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 often 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, and 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.

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, 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[1] 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 an unjustifiably long time[2]. 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 are ersatz, have led to:

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'

[1] 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.

[2] 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)

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;[3]
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 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,

[3] 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.

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[4].

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!

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).

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 statistically significant.

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

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.

In spite of 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, and to establish a baseline of credible scientific methodology in cloud seeding trials.

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 also appeared in the journal literature and that have been subject to *both* analysis and reanalysis or critical commentaries that weakened their status.[5] Table 1 strongly suggests that the answer to the question posed in the title

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

of this section 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, the independent re-analyst in cloud seeding 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 reanalyst will confirm an a report of increases in precipitation due to seeding can be rejected at the 0.04 level. The outcomes of reanalyses by the original experimenters also often result in a weakened claim for seeding effects or cannot substantiate them, corroborating this trend. No independent re-analyst has found indications of a seeding effect *larger* than was in the original report.

In most of these cases, the re-analyst 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. Such findings are sometimes called "lucky draws" or more technically a "Type I" statistical errors where the null 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 and need to be strengthened.

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. Figure 3 is a plot of western Colorado runoffs against the Wolf Creek Pass Experiment target runoffs that emphasizes this point. 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 Highly Acclaimed Randomized Cloud Seeding Experiments in 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 these 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 that 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 third, a seasonally randomized experiment at Wolf Creek Pass that ran for six complete winter seasons (Morel-Seytoux and Saheli 1973). These experiments appeared to confirm one another in the conditions in which seeding produced increases in snowfall; when the 500 hPa temperatures were above -20°C 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 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., $\geq$-20° 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 termed "static carried out in Colorado" (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 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.

7. The unraveling of the Colorado 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."

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[6].

[6] Presented by J. O. Rhea.

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).

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 due to seeding.

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 a comparison, the cloud seeding generator releases studied by Hobbs 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, in spite of 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 the 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) further 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 (RH93, hereafter RH93, Figure 1). There was no further indication of a seeding affect at that 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.

Seeding logistical problems, and as yet, inexplicable interruptions in the flow of random draws affected the outcome of the Climax experiments were also exposed by 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 discovered 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, if well-planned, and 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).

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, moreover, gain widespread acceptance as solid, unambiguous cloud seeding results as seen 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 really 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 or her own viewpoint. Recently, the flagship journal of the American Meteorological Society, its *Bulletin*, has made this process more susceptible to bias since it only now accepts only cursory descriptions of manuscripts in “proposals.” These can be deflected even before peer-review takes place by editors who don’t like what an article might be about rather than letting reviewers decide on suitability for its magazine.

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 "Code of Silence";

r) the most knowledgeable critics within a cloud seeding establishment may be under financial duress if they comment critically and publically 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). 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), 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.

A suggestion of natural bias was that in both the Colorado and Israeli experiments, the seeding effect was confined to a precipitation duration effect, a report compatible with the kind of seeding carried out. In other words, the experimenters were reporting that the seeding of naturally non-precipitating supercooled clouds was so efficient that it made them precipitate at the *same rate* as natural clouds, a red flag, and an unlikely outcome. The alternative to this finding by the experimenters was that they 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). 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 as "project reports" within the agency that was largely funding these experiments.[7] The scientific personnel within the agency also did not act to publish or make known these findings, in effect constrained by the "Code of Silence."

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). The same could be said about the Colorado experiments; the experimenters apparently never examined rawinsonde data during storms in the Rockies concerning the heights of cloud tops in the manner in which they selected their temperatures (during the 12 h of an experimental 24 day with the greatest precipitation). The Colorado experimenters also had a vertically-pointed 3-cm radar (as did the experimenters in Israel) and both were unable to see the fallacy of their claims: in Colorado, that 500 hPa and cloud tops were well-correlated (e.g., Grant and Mielke, 1967) and Grant and Elliott 1974) using a graduate student's thesis (Furman 1967) to support this claim, not counter it. However, HR79 found that the evidence alluded to in Furman (1967) by Grant and Elliott (1974) was nowhere to be found.

The experiences in Colorado strongly suggest that there is a role for day-to-day weather forecasters in the review of manuscripts on cloud seeding. The cloud properties and storm types that might befuddle cloud seeding experiments within their forecasting domain would be well-

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

known and such claims as that the 500 mb temperature and cloud tops were well-correlated would never have withstood the inspection by an everyday weather forecaster. It only stood up to reviewers of the Colorado experimenters' many manuscripts.

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 (Vernon, 1955) with the U. S. Weather Bureau in San Francisco (Neyman et al. 1961).

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, there have been a more rapid advance in cloud seeding experimentation because so many questions would have been raised immediately and investigated.

9. 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 those stories are old hat. Have we learned from the these painful, costly lessons of inadequate peer review since the Colorado and Israeli experiences and have "closed the gaps" to faulty literature?

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 (CRBPP) of the late 1960s and early 1970s, the latter undertaken in to replicate the apparent large (but in reality, non-existent) increases in snowfall that were being reported in the Climax I and II, and Wolf Creek Pass 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 dozens to tens of thousands of workers dependent on funding of cloud seeding programs, are surely ripe for producing unreliable results concerning cloud seeding research. This is 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 editorials about the causes of fraud in science in Nature (2008) and earlier by Kennedy (2003).

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 syndrome" 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 of the National Center for Atmospheric Research 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. In any reading a person would find it had no indications of bias, and all relevant literature is cited. The same can be said for Bruintjes et al. 1994 in an evaluation of cloud microstructure and seeding potential in the mountains of Arizona, and numerous other publications.

Presently NCAR (using a somewhat 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 scientists in the future? Evaluate the early literature already emanating from NCAR.

Surprisingly, because it is "NCAR", the nation's leading atmospheric science organization, in Breed et al. (2014) of NCAR, and in their flagship manuscript describing the many robust elements of the design of the $9 million project, signs of bias are nevertheless plain to see. Examples of citations prove this controversial assertion that unreliable reporting still lives in the cloud seeding domain, and, specifically, in some quarters of NCAR:

1) The formerly illustrious Climax, CO, randomized experiments (e.g., as seen by the NAS 1973), whose rise and fall cycle are discussed at length in the present article, are cited without their full context by Breed et al. 2014. These researchers cite only: Mielke et al (1981), Mielke et al (1982), and Grant (1986). The "rise" is documented, but not the fall.

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 having a claimed viable physical foundation; that the 500 mb stratifications in these papers are related to cloud tops and cloud top temperatures to ice particle concentrations[1].

There are no further citations of the many that should have appeared for the reader concerning the actual outcome of these experiments; namely, a null result in both Climax I and II (M79) due to storm biases, to irregularities in experimental data processed by the experimenters (RH87, M95, RH95a) lack of execution of the experiment under the rules of its randomization, and logistical impediments (RH93) . The stratifications by 500 mb temperatures by Mielke et al 1981 have no relation to cloud microstructure or cloud dimensions as claimed as M79 himself once explained, corroborated by HR79. 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 bogus.

In slanted writing 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 the suggestion of an orographic seeding success for their journal readers, and perhaps for the sponsors of the Wyoming project.

One-sided citing of prior work produces material damage to the careers of the researchers who "outed" the faults in the Colorado seeding experiments by minimizing their work, making it less cited, less impactful. What was required in Breed et al 2014 was a simple sentence, or even just a footnote, beginning with the word, "However, ….", with one or two references to those who discovered the flaws. But apparently endemic bias prevented even that.

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[1]"warmer orographic cloud-top temperatures prevailed (indexed by the 500mb temperature being $\geq$ - 20°C for both Climax I and II", quoted from Mielke et al. 1981.

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 readers are misled about prior seeding work in the Rockies.

Shultz (2009) reinforces this point: "One-sided reviews of the literature that ignore alternative points of view, however, can be easily recognized by the audience, leading to discrediting of your work as being biased and offending neglected authors…"

However, since Breed et al. 2014 describe the 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. The Climax experiments were single target area experiments. Could it possibly be that these authors were also not aware of the many faults uncovered in the Climax experiments by M79, HR79, Rhea 1983, RH87, RH93, and RH95a, including indications of data irregularities?

2) Breed et al. 2014 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 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 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, Cotton 1986, Cotton and Pielke 2007).

These complications concerning the 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. Instead, Breed et al. rely on observations of supercooled water to infer cloud seeding potential without knowing how that liquid water might be consumed by natural ice-forming processes a few minutes of travel downwind. Liquid water at the leading portions of orographic clouds is common, since natural ice-formation requires the liquid stage to very low temperatures. It does not necessarily imply unambiguous seeding potential unless it can be shown to have existed over and downwind of a target sans the complication of natural precipitation.

10. 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 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. Articles on cloud seeding due to the great breadth of territory covered by those articles and the questions they raise: 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 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 they are used 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 which are then compared for storm bias evaluations.

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. (The present author has a small "confirmation bias.")

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.

11. Conclusions

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

Perhaps this is not surprising that peer-review and self-reporting of problems can't be eradicated so easily. Kennedy (2003) in a Science editorial concerning proven cases of fraud in physics and the biological sciences, informed readers that the main driver of fraud was "career enhancement."

Unless we believe we atmospheric researchers are somehow superior to medical or physics researchers, the pressure to improve or maintain our positions in life will drive some to be less forthcoming in their publications as we have seen, or worse, concerning 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. There appears to be no tendency to report spurious large or small effective radii in clouds. 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 and the data upon which they rest and upon 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 they can learn what they have been paying for during all those years of operational seeding.

Israel paid a dear price for ineffective operational seeding (Sharon et al. 2008) and the Bureau of Reclamation's Division of Atmospheric Water Resources Management a dear price on an experiment (the CRBPP) to replicate non-existent results, both due to hundreds of pages of faulty research published in our journals and in conference preprints by those who had the most to gain in prestige, continued funding, and in the confirmation of *a priori* beliefs. Surely, more flawed operational seeding projects will be "outed" if randomization was mandated for commercial projects, 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 helped to elucidate many points after 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 the past three decades.

Thanks is also due to the late Melanie Richter-Bernburg, Indiana Press, for her careful editing of the original manuscript, and to Alar1, Inc., for the computer grant which allowed the original work to be carried out in the late 1990s.

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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. River runoffs throughout western Colorado with southwest and west exposures plotted against the target runoffs in the Wolf Creek Pass Experiment also having a southwest exposure. This plot demonstrates that it was the control watershed runoffs that were anomalously low during the seeded seasons (X's) of that experiment.

Figure 4. 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 as 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.”

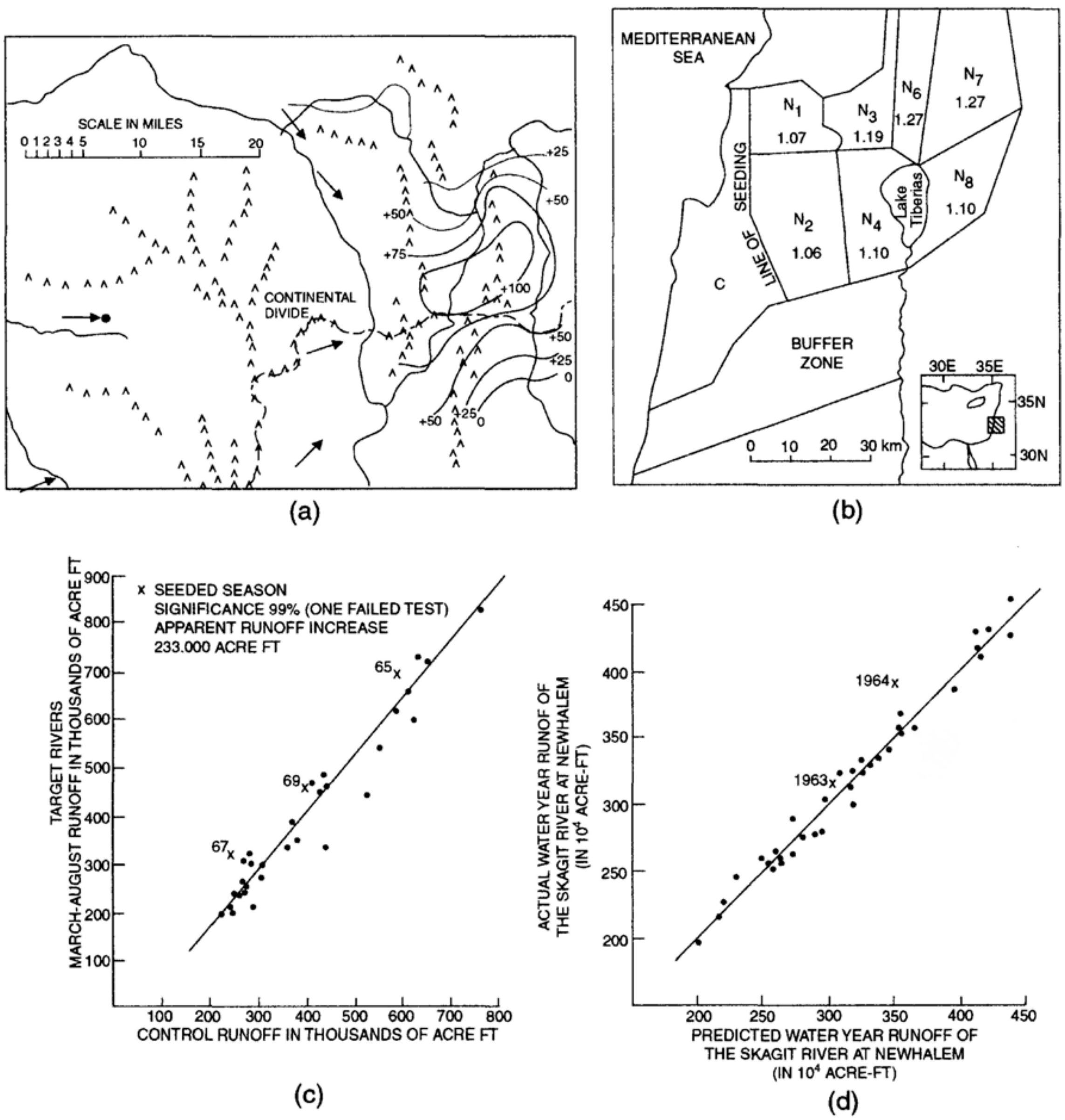


Figure 1

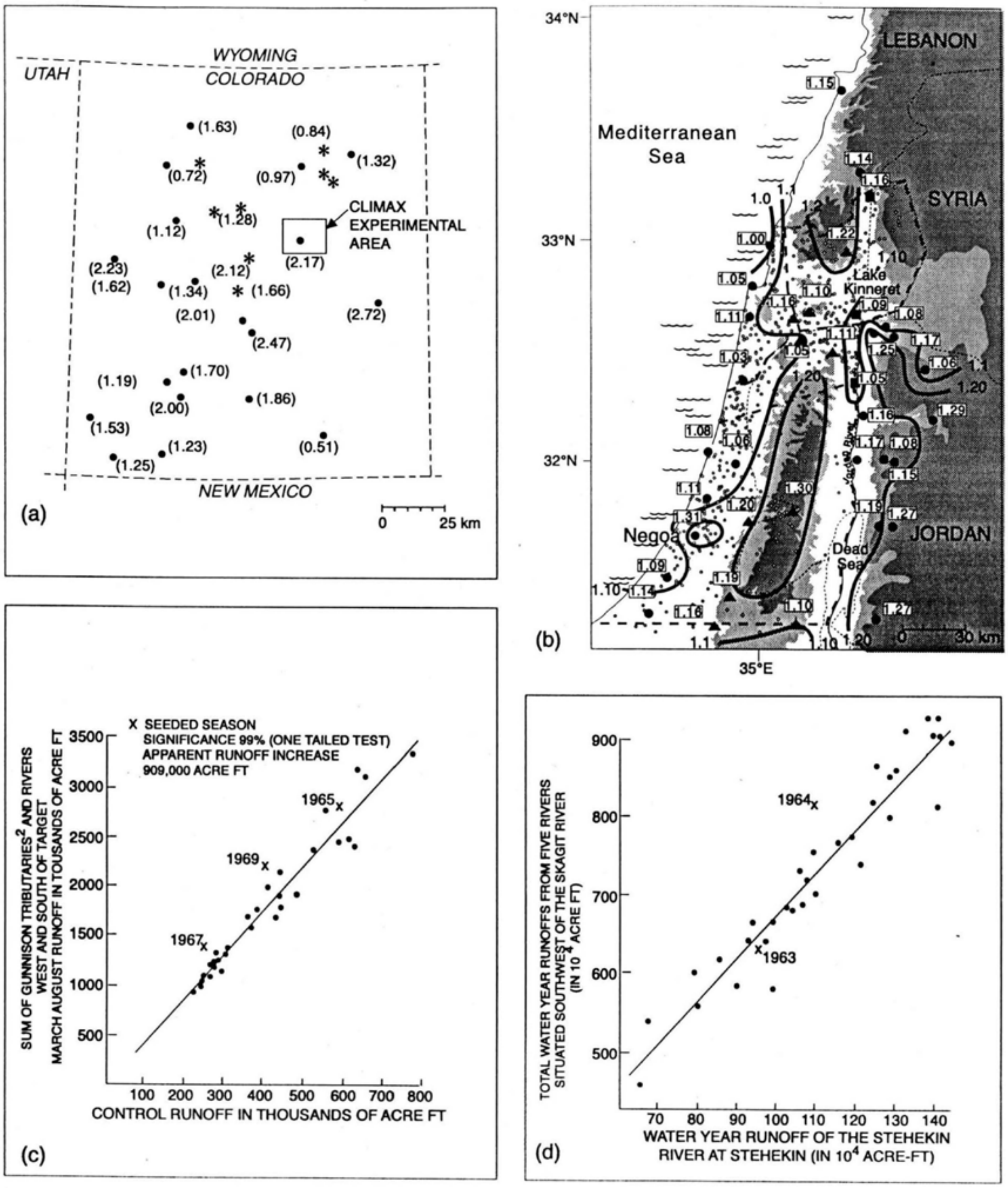
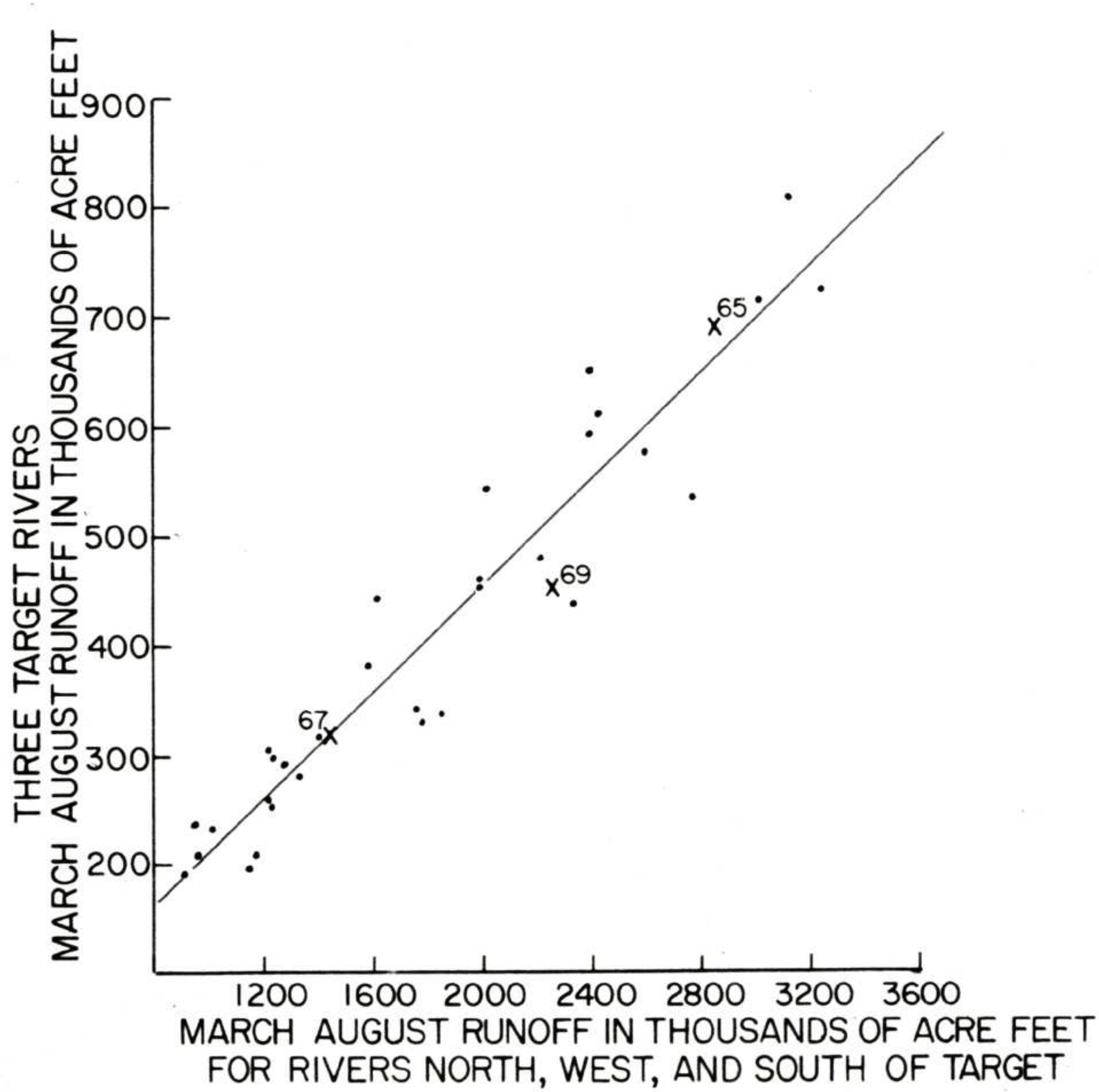


Figure 2



THREE TARGET RIVERS
MARCH AUGUST RUNOFF IN THOUSANDS OF ACRE FEET
900
800
700
600
500
400
300
200
1200
1600
2000
2400
2800
3200
3600
MARCH AUGUST RUNOFF IN THOUSANDS OF ACRE FEET
FOR RIVERS NORTH, WEST, AND SOUTH OF TARGET
67
69
65

Figure 3