Test Fails to Confirm Cloud Seeding Effect

A $3-million weather modification experiment has failed to provide convincing evidence that seeding cumulus clouds enhances rainfall

The Florida Area Cumulus Experiment–phase 2 (FACE-2) was to be the final, convincing demonstration by American scientists that they could wring extra rain out of cumulus clouds, the fluffy mounds that mark many summer skies. In 1978, having completed an initial, exploratory experiment (FACE-1), a group headed by National Oceanic and Atmospheric Administration (NOAA) scientists began a weather modification experiment fashioned after statistically rigorous, double-blind clinical drug trials. None of the experimenters flying over the clouds of south Florida knew at the time on which days they were actually seeding clouds with fine silver iodide particles, and on which they were merely sprinkling them with an inert sand "placebo."

The moment of truth came on 30 November 1981, when project director William Woodley of NOAA in Boulder and Ronald Biondini, a project statistician, unlocked the vault that held the secret record of seeding. "Things looked real good" at first, recalls Woodley. But, as they checked the rest of the days of the experiment, they saw that they had a serious problem. Subsequent statistical tests bore their doubts out—FACE-2 failed to confirm the suggestive but statistically weak positive results of FACE-1. These disappointing results do not necessarily reflect on attempts to coax more precipitation out of clouds elsewhere, but they do highlight the reasons for the changing approach to field experimentation in weather modification.

Part of the reason for the failure of FACE-2 to confirm previous results was obvious when the cloud treatment decisions were revealed. The problem was an outlier, a day on which more than six times as much rain fell as on the typical experimental day when no seeding occurred. As it turned out, the wet outlier was a no-seed day. Because no one expects cloud seeding to produce more than a 20 to 30 percent increase in rain, that kind of outlier can wreak havoc with some types of statistical analysis. FACE-1 had shown a 25 percent increase in rainfall on days with seeding compared to days without seeding. Statistical analysis indicated that there was only a 10 percent probability that the increase resulted solely from chance. FACE researchers had hoped to do at least as well during phase 2, but the increase they observed was only 5 percent, and there is a 40 percent probability that it was the result of chance.

The temptation would be strong to throw out the odd day as a quirk, but the FACE-2 experimenters cannot. Theirs was a confirmatory experiment whose design and analysis had been unalterably specified beforehand—no fiddling allowed. During the exploratory phase experiment, FACE-1, they could and did change the experiment, including such important factors as the composition of the flares that produce the silver iodide particles, the criteria for selecting the clouds to be seeded, and the plans for analysis of the results. During FACE-2, nothing could be changed.

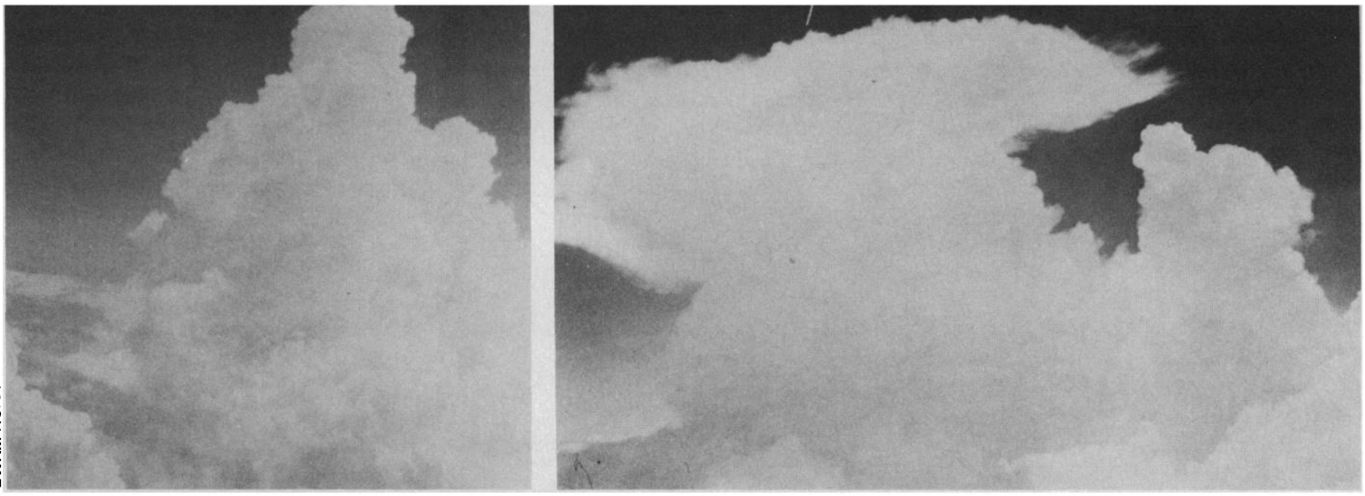
This two-phase approach, exploratory followed by confirmatory, had been urged upon weather modification researchers by statisticians, most prominently in a 1978 report to the Secretary of Commerce's Weather Modification Advisory Board (*Science*, 24 November 1978, p. 860). FACE statisticians and advisers insisted on the second, confirmatory phase, Woodley says. FACE-1, an areawide experiment based on results of seeding individual clouds, had been too much of a learning experience to stand alone as evidence of the efficacy of cumulus cloud seeding.

Although FACE-2 did not prove the point intended by its designers, the results are not without value, project members say. If the outlier is dropped, which admittedly is a violation of the rules of the game, the positive effect on rainfall increases to about 25 percent and the probability that it resulted from chance drops to 13 percent. That is similar to the results of FACE-1. "More than likely, the results show a treatment effect," says Biondini. "It's just not the effect we said we would find. It's something like a well-controlled exploratory experiment," he says. John Flueck, a FACE statistician from Temple University who has been something of a gadfly within the project, agrees. "Although there is no way that we confirmed," he says, "FACE-2 is also suggestive, as was FACE-1, of a treatment effect. It's encouraging." Unfortunately, researchers were not looking for further encouragement, they were hoping to be convinced.

The underlying problem with FACE-2 was failure to cope with the inherent and largely unpredictable variability of weather. Several means of doing so were attempted but were ultimately unsuccessful. One was to pile up enough experimental days that an extreme day or two could not swamp the effects of seeding. At first, Woodley argued that 35 days, selected during May to September of a single year, would suffice for FACE-2. But his statistical advisers strongly objected to the relatively small number, and local Florida vegetable farmers would not stand for an extra dousing of their spring plantings and fall crops. So NOAA management reluctantly extended FACE-2 to a greater number of days spread out over two summers, and eventually three, but no more. That would have to be long enough for an experiment that they originally had been told would be superfluous.

Three summers, it turned out, were not enough. Woodley had hoped to find 60 suitable experimental days in that time. Flueck says that he would have preferred to have at least 75 days, which was the size of the sample in FACE-1. The final total was 51 days, 25 with seeding and 26 without. Even before the field work ended, Bernard Silverman of the Bureau of Reclamation in Denver formally questioned the adequacy of that sample size. As a result, Woodley had Flueck study the problem, drawing on FACE-1 analyses completed after the beginning of FACE-2. In the words of a FACE-2 group report, Flueck found that "when all of the relevant elements are examined, it appears that FACE-2 is a risky confirmatory experiment." The relatively high probability that the positive effect was the result of chance, even with the outlier removed, is due in large part to the small sample size, Woodley says.

Another approach to dealing with the vagaries of the weather was to select only days on which a seeding effect could be most easily detected. Balloon soundings of the atmosphere indicated the days on which clouds would be most susceptible to seeding. Ground-based ra-



OWRM-NOAA

Dynamic seeding of cumulus cloud enhances cloud growth

Seeding of cumulus cloud (left, 7 minutes after seeding) produces growth (right, 24 minutes after) that may lead to greater rainfall.

dar showed how much natural rain was falling in and around the seeding target area before the day's operations began; the less there was, the more likely that rain generated by seeding would not be overwhelmed by natural rain.

These objective tests aside, the final decision on the designation of an experimental day was a subjective one, a vote among the experimenters, that included personal observation of the weather from the air. On the day of the outlier, for example, the radar operator advised strongly that operations be stopped after the release of 60 "flares"—the final go/no-go decision point. (That the randomization process had selected inert sand instead of silver iodide flares was, of course, not known to any of the operators.) There was too much rain in the western part of the area, she said. As Woodley recalls it, considerable discussion ensued. It included the radar operator on the ground and two colleagues and himself in separate seeding planes in the air. The final vote was a 2-2 tie, which Woodley had the power to break and did—it would be an experimental day. The change in the weather that he expected never materialized. It rained well into the evening, the day's total rainfall reaching 26 millimeters compared with the mean of 4 millimeters for nonseeded days. That placed the outlier 4 standard deviations above the mean.

A third approach to accounting for natural variability was rejected as a formal part of the FACE-2 analysis, but so far it, too, has failed to strengthen the results. Woodley and Flueck reasoned that if some of the natural rain could be predicted on the basis of preseeding observations, less variability would be left to interfere with detection of the seeding effect. FACE-1 results analyzed with this method looked promising, but when the same predictive scheme was applied to the FACE-2 results it was far less useful. Flueck and Woodley are continuing to search for more powerful predictors.

Even if the seeding effect of FACE-2 had been statistically convincing, some weather researchers say, the effort would not have met the high standards now expected of weather modification experiments. It would have appeared to work, but nobody would have known why. In earlier experiments, researchers generally treated a cloud as a black box, sprinkling silver iodide into it and waiting for rain or snow to come out the bottom. Although they knew that what happened within the cloud was crucial to increasing precipitation, they were at a loss as to how the process worked or how to measure it. That has changed. "A black box experiment in the 1980's cannot be justified," says Woodley. "We can do better than that."

The FACE-2 clouds were not entirely black boxes. Instrumented aircraft characterized particles of liquid water and ice within some clouds and measured vertical air motion. Some of the ice particles they collected contained silver, indicating that the silver iodide was indeed promoting the desired conversion of liquid water to ice—the first step in making rain from cumulus clouds. But the chain of events linking water droplet freezing to increased rainfall is a long and complex one—the chain proposed by the FACE-1 group had 11 steps. Only the last step, the rainfall, was routinely measured. The complete chain must involve not only the microphysics of cloud particles, but also the dynamics of cloud motion and growth. In the case of FACE-2, things get very fuzzy after the first few steps. "The whole process," Woodley says, "seems to be far more complex than we thought before FACE-2." Adds Flueck, "Most researchers are coming to the realization that you have to know your clouds."

If there is a bright spot in the FACE-2 results, it may be the apparent seeding effects outside the intended target area. Extra-area effects—precipitation changes outside the immediate area that is seeded—have been claimed for some other experiments, but their reality has been debated. Woodley's tentative suggestion of such an effect in FACE-2 promises to be equally controversial. He and Jose Meitin, also with NOAA in Boulder, have made a preliminary analysis of rainfall over the entire Florida region. They estimated regional rainfall from a combination of target area rain gauge records and estimates made from satellite images.

Woodley and Meitin tentatively conclude that seeding may have produced two areas of increased rainfall—one within the target area and one downwind. That is typical of other reported extra-area effects. The real surprise, Woodley says, is that there also appears to be an indication of decreased rainfall upwind during the 6 hours after seeding. The net effect over the entire region was an increase, according to the present interpretation of the satellite data.

Woodley guesses that, if the satellite observations are correct, the FACE group's seeding of clouds over the target area may have affected atmospheric circulation over a much larger area. Seeding was intended to accelerate and expand the natural cloud growth that leads to rainfall. The resulting increased vertical motion may have intensified naturally dry areas where air tends to descend, Woodley speculates. That would tend to produce a net decrease in rainfall upwind, where the lingering positive effects of seeding could not affect it.

Although encouraging in some respects, FACE-2 failed to achieve its primary objective—confirmation of FACE-1. These results stand as another warning that weather modification researchers must be totally committed to statistically sound experiments. It also probably marks the end of experiments lacking a reasonably sound investigation of physical processes. Some other weather modification experiments appear to have achieved limited success by paying close attention to these lessons. A future story will investigate the results of these experiments and the prospects for practical weather modification.

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Additional Reading

1. *The Management of Weather Resources*, vol. 2, *The Role of Statistics in Weather Resources Management* (Department of Commerce, Washington, D.C., 1978).
2. W. L. Woodley *et al.*, *J. Appl. Meteorol.* **21**, 139 (1982).
3. ——, *Bull. Am. Meteorol. Soc.* **63**, 273 (1982).

Slower Magnetic Fusion Pace Set

DOE drops its next big machine and establishes a Magnetic Fusion Advisory Committee to tell it how to progress to fusion power on a constant budget

It seemed to be too good to be true, and it was. In September 1980, Congress passed by unanimous voice vote the Magnetic Fusion Energy Engineering Act that called for the demonstration of an electricity-producing fusion reactor by the turn of the century. To spend the $20 billion that the act foresaw as necessary to achieve this goal, the Department of Energy (DOE) would have to more than double its annual expenditures on magnetic fusion, then at $394 million.

But the bill neither authorized nor appropriated a penny for this purpose, and the magnetic fusion budget likely to be adopted for fiscal 1983 is $444 million, which is $10 million below this year's figure. Administration spokesmen have repeatedly said that level spending should be the optimistic expectation for a number of years. DOE's former fusion chief, Edwin Kintner, resigned in protest last November. The latest casualty is the Fusion Engineering Device (FED). DOE has begun circulating within the fusion community a draft of a fusion plan that foresees no start before 1990, at the earliest, on any big new facilities—such as the FED—of the type that must precede a demonstration reactor.

All magnetic fusion experiments up to now have been—and those in the soon-to-be-completed Tokamak Fusion Test Reactor (TFTR) at the Princeton Plasma Physics Laboratory will be—investigations of the physics of plasmas and of how to bring plasma temperatures and densities up to those required in a working fusion reactor. The FED was to be the first machine aimed at collecting data on the numerous systems other than the plasma itself needed in a power-producing device. The data would contribute to the design of the demonstration reactor. The fusion energy act mandates that an FED be running by 1990 at the latest. A panel of DOE's Energy Research Advisory Board agreed that the time was right to move fusion into an engineering development phase and set $1 billion as a reasonable cost for such a machine, a finding that the board as a whole endorsed in August 1980.

But this year in late May, Alvin Trivelpiece, DOE's director of energy research, established a Magnetic Fusion Advisory Committee and hastily summoned the group to a 1 June meeting. Given the current budget situation, Trivelpiece explained to attendees of a recent "Industry-Government Seminar on Fusion Energy Development"* "the idea that we will be able to proceed immediately with the FED is a little bit hard to imagine." So, one charge to the new committee is to see "to what extent we can fulfill or satisfy some aspects of the intent of the magnetic fusion act using the equipment we now have."

Also in late May at a controversy-filled meeting with fusion scientists, officials from DOE's Office of Magnetic Fusion expressed a desire to abandon the FED. DOE's new strategy is contained in a document, now in draft form and circulating for comment, that will be part of a congressionally mandated plan for the development of fusion energy. Somewhere around 1992 will commence "the design, construction, and operation of the first fusion power-generating device, the engineering test reactor (ETR). The ETR will include elements of both the FED and [the demonstration reactor] from the previous strategy."

The turnaround is a bit surprising because as late as this April, DOE's current fusion chief, John Clarke, explained at a meeting of the American Physical Society that the department had great hopes of trimming the cost of the FED, thereby making it affordable. Nonetheless, a slower fusion program was all but inevitable.

When the Reagan Administration took office in January 1981, it made no bones about saying that its first objective was to reduce federal spending and slashed $46 million from former President Carter's fiscal 1982 fusion budget, which itself was well below the goal set by the fusion energy act. In July 1981, Secretary of Energy James Edwards wrote, "We have established that it is premature to establish fully the national magnetic fusion engineering center at this time." The center was the organization that was to build and operate the FED, among other things. Finally, in a November 1981 report, the Energy Research Advisory Board that had earlier endorsed a faster paced, engineering-oriented fusion program concluded that "a stretch-out of the program is possible if budgetary pressures demanded it."

The Administration's current view of magnetic fusion was presented at the industry-government seminar by John Marcum, an assistant director in the Office of Science and Technology Policy. Marcum discussed three points: the need for fusion, its technical progress, and budget prospects.

With regard to the need for fusion, Marcum observed that last year the United States produced almost 90 percent of the energy that it consumed. Moreover, he said, there are 50 years of exploitable oil and natural gas, 400 years of untapped coal reserves, and up to several thousand years of uranium if the breeder reactor works out. "The need or demand for fusion is clearly not a pressing need, not an urgent matter."

On the technical side, Marcum surprised much of the audience with the assertion that progress had slowed in the past 2 years. He referred specifically to scaling that was not as good as expected, presumably alluding to experiments on the Alcator C tokamak at the Massachusetts Institute of Technology and on the ISX-B, also a tokamak, at the Oak Ridge

*Industry-Government Seminar on Fusion Energy Development, sponsored by Atomic Industrial Forum, Electric Power Research Institute, and Fusion Power Associates, Washington, D.C., 22 June 1982.

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