On the Status and Needs of Cloud Seeding:

Viewpoint from a Reanalyst

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Abstract

An overview of cloud seeding is presented from the vantage point of one who has worked in many facets of this field for more than 30 years, including having reanalyzed a number of experiments. From these experiences, the physical obstacles that prevent clearcut successes from being achieved and the subjective obstacles that have repeatedly compromised the literature in cloud seeding are addressed. To solve the first of these obstacles, a "baseline" cloud seeding experiment is described that might clarify the some of the physical uncertainties associated with seeding clouds. It is also recommended that two cloud seeding experiments that had favorable first phases but were not replicated be resuscitated.

Some suggestions are offered that would likely minimize the "confirmation bias" that plagues the domain of weather modification and compromises its literature. Finally, some suggestions are offered that would improve the often contentious relationship between the academic community and the purveyors of cloud seeding services.

1. Introduction and background

Several overviews on the scientific status of cloud seeding and its needs have appeared in the in recent years (e.g., Bruintjes 1999, Silverman 2001, National Academy of Sciences 2003--hereafter, NAS 2003), List 2004, Garstang et al. 2005, and online, by Cotton (http// The sum of these viewpoints is that there may be periods when commercial seeding can be applied based on "evidence" of small positive cloud seeding effects in certain kinds of clouds, but the kind of proof demanded by scientists in the normal business of science, namely, that obtained in randomized and independently replicated experiments does not exist. However, to produce such proof given the variability of precipitation and clouds within the storms that are seeded, makes producing a large enough increase to be detectable and statistically significant (say 10% or more increase in precipitation) in an experiment of a few years or so virtually impossible.

On the other hand, only a 1% increase in precipitation from ground seeding can, in some cases, be viable from a cost-benefit standpoint as has been pointed out in past summaries of cloud seeding experiments (e.g., Neiburger 1969). Due to this minimal effect on precipitation, with no hope of proving it statistically, commercial cloud seeding often proceeds since it is believed by the sponsors and purveyors that decreasing precipitation due to cloud seeding is unlikely, though some recent evidence has been published that suggests otherwise.

The purpose of this article is to add to these assessments of the status of cloud seeding skill by providing the unique viewpoint on this field from the perspective of one who was a forecaster for a large randomized cloud seeding experiment (the Colorado River Basin Pilot Project, hereafter CRBPP), who has worked within the commercial cloud seeding community[2], and following these experiences in the early to mid-1970s, has been employed in academia where he has been concerned with airborne studies of clouds and the onset and concentrations of ice particles (Hobbs and Rangno 1985; 1990, 1998, Rangno and Hobbs 1988, 1991, 1994, 2001, 2005). Work in academia work has also included the study of ground and airborne releases of silver iodide in the Sierras (Rangno et al. 1977), in Utah, and operational and experimental dry ice cloud seeding (e.g., Hobbs et al. 1981). He was also the discovery of inadvertent cloud seeding by aircraft (Rangno and Hobbs 1983, 1984).

During the authors first job as a forecasting meteorologist in the CRBPP, he became aware of a "cultural norm" within the domain of cloud seeding that appeared to cause workers there to obfuscate and minimize major problems that impacted that particular experiment. The CRBPP was a massive randomized, state-of-the-art experiment which was intended to be an independent

[2] For North American Weather Consultants and Atmospherics, Inc.

replication of stunning results of several randomized experiments conducted in the Rockies during the 1960s (e.g., Grant and Mielke 1967, Mielke et al 1970, 1971, Morel-Seytoux and Saheli (1973). The CRBPP was designed by those same researchers.

However, when profound discrepancies were discovered in the assumptions and hypotheses of the original researchers during the CRBPP and in a prior experiment in the Park Range Project (Rhea et al. 1969), news of these important scientific developments was not passed along to the wider atmospheric community in the form of published papers, but rather remained within the cloud seeding community as "gray literature", voluminous "Final Reports" to project sponsors (usually the Bureau of Reclamation's Division of Atmospheric Water Resources Management) that did not give those discrepancies appropriate attention, if they were addressed at all. The author, going into the CRBPP, and perhaps naively, thought important discrepancies in the assumptions made by prior researchers would be brought to the attention of the outside atmospheric community in a timely manner. This did not happen. The lesson learned by the author during the CRBPP in the early and mid-1970s suggested to him that many peer-reviewed journal reports of cloud seeding successes were likely flawed no matter how highly regarded those reports might be by national panels, as were the experiments on which the CRBPP was based (e.g. Malone et al. 1973).

It was a fruitful perception. It led to a number of reanalysis efforts, enumerated below.

Inspired by the story of American physicist R. W. Wood concerning the "N-Ray" episode at the turn of the 20th (e.g., Broad and Wade 1982), and dubious of the clouds reports emanating from the experimenters, the author traveled to Israel to evaluate those clouds that had been described on numerous occasions in journals, but did not appear as described from a cursory review of easily available rawinsonde data when rain was falling from the National Climatic Data Center.

In addition to this foray and the reanalyses, the author has also published a number of comments on published cloud seeding results (e. g., Hobbs and Rangno 1979, Rangno and Hobbs 1980a.b, Rangno 2000).

The efforts by the author have been, for the most part, with the late Prof. Peter V. Hobbs[3], Hobbs (2001) expressed his own viewpoint on cloud seeding, making a special point of addressing the unreliability of this field's literature. Readers may find this assertion by Hobbs' surprising because the literature alluded to by Hobbs appeared in peer-reviewed journals, considered a barrier to the dissemination of unreliable findings. However, it is not the first time such an opinion on the cloud seeding literature has been offered (e.g., Byers 1965, Neryman 1980). The present article also addresses the problem of a peer-review process that has allowed unreliable findings to be published in the first place.

Because the author is of the opinion that vested interests and *a priori* viewpoints need to be "put on the table", especially in journal articles that concern cloud seeding, he points out that he has no monetary stake in whether seeding "works" or not; he is not employed in this field. It should also be pointed out that the author's reanalyses and commentaries on cloud seeding with one exception, were unpaid efforts done on his own time, a note that is necessary because it indicates perhaps an "undue interest" in the field that readers should be aware of.

In spite of the many negative aspects of the author's experiences in the cloud seeding domain from which the viewpoints below arise from, he nevertheless considers himself an optimist about the prospects of proving that seeding can produce useful results of precipitation in limited situations and suggests how that might be accomplished later in the article.

2. Confronting the unreliability factor in the cloud seeding literature; an author's journey.

The greatest omission in the recent overviews on the status of cloud seeding listed in the introduction is that they did not address the problem of the unreliability of reports of cloud seeding successes when they are scrutinized by independent workers. If there was ever a 900 lb. gorilla in the living room that is being ignored, this is it. This "unreliability" factor has plagued the domain of weather modification since the discovery of seeding with Dry Ice in 1946.

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[3] Peter V. Hobbs and the author were the recipient of an *Excellence in Weather Modification* Prize in 2005, part of a series of prizes awarded annually by the United Nation's World Meteorological Organization in conjunction with the United Arab Emirates.

This is not news.

Several long term observers of this field have made shocking observations about the cloud seeding literature over the years: Braham (1979) stated that the literature was viewed with “disdain and suspicion”. A prize-winning statistician, after more than 20 years of involvement in this field, concluded that the cloud seeding literature was “unreliable” (Neyman 1980). Hobbs (2001) who participated in numerous scientific studies of cloud seeding, on panels that assessed cloud seeding skill, also asserted in his final take on the field that the cloud seeding literature is “unreliable”. Most recently, List (2004) asks, “Why (weather modification) has received such a poor reputation?” The above assessments of the cloud seeding literature are remarkable because they were made by leading scientists, hardly “fringe” elements of the field!

What other field in atmospheric sciences, or any domain of science, would several long term observers describe a whole body of literature turned out by its specialists as “unreliable”? Can we imagine statements like “viewed with disdain and suspicion” being made about, say, paleoclimatology, studies of El Nino, or of synoptic meteorology?

This writer, after more than 30 years of his own involvement with this field, from both sides of the “seeding fence”, regrettably, agrees with the assessments of Braham, Neyman and Hobbs; the cloud seeding literature *per se* cannot be trusted on its face specifically when results have been reported by those who stand to gain from a reported successful seeding outcome.

It is hardly news to the overall scientific community that those who have vested interests in the positive outcome of an experiment are likely to report findings that benefit themselves as we have seen recently in the medical and pharmaceutical arenas (e.g., Kennedy, 2004). But we pretend to exist that these same economic forces do not distort the reporting of cloud seeding results. We cannot see the parallels between drug makers who might hide unfavorable drug test results and those that have hidden unfavorable results in the domain of cloud seeding. This ethical problem in our own domain should be “put on the table” and not swept under the rug any longer, no matter how embarrassing and difficult it is for us to address.

In particular, consider the author’s own journey through six apparently highly successful cloud seeding projects or randomized experiments, the Wolf Creek Pass, the Skagit, the Climax I and II, and the Israeli I and II, in that order, all lauded by various observers or national panels. In process of reanalyzing each one, reanalyses that began with the raw data, major flaws were discovered. None of these experiments were the successes they were initially reported to be. *En toto*, the descriptions of these six experiments and the pluperfect seedable clouds that made them so credible consumed hundreds of peer-reviewed journal pages.

This sequence of outcomes experienced by the author as a “serial reanalyst” is analogous to a quality control officer that tests the claim of a manufacturer of ping pong balls. The manufacturer claims that only one out of 50 had been inadvertently dented in the manufacturing process (corresponding approximately to the statistical significance claimed in the experiments reanalyzed by the author). The quality control officer then opens a sample of 50 ping pong balls in a shipment from this manufacturer and finds that the first six are flat on one side!

What does this say about the claim of the manufacturer and the likely quality of the ping pong balls in the rest of the shipment? The conclusion is obvious.

Similarly, what does the author’s results concerning the only six experiments, each having a high statistical significance that the results were not due to chance, say about the rest of the cloud seeding literature? That conclusion, too, is obvious.

The Cloud Seeding Culture “Code of Silence”

But how can these publications appear? These experiments involved numerous workers, authors and co-authors. They happen because there is a “Code of Silence” that operates to husband discrepancies within the culture rather than reporting them in a timely manner to the wider outside community. From the author’s experiences in the CRBPP disparities in underlying hypotheses that might cause an experiment to falter in the eyes of the outside atmospheric science community remain hidden from view within domain of cloud seeding workers. For example, following the Park Range experiment in the mid and late 1960s (Rhea et al. 1969) and in the first years of the CRBPP, it became apparent that there was a serious discrepancy in the hypothesis relating 500 hPa and cloud top temperatures in the Rockies. Experimenters in Colorado had claimed, in explaining why they appeared to have stunning statistical successes at Climax, Colorado, when the 500 hPa temperature was $\geq$-20 C (e.g., Grant and Mielke 1967; Mielke et al 1970, 1971) that there was a relationship between these two parameters that allowed them to infer the cloud top temperatures, and therefore, stratifying their statistical results by 500 hPa temperatures explained deficits in . that numerous researchers in the cloud seeding culture became aware of a major fault in the Climax hypotheses beginning in the late 1960s. The Climax experiment results were stratified by categories of 500 hPa temperature with the highest temperatures associated with large (50-100%) increases in daily precipitation at Climax that were statistically significant (e.g., Mielke et al. 1970). These 500 hPa temperatures, it was asserted by the experimenters, were close to cloud top temperatures (e.g. Grant et al. 1967). However, with the conduct of the Park Range Project in the mid and later 1960s, a project that unlike Climax incorporated numerous rawinsonde launches during storms, it became evident that 500 hPa temperatures were not associated much with cloud top temperatures (Rhea et al. 1969). During the CRBPP when rawinsondes were launched every three hours, this lack of a correspondence between cloud top temperatures and 500 hPa temperatures was again verified. However, word of this crucial flaw in the stratifications by the experimenters in explaining their statistical results did not reach the journals until 1979 (Rangno 1979; Hobbs and Rangno 1979) though it was reported internally to the BOR on several occasions (e.g., Rangno 1972). In fact, journal articles describing results of experiments in the Rockies continued to rely on these stratifications even when the authors knew there were serious problems with their assumptions that they did not address (e.g., Grant and Elliot 1974; Grant and Kahan 1974)!

Why? Is there a code of silence regarding "bad news" in the cloud seeding culture?

It would appear so, and the author was a part of that culture in the CRBPP.

From the author's vantage point of more than 30 years as an observer and participant in this field, the situation in the cloud seeding culture today may not be much different from the "mass delusion" that occurred in the N-Ray episode of the early 1900s described by Broad and Wade (1982). In the N-Ray episode, many French scientists continued to report detecting N-Rays, which do not exist, long after Rene Blondlot's N-Ray reports were discredited by an American physicist and skeptic, R. W. Wood, who traveled to France to see N-Rays for himself and exposed the delusion.[4]

Some science historians have attributed this "mass delusion" of N-Ray detection in France to human factors such as a tightly woven scientific culture within the French scientific community and French nationalism.

"Tightly woven scientific culture"? Sound familiar?

In analogy with the N-Ray episode, in the domain of cloud seeding we have a small cadre of workers, a "tightly woven culture" if you will, in academia, government, and in the private sector that perpetually "see" cloud seeding successes in one form or another, and provide reports on their perceived cloud seeding successes not only in our peer-reviewed journals but also in carefully crafted news releases for widespread public consumption from their academic or governmental institutions (e.g., by Desert Research Institute of the University of Nevada, and formerly, from the Bureau of Reclamation's Division of Atmospheric Water Resources Management).

Or, as CEOs of cloud seeding companies doing commercial projects, they merely assert directly to newspaper reporters how much they increased snow in the last storm, as one seeding purveyor has done in Colorado.

Like N-Rays, the increases in precipitation due to seeding that this small cadre of workers in this cloud seeding culture perpetually "see" have not been detected in our best randomized, confirmatory phase experiments. Thus, it is a good bet that the recent and continuing journal articles purporting cloud seeding increases in precipitation in various non-randomized commercial projects,

[4] The story of N-Rays combined with the rejection of a short paper in 1983 that asserted that the clouds of Israel were not being described accurately by the cloud seeding experimenters there, inspired the author's trip to Israel in 1986 to see the clouds for himself. The resulting paper was published in the *Quart. J. Roy. Meteor. Soc*. in 1988.

from orographic to summer cumulus cloud seedings with reported effects that last for hours, will *not* be verified in well-executed randomized experiments[5].

But this is a deliberate and provocative challenge to the WMA adherents: give us, the wider scientific community, and your customers, the evidence they deserve. No one will be happier than the author if you can do it!

Finally, and continuing to be very troubling to this author, is the cloud seeding culture's lack of reaction to the delayed reporting of critical results in two key sets of experiments, those in Colorado and in Israel. It could be said that this lack of reaction tells us even more about what kind of a "science" we are in this domain. The facts about the delayed reporting in the experiments in Colorado and in Israel are well known throughout the cloud seeding and academic community that specializes in the studies of weather modification. But perhaps in this domain we are so woeful as a "science" that no one in it is surprised or even concerned by these occurrences. Such revelations as the sudden appearance of half an experiment's previously omitted data 15 years after the experiment ended are treated by us as "business as usual"; there is no outcry, no indignation, no journal editorials lamenting delayed reporting, no explanations demanded as there would be the case in legitimate scientific endeavors (e.g., Kennedy 2003).

And there is no reason to believe that things have changed. We continue to fail to understand that to tolerate corruption is to be corrupted

Moreover, when the author looked behind the scenes at the two most highly acclaimed sets of randomized experiments, those in Colorado and Israel, he saw scientists, while reporting on their cloud seeding experiments in peer-reviewed journals, omit results, claim to have done things that they did not do, or could not have done, overlook favorable draws on seeded days that were region wide and could have been detected with but a cursory check, describe clouds that do not exist, hide results of new experiments that were not compatible with earlier reports, deny outside skeptical colleagues access to their radars for the purpose of checking suspect cloud reports (i.e., the author in 1986 in Israel). Finally, and with a high probability in one case ($p=0.001$), target gauge data were altered to benefit seeded days in a critical "confirmatory" experiment.

What is most astonishing about these acts is that they are only those ones that the author has encountered in his investigations of only six of the numerous journal- published cloud seeding experiments! Moreover, these six were not "backwater" experiments, but were widely deemed as our *best* efforts and had been validated by numerous other experts in the field (e.g., National Academy of Sciences 1973, Mason 1980, 1982, Kerr 1982, Dennis 1989).

How ironic and telling these experiences are about the murky scientific domain of cloud seeding!

Otherwise decent, well-intentioned scientists were seduced into "doing something" about the weather and droughts, in particular, no doubt wanting to alleviate human suffering. Unfortunately, this desire caused them to lose sight of their scientific training and want to do something about their data as well.

However, the darker thought that these acts could also have been about "career enhancement" cannot be discarded. Kennedy (2003) observed that *every* proven case of scientific misconduct in medicine, biology and physics had a *single* cause: the desire for career enhancement. And in our cloud seeding domain, no one ever got a job by saying that cloud seeding doesn't work to a local water board! It is probably equally true that additional funding from more demanding organizations such as the National Science Foundation may hinge on a seeding success having been "found" in a preliminary experiment.

How can we eliminate the pathology that repeatedly leads to faulty claims about cloud seeding successes being published in the first place?

From the author's unique experience as a "serial reanalyst", some practical rules to insure that what is published is as close to the truth as we can achieve are:

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[5] The difficulty of seeding cumulus clouds was well understood by J. Simpson in Sax et al. (1975) who wrote that "cumulus clouds can do anything." In this regard, readers should try "mentally seeding" cumulus clouds on the next convective day. You'll be amazed at the "effects" that you can sometimes have!

The results of a cloud seeding experiment should not be considered reliable if it has been analyzed solely by those who conducted it, or by others who have vested commercial interests in cloud seeding. Independent validation by those who are not dependent on cloud seeding funding is critical for maximum credibility.

Regional evaluations of experimental days must be made mandatory when cloud seeding results are reported. No cloud seeding experiment should be considered reliable if the control rain gage stations or other covariates were not selected before the first day of the experiment.

Those who have knowledge of seeding activities should *never* measure the precipitation in an experiment, or evaluate radar imagery.

We should require notices of conflict of interest or other vested interests on the part of those reporting the results of cloud seeding.

We should also work to *mandate* randomization of commercial seeding projects via state or federal laws.

And we should also require "full disclosure" to customers when seeding services are sold to them.

Garstang et al. (2005) noted the paradox of decreased funding from federal sources for cloud seeding studies such as through the National Science Foundation while the funding of cloud seeding activities by local governments has ballooned.

Why does such a paradox exist? The proposals for cloud seeding research funding must go through peer review; proposals for cloud seeding services likely are only seen by local governments and water boards, and perhaps due to a lack of expertise in the subject, more likely to fund cloud seeding activities, which can also be self-serving for sponsors; they appear to their less informed constituents to be solving a problem.

We should try to convince our commercial seeding purveyors and institutions like DRI that "full disclosure" such as laying out the full debate between the NAS 2003 and the WMA 2004 to their customers so that we know that they have access to all the information as is done in "Voter's Pamphlets."

This last point may well be the one in which the wider academic community and the WMA adherents have the most friction. One purveyor within the seeding culture, when he plied his wares to a local water, like a used car dealer who rolls back the mileage on his cars, "rolled back" the published literature on cloud seeding 20-30 years and presented outdated, discredited literature to convince his customer that "cloud seeding works."

It was a highly successful, though not laudable maneuver by that cloud seeding company; it obtained a large seeding contract from the Colorado Water Conservation Board that year.

Examples of biased, one-sided presentations can be found, too, even at academic institutions which are supposed to be bastions of objectivity. One can compare the comprehensive summary of cloud seeding and the difficulties and challenges in that domain prepared by Cotton and Pielke online at Colorado State University, a university long involved in cloud seeding and ice nucleation studies, with the limited, "optimistic" presentation of cloud seeding facts offered by the Desert Research Institute (DRI) of the University of Nevada. DRI, which is also engaged in a state commercial-style cloud seeding program in Nevada, also produces a one-sided newsletter on its cloud seeding activities. We need to stop this kind of "PR" and be honest with who we are selling cloud seeding services to by giving them all the facts.

It may be that since "doing something" is better than doing nothing, that programs like the State of Nevada's would continue anyway. Nevertheless, everyone would be a bit happier about it if there was "full disclosure."

Since List (2004) in his view of what the field of cloud seeding needs dedicated his article to the memory of K. Ruben Gabriel, the Israeli statistician who reported on those cloud seeding experiments on several occasions, it is appropriate to recall Gabriel's own take on the "human problem" within the cloud seeding culture in 1967, a viewpoint that was echoed by *Science* editor Donald Kennedy (2004) 37 years later in an editorial entitled, "The Old File Drawer Problem." Gabriel wrote:

"An interesting speculation (Kruskal, 1965, personal communication) is that experiments tend to be discontinued after a few years of apparently poor results. Experiments with 'unsuccessful' results in the first season or two may often not be reported at all. As a result, the experiments whose results are published would be those with initial 'successes' which are usually followed, sooner or later, by less 'successful' seasons. This could account for the apparent downward "trend" among published experiments."

In a sad irony, the discussion by Gabriel of the omission and delayed reporting syndrome in the cloud seeding domain became "prophesy" for the Israeli II and III experiments (e.g., Gabriel and Rosenfeld 1990, Rangno and Hobbs 1995) when the experimenters omitted half of their results for the second experiment, and neglected to report the indications of decreased rain due to seeding in the third experiment until it had reached the 15 year mark!

To return to the rhetorical question that List (2004) posed, is it any wonder then, "…that weather modification has gone downhill and why it has received such a bad reputation?"

2. A solution to the seeding dilemma.

There is a way out of the "seeding wilderness." The complexity of the processes that lead to natural precipitation (e.g., Braham 1968) is well known and are now largely quantified in our best numerical models. To avoid many of these complexities, the execution of the simplest possible cloud seeding experiment is called for, one in which only naturally non-precipitating, low-based supercooled clouds are targeted for seeding. Chappell (1967) put it well in his Master's Thesis concerning the early results of the Climax, Colorado, cloud seeding experiments: "seed clouds, not precipitation."

No greater wisdom has been expressed concerning how the current cloud seeding brouhaha we face today could be resolved.

But no one has produced a dataset that can tell us how frequently low-based non-precipitating clouds having, say, a minimum depth of at least 0.5 to 1 km occur. Or what their duration, or their horizontal dimensions might be. Such clouds will certainly be relatively shallow, and their limited depths *might* be an impediment to obtaining economically viable amounts of precipitation.

However, when such clouds are cataloged, they offer the critical advantage that when they are seeded, the precipitation that reaches the ground will be solely that due to seeding. Furthermore, the characteristics of plume dispersion from ground or airborne releases in such an experiment could be evaluated, including achieving answers to the questions of whether clouds are overseeded, underseeded, and targeting accuracy in ground and airborne releases.

Figure 1 is an example of what a seeding window might look like for non-precipitating supercooled clouds, in this case, cumulus clouds (non-severe ones). This particular diagram is for the onset of ice particles as determined by Rangno and Hobbs (1988, 1995) from their own cloud studies and from a number of field programs around the world. To the left of the solid line (the hatched region) is that domain of cloud tops that would be most likely to be non-precipitating due to the formation of ice crystals. A similar diagram could be obtained for cumulus clouds for hygroscopic seeding purposes.

Naturally, the surest, though most expensive way to evaluate the effectiveness of seeding in stratiform supercooled, non-precipitating clouds is by airborne releases of seeding material. In these cases, the air crew, with a modicum of onboard measurements, can determine the distance upwind to release the seeding agent so that the precipitation induced reaches the ground at the target. Furthermore, from such an airborne platform, the time of arrival of the seeding plume can be predicted as was done in Hobbs et al. (1981) and later by Humphries (1984). In this scenario, a large number of experimental cases could be built up in a short time when, presumably, layer clouds in particular are relatively constant in dimension and microstructure. However, testing the efficacy of ground seeding, the method that is used in most commonly in commercial cloud seeding operations, would be a mandatory component to see whether this method has been viable.

As a start on the suggested "baseline" cloud seeding experiment described above, we should begin by establishing the climatology of non-precipitating, supercooled clouds, their depths, and their base heights above viable seeding targets, and their horizontal dimensions, the latter critical for targeting; a supercooled cloud overhead of a target is useless unless it extends appreciably upwind. It may be that such datasets can be gleaned from such long-lived past projects such as the Sierra Cooperative Pilot Project or in the Colorado Orographic Seeding Experiment, presuming the data were not discarded.

Resuscitating Promising Exploratory Phase Randomized Cloud Seeding Experiments

The Lake Almanor and Bridger Range randomized cloud seeding experiments should be resuscitated, each having promising exploratory phases, and confirmatory phases carried out. This would also be a second strong component of re-establishing scientific credibility in the cloud seeding domain.

Mooney and Lunn (1969) reported on the first phase of the Lake Almanor cloud seeding experiment in the northern California Sierra mountain range. Results suggested that one particular partition, "cold westerly" had produced large increases in precipitation. A second "confirmatory" phase was carried out, but the results were not published in the peer-reviewed literature, but rather in a preprint. Strangely, the results of the 3 year experiment for the "cold westerly" category were not mentioned. Nevertheless, this experiment was sufficiently promising, even though it took place in an environment where high natural ice particle concentrations would be expected. Seeding operations would be carried out exactly as they had been in the first Lake Almanor experiment, but the evaluations and the measurement of precipitation would, of course, be done independently.

The Bridger Range Project (Super and Heimbach 1983) in Montana also falls into the same promising category as the Lake Almanor experiment; one that is suggestive-of-success but was not followed up by an independent replication. A confirmatory phase should be carried out for this project as well with the same stipulations as above.

The extreme advantage that we have today over previous eras in carrying out such programs is that we have had vast improvements in our abilities to predict the natural precipitation via numerical models. Following the lead of Colorado State University scientists (Cotton, private communication, 2005), models such as RAMS should be invoked in the evaluations of these experiments if they are resumed.

Commercial seeding operations should transition to randomized experiments as conditions allow.

We should mandate the randomization of commercial projects. We would not be having the continuing, sporadic brouhahas over cloud seeding if randomization of these projects via state or federal laws had occurred 20-50 years ago when some of the same clients as today began hiring commercial seeding operations (e.g., Elliott 1976). And it is a good bet that such seeding activities are likely to continue for the same locations for another 20-50 years as long as we have no definitive answers on whether it is doing anything at all. In extreme drought situations, perhaps randomization can be dropped. But in routine water supply situations, randomization is essential.

Mandate the archiving of datasets associated with cloud seeding activities

We also need to mandate the retention of datasets and documentation concerning cloud seeding experiments and commercial operations. In most cases, this is already done. However, there are discouraging exceptions. For example, a dense network of precipitation gauges was established in support of the SCPP across the Sierra Nevada mountain range along Interstate 80 over Donner Pass and it was maintained for many years in the 1970s and 1980s. However, that data, archived on "round tapes" was not saved to newer data formats, but discarded (D. Reynolds, private communication) ! Data from the Lake Almanor experiment (Mooney and Lunn 1967) was also discarded (B. Marler, private communication, 2004). We could have learned more from these two projects.

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FIGURE CAPTION

Figure. The relationship between the characteristic cloud top temperature at which ice appears in concentrations of of about one per liter in continental cumulus clouds and cloud base temperature (after Rangno and Hobbs 1995). The hatched region is that in which non-precipitating clouds with supercooled tops would be expected.

It is not debatable that faulty reports of cloud seeding successes have had large negative economic impacts. Would the Bureau of Reclamation (BOR) have undertaken the multi-million dollar, high-tech randomized cloud seeding experiment, the Colorado River Basin Pilot Project-CRBPP) in 1969-70 had word of a lucky draw in Climax I and in the early seasons of the Wolf Creek Pass experiments been reported in a timely manner? Or would the BOR have pursued more basic research to understand clouds and precipitation instead?

The CRBPP was doomed to failure, and only belatedly in 1974 and 1975 did the BOR contract for airborne studies of the clouds they were seeding (e.g., Hobbs et al. 1975, Marwitz et al. 1976).

Would the NAS 1973 Panel on Weather Modification have placed their seal of approval on the Climax and Wolf Creek Pass experiments, an approval that likely spurred commercial seeding projects for many years thereafter in the Rockies had word of ?

Not likely.

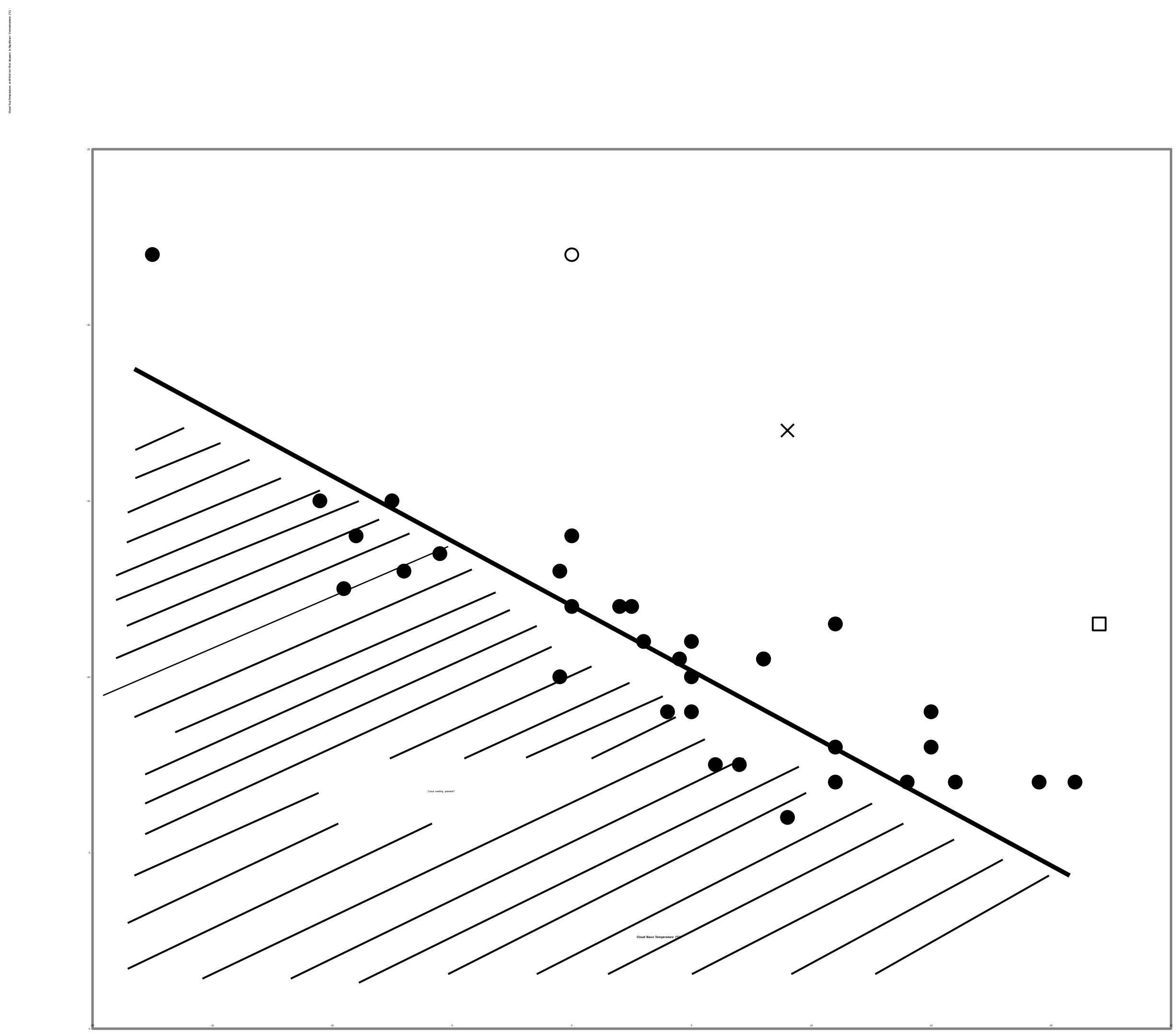
Had the NAS 1973 gotten word that the stratifications of seeding effects in the Climax experiments were based on a faulty concept about the height of cloud tops, would they have written so highly of the Climax experiments?

Not likely

Would the government of Israel have begun operational seeding of the northern portion of Israel beginning with the 1975-76 rainy season had the full, null result of the second randomized experiment been reported in a timely manner? Would Kerr (1982) have described these experiments, the "One Success in 35 Years"? Would nearby Arab states have undertaken cloud seeding?

Not likely.

Thus, delays in reporting or finding "lucky draws" by the cloud seeders themselves have likely had enormous negative economic consequences, except of course, on the seeders themselves who benefited from their oversights, "errors" and omissions.


Cloud Top Temperature at Which Ice First Appears in Significant Concentrations (°C)
Cloud seeding potential?
Cloud Base Temperature (°C)
-5
0
5