

Dear Mr. Rangno,

We have received the reviews of your manuscript, and an editorial decision has been reached. This manuscript is rejected for publication in the *Bulletin of the AMS*. The decision details and the reviewer's comments appear below. One reviewer recommended outright rejection; a second reviewer noted the harsh tone and need for more objectivity.

BAMS thinks that since the issues involved are too contentious and are not yet settled, the use of the terms history and lessons for the future are not applicable.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,  
James R. Fleming  
Editor  
*Bulletin of the American Meteorological Society*

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AUTHOR'S RESPONSE TO THE REJECTION OF BAMS-D-18-0260,

“The Rise and Fall of Cloud Seeding in Israel:  
A History with Lessons for the Future”

First, I thank the Special Editor (hereafter, “SE”, and the reviewers, Dr. Rosenfeld, hereafter DR, and the anonymous Reviewer 2, hereafter R2, for spending valuable time reviewing BAMS-D-18-0260 (hereafter, B-18).

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From a former BAMS organizational issue:

*“Bulletin of the American Meteorological Society*  
(BAMS) publishes papers on historical and scientific topics that are of general interest to the AMS membership. **It also publishes papers in areas of current scientific controversy and debate, as well as review articles.”**

Dealing with controversy in a public way, as in BAMS, is good for science.

The BAMS leadership over many decades understood this as is demonstrated in the organizational quote above. This helps the BAMS readership make decisions on controversial topics. Ironically, the only contentiousness in B-18 is in the conclusions and reflection sections. All text prior to those segments is an historical account of the reporting and of the various interpretations of seeding; by those who have promulgated seeding on their countrymen for decades, and by the external skeptics who came to question those claims.

Contentious issues are still are a hallmark of BAMS. Examples: BAMS published an article on the highly contentious issue of geoengineering in November 2018. Geoengineering does not represent “settled” science, but rather as far from it as can possibly be imagined.

Cloud seeding has been the topic of several BAMS articles over the past 15 years, another “unsettled” topic and one that engenders controversy (e.g., Garstang et al. 2005).

Rangno and Hobbs (1995) was a classic example of the power of publishing in a controversial domain. Seeding partisans, D. Rosenfeld (DR) (who recommended rejecting RH95, of course), W. Woodley, A. S. Dennis and H. Orville all weighed in afterwards (1997) with “Comments.” We responded convincingly. How convincingly?

The Israel National Water Authority (INWA) after reading RH95, and the ensuing exchanges in 1997, decided to hire an independent panel to see what they had been getting from their long-term operational seeding program promulgated by seeding partisans<sup>1</sup>. The independent panel (Kessler et al. 2006) formed by the INWA years earlier, could find no viable result from operational cloud seeding, a conclusion corroborated by Levin et al. (2010).

Should we be surprised by these findings? I think the SE would answer, “no.”

The seeding program was terminated Sharon et al. (2008) who distilled Kessler et al.’s 2006 report. The squalls that followed, with claims of air pollution having suddenly canceled out seeding effects, were unconvincing to the INWA. They did not restart the operational program due to those claims.

Imagine the loss if reviewer DR had succeeded in having RH95 rejected!

Or that RH95 did not have as our Special Editor, Randall Koenig, who was experienced in cloud microstructure and cloud seeding directly and who could see the specious elements in DR’s commentary (as did the INWA).

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<sup>1</sup> Y. Goldreich, 2018, personal communication. Prof. Goldreich, Bar-Ilan University, is the author of “*The Climate of Israel*”, 2003, which contains a chapter on cloud seeding.

We know that B-18 contains dark elements; the falsification of the Israel-2 experiment via the omission of half of the experimental data to make the experiment look more successful than it really was (the very definition of “falsification”). This poses, of course, a knotty discussion problem in B-18 which I tried to step around without using the word, “misconduct” even if we all know that’s what it was. (See also Gabriel and Rosenfeld’s 1990 delicate discussion of this issue as they introduce us to the withheld results of Israel-2.)

B-18 was not written from the vantage point of a scientist wanting to protect certain individuals/institutions and even science itself from the shadow cast by misconduct (which the reader might be led to in B-18). It was written to tell a science story exactly as it happened, presenting both sides of the arguments fairly, and in the end, reflect on it and how can it be avoided in the future.

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Is BAMS truly of the belief that we can learn nothing from this chapter of cloud seeding in Israel? It would seem so in the SE’s mind, and it does not seem defensible.

And *should we* ignore the misconduct that took place in the reporting of Israel-2? Kornfeld and Titus (2016, *Nature Geoscience* argue, “no” emphatically in their editorial, “Stop Ignoring Misconduct.” Ben-Yehuda and Oliver-Lumerman (2017) offer similar advice in their comprehensive examination of “*Fraud and Misconduct in Research*”<sup>2</sup>.

Will BAMS defer?

What makes this story relevant today, you might ask, since the original experimenters have passed?

The protégé of the experimenters who falsified Israel-2, reviewer DR, was there in 1981 when it first occurred in our peer-reviewed literature. He continues to work for the HUJ in the cloud seeding domain. This makes B-18 more critical for publication. We hope when B-18 is published that DR can offer his “inside” view and why he didn’t come forward in a timely manner. Instead he left it to the Israeli experiments’ “chief meteorologist”, Mr. Karl Rosner, to come out publicly upon his retirement in late 1985 or early 1986 to have the withheld results of the south target of Israel-2 published<sup>3</sup>.

Today, some of those cloud seeding zealots of the kind Dr. Fleming wrote about in *Fixing the Sky*, masquerade under the cloak of academia. We need better protections from them and publishing the Israeli story will help do that.

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<sup>2</sup> University of Michigan Press.

<sup>3</sup> The original experimenters continued to refuse Mr. Rosner’s demands, leaving it to Gabriel and Rosenfeld (1990) to inform us of the full results of Israel-2 after the lead experimenter passed, results of Israel-2 that led to so many questions about seeding efficacy in Israel and the microstructure of the Israeli clouds.

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On the SE's email notification to me of his decision to reject B-18

I did not read the reviews forwarded by the SE until many days after that e-mail arrived due to its negative descriptions of the two reviews (see above). I figured B-18 was "2 and BBQ" (the manuscript), a phrase pinched from double elimination baseball and softball tournaments when a team loses its first two games and then has a team picnic instead of playing.

But when I read the reviews, and one of the two was "accept", "minor revisions", "important paper", it was *not* "2 and BBQ" as I supposed! I was surprised that the SE didn't mention the diversity of the reviews in his e-mail, which then made it look, if unintentionally, hostile after learning about the split.

In the big picture, as I see it, the SE and myself as on the same team; outing and writing about checkered findings in the cloud seeding domain. I have reanalyzed six cloud seeding experiments (none were what they were reported to be by the original experimenters), commented on suspect findings in the literature. Dr. Fleming has described the checkered early history of a field rife with confirmation bias and misconduct in his marvelous book, *Fixing the Sky*.

In both venues, mine and his, the public has been shown to be the loser.  
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My understanding from two other former journal chief editors is that in evenly split decisions such as with B-18, it is unusual to result in an outright, permanent rejection as the SE has communicated to me. More commonly, the response is, "accept only upon satisfactory revision", or words to that effect. See Schultz (2009), *Eloquent Science*, on the role of editors and their decision-making processes.

Therefore, I have a major concern with the SE's decision and "BAMS" (?) assertion that B-18 is "too contentious" and the issues discussed, as fully as I do, are "not settled."

There are no definitions of "too contentious", and "not settled." These are subjective terms and there were no specific areas of the text indicated where that was thought to be the case.

However, the original experimenters (GN76, GN81, G81, and the several peer-reviewed journal articles thereafter) "*earned*" harshness when they did not report all the results for Israel-2. In doing so, they misled not only the scientific community, but also their own people. *Surely* you agree.

Nevertheless, on behalf of R2, I have removed some sentences/phrasing or revised them. If R2 is an Israeli scientist, it is doubtful I have removed enough, however.  
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“BAMS” (?) asserts the seeding issue in Israel is “not settled.”

Let me correct that impression. The INWA, the funder and final arbiter of cloud seeding in Israel, “settled” the issue when it terminated operational cloud seeding, abandoning any thoughts that the three prior Israeli experiments nor the decades-long operational cloud seeding program had produced viable results, or that pollution had canceled seeding increases out, as argued by the seeding promulgators.

The terminus of operational seeding marked a clear, settled point. The INWA’s action, and lack of restarting operational seeding, was based on three independent re-analyses of the HUU’s published cloud seeding work.

Do we really need more independent re-analyses of the HUU seeding claims?

Furthermore, the INWA has moved on with a new randomized experiment whose results have not yet been reported after six years of experimentation. The new experiment, Israel-4, is *randomized* because it is not known whether cloud seeding will be a viable way for Israel to add the water it needs. Carrying out a randomized experiment is more *de facto* proof that the prior seeding results have been deemed unreliable and have been “settled.” The INWA is starting over.

Why isn’t this clear to “BAMS”?

You have to keep in my that it is virtually impossible for researchers at the HUU to admit that their faulty cloud work and misconduct cost their country millions in wasted seeding efforts, and they will fight to keep manuscripts like B-18 from ever reaching their scientific peers or their countrymen.

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I have lost confidence that B-18 has been reviewed even-handedly.

While I deeply admire Dr. Fleming’s work describing early cloud seeding efforts, B-18 needs a more knowledgeable editor concerning the clouds and cloud seeding in Israel. And it certainly needs one or more new reviewers to break a deadlock between an “accept, minor revisions” recommendation, and a “reject” recommendation by a conflicted reviewer who has promulgated seeding to his countrymen and elsewhere in the world for decades.

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“Conflicts of Interest”:

“There should be no conflict of interest nor the **appearance** of conflict of interest” in making decisions concerning publication of scientific articles.

There are two elements.

- 1) Perhaps DR's conflict of interest seeding baggage was not known to "BAMS" or the SE? Though that would surprise, I must ask. The reviews of those who are conflicted, have vested interests in a topic, can't be considered to produce candid or reliable reviews, at least until a response from an author to a conflicted reviewer's comments are studied carefully.

For whatever reason, I was not allowed by BAMS to respond even to the conflicted reviewer, DR, suggesting an agenda.

- 2) The SE is an alumnus of Colorado State University and was there during the time the benchmark Climax randomized experiments were being reported in the literature. Those experiments are alluded to in B-18. I addressed (generally with Prof. Peter V. Hobbs) various ersatz CSU seeding results and claims regarding Climax and the Wolf Creek Pass experiments in the literature on several occasions for many years as the original experimenters kept re-packaging the same ersatz claims. What was I supposed to do? Ignore claims I knew were bogus? I think someone not having a background with CSU would be more appropriate (this in spite of my respect for his book!)
- 3) Moreover, in 1983 I asked the AMS (via D. Landrigan), the NAS, and internally, CSU, to investigate the claim by Mielke in 1979 that the CSU experimenters had only discovered the Type I error in the Climax experiments "very recently." (Mielke 1979, *Commun. Stat.*)

In fact, the CSU experimenters were carrying out the very same study Mielke described in 1979 in **June 1969!** I have respect for Dr. Mielke; he likely forgot.

The former time was when large contracts were being signed in support of the Colorado River Basin Pilot Project based on the CSU seeding work<sup>4</sup>. All would have been lost with the discovery of a Type I statistical error in 1969 regarding the Climax I experiment.

Professor Lewis O. Grant in a conference meeting shortly thereafter informed his audience of this investigation, which then made it public, one I hoped would be carried out behind the scenes as I wrote to Dr. Landrigan. Both Prof. Grant and Prof. Mielke were two of the most beloved figures on the cloud seeding scene in those days; their work highly acclaimed by the NRC-NAS (1973) as having proved orographic cloud seeding. Few at CSU are fond of my work with Prof. Hobbs in the CSU domain for this and for published reasons!

If the SE is conflicted, or "BAMS" is conflicted over these elements, it would explain his terse, incomplete email to me that contained claims that were inaccurate about B-18 such as "not settled."

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Why is this important today if the original experimenters have passed?

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<sup>4</sup> Please see the acknowledgements in Mielke et al. (1970).

The Israeli experimenters' protégé, DR, seems to have learned well from them about omitting critical data related to cloud seeding.

In his review of B-18 DR states that the HUJ has “never been able to measure ice particle concentrations.” Indeed, the HUJ researchers have not reported ice particle concentrations in their many research flights since 1990 except in a single “modal” value in newly risen Cumulus tops by Freud et al. 2015.

Having spent 11 weeks in Israel chasing storms as an experienced researcher in airborne cloud studies, I can tell you why ice particle concentrations are hidden from us by DR; they're really high in the natural clouds of Israel on a routine basis!

This does not bode well for cloud seeding operations. We must thank Levin et al. (1996) for publishing a few cloud top vs. ice particle concentrations from HUJ flights that otherwise we would not have seen.

Dr. Duncan Axisa, of DMT (and former president of the Weather Modification Association) concerning DR's claim:

“They (the HUJ) could have reported accurate ice particle concentrations from our probe if they had wanted to<sup>5</sup>.”

Please correspond with Droplet Measurement Technologies regarding the 2-D probe that was purchased by the HUJ, and are using on their research aircraft.

<http://www.dropletmeasurement.com/>

DR's untruthful assertion about ice particle concentrations was either not read by the SE, or he was not aware that it **was** untruthful. This is one reason why I believe the SE should recuse himself in favor of a more knowledgeable SE who will recognize this and other disingenuous aspects of DR's review that were not recognized by the current SE.

Moreover, to reject B-18 on the ersatz basis of “too harsh”, contentious, not settled, “needs more objectivity” (all false) would be to short-change those who came forward to “out” the Israel-2 omission; Mr. Karl Rosner, the Israeli experiments, Chief Meteorologist”; those who re-analyzed the experiments (RH95, Levin et al. 2010), those who overturned the faulty cloud reports (R88; Levin on several occasions), and who evaluated the inadequate airborne seeding method used in the Israel experiments (RH95; Levin et al. 1997).

For these many reasons above, I recommend as reviewers, or to act as a replacement (s) for the current SE, Professor Bart Geerts, Chair, Atmos. Sci., U. of Wyoming, Dr. Roelof Bruintjes, of NCAR, or Dr. Robert Rauber. U of Illinois, Champagne-Urbana.

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<sup>5</sup> 2018, private communication.

You will get an objective review/decision on B-18 from these sources, one that I can live with. The first two have seen an early version of B-18; all three are familiar with cloud microphysics and cloud seeding; the first two, with the Israeli cloud seeding work.

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Believing that there is a valid window of acceptance of B-18, I have journeyed on with comprehensive responses to the reviewers (in a separate document) believing it's the right thing to do to examine those responses, that "BAMS" will see the light and that it's in their best interests to do so.

I recommend, too, that BAMS publish the reviews and my responses to them as "supplemental material", a procedure being adopted by the European Geophysical Union with one aim being the reduction of disingenuous reviews by partisans. BAMS readers will appreciate this new "openness."

Too, I would look forward to DR's "Comments" following publication of B-18, or even alongside it. My hope would be that he makes the same kinds of statements he makes in attempting to deflect this account from publication. He will degrade the HUJ if he does, but we will also know who he is at this point.

Debate is a critical part of science, as the BAMS organizational quote demonstrates at the outset of this rebuttal.

Let the revised B-18 proceed to publication in BAMS; let the debate begin.

Let DR claim to the world that the HUJ researchers have never been able to measure ice particle concentrations, thus degrading his great university.

Let B-18 be among the more than 60 prior articles in BAMS about cloud seeding, including reviews.

Please follow the EGU and publish the reviews and my responses to them! This would be an example of the very definition of "openness" in science.

Sincerely, and respectfully,

Art L. R.

-----some additional personal background-----

In submitting the manuscript describing the results of my 1986 Israel cloud investigation to the *Quarterly Journal of the R. M. S.*, Prof. Peter Hobbs and I both knew that it could not be published in an AMS journal. The likely reviewers of it had heard in conference proceedings or read in peer-reviewed journals about the ultra-ripe-for-seeding clouds in Israel on too many occasions. The obvious question for reviewers would then be, "How could they not know about what I was reporting?" This was primarily the reason my Note-sized paper on the Israeli experiments asserting for one thing that the clouds were

not being described accurately by the experimenters was rejected in 1983<sup>6</sup>; three of the four reviewers felt there was too much published contrary evidence<sup>7</sup>.

The findings in the QJ paper (R88) were confirmed in aircraft observations a few years later, and most recently by Freud et al. 2015 (*Atmos. Res.*) when the latter described the high precipitation efficiency of Israeli clouds that they had just discovered.

In 1986, I literally functioned in my altruistic effort (“own time; own dime”)<sup>8</sup> as one of the many proposed outside research aircraft missions that couldn’t get in (G. Vali, 1986, personal correspondence). Sir B. J. Mason (via Peter Hobbs) and Peter Hobbs: “No one’s been able to get an aircraft in there.” I was there because I was pretty sure the people of Israel were paying for cloud seeding that was not likely to be successful based on the view of those clouds I had gained in formulating that 1983 paper.

There is no satisfactory explanation for experimenters not knowing these clouds, and not allowing outside research aircraft in. One former seeding pilot I spoke with at Sade Dov airport got angry when I told him scientists thought you couldn’t fly research in Israel. That pilot now flew tourists over Masada on a regular basis.

You will also have to decide for yourselves about why the HJ experimenters couldn’t detect (or didn’t report) what I found in R88 with all the tools that they had. In the darkest view, it was consistent with a pattern of omission of “negative” findings that would have undercut the fame the Israeli experiments were to achieve, and of those who conducted them.

Only Reviewer DR, whose work with radar and satellite data began in the late 1970s, can tell us the full story. B-18 will raise uncomfortable questions for him.

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<sup>6</sup> Hobbs was on sabbatical that year.

<sup>7</sup> A. Gagin was one of the three of four reviewers the rejected that 1983 note.

<sup>8</sup> I had no affiliation in 1986 through December of 1987, having quit the job I loved at the end of 1985 in protest over authorship/credit issues. Peter Hobbs and I, in one of the great science reconciliations of our time, reconciled our differences when I ran out of my savings and begged for my job back.