Experimentation Involving Controversial Scientific and Technological Issues: Weather Modification as a Case Illustration

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Abstract

The development of weather modification requires resolution of a number of scientific questions. To resolve them necessitates field experiments that frequently extend over many years and cost millions of dollars. These projects usually are highly visible to the scientific community and, often, to the public as well. Weather modification and similarly risky technical efforts requiring field research typically involve a large number of scientists with varying interests and incentives. As they seek to resolve certain scientific controversies, the projects also generate other conflicts that are organizational, budgetary, and sometimes public. The scientific conflicts cannot be separated from these controversies in their environment. Solving the one kind requires dealing with the others.

To learn how such projects should be designed, conducted, and evaluated, we studied four major weather-modification projects and determined the origin and resolution of their scientific, management, and policy controversies. The assessment revealed that to conduct major field experiments concerning scientific topics viewed as controversial within the scientific community is extremely difficult due to the multifaceted nature of the scientific controversy. The major scientific controversies were a result of six factors, including 1) proceeding with an inadequate scientific knowledge base; 2) a flawed project-planning process; 3) differing views between funding agencies and project scientists; 4) lack of continuing commitment by the principal agency conducting the experiment; 5) changes in project directors; and 6) poor performance by project scientists.

This study reveals that, in order to minimize scientific controversy, certain procedures should be followed that impact on the environment of the project as well as the intrinsic science performed therein. First, an initial, in-depth assessment of the need for the proposed project vs the state of scientific readiness must be conducted using the most credible scientists in the field. Then, the most knowledgeable scientists should be involved in the planning process, and third, the major funding entity—usually the federal government—must make a commitment adequate in both time and resources. The selection of a single institution clearly committed to the study of scientific issues to be investigated is a critical fourth factor, and those selected as project directors must have demonstrated scientific *and* management skills. Finally, a project needs thorough and frequent oversight by a knowledgeable and prestigious group. Ideally, those involved in the planning would have strong input in the evaluation.

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1. Introduction

There are several major concerns that have arisen today about the conduct of American science. These include matters such as scientific integrity, federal (especially military) control of research, basic vs applied research, big vs small science, and relations between universities and industry in the name of economic competitiveness (Carroll 1986; Brooks 1987; Carey 1988). A trend expected to increase in the future is that involving many scientists from diverse disciplines working together to address major scientific and technological questions (Branscomb 1987). Unfortunately, the prospects for conflict and controversy are high. There is also growing public disillusionment with science and technology (Bender 1986), fueled by scientists who disagree in public about the nature of scientific facts involving "risky" technologies. Such disagreements may enlarge with the number of scientists involved. Yet science must push forward to address issues seen as quite controversial by some of the scientific community and the public.

This is especially the case where the development of certain technologies requires resolution of scientific uncertainty in a highly visible field situation. Examples of such research and development activities include means of disposal and storage of toxic and hazardous wastes, weather modification, testing of certain defense systems, biomedical experimentation, and alternative energy sources. Common to such experimental efforts are diverse scientific views as to whether experiments can or should be attempted, and the effort is often closely monitored by the public and the media.

When development of an unproven technology or research into a scientific problem requires experimentation in a potentially contentious public and/or scientific environment, how should the research be planned, organized, and conducted to minimize controversy? Science moves forward through a process involving peer review which can lead to beneficial scientific controversy often resolved in the literature and by continuing research. This study focused on how scientific and management controversies developed and were resolved during four weather modification projects involving major field experimentation. It was not the intent of this study to assess how well the scientific questions of weather modification have been answered nor to recommend how the field of weather modification should proceed. We believe there are important lessons to be learned here for future field experimentation in any subject area and we remind the reader that field experimentation is but one part of the studies involved in complex scientific research.

The conflicts found in these weather modification experiments provide valuable lessons for future potentially contentious experimentation. The conflicts involved project scientists, project directors, program managers of funding agencies, and the public.

Costly and long-lasting weather modification experiments of the recent past were chosen for case analyses because each experienced a myriad of scientific, management, and policy controversies. Each project lasted more than 10 yr, and the basic phases of each (conceptual through termination) were used as the temporal framework for analysis. In each phase, we discerned the origin of conflict and how each was resolved.

The four projects selected for study included the National Hail Research Experiment (NHRE), the Florida Area Cumulus Experiment (FACE), the Precipitation Augmentation for Crops Experiment (PACE), and the Sierra Cooperative Pilot Project (SCPP). These were selected because 1) they represented long-term field experiments; 2) they dealt with modification of different weather phenomena, including hail, rain, and snowfall; 3) they were conducted by different groups of agencies and funded by different federal (and state) agencies; 4) they were among the most expensive field experiments in weather modification; and 5) they rate among the most widely known weather modification experiments. Table 1 presents characteristics of these projects.

The origins of these projects relate to major forces that shaped United States science over the past 30 yr. The NHRE evolved in the mid-1960s as a reaction to Soviet claims of success in hail suppression, reflecting our technological race with the Soviets—a driving force in scientific development in the 1960s (Brooks 1987). FACE was conceived by U.S. Weather Bureau scientists experimenting in the Caribbean, but it was given emphasis in the mid-1960s when the Weather Bureau was reorganized into a new agency with a broader environmental science orientation (Environmental Science Services Administration[ESSA]). Funding for NHRE, at the start of its exploratory phase, was shifted to the Research Applied to National Needs (RANN) Program at the National Science Foundation (NSF). RANN was another effort to use science for broader national purposes. SCPP evolved in California and PACE in Illinois as a result of local (state) interest in developing and applying technology seen as beneficial to the economy.

Information on each project and its scientific issues was collected in various ways. Issue areas identified in each approach included project design, unresolved scientific issues, funding, leadership, conduct of operations, participants, and evaluation. The controversies of each experiment were sorted according to six project phases (often overlapping): 1) conception; 2) planning; 3) adoption;

TABLE 1. Characteristics of the weather modification experiments.

Characteristics	NHRE	FACE	SCPP	PACE
Primary scientific focus upon	hail suppression and hailstorms of the High Plains	tropical clouds and rain enhancement	winter storms and rain enhancement	summer rain enhancement and effects
Where conducted	northeast Colorado	southern Florida	central California	central Illinois
When conceived and conducted	1966–79	1968–80	1976–88	1971–90
Duration of active research	1967–79 (13 yr)	1963–80 (18 yr)	1977–88 (12 yr)	1971–73 1977–90 (16 yr)
Total funding, (millions of $)	~$35	~$25	~$25	~$7
Primary funding agencies	NSF*	NOAA†	DOI‡	DOI/ILL@ NOAA/Illinois 1977–88
Primary agency conducting experiment	National Center for Atmospheric Research	NOAA	DOI	Illinois State Water Survey
Scope of participating scientists	nationwide	largely NOAA	nationwide	largely Illinois

* NSF National Science Foundation
† NOAA National Oceanic and Atmospheric Administration
‡ DOI Bureau of Reclamation, Department of Interior
@ ILL Illinois State Water Survey

4) implementation; 5) evaluation; and 6) termination.

The scientific literature was reviewed and examined to discern the scientific issues and evidence of controversies relating to them. Certain projects, such as NHRE, brought substantive controversies into the literature, whereas others, such as PACE, did not. Information came from the study of project files containing memorandam, correspondence, contracts, budgets, and purchase orders. These were assessed in the agencies that conducted the experiments and at the major federal agencies that funded the research.

Structured interviews with those involved in the experiments provided valuable information that filled gaps in knowledge where files were sparse. Four classes of individuals were interviewed: 1) program managers and leaders in relevant federal agencies; 2) project directors; 3) scientific participants in the project; and 4) other members of the weather modification community that served as reviewers or advisors to the experiments. These interviews were structured to gather information about each phase—conceptual through termination—of the project. Prior data collection allowed us to develop specific questions in each interview. All those interviewed were told about the goals of the project, and each was asked to review his/her involvement with the project. Each was asked to identify controversies they recalled and to recall the causes and resolution of each controversy. Between 6 and 14 people were interviewed at length about each project, and their views about the controversial issues were cross-checked with other views, published information, and file material.

A history of each project was developed (Changnon 1987a, 1987b, 1987c, 1988) to serve as a guide to the assessments of controversies and their resolutions. Controversies fell into two, broad types: those that developed among scientists (usually between participants in the experiments), and those emanating from various project-management personnel. Ironically, public controversies were minimal, owing to strong public-information activities, and the relative sparsity of people living in the experimental areas.

2. The projects

a. National Hail Research Experiment

This was a "national program" that involved many atmospheric scientists and institutions. It was the most costly and well-publicized of all four experiments (see table 1). The concept of NHRE was born out of Soviet claims in the early 1960s of considerable success in suppressing hail in the Caucasas Mountains (Sulakvelidze 1966; Battan 1965). This created great interest, particularly in certain federal agencies (NSF 1965), and a committee of nationally prominent scientists was convened to consider the concept of a United States project (ICAS 1966). Some United States scientists objected, saying the scientific community knew too little for an experiment (Hail Instrument Subcommittee 1966), but after 2 yr of deliberation, the national committee presented a plan with a strong recommendation to pursue a major experiment with twin objectives: to study hailstorms, and to test the Soviet hypothesis of hail modification (NSF 1968). In the project's implementation phase, preexperimental milestones to resolve major scientific uncertainties and to develop essential instrumentation were not met (Goyer 1971), but the project proceeded under federal pressure to launch the full-scale cloud-seeding experiment in 1972 in northeastern Colorado (NCAR 1969; Changnon and Lambright 1987). The project was largely funded by the National Science Foundation (NSF), and the project was directed and managed by the National Center for Atmospheric Research (NCAR), with nearly 100 scientists from 15 institutions involved (Changnon et al. 1977).

After 3 yr of field trials with no evidence of changes in hailfalls including impact energy and hailstone sizes (Flueck and Mielke 1975; NCAR 1976), the major suppression experiment was ended in 1975 amidst controversies over its design, operation, and evaluation (NSF 1974; NCAR 1976). This was followed by 4 yr of data analysis, research, and further field studies, before the entire effort was terminated in 1979. Certain goals were partially achieved. More was learned about hailstorms in the lee of the Rocky Mountains, for example. But debates over whether NHRE was a viable test of the Soviet hailstorm suppression approach went unresolved. Conflict occurred in all phases of NHRE among participating scientists, NCAR leaders, NSF managers, and project directors.

b. Florida Area Cumulus Experiment

The history of FACE is very different from that of NHRE. FACE evolved out of successful field experiments conducted in the early 1960s by atmospheric scientists attempting to achieve growth of single cumulus clouds in the Caribbean by the seeding of clouds as part of NOAA's Stormfury Project (Malkus and Simpson 1964). The experimentation was moved for logistical reasons (access to weather radars and new seeding flares for safe overland use) to the Florida peninsula in 1965 and became the Florida Cumulus Seeding Experiment. This project was largely funded by the National Oceanic and Atmospheric Administration (NOAA) and until its termination, conducted largely by a group of NOAA scientists, with only minimal involvement by external scientists. The initial objective was study of the effects of massive cloud seeding on cloud evolution and growth (Simpson et al. 1972). FACE became identified in 1970 as an experiment to discern whether massive seeding led to increased rainfall from Florida tropical cumulus (Simpson and Woodley 1975).

After more than a decade of "exploratory" experimentation (1966–77), the evidence that clouds could be altered to create more rainfall was very encouraging (Woodley et al. 1977). This led to a final "confirmatory" phase of FACE (1978–80). However, this phase was terminated when the rain change obtained did not match that expected based on the earlier findings of FACE (Woodley and Flueck 1984). Several scientific controversies related to FACE were documented in the literature and thus appropriately resolved. These included use of radars to measure rain (Cataneo 1971; Woodley 1971), cloud physics (Battan 1970; Simpson 1970), and employment of cloud models (Warner 1980; Simpson and Cooper 1981). The most important disagreement in FACE occurred in its final phase, however, and largely concerned interpretations and evaluations of findings (Kerr 1982; Changnon and Semonin 1982; Nickerson 1979; Flueck et al. 1981). There were a number of intermittent conflicts throughout the project between NOAA managers and the project directors (Changnon and Lambright 1987) over relative priority and funding for FACE, as well as technical direction. At one point there was serious intent to move FACE to another state. These intraagency issues greatly affected the progress and final outcome of FACE. For example, FACE funding commitments were made annually by NOAA leaders, and this complicated the annual planning and effectively negated long-term planning for field operations.

c. *Sierra Cooperative Pilot Program*

The SCPP evolved in California during 1976–77 as a result of state and federal (Bureau of Reclamation) desires to increase water storage in a major reservoir being planned in the Sierra Nevada. This project was conceived in a region where cloud seeding to enhance snowfall during the winter season had been conducted in an operational (nonexperimental) status since the early 1950s. Hence, the original concept of SCPP was to conduct a scientifically based demonstration of a technology locally believed by many to be largely operational and proven.

The project, under the proven leadership of the Bureau of Reclamation, was designed as an exploratory scientific experiment (Vardiman 1979) with many scientists from different institutions involved (Reynolds and Dennis 1987). Five years of seeding provided some evidence of changes in clouds, but misleading information from faulty field instrumentation had obfuscated progress (Changnon 1988). After these problems were resolved, a new approach evolved. A new project director took charge, and a second phase of the experiment was conducted during 1982–86. The goal was to develop the scientific underpinning for the enhancement of winter-season precipitation in the Sierra Nevada. Federal funding was terminated in 1987, however, due to overall budget cutbacks. Final results remain inconclusive because summary research efforts ended due to lack of resources. Principal controversies occurred between scientists participating in the project, particularly in its first phase, over the initial design (Elliott and Griffith 1979), the structure of winter storms (Gordon and Marwitz 1981; Stewart and Marwitz 1982; Heggli 1986), and over effects from the cloud seeding (Rodi and Flueck 1986). Several of these problems emanated from poor instrumentation.

d. *Precipitation Augmentation for Crops Experiment*

This Illinois-based field experiment began in 1971 and exists today. It has been jointly funded by state and federal agencies (initially from the Bureau of Reclamation), and conducted largely by scientists of the Illinois State Water Survey, a state research agency (Changnon 1986). Following a 3-yr hiatus (1974–76) due to federal budget cuts, the project was rejuvenated with NOAA funding in 1977. When NOAA halted all weather-modification research in 1982, PACE was continued with congressionally mandated funds, directed through NOAA.

PACE was a "bottoms up" experiment planned by four midwestern state universities and the Illinois State Water Survey. The experiment had a dual objective: to determine whether summer rainfall in the Midwest could be increased, and to examine the impacts of altered rainfall. It was conceived as a reaction to agricultural concerns over the lack of timely rainfall in many summers and to regional pressures to use unproven cloud seeding methods in the Midwest. PACE suffered from discontinuous federal support, funding levels lower than planned (table 1), and controversies over scientific issues between the state project director and the federal funding agencies (Changnon 1987a). Hence, progress in reaching objectives has been slow.

3. Controversies among participating scientists

Scientific controversies in the four projects included those between scientists participating in the project, between scientists involved in the project and the project director, and between project scientists and the external scientific community. (For purposes of analysis, this category excludes scientific controversies that developed between project scientists or directors, and the funding agency staff. These are treated in section 4.) The nature of the controversies was assessed along with their resolutions.

Controversies among scientists fell into seven categories. The first of these related to inadequate data and/or knowledge about the subject to be investigated before and during the project (Hsu and Huff 1986). Differing views over the status of relevant knowledge (considerable vs none) caused dispute in all four projects. We found this to be a major problem in NHRE because some believed there was inadequate knowledge of hailstorm characteristics and approaches to hail suppression. Such inadequacies were seen to embrace widely different levels of scientific certainty by most project scientists, and our conclusion is that they remained a constant source of tension in NHRE.

The second type of controversy evolved from varying interpretations of goals for the projects. These in turn affected the design of the experiments. Considerable confusion over goals existed during NHRE. One can expect that a large field program would embrace a large number of objectives set by the various project scientists but, surprisingly, confusion existed over the central goals of the project. Some scientists saw NHRE as being a hail suppression experiment (Tombaugh 1975); others saw it as a test of the Soviet method of hail suppression (Swinbank 1971); and others saw it essentially as a study of hailstorms (NSF 1974). The FACE project, during its evolution from a cloud-modification experiment to a test of rain enhancement, experienced controversy over shifting objectives and whether the shifts were scientifically justified. SCPP was plagued by controversies among certain participating scientists who emphasized different goals of the experiment ranging from needs to understand very basic physical processes, to needs to focus studies on the end project—more precipitation (Changnon 1988). PACE scientists debated the objectives and scientific approach with federal managers who saw the scientific approach as too slow and too focused on resolving scientific issues they believed already resolved elsewhere or inconsequential (Changnon 1987a).

The third set of controversies related to planning and the conduct of field operations. Field experimentation in weather modification was complicated, involving many forms of instrumentation, including different types of aircraft, weather radars, and weather-forecasting facilities. We found that operations involving a large number of scientists, each with responsibility for a portion of the total project, were very troublesome. Individuals and groups with different instrumentation (and different subobjectives) were involved in controversies in NHRE and SCPP, and both projects involved a multitude of scientists from different organizations. This type of friction did not appear as important a problem in FACE and PACE—projects conducted largely by a group of scientists from a single agency.

Another form of controversy emanated from varying approaches to gathering critical physical phenomena measurements and the adequacy of instrumentation for these measurements. For example, NHRE required both remote sensing and surface sensing of hail, and many of the instruments to perform these measurements were not available when the experiment started (Hail Instrument Subcommittee 1966). Adequacy of the various instruments remained a source of controversy throughout the experiment (Goyer 1971; Congressional Research Service 1978). Furthermore, during all four projects, there was never a period when funding was deemed adequate to meet all needs (Changnon and Lambright 1987); therefore; priorities had to be set as to which measurements would be made. These decisions often led to controversies, again more prevalent in the "national" experiments, that involved scientists from several institutions (SCPP and NHRE).

Weather modification experiments typically have relied on a mixture of statistical and physical evaluation approaches to discern effects of cloud seeding. Evaluation approaches were sources of controversy in all four experiments (Hsu and Huff 1986). Early phase evaluation concepts of "exploratory efforts" vs more "confirmatory efforts" in later phases of the experiments were often misconstrued (Hitschfeld 1974; Statistical Task Force 1978). For example, NHRE, which we believe should have begun with an exploratory seeding concept germane to weather conditions at the United States site, was designed and evaluated as a confirmatory experiment (i.e., confirmation of the Soviet claims). This was a factor that created considerable scientific controversy before, during, and after the experiment (Atlas 1975, 1977; Schickedanz and Changnon 1971; Changnon and Morgan 1976; Flueck and Mielke 1975; Henderson 1978; NHRE 1975). Serious scientific controversies developed around the evaluation of FACE, particularly its first phase (Nickerson 1979, 1981; Flueck et al. 1981). SCPP experienced, throughout much of its early work, scientific controversy over the design and emphasis on physical vs statistical evaluation (Reynolds and Dennis 1986; Changnon 1988).

The sixth area of scientific controversy amongst participants evolved from differences in the interpretation of the results. As might be expected with differing measurement approaches and uses of varying evaluation techniques, the claims about project findings ranged widely. For example, in NHRE, some assessments of the 3-yr suppression experiment concluded there was some effect, whereas others indicated hail suppression had been definitely refuted (Changnon et al. 1977). Other controversies ranged over interpretation of different measurement results of the same events. For example, some involved in FACE interpreted changes in radar echoes to be related to increases in rainfall at the ground, whereas others challenged such interpretations (Cataneo 1971; Woodley 1971).

Another area of scientific controversy concerned exchange of data between scientists, efforts to jointly interpret results, and preparation of coauthored scientific papers. Some scientists in NHRE completed their data-evaluation efforts early, whereas others took exorbitant time, with conflicts evolving from the timeliness of sharing desired data among participants of the project (NSF 1974). Subsequent efforts to agree on findings and to write multiauthored scientific papers became disputatious (Changnon 1987c).

4. Resolution of conflicts among scientists

How were the various scientific conflicts resolved? If future major experiments involving controversial scientific and technological prospects are to be conducted, means of resolving controversies—as well as understanding how they arise—are important to define. Study of the controversies in these weather projects revealed that one of the expected and desirable ways used to achieve resolution of the scientific differences—an approach found in all projects—was through published scientific articles that debated issues in print that were initially raised verbally.

A second means of resolving scientific controversies related to changes in funding. In these instances, controversies over whether a certain subexperiment was to be conducted or continued were often decided by the funding agency through the provision of funds. Funding additions or subtractions were utilized to control controversies in NHRE. Additional research focusing on controversial issues was a third means used to resolve controversy, particularly those relating to types of measurements or instrumentation.

A fourth means of resolving controversy employed in all four projects involved convening external review panels, comprised of external consultants or advisory groups. In these instances, the project director and/or funding agency utilized peers to assess portions or all of the project, including the area of scientific controversy. In these instances, the recommendations of the external peers frequently resulted in the resolution of the controversy.

Scientific controversy that evolved from competing ideas as to measurements, instrumentation, or evaluation techniques was partially resolved by pursuing parallel research tracks. This approach was used in all experiments in efforts to resolve such conflicts.

The most common means of "resolving"conflict found in the projects was simply to ignore the controversy. That is, the project leader and those not involved in the controversy simply proceeded without any attempt at resolution. In these instances, the controversies eventually "died" after the passage of time or by the voluntary departure of participants from the project.

The final means of resolving scientific controversies was to directly remove particpants and/or the leadership of the project. This was done in NHRE and SCPP. Scientific controversies during NHRE led to the forced (funding support eliminated) or willful departure of certain participants (NSF 1974). Also, changes in the leadership were made in NHRE when the level of conflict became excessive (Changnon 1987c).

5. Management-related scientific issues and controversies

Another set of controversies involving scientific issues related to differences between project scientists, or project directors, and the project-management agency and representatives of funding agencies. These were labeled "management-based issues," but they all focused on specific scientific topics.

One set of problems, particularly in PACE and NHRE, related to different views regarding scientific goals and project priorities between the leaders of funding agencies wherein two or more agencies were responsible. For example, in the Illinois effort, the goals and priorities of the federal funding agencies and the State of Illinois differed greatly and created controversies about project objectives. One of the reasons for the multiplicity of goals being an unending source of controversy is the tendency of program managers to be polarized between two views: 1) we pay them (scientists) and they must do our bidding, and 2) we are using them and must respect their goals. The first view led to "top-down" planning and insensitivity to serious questions of feasibility and opportunity, whereas the second view (bottom-up) led to difficulty in keeping the projects organized and moving forward to achieve a unified result.

Similarly, different views between the funding agencies and project directors over the status of scientific knowledge about the topics led to conflicts. In NHRE, the NSF/RANN program leaders perceived that NHRE should be focusing on developing a hail-suppression technology (Tombaugh 1975; Downie and Dirks 1976), whereas the NCAR leadership saw it essentially as a fundamental scientific study of hailstorms (Atlas 1975). In the PACE program, the scientists saw the experiment as a step-by-step project systematically resolving scientific uncertainties, whereas the leaders of the Bureau of Reclamation funding the project felt that the knowledge of Midwestern summer clouds was farther advanced and wanted to have the existing modification technology tested sooner.

An important source of scientific controversies in the NHRE, FACE, and SCPP programs related to changes in key project leaders, either the program managers in the funding agencies or the project directors in the field. In every instance of a leader change, different scientific philosophies about the status of weather modification and the project objectives occurred, producing shifts in emphasis, and fueling controversies, especially during FACE.

Scientific controversy often emanated from differences in the perceived levels of scientific certainty, or uncertainty, about the status of the project and its funding. This was often translated into altered funding commitments. For example, after NHRE was moved from NSF/Atmospheric Sciences to NSF/RANN, NHRE was seen by NSF as a fixed 5-yr field experiment to develop a hail-suppression technology. NCAR, in managing NHRE, saw it as a long-term experiment without any certain outcome, in line with NSF/atmospheric sciences perspective. The result was NSF control by year-to-year funding increments, leading to disputes over annual project objectives and the scientific interpretations of results.

NOAA managers of FACE also made annual funding increments to the project. This pressure to show quick results, or lose funding in the next annual budget and appropriations processes, detrimentally affected objectives, progress, and presentation of findings. Uncertainty over long-term funding of research programs remains a key problem for United States science (Sommer 1987). One can only speculate, but this study suggests that federal program administrators' desires for quick results probably occur because 1) of agency pressures for successes within political time frames (i.e., 4 yr); 2) of the problems of defending high-cost, slow-advancing field research; and 3) they cannot adequately comprehend the complexity of the field effort.

This problem was related to a fifth type of scientific controversy between the funding agency and project management: the lack of defined or agreeable scientific milestones to measure progress or to declare the experiment complete. Branscomb (1985) pointed to the need to carefully resolve the issue of when an experiment is ended. Failure to set milestones, either through uncertainty or neglect, inevitably led to scientific disagreements over the rate of progress. This was very evident in NHRE, FACE, PACE, and SCPP.

Controversy was also rooted in the selection of certain project directors or project scientists without apparent technical justification or proper assessment procedures. For example, in NHRE, the first project director had no credentials in hailstorm studies or weather modification, and was selected by NCAR management with little or no "advice and consent" by the concerned scientific community. Occasionally, certain groups were funded by contract to perform research and field efforts under NHRE without the normal peer-review process, whereas other scientific participants had to go through peer review. This created a sense of "inside" and "outside" scientists within the same project. It was seen to pit "inside" NCAR people against "outside" others involved with the project (Changnon 1987c).

The resolution of these management-related scientific controversies varied widely. A common method of resolution was for one side of the controversy to simply ignore the issue and, in essence, fail to fulfill commitments. Another means of resolution involved either programmatic or leadership changes. A third means of resolving management-rooted controversies was for management to reduce or terminate funding to individuals or institutions on one side of the controversy. On occasion, external reviews were used to seek a verdict to a controversy. Another means of resolution involved structured negotiations of differences. Most often the issue was resolved by essentially "giving up" with no overt act to resolve the dispute.

For those issues where additional funds would help resolve the controversy, the funding agency would sometimes provide additional funds (or find an alternative source of funds). One resolution of funding-agency/project-management controversies utilized by the project leadership was to appeal to the wider scientific community through informal contacts, writing of papers, and presentations at professional meetings. The aim was to obtain broad support for their ideas, concepts, and findings. By this means, community pressure was applied to the funding agency.

6. Factors causing major scientific controversies

Examination of the scientific issues and controversies found in these field experiments in weather modification led us to identify six factors as the root of most scientific controversies.

a. Inadequate scientific knowledge

Many scientific controversies before, during, and after the experiments revealed a real or perceived lack of scientific knowledge about key topics under investigation. This could be expected in most scientific experimentation, but it was prevalent in all four experiments and we believe most evident in NHRE. The status of scientific knowledge about hail suppression (NHRE) and convective rainfall enhancement (FACE and PACE) was meager and the experiments' principals faced major unknowns about how atmospheric processes would react to cloud seeding conducted in a myriad of ways. Knowledge about effects of seeding orographic clouds to enhance winter snowfall (SCPP) was further advanced but still lacked fundamental knowledge of how seeded clouds would behave and how to detect effects. The Statistical Task Force (1978), in assessing the field, concluded that scientists had not yet recognized the scientific complexities nor the need to perform convincing experimentation ("Why are we in this purgatory, between the heaven of conclusive success and the hell of apparent uselessness?") The issue of evaluating seeding effects (bigger clouds, more rain, etc.) using both physical measurements and statistical tests to establish proof of weather changes was another area of little knowledge. It is an obvious, yet fundamental source of scientific controversy when study of a complex topic is being addressed.

b. Flawed planning processes

The planning processes, such as those for NHRE and FACE, became a continuing source of scientific dispute. Uncertainties over future funding, as well as scientific unknowns, tended to make several project goals and objectives vague, with little or no clear planning beyond events of the next year. Furthermore, plans often were not adjusted when events shifted or new results appeared. Controversies also grew out of a lack of scientific consensus over project plans or their subsequent revisions. We found it notable that scientists lacking in expertise were involved in the planning and related decision processes. The "top-down" planning process in NHRE and SCPP—which deliberately attempted to involve a large number of scientists, disciplines, and institutions—appears to be more prone to generating controversy than the "bottom-up" process involved in establishing PACE and FACE. This suggests that it is hard to get everybody into the act and still accomplish anything, at least where wide participation is a goal seemingly independent of technical purpose.

c. Scientific issues ignored by funding agencies

A problem common to all four projects related to differences between the goals, objectives, and perceptions of the funding agencies and those of the project scientists. The funding agencies often created problems for project scientists by shifting project objectives, as NSF did in NHRE and NOAA did in FACE. In general, funding agencies were unwilling (or unable) to commit to project support seen as sufficiently long-term by the project scientists. This led to pressures on the scientists to rapidly disseminate "results" to show progress. These pressures also gave rise to misleading scientific interpretations which disrupted projects. Federal agencies, such as NSF/RANN, attempted to make decisions project scientists saw as their perogative. In general, we found that the federal agencies funding these large field experiments generally had a "different agenda" than did the scientists directly participating in the project.

d. Lack of commitment of project-management agencies

Problems in NHRE, and to a lesser extent in the other three experiments, related to the fact that key staff in the organization managing the project (and not involved directly in the experiment) were not strongly committed to the scientific concepts central to the project. This appears to us to be most evident in NHRE, wherein NCAR was managing a major research project that involved scientific thrusts (cloud seeding) found questionable by many NCAR scientists not involved in the project. Indeed, such views were exaggerated by funding competition. In NOAA, particularly, where research budgets were always under siege, weather modification's loss could mean gains in other weather-research projects.

e. Project director changes

A problem in FACE, NHRE, and SCPP related to the fact that the experiments lasted sufficiently long to lead to more than one project director. Each new director brought different views and scientific philosophies which altered the project and renewed previously dampened controversies. In certain cases, project directors selected were viewed by participants as having inappropriate experience for directing the project. In every instance, the shifts in project leadership provided an additional basis for scientific controversies.

f. Poor performance by project scientists

In multiinstitutional projects, such as NHRE and SCPP, involving scientists from several universities and institutions, several scientific controversies evolved from the erratic behavior and differing philosophies of the participating scientists. Some of the differences were real; others were related to inexperience in working in large field projects requiring close cooperation and occasional sacrifices of personal objectives in field operations and in subsequent exchange of data. Many scientists from different institutions were involved in field experiments funded only on an annual basis. They saw subprojects as more important than project-wide objectives. Some scientists were clearly pursuing personal goals of visibility, even if that hurt the project.

7. Lessons for the future

The conduct of "big science" (or even "intermediate-scale" science) is obviously complex. Furthermore, the conduct of major experiments dealing with subjects seen as controversial by the scientific community and/or the general public is extremely difficult. Another likely dimension of such experiments is that they will be relatively costly (by some measure of the science involved), and they will typically involve multiyear efforts in planning, exploration, field experimentation, and final evaluation.

Clearly, many factors can lead to controversies that, in turn, can foil and ultimately limit or destroy experiments dealing with controversial subjects. Various ways to resolve these have been used, but essentially as reactions to problems. We concluded that certain key activities and arrangements can be pursued to minimize controversies and to help ensure the success of such scientific experiments.

First, we conclude that it is important to conduct a thorough analysis of the need for the experiment, particularly to determine whether the status of existing knowledge is deemed adequate by the scientific community to proceed. This must be carefully weighed against the need to develop the technology or to resolve a major scientific problem. This appears to be most appropriately done through in-depth assessments by unbiased entities, such as the National Academy of Sciences (NAS). It appears necessary to ensure collection of a broad spectrum of views from the scientific community, from users of the results, and from the potential federal, state, and/or private funding institutions. It may even be wise to have a second assessment of need and scientific support as a "check and balance" for the initial assessment. This approach will cause delay, but it has the potential of providing broader scientific support for those projects that do get started.

Second, we see the establishment of a planning process based on a mix of leading scientists knowledgeable in the topic under investigation as important. They should be seen as extremely credible by the scientific communities involved in the future experimentation. The planning process should also develop a specific long-term plan identifying 1) the essential questions and issues to be resolved; 2) the goals and objectives; 3) the milestones to be achieved before progressing into the various (often expensive) phases of the experiment; and 4) the funding requirements. Planning should be viewed as a continuing process and should be a mixture of views of the scientists and program managers to achieve the advantages inherent in both top-down and bottom-up management. A key planning issue is the duration of the project, to be specified in the plan but then reviewed and altered as the project findings develop. It must be made clear who has the responsibility for recommending changes and who has the authority to change the plan. It is worth noting that the length of major projects (12 to 18 yr, table 1), plus the years of ensuing analysis, likely mean that such projects are designed by one generation and evaluated by another.

Given that wide consensus to pursue a major endeavor has been achieved and a definitive plan exists, our third recommendation relates to the federal government, assuming it will play a key role in making the project possible through funding. At the highest relevant levels of the government (including the Office of Management and Budget [OMB], congress, and concerned interagency committees), the project plan, including a realistic assessment of project duration and funding requirements, should be presented and reviewed. From this process, a national commitment to multiyear support should be obtained with the highest possible guarantee of continuity. This process will identify the primary federal agency to support the effort. Secondary, but key support from other agencies probably should not be set at significant levels; past experience has shown that such interagency commitments to a long-term effort are often not sustained. There should be a commitment, on a priority basis, from a particular "lead" agency.

Our fourth lesson relates to the selection of the entity to conduct the experiment. The weather modification assessment suggests that selection of a single entity, be it a group (laboratory) within the federal agency supporting the effort, or an outside entity such as a university, greatly enhances success. Further, this institution should contain much of the scientific expertise needed in the project, have exhibited a long-term commitment to the topic, and be intellectually committed to the concepts of the experiment. Most of the expertise and facilities needed should be housed and managed directly by this institutional entity as in FACE and PACE. The more performers, the less harmony; also, the less accountability for accomplishment.

The fifth recommendation we derived relates to the selection of the project director. To maintain the credibility of the experiment and the commitment of scientists involved, this choice must result from a process seen as credible and realistic by scientific and user communities. If scientists of many institutions are to be involved in the experiment, this selection should be done by a process that involves their input, and the primary funding agency should be involved in the selection process.

The sixth lesson relates to the conduct of the project to ensure its funding and maintenance through its course. We recommend that project management include a task force of senior scientists involved in the project with the role to advise project management in the selection of subexperiments and overall operations.

Second, a national oversight panel composed of nationally influential scientists should also be established and used frequently to oversee the scientific aspects and to ensure that the project director, home institution, and funding agencies maintain attention to goals, redefine objectives and priorities as needed, and establish funding levels to ensure the completion of the experiment. This panel must have the responsibility for recommending changes in the plan, establishing priorities, and for adjudicating conflicts so as to protect the scientific goals of the project. The recommended planning body may well serve as this oversight panel.

A "local" area advisory panel at the site of the experiment is our third recommended group. This could serve as an interface for project information and allow local concerns to be openly expressed during the project operations. This represents a lot of participation at the policy level, but the role of the scientists is seen as advisory and protective of project goals. It is not seen as interfering with project scientists. Unnecessary controversies make the project organization weak. The key is to strengthen the capacity of project scientists by bolstering their position vis-a-vis the broader scientific community and resource providers.

We conclude that given future use of these recommendations, scientific controversy will still develop. To be prepared for this eventuality and to understand its sources should help a meaningful resolution to be reached. Indeed, honest controversy is essential for scientific progress. But, if there is too much management-related controversy, as found in the four weather-modification field trials, the disputes become barriers to advance.

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