2. Lessons Learned From the Acid Deposition Research Experience: An Historical Perspective

Anthony C. Janetos[1]

I began a fifteen-year career as a federal science program manager in the middle of the 1980s, when I took a position in the U.S. Environmental Protection Agency's (EPA) Office of Research and Development on the Acid Deposition Research Staff. EPA was emerging from an unprecedented trough in public perception and official performance. William Ruckelshaus had returned as Administrator, in part to repair the damage done by the previous Administrator, and among the many vexing environmental issues that needed to be addressed, the challenge of acid deposition was among the greatest. In this chapter, I reflect not so much on the science of acid deposition per se, but on my personal experiences as a participant in an important federal science and assessment program on a very visible public environmental issue, and what lessons can be drawn from them.

For purposes of clarity, I will address four categories of experiences:

- Scientific lessons, focusing especially on the design of science and assessment programs;
- Policy and political lessons, focusing on whether the science really affected policy decisions and what it finally took to get policy action;
- Institutional lessons, examining the challenges to coordination and collaboration in large, interagency programs and implications for today's issues; and
- Career lessons, examining the incentives and disincentives for managers to participate in such programs.

1. Scientific Lessons

1.1. Background

The basis of the acid deposition issue in the United States in the 1980s had been forming for over a decade. The observations of acidified surface

[1] The views presented in this chapter are my thoughts about my own learning experiences in the National Acid Precipitation Assessment Program and beyond.

waters, mostly in lakes in the Northeast, often accompanied by dramatic reductions in fish and other biological populations, mirrored phenomena that had also been documented in Europe. Canadian lakes in the eastern provinces showed identical phenomena. Europe, Canada and the eastern United States also exhibited acidity in rainfall that far surpassed the expected natural background, accompanied with high levels of deposition of sulfate, both in solution in rainfall and in particulate form. Nitrate deposition was also much higher than expected from natural processes (see below). By the mid-1980s, there was relatively strong scientific consensus that in many places, the deposition of strong mineral acids in rain and snow had in fact led to acidification of surface waters over time. The chemical processes in soils were relatively less well understood, and there was a controversy over whether there would be additional delayed consequences of continued deposition. There was very little debate over where the sources of the excess sulfate were: they were understood to be the big stationary sources of coal-fired power plants, mostly although not exclusively in the midwestern states of Ohio, Indiana and Kentucky, but also including many large sources throughout the Northeast. It was also clear by that time that U.S. sources of sulfate influenced Canadian resources, and vice versa.

There was also beginning to be concern over acid deposition's potential effects on forests. Coniferous forests in Scandinavia, Germany, and Eastern Europe were experiencing a dieback phenomenon that was characterized by foliar damage, reductions in growth rates, a surprising variety of leaf pathologies, and eventual tree death. In part because there were few other candidates, air pollution was strongly suspected to be a cause of the dieback. In the northeastern United States, at high elevations in the Adirondacks and even further south in the Appalachians, a similar, but not identical dieback was beginning to be noticed. Red spruce was the species most affected, and symptoms included obvious damage to its needles, reduced growth rates, and increased mortality in affected stands. Air pollution was also a strong candidate as a cause, in part because there were no other obvious candidates for this pathology, and in part because the early stages of the phenomenon were limited to high elevation, high deposition regions. The growing visibility of this phenomenon, and the possibility of its link to acid deposition and air pollution, only served to increase the public's attention to acid deposition and as a spur to increased federal science funding to quantify and understand these phenomena.

The policy scene was substantially less clear. It was known that reductions in sulfur emissions were going to be needed to reduce the levels of lake acidification, but the amounts of reductions, the costs of doing so, the regulatory mechanisms to be used, and the fact that the environmental damage of concern was in many ways separate from where the emissions originated all served to make policy and political solutions difficult to negotiate. Although debate was vigorous, and the political rhetoric heated, there was no policy in place for dealing with the issue under the Clean Air Act.

1.2. The Importance of Time Series

The consensus in scientific understanding at the time was largely due to having a few locations, such as the Hubbard Brook Experimental Forest, described in detail in subsequent chapters, where there were high quality time series of both rainfall pH and surface water acidity, and where very careful experimentation had been done to understand the processes involved. Such high quality time series were of enormous value in understanding the processes by which acid deposition was affecting surface waters, soils, and ultimately forest ecosystems.

But the forest dieback phenomenon lacked such carefully investigated, long time series. As a result, this newer phenomenon was relatively poorly understood, and there were simultaneously arguments over its very existence as a significant environmental issue, as well as the mechanisms by which acid deposition might or might not be involved. Added to the complexity was the realization that the phenomena observed in forests were not immediately diagnosable as being due specifically to an air pollution stress. This was quite unlike the situation with surface waters, where the acidification and loss of aquatic life was clearly due to the continued, long-term deposition of strong mineral acids; the only real questions were the particular soil chemical processes involved, how much buffering capacity there was, and how quickly the systems might respond to changes in deposition. The situation for forests was far less clear.

1.3. The Importance of Extent and Magnitude

There was a significant difference between the desire for knowledge on the part of the policy and regulatory community within EPA (and more broadly, the federal agencies), and those of the scientific community. For the most part, the scientists involved were interested in understanding the processes by which acid deposition affected ecosystems. The policy community respected and valued this mechanistic knowledge base, of course, because it was fundamentally important to establishing the cause-and-effect linkage that was itself fundamentally important to establishing levels of emissions reductions that were going to be necessary. But the policy community also needed to know the overall extent and magnitude of the (then) current effects that could be attributed to acid deposition. It also needed to know the degree to which the current effects might constitute all there were going to be, or whether substantial additional effects might be in the offing, or what the ecosystem's sensitivities might be, and whether there were thresholds in effects such that very rapid or large additional changes might be coming.

These concerns went well beyond the traditional academic interests in understanding mechanisms and processes. They quickly entered the realms of environmental assessment (how much damage and where) and predictability (what might the future hold under different policy/environmental scenarios). The scientific community could address these concerns but at least in the beginning, the research programs in place were ill-equipped to do so. They had been reasonably designed to understand processes and mechanisms, but not designed to quantify the extent and magnitude, nor designed to investigate alternative futures.

It was in the mid-to-late 1980s that the EPA and federal research programs were dramatically re-oriented to provide quantitative information on the extent and magnitude of acidification effects on surface waters to respond to this need of the policy community. EPA's survey of the extent of acidified surface waters in lakes, and later, in streams, provided substantial information on the degree to which the environment had already been altered by acid deposition. New mechanistic research on forests, initiated around the same time, provided much better information on the processes leading to the observed forest dieback phenomena, and the U. S. Forest Service developed visual survey methodologies that began to be incorporated into its routine forest survey methodologies to estimate the extent of forest dieback.

1.4. Working in Parallel

A natural and logical way for scientists and laypeople alike to understand the genesis of any environmental phenomenon is to work from its origins, to its consequences, to remedies. For acid deposition this meant following the path from sulfur and nitrogen emissions, to atmospheric chemistry and transport, to deposition processes, to ecological consequences, and then to treatment (e.g., liming of lakes to reduce acidification) or to reduction of emissions. This template outlined the major delineations of the EPA and federal research programs.

In terms of program design, this logical structure becomes problematic. There was a tendency in both the scientific community and in the policy communities to view acid deposition as primarily an atmospheric issue. This was not surprising, but it was limiting. Programs that are designed to treat environmental problems in essentially chronological order presuppose that there are questions about whether the atmospheric stresses actually lead to ecological consequences, i.e., whether there really are cause and effect mechanisms. In the case of acid deposition and the deposition of surface waters, nearly a decade of research was spent on the mechanisms of acid formation and transport in the atmosphere, when what had driven the public's interest and concern were observed consequences that by the mid-80s had already been linked to deposition. The real question from the standpoint of consequences was how extensive the damage was, and whether it was going to get worse.

When the forest issues began to be raised, those questions were magnified. It was unrealistic to expect that the research programs initiated in the mid-80s were going to yield quantitative information on processes and extent of the phenomena in a short period of time. But the consequence of structuring the overall program in the same way as it was most easily understood was that the bulk of the funding had already been put into the atmospheric components of the problem. So there were extensive field experiments on model validation, and indeed, extensive model development. But the major questions that were being asked by the policy community were not so much about the atmospheric science issues as they were about the extent, magnitude and potential futures of ecological consequences. Unfortunately, significant research funding on ecosystem effects was precluded because of the original formulation of the problem as being primarily an atmospheric issue. Had the ecological research been undertaken when the phenomena were first observed, in parallel with the atmospheric research, there would have been substantially more and better information about potential target loadings and whether thresholds for damage existed. In addition, the fact that the ecological research required had intrinsically longer time scales, i.e., simply took longer to do in the field, than the atmospheric research, meant that it was very difficult to respond to the degree of urgency that had already entered the public, policy, and political debate.

Even though one can argue that there should have been more work on ecological effects funded in parallel with the atmospheric science, one cannot argue that the atmospheric science was unimportant. Obviously, it was the understanding of the atmosphere's behavior that made specific policy recommendations about targets, timetables, technologies and compliance costs, and ultimately emissions caps possible to have in a substantive way.

In my view, the program would have been more responsive to the actual needs of the policy makers had it been structured and funded in a more "Bayesian" fashion. It could then have asked and addressed fundamental questions about the ecological consequences of different deposition levels at the same time as it sponsored research on the atmospheric and engineering challenges involved in controlling emissions and deposition. It is certainly possible that this would have meant less overall investment in the atmospheric sciences in the research program. I can only note, however, that the technical details of the vast majority of atmospheric sciences supported in the acid deposition program did not enter the policy discussions about cap and trade legislation in any significant way.

1.5. Holes in the Strategy

At the time, it was reasonably well understood among the ecological scientists involved in the acid deposition program that there were potentially significant gaps in the overall research strategy. For instance, the deposition of nitrates also has an acidifying effect on most of the soils in the eastern United States, as had already been documented in parts of Europe. Nitrogen deposition also came into sharp focus as an important contributor to the forest dieback problems experienced by red spruce, especially at high elevations in the Adirondacks and Appalachians. There was also considerable concern among the scientists working on forests, in particular, that there were synergistic effects of other air pollutants, especially ozone, and acid deposition that were important stressors on forests in the eastern United States. The acid deposition program simply could not deal with all of these issues at once, and the policy/regulatory apparatus of EPA certainly could not deal with these issues of multiple environmental stresses. The end result was that there was policy action that was quite effective at reducing the emissions and deposition of sulfur, but nitrogen and ozone issues were left for another day, regardless of the fact that it was already well-understood that they had become important stressors on the ecosystems.

The final known gap in the overall strategy was ensuring that adequate monitoring of both deposition and effects would continue. The research staffs in both EPA and other agencies were acutely aware of this need. Indeed, some of the deposition chemistry monitoring has continued to this day, in spite of substantial pressure on the funding of such monitoring networks. But the situation for effects has been less impressive. A few of the long-term research sites, such as Hubbard Brook, have been able to maintain their research support over the years. But several of the research networks that were originally implemented by the EPA focused on understanding the links between deposition and forest dieback, were quickly cannibalized for funds for new research interests by the early 1990s. Subsequent surveys of lake and stream chemistry optimized for change detection were not done, with the result that if this problem were to arise anew today, we would almost be limited to the same set of long-term research sites as we had in 1984 to find high quality time series of environmental data.

2. Policy and Political Lessons

Did the science matter? The National Acid Precipitation Assessment Program (NAPAP) was, at the time, the biggest interagency research and assessment program that the federal government had organized to understand an environmental problem. Was that investment worth it?

In my view, the answer is both yes and no. Yes, because the research clearly provided important information in terms of developing understanding about the extent, rates, and magnitudes of the consequences of acid deposition, and about the deposition itself. No, because at least some of the research was not focused well on real decision-making. For example, some of the dose-response research on materials damage was successful in identifying damage functions, but it was not focused on the factors that people actually used in real decisions about replacement, repainting, and recovery. The end result was that the cost numbers for potential damages were largely irrelevant to any sensible emissions reduction program.

Another way in which popular perception of policy interests was at odds with expressed policy interests was in the realm of health effects. It is a tenet of many people concerned with environmental issues that human health effects are in some way the *gold standard.* In the acid deposition program, research on potential health effects focused mainly on the potential health effects of acidified ground and surface water leaching metals out of municipal water systems. This turned out, however, to be largely a hypothetical concern. Interestingly, integrated assessment models done at the time identified potential health consequences as a major concern in policy formulation, although their probability of occurrence was understood to be quite low. In large part, this was because such models could quantify in economic terms the health impacts in terms of treatment costs and lost income, but could not quantify at all the consequences of losses in ecosystems and their services in economic terms. Thus, the very low probability of health consequences trumped the known large ecosystem consequences because the latter were calculated as having almost no economic value.

On the other hand, the public really did care about ecological outcomes, even when clear and large economic consequences were not identified. The sense of place that many people have turned out to be enormously important, as reflected in their acceptance of bearing regulatory costs in order to protect a region's ecological heritage. This was true in acid deposition, and remains true today in the climate change debate, even as we begin to understand more about the role that ecosystems play in human well-being by providing services.

The final aspect of policy and political lessons to be addressed here is the notion of whether the reduction in sulfur emissions through the cap-and-trade program in the Clean Air Act amendments has been successful. From one perspective, the answer is obviously yes. An enormous amount of sulfur that would have been emitted into the atmosphere has not been, and the overall cost of compliance with the targets is lower than they would have been under standard command and control regulations. However, as other chapters in this volume point out, there are still substantial numbers of acidified lakes and damaged forests in the northeastern United States. The recovery of systems has been much slower than was originally thought, and the gaps in the regulatory system, in particular the relative inattention to nitrogen sources from transportation sources, means that there is still substantial acid input to those systems.

This argues that judgments about the success or failure of the regulatory regime for acid deposition are both more difficult than anticipated and possibly premature. We must move towards an evaluation system that at the very least incorporates periodic environmental assessment of the state of the physical and ecological outcomes of concern as well as measures of economic and regulatory effectiveness. Such a system should be able to maintain financial support of the necessary ecological monitoring as well as monitoring of the atmospheric stressors, and include measures of economic costs and benefits, and those other measures of costs and benefits that are difficult to price and therefore trade in markets.

3. Institutional Lessons

At the time, NAPAP was the biggest coordinated interagency research and assessment program that was dedicated to studying the processes, impacts, and potential solutions of a national environmental problem. At its height, the research budget exceeded $60M annually, and more than a dozen agencies participated, about half of them actively. The program was the province of the Research and Development arms of the various agencies, and their policy equivalents served on an oversight board of agency executives. EPA's Office of Research and Development, where I worked, was the largest single player in terms of budget and people, and sponsored research both in its own laboratories and in the external university community.

But an interesting feature then, as now, of large interagency research programs in the federal government was the diversity of philosophies that the agencies had about research and the diversity of relationships with the external scientific community. Many agencies had in-house scientific expertise, and those scientists had the normal relationships with their peers in universities, think tanks, and consulting companies that one would expect to see anywhere. But those agencies often had relatively little experience in funding outside scientists, especially using competitively awarded, peer-reviewed grants. Others had primarily in-house science management expertise, and were oriented primarily towards the award of grants to outside scientists. Still others, including EPA at the time, had a mixture of each, but had not often made an institutional commitment to one mode of operation or another. The result of this last mode of relating to the broader scientific community was a continual tension between the external community and the internal community of agency scientists over scarce resources.

It was also the case that the agencies in NAPAP had very different management philosophies, which also led to some tensions. A few, such as EPA, had traditions of reasonably strong central management. Others were extensively decentralized. Understandably, this difference in agencies' approaches to their own management led to tensions within NAPAP, especially when decisions needed to be made quickly, or when longer-term strategic decisions required different lengths of time to be vetted in the participating agencies.

However, there was one common feature to all of the science agencies involved in NAPAP. All experienced at least some degree of difficulty in communicating with their internal policy counterparts. There often was some degree of distrust, often driven by what the science managers perceived as a desire for too-rapid decision making by their policy colleagues. But in my view, there was primarily a lack of a common understanding between policy analysts/managers and scientist/managers about what science would in fact be helpful and useful in making policy recommendations and decisions.

Interestingly, NAPAP was quite effective at galvanizing participation in research with the private sector. The Electric Power Research Institute (EPRI) and the National Council for Air and Stream Improvement (NCASI) each brought substantial intellectual and financial resources to the table on ecological research, for example. They became important partners in both field and modeling research, and many of the collaborations that began in NAPAP have lasted to the present day, focusing on different topics now, but building on a strong relationship of trust that began in NAPAP.

Finally, the creation and use of the interagency program office was, I believe, extraordinarily important for NAPAP's functioning. Many of the agencies preferred to keep the central office weak; doing so enhanced their own ability to influence NAPAP's agenda. But the central office provided a neutral forum for the agency participants to meet and negotiate, and indeed many of the programs that NAPAP sponsored went far beyond simply coordinating the efforts of individual agencies. They required real negotiation over activities, organizational structures, schedules, and resources, and would have been far more difficult to achieve without that central focal point.

Where the central office of NAPAP finally proved its worth, however, was in the assessment process. After a very visible, public false start on assessment, where an Executive Director of NAPAP put his personal political conclusions on the results of the science without benefit of peer review, damaging NAPAP's credibility nearly beyond repair, the central office played a crucial role in restoring that credibility. The new Executive Director, James Mahoney, very visibly took steps to restore transparency to NAPAP's assessment processes, ensured that a set of high-quality scientific assessments were done, reviewed, and revised, and generally brought a high level of professionalism back to the program's operations. These steps could not have been taken without a central office that was strong enough and visible enough to ensure that there was some management consistency across a multitude of efforts.

4. Career Lessons

Finally, I will touch on some career lessons that stem from my experience with NAPAP. One of the interesting features of many of the people involved with NAPAP was how relatively inexperienced so many of them were. Of course, there were senior agency managers involved in each agency. But there was also a plethora of junior, early and mid-career employees in each agency, many of whom had been entrusted with a great deal of authority and responsibility. NAPAP was for many of them a seminal feature of their professional development, as it was for me.

A large cadre of these participants has gone on to have extremely interesting and quite varied careers in government, academia, industry, environmental groups, and think tanks. I am convinced that this cadre of people learned early on that it was indeed possible to have meaningful collaboration among different government partners, among the government and industry and the NGOs, and that they have used this knowledge to their advantage in their own careers. Indeed, it is now relatively common for serious environmental problems to generate an interagency scientific response, and the experience that the NAPAP participants had themselves, and the institutional experience of their agencies suggests that current interagency partnerships have sometimes been made easier and more productive as a result.

It is also true, I believe, that a large part of the learning experience of the NAPAP participants was a far better understanding of how science supported by the federal government and private sector does or does not interact with policy and decision making. Acid deposition was and remains a case with large environmental stakes, and potentially large compliance costs. The knowledge we gained in NAPAP will continue to benefit us as we deal with the current challenges of acid in the environment.

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