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

Closing the Gaps

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ABSTRACT

Scientific knowledge published in journals is supposed to be of a special, verifiable kind which reaches journals through the peer-review process. However, the barriers against poorly supported claims occasionally fail. In the domain of the cloud seeding literature, independent reanalyses of the results of journal-published randomized cloud seeding experiments that appear to have produced positive results, even those that have been widely accepted by our best scientists, have often led to the discovery of flaws that contravene or at least cast significant doubt on the original report. These flaws could have been, and perhaps, should have been detected in the peer review process. They have had several aspects: 1) a Type I statistical error (or lucky draw) that favored naturally heavier precipitation in the target on seeded days, 2) the positive results of the experiment are dependent on a subset of the stations of many that could have been used as controls; 3) the controls were not selected prior to the experiment, but during or following the experiment and usually were the product of extensive searches; 4) the controls chosen by the experimenters exhibit relatively low precipitation or runoff during the experiment rather than the target having more due to seeding in a wider, regional view, 5) the seeding potential of the clouds in the experiment was overestimated, 6) the seeding efficiency of the experiment was overestimated, 7) faulty data used by the experimenters helped create an ersatz seeding effect, and 8), omissions of experimental data or delayed reporting of new results made it appear that experiments were more successful than they actually were.

A review of two widely acclaimed sets of randomized cloud seeding experiments exhibiting these flaws are used to illustrate that the barriers to the peer-reviewed journal publication of faulty scientific claims have gaps. It is

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[1] The viewpoints expressed in this article are solely those of the author and do not necessarily represent those of the Atmospheric Sciences Department of the University of Washington. The title is that of the original submission in 1997.

suggested that several steps can be taken to remedy the situation. These steps include mandatory reporting of random decisions and other project data in real time, mandatory analysis requirements, and use of a wider range of experts in the review of manuscripts on cloud seeding.

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, 1995, Foster and Huber 1997). These barriers are intended to prevent faulty or poorly supported claims from appearing in print. 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 such claims 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.

In this article, the author will lay out the barriers to the publication and acceptance of faulty science; 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; discuss the results of randomized experiments and whether randomization worked as advertised to eliminate storm and experimenter bias, discuss examples of faulty cloud seeding literature, focusing in detail on two examples of randomized experiments that were widely accepted and how faults caused their unraveling. The article will come full circle by examining whether the peer-review process and other mechanisms against faulty claims could have, and perhaps should have, caught the missteps in the original journal published reports. Some remedies against faulty claims are suggested based on these case studies and the author’s other experiences in this field.

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 papers 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 important data, it is assumed they will report the error and retract or modify their findings in a timely manner.

e) 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.

And, 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 (e.g., Feder and Stewart 1987):

f) Independent validation of experimental results.

In this situation, 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. 1978). 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 the numerical results of the experiment; only arithmetic or data errors, or regional patterns that were not noticed by the original experimenters can emerge.

However, the barriers to the publication of faulty scientific claims 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. This article examines, in particular, weaknesses of these barriers in the journal reporting of randomized cloud seeding experiments.

The journal literature in cloud seeding, of course, 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). Braham (1979), surveying the field observed 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, differences of opinion in this field have flared in exchanges between Rangno and Hobbs 1995 and Mielke 1995, and between Rangno and Hobbs 1997a, b; and several other researchers (Rosenfeld 1997; Woodley 1997; Dennis and Orville 1997) and between Orville 1998 and Young 1998. Kessler (1998) has offered a critique of "dynamic seeding" results reported in the journals.

Why is there so much debate and so many differences of opinion? And, how are these differences of opinion related to failures or inadequacies of the present peer-review process? There are several answers to these questions: attempts at replications of successful cloud seeding results have failed to confirm the original positive result[2], apparently successful experiment s are deemed by a wider or differing groups of experts not to meet rigorous scientific standards of conduct, and reanalyses of experiments turn up flaws that cast doubt on the original report of a seeding success (suggesting weaknesses in the barriers to faulty claims). Differences of opinion then erupt over whether an attempt to replicate a successful seeding result was implemented correctly, or over the validity of a reanalysis. These differences of opinion can be both seen as due to a lack of knowledge about exactly what happens when clouds are seeded, and as a measure that the field has vigor and a healthy amount of skepticism, and perhaps, a polarization of viewpoints. In the two sets of acclaimed experiments discussed below, 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 an unjustifiably long time.

Independent replications of promising experiment results, in which the most satisfying

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[2] This may be caused by the occurrence of different weather regimes between the two experiments.

type of confirmation might be achieved, is almost never attempted because field experiments are complex, multi-faceted operations, costly, and the replication of successful results in a Montana experiment may not appeal to, or be practical for an independent group in Missouri to carry out in Montana or similar locale. Also, it has been argued that scientists prefer to work on new projects rather than write proposals to replicate someone else's success (e.g., Broad and Wade (1982). If this is true, it may be that we have lost sight of just how important it is to have independent replications of promising results.

Otherwise good experiments are also compromised 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, also lessens to a degree the credibility of the experiments.

The results of cloud seeding experiments appear to stimulate different responses depending on the sign of the result. Evaluations of cloud seeding experiments published in journals that find that seeding decreased rainfall can have a cautionary effect on cloud seeding activities[3] and can invite, as did Project Whitetop, vigorous debate and indepedent reanalyses over many years (e.g., Braham 1979). However, the reports of cloud seeding successes do not appear to lead to such profound stimulation of reanalysis activity involving numerous independent investigators. 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 outcome. For example, erroneous reports of a cloud seeding success, backed by what appear to be solid and supportive cloud microstructural studies (which in reality were also flawed), can:

1. delay 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'

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[3] 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.

own reports of cloud microstructure have made it appear that new, similar studies had a low priority,

2. discourage 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[4],
3. cause inaccurate assessments of cloud seeding skill by professional organizations which monitor the field-at-large;[5]
4. lead 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. erode public confidence in the scientific establishment, as when any faulty scientific claim is rectified, but especially when that claim has been widely reported beforehand.

A brief review of the history of cloud seeding experimentation below will illustrate some of the problems that were encountered by the early experimenters and why randomization of experiments became the *modus operandi* and, for the most part, the "going currency" for publication of cloud seeding results in a peer-reviewed journal.

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

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

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[4] It has also been suggested that there is little "glory" in replicating someone else's success and scientists would rather pursue new lines of research (Broad and Wade 1982).

[5] 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.

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 with confidence whether seeding had merely accelerated a natural event that was going to occur sometime later. Similar, natural clouds in the vicinity were almost always precipitating as well during these experiments. 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 cloud producing heavy rain as Kraus and Squires (1947) had reported. 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).

Also, when U. S. Weather Bureau personnel or other independent meteorologists examined early published claims of cloud seeding successes (e.g., MacCready 1952), they often found ambiguous or insufficient evidence 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).

However, some commercial cloud seeding operators argued that government scientists were not as experienced as they were in carrying out seeding. 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 statistically significant. No detectable effects of seeding were found in other geographic settings.

Thom’s findings, however, were subject to severe criticisms by some statisticians. This was mainly because the commercial projects Thom examined were not randomized, were subject to optional starting and stopping times, and because they were only a few of the many commercial projects that had been carried out.

It was becoming clear from the vigorous debate swirling around cloud seeding in the early and mid-1950s that the detection and proof of an economically important effect 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, and to establish a baseline of credible scientific methodology in cloud seeding. 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 those randomized experiments that have been subject to *both* analysis and reanalysis[6] and which have also appeared in the journal literature, suggests that the answer to this question posed above is "no." Table 1 shows that something strange happens when randomized experiments are reanalyzed, especially 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, as perhaps occurs in replications in other physical sciences, the independent reanalyst in cloud seeding most often finds insufficient evidence for a previously claimed seeding effect. For example, in Table 1 flaws were found in fourteen of the 16 reports of increases in precipitation due to seeding, flaws in those analyses that weakened or eliminated their credibility were found in eleven by independent reanalysts and commentators. Using the binomial theorem, the null hypothesis that an independent reanalyst will confirm the original result can be rejected at the 0.01 level.

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

Table 1. List of journal-published reanalyses 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. 1969	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
Santa Barbara II	Bradley et al. 1980	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	Mielke 1979	No
	Hobbs and Rangno 1979	
	Mason 1982	Yes*
	Mielke et al. 1981	Yes
	Mielke et al. 1982	Yes
	Mielke and Medina 1983	Yes
	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 (

[¢] Original results indicated statistically significant decreases in rainfall on seeded days.

?1 Suggested that evidence for a positive seeding effects were also found, though it may not have been given

In most of these cases, the reanalyst has expanded the analysis of 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 (sometimes called a "lucky draw" or a "Type I" statistical error.)

Therefore, Table 1 suggests that flawed reports of randomized cloud seeding successes have breached journal barriers against the publication of faulty claims. The flaws discovered have also not appeared to have been due to "SORTIES" (search and destroy missions) by anti-seeding fanatics using esoteric variables to dispose of seeding effects.

The purpose of this review is to find out why reanalysts often 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 were inadequate and need to be strengthened. In these key experiments, both reanalysts and the original experimenters themselves played roles in exposing flaws.

Since an article critiquing the peer-review process and cloud seeding simultaneously is doubly provocative, it should be mentioned that the author has been employed as a meteorologist on both sides of the "seeding fence": in the service of commercial cloud seeding projects in South Dakota , India, California, and Washington State, and as a forecaster with the Colorado River Basin Pilot Project (CRBPP), a large randomized cloud seeding experiment carried out in southwest Colorado. Following the CRBPP, the author initiated several reanalyses and comments concerning a number of randomized cloud seeding experiments over the next 25 years (most with Prof. Peter V. Hobbs); they are among those listed in Table 1.

Though the author has often been involved in reanalyses of cloud seeding experiments that tended to diminish the credibility of published claims of a cloud seeding success, he is nevertheless of the opinion (has a "positive bias") that small amounts of precipitation over seasonal totals (perhaps a few percent) are probably produced in well-designed seasonal cloud seeding projects in orographic settings.

5. Examples of Faulty Literature

Figure 1a-d shows data from several journal-published cloud seeding experiments that seemed to support rather unambiguously 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 a seeding effect in the target area (usually after an extensive search) were used for upwind and sidewind regions, the same precipitation or runoff anomalies attributed to seeding were also seen. Hence, in a region-wide view it was a small group of *controls* that had actually behaved anomalously on seeded days rather than the seeded target area. The controls were exceptional because they had experienced less precipitation or runoff relative to most of the other stations and watersheds in the region. The complete discussions of these seemingly robust experiments can be found in the references in the figure caption. The results of the reanalyses, by the way, should not be construed as meaning that there was no seeding effect whatsoever in those experiments; it simply weren't detectable.

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 in actual practice.

The "sign" of these claims by the experimenters, one cannot fail to observe, is generally in the same direction; that is, to report that the cloud seeding experiment was more successful than it actually was either in the results themselves or in many subsidiary statements about how the experiments were carried out. Because of this tendency, the errors do not appear to be random;

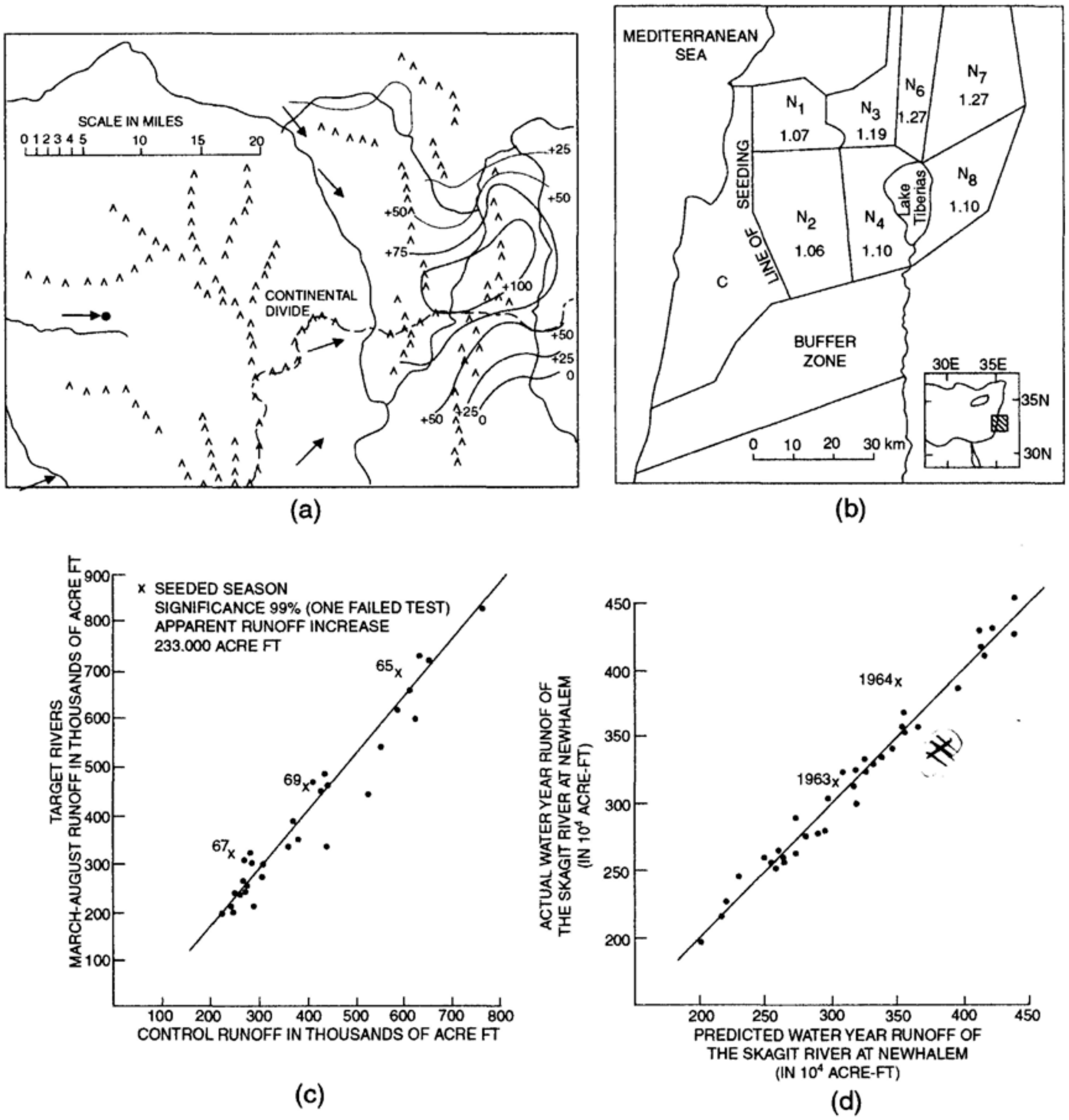

Figure 1: a) map of the percentage of increases in snowfall attributed to cloud seeding in the Climax I randomized experiment when the 500 hPa temperature was ≥-20° C (after Mielke et al. 1970); b) map of the seed/no seed double ratios for the North target area of Israeli II and its subsections (denoted by the letter N with subscripts) (after Gagin and Neumann 1981, 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).

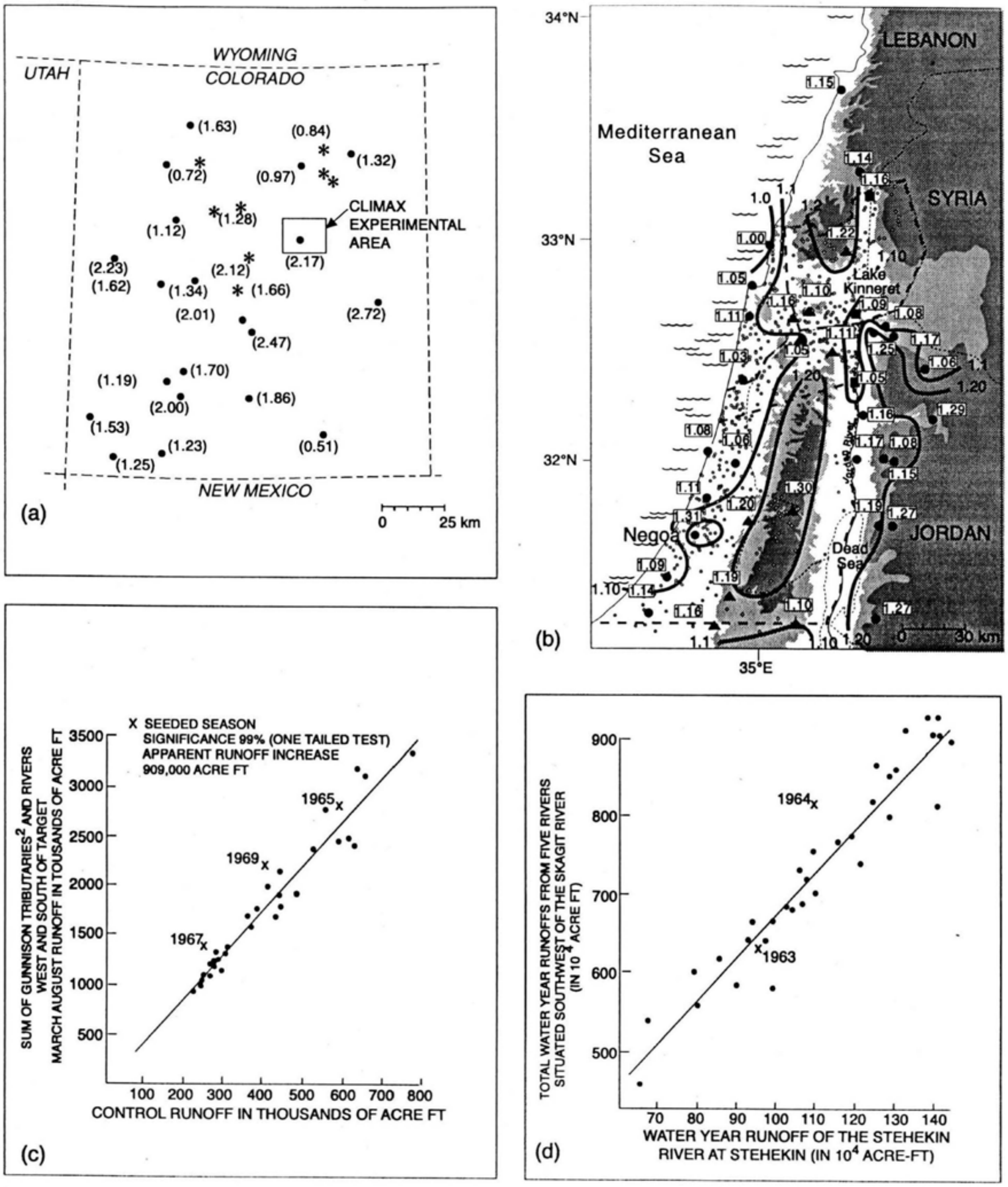

Figure 2. Evaluations of the same reports over a wider field of view; a) Colorado, Climax I, seed/no seed ratios b) Israel, on north target area seeded days, seed/no seed ratios, c) Wolf Creek Pass seeded seasons in watersheds that were sidewind and upwind of Wolf Creek Pass, and d) the Skagit Project 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.

we can conclude that subjective factors may have 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.

Figure 2 shows the results of these experiments, analyzed over wider areas that could not have been seeded.

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 sincere, 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. Two Highly Acclaimed Sets of Randomized Cloud Seeding Experiments

a. *Cloud seeding in the Colorado.*

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). Even today, the original results of these experiments in the Rockies continue to be cited by a few authors (e.g., Cotton and Pielke 1992, 1995), though they have generally fallen out of favor with most scientists.

Why were these Colorado experiments so convincing to the scientific community? 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 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), and the seasonally randomized experiment at Wolf Creek Pass ran for six complete winter seasons (Morel-Seytoux and Saheli 1973). They appeared to confirm one another in the conditions in which seeding produced increases in snowfall; when the 500 hPa temperatures were above -20° to -23° C, large increases in snowfall occurred when the clouds were seeded. 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 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 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., $\geq$-20° C) during storms, cloud top temperatures had to be warm, and the clouds, they further reasoned, contained little natural ice. 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 termed “static” (e.g., Dennis 1980) carried out in Colorado.

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 in the scientific community that the seeding effects reported were real and not mere statistical flukes (e.g., National Academy of Sciences 1973).

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*

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), 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[7] each way upwind of each target on its seeded day[8]. This seeding method was identical to, and probably patterned after that used in the important Project Whitetop experiment then underway in the U. S.

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, Gabriel and Feder 1969, Gabriel and Baras 1970, Wurtele 1971, Gagin and Neumann 1974). 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 (Gagin and Neumann 1974). 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, Gagin and Neumann 1974). This discovery was later inferred by the experimenters to be unintended seeding effect (Gagin and Neumann 1974), 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." 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 of Israeli I in target areas and, apparently, in the buffer zone (Gabriel 1967). Brier et al. (1973) examined rainfall in Lebanon and Jordan, and

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[7] Legs were shortened in those cases where there was no clouds ahead.

[8] Several ground generators were located in the extreme northeast of the North target area near the Syrian border.

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 (see Rangno and Hobbs 1995b).

A second daily randomized experiment, referred to as Israeli II, was carried out 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., Gagin and Neumann 1981). 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 (Gagin and Neumann 1974; 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 area 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 ground generators. 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.

The second experiment in Israel, therefore, had two design/evaluation components, a crossover design using the combined data from both targets, and a target/control design for the North target area. Note that in the crossover experiment there are two ways of examining the results for each target area: using the precipitation data for one target on all of its seeded and control days, and using the precipitation in the adjacent , non-seeded target on the days that the other target area is seeded. According to Gagin and Neumann (1974), the advantage of the latter method 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[9].

However, the results of the completed second experiment were limited for more than 14

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[9] 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."

years to just target-control evaluations of the North (e.g., Gagin and Neumann 1976, 1981, Gagin 1981, 1986, 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 (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 concerning the clouds of Israel also 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., Gagin 1975, 1981, 1986, Gagin and Neumann 1976, 1981, Figure 3). Thus, the microstructure of the 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. Ice multiplication was also reported not to occur in the clouds of Israel (e.g., Gagin 1975, 1981, 1986).

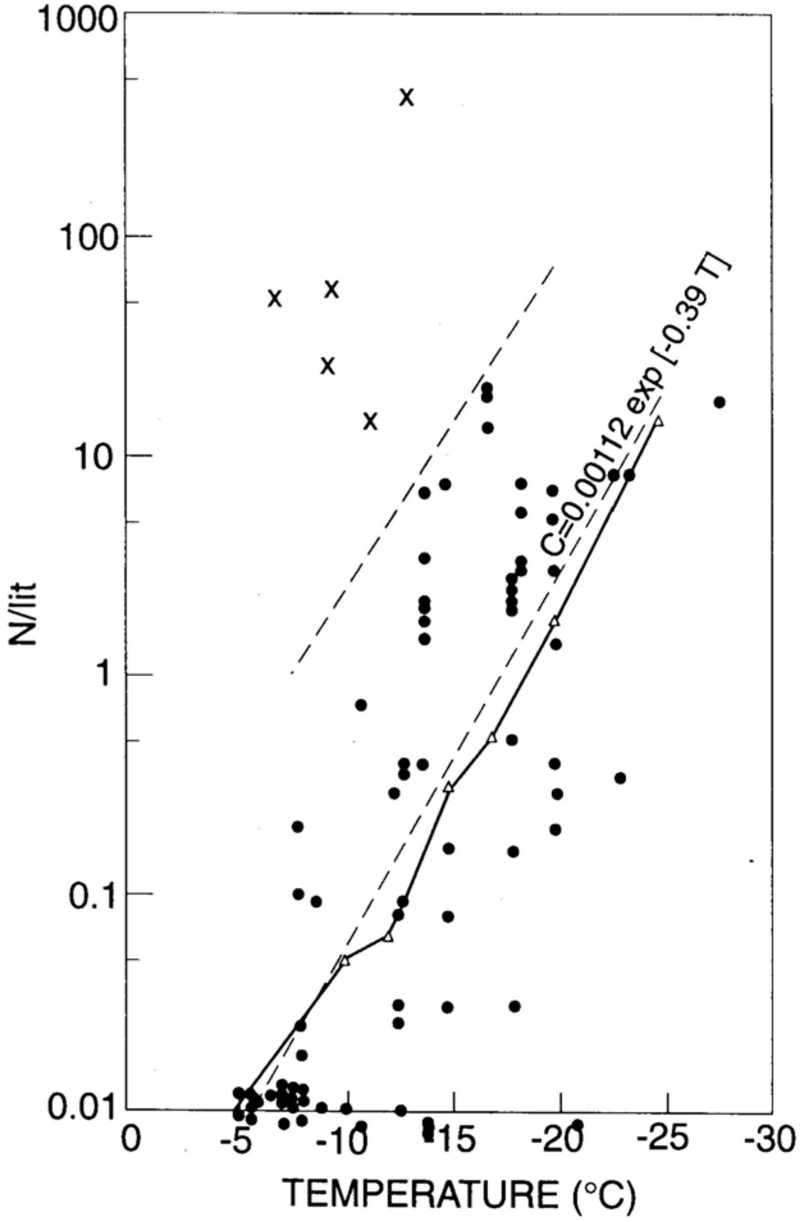

Figure 3. Ice crystal concentrations vs. cloud top temperature (dots), including the least squares regression (dashed line) for these data (after Gagin 1975). In the original equation shown, the letter "C" denotes ice crystal concentration and the letter "T", the cloud top temperature. The solid line 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."

Also, in further defining the statistical results of the second experiment, HUJ scientists reported that radar studies conducted during Israeli II showed that it was *only* those clouds with radar tops between -12° and -21° C that were responsible for the overall increases in rainfall (e.g., Gagin and Neumann 1976, 1981, Gagin 1981, 1986). When the Israeli II results were confined to the effects of seeding on clouds with radar tops between -12° and -21° C, the results were an impressive 40-50% and 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., Gagin and Neumann 1981).

In both the exact temperature range in which seeding appeared to have produced the greatest results, and in the magnitude of the response 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; they appeared to have *measured* the tops of the clouds which 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 (Mielke 1979, Hobbs and Rangno 1979, 1993 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., Gagin and Neumann 1981).

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, Gagin 1986, Gagin and Gabriel 1987). These last findings lent additional credibility to the respective results since, as noted before, the relatively low temperature 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.

Hence, in every way, in spite of the differences in seeding techniques (ground vs. airborne) and the types of clouds seeded (stratiform vs. cumuliform), the results of the two experiments were seeming mirror images of the other. The sets of experiments conducted in Israel, too, were considered complete and credible in every way, just as they had been in Colorado. Thus, with the Climax I and II, and the Israeli I and II 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 (e.g., Cotton and Pielke 1992, 1995).

But should they have been? Could all of these glowing statistical results supported by seemingly solid cloud microstructure studies and various subtle, supportive seeding effects really be "scientific mirages" (Foster and Huber 1997) and still be published in our peer-reviewed journals? The answer to this last question appears to be "yes."

7. The unraveling of the experiments.

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) followed with a stunning report that *both* Climax I and II experiments had been impacted by Type I statistical errors due to lucky 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 findings were repeated by Grant et al. (1979).

Hobbs and Rangno (1979), independently reexamining the foundations of the Climax experiments, found that the experimenters had little or 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 by the experimenters (e.g., Grant and Mielke, 1967, Grant and Elliott 1974).

The WCP experiment, the third piece of the Colorado cloud seeding triad, was also reanalyzed at about this time. It was found that this experiment, too, had suffered from a lucky draw or Type I statistical error (Rangno 1979). The effects that had been attributed to seeding in the target watersheds were also observed over a several state region when the same controls used by the experimenters to elucidate the effects of seeding were used in conjunction with runoff from a several state area. 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. Also, Hobbs et al. (1975) found that seeding material was not reaching the clouds, or did not reach them at locations which could have produced an effect on precipitation in the target in a later experiment that was designed to replicate the WCP and Climax results at Wolf Creek Pass. This later experiment also had a greater number of generators, 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), thus weakening the early claims of high seeding potential for wintertime clouds in Colorado (e.g., Grant 1968). Little correlation between cloud top temperatures and ice particle concentrations was found (Vardiman and Hartzell 1976, DeMott et al. 1982), 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 sound physical basis to explain the supposed results.

Later, however, the Colorado experimenters began publishing new reanalyses of the Climax experiments in the early 1980s. These reanalyses attempted to account for the uneven random draws in Climax I and II; the new results suggested that cloud seeding had, indeed, increased snowfall when the 500 hPa temperatures were $\geq$-20° C (e.g., Mielke et al. 1981). In spite of the Mielke (1979) statement that the upper level temperatures could not have indexed cloud top temperatures in the Rockies, Mielke et al. 1981 nevertheless renewed the claim that the 500 hPa temperature stratifications in their new reanalyses were linked to cloud tops (and, presumably, ice particle concentrations). To date, no evidence has been presented in support of this renewed claim.

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 (Rhea 1983), to the discovery that the published precipitation data for the key, independently maintained gauge at Climax did not match that used by the experimenters (Rangno and Hobbs 1987, 1995a). The seeding effect at Climax at the key cooperative “independent” gauge was nil after the date that the experimenters had selected their subset of control stations in Climax I, and remained nil throughout Climax II when these published precipitation values were used (Rangno and Hobbs 1993). Rhea (1983) also found that when the precipitation data at the control stations were synchronized with the target, the seeding effect diminished to statistical non-significance in Climax II. Logistical problems were also discussed by Rangno and Hobbs (1993). Mielke (1995) has addressed some of these questions and reiterated his belief that the positive results published by the experimenters over many years are, in fact, actual seeding effects at Climax.

The impact of the published results of the Climax and WCP experiments--before the many problems discussed above were discovered in the late 1970s--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., Mason 1980; 1982), there was also the practical impact of having those flawed results lead to an ambitious attempt at an independent replication costing several million dollars in the Colorado River Basin Pilot Project (e. g., Braham 1979). Not surprisingly, 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. 1978, Elliott 1979, 1986, Braham 1979, Rangno 1979, Hobbs 1980, Rangno and Hobbs 1980a). These problems were mainly due to discrepancies in the original experimenters’ assumptions about clouds. 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 (Rangno 1979; see also Elliott et al. 1978; Rangno and Hobbs 1980a for wider discussion of the CRBPP results).

b. *The experiments in Israel*

A similar erosion of confidence in the results of the Israeli experiments has also occurred in the past ten years (e.g., Rangno 1988, Hobbs and Rangno 1988, 1995b, 1997a, 1997b, Gabriel and Rosenfeld 1990, Rosenfeld and Farbstein 1992). Wider analyses of the statistical results, inclusion of previously omitted data from the second experiment, and recent cloud microstructure reports have revealed major discrepancies. For example, when the previously omitted half of the Israeli II crossover experiment (those for the “South” target area) was included to complete the analysis of this experiment, a null result was found; there was no difference between the rainfall on seeded and control days in Israeli II (e.g., Gabriel and Rosenfeld 1990). Rainfall had been unusually heavy in both target areas on the days when the North target area was seeded. Thus, when the results of the two target areas were combined (in a two single experiment approach) apparent rainfall increases in the North target area on seeded days were canceled out by apparent decreases in the South target area (Gabriel and Rosenfeld 1990; Rosenfeld and Farbstein 1992). This appearance of decreases in rainfall due to seeding in the South target area was because the seeded days there could not keep up with the unusually (from a daily climatological viewpoint) heavy rainfall on the control days even if there had been a modest seeding effect comparable to 10%. For example, according to Gabriel and Rosenfeld (1990), 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 normal daily amount . Therefore, for a seeding effect in the South target area to have had a chance of being statistically significant and positive, the average rainfall on the seeded days in the South would have had to have even more anomalous rainfall than did the control days, 50% or more above the normal daily amount, a climatological impossibility over a six year period since these anomalies would have to involve *all* the rainy days of whole seasons, and thus consecutive seasonal rain totals of about 150% for six straight seasons.

Actually the use of the alternate target’s rainfall as the control rainfall when the other target was being seeded to account for the meteorological bias, such as that which occurred on North seeded days in Israeli II, was planned by Gagin and Neumann 1974 but was never carried out by those researchers, nor by Gabriel and Rosenfeld (1990), nor by Rosenfeld and Farbstein (1992) in subsequent reanalyses.

Rangno and Hobbs (1995b) 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. And, when the alternate target's precipitation *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 total experiment by Gabriel and Rosenfeld (1990). 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 Rangno and Hobbs 1995b, 1997a. Rangno and Hobbs (1995b) 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) because the experimenters' used different evaluation techniques in each of the two experiments.

Most recently, it has been suggested by Rosenfeld (1997), in as yet unpublished studies, that the seeding effects 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 (negative) results in the southernmost target areas. For a more complete discussion of these interesting experiments and differing interpretations, see Rosenfeld (1997), Dennis and Orville (1997), Woodley (1997), Ben-Zvi (1997), Rangno and Hobbs (1997a, 1997b).

Finally, the results of a third randomized experiment, conducted in Israel for 18 years beginning with the end of Israeli II in 1975, has recently been reported at conference. This experiment, confined to the South target area of Israeli II, has suggested decreases in rainfall of 5-10% on seeded days (Rosenfeld 1998). The first preliminary result of this experiment (similar to that given above) was reported by Rosenfeld and Farbstein in 1992[10]. These results appear to support the lack of seeding effects in Israeli I and II deduced by Rangno and Hobbs (1995), but also support the argument for a lack of a positive seeding effects in the southern targets in all three experiments (Rosenfeld 1997, 1998).

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[10] Delayed reporting may also have afflicted the Climax experiments. Mielke et al. (1970), writing in June 1969, disclosed that they were *currently* (author's emphasis) examining the precipitation data at all western Colorado precipitation gauges in the context of the Climax I experiment. The outcome of this regional study of seeding effects was not mentioned again by the experimenters until Mielke (1979.)

But the Israeli experiments were not just apparent statistical successes standing in isolation. They were buttressed by seemingly solid cloud microstructure reports. Figure 3 is a plot of cloud microstructural 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, Silverman 1986, Dennis 1989). The great seeding potential seen in these data was because the clouds appeared to be able to form only a few ice crystals per liter on average even when cloud tops were as cold as -21° C (dots in Figure 3). 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 (e.g., Mason 1971). Because cloud bases in the eastern Mediterranean are located at temperatures 5-10° 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 of which the introduction of artificial ice crystals (and later as raindrops) could take advantage. These cloud 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 (e.g., Kerr 1982, Silverman 1986, Dennis 1989).

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 et al. (1996) in clouds with tops from only about -7° to about -15° 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 five flights.

It is now generally agreed that the clouds of the eastern Mediterranean could not be accurately described by the original experimenters (e.g., Rangno and Hobbs 1995b, 1997a, Woodley 1997, Rosenfeld, 1997).

Thus, the "mirror image" cloud microstructure reports that matched those in the Colorado Rockies, and which also appeared to explain *why* seeding had worked in Israel, were deficient. Neither the clouds in Colorado nor those in Israel are virtually ice-free until their tops are colder than -20° C. Ice multiplication is now known to be active in both locations (*loc. cit.*). And no one has yet documented the cloud that is responsible for producing virtually the entire statistical significance in precipitation in either project; the cloud that does not naturally precipitate until seeded, and then when it is precipitating, it does so at the same rate as natural precipitation. Only such non-precipitating clouds could have provided the extra duration seeding effect responsible in *both* experiments on which the statistical significance rested (e.g., Chappell et al 1971; Gagin 1986; Gagin and Gabriel 1987).

Lastly, Rangno and Hobbs (1995b, 1997b) concluded that the stratifications of seeding effects by cloud top temperatures in Israeli 2 (they were not done in Israeli 1) are unreliable because of inadequacies of the 3-cm wavelength radar used by the experimenters for this task, due in large part by the radar's distance from the North target area.

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; too few natural ice crystals were found, and these reports of low ice-producing clouds buttressed the statistical results of both of these experiments.

8. Why Did Peer Review Fall Short?

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 by our highest professional organizations and panels? 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 none can 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 only a subset of the available control stations in the area examined that were also selected after or mid-way through the experiment after an extensive search;
b) the choice of controls 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 the seeding methods was over-estimated;
g) faulty 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) the results of follow-up experiments which did not replicate the results of previous, “successful” experiment 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 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 which favors their viewpoints to be published;

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, 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 private investigators. An implicit trust exists between authors of manuscripts and reviewers which is that 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 they are reporting was an isolated anomaly. 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 by the reviewer 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 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), it would have helped them confront a probable Type I statistical error (or "good draw"), one that they were unable to detect until ten years later. 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. It is suggested that this may have happened because it was believed that the randomization of experiment itself, conducted over five years, was supposed to 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., Gagin and Neumann 1981). 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 confronted evidence of draws that favored seeded days in their analyses, 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; the seeding of natural non-precipitating clouds was so efficient that it made them precipitate at the same rate as natural clouds. The alternative to such an inference was that the experimenters were dealing with a natural bias that had produced the "duration effect" on seeded days.

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). Indeed, some workers in the Rockies (Rhea et al. 1969, Rangno 1972, Elliott et al. 1973), were already reporting as the experiments in the Rockies wound down or ended 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 as "project reports" within the agency that was largely funding these experiments.[11] 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 ice crystals until the tops were colder than -21° C (e.g., Gagin 1975).

One might assume, reasonably, that improving systems of measurement would have had an effect; if the 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). On the other hand, 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 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 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 (e.g. Hobbs and Rangno 1979; Rangno and Hobbs 1993.)

These two experiences strongly suggest that there is a role for day-to-day weather

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

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 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? Or did the apparent advance/reanalysis/retreat/re-study/advance "model" do as well in advancing the field?

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 review process*

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 and due to the great breadth of territory covered by articles on cloud seeding and the questions raised: 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 statisticians,
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 incomplete results that have been published in our journals thus far.

b. *Improving the analyses of cloud seeding experiments submitted for publication.*

There should be *a priori* mandatory design, analysis, and archiving requirements for every cloud seeding experiment. These mandatory requirements should include:

1. Maps of the test statistic that was used to evaluate the effectiveness of seeding in the target will be shown for all available stations covering an area of at least 250 km radius extending from the center of the target area.
2. 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 1600 or 1700 LST, the times at which the maximum number of NOAA cooperative gauges are read for 24 h totals.

3. Control precipitation *stations* or other covariates against which the effect of seeding will be tested; they must be *publicly* identified *before* an experiment begins.

4. The random decisions of the experiment should be placed in a public repository at the time they are made.
5. 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.
6. Where radar is installed to evaluate seeding effects, the radar will 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.
7. All target and control 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.
8. 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. Precipitation gauges and hydrological measurements must be tamper-proof.
9. Submitted papers that profess to find a seeding effect (or lack of one) based on *post facto* selected controls will not be considered for publication *unless* they

specifically point out the degree to which their investigations have degraded any statistical significance (or lack of it) they find. Some limited reanalyses, those that merely extend the results of the same statistical test to a larger domain, are excluded from this since multiplicity is not involved.

10. Withholding results from cloud seeding experiments for more than five years following the termination of an experiment will not be tolerated and scientists who do so subject to censure.

11. Those who conduct or promote commercial cloud seeding shall not evaluate cloud seeding experiments. This must be left to independent groups.
12. High resolution numerical models (e.g., the Penn State/National Center for Atmospheric Research "MM5" model , etc.) 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 results.*

Following the lead of several leading medical journals, American Meteorological Society 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 that 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.

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

10. Conclusions

This review has suggested 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. 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 activities, such as measuring the size of the effective radius in stratocumulus clouds. There is no tendency to report spurious large effective radii in clouds. On the other hand, we seem to care an awful lot about whether a seeding effect in an experiment is positive (e. g., Table 1).

It is easy to explain the lapses in reporting and peer review oversights discussed in Sections 3 and 4 as the result of an inherent temptation to believe, and solely find, that a cloud seeding experiment was a “success,” that beneficial increases in precipitation actually occurred in the target when the clouds were seeded. A cloud seeding success, in the short term, means glory, perhaps verifying *a priori* personal convictions, and probably means a strong likelihood of continued funding. But in the author’s view, a carefully executed cloud seeding experiment that finds no detectable effect on precipitation is also a success in every scientific sense of the word. And it likely means faster progress in cloud seeding since new questions will likely narrow the focus on the problem to better cloud seeding targets (improved "blocking") and seeding methodologies.

We must also search for the reasons why the release of silver iodide into the air causes us to generally suspend our normal skeptical faculties as workers in science. Why *is* the susceptibility for self-deception and exaggeration so strong in this domain? After 30 years of being on both sides of the "seeding fence," this author is inclined to agree with Neyman's (1980) provocative assessment of the cloud seeding literature cited in the introduction. Consider why: the author has examined six cloud seeding experiments in detail--in excruciating detail of the kind not usually given to other scientific papers--the Wolf Creek Pass, the Skagit, Climax I and II, and Israeli I and II--and *without exception,* these experiments were contaminated by important flaws that compromised or eliminated their credibility. They were all were published as having achieved statistically significant results in precipitation or runoff solely due to seeding.

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 review of the cloud seeding literature on which our present AMS and World Meteorological Organization official assessments rely is a reasonable, essential one. We owe it to the public that funds our efforts.

Because the author is optimistic concerning the potential of at least orographic seeding to provide small, though measurable results that have economic worth, we are also at a crossroads where we owe the public something else; the planning and implementing of the next generation of randomized experiments. We need more than just "evidence" that cloud seeding in orographic settings has increased precipitation by 10% after 50 years of experimentation, as our AMS Policy Statement on Planned and Inadvertent Weather Modification asserts (Amer. Meteor.

Soc., 1998). This assessment must be *demonstrated* in experiments and replications of the stringent kind routinely demanded by scientists in other fields. After all, the “enemy,” it appears, is “us.”

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 helped to elucidate many points once they were.

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 the past two decades.

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Table 1. List of journal-published reanalyses 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. 1969	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
Santa Barbara II	Bradley et al. 1980	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	Mielke 1979	No
	Hobbs and Rangno 1979	
	Mason 1982	Yes*
	Mielke et al. 1981	Yes
	Mielke et al. 1982	Yes
	Mielke and Medina 1983	Yes
	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 (?)

[¢] Original results indicated statistically significant decreases in rainfall on seeded days.

?1 Suggested that evidence for a positive seeding effects were also found, though it may not have been given attention due to the overall decreases in rain indicated.

? See this reference for further discussion concerning ambiguous results.

*The reanalysis was performed by persons not associated with the original published results of the original experiment *or* the institution which conducted it.

[†] Colorado River Basin Pilot Project

Figure Captions

Comparable or greater results to those shown here for the target areas were also found in areas which could not have been seeded (e.g., Hobbs and Rangno 1978, Mielke, 1979; Grant et al. 1979; Rangno 1979, Rangno and Hobbs 1995b.)

Figure 2