The Cloud Seeding Literature and the Journal Barriers to Faulty Claims:

Closing the Gaps

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ABSTRACT

Independent re-analyses of journal-published cloud seeding experiments have usually led to the discovery of flaws that contravene or at least cast significant doubt on the original published report. These flaws could have been, and perhaps, should have been, detected in the peer review process prior to publication. The flaws have recurring aspects. A review of two highly acclaimed sets of randomized cloud seeding experiments demonstrating these flaws are used to illustrate weaknesses in our peer-review system. Whether these weaknesses in peer review are still present in contemporary cloud seeding literature is also investigated; the answer is, "yes."

Several steps are suggested to improve peer review in the cloud seeding literature. These steps include mandatory reporting of random decisions and other project data in real time, mandatory analysis requirements, use of our best models to elucidate biases in random draws, and use of a wider range of independent experts in the review of cloud seeding manuscripts among others.

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1. Introduction

Scientific articles published in peer-reviewed journals, such as our American Meteorological Society journals, disseminate special knowledge that must overcome several barriers before it can appear in print (e.g., National Academy of Sciences 1989, 2009, Foster and Huber 1997). These barriers are intended to prevent faulty or poorly supported claims from appearing. Should a false claim nevertheless be inadvertently published, those members of the journal readership with expertise in the topic can be expected to, and some would say, have a responsibility to publish criticisms of faulty claims so that they are prevented from being widely accepted. Because the acceptance of faulty science is minimized, science moves forward and society benefits. This process is much like the dominant team, "truth", in a never-ending baseball pennant race in which the teams "honest error," "self-deception," and "fraud" occasionally win a few games. However, these never influence the "final" outcome.

The barriers to the publication and acceptance of faulty science will be laid out; followed by brief review the history of modern cloud seeding to demonstrate the difficulties that "proof of an effect" posed and the subsequent rationale for randomization of experiments.

The results of two sets of randomized cloud seeding experiments are examined in detail to investigate whether randomization worked as advertised to eliminate storm and experimenter bias.

The question will addressed whether peer-review should have caught the missteps in the original journal published manuscripts that were subsequently documented. Some remedies against faulty claims are suggested based on these case studies.

2. What are the barriers to the publication and acceptance of faulty scientific claims?

a) *Peer review of proposals*.

Faulty science is less likely to be funded in the first place because proposals for scientific research are reviewed by two or three scientists familiar with the area in which the proposed research is submitted. They determine whether the research is sound and worthy of financial support. Unfunded (hobbyist) research is less likely to be submitted for publication than is funded research--which can be seen as both an asset and a liability.

b) *Peer review of articles submitted for publication in scientific journals*.

Faulty science is less likely to appear in scientific journals because submitted articles are also subjected to reviews by two or more scientists who are supposed to be experts on the subject of the article.

c) *Post-publication critiques of published articles by the journal readership or reviewers who feel an article is flawed.*

Problems or questions about suspect research that may have leaked through the first two barriers can be discussed in open literature for a further redress of the claims made in the original article.

d) *Self-correction*.

Should the authors of a paper discover an error in their conclusions or in important data, it is assumed they will report the error and retract or modify their findings in a timely manner.

e*) Independent replication*.

This is the most important barrier to the acceptance of faulty science. Experimental results must be replicated, and replications considered routine before they are subject to widespread acceptance. For maximum credibility, replication of experiments is carried out by laboratories or workers who are independent of the original researchers or the institutions from which the initial findings emanated.

Due to the public nature of cloud seeding experiments, we also have an additional safeguard that is tantamount to reviewing the lab notes and data of laboratory experimenters since precipitation data on which the results rest are often available through government publications:

f) *Independent validation of experimental results via reanalysis*

A researcher uses the same data sources (runoff or precipitation data that is often publicly available) that the original experimenters stated they used to form their conclusions. The independent researcher tries to replicate or expand the reported result based on these data using the same test statistic. Searches for alternative controls or other variables not considered by the original experimenters usually do not occur. This is because *post facto* investigations using alternative variables can lead to problems of multiplicity, that is looking through too many variables, which by chance can either validate or nullify a reported result (e.g., Tukey et al. 1978a, b).

Therefore, the independent investigator has a special duty to demonstrate that his results are a plausible extension of the methods and variables used by the original experimenters. In this most limited form, a reanalysis can be considered a form of independent replication of an experiment; only data errors, or regional patterns that were not noticed by the original experimenters can emerge.

The persistent character of the cloud seeding literature: controversy and disdain

The barriers to the publication of faulty scientific claims described above have been known to fail, sometimes spectacularly (e.g., Broad and Wade 1982, Feder and Stewart 1987, Foster and Huber 1997). Hence, we should not be surprised if we discover failures in our own domain of cloud seeding. The journal literature in cloud seeding has been subject to lively debate and strong differences of opinion throughout its history (e.g., Fleagle et al. 1969, Byers 1974, Elliott 1974, 1986, Braham 1979, Changnon and Lambright 1990), and it can be argued that this is due to faulty literature reaching the journals.

Some of the assessments by leading academicians, responding to exaggerated claims of seeding effects, and faulty evaluations, have been severe. Surveying the field, Byers (1965) wrote that, "In many parts of the world, including the United States, public policy concerning weather control' is often guided by claims of cloud-seeding success based on evidence so questionable as to seem farcical to a sophisticated statistician." Braham (1979), echoing Byers 15 years later suggested that, "....within meteorology and statistics alike, weather modification and weather modifiers are often viewed with suspicion and disdain." And one prominent statistician who was intimately involved in this field for 30 years was moved to conclude that "much of the cloud seeding literature is slanted and unreliable," (Neyman 1980). Most recently, Hobbs (2001), commenting on a recent survey of cloud seeding experiments by Silverman (2001), echoed Neyman's assertion, describing the cloud seeding literature as "often unreliable."

What other field of science would have so many perverse statements by respected academicians concerning their own field? And why is this?

There is a simple answer: inadequate reviews of cloud seeding manuscripts that repeatedly allowed faulty claims to enter the field's literature. And because faulty literature enters the field so often, it triggers needless controversies (e.g., Changnon and Lambright 1990), which may not have occurred had reviews of manuscripts been stronger in the first place.

Experiments are also compromised and instigate controversy when the measurement of precipitation, choices of control gauges, or other critical experiment variables and the experimental data are collected and archived by the same organization that potentially benefits from a successful experiment. This introduces the possibility of bias (unintended or otherwise), and therefore, degrades the credibility of experiments, and fuels controversy (Rangno and Hobbs 1995a, hereafter RH95a; Mielke 1995)

Whether a cloud seeding experiment appeared to produce an increase or a decrease in rain appears to stimulate different responses. Evaluations of cloud seeding experiments published in journals that find that seeding decreased rainfall can have a cautionary effect on cloud seeding activities[2] and can invite, as did Project Whitetop, vigorous debate and independent re-analyses over many years (e.g., Braham 1979).

However, reports of cloud seeding successes do not appear to lead to such profound immediate stimulation of reanalysis activity involving numerous independent investigators as did Project Whitetop. In the two sets of acclaimed experiments examined in detail in this article, it was the *absence* of vigorous debate about them when they were first being reported, and for many years thereafter, that ultimately allowed them to prosper and gain a large amount of "scientific inertia" as unambiguous successes for a long time[3]. Yet, it can be argued that published reports of an ersatz cloud seeding success can have far more profound and costly consequences than a negative cloud seeding outcome. For example, erroneous published reports of a cloud seeding success, backed by what appear to be solid and supportive cloud microstructural studies (which in reality, were ersatz, have led to:

[2] No replication of Project Whitetop was considered due to moral considerations following the initial analyses which found evidence for widespread decreases in rainfall due to seeding.

[3] The Climax experiments, whose flaws are discussed at length in this article, are nevertheless being cited even today by a few researchers as having indicated increases in snowfall (e.g., Breed et al. 2014)

1. delayed progress in weather modification by delaying field studies of cloud microstructure and dispersion of the seeding agent that are needed but are skipped because the journal-reported statistical successes accompanied by the experimenters' reports of cloud microstructure have made it appear that new, similar studies had a low priority,
2. discouraged funding of *independent* efforts to replicate results since, in view of the high cost and complexity of field experiments, and in the face of "proven" results, it may be deemed that these are not needed or feasible,
3. caused inaccurate assessments of cloud seeding skill by professional organizations which monitor the field-at-large;[4]
4. led to ill-advised and costly non-scientific, commercial cloud seeding projects funded by local governments or private companies which have relied on misleading assessments of the status of cloud seeding by respected professional organizations;
5. eroded public confidence in the scientific establishment, as when any faulty scientific research is overturned.

3. Brief history of modern cloud seeding: the rationale for randomization.

The following highlights of early cloud seeding experimentation will illustrate the problems that were encountered by the early experimenters and why randomization of experiments became the *modus operandi* and for credible cloud seeding results published in peer-reviewed journals.

Attempts to replicate the spectacular seeding results first reported in the literature (Schaefer 1946, and Kraus and Squires 1947) met with limited success and soon, with

[4] For example, assessments by the National Academy of Science's Panel on Weather Modification, American Meteorological Society's and World Meteorological Organization's Committee on Planned and Inadvertent Weather Modification.

controversy. While it was easy to create ice canals in thin supercooled Altocumulus clouds as Schaefer (1946) had done, the demonstration of a seeding effect in more complex situations was daunting. When the U. S. Weather Bureau attempted to replicate the results that were beginning to appear in the literature in the late 1940s, it was not clear in their experiments whether more precipitation was reaching the ground than would have occurred naturally (Coons et al. 1949, Coons and Gunn 1951). This was because when precipitation did reach the ground after a cloud had been seeded, it could not be determined whether seeding had merely accelerated a natural event that was going to occur. Similar, natural clouds in the vicinity were almost always precipitating. And, no one knew whether the precipitation that did fall after seeding was more or less than would have evolved naturally. Often, only trivial amounts of precipitation reached the ground. In no case, were they able to replicate the spectacular isolated growth of a Cumulus cloud into a Cumulonimbus that produced heavy rain over "at least 20 square miles" area as Kraus and Squires (1947) had reported, causing so much excitement.[5]

In addition, Coons et al found a flaw in the underlying hypotheses behind cloud seeding; that clouds were largely ice-free until their tops were colder than about -20°C when they encountered cloud warm-based clouds with ice in tops as warm as -6°C. Coons et al were to report what was documented in Missouri by Koenig (1963) and Braham (1964) almost 15 years ahead of them.

A series of more sophisticated experiments than those by Coons et al. (1949) were carried out by government and academic scientists a few years later, but once again, the results were ambiguous or no effects at all were observed (Pettersen et al. 1956).

When U. S. Weather Bureau personnel or other independent meteorologists examined early published claims of cloud seeding successes from seeding projects (e.g., MacCready 1952), they often found that the evidence was actually ambiguous or insufficient to support the original claim because the experimenters used rather limited data or statistical tests (e.g., Brier and Enger 1952, Amer. Meteor. Soc. 1953).

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[5] Apparently Kraus and Squires were never again able to produce the effect they reported in 1947 in their subsequent flights.

However, some commercial cloud seeding operators argued that government scientists were not as experienced as they were in carrying out seeding projects. In response to these claims, Thom (1957), on behalf of the Advisory Committee on Weather Control, evaluated a select number of commercial cloud seeding projects that appeared to have the best data bases. Thom concluded from his analyses that precipitation, in fact, had been increased by about 10% in several commercially-run orographic projects. These increases were deemed statistically significant by Thom. Thom found no detectable effects of cloud seeding in non-orographic settings.

Thom's findings, however, were subject to severe criticisms by some statisticians (e.g, Brownlee 1960, Neyman and Scott 1961). This was mainly because the commercial projects Thom examined were not randomized, were subject to optional starting and stopping times which could create spurious seeding effects, and because they were only a few of the many commercial orographic projects that had been carried out.

Despite these criticisms from statisticians, the idea that precipitation might be increased in orographic settings by cloud seeding has remained a doctrine supported by the Amer. Meteor. Soc. since Thom's report (e.g., Amer. Meteor. Soc., 2011).

It was becoming clear from the vigorous debate swirling around cloud seeding in the early and mid-1950s that the detection and scientifically acceptable proof of an economically important effect from seeding clouds was going to be much more difficult to prove than had been initially expected. Only careful, randomized experiments would be able to properly evaluate the effects of seeding so that experimenter (and storm) bias could be removed as much as possible from the seeding trials and evaluations, to establish a baseline of credible scientific methodology.

The era of randomized experiments was then launched with the beginning of several important long-term experiments in Australia, United States, and Israel in the late 1950s or early 1960s (cf., Mason 1980; 1982).

4. The era of randomization of cloud seeding experiments: Did it remove "experimenter" and "storm" bias as intended?

Table 1 is a list of randomized experiments that have appeared in the journal literature and have been subject to *both* analysis and reanalysis or critical commentaries. Table 1 strongly suggests that the answer to the question posed in the title of this section is "no." This is because those re-analyses and commentaries significantly weakened or removed the initial results; serious flaws were discovered that had escaped the attention of the original experimenters.[6]

Table 1 reveals this when randomized experiments are reanalyzed, usually by those who did not take part in the experiments. Instead of the independent evaluations of cloud seeding experiments merely confirming or expanding the original (usually optimistic) finding, the independent re-analyst most often finds insufficient evidence for a previously claimed seeding effect.

For example, in Table 1 flaws were found in 13 of the 18 original reports of increases in precipitation due to seeding. The flaws in those analyses were serious enough that they weakened or eliminated the credibility of the former optimistic result. Using the binomial theorem, the null hypothesis that an independent re-analyst will confirm an a report of increased precipitation due to seeding can be rejected at the 0.04 level. The outcomes of the few re-analyses by the original experimenters also often result in a weakened claim for seeding effects or cannot substantiate them, corroborating this trend (e.g., Gelhaus et al. 1974; Mielke 1979; Gabriel and Rosenfeld 1990). No independent re-analyst has found indications of a seeding effect *larger* than was in the original reports by experimenters.

In most of these cases, the independent re-analyst expanded the original analysis by the experimenters to find that the same effect attributed to seeding in the target was also observed in regions where seeding could not have occurred or would have been minimal. Such findings are sometimes called "lucky draws" or more technically a "Type I" statistical errors where the null

[6] The latter have usually been carried out by individuals removed from the conduct of the experiment.

hypothesis of no seeding effect has been erroneously rejected.

Therefore, Table 1 suggests that flawed reports of randomized cloud seeding successes have breached journal barriers against the publication of faulty claims on numerous, and ultimately, with costly ramifications. The flaws discovered do not appear to have been dredged up in "SORTIES" (search and destroy missions) by anti-seeding fanatics using esoteric variables to dispose of seeding effects. Rather, re-analysts have used the original experimenters' own statistical tests.

The purpose of this review is to find out why journal published re-analysts and "commentators" almost always turn up major flaws that the original experimenters, and implicitly, the reviewers of such papers, failed to recognize. In doing so, the author will examine the two most widely accepted, but ultimately flawed sets of randomized cloud seeding experiments to make the point that the barriers to the publication of faulty claims in the peer-reviewed journal literature are inadequate.

5. Examples of Faulty Literature that Breached Peer Review

Figure 1a-d shows data from several journal-published cloud seeding experiments that seemed to unambiguously support the case for a strong effect on precipitation or runoff due to cloud seeding. However, in each of the cases shown in Figure 1, when the same controls that the experimenter chose to elucidate seeding effects in the target area were used for upwind and side wind regions, the same precipitation or runoff anomalies attributed to seeding were also seen (Figure 2). Hence, in a region-wide view it was a small group of *controls* that had actually behaved anomalously on seeded days (having low precipitation or runoff) rather than the target area having a localized, positive one. The complete discussions of these seemingly robust experiments can be found in the references in the figure caption. The results of the re-analyses, by the way, should not be construed as meaning that there no seeding effect whatsoever in those experiments; it simply wasn't detectable in a statistically-significant way.

In the four examples shown in Figures 1, the Type I errors (random draws that favored seeded days or seasons relative to the controls) were not caught in the peer review process; nor were there journal-published criticisms of these faulty cloud seeding claims for many years, if at all. Nor did the authors of the papers themselves detect faults, or, if they did, did not find them until many years after the fact. Thus, the journal barriers that we depend on to prevent the initial publication of faulty claims in the cloud seeding domain *do* have gaps, and self-detection of flaws does not come into play.

The "sign" of the faulty claims by the experimenters, one cannot fail to observe, is generally in the same direction; that is, to report that a cloud seeding experiment was more successful than it actually was. Also, many subsidiary statements about how the experiments were carried out that made the findings look more robust were, in fact, ersatz. Because of this tendency, the errors by experimenters evaluating their own experiments do not appear to be random; we can confidently conclude that subjective factors crept into the reporting of cloud seeding experiments by the scientists who originally conducted them.

It should not be surprising that this might happen; "blind" and "double blind" experiments are an accepted way of conducting laboratory experiments, not because we think that most lab doctors are crooks and will cheat if they have the chance; but rather because we have learned painful lessons about how powerful subjective feelings can be in our interpretations of the "cure" we've administered.

However, the kinds of stringent precautions as those mandated in laboratory experiments are rarely completely taken in cloud seeding experiments, leaving the door open for subjective influence.

In this context, it becomes relevant, therefore, to try and determine why the peer review process failed in the realm of the cloud seeding literature and what remedies there might be against intrusions of sincerely believed, though misleading reports.

In the next section, a detailed look into this problem reveals that several sometimes subtle but recurring factors crept into the original analyses that misled both the experimenters, the reviewers, and ultimately, the journal readership for many years. In the following Section, two of the four experiments in Fig. 1 will be examined more closely.

6. An Examination of Two Highly Acclaimed Sets of Randomized Cloud Seeding Experiments

a. *Cloud seeding in the Colorado.*

"Hence, in the longest randomized cloud-seeding project in the United States (at Climax, CO), involving cold orographic winter clouds, it has been demonstrated that precipitation can be substantially increased and on a determinate basis." National Academy of Sciences (1973)

A series of three extremely important and apparently highly successful randomized cloud seeding experiments took place at Climax and Wolf Creek Pass, Colorado, during the 1960s. For a time, these experiments appeared to end the remaining doubt about whether cloud seeding in mountainous regions could produce significant snowfall increases under certain conditions. The results were stunning--increases of 50% and more were reported on favorable days (e.g. Grant and Mielke 1967); and the results were widely quoted without reservations by prestigious national panels and in numerous textbooks (e.g., National Academy of Sciences 1973, Sax et al. 1975, American Meteorological Society 1984, Wallace and Hobbs 1977, Mason 1980, 1982, Moran and Morgan 1986). The results of the experiments in the Rockies continued to be cited by a few authors (e.g., Cotton and Pielke 1995, 2007, Breed et al. 2014), though they have generally fallen out of favor with most scientists for reasons that will be made clear.

Why were these Colorado experiments so convincing to the scientific community when they were first reported?

They were so convincing, *en toto,* because they appeared to provide very strong evidence of snowfall increases in no less than *three* independent, relatively long-term, randomized experiments. The first two, the daily randomized Climax I and II experiments ran for portions of eleven winter seasons (Grant and Mielke 1967; Mielke et al. 1970, 1971; Chappell et al. 1971; Grant and Kahan 1974). The third, a seasonally randomized experiment at Wolf Creek Pass, CO, ran for six complete winter seasons (Morel-Seytoux and Saheli 1973). These experiments appeared to confirm one another in the conditions under which seeding produced increases in snowfall. This was when the 500 hPa temperatures were above -20°C to -23° C; large increases in snowfall occurred when the clouds were seeded under this conditon. In the Wolf Creek Pass experiment, the extra snowfall produced over the entire seeded winter seasons was seen in large amounts of extra runoff from the target rivers in the three seeded seasons when compared with control river runoff (Fig. 1a).

Also lending credibility to these statistical results was the fact that the experimenters also had what appeared to be a plausible reason why the increases in snowfall had occurred. The 500 hPa temperatures, they claimed, were markers for cloud top temperatures (e.g., Grant and Mielke 1967; Mielke et al. 1981), and that cloud top temperatures, in turn, were measures of the ice crystal concentrations in the clouds (e.g., Grant 1968). Therefore, when 500 hPa temperatures were high (i.e., ≥-23° C) during storms, cloud top temperatures had to be warm, and the clouds, they further reasoned, contained so little natural ice that they were unable to precipitate.

Also, ice multiplication, a phenomenon in which ice crystal concentrations are far higher than those that can be accounted for by ice nucleus concentrations (e.g., Hobbs 1969, Auer et al. 1969), did not occur in the Rockies (Grant 1968). Ice multiplication is considered strongly detrimental to the type of cloud seeding carried out, termed "static" carried out (e.g., Dennis 1980). In static seeding, the clouds are targeted with relatively small amounts of silver iodide, just enough to get them to precipitate.

Lending further credibility to the descriptions of the Colorado results was the fact that the seeding effect was limited to extending the duration of snowfall only and had no discernible effect on intensity (e.g., Chappell et al. 1971). This was compatible with the type of clouds being seeded and the way that they had been seeded--cold wintertime stratiform clouds seeded by ground generators which released relatively small doses of silver iodide--an intensity change produced by the small amounts of seeding material released would have been difficult to explain.

These three Colorado experiments, therefore, comprised an amazingly complete and stunningly successful picture of cloud seeding results founded in what appeared to be a logical physical picture. It is not hard to understand why the journal publication of these many results and the many supporting factors instilled great confidence, a consensus, in the scientific community that the seeding effects reported in Colorado were real and not mere statistical flukes (e.g., National Academy of Sciences 1973, Mason 1980).

Further, the reports from the Colorado scientists concerning their experiments appeared at a time of increasing optimism on the part of the scientific community about the ability of cloud seeding to increase snowfall in orographic clouds (e. g., National Academy of Sciences 1966). The scientific community in weather modification was primed for a success to be reported in a randomized orographic cloud seeding experiment.

b. *Cloud seeding in Israel*

"Almost every review of the status of weather modification published since 1970 has described the Israeli experiments as providing the most convincing evidence available anywhere that cloud seeding can, in fact, increase average rainfall over an area. The credibility of the reported rainfall increases from Israel I and Israel II is due to impressive compilations of statistics and to Dr. Gagin 's cloud physics studies, which provided a plausible explanation for the rainfall increases suggested by the statistical analyses". Arnett S. Dennis (1989)

At about the same time the Climax and Wolf Creek Pass experiments were first being reported in the journals in the mid and later 1960s, another landmark experiment conducted in Israel was also being reported for the first time in the peer-reviewed literature. The experiments were conducted under the aegis of scientists at the Hebrew University of Jerusalem (HUJ). The first of two daily randomized experiments (called Israeli I[7]), had two targets, one of which was designated in advance to be seeded each day during the Israeli rainy season. This type of experiment has been referred to as a "crossover" experiment in which the results of seeding are combined from the two target areas. In this way, the experimental data builds rapidly compared to single target experiments. It was assumed, at least in this case, that there is appreciable correlation in rainfall between the two targets and that the natural cloud microstructure in the two targets is virtually the same. The two targets were separated by a small "buffer zone" that was to be left unseeded. The seeding in the first experiment was carried out by a single aircraft flying parallel to and within about 10 km of the coastline for about 65-75 km legs[8] each way upwind of each target on its seeded day[9]. After 2 ½ winter seasons, the seeding track was moved inland (Neumann et al 1967).

This seeding method was identical to, and probably patterned after that used in the important Project Whitetop experiment then underway in the U. S. (Braham 1979).

The first experiment lasted six and a half rainy seasons. The results of seeding appeared to have produced statistically significant increases in rainfall of 15% when the results in *both* targets (called "North" and "Center") were combined (e.g., Gabriel 1967a, 1967b, Neumann et al 1967, Gabriel and Feder 1969, Gabriel and Baras 1970, Wurtele 1971, Gagin and Neumann 1974, hereafter, GN74). Further, the seeding effects were larger in the Center target area than in the North target area, and they were larger farther inland from the coastline (GN74).

Oddly, the seeding effects were greatest of all in the small "buffer zone" region between the two targets that the seeding aircraft had tried to avoid (Wurtele 1971, GN74). This discovery was later inferred by the experimenters to be unintended seeding effect (GN74), though Wurtele (1971) had quoted the Chief Meteorologist of the seeding experiment stating that seeding could only have affected the buffer zone "5-10% of the time" and "probably less."

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[7] Both Roman numerals and numbers have been used when referring to these experiments over the years.
[8] Legs were shortened in those cases where there was no clouds ahead, at least during daylight flights.
[9] Several ground generators were located in the extreme northeast of the North target area near the Syrian border.

Most remarkably. perhaps, line seeding was carried out for an average of only 4 h per day by this single aircraft to produce the statistically significant results in each target area and, apparently, in the buffer zone (Gabriel 1967, Table 1). Brier et al. (1973), in an independent re-analysis, examined rainfall in Lebanon and Jordan, and while confirming and extending the seeding effects, they also found some indications for seeding effects in regions which could have only been marginally seeded if at all.

A second daily randomized experiment, Israeli 2, was carried out from 1969-70 to 1974-75. This second experiment was also a crossover experiment in which random seeding took place in two target areas, this time called "North" and "South." The North target area was shifted inland from Israeli I (e.g., GN81). The South target area was appreciably larger than in Israeli I. It included the area of the "Center" target area of Israeli I as well as a large area to the south (GN74; Gabriel and Rosenfeld 1990). A narrow coastal region located upwind of the North target area that exhibited a high correlation in rainfall ($r \approx 0.9$) with the North target farther inland was designated as a control area since the target for the North had been shifted inland from the coastline.

The amount of seeding was significantly increased from the first experiment by adding a second aircraft and installing a network of 42 ground generators (NAS 1973). The ground generators were added for more effective seeding of the inland hill region than had been the case in the first experiment. However, the complete seeding details of the second experiment have not yet been reported.

Israeli 2, therefore, had several design/evaluation components, 1) a crossover design using the combined data from both targets, 2) a target/control design for the North target area, 3). using the rainfall data for one target on all of its seeded and control days (single area evaluation), 4) using the rainfall in the adjacent, non-seeded target on the days that seeding takes place in the adjacent target. According to GN74, the advantage of the latter method, (4), was to eliminate (the inevitable) storm bias on the seeded days of each target because a heavy storm was likely to affect both regions on the same day because of their proximity. It was a sound argument[10].

However, the results of the completed second experiment were limited for more than 14 years to just target-control evaluations of the North (e.g., GN81, Gagin 1981, hereafter, G81, Gagin 1986, hereafter, G86, Gagin and Gabriel 1987). These limited evaluations of the second experiment appeared to offer an unambiguous confirmation of the seeding results of Israeli I and were cited on numerous occasions by other scientists as having demonstrated a confirmation of the first experiment, and as a "stand alone" seeding success by numerous scientists and organizations (e.g., Tukey et al. 1978a, 1978b, Simpson 1979, Mason 1982, Kerr 1982, Braham 1986, Silverman 1986, Cotton 1986, Dennis 1989; World Meteorological Organization 1992, Young 1993, Cotton and Pielke 1992, 1995).

But another ingredient for widespread acceptance of the statistical results of the Israeli experiments was in the making during the 1970s: cloud microstructure reports began to appear in the journal literature. These reports described the clouds of Israel as unusually ripe with seeding potential. Just as the scientists had in the Colorado experiments, the HUJ scientists reported that ice crystals were relatively rare in Israeli clouds until their tops became colder than -21° C (e.g., G75, 1981, 1986, Gagin and Neumann 1976, 1981, Figure 3, dots).

Because cloud bases in the eastern Mediterranean are located at temperatures almost exclusively between 5°C-12°C at around 700-800 m above sea level, it appeared that there was a relatively great depth of liquid water both above and below the freezing level in which the introduction of artificial ice crystals (and later as raindrops) could take advantage. These cloud

[10] Gagin and Neumann (1974) wrote: "In the (crossover) design only one of the two experiment areas is seeded on any one day, the area being determined in a random manner. On the same day the second area serves as a 'control' area."

reports lent considerable credibility among scientists to the view that the statistically significant results obtained in two randomized cloud seeding experiments were indeed real and not statistical flukes.

Thus, the ice-forming characteristics wintertime cumuliform clouds of Israel, as unlikely as it might seem at first glance, was being reported as a mirror image of the cloud microstructure of the wintertime stratiform clouds in Colorado. Nor did ice multiplication occur in the clouds of Israel according to researchers (e.g., G75, G81, G86).

In refining the statistical results of Israeli 2, HUJ scientists reported that radar studies showed that it was *only* those clouds with radar tops between -12° and -21° C that were responsible for the increases in rainfall (e.g., Gagin and Neumann 1976, GN81, G81, G86). When the Israeli 2 results were confined to the effects of seeding on clouds with radar tops between -12° and -21° C, rainfall was increased by seeding by 40-50%. More importantly, this "cloud top" temperature stratification improved the already statistically-significant overall results for the North target area (in the target/control evaluations--e.g., GN81).

In both the exact temperature range in which seeding appeared to have produced the greatest seeding results, and in the magnitude of the response in precipitation to seeding, the Israeli experiments appeared to be a mirror image of the results that had been reported by Colorado scientists a few years earlier.

But the HUJ scientists had an important edge over the Colorado experimenters; they appeared to have *measured* the tops of the clouds that produced the large seeding results whereas, in contrast, the Colorado scientists had merely *assumed* that a strong relationship existed between 500 hPa and cloud top temperatures and had not actually measured cloud top temperatures (M79, Hobbs and Rangno 1979, hereafter, HR79, Grant 1986).

Also, both the Colorado and HUJ experimenters had presented results that the seeding effect ceased at cloud top temperatures above about -12° C. This was because of the low nucleating activity of the silver iodide used to seed their respective clouds at these higher temperatures and because the clouds with top temperatures this warm were too shallow to produce appreciable precipitation at the ground even if extra ice crystals did form (e.g., GN81).

It was also reasoned by both groups of experimenters that the presence of too many natural ice crystals (>about 10 l^{-1}) had resulted in a cutoff of the positive cloud seeding effects at "cloud top" temperatures below about -20° C.

The final parallel reported between the experimenters in Colorado and Israel was that the effect of seeding was to increase the duration of precipitation with little if any effect on the intensity (e.g., Chappell et al. 1971, G86, Gagin and Gabriel 1987). These last findings lent additional credibility to their respective results, as noted previously, the relatively low temperatures of the clouds and the small doses of the seeding agent released made it seem reasonable to most other scientists that *only* the duration of precipitation could have been affected by this type of seeding in both experiments.

Hence, in every way, despite the differences in seeding techniques (ground vs. airborne) and the types of clouds seeded (stratiform vs. cumuliform), the results of the two experiments were virtually identical. The two sets of experiments conducted in Israel, too, were considered complete and credible in every way by the scientific community, just as they had been in Colorado. Thus, with the Climax I and II, and the Israeli 1 and 2 statistical and supporting microstructure reports in hand, the 1970s and early 1980s were indeed the "glory years" of confidence (and federal funding) in the field of cloud seeding as described by Cotton and Pielke (1995, 2007).

7. The unraveling of the experiments.

Could all of these glowing statistical results supported by seemingly solid cloud microstructure studies and various subtle, supportive seeding effects described above really be "scientific mirages" (Foster and Huber 1997)? And as such, could they still be published in our peer-reviewed journals?

As we will see, the answer to this last question appears to be, "yes."

a. The Colorado experiments

The experiments at Climax and Wolf Creek Pass probably first began to unravel with the reanalysis by Meltesen et al. (1978) who showed that a natural storm bias on seeded days led to the misperception that seeding had increased snowfall downwind from Climax in the eastern Colorado plains. Melteson et al.'s report meant, indirectly, that the random draw of the Climax experiments had been meteorologically uneven as well. Mielke (1979, hereafter, M79) followed with a stunning report that *both* Climax I and II experiments had been impacted by Type I statistical errors ("lucky draws") due to random draws that produced naturally heavier precipitation on seeded days. He reported that the effects on precipitation at Climax, which had been attributed to seeding, were also observed over wide areas of western Colorado that could not have been seeded.

But Mielke went even farther: he also acknowledged that the stratifications of the experiments by upper level temperatures were based on a faulty understanding of the meteorology in the region and that cloud top temperatures could not, in fact, have been reliably known in the Climax experiments. Mielke's 1979 findings were repeated by Grant et al. (1979) at conference[11].

HR79, independently examining the foundations of the Climax experiments, found that the experimenters had no evidence for their original claims of a close relationship between upper level temperatures and cloud top temperatures. In fact, Cooper and Marwitz (1980) found that the coldest precipitating cloud tops—those well above the 500 hPa level in winter storms in the Rockies--were usually associated with higher temperatures at 500 hPa, thus further undercutting the assumption of a viable link between those two temperatures as had been claimed (e.g., Grant and Mielke, 1967, Grant and Elliott 1974, Mielke et al. 1981).

[11] Presented by J. O. Rhea.

The WCP experiment, the third piece of the Colorado cloud seeding triad, was also reanalyzed at this time. It was found that this experiment, too, had suffered from a lucky draw or Type I statistical error (Rangno 1979, hereafter, R79). The effects that had been attributed to seeding in the target watersheds were also observed over a several state region. Because so many watersheds in a multi-state region were high relative to the chosen controls, it was the control runoff that had behaved anomalously rather than the target area runoff in the WCP.

In later dispersion studies at Wolf Creek Pass, Hobbs et al. (1975) found that seeding material was not reaching the clouds, or if it did, it was at locations so close to the crest that could not have produced a fallout of snow in the target. For comparison, the cloud seeding generator releases studied by Hobbs et al. were more numerous and they were situated at higher locations than those that had been used in the WCP experiment.

Moreover, ice multiplication *does* occur in the wintertime clouds of the Rockies (Auer et al. 1969, Vardiman 1972, Vardiman and Grant 1972a, 1972b, Cooper and Saunders 1980, Cooper and Vali 1981). These findings weakened the early claims of high seeding potential for wintertime clouds in Colorado (e.g., Grant 1968). And little correlation between cloud top temperatures and ice particle concentrations was found (Vardiman and Hartzell 1976) at Wolf Creek Pass, and by DeMott et al. (1982) in the central Rockies, further undercutting a crucial physical argument used to explain the Climax and WCP statistical results.

Thus, the Climax and Wolf Creek Pass experiments were mortally flawed by uneven random draws that favored seeded days and by the lack of a physical basis to explain the supposed results.

However, despite these mortal flaws, the Colorado experimenters began publishing new reanalyses of the Climax experiments. These reanalyses attempted to account for the uneven random draws in Climax I and II as reported by M79; the new results suggested that cloud seeding had, indeed, increased snowfall when the 500 hPa temperatures were $\geq$-20° C though by not nearly as much as had been indicated in the earlier studies (e.g., Mielke et al. 1981). In spite of the M79 statement that the upper level temperatures could not have indexed cloud top temperatures in the Rockies, Mielke et al. 1981 nevertheless renewed that claim; the 500 hPa temperature stratifications in their new analyses were linked to cloud tops (and, presumably, ice particle concentrations). To date, no evidence has been presented in support of these renewed claims.

Additional problems with the Climax experiments soon surfaced, however; these ranged from the experimenters having used a different observational day for the control station precipitation than they had previously used (Rhea 1983), to the discovery that publically-available published precipitation data for the key, independently-maintained gauge at Climax did not match that used by the experimenters (Rangno and Hobbs 1987, 1995a, hereafter, RH87, RH95a).

Rhea (1983) reported that when the precipitation data at the control stations were synchronized with the target, the seeding effect in Climax II diminished to statistical non-significance; it had not replicated Climax I after all.

A critical flaw discovered in Climax I was that the increases in snowfall due to seeding at Climax at the cooperative "independent" gauge disappeared *after* the date (halfway through Climax I) that the experimenters had selected their subset of control stations (Rangno and Hobbs 1993, hereafter RH93, Figure 1). There was no further indication of a seeding affect at that central gauge after that date throughout the rest of the Climax I and II experiments (RH93). This phenomenon suggests data dredging to find an effect that did not actually exist as subsequent data proved.

Seeding logistical problems, and as yet, inexplicable interruptions in the flow of random draws affected the outcome of the Climax experiments were also exposed in RH93.

Mielke (1995) has addressed some of these questions and reiterated his belief that the increases in snow purported by the experimenters over many years in their publications were, in fact, real.

The impact of the published results of the Climax and WCP experiments--before the many problems discussed above were beginning to be reported in the late 1970s and 1980s--was profound. They not only appeared to have established beyond a doubt in the most skeptical scientific minds that cloud seeding really worked in mountainous regions (e.g., NAS73, Mason 1980; 1982), there was also the practical impact of having those flawed results lead to an ambitious, well-planned, and extremely costly attempt at an independent replication of the Colorado experimenters' results in a new sophisticated randomized experiment, the Colorado River Basin Pilot Project (e. g., Braham 1979).

Not surprisingly, during the Colorado River Basin Pilot Project (CRBPP), the attempt to replicate the results at Climax and Wolf Creek Pass, met with numerous operational problems during its five-year lifetime (Elliott et al. 1973, 1978, Elliott 1979, 1986, Braham 1979, R79, Hobbs 1980, Rangno and Hobbs 1980a). These operational problems mainly arose due to discrepancies in the original experimenters' assumptions about clouds and where their tops were located. Ultimately, the CRBPP failed to replicate the results of the Climax and WCP when the same methods used by the experimenters to stratify seeding effects were also used (R79; see also Elliott et al. 1978; Rangno and Hobbs 1980a for wider discussion of the CRBPP results).

b. *The experiments in Israel; erosion of the original cloud reports*

A similar erosion of confidence in the results of the Israeli experiments has also occurred over the past 30 years. The Israeli experiments were not just apparent statistical successes standing in isolation. The statistical results were buttressed by seemingly solid cloud microstructure reports. Figure 3 (dots) is a plot of cloud ice data given in support of the statistically successful cloud seeding experiments. These data led scientists worldwide to believe for many years that the wintertime cumuliform clouds in the eastern Mediterranean were unusually ripe with seeding potential (e.g., Kerr 1982, Mossop 1985, Silverman 1986, Dennis 1989).

The great seeding potential seen in these data was because the clouds appeared to be able to form only a relatively few ice crystals per liter on average even when cloud tops were as cold as -21° C. This meant that the introduction of a seeding agent was required to form ice crystals for an effective release of precipitation from these clouds, thought to require concentrations of a few tens per liter for an effective release of precipitation (e.g., Mason 1971, Dennis 1980).

The "Xs" in Figure 3, however, represent later airborne measurements collected in the eastern Mediterranean with modern probes that show quite a different picture than could be deduced by the original experimenters. Ice crystal concentrations of tens to hundreds per liter were encountered by Levin 1992, 1994, and Levin et al. (1996) near cloud tops with temperatures of about -6° to about -13° C. According to the original cloud reports, this was a cloud top temperature range in which very few if any ice crystals were supposed to occur. Perhaps the most notable aspect of this finding was that Levin et al. gathered these surprising results on only four days of sampling on six flights. Levin's reports were preceded by an analysis of Israeli rawin data that indicated that rain fell from clouds with tops warmer than -10° C, and that warm rain formed in them (Rangno 1988). These findings have been supported in satellite data (Ramanathan et al. 2001).

Finally, Freud et al. 2015 have confirmed these reports by reporting that the natural precipitating efficiency of Israeli clouds moving in from the Mediterranean was so high that by the time their tops reached just -3 C°, they were already precipitating and unsuitable for seeding with silver iodide. This is about at the 700 mb level during most storms.

Due to these new ice-forming results, the clouds of Israel no longer stand out from similar clouds as noticed by Rangno and Hobbs (1988), updated in RH95. There is no longer debate about the unsuitability of Israeli clouds as targets for cloud seeding.

Thus, the "mirror image" cloud microstructure reports that matched those in the Colorado Rockies, and also appeared to explain *why* seeding had worked in Israel, were faulty. Neither the clouds in Colorado nor those in Israel are virtually ice-free until their tops are colder than -20° C. And, ice multiplication is now known to be active in both locations (*loc. cit.*).

Researchers have attributed the ice-forming efficiency of Israeli clouds to various causes; dust particles (Rosenfeld and Farbstein 1992), dust particles coated with sulfates (Levin et al 1996, 2005) and due to large cloud droplets resulting from Mediterranean Sea spray (Freud et al. 2015).

Moreover, no one has yet documented the cloud that is responsible for producing virtually the entire statistical significance in precipitation in both the Colorado and Israeli projects; the deep cloud that does not naturally precipitate until seeded, and then when seeded, precipitates at the same rate as natural precipitation. Only the routine presence of such non-precipitating natural clouds could have provided the "extra duration" due to seeding responsible in *both* experiments for the statistical significance (e.g., Chappell et al 1971; G86; Gagin and Gabriel 1987).

Lastly, RH95b, 1997b) concluded that the stratifications of seeding effects by cloud top temperatures in Israeli 2 are unreliable due to inadequacies of the 3-cm wavelength radar used by the experimenters for this task. This conclusion was reached in large part by the radar's distance from the North target area for which the experimenters reported "modal" cloud tops.

b. Erosion of the statistical reports for Israeli 2.

The new cloud assessments in the late 1980s and 1990s, were accompanied by new statistical reports for Israeli 2. The full analysis of Israeli 2 using the results of random seeding on both targets, revealed that there had been a null result of seeding; it had not replicated Israeli 1 after all as had been believed for many years. Rainfall had been unusually heavy in *both* target areas on the days when the North target area was seeded (Gabriel and Rosenfeld 1990). When the results of the two target areas were combined what appeared to be rainfall increases in the North target area on seeded days were canceled out by apparent decreases in rainfall due to seeding in the South target area. Gabriel and Rosenfeld (1990) found that the average rainfall in the South target area on control days (which are the same days as when the North target area was seeded) was 30-40% above historical daily average by "several standard errors" and was "statistically significant" a remarkable finding[12].

RH95b showed that the same effects described above (heavy rain on North target area seeded days) extended as far north as Beirut, Lebanon, and throughout western and central Jordan downwind of the South target area. When the South target's rainfall *was* used as a control for the seeded target, the seeding effect in the North target area was reduced to -3%, nearly the same result as had been reported for the experiment by Gabriel and Rosenfeld (1990) using a different set of rain gauges.

Ironically, using the pre-planned crossover design described by Gagin and Neumann (1974) produced the worst result (-3%) of the several precipitation comparisons made by RH95b, 1997a. RH95b attributed the Israeli I and II results to Type I statistical errors, some of which were obscured in the reports preceding Gabriel and Rosenfeld (1990) full analysis because the experimenters' used different evaluation techniques in each of the two sets of experiments.

Rosenfeld (1997) has suggested that the seeding effects on rainfall, using the buffer zone precipitation as a control, show a consistent positive seeding result in the two Israeli experiments that is confined to the North target area, with non-statistically significant decreases in rainfall suggested in the Center and South target areas. For a more complete discussion of these interesting experiments and differing interpretations of the Israeli experiments, see Rosenfeld (1997), Dennis and Orville (1997), Woodley (1997), Ben-Zvi (1997), Rangno and Hobbs (1997a, 1997b, 1997c, 1997d, 1997e).

c. Israeli 3

The final results of a third randomized experiment, Israeli 3, were reported by Rosenfeld (1998). This experiment began in 1975 and was confined to the South target area of Israeli II.

[12] An inspection of the random sequence for Israeli 2 showed that it is very different from that used in Israeli 1. In Israeli 1, the same decision occurred on the following day on 13% of the draws, but on 59% of the draws in Israeli 2, the next day had the same decision. Could this account for the lopsided draw?

The results decreases in rainfall of 5-10% on seeded days after 19 winter seasons and nearly 1000 random decisions (Rosenfeld 1998). The null result in Israeli 3 appears to support the lack of seeding effects in Israeli 1 and 2 deduced by RH95b), but also supports the interpretation by Rosenfeld (1997) of a lack of decreases in rain due to seeding in the southern targets of all three experiments over almost 30 years.

Additional re-analyses of seeding on rainfall in Israeli 2 were reported by Levin et al 2010 who concluded that the appearance of increases in rainfall in Israeli 2 was due to synoptic factors and not due to seeding. This finding was criticized by Ben-Zvi et al (2011) with a Reply by Levin et al 2011.

In sum, not only were the statistical results of the Colorado and Israeli experiments undermined by similar flaws and omissions; so too were the experimenters' cloud reports and stratifications by cloud top temperatures flawed in similar ways. In both cases, the experimenters were unable to correctly assess their clouds; they reported far too low natural ice particle concentrations in both locales, but ones that the supported the seeding effects they were reporting.

8. Why Did Peer Review Fail?

How did all of this happen? How could so many reports fraught with faulty conclusions based on inadequate evidence slip into the published literature and gain widespread acceptance as solid, unambiguous cloud seeding results when evaluated by our highest professional organizations, panels, and individual scientists? What went wrong from the beginning that could have been, and perhaps should have been, caught in the peer review process?

Why *do* reviews of manuscripts sometimes fail? And why don't the mechanisms of journal post-publication criticisms, or author self-correction, seem to work?

Many answers to these questions are obvious to those who do reviews, or have had manuscripts reviewed, but what factors were responsible can't be known for sure until the reviewers of the faulty journal articles discuss what happened.

Faults in the original analyses that were missed by reviewers included:

a) the control or target stations for the cloud seeding experiment were not selected before the experiment began. Instead, the optimistic statistical result was due to the use of a subset of the available control stations were selected after or mid-way through the experiment;

b) the choice of controls, among many that could have been chosen, profoundly affected whether the experiment appeared successful;

c) the experimenters did not carry out regional analyses that would have shown that the same effect which they attributed to cloud seeding in the target area had occurred over a wide region which could not been seeded;

d) the seeding potential of the clouds was over-estimated by the experimenters because they found lower concentrations of ice crystals in clouds than actually exist;

e) the experimenters reported relationships between cloud top temperatures and ice crystal concentrations that do not exist;

f) the efficiency of seeding methods was over-estimated;

g) ersatz data that enhanced the statistical results of an experiment were used;

h) portions of experiments that cast doubt on a cloud seeding success were omitted from published analyses, thus making the experiment appear more successful than it really was;

i) results of follow-up experiments which did not replicate the results of previous, "successful" experiments were not reported.

But *why* didn't reviewers of these many papers catch these many faults? And why didn't those who knew there were problems in some experiments (such as the author) comment on published papers having ersatz data or physical arguments? The following list of likely factors will not surprise anyone, but they do, as we shall see later, represent continuing obstacles that must be overcome in the review process:

j) reviewers are too busy to do a proper job;

k) reviewers of papers and proposals are not skeptical enough about some of the claims contained in papers because they are, perhaps, naive about human nature and the temptation to improve the outcome of cloud seeding experiments (any paper?) due to self-deception or other reasons;

l) reviewers have their own agendas and allow weakly supported science to get published that favors their viewpoints;

m) the journal editor has a viewpoint and distributes submitted papers on cloud seeding to those whose reviews are likely to agree with his own viewpoint;

n) the selection of reviewers by journal editors is often too narrow in expertise for the breadth of territory covered by a paper on cloud seeding (i. e., statistics, cloud microstructure, dispersion, synoptic meteorology);

o) some scientists believe that post-publication peer review criticism of papers is, per se, not a useful scientific activity and detracts from other, funded work even when they are skeptical of published results. Hence, they ignore or do not cite work they are skeptical of;

p) open criticism of a colleague may not occur because a potential critic may feel that his/her chances of receiving grants or having papers published might diminish if the colleague is likely to review his/her papers or proposals;

q) the most knowledgeable critics of published papers are probably those *within* the same institution from which faulty research emanates and are not likely to comment on questionable work because of an unwritten "it's in the family" code of conduct;

r) the most knowledgeable critics within a cloud seeding establishment may be under financial duress if they comment critically on their own organization's work;

s) knowledgeable critics within the same institution are, *ipso facto*, unlikely to be anonymous reviewers of work emanating from the same institution;

t) the randomization of the experiments themselves, in the absence of experience about how perniciously uneven random draws could be even over periods of years (e.g., Israeli 2 and 3, perhaps led to a misplaced assurance of no storm (or experimenter) bias.

Perhaps, given this list, we should be surprised if *any* valid results are published!

And, we can be sure, and can commiserate that these are not problems that have solely afflicted the domain of cloud seeding (e.g., Feder and Stewart 1987, Foster and Huber 1997.)

On the other hand, reviewers should not have to be "gumshoes" (private investigators) looking for the omitted data or other mischief. An implicit trust exists between authors of manuscripts and reviewers which is when the authors of a manuscript state that they did something, the reviewer should be able to assume that they did it, and that the seeding effect the experimenters are reporting was an isolated anomaly in the target. And that they have cited all of the pertinent literature for the reader as a background.

For example, when experimenters report that they have examined many precipitation gauges or watershed runoffs for use as covariates before selecting the ones that they did to test a seeding effect (as did Morel-Seytoux and Saheli 1973, Hastay and Gladwell 1969, Mielke et al. 1970, etc.), it is assumed, as a trust issue by reviewers, that any problems or contrary evidence to a "successful experiment" that may have turned up in the search will be reported.

But, as the experiments examined above show, this apparently did not happen. Had reviewers insisted that Mielke et al. (1970) display the results of the seed/no seed precipitation ratios for "all western Colorado gauges" in Climax I (which the experimenters stated they were already evaluating at that time), it would have helped them confront a Type I statistical error (or "good draw"), one that they were unable to detect until ten years later (M79). The demand for statewide seed/no seed ratios in the Climax experiments was apparently never made by any of the reviewers of the several papers on those experiments. Perhaps it was believed by reviewers that the randomization of experiment itself, conducted over five years in both Climax I and II, would take care of uneven draws--why expect them?

The same can be said about the value of regional plots for the Wolf Creek Pass experiment (Morel-Seytoux and Saheli 1973), the Skagit Project (Hastay and Gladwell 1969), and in the Israeli experiments (e.g., GN81). Had the authors been required to display their statistical results on a regional-scale (against the controls they chose for measuring seeding effects), they would have been forced to confront evidence of uneven random draws that favored seeded days, and, at least, would have had to explain them.

Another indication of a problem, perhaps obvious only in retrospect, was that in both the Colorado and Israeli experiments the seeding effect was confined to a precipitation duration effect, a report, however, that was compatible with the kind of seeding carried out. However. the seeding of natural non-precipitating clouds was so efficient that it made them precipitate at the same rate as natural clouds, a red flag, a highly unlikely outcome. The alternative to this inference of causing non-precipitating clouds to precipitate was that the experimenters were dealing with a natural bias that had produced the misperception of extended "duration effects" on seeded days.

Moreover, day-to-day weather forecasters in the Rockies with the National Weather Service who plotted rawinsondes by hand in the days of the Climax experiments are not likely to have accepted the claim by the Colorado experimenters of a close correspondence between cloud top and 500 hPa temperatures (e.g., Grant and Mielke 1967, Mielke et al. 1981). Indeed, some cloud seeding workers in the Rockies (Rhea et al. 1969, Rangno 1972, Elliott et al. 1973), were already reporting that there were problems with this assumption. However, none of these latter findings were widely distributed, nor were they submitted for publication in peer-reviewed journals. Rather, they remained husbanded in "project reports" within the agency that was largely funding these experiments.[13] The scientific personnel within the agency also did not act to publish or make known these findings.

Weather forecasters in the Israel Meteorological Service (IMS) were also aware that significant rain fell from clouds with tops equal to or warmer than -10° C (tops that are generally between 3.5 and 4.5 km above sea level). Such knowledge by the IMS forecasters ran counter to the claims contained in cloud microstructure reports that were appearing in foreign journals purporting that the clouds of Israel were very inefficient producers of rainfall (viz., could not form any ice crystals until the tops became colder than -14° C, and not many until the tops were colder than -21° C (e.g., G75).

One might assume, reasonably, that improving systems of measurements would have had an effect; if these researchers had only had modern instrumentation these faulty reports could never have appeared. Strangely, and perhaps pointing to subjective influence, this is not true. For example, the first sign that something was seriously amiss with the cloud microstructure reports in Israel were deduced by an analysis of conventional, and widely available rawinsonde data (Rangno 1988).

Moreover, the HUJ experimenters themselves had, for two consecutive rainy seasons (1976-1977 and 1977-1978), measured the tops of clouds with no less than two radars, one a 5-cm scanning radar located at Ben Gurion Airport, and a 3-cm vertically-pointed radar located near their offices at the HUJ. They also used an instrumented aircraft to verify cloud top heights over the vertically-pointed radar (Gagin 1980). METEOSAT thermal imagery, as well as rawinsonde data from which to deduce cloud tops heights and temperatures was also available.

And yet, despite these many tools, they were still unable to discern, or more accurately, report, that their cloud reports were in substantial error.

Recall, too, that in the Colorado Rockies, the experimenters also had a vertically-pointed 3-cm radar and they, too, were unable to see the fallacy of their claims that 500 hPa and cloud tops were well-correlated ("outed" by Hobbs and Rangno 1979; RH93.)

[13] The now defunct Atmospheric Water Resources Management Division of the Bureau of Reclamation.

These two experiences in Colorado and Israel strongly suggest that there is a role for day-to-day weather forecasters in the review of manuscripts on cloud seeding and the cloud properties and storm types that might befuddle cloud seeding experiments within their forecasting domain. It is noteworthy in this discussion that the problem of "storm types" and their ability to compromise cloud seeding experiments was brought to the attention of those evaluating a cloud seeding experiment by a weather forecaster (E. M. Vernon) with the U. S. Weather Bureau in San Francisco (Neyman et al. 1960).

It has been suggested, too, that if the reports of the true ambiguity of many of the experiments in Table 1 had been reported initially, would there have been a more rapid advance in cloud seeding experimentation because so many questions would have been raised immediately and likely investigated.

9. Some Remedies

Several recurring themes in the "pathology" of faulty published results suggest a few remedies for improving cloud seeding manuscripts. Many of these have been suggested in the past (e.g., Court 1960, Neyman and Scott 1967, Dennis 1980), but are worth recalling here.

a. *Improving the reliability of published cloud seeding research*

A panel of experts representing several disciplines should be given the responsibility for assessing the quality of manuscripts submitted on cloud seeding. This is because of the strong subjective influences that appear to creep into the evaluation of cloud seeding experiments by those who conduct them or have vested interests. Manuscripts on cloud seeding, due to the great breadth of territory covered and the questions they raise, such as:

Was it likely that the seeding agent was transported to the proper locations and in the right concentrations at a reasonable point upwind of the target area?

Was the statistical conduct of the experiment proper? Were the clouds likely to have responded favorably to artificial increases in concentrations of ice crystals?

Are the cloud reports representative of the region?

Could differing storm-types on seeded or control days have affected the experiment? A review panel to answer these questions might consist of:

1. two independent statisticians, neither associated with the institution carrying out the cloud seeding experiment.
2. one or more experts in airborne cloud microstructure measurements,
3. one or more experts in diffusion,
4. one or more weather forecasters or synoptic meteorologists with expertise in the region in question,
5. an anonymous reviewer from within the department or institution from which the cloud seeding report emerges.

The formation of a panel to evaluate manuscripts on cloud seeding experiments may seem like a drastic measure. However, efforts suggested by this recommendation must be weighed against the cost of the faulty or partially-reported results that have been published in our journals thus far that misled us.

b. *Improving the robustness of cloud seeding experiments reported in journals.*

Mandatory requirements should include:

1. Reporting the results of experiments using all experimental units. Subsets of days/units, and why should follow, not precede the full analysis (often not presented).

2. Regional maps of the test statistic used to evaluate the effectiveness of seeding in the target will be shown for all available stations.
3. An experimental unit chosen to maximize the amount of independent data that can be used to evaluate the results of seeding in an experiment. For example, if a 24 h experimental day is used in the U. S., it should end at 0700 or 0900 LST, the times at which the maximum number of NOAA cooperative gauges are read for 24 h totals.
4. Control precipitation *stations* or other covariates against which the effect of seeding will be tested must be *publicly* identified *before* an experiment begins.
5. The random decisions of the experiment should be placed in a public repository at the time they are made.
6. Daily records of the hours of aircraft and/or ground seeding operations, rate of seeding, and the percent of the clouds/precipitation that was actually seeded will be made available for public inspection at the end of each experimental unit. Preferably these data would be placed on-line in a near real-time basis.
7. Where radar is installed to evaluate seeding effects, it should be operated by, and the analyses of the radar data performed by groups that are independent of the experiment and have no knowledge of the random seeding decisions in real time.
8. All precipitation and radar data will be placed in a public archive as the experiment progresses. Preferably these would be available on-line as close to real time as practicable.
9. Where special networks of precipitation gauges are installed for the purpose of analyzing cloud seeding experiments, the gauge readings must be made by an independent organization that is not aware of whether an experimental period has been seeded or not (as in the CRBPP).
10. Precipitation gauges, measurements, and hydrological data must be tamper-proof.

11. The National Weather Service forecast for the time closest to experimental units must also be archived.
12. Submitted papers that profess to find a seeding effect (or lack of one) based on *post facto* selected controls should not be considered for publication *unless* it is made clear that it is the result of exploratory analyses and confidence in any result presented is degraded and should be used with caution.
13. Omitting results from cloud seeding experiments for more than five years following completion of an experiment will be considered misconduct.
14. Those who design, conduct, or promote commercial cloud seeding should *never* evaluate cloud seeding experiments. This must be left to independent groups such as university statistical departments.
15. High resolution numerical models (e.g., Morrison et al. 2015) should be used to produce estimates of natural precipitation on control and seeded days.

c. *The authors of cloud seeding studies should disclose their vested interests in the outcomes of cloud seeding experiments and key personnel should attest to the validity of the result being reported.*

Following the lead of several leading medical journals, American Meteorological Society and other journals should also require a "disclosure" statement signed by the author(s) that is either privately addressed to the journal editor (to be used at his discretion), or appears at the conclusion of each article on cloud seeding. Such a disclosure statement should include the following information:

1. Authors must divulge whether their employment is dependent upon the "sign" of the cloud seeding results presented.
2. Authors and their associates (e.g., radar technicians/ meteorologists/forecasters

who monitor cloud systems, pilots performing seeding missions, etc.) must also be signatories on statements accompanying submitted manuscripts indicating that the conditions and results described in the paper are true to the best of their knowledge.

3. Their *a priori* convictions about cloud seeding.

We must also encourage workers who know of discrepancies in the descriptions of cloud seeding experiments to report them to the scientific community. The author regrets not having done so during the CRBPP in the early 1970s when discrepancies were being documented concerning the Climax and Wolf Creek Pass cloud top height hypotheses.

10. Has Peer Review improved? A Brief Examination of Recent Cloud Seeding Literature

The foregoing analyses have demonstrated that peer review was inadequate on numerous occasions in the cloud seeding literature in past decades. But these stories are old hat. Have we learned from these painful, costly lessons of inadequate peer review since the Colorado and Israeli experiences and have we “closed the gaps” to faulty literature? Many of the suggestions in this article have been put forward since the era of modern cloud seeding began. But have they been implemented to root out bias?

In this section, we now examine recent publications for signs of increased peer-review robustness in the renewed cloud seeding activity centered around a massive, $9 million dollar randomized experiment in Wyoming, one resembling in scope and planning, the Colorado River Basin Pilot Project of the early 1970s. The latter was undertaken in to replicate the apparent large (but in reality, non-existent) increases in snowfall that were being reported in the Climax I, II and Wolf Creek Pass randomized experiments.

In this review, we keep in mind that organizations that are vested in weather modification, such as the now defunct Atmospheric Water Resources Management/Research division of the Bureau of Reclamation, some universities with persistent cloud seeding programs and research, segments of NCAR, and nations with tens of thousands of workers dependent on funding of cloud seeding programs, are surely ripe for producing slanted, unreliable results concerning cloud seeding research due to the inherent pressures of having to prove a viable cloud seeding effect to maintain funding. This is probably one of the more obvious concerns by this author going into this review, as it should be for all of us outside the cloud seeding culture.

Think of the faulty research that emanated from powerhouse research universities here in the US (Colorado State University) and in Israel (Hebrew University of Jerusalem) as examples of how vested interests (jobs and funding) and likely *a priori* beliefs, created an environment for corrupted research, an issue recently addressed in an editorial about the causes of fraud in science (Nature 2008).

Within such environments in weather modification, only reports finding that seeding did not increase precipitation are virtually certain to be reliable. Those reports, including field experiments, case studies, model simulations, statistical analyses that conclude increases in precipitation from institutions under “seeding funding duress” must necessarily be given extra attention. They might be valid and thorough in every way, but they must be reviewed with extra vigor as will be demonstrated.

The National Center for Atmospheric Research and cloud seeding

The NCAR Research Applications Laboratory (RAL) group has been involved with seeding assessment programs for many years and has produced extremely thorough Final Reports, such as NCAR RAL (2005) for rainfall assessment program in the United Arab Emirates. No stone was left unturned and the report included all the warts that happen in field programs. There are no indications of bias, and all relevant literature is cited.

Presently NCAR (using a different set of researchers), has become heavily involved with cloud seeding research in Wyoming. And due to that involvement, worth millions, has already made a basic misstep that will undermine the credibility of any reported “success” from this otherwise well-planned program of research.

The compromising misstep?

Having the same organization that planned a $9 million dollar experiment (NCAR), evaluate its results, NCAR (Breed et al. 2014). For all of its faults, even the Bureau of Reclamation’s Division of Atmospheric Water Resources Management knew better when it planned the massive Colorado River Basin Pilot Project than to have its own scientists evaluate its results!

But how can we tell if slanted reporting of seeding effects are occurring, or are likely to occur from NCAR in the future? Answer: evaluate the early literature already emanating from NCAR. We focus on two examples.

Surprisingly, in Breed et al. (2014) of NCAR, the signs of bias are rife. Here are examples of citations that prove that unreliable reporting still lives in the cloud seeding domain, and, specifically, in some quarters of NCAR:

1) The former illustrious Climax, CO, randomized experiments, whose rise and fall cycle are discussed at length in the present paper, are cited in Breed et al. 2014: Mielke et al (1981), Mielke et al (1982), and Grant (1986).

There are no further citations of the many concerning the compromising flaws in those experiments (a null result). They no longer have credibility outside of NCAR.

In reading the first two citations by Breed et al., the reader will be led to reports of statistically-significant cloud seeding increases in snowfall. Today those results are known to be bogus (e.g., RH95a) as well as the underlying microphysical foundations of those experiments (e.g., M79, among many others that could be cited). Grant (1986) was not candid about those missteps, whereas M79 was.

Citations that only refer to the “happy” results reported by the Colorado experimenters, without filling in the whole, sad, costly story, is tantamount to citing Fleischmann and Pons

(1989) as having provided evidence of “cold fusion” without citing the follow up research that proved it was a bogus.

In slanted publications, a second aspect is that references documenting the major faults in the Colorado experiments will, of course, *not* be cited, as is observed in Breed et al. 2014 to maintain a one-sided view of those experiments.

A slightly paraphrased version of the FTC Statement on consumer fraud is worth recalling in the context of Breed et al.’s limited citations and for other researchers who practice one-sided citations:

“Certain elements undergird all deception cases. First, there must be a representation, omission or practice that is likely to mislead the (journal reader).” In Breed et al. 2014 the reader is clearly misled about prior seeding work in the Rockies.

However, because Breed et al. 2014 describe the single area Climax experiments as “crossover” experiments, it indicates that neither the authors, nor the reviewers of their manuscript were familiar with the topic they were addressing. Could it be that they were also not aware of the many faults uncovered in the Climax experiments by HR79, Rhea (1983), RH87, RH93, and RH95a, that included suggestions of data tampering?

2) Breed et al. 2014 also do not address the large number of ice multiplication findings that have been reported in the Rockies (e.g., Auer et al 1969; Vardiman 1978, Marwitz and Cooper 1980, Cooper and Vali 1981), nor that cloud tops and cloud ice concentrations have been found to be uncorrelated (e.g., Vardiman and Hartzell 1976, DeMott et al 1982).

Ice multiplication is generally considered a scourge to increasing precipitation via static cloud seeding (e.g., Dennis 1980) and a relationship between cloud top temperatures and ice particle concentrations, has been a mantra of seeding partisans in defining seeding “windows” (e.g., Grant 1968, 1986; Grant and Elliott 1974).

These aspects of clouds in the Rockies should have been addressed in Breed et al. 2014 and not ignored. Since this information presents complications to seeding, ones that no model has satisfactorily solved, it can be presumed that this is the reason these topics are not discussed since funding might be compromised.

In the Israel literature, Freud et al. (2015) can also be viewed has having similar faults such as one-sided citing and omitting relevant literature in the way that Breed et al. 2014 did.

Ironically, and amazingly so, the reporting of the Colorado and Israeli cloud seeding literature continue to mirror one another long after the glory days of ersatz "successes."

11. Conclusions

This review has demonstrated that randomization of a cloud seeding experiment *per se* does not appear to compensate for experimenter bias or other non-scientific factors that appear to operate in the realm of cloud seeding experiments. It has been demonstrated that the same costly problems of inadequate, or friendly peer reviews still persist in this literature today.

Perhaps it is not surprising that it can't be eradicated so easily. Donald Kennedy (2003) in a *Science* editorial concerning proved cases of fraud in physics and the biological sciences, informed readers that the main driver of fraud was "career enhancement."

Unless we believe that atmospheric researchers in cloud seeding are somehow superior to medical or physics researchers, the force to improve or maintain our positions in life will drive some to be less forthcoming or worse concerning their cloud seeding research. We must be vigilante and implement as many safeguards as we can.

As scientists, it appears to this author that we are more emotionally involved in the outcome of a randomized cloud seeding experiment than we are about the outcome of our other research activities, such as measuring the size of the effective radius in Stratocumulus clouds in the Atlantic Ocean. On the other hand, we seem to care an awful lot about whether a seeding has increased precipitation in our own experiments (e. g., Table 1).

Like a Hollywood movie set, which exudes glamour and authenticity when viewed from the front--an empty shell no doubt lies behind other, non-independently scrutinized reports of cloud seeding successes, some of which may still be relied upon by our most distinguished scientists and panels in their assessments of cloud seeding. The author believes that Neyman's (1980) call for a careful, comprehensive *independent* review of the cloud seeding literature on which our present AMS and World Meteorological Organization official assessments rely was a reasonable, essential one.

Furthermore, it is suggested that we could learn so much more from long term commercial seeding operators if they would only randomize their efforts, with independent (university) evaluations. Mandating in federal law that all commercial projects randomize their seeding operations should be considered. We owe it to the public to carry out randomized experiments instead of purely operational ones and to evaluate them in a robust, scientific way to so that the public, the operators and funders can learn what they have been doing all those years of seeding.

Israel paid a dear price for operational seeding (Sharon et al. 2008), and the Bureau of Reclamation one, too, on an experiment to replicate non-existent results. Both of these painful chapters were both to faulty research published in our journals by those who had the most to gain. Surely, more flawed operational seeding projects will be "outed" if randomization was mandated, a good thing.

Acknowledgments. This paper is dedicated to Jerzy Neyman because his presence on the cloud seeding scene as a leading statistician from the 1950s to 1980 and his intense scrutiny of the cloud seeding literature no doubt improved articles on cloud seeding even before they were submitted and he helped to elucidate many points once they were. Surely, he would have made a difference had he lived to see the initial, partial results being reported for Israeli 2.

While this article represents solely the viewpoint of the author, he nevertheless owes a considerable debt to Peter V. Hobbs for his steadfast support and advice in these matters over three decades.

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REFERENCES

American Meteorological Society, 1953: Statement on weather modification. *Bull. Amer. Meteor. Soc.*, **34**, 218-219.

__________________________, 1984: Statement on planned and inadvertent weather modification. *Bull. Amer. Meteor. Soc.*, **66**, 447–448.

__________________________, 1998: Statement on planned and inadvertent weather modification. *Bull. Amer. Meteor. Soc.*, **79**, 2771–2772.

__________________________, 2011: Statement on planned weather modification. *Bull. Amer. Meteor. Soc.*.,.

Auer, A. H., D. L. Veal, and J. D. Marwitz, 1969: Observations of ice crystals and ice nuclei observations in stable cap clouds. *J. Atmos. Sci.*, **26**, 1342-1343.

Ben-Zvi, A., 1997: Comments on "A new look at the Israeli randomized cloud seeding experiments." *J. Appl. Meteor.*, **36**, 255-256.

Bradley, R. A., S. S. Srivastava, and A. Lanzdorf , 1978: An examination of the effects of cloud seeding in Phase I of the Santa Barbara convective band seeding test program. Florida State University, Dept. of Statistics, 67pp.

____, 1979: Some approaches to statistical analysis of a weather modification experiment. *Commun. Statist.-Theor. Meth.,* A(8), **11**, 1049-1081.

Braham, R. R., Jr., 1979: Field experimentation in weather modification, *J. Amer. Statist. Assoc.,* **74,** 57-68.

________________, 1986: Rainfall enhancement--a scientific challenge. *Rainfall*

Enhancement--A Scientific Challenge, Meteor. Monogr., **21**, No. 43, 1–5.

Breed, D., R. Rasmussen, C. Weeks, B. Boe., T. Deshler, 2014: Evaluating winter orographic cloud seeding: design of the Wyoming weather modification pilot project (WWMPP). *J. Appl. Meteor. Climate*, **53**, 282-299.

Brier, G. W., and I. Enger, 1952: An analysis of the results of the 1951 cloud seeding operations in central Arizona. *Bull. Amer. Meteor. Soc.*, **23**, 208-210.

_________, L. O. Grant, and P. W. Mielke, Jr., 1973: An evaluation of extended area effects from attempts to modify local clouds and cloud systems. *Proc., WMO/IAMAP Scien. Conf. on Weather Modification,* Tashkent, World Meteor. Org., 439-447.

Broad, W. J., and N. Wade, 1982: *Betrayers of the Truth*. Simon and Schuster, 256pp.

Brownlee, K. A., 1960: Statistical evaluation of cloud seeding operations. *J. Amer. Statist. Assoc.*, **55**, 446-453.

Bruintjes, R. E., T. L. Clark, and W. D. Hall, 1995: The dispersion of tracer materials in central Arizona: Comparison between observations and model results. *J.Appl. Meteor.*, **14**, 971-988.

Byers, H. R., 1965: *Elements of Cloud Physics*. University of Chicago Press, Chicago, 199pp.

__________., 1974: History of weather modification. *Weather and Climate Modification,* W. N. Hess, Ed., John Wiley and Sons, 3-44.

Changnon, S. A., and W. H. Lambright, 1990: Experimentation involving controversial and technological issues: weather modification as a case illustration. *Bull. Amer. Meteor. Soc.*, **71**, 334-344.

Chappell, C, A., L. O. Grant, and P. W. Mielke, Jr., 1971: Cloud seeding effects on precipitation intensity and duration of wintertime orographic clouds. *J. Appl. Meteor.*, **10**, 1006-1010.

Coons, R. D., E. L. Jones, and R. Gunn, 1949: Third Partial Report on Artificial Production of Precipitation Orographic Stratiform Clouds-California, 1949, *Bull. Amer. Meteor. Soc.*, **30**, 255-256.

________________________________, 1949: Artificial production of precipitation. *Third Partial Report: Orographic Stratiform Clouds--California, 1949. Fourth Partial Report: Cumuliform Clouds--Gulf States, 1949*. U. S. Weather Bureau Res. Paper No. 33, Government Printing Office, Washington, 46 pp.

_________, and R. Gunn, 1951: Relation of artificial cloud-modification to the production of precipitation. *Compendium of Meteorology*. Amer. Meteor. Soc., 235-241.

Cooper, W. A., and J. D. Marwitz, 1980: Winter storms over the San Juan mountains. Part III. Seeding potential. *J. Appl. Meteor.*, **19**, 942-949.

___________, and C. P. R. Saunders, 1980: Winter storms over the San Juan mountains. Part II. Microphysical processes. *J. Appl. Meteor.*, **19**, 927-941.

__________, and G. Vali, 1981: The origin of ice in mountain cap clouds. *J. Atmos. Sci.*, **38**, 1244-1259.

Cotton, W. R., 1986: Testing, implementation, and evolution of seeding concepts--a review. In *Precipitation Enhancement--A Scientific Challenge*, R. R. Braham, Jr., Ed., Meteor. Monographs, **43**, Amer. Meteor. Soc., 139-149.

__________, and R. A. Pielke, 1992: *Human Impacts on Weather and Climate*. ASteR Press, 271pp.

__________, and __________, 1995: *Human Impacts on Weather and Climate*, 1st edition, Cambridge University Press, 288pp.

__________, and __________, 2007: *Human Impacts on Weather and Climate*, 2nd edition, Cambridge University Press, 308pp.

Court, A., 1960: Evaluation of cloud seeding trials. *J. Irrig. and Drainage Div., Proc. Am. Soc. Civ. Eng.*, **86**, No. IR 1, 121-126.

Dawkins, S. M., and E. L. Scott, 1979: Comment, *J. Amer. Statist. Assoc.,* **74,** 70-77.

Decker, W. L., L-N. Chang, and G. F. Krause, 1971: An evaluation of the Whitetop cloud seeding experiment through a covariance analysis. *J. Appl. Meteor.*, **10**, 1193-1197.

DeMott, P. J., W. G. Finnegan, and L. O. Grant, 1982: A study of ice crystal formation in a stably stratified orographic cloud. *Preprints, Cloud Phys. Conf.*, Chicago, Amer. Meteor. Soc., 488-490.

Dennis, A. S., 1980: *Weather Modification by Cloud Seeding*. Academic Press, 267pp.

_________, 1989: Editorial to the A. Gagin Memorial Issue of the *J. Appl. Meteor.*, **28**, 1013.

__________, and H. D. Orville, 1997: Comments on "A new look at tbe Israeli cloud seeding experiments." *J. Appl. Meteor.*, **36**, 277-278

Elliott, R. D., 1974: Experience of the private sector. *Weather and Climate Modification,* W. N. Hess, Ed., John Wiley and Sons, 45-90.

__________, 1979: Comment on "Field experimentation in weather modification", *J. Amer. Statist. Assoc.* **74**, 77.

_________, 1986: Review of wintertime orographic cloud seeding. *Precipitation Enhancement--A Scientific Challenge*, R. R. Braham, Jr., Ed., Meteor. Monographs, **43**, Amer. Meteor. Soc., 87-103.

_________, R. W. Shaffer, A. Court, and J. F. Hannaford, 1973: Colorado River Basin Pilot Project Comprehensive Evaluation Report. *Interim Report to the Bureau of Reclamation,* Aerometric Research, Inc., Goleta, CA, 641pp.

__________, __________, ________, _______________, 1978: Randomized cloud seeding in the San Juan mountains, Colorado. *J. Climate Appl. Meteor.*, **17**, 1298-1318.

Feder, N., and W. W. Stewart, 1987: The integrity of the scientific literature. *Nature*, **325**, 207-214.

Fleagle, R. G., P. V. Hobbs, J. E. McDonald, D. G. Chapman, G. J. F. MacDonald, W. T. Edmonson, W. R. D. Sewell, J. A. Crutchfield, R. S. Hunt, 1969: *Weather Modification: Science and Public Policy*. University of Washington Press, Seattle, WA, 147pp.

Fleischmann, M., and S. Pons, 1989: Electrochemically induced nuclear fusion of deuterium. *Journal of Electroanalytical Chemistry,* **261**, 301-308.

Foster, K. R., and P. W. Huber, 1997: *Judging Science--Scientific Knowledge and the Federal Courts*. The MIT Press, Cambridge, MA, 333pp.

Gabriel, K. R., 1967a: The Israeli artificial rainfall stimulation experiment: statistical evaluation for the period 1961–1965. *Proc. Fifth Berkeley Symposium on Mathematical Statistics and Probability*, Vol. 5, L. M. Le Cam and J. Neyman, eds., University of California Press, 91–113.

_______, 1967b: Recent results of the Israeli artificial rainfall stimulation experiment. *J. Appl. Meteor.*, **6**, 437-438.

_______, and J. Baras, 1970: The Israeli rainmaking experiment, 1961–1967: final statistical tables and evaluation. *Tech. Rep., Dept. Meteor., Hebrew University of Jerusalem, Jerusalem*, 47 pp.

_______, and P. Feder, 1969: On the distribution of statistics suitable for evaluating rainfall stimulation experiments. *Technometrics*, **11**, 149-160.

_______, and D. Rosenfeld, 1990: The second Israeli rainfall stimulation experiment: analysis of rainfall on both target area. *J. Appl. Meteor.*, **29**, 1055–1067.

___________., J. Neumann, and A. Gagin, 1967: Cloud seeding and cloud physics in Israel: results and problems. *Proc. Intern. Conf. on Water for Peace*. Water for Peace, Vol. 2, 375-388.

Gagin, A., 1975: The ice phase in winter continental cumulus clouds. *J. Atmos. Sci.*, **32**, 1604–1614.

________, 1980: The relationship between the depth of cumuliform clouds and their raindrop characteristics. *J. Res. Atmos.*, **14**, 409-422.

________, 1981: The Israeli rainfall enhancement experiments. A physical overview. *J. Wea. Modif.*, **13**, 1–13.

________, 1986: Evaluation of "static" and "dynamic" seeding concepts through analyses of Israeli II experiment and FACE-2 experiments. In *Rainfall Enhancement--A Scientific Challenge*, Meteor. Monogr., **43**, Amer. Meteor. Soc., 63–70.

_______, and K. R. Gabriel, 1987: Analysis of recording rain gauge data for the Israeli II experiment. Part I: Effects of cloud seeding on the components of daily rainfall. *J. Climate Appl. Meteor.*, **26**, 913–926.

_______, and J. Neumann, 1974: Rain stimulation and cloud physics in Israel. *Weather and Climate Modification*, W. N. Hess, Ed., John Wiley and Sons, 454–494.

_______, and _________, 1976: The second Israeli cloud seeding experiment--the effect of seeding on varying cloud populations. *Proc. 2nd WMO Scientific Conf. on Weather Modification*, Boulder, World Meteor. Org., 195–204.

_______, and _________, 1981: The second Israeli randomized cloud seeding experiment: evaluation of results. *J. Appl. Meteor.*, **20**, 1301–1311.

Garstang, M., R. Bruintjes, R. Serafin, H. Oroville, B. Boe, W. R. Cotton, J, Warburton, 2005: Weather Modification; Finding common ground. *Bull. Amer. Meteor. Soc*. **86**, 647-655.

Gelhaus, J. W., A. S. Dennis, and M. R. Schock, 1974: Possibility of a Type I statistical error in analysis of a randomized cloud seeding project in South Dakota. *J. Appl. Meteor.*, **13**, 383-386.

Grant, L. O., 1968: The role of ice nuclei in the formation of precipitation. *Proc. Intern. Conf. Cloud Phys.*, Toronto, Amer. Meteor. Soc., 305-310.

__________, 1986: Hypotheses for the Climax wintertime orographic cloud seeding experiments. In *Precipitation Enhancement--A Scientific Challenge*, R. R. Braham, Jr., Ed., Meteor. Monographs, **43**, Amer. Meteor. Soc., 105-108.

__________, and R. D. Elliott, 1974: The cloud seeding temperature window. *J. Appl. Meteor.*, **13**, 355-363.

__________, and A. M. Kahan, 1974: Weather modification for augmenting orographic precipitation. *Weather and Climate Modification*, W. N. Hess, ed., John Wiley and Sons, 282-317.

__________, and P. W. Mielke, Jr., 1967: A randomized cloud seeding experiment at Climax, Colorado 1960-1965. *Proc. Fifth Berkeley Symposium on Mathematical Statistics and Probability, Vol. 5,* University of California Press, 115-131.

__________, J. O. Rhea, G. T. Meltesen, G. J. Mulvey, and P. W. Mielke, Jr., 1979: Continuing analysis of the Climax weather modification experiments. *Seventh Conf. On Planned and Inadvertent Weather Modification,* Banff, The Amer. Meteor. Soc., J43-J45.

Hastay, M., and J. Gladwell, 1969: Statistical evaluations of a cloud-seeding program at the streamflow control level. *J. Hydrol.*, **9**, 117-135.

Hill, G. E., 1980: The dispersion of airborne-released silver iodide into orographic clouds. *J.Appl. Meteor.*, **19**, 978-985.

Hobbs, P. V., 1969: Ice multiplication in clouds. *J. Atmos. Sci.*, **26**, 315-318.

___________, 1980: Lessons to be learned from the reanalysis of several cloud seeding experiments. *Preprints, Intern. Cloud Physics Conf.,* Clermont-Ferrand, Amer. Meteor. Soc., 88-91.

___________, 1975: The nature of winter clouds and precipitation in the Cascade mountains and their modification by artificial seeding. Part III. Case studies of the effects of seeding. *J. Appl. Meteor.*, **14**, 819-858.

__________., 2001: Comments on "A critical assessment of glaciogenic seeding of convective clouds to enhance rainfall". *Bull. Amer. Meteor. Soc.*, **82**, 2845-2846.

___________, and A. L. Rangno, 1978: A reanalysis of the Skagit cloud seeding project. *J. Appl. Meteor.*, **17**, 1661–1666.

___________, and ___________, 1979: Comments on the Climax randomized cloud seeding experiments. *J. Appl. Meteor.*, **18**, 1233-1237.

___________, L. F. Radke, J. R. Fleming, and D. G. Atkinson, 1975: Airborne ice nucleus and cloud microstructure measurements in naturally and artificially seeded situations over the San Juan mountains in Colorado. *Research Report X*, Cloud Physics Group, Atmos. Sci. Dept., University of Washington, Seattle, 98195-1640.

Kerr, R. A., 1982: Cloud seeding: one success in 35 years. *Science*, **217**, 519–522.

Kennedy, D., 2003: Research Fraud and Public Policy. Science, **300**, 393.

Kessler, E., 1998: Comments on cloud seeding. *Bull. Amer. Meteor. Soc.,* **79**, 1406-1410.

Kraus, E. B., and P. Squires, 1947: Experiments on the stimulation of clouds to produce rain. *Nature*, **159**, 489.

Lawson, R. P., and T. L. Jensen, 1998: Improved microphysical measurements in mixed phase clouds. *Preprints, Conf. Cloud Phys.,* Everett, WA, Amer. Meteor. Soc. 451-454.

Levin, Z., E. Ganor, and V. Gladstein, 1996: The effects of desert particles coated with sulfate on rain formation in the eastern Mediterranean. *J. Appl. Meteor.*, **35**, 1511-1523.

_________., N. Halfon, and P. Alpert, 2010: Reassessment of rain enhancement experiments and operations in Israel including synoptic considerations. *J. Atmos. Res.*, **97**, 513-525.

_________., N. Halfon, and P. Alpert, 2011: Reply to comments by Ben-Zvi, A., D. Rosenfeld and A. Givati on the paper: Levin, Z., N. Halfon and P. Alpert, "Reassessment of rain experiments and operations in Israel including synoptic considerations." . *J. Atmos. Res.*, **98**, 593-596.

List, R., 2004: Weather modification—A scenario for the future. *Bull. Amer. Meteor. Soc.* **85**, 51-63.

Lovasich, J. L., J. Neyman, E. L. Scott, and J. A. Smith, 1969a: Wind directions aloft and effects of seeding on precipitation in the Whitetop experiment. *Proc. National Acad. Sci*., **64**, 810-817.

___________, _________, _________, _____________, 1969b: Statistical aspects of rain stimulation--problems and prospects. *Review of the Intern. Statist. Institute*, **38**, 155-170.

___________, _________, _________, _____________, 1971a: Further studies of the effects of cloud seeding in the Whitetop experiment. *Proc. of the National Acad. Sci.*, **68**, 147-151.

___________, _________, _________, _____________, 1971b: Hypothetical explanations of the negative effects of cloud seeding in the Whitetop experiment. *Proc. of the National Acad. Sci.*, **68**, 2643-2646.

MacCready, P., 1952: Results of cloud seeding in central Arizona, winter 1951. *Bull. Amer. Meteor. Soc.*, **23**, 48-52.

Mason, B. J., 1971: *The Physics of Clouds.* Oxford University Press, 671pp.

___________: A review of three long-term cloud-seeding experiments. *Meteor. Mag.*, **109**, 335-344.

___________, 1982: Personal Reflections on 35 Years of Cloud Seeding. *Contemp. Phys.*, **23**, 311-327.

Meltesen, G. T., J. O. Rhea, G. J. Mulvey, and L. O. Grant, 1978: Certain problems in post hoc analysis of samples from heterogeneous populations and skewed distributions. *Preprints*., *9th National Conf. on Wea. Mod.*, Amer. Meteor. Soc., 388-391.

Mielke, P. W., Jr., 1979: Comment on field experimentation in weather modification. *J. Amer. Statist. Assoc.*, **74**, 87-88.

_____________, 1995: Comments on the Climax I and II experiments including replies to Rangno and Hobbs. *J. Appl. Meteor.*, **34**, 1228-1232.

_____________, and J. G. Medina, 1983: A new covariate procedure for estimating treatment differences with application to Climax I and II experiments. *J. Climate Appl. Meteor., **22**,* 1290-1295.

_____________, K. Berry, J. G. Medina, 1982: Climax I and Climax II: distortion resistant residuals. *J. Climate Appl., **21**,* 788-792.

_____________, L. O. Grant, and C. F. Chappell, 1970: Elevation and spatial variation effects of wintertime orographic cloud seeding. *J. Appl. Meteor.*, **9**, 476-488. *Corrigenda*, **10**, 842, **15**, 801.

______________, L. O. Grant, and C. F. Chappell, 1971: An independent replication of the Climax wintertime orographic cloud seeding experiment. *J. Appl. Meteor.*, **10**, 1198-1212.

________, G. W. Brier, L. O. Grant, G. J. Mulvey, and P. N. Rosenweig, 1981: A statistical reanalysis of the replicated Climax I and II wintertime orographic cloud seeding experiments. *J. Appl. Meteor.*, **20**, 643-659.

Moran, J. P., and L. W. Morgan, 1986: *Meteorology. The Atmosphere and the Science of Weather,* Burgess Publishing, 502pp.

Morel-Seytoux, H. J., and F. Saheli, 1973: Test of runoff increase due to precipitation management for the Colorado River Basin Pilot Project. *J. Appl. Meteor.*, **12**, 322-337.

Morrison, H., G.. K. Bryan, K. Ikeda, S. A. Tessdendorf, , G. Thompson, 2015: Parameterization of cloud microphysics based on the prediction of bulk ice particle properties. Part II: case study comparisons with observations and other schemes: *J. Atmos. Sci.,* **72**, 312-339.

Mossop, S. C., 1985: The origin and concentrations of ice crystals in clouds. *Bull. Amer. Meteor. Soc.*, **66**, 264–273.

National Academy of Sciences-National Research Council, Committee on Planned and Inadvertent Weather Modification, 1966: *Weather and Climate Modification: Problems and Prospects*, Vol. I, Summary and Recommendations. Final Report of the Panel on Weather and Climate Modification to the Committee on Atmospheric Sciences, NAS-NRC Publication 1350, 57pp.

National Center for Atmospheric Research, Research Applications Laboratory, 2005: Final Report to the United Arab Emirates: Rainfall enhancement and air chemistry studies. 634pp.

____, 1973: *Weather and Climate Modification: Progress and Problems*, T. F. Malone, Ed., Government Printing Office, Washington, D. C., 258 pp.

____, Committee on Science, Engineering, and Public Policy, 1989: *On Being A Scientist*, National Academy Press, 26pp.

____, Committee on Science, Engineering, and Public Policy, 1995: *On Being A Scientist*, 2nd Edition, National Academy Press, 27pp.

National Research Council, 2003: *Critical Issues in Weather Modification Research.* National Academy Press, 123 pp

____, Committee on Science, Engineering, and Public Policy, 2009: *On Being A Scientist*, 2nd Edition, National Academy Press, 82pp.

Nature editorial, 2008: Solutions, not scapegoats, **453**, 957.

Neyman, J., 1980: Comments on the special issue of communications in Statistics (Vol. A8, No. 10) concerned with weather modification experiments. *Commun. Statist. Method.,* **A9** (9), 965-992.

Neyman, J. and Scott, E. L. (1961). Further comments on the final report of the advisory committee on weather control, *J. Amer. Statist. Assoc.* , **56**, 590-600.

__________, and E. L. Scott, 1967: Planning an experiment with cloud seeding. *Proc. Fifth Berkeley Symposium on Mathematical Statistics and Probability, Vol. V*, University of California Press, 327–350.

__________, ____________, and M. Vasilevskis, 1960: Statistical evaluation of the Santa Barbara randomized cloud-seeding experiment. *Bull. Amer. Meteor. Soc.,* **41**, 531–547.

___________, E. L. Scott, and M. A. Wells, 1969a: Statistics in meteorology, *Review of Intern. Statist. Institute*, **37**, 119-148.

___________, ________, and J. A. Smith, 1969b: Areal spread of the effect of cloud seeding at the Whitetop experiment , *Science*, **163**, 1444-1445.

Nickerson, E. C., 1979: FACE rainfall results: Seeding effect or natural variability? *J. Appl.*

Meteor., **18**, 1097-1105.

___________, 1981: Reply--The FACE-1 seeding effect revisited" (to Flueck et al.), *J. Appl. Meteor.*, **20**, 108-114.

Orville, H. D., 1997: Comments on "Weather modification--a theoretician's viewpoint." *Bull. Amer. Meteor. Soc.*, **78**, 2010-2011.

Pettersen, S., J. Spar, F. Hall, R. R. Braham, Jr., H. R. Byers, H. J. aufm Kampf, J. J. Kelly, and H. K. Weickmann, 1956*: Cloud and Weather Modification: A group of field experiments.* Meteor. Monogr., **2**, 11, Amer. Meteor. Soc., 111pp.

Ramanathan, V., P. J. Crutzen, J. T. Kiehl, and D. Rosenfeld, 2001: Aerosols, climate and the hydrological cycle. *Science*, **294**, 2119-2124.

Rangno, A. L., 1972: Case study on some characteristics of the specially monitored storm episodes within the Colorado River Basin Pilot Project. *Report to the Bureau of Reclamation*, 105pp.

___________, 1979: A reanalysis of the Wolf Creek Pass cloud seeding experiment. *J. Appl. Meteor.*, **18**, 579–605.

___________, 1986: How good are our conceptual models of orographic clouds? In *Precipitation Enhancement--A Scientific Challenge*, R. R. Braham, Jr., Ed., Meteor. Monographs, **43**, Amer. Meteor. Soc., 115-124.

___________, 1988: Rain from clouds with tops warmer than –10° C in Israel. *Quart. J. Roy. Meteor. Soc.*, **114**, 495–513.

___________, and P. V. Hobbs, 1980a: Comments on "Randomized seeding in the San Juan mountains of Colorado." *J. Appl. Meteor.*, **19**, 346-350.

___________, and ___________, 1980b: Comments on "Generalized criteria for seeding winter orographic clouds". *J. Appl. Meteor.*, **19**, 906-907.

___________, and ___________, 1981: Comments on “Reanalysis of ‘Generalized criteria for seeding winter orographic clouds’”, *J. Appl. Meteor*., **20**, 216.

___________, and ___________, 1987: A re-evaluation of the Climax cloud seeding experiments using NOAA published data. *J. Climate Appl. Meteor.*, **26**, 757-762.

___________, and ____________, 1988: Criteria for the development of significant concentrations of ice particles in cumulus clouds. *Atmos. Res.*, **21**, 1–13.

___________, and _________, 1993: Further analyses of the Climax cloud-seeding experiments. *J. Appl. Meteor.*, **32**, 1837-1847.

___________, and _________, 1995a: A new look at the Israeli cloud seeding experiments. *J. Appl. Meteor*., **34**, 1169-1193.

___________, and _________, 1995b: Reply to Gabriel and Mielke. *J. Appl. Meteor*., **34**, 1233-1238.

___________, and _________, 1997a: Reply to Rosenfeld. *J. Appl. Meteor.*, **36**, 272-276.

___________, and _________, 1997b: *Comprehensive Reply to Rosenfeld*, Cloud and Aerosol Research Group, Department of Atmospheric Sciences, University of Washington, 25pp.

___________, and _________, 1997c: Reply to Ben-Zvi. *J. Appl. Meteor.*, **36**, 257-259

___________, and _________, 1997d: Reply to Dennis and Orville. *J. Appl. Meteor.*, **36**, 279.

___________, and _________, 1997e: Reply to Woodley. *J. Appl. Meteor.*, **36**, 253.

___________, and _________, 1997b

____________., P. V. Hobbs, and L. F. Radke, 1977: Tracer and diffusion and cloud microphysical studies in the American River Basin. Final Report to the Bureau of Reclamation, Report X from the Cloud and Aerosol Research Group, University of Washington, Seattle.

Reynolds, D. W., 1988: A report on winter snowpack-augmentation. *Bull Amer. Meteor. Soc.,*

69, 1290-1300.

Rhea, J. O., 1983: "Comments on 'A statistical reanalysis of the replicated Climax I and II wintertime orographic cloud seeding experiments.'" *J. Climate Appl. Meteor.,* **22**, 1475-1481.

__________, L. G. Davis, and P. T. Willis, 1969: The Park Range Atmospheric Water Resources Project. *Final Report to the Bureau of Reclamation,* 285pp.

Rosenfeld, D., 1997: Comment on "Reanalysis of the Israeli Cloud Seeding Experiments", *J. Appl. Meteor.*, **36**, 260-271.

___________: The third Israeli randomized cloud seeding experiment in the south: evaluation of the results and review of all three experiments. *Preprints, 14th Conf. on Planned and Inadvertent Wea. Modif.,* Everett, Amer. Meteor. Soc. 565-568.

__________, and H. Farbstein, 1992: Possible influence of desert dust on seedability of clouds in Israel. *J. Appl. Meteor.*, **31**, 722–731.

Rottner, D., L. Vardiman, and J. A. Moore, 1980: Reanalysis of "Generalized Criteria for Seeding Winter Orographic Clouds", *J. Appl. Meteor.*, **19**, 622-626.

_________, _________, and __________, 1981: Reply to Rangno and Hobbs, *J. Appl. Meteor.*, **20**, 217.

Sax, R. I., S. A. Changnon, L. O. Grant, W. F. Hitchfield, P. V. Hobbs, A. M. Kahan, and J. S. Simpson, 1975 :Weather modification: where are we now and where are we going? An editorial overview. *J. Appl. Meteor.*, **14**, 652–672.

Schaefer, V. A., 1946: The production of ice crystals in a cloud of supercooled water droplets. *Science*, **104**, 457-459.

Sharon, D., A. Kessler, A. Cohen, and E. Doveh, 2008: The history and recent revision of Israel's cloud seeding program. *Isr. J. Earth Sci.*, **57**, 65-69.

Silverman, B. A., 1986: Static mode seeding of summer cumuli-a review. *Rainfall Enhancement--A Scientific Challenge*, Meteor. Monogr., **21**, No. 43, Amer. Meteor. Soc., 7–24.

_____________., 2001. A critical assessment of glaciogenic seeding of convective clouds for rainfall enhancement. *Bull. Am. Meteor. Soc*., **82**, 903-924.

Simpson, J. S., 1979: Comment on "Field experimentation in weather modification." *J. Amer. Statist. Assoc*., **74**, 95-97.

____________ and W. L. Woodley, 1975: Florida area cumulus experiments 1970-1973 rainfall results. *J. Appl. Meteor.*, **14**, 734-744.

Super. A. B., 1974: Silver Iodide Plume Characteristics Over the Bridger Mountain. Range, Montana. *J. Appl. Meteor.* **13**, 62-70.

Super, A. B., and A. Huggins, 1992: Investigations of the targeting of the ground-released silver iodide in Utah: Part II: aircraft observations. *J. Wea. Mod.,* **24**, 35-48.

Thom, H. C. S., 1957: An evaluation of a series of orographic cloud seeding operations. *Final Report of the Advisory Committee on Weather Control*, Vol. II, Government Printing Office, 25-49.

Tukey, J. W., Jones, L. V., and D. R. Brillinger, 1978a: The Management of Weather Resources, Vol. I, Proposals for a National Policy and Program. *Report of the Statistical Task Force to the Weather Modification Advisory Board*, Government Printing Office. 118pp.

_________, D. R. Brillinger, and L. V. Jones, 1978b: *Report of the Statistical Task Force to the Weather Modification Advisory Board*, Vol. II. U. S. Government Printing Office, pE-3.

Vardiman, L., 1972: Ice crystal multiplication in convective elements of winter orographic clouds. *Atmos. Sci. Paper No. 191*, Dept. Atmos. Sciences, Colorado State University, Fort Collins, CO 80511, 88pp.

___________, 1978: The generation of secondary ice particles in cloud crystal-crystal collisions. *J. Atmos. Sci.*, **35**, 2168–2180.

__________, and L. O. Grant, 1972a: A case study of ice crystal multiplication by mechanical fracturing. *Abstracts, Intern. Cloud Physics Conference*, London, Amer. Meteor. Soc., 22-23.

__________, and _________, 1972b: A study of ice crystal concentrations in convective elements of winter orographic clouds. *Preprints, Third Conference on Weather Modification*, Amer. Meteor. Soc., 113-118.

___________, and C. L. Hartzell, 1976: Investigation of precipitating ice crystals from natural and seeded winter orographic clouds. *Final Report to the Bureau of Reclamation*, Western Scientific Services, Inc., 129 pp.

__________, and J. A. Moore, 1978: Generalized criteria for seeding winter orographic clouds, *J. Appl. Meteor.*, **17**, 1769-1777.

Wallace, J. M., and P. V. Hobbs, 1977: *Atmospheric Science: An Introductory Survey*. Academic Press, 467 pp.

Woodley, W., 1997: Comments on "A new look at the Israeli Randomized cloud seeding experiments." *J. Appl. Meteor.,* **36**, 250-252.

World Meteorological Organization, 1992: *Statement on Planned and Inadvertent Weather Modification*, World Meteorological Organization, Geneva.

Wurtele, Z., 1971: Analysis of the Israeli cloud seeding experiment by means of concomitant meteorological variables. *J. Appl. Meteor.*, **10**, 1185–1192.

Xue, L., S. A. Tessendorf, E. Nelson, R. Rasmussen, and D. Breed, 2013: Implementation of a Silver Iodide Cloud-Seeding Parameterization in WRF. Part II: 3D Simulations of Actual Seeding Events and Sensitivity Tests. *J. Appl. Meteor. Climate,* **52**, 1458-1476.

Young, K. C., 1993: *Microphysical Processes in Clouds*. Oxford University Press, 335-336.

__________ , 1997: Reply to Orville. *Bull. Amer. Meteor. Soc.*, **79**, 2011-2013.

Table 1. List of journal-published re-analyses and critical comments on randomized cloud seeding experiments and their conclusions relative to the initial ones reported by the experimenters.

Experiment	Reanalyst/Commentator	Original findings confirmed?
Whitetop[¢]	Lovasich et al. 1969a,b	Yes*
	Neyman et al. 1969a,b	Yes*
	Decker et al. 1971	No*
	Lovisich et al. 1971a,b	Yes*
	Braham 1979	Yes (?)
	Dawkins and Scott 1979	Yes*
Grand River	Gelhaus et al. 1974	No
Climax, Wolf Creek Pass, and others	Grant and Elliott 1974	Yes, all
Santa Barbara II	Bradley et al. 1978, 1979	No then Yes*?
Tasmania	Mason (1980, 1982)	No*
Florida Area Cumulus-1	E. C. Nickerson 1979, 1981	No*
	Mason 1980, 1982	No*
Wolf Creek Pass	Rangno 1979	No*
Climax I and II	M79	No, both
	Hobbs and Rangno 1979	No
	Mason 1982	Yes*
	Mielke et al. 1981	Yes, both
	Mielke et al. 1982	Yes, both
	Mielke and Medina 1983	Yes, both
	Rhea 1983	No*
	Rangno and Hobbs 1987, 1993, 1995a	No*
CRBPP[†]	Rangno and Hobbs 1980a	No
Climax, and several others	Vardiman and Moore 1978	Yes
Climax, and several others	Rangno and Hobbs 1980b, 1981	No*
Climax, and several others	Rottner et al. 1980, 1981	No*
Israeli I	Wurtele (1971)	Yes* (?)
	Mason 1980, 1982	Yes*
	Rangno and Hobbs 1995b, 1997a, 1997b	No*
	Rosenfeld (1997)	Yes (?)
Israeli II	Mason 1980, 1982	Yes*
	Gabriel and Rosenfeld 1990	No (?)
	Rosenfeld and Farbstein 1992	Yes (?)
	Rangno and Hobbs 1995b	No*
	Rangno and Hobbs (1997a,b)	No*
	Rosenfeld (1997)	Yes (?)
	Silverman (2001)	No*
	Levin et al (2010	No*

[¢]Original result suggested decreases in rainfall on seeded days.
? Suggests ambiguous results; evidence for a positive seeding effects were also found, amid indications of no effect.
? See this reference for further discussion concerning ambiguous results.
*The reanalysis was performed by persons not associated with the original experimenters *or* the institution that conducted it.
[†]Colorado River Basin Pilot Project

Figure Captions

Figure 1: a) map of the percentage of increases in snowfall attributed to cloud seeding in the Climax I randomized experiment when the 500 hPa temperature was $\geq$-20° C (after Mielke et al. 1970); b) map of the seed/no seed double ratios for the North target area of Israeli II and its subsections (denoted by the letter N with subscripts) (after GN81, Table 5). Those double ratio values above 1.00 suggest a seeding effect of the same magnitude (in percent) as the fractional value above or below 1.00. The letter C in (1b) marks the control region; c) the target runoffs of the seeded seasons (denoted by X's) and non-seeded seasons (denoted by dots) vs. the control runoffs for the Wolf Creek Pass experiment (after Morel-Seytoux and Saheli 1973); and d) the same as (c) for the Skagit River Project target and control runoffs (after Hasty and Gladwell 1969).

Figure 2. Evaluations of the same reports over a wider field of view; a) Colorado, Climax I, seed/no seed ratios b) Israel, on north target area seeded days, seed/no seed ratios, c) Wolf Creek Pass seeded seasons in watersheds that were sidewind and upwind of Wolf Creek Pass, and d) the Skagit Project runoff for rivers sidewind and upwind of the Skagit River at Newhalem target watershed. These evaluations show that what the experimenters reported as seeding effects were observed over a wide area and could not have been due to seeding.

Figure 3. Ice crystal concentrations vs. cloud top temperature (dots), including the least squares regression (dashed line) for these data (after Gagin 1975). In the original equation shown, the letter "C" denotes ice crystal concentration and the letter "T", the cloud top temperature. The solid line with the open triangles denotes average ice nucleus spectrum. The "X's" are ice crystal concentrations recently measured by Levin et al. (1996). The upper dashed line represents a criteria suggested by Hobbs (1969) above which the observed concentrations of ice crystals qualify as a case of "ice multiplication."

Figure 4. Simulation in the HYSPLIT 2010 12-km dispersion model of the vertical plume dimensions of a 100 acre fire. The immediate vertical rise is not applicable to a cloud seeding generator which produces, in contrast virtually no heat. The modeled AgI plume top by Xue et al 2013 is 2-km higher for unknown reasons.

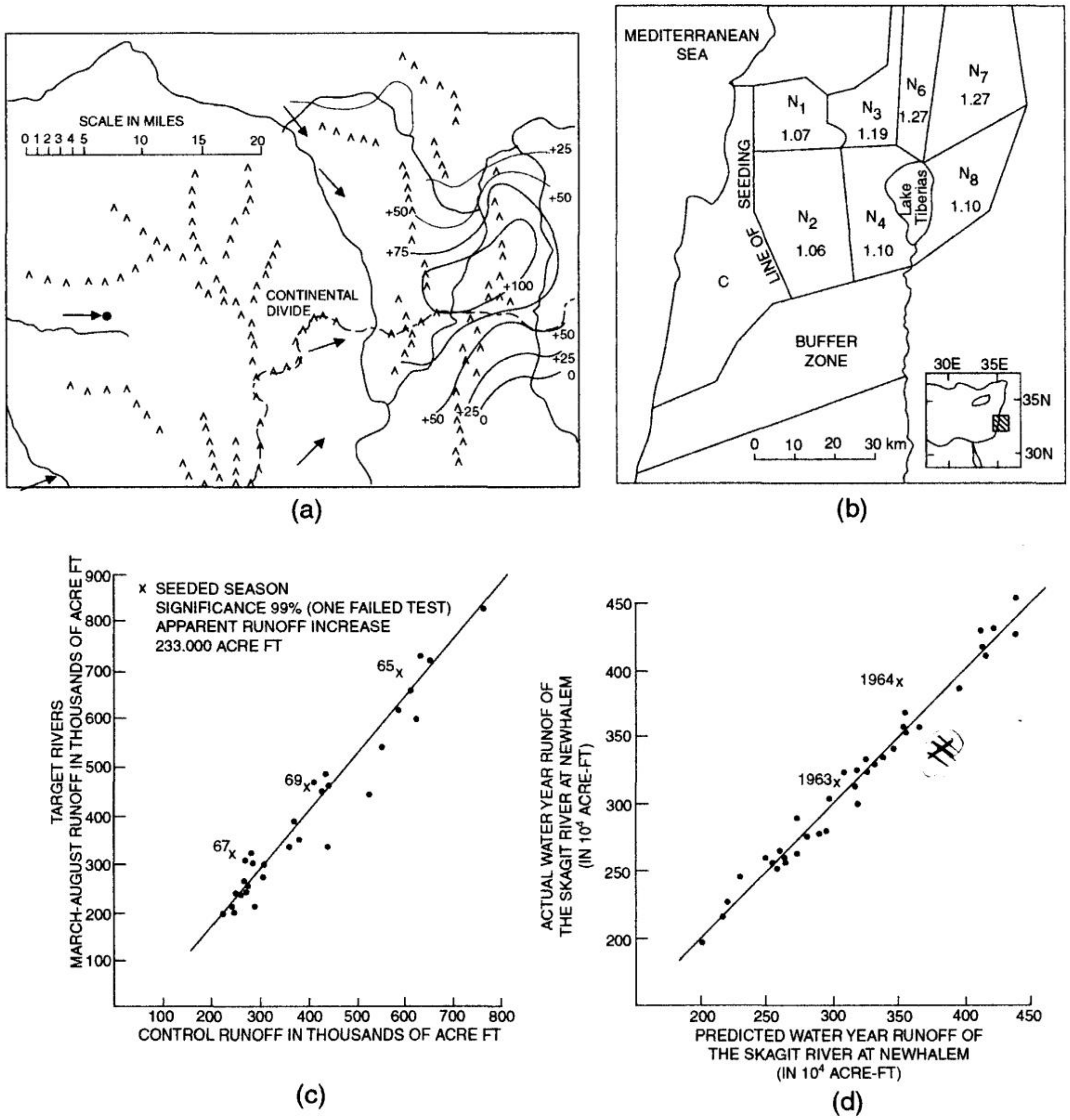

SCALE IN MILES
0 1 2 3 4 5 10 15 20
CONTINENTAL DIVIDE
+25
+50
+75
+100
0
(a)
MEDITERRANEAN SEA
N_1 1.07
N_3 1.19
N_6 1.27
N_7 1.27
N_2 1.06
N_4 1.10
N_8 1.10
Lake Tiberias
LINE OF SEEDING
C
BUFFER ZONE
0 10 20 30 km
30E 35E
35N
30N
(b)
x SEEDED SEASON
SIGNIFICANCE 99% (ONE FAILED TEST)
APPARENT RUNOFF INCREASE
233.000 ACRE FT
65x
69x
67x
TARGET RIVERS
MARCH-AUGUST RUNOFF IN THOUSANDS OF ACRE FT
CONTROL RUNOFF IN THOUSANDS OF ACRE FT
(c)
1964x
1963x
ACTUAL WATER YEAR RUNOF OF THE SKAGIT RIVER AT NEWHALEM (IN 10^4 ACRE-FT)
PREDICTED WATER YEAR RUNOFF OF THE SKAGIT RIVER AT NEWHALEM (IN 10^4 ACRE-FT)
(d)

Figure 1

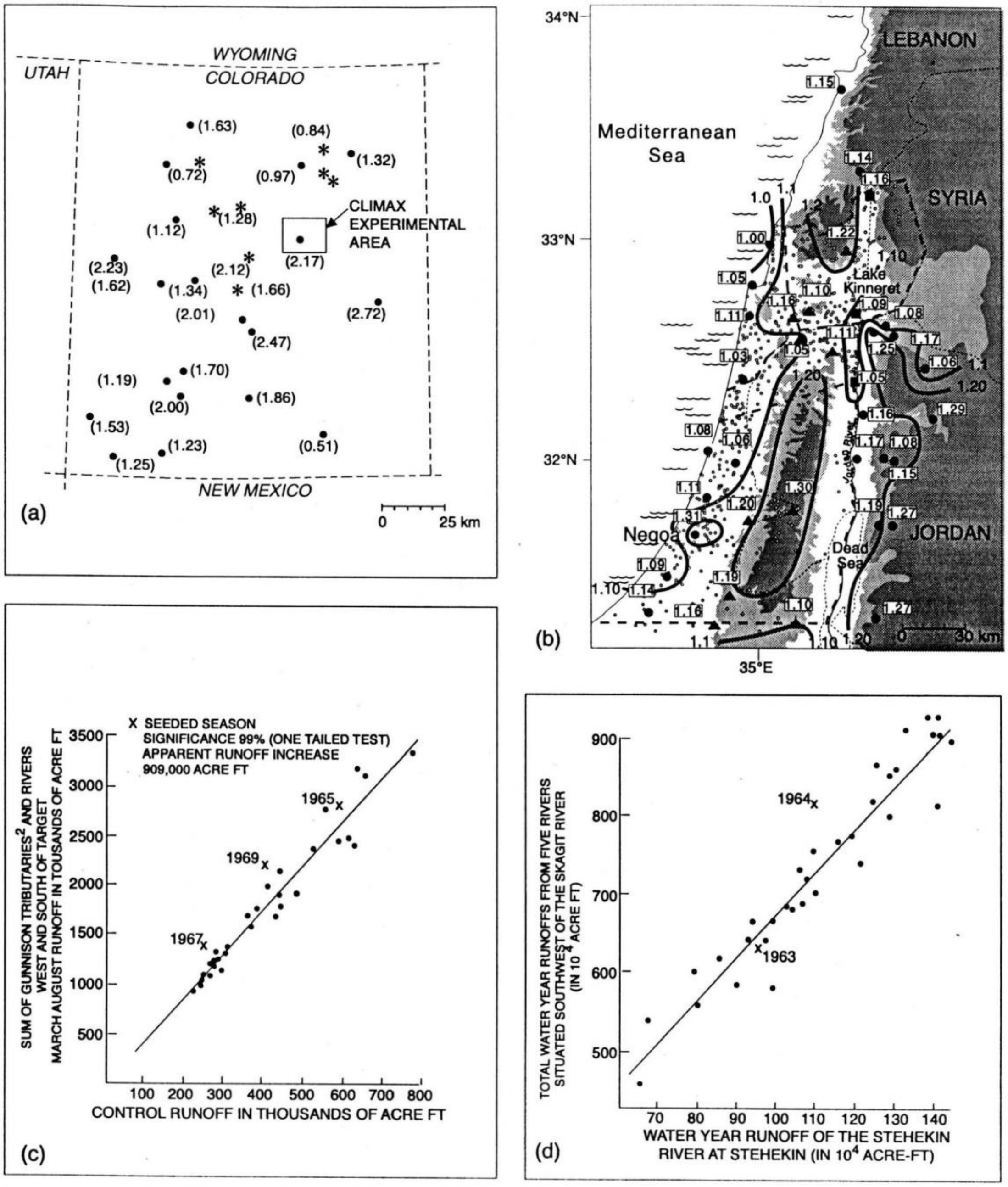
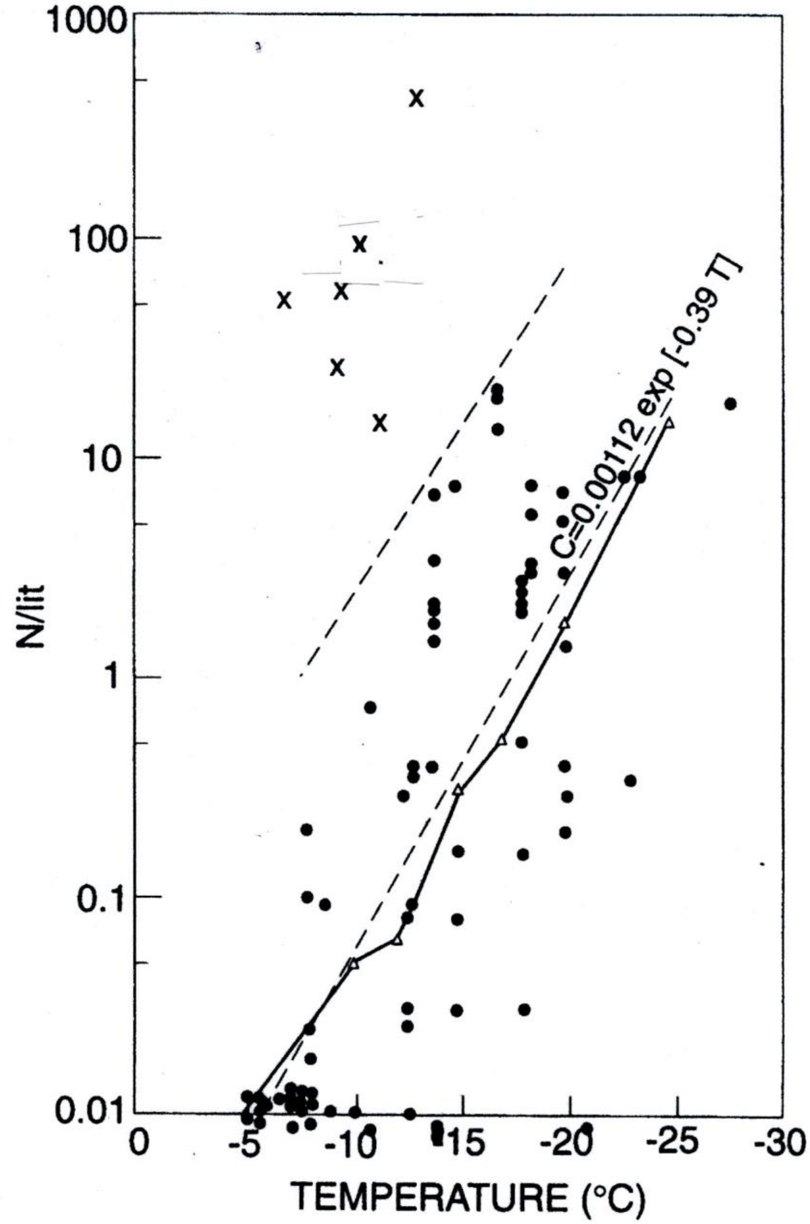

WYOMING
UTAH
COLORADO
CLIMAX EXPERIMENTAL AREA
NEW MEXICO
(a)
0 25 km
34°N
33°N
32°N
35°E
LEBANON
SYRIA
JORDAN
Mediterranean Sea
Lake Kinneret
Dead Sea
Negoa
(b)
X SEEDED SEASON
SIGNIFICANCE 99% (ONE TAILED TEST)
APPARENT RUNOFF INCREASE
909,000 ACRE FT
SUM OF GUNNISON TRIBUTARIES2 AND RIVERS WEST AND SOUTH OF TARGET MARCH AUGUST RUNOFF IN TOUSANDS OF ACRE FT
CONTROL RUNOFF IN THOUSANDS OF ACRE FT
(c)
TOTAL WATER YEAR RUNOFFS FROM FIVE RIVERS SITUATED SOUTHWEST OF THE SKAGIT RIVER (IN 10^4 ACRE FT)
WATER YEAR RUNOFF OF THE STEHEKIN RIVER AT STEHEKIN (IN 10^4 ACRE-FT)
(d)

Figure 2



1665

1666 Figure 3

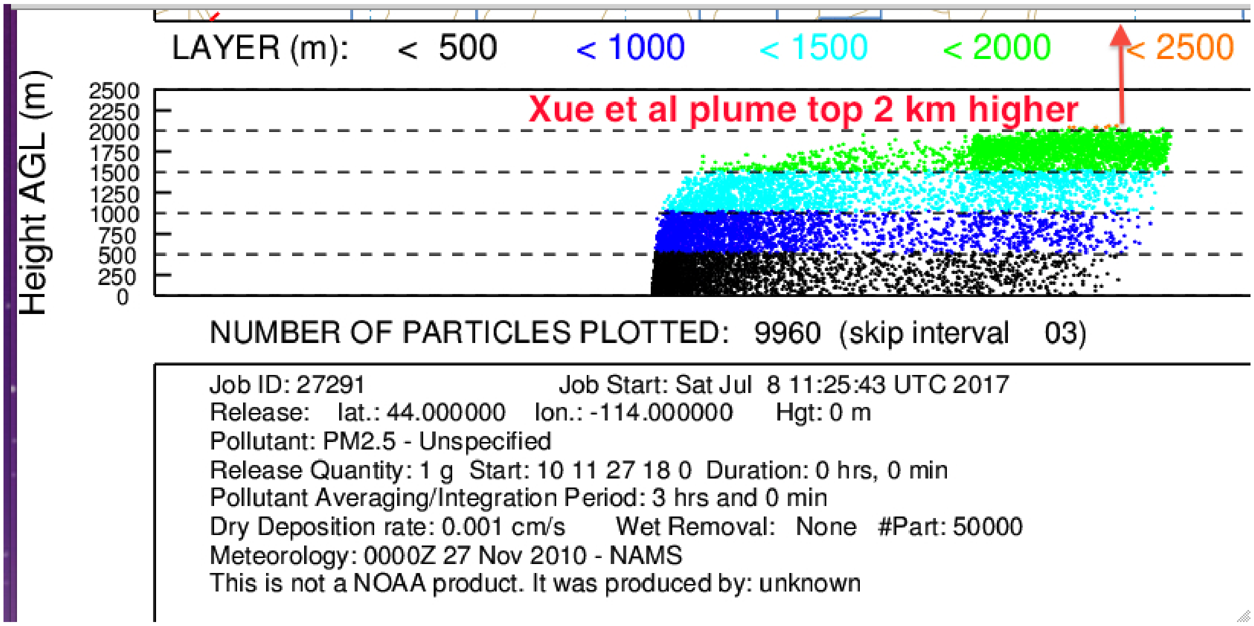

LAYER (m): < 500 < 1000 < 1500 < 2000 < 2500
Xue et al plume top 2 km higher
Height AGL (m)
2500
2250
2000
1750
1500
1250
1000
750
500
250
0
NUMBER OF PARTICLES PLOTTED: 9960 (skip interval 03)
Job ID: 27291 Job Start: Sat Jul 8 11:25:43 UTC 2017
Release: lat.: 44.000000 lon.: -114.000000 Hgt: 0 m
Pollutant: PM2.5 - Unspecified
Release Quantity: 1 g Start: 10 11 27 18 0 Duration: 0 hrs, 0 min
Pollutant Averaging/Integration Period: 3 hrs and 0 min
Dry Deposition rate: 0.001 cm/s Wet Removal: None #Part: 50000
Meteorology: 0000Z 27 Nov 2010 - NAMS
This is not a NOAA product. It was produced by: unknown

Figure 4