's political objectives were virtually isolated from the program. Yet NAPAP ultimately produced some very good science, and some of the best has now been published in prominent refereed journals and easily accessible books where it is readily available. If nothing else, the history of NAPAP proves that American science is resilient! To put NAPAP in perspective, it is necessary to review parallel American activities with respect to acid precipitation.

[1] Manuscript received 10 October 1991.

[2] For reprints of this Special Feature, see footnote 1, page 103.

ACID DEPOSITION RESEARCH BEFORE NAPAP

Many of the North American studies now regarded as classics in acid rain research were done before NAPAP or any other formal acid deposition program. The discovery that the problem was present in North America by Gorham and Gordon (1960), the proof that it was a widespread problem by Likens et al. (1972), and the documentation of rapid and devastating effects on fishes by Beamish and Harvey (1972) must be regarded as seminal works. In the late 1970s the NADP (National Acid Deposition Program), a small-budget operation administered by Ellis Cowling from North Carolina State University, was the only sign of an American program to study acid rain. Only a few hundred thousand dollars a year were available, allocated on the basis of peer-reviewed proposals. Funding was available directly to university investigators and even foreign scientists—indeed, NADP actually awarded our group funds for experimental lake acidification in Canada, although the ensuing rapid decline in relations between the United States and Canada over acid precipitation prevented us from ever accepting the money. This was also the era when straightforward negotiations were in progress toward a memorandum of intent (MOI) between Canada and the U.S. to control acid precipitation. The scientific basis for the MOI was provided by a group of knowledgeable scientists from both countries. Remarkably, the major issue at the time was whether Canadian emissions from a small proposed coal-fired power plant at Atikokan, Ontario, would cause significant acidification in the Boundary Waters Canoe area in northern Minnesota! This small initial focus broadened very quickly once the extent of trans boundary movement of strong acids and their precursors in the atmosphere was assessed.

NAPAP, 1980–1987

NAPAP was created to supercede NADP in 1980, just before the Reagan administration's assumption of power in January 1981. Funding was increased manyfold, and administration of funds was transferred from the trusted NADP group to the United States Environmental Protection Agency (EPA), Department of Energy (DOE), and other federal departments. For the next several years, NAPAP cannot be evaluated in isolation from the U.S. political agenda and the actions of powerful federal departments. It became particularly closely linked with policy branches in EPA (then in the notorious Gorsuch era) and DOE. Peculiar things happened. Knowledgeable American scientists on the MOI negotiating team were replaced by virtually unknown junior scientists, most of whom had no previous experience with acidic deposition. Almost all of them were skeptical that acid deposition was a problem. Dialogue over how to decrease acidic precipitation was replaced with the philosophy that "not enough is known." This view fit well with the American Political Agenda at the time, and with the hunger for new funding by U.S. aquatic scientists. Incredibly, the first MOI (1983) had two summaries reaching vastly different conclusions: one Canadian and one American! The American summary largely ignored all previous research on the acid deposition problem, even though some was internationally regarded as of high calibre.

Many U.S. scientists who were internationally known for the excellence of their acid rain work disassociated themselves from the NAPAP program. The lack of guidance from experienced scientists in its early years left NAPAP floundering, and the program appeared to rush off in all directions, sponsoring science that was not really pertinent to assessment of the damage caused by acid rain, or to developing useful emission control policies. The publications and talks on acid precipitation of such notable ecologists as Gene E. Likens, Eville Gorham, Orie L. Loucks, and Gary E. Glass seemed as if they were describing a different country than the one represented by NAPAP. The report of the U.S. National Academy of Science's Committee on the Atmosphere and the Biosphere (CAB 1981), which expressed the opinion that acid precipitation was an important environmental problem, was widely discounted as biased, even though the report was subjected to the Academy's usual stringent review process before publication. The sources of criticism proved impossible to trace, and no specific criticisms of the report's contents were ever committed to paper. For example, statements denigrating the report were attributed by the media to the President of the U.S. National Academy of Sciences and the Chairman of the U.S. President's committee on acid precipitation, even though both individuals denied that they had ever made such remarks! It was even rumored that the report was a "Canadian Conspiracy," due to the inclusion of several well-known Canadian scientists on the committee.

Curiously, the Reagan administration, widely known for its disregard for the environment, indulged NAPAP by allocating more and more money. Many huge acidification projects were begun, eventually involving >3000 scientists and a half-billion dollars. The "not enough is known" slogan was used by agency policy officials as license for both lavish scientific funding and for delaying any controls of sulfur oxide emissions. This stage of NAPAP reached its climax in 1987, when the executive summary of NAPAP's interim assessment (NAPAP 1987) stated that the acid precipitation problem was small and exaggerated—a statement that brought scathing criticism from eminent non-NAPAP scientists both within the U.S. and internationally (for example, see Roberts 1987).

I was among those critical of the 1987 NAPAP report. For brevity, I will give only one example of the many reasons for disagreement with NAPAP's conclusions. One key point of contention in the aquatic ecology part of the 1987 NAPAP report was its assertion that few American lakes were damaged by acid rain because their pH values were not <5. Studies in Scandinavia, Canada, and the U.S. had already shown that biological damage began to occur at pH values <6.0 (Okland and Okland 1980, Eilers et al. 1984, Schindler et al. 1985), but these were ignored by NAPAP's interim report. NAPAP also used absolute pH rather than pH change to assess damage; for example, a lake following the same course as our experimental Lake 223, where pH decreased from 6.5 to 5.05, would be classified as undamaged by NAPAP's criterion, despite evidence from our work that an overall decrease in the number of species of 30–35% would result (Schindler et al. 1985). On the other hand, a bog lake with a natural pH of 5 would be considered as damaged.

Despite the total lack of scientific evidence, NAPAP concluded that rapid reductions in acidifying emissions would have little positive effect on lakes (a conclusion now refuted by studies in Canada, Norway, and Sweden, reviewed by Schindler et al. 1991).

Key pieces of NAPAP research that showed declining pH values in the 20th century (for example, paleoecological studies in the Adirondacks) were mysteriously omitted from the 1987 interim report, even though results had already been widely exposed in international scientific meetings. These shortcomings in NAPAP's report led the Canadian Minister of Environment to refer to it as "voodoo science." NAPAP was regarded by the international scientific community as a laughingstock. A number of key politicians entered the fray, attempting to silence critics of NAPAP with threats of defamation lawsuits, termination of research funding, or, in the case of scientists outside the U.S. like myself, objections through diplomatic channels. Shades of Lysenko and the McCarthy era! (This will be a juicy chapter in my memoirs someday.) This stage of NAPAP terminated with the resignation of NAPAP's director, J. Lawrence Kulp. Shortly thereafter, a NAPAP representative officially retracted the summary volume at a Congressional hearing (Loucks 1992).

NAPAP 1987–1990

Resurrection of scientific credibility became a major objective of the final phase of NAPAP, under James Mahoney. Scientific results were exposed to criticism in international meetings and by solicited peer reviews of regional case histories, state-of-science documents, and journal manuscripts. Scientific criticisms of the program's findings were printed in public review drafts of the document. As a result, the final >6000-page report of NAPAP, and resulting primary publications, are vastly different from the interim assessment. The 20th-century decline in pH of a high proportion of acid-sensitive Adirondack lakes is conclusively deduced from paleoecological evidence, and NAPAP concludes that acidic deposition has caused the acidification of many lakes and streams in the eastern U.S. In the Adirondacks and elsewhere in the Northeast, numerous populations of trout and forage fishes have been lost. Seventy-five percent of acidic streams and 47% of acidic lakes in the eastern U.S. are acidic because of acidic deposition (Baker et al. 1991). NAPAP now agrees with others that lakes begin to become biotically impoverished at pH values below 6.0, an acidity threshold 10-fold lower than that used in the interim assessment. It also concludes that reducing sulfur emissions would cause lakes to recover rather rapidly (though real data from Canada and Scandinavia, where sulfate emissions have now been reduced for 10 yr or more, still indicate that the recovery will be more rapid than that predicted by the unvalidated, expensive NAPAP models). Only key NAPAP officials know whether this turnabout was the result of declining political interference in the post-Reagan era, or more enlightened project management—but in either case Mahoney brought about a major improvement in NAPAP.

Incredibly, the executive summary of NAPAP (called "Draft Assessment Highlights") once again reads like it is summarizing something other than NAPAP's science. Even though NAPAP reports describe a scientific problem of enormous proportions, the executive summary greatly understates the problem. The strong effects of acidic deposition on eastern freshwaters documented in the report and, for example, by Baker et al. (1991) and Sullivan et al. (1990) are not mentioned. Effects of acid deposition on health, soil, and forest problems are made to sound as if NAPAP research has given them a clean bill of health, while the actual reports either show strong correlation with acid rain or that no conclusions can be drawn until further studies are done. As a result, considerable mistrust of NAPAP and its programs remains among scientists, as well as in the environmental community (see, for example, Moore 1991, Loucks 1992). The television program "60 Minutes," where Mahoney and scientists cynical about the severity of the acid precipitation problem soft-pedalled the results of NAPAP and other recent acidification studies, served to heighten the mistrust. Clearly, the "Highlights" agenda is not a scientific one.

NAPAP's Lists of Publications

The huge size of NAPAP's final report makes it very unlikely that any one scientist will ever read it thoughtfully from cover to cover. I certainly don't intend to. There are gold nuggets, but separating them from the pedestrian is analogous to placer mining. NAPAP references also contain an extremely high proportion of "gray literature"—meeting abstracts, intra-agency reports, conference proceedings in publications that are not readily accessible and are not peer reviewed. The list is also padded, at least to a slight degree. For example, I was surprised to find a paper that I co-authored on the list of NAPAP's publications, even though it was not supported by NAPAP and was totally unrelated to any NAPAP objective. Likewise, Gene Likens's book on Mirror Lake is on NAPAP's list, even though it is not really an acid precipitation study, and was never funded by NAPAP (G. E. Likens, *personal communication*). Some papers are also listed in more than one category, making the total list of publications appear larger than it really is.

A perusal of titles and authors, plus what I have read, leads me to believe that the parts of NAPAP dealing with atmospheric transport, chemistry, and paleoecology are quite strong, the agricultural and fish-related parts mediocre, the forest parts weak (largely due to being late in starting), and aquatic biology other than fish and paleoecology are almost non-existent. Modelling efforts, both of water quality and atmospheric transport/transformation are also quite sophisticated, although the scarcity of field studies leaves most of the models unvalidated and curiously devoid of ecological content. It is discouraging that in the total list I could not identify one real "breakthrough" in the understanding of acid deposition, though there are some good, solid pieces of documentation.

The price tags for some of the studies are outrageous. Canadian scientists used to joke that the money used by NAPAP for visual aids in meetings would be enough to fund the entire Canadian acid precipitation program.

Among the problems scarcely touched by NAPAP are: nitrogen emissions and deposition, episodic acidification, ecosystem-scale and long-term studies, and studies of the effects in Canada of American emissions (for example, in Report 9: Current Status of Surface Water Acid-Base Chemistry [NAPAP 1990], Canada merits 34 pages, the world outside North America only 15). Cook (1988) edited an interim report on the acid rain problem in Canada for NAPAP.

Megaprojects and Megamodels: How to Blow $570 000 000

NAPAP represents the ultimate American fixation with scientific megaprojects, megamanagement, and megamodels. Its Regional Acid Deposition Model (RADM) consumed millions of dollars, thousands of man-hours, and years to build. Hourly emissions of pollutants from all major sources, their transport and reactions in the atmosphere, and deposition patterns are combined. Yet one must agree with Roberts (1991) that the model does not go significantly beyond the hazarded guesses of a 1983 National Academy Committee (NAS 1983), that local differences in emissions did not matter when managing a problem on a large regional scale. The RADM model was not completed in time to affect sulfur oxide control policies (in all fairness, it was not designed to be), and it probably would have had little effect on Congress's decision to control sulfur oxides even if it had been available.

Several plans to acidify entire watersheds were afoot in the early 1980s, and several multi-million dollar proposals for such programs were circulated by NAPAP in the ecological community. These proposals revealed an interesting difference between agency funding in the U.S. and Canada. At one point a group of EPA administrators and internationally reknowned ecologists descended on the Experimental Lakes Area to view a watershed-scale acidification project that we had "bootstrapped" on a wetland system. Using a low-head site near a lake, which allowed us to use the research station's garbage tractor to power irrigation pumps, an election-year unemployment reduction program, and some moonlighting by volunteers, our project cost <$50 000 to construct. It has run for 9 yr, for <$100 000 per year. (After 8 yr the recovery phase of this study was begun in 1991.) We had plans to do the same with a nearby forested watershed, but were never able to find the necessary $250 000, despite good scientific reviews. An attempt to obtain NAPAP money for a group of University of Minnesota scientists to participate in these studies, with matching funds provided by Fisheries and Oceans Canada also failed, despite excellent reviews and a proposed budget an order of magnitude lower than proposed for other sites. Politics were, and still are an important part of NAPAP's agenda.

When a NAPAP-sponsored watershed acidification was finally launched in 1988, it had a multimillion dollar price tag. It ran for only a few months before its budget was cancelled, much to the chagrin of the many scientists who spent months designing and planning it.

Millions of NAPAP dollars were also spent on the National Surface Water Survey (NSWS), "snapshot" late-summer chemical fingerprints of lakes and streams done by using helicopters (Linthurst et al. 1986, Landers et al. 1987). These studies yielded a very nice, if expensive, data set for late-summer chemistry, which we (Schindler et al. 1989*a, b*) and others have used to construct models of damage to lakes from acidic deposition. But there is no temporal analog to this massive study. No long-term studies were done in NAPAP, despite its 10-yr lifetime, despite the fact that rates of acidification were one of the key issues in the acid precipitation debates. Even seasonal studies done as a second NSWS study are still to be reported.

Perhaps the best value of large NAPAP aquatic programs was the paleoacidification study. This study was actually begun by the Electric Power Research Institute, in its PIRLA (Paleolimnological Investigation of Recent Lake Acidification) study. Over 20 scientists from a variety of institutions participated. It joined NAPAP mid-stream. Using the dated fossils of pH-sensitive diatoms and chrysophyceans in lake sediments, the PIRLA group showed conclusively that most of the 40% of lakes in the Adirondacks with original pH values <6.0 had acidified in the 20th century. To the disappointment of cynics, the timing of lake acidification verified that acid precipitation rather than land-use changes had caused the declines (Charles et al. 1990). The extent of acidification in other areas of the U.S. was also assessed, and shown to range from moderate to almost nonexistent, depending largely on the acidity of deposition. Many of the participating scientists are now analyzing other environmental problems, such as effects of trace metals and climatic warming. Due to decades of disregard for long-term monitoring and biological surveys, this paleoecological group will have to provide the background information for assessments of change in American ecosystems for decades to come, an example of how NAPAP's total value will only emerge in the decades ahead.

As mentioned above, NAPAP's assessments have been based on data collected in the "lower 48 states" plus a very cursory review of results in Canada and other countries (see also Roberts 1991). The fact that acidified lakes in Canada numbered in the hundreds of thousands, rather than merely thousands (for example, Minns et al. 1990), is unmentioned, but it should certainly be an important consideration in U.S. policy development.

The passage in 1990 by Congress of sulfur oxide emissions controls as amendments to the Clean Air Act has resulted in much criticism, most from proponents of the coal, oil, or power industries, who argue that the environmental benefits will be too few to justify the enormous cost of control technology. I doubt whether this would be the case if Canadian environmental benefits were considered, a shortcoming that NAPAP could still remedy. Optimistically, NAPAP may look better in retrospect. Some of the young scientists who began their careers with NAPAP with little guidance have emerged as mature, respected scientists. Many of NAPAP's most important projects began late or were delayed for bureaucratic reasons, and results will still be forthcoming.

What More Should We Have Expected?

What more could be expected of half a billion dollars? About 10-fold more. Only one ecosystem-scale experiment, the Little Rock Lake Project, was included in NAPAP, despite the internationally recognized need for such studies, and the presence of several internationally famous ecosystem experimentalists in the community of U.S. acid rain scientists. Without frequently applied peer pressure to EPA from the international scientific community, the Little Rock Lake study would have been terminated before it could fulfill its study objectives. Other proposals for ecosystem-scale projects proposed Pentagon-style budgets, bureaucratic and logistic nightmares for project management, and peculiarly intractable hypotheses or study objectives. It is obvious that the talent for designing affordable, tractable ecosystem-scale studies is still the province of a few individuals, not megaproject panels.

I believe that a few well-designed experiments initiated early in NAPAP could have provided conclusive tests of some key hypotheses. A few decade-long studies in areas like the Adirondacks would have yielded valuable information on long-term trends in lake chemistry and biology. Even the deployment of a few biologists with nets to document the presence or absence of acid-sensitive taxa would have allowed some assessment of the extent of biotic impoverishment in lakes and streams. The U.S. still does not have the background biological survey that S. A. Forbes called for over a century ago (Forbes 1883)!

NAPAP's fish results for the Adirondacks underscore the urgency of this undertaking: the disappearance of fishes in the past several decades from all causes was three-fold higher than could be attributed to acid deposition alone. In the case of brook trout, 32% of populations had disappeared in <20 yr. For forage fish a whopping 45% of populations disappeared in the same period! Causes of this biotic impoverishment are not described in detail, but are reported to include reclamation, changes in stocking policy for sport fish, and introductions of exotic species as well as lake acidification. Our modelling results also suggest widespread biotic impoverishment of lakes in the northeastern U.S. (Schindler et al. 1989*a, b*).

What Could NAPAP Still Do?

NAPAP will continue for some time, and useful studies could still be undertaken. Damage to Canadian aquatic ecosystems caused by American emissions has still not been addressed. No analyses of the effects of recent control policies on ecosystems in either country have been done. I am sure that inclusion of even a rudimentary analysis of the Canadian situation would stem much of the recent criticism of the amendments to the Clean Air Act by pro-industry lobbyists. Important studies also remain to be undertaken in the U.S. Documenting rates of ecosystem recovery under reduced sulfur oxide emissions is essential for evaluating future policy; this is always better done in hindsight, and its value is almost always overlooked. NAPAP could still undertake a biological survey, which would give a baseline for evaluating recovery of lakes, as well as for biotic impoverishment caused by stresses other than acid rain. The effects of nitrate and ammonium deposition, already recognized as important in Europe, deserve more study (Kelly et al. 1990). Episodic events and nitrogen deposition remain as largely unassessed problems. Studying the recovery of Little Rock Lake would provide valuable insight into how rapidly and completely acidified lakes might recover. One reason that the NAPAP assessment to date still minimizes the effects of acid deposition on freshwaters is that it excludes lakes of <4 ha. Some of these problems will be corrected by second-phase seasonal studies that are under way, but still unreported. The scientific expertise developed under NAPAP would be invaluable in undertaking these tasks, and one hopes that it will still be done.

Did NAPAP Make a Difference?

Despite some very good science, it is difficult to find examples where NAPAP studies greatly changed the world view of acidic precipitation. Few of the predictions made in the 1970s by reputable scientists were altered by NAPAP's findings. Some would dispute this. They state that some scientists in the 1970s were predicting a continued decline in the pH of lakes. With some exceptions (for example, Dillon et al. 1987) this has not happened. But the predictors assumed that sulfur oxide emissions would continue to increase, or at least remain constant. Instead, sulfur oxide emissions have declined, by over 50% in eastern Canada and 25% in the Northeastern U.S. The Clean Air Act of 1970, which took nearly a decade to implement fully, caused industries to begin cutting sulfur emissions, causing lakes to deteriorate less in the 1980s than had been predicted.

How could NAPAP have been done better? In the climate of the early 1970s one would have hoped for a cooperative U.S.–Canada acid rain program that ignored political boundaries and agendas. Designed by the best scientific minds in both countries, it could have resulted in studies of unsurpassed quality for much less money. Logic dictates that problems of international scope require coordinated efforts that are free from restrictions imposed by national boundaries or departments controlled by politicians. Perhaps a "free science agreement" will someday be a part of the negotiations now restricted entirly to free trade in commercial goods.

Is NAPAP a Model for Future Studies?

Yes, if we consider it as an example of how *not* to perform a large-scale assessment. The lesson that throwing a lot of money at science does not buy instant answers or instant excellence seems to be a hard one for bureaucrats and politicians to learn. The history of NAPAP appears about to repeat itself under the global change banner. Again, the American megaprojects, megaproposals, megacommittees, and megamodels are much in evidence. Once again, reports from specially appointed committees of scientists with expertise peripheral to the problem are used as an excuse for delaying action until the problem is conclusively proved to exist. Once again, megadollar budgets support computer modelling and remote sensing, with little devoted to real ecology. Unless such large projects can be totally dissected from American politics, I expect a long period of procrastination, accompanied by an expensive, disorderly megascience program controlled by powerful agencies under the thumbs of politicians.

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