

CHANGING PERCEPTIONS OF THE ISRAELI WEATHER MODIFICATION PROGRAM

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ABSTRACT

During the 1970s the Israeli weather modification program came to be viewed as a unique demonstration of the ability of cloud seeding to increase rainfall at the ground. This distorted view of its importance may have hampered the development of cloud seeding technology in the United States and in the World Meteorological Organization's (WMO) Precipitation Enhancement Project (PEP). Recent work has led to more conservative estimates of its relevance to the possibility of successful weather modification in other parts of the world.

1. PROMOTION OF THE ISRAELI PROGRAM AS A UNIQUE SUCCESS STORY

At the First WMO Scientific Conference on Weather Modification, which was held in Tashkent, USSR in 1973, Mr. Jack Warner presented a review paper on the status of precipitation enhancement. He stated that it was impossible to evaluate operational cloud seeding programs with any degree of certainty, and mentioned several requirements for establishing the reality of rainfall increases at the ground in a randomized experiment. The two principal requirements were that the experiment must show statistically significant evidence of increases and a plausible physical explanation for them. Warner stated further that very few cloud seeding experiments met the requirements. He viewed the Israeli program and the Climax program in Colorado as acceptable and thought that the randomized crossover experiment then being conducted in the Australian state of Tasmania might qualify in time.

Warner's acceptance of the Israeli results as real was based on the statistically significant evidence of increases obtained in the randomized project Israel 1, which ran from 1960 to 1967, and on related physical considerations. Using radar and aircraft data, Dr. Abraham Gagin had provided evidence that winter clouds in Israel were continental in nature, despite the proximity of the Mediterranean Sea, and therefore unlikely to produce rain by coalescence, and also deficient in natural ice nuclei. In other words, the clouds were seedable

according to the simplest version of the static seeding hypothesis, which had been given standing by the WMO itself.

I do not recall any vigorous rebuttal from the audience in Tashkent to Warner's sweeping conclusions. In retrospect, it is amazing how casually the work of Dr. Herbert Thom for the Advisory Committee on Weather Control in the 1950s, which dealt chiefly with operational programs, and the findings from several randomized projects carried out in countries other than Israel during the 1960s were discarded.

By the time the Second WMO Scientific Conference on Weather Modification convened in Boulder, Colorado in 1976, the number of "successful" programs had shrunk to one. Some questions had been raised about the randomization in the Climax experiments, which days **really** should be included in the data base, and the apparently conflicting results obtained from various data subsets. Little was heard about the Tasmanian project. Apparently its results were not as clear-cut as the program sponsors and operators had hoped for a few years earlier. On the other hand, confidence in the Israeli results was strengthened by the perception that the success of Israel 1 had been replicated in a confirmatory experiment (Israel 2, conducted from 1969 to 1975).

To the scientific reasons for accepting the apparent Israeli success as real, I would add a psychological one. By 1976, meteorologists had been subjected to 30 years of claims and counter-claims about the efficacy of cloud seeding. Some of

them appeared relieved to learn that there was only one program worthy of serious study, and that they could safely ignore the mass of confusing material that had been published about the rest.

2. IMPACT ON PUBLIC OPINION AND ON PEP

The unique status of the Israeli program was conveyed to reporters covering the 1976 conference by individuals whose positions in scientific organizations lent weight to their opinions, regardless of scientific merit. For the science reporters, the story had just the right "spin" to make it irresistible. Resultant articles mentioned the peculiar fact that only one country, Israel, produced clouds that had the complete set of characteristics required for successful precipitation enhancement. A 1982 article in *Science* was entitled "Cloud seeding: One success in 35 years." Dissemination of this view through both the scientific literature and the popular press must have contributed to the decline in political support for weather modification research that became increasingly apparent in the United States in the late 1970s and practically eliminated Federal funding for such research by 1988.

In November 1976 a WMO site-survey team visited Spain, Algeria, and Tunisia to look at possible sites for PEP, determine the availability of relevant climatological data, and see what logistical support the host countries could provide. Gagin, Warner, Dr. N. I. Vulfson, and I were members of the team, which was led by Dr. Rumen Bojkov of WMO.

Some members of the team argued that PEP should be designed as a replica of the Israeli program. This proposed requirement posed a dilemma. If the Israeli program succeeded because Israeli clouds possessed unique characteristics, then the only logical place to replicate it would be Israel itself. Other team members thought that a site exposed to maritime influences might be satisfactory. The apparent success of some projects along the coast of California lent support to their views.

The final choice of Valladolid, Spain as the base for PEP was influenced by many factors, with logistical considerations being among the most important. Although maritime air masses often pass over Spain during the winter, the location of Valladolid, over 300 km from the west coast of the

Iberian Peninsula and to the lee of some significant mountain ranges, made it marginally acceptable to persons favoring a continental site.

Site selection was not the only aspect of PEP influenced by the Israeli program. The acceptance of the static seeding hypothesis in its simplest form led some PEP scientists to assume that only non-precipitating, ice-free clouds with supercooled tops would be seedable. This assumption led to some very conservative estimates of the potential for precipitation enhancement by cloud seeding in the Valladolid area, and enthusiasm for PEP began to wane. PEP never conducted any seeding that promised economically important rainfall increases at the ground. Eventually, it just died.

4. REASSESSMENT OF THE ISRAELI RESULTS

Despite the accolades extended to the Israeli program, not all meteorologists accepted its purported results without question. The reported results were suspect on both statistical and micro-physical grounds.

Contrary to the prevailing wisdom of the time, Israel 2 was not a replica of Israel 1. The two projects had different target areas and there were differences in the conduct of the seeding. Seeding was conducted mainly from aircraft, and the tracks of the seeding aircraft were different for the two projects. In addition, some silver iodide generators were operated on the ground, and documentation of generator locations and criteria for their operation for the two projects was not readily available.

The methodology of the first statistical evaluations of Israel 2 differed markedly from that of Israel 1. Although Israel 2 used a crossover design in the conduct of the seeding, the first published evaluations dealt only with the north target area and evaluated it with the aid of upwind control stations. Nothing was said about the effects of seeding in the south area. In view of all these changes, Israel 2 should never have been accepted as a confirmatory experiment.

At the 1989 annual meeting of the Weather Modification Association, Drs. Ruben Gabriel and Daniel Rosenfeld presented a statistical analysis of Israel 2 rainfall data according to the crossover design. The analysis yielded no evidence of overall rainfall increases or decreases. They went on to

consider events in the north and south target areas separately, and found some indications of precipitation increases in the north area and decreases in the south area. These points are discussed at length in recent papers by Rosenfeld and his colleagues, who have postulated a desert-dust hypothesis to explain the lack of precipitation increases in the south area. However, the crossover design is based on the assumption that seeding effects in both target areas are the same. Any investigation that goes beyond that assumption is only exploratory.

The microphysical explanations offered by Gagin have also been questioned. Satellite photos show that much of the winter rain in Israel falls from banded cloud structures, often overlain by cirrus and cirrostratus clouds, and not from the isolated cumulus congestus clouds that figured prominently in his data base.

Valuable work on the microphysical aspects of Israeli clouds and precipitation mechanisms in them has been published by Mr. Arthur Rangno and Dr. Peter Hobbs. Their data indicate that rain sometimes falls in Israel from shallow clouds barely reaching to the 0°C level and that ice particles are found in many clouds at temperatures only slightly below 0°C. These findings bring into question the physical arguments that seemed so persuasive in 1976.

4. A PERSONAL VIEW

The most solid evidence of artificial precipitation enhancement in the Israeli program comes from Israel 1, which was *analyzed according to the original crossover design*. Cross-target contamination can not account for such an apparent effect because, if there is no effect, contamination can not matter. A tendency for natural rain to be heavier on north-seed or center-seed days would not account for the apparent effect either, because a randomized crossover experiment is designed to compensate for such a possibility. The probability that the apparent effect is the result of a bad draw is given by the p-value, which is near 0.02. The fact that the static seeding hypothesis in its simplest form has not proven adequate to explain that apparent success is not, of itself, sufficient grounds for rejecting it. After all, there are other conceptual

models of seeding effects. The failure of Israel 2 (viewed as a single crossover experiment) to confirm is not sufficient grounds for rejection of Israel 1 results either because, as we have seen, the two projects differed in locations of target areas and in seeding procedures.

The Israeli program is like many others in its mix of contradictory indications regarding results. Perhaps the current lively debate over the significance of the recent findings, in both its statistical and microphysical aspects, will prove more helpful in the long run than did the misguided impression that the problem of precipitation enhancement had been solved in a single, unique program.

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