

1
2 Responses to the comments of Dr. Daniel Rosenfeld (hereafter, DR) concerning
3 BAMS-D-18-0260:

4
5 Overview

6
7 It is this writer's conviction that adversarial reviewers do the best job of finding
8 faults in a paper. That's why I recommended DR as my first reviewer.

9
10 And he did exactly as expected, including his recommendation to reject this
11 manuscript. Its what adversaries do, and as he had in 1994 for the influential
12 Rangno and Hobbs (1995) reanalysis of the Israeli experiments.

13
14 DR makes several useful comments, and the ms has been adjusted to take those
15 into account. For example, ice particle concentrations reported by Levin might
16 be too high due to shattering artifacts as DR suggests. Those in Figure 3 have
17 therefore been reduced by half, more than artifacts could have reasonably
18 contributed to Levin's concentrations. It was stated that the dust hypothesis was
19 "abandoned" and DR indicated that that is not the case and the word has been
20 removed.

21
22 We also take note of DR's heavy conflict of interest, since he and his colleagues
23 at the HUJ have promulgated cloud seeding on his government for decades¹.

24
25 An example of a conflicted, statement by DR designed to prevent the scientific
26 community from learning about the high concentrations of ice particles in Israeli
27 clouds, ones that they have been flying in intermittently since 1990:

28
29 **"For lack of suitable instruments, it was never possible to establish the**
30 **concentrations of ice crystals in Israel clouds."**

31
32 DR claims in this review² that the modern 2-D probe they've used recently, one
33 manufactured by Droplet Measurement Technologies, Inc., cannot measure ice
34 particle concentrations accurately.

35
36 I contacted the manufacturer of the modern 2-DC probe used in Freud et al.
37 2015, Droplet Measurement Technologies (DMT), about DR's claim that it
38 produces "uncertain" ice particle concentrations (here in this review and
39 previously in Freud et al. 2015³).

40
41 Duncan Axisa, of DMT and former president of the Weather Modification
42 Association, stated this in 2018:

¹ We also note that reviews by conflicted reviewers, though they may be valuable, are generally downgraded; that does not seem a policy of the AMS here as it should.

² This claim was also stated in his co-authored paper with Freud et al. 2015, *Atmos Res*.

³ <http://www.dropletmeasurement.com/products>

43
44
45
46
47
48
49
50
51
52
53
54
55
56
57
58
59
60
61
62
63
64
65
66
67
68
69
70
71
72
73
74
75
76
77
78
79
80
81
82
83
84
85
86

“They could have reported accurate ice particle concentrations with our probes if they had wanted to.”

Of course, all of us in airborne research, current and past, already knew this. Perhaps DR was trying to take advantage of perhaps naive SE and BAMS editors who may not know this?

I hope, too, that the SE and the BAMS editors will correspond with Dr. Axisa, or the CEO of DMT, Dr. Daryl Baumgardner, or those who use the DMT 2-DC probe to further corroborate this assertion. The SE and the BAMS editors will then learn firsthand about the unreliability of DR’s comments.

This unreliability is not news to those who have questioned DR’s publications in the past or dealt with him in “Comments” and “Replies.” This unreliability factor will be shown to occur on several occasions in the comments of DR:

- Cites his own work in criticisms in BAMS-D-18-0260 but does not mention that the work he references was overturned when examined by outsiders.
- Misconstrues text (possibly due to a language problem; English is not his first language)
- Is not candid about his history with the HUJ.

Specific responses to the comments of DR.

Comments included in this letter:

Reviewer #1: REJECT

Overview

Art Rangno in his submission (R19) describes the papers that presented the apparent successes of Israeli-1 and 2 experiments as "scientific mirage" (line 527) and that the respective papers should not have been published if they were to be reviewed with proper scientific rigor.

There were **two** mirages: the first one was the seeming *unambiguous* statistical successes of the first two experiments *as they had been reported* by the experimenters; the second one was the mirage of ultra-ripe for seeding Israeli clouds.

A question by a careful reviewer for Gaglin and Neumann (1974—hereafter, GN74) might have been: “How could your ice concentrations vs. cloud top

87 temperature plot (Fig. 13.16) be correct if your radar data (Fig. 13.4) shows rain
88 falling from some clouds with cloud top temperatures at and just below freezing?"

89
90 (The text has not been changed.)

91
92 Since climate change is weather modification in grand scale, I make a counter
93 argument that if this kind of scientific rigor would have been applied to all papers,
94 hardly any paper about assessing anthropogenic impacts on climate change
95 would have been publishable. This is especially true for papers that assess
96 aerosol cloud-mediated radiative effects, which is a hot subject that represents
97 the greatest uncertainty in anthropogenic climate forcing.

98
99 DR makes an interesting point about anthropogenic impacts and their
100 uncertainties, though it doesn't seem to be relevant here.

101
102 In BAMS-D-18-0260 I contend that reviewers should have demanded all the
103 results for Israeli 2, as one facet of "rigorous reviewing⁴." It was heartening to
104 read later that DR agrees with this viewpoint.

105
106 The exposition of the south target results (where cloud tops are warmer as a rule
107 than in the North target) in a timely manner (i.e., in GN76 and in GN81), with
108 strong, but ersatz, suggestions of decreases in rain due to seeding, would have
109 raised numerous questions about the Israeli experiments, the foremost would
110 have been:

111
112 "How could such ripe-for-seeding clouds that we have heard so much about in
113 our journals and at conference, have been associated with decreases in rain due
114 to seeding in the South target?"

115
116 It would have been almost impossible to deflect independent, outside airborne
117 measurements of Israeli clouds had we gotten the full story of Israeli 2 from the
118 outset.

119
120 So, should we accept R19 position and publish only papers that have absolute
121 proofs, or should we publish more studies with more incremental science and
122 learn from the process (also from our errors) as we accumulate a wide body of
123 literature? In my view the answer is obviously the latter option.

124
125 About "incremental" science: The Israeli 2 experimenters, unfortunately, were
126 not interested in a gradual progression to a seeding success. By omitting critical

⁴ Professor Ruben Gabriel wrote to me that even he had forgotten about the South target!

127 data in Israeli 2, they leapt forward and presented us with a partial report of a
128 success we all believed in and thought was complete⁵.

129

130 If the Editor agrees, than what point is served by publishing R19, while he does
131 not provide any new facts? He presents already published materials in an overly
132 dramatized fashion with **inaccuracies and exaggerations** that serve the points
133 that he tries to make, while positioning himself in the ultra-pure tower of ivory⁶.

134

135 "...already published materials":

136

137 The many articles on the Israeli experiments are scattered over numerous
138 journals and conference preprints over the past 50 years; they total more than
139 700 pages. Surely DR is not suggesting that someone who wants to know the
140 history of the experiments discussed here, from the Israeli 1 to the termination of
141 operational seeding, can examine all of those! (The preprints, in particular, are
142 extremely hard to find.)

143

144 DR writes that he sees "inaccuracies and exaggerations":

145

146 Minor corrections regarding Israeli 3 were provided by Reviewer 2, and those
147 points kindly brought out by DR (presumably what he is talking about above)
148 have been addressed and the text revised. For example, as noted, the ice
149 particle concentrations in Figure 3 were halved due to the possibility of probe
150 shattering ice artifacts mentioned by DR.

151 The sentence about "overall unsuitability" has been modified.

152 The word "abandoned" that DR objected to has been removed.

153 -----

154

155 DR questions the usefulness of this article in BAMS.

156

157 The appearance of this article in BAMS will be timely and necessary with cloud
158 seeding activities on the rise due to the climate change-induced drought likely
159 ahead. This account, too, might well inspire those who might wish to bravely do
160 as the Israel National Water Authority did, hire an independent panel to
161 investigate commercial operators (never ending) claims of "success" to test their
162 validity.

163

164 While the appearance of this article in BAMS will **not** be welcomed by those
165 having vested interests in seeding, it will be welcomed by those paying for it.

⁵ DR was a graduate student in the offices of the experimenters when these deviations from standard scientific reporting occurred. Its too bad his voice wasn't, or couldn't, be heard on this matter.

⁶ I was a staff member without an advanced degree, lacking such a perk as "tenure" and always had, "light at the end of the funding tunnel", namely, the end to a grant that sustained me was always visible just over the horizon. Moreover, all the work I did in cloud seeding was on my own initiative, and with a single exception, was done on my own time and dime, including the costly trip to Israel for 11 weeks.

166
167
168
169
170
171
172
173
174
175
176
177
178
179
180
181
182
183
184
185
186
187
188
189
190
191
192
193
194
195
196
197
198
199
200
201
202
203
204
205
206
207
208

If the case that this is insufficient in the view of the Editor to disqualify this paper, I have at least to put the record straight in the remainder of my review.

The suitability of the clouds to glaciogenic seeding

The Israeli-1 experiment started before any cloud physics measurements were conducted in the Israeli clouds, based on an assumed physical conceptual model. Cloud physics measurements were commenced during the conduct of the experiments. The publicized results by Gagin and Neumann (1975-**sic**) presented a picture of clouds that are ideally suitable for the seeding concept.

In retrospect, it appears that Gagin and Neumann cherry picked the clouds with the highest supercooled water but did not yet have the perspective to put them in the context of their relative frequency.

I do not believe that GN cherry-picked their cloud reports.

I believe that they were just inexperienced in the airborne sampling of cumuliform clouds. Too, they did not sample clouds that are representative of those that produce appreciable rain in Israel, but rather much smaller ones⁷. Perhaps this is what DR means by “cherry-picked”? That context would certainly be true. That was the major problem in the early cloud reports; not sampling the wide-complexes, 10s of km wide, that produce a typical appreciable rain events in Israel.

Some background for the SE, the BAMS editors, and DR on the early airborne measurements (likely unknown by DR as well) and by most anyone else. Mr. Rosner, the Israeli experiments’ Chief Meteorologist, told me during my 1986 cloud investigation in Israel that during the early cloud studies by GN74 and Gagin (1975—hereafter G75) high ice particle concentrations *were*, in fact, indicated by the Continuous Particle Sampler (MacCready 1964, *JAM*) used in the airborne sampling Israeli clouds.

But those instances were cast aside, Rosner told me, because the experimenters believed that the Formvar-coated film in that device had jammed (a common problem) with that instrument. Such high ice particle concentrations that were indicated didn’t seem credible in the experimenters’ eyes.

I have no problem with what the experimenters did. Sometimes those things happen, and you make your best judgement, which is apparently what happened in those early 1970s flights. I don’t think anyone can criticize them too much for that.

⁷All less than 5 km wide (e.g., G75).

209 But why couldn't the experimenters and DR see the problems with their cloud
210 reports by simply standing on a Tel Aviv beach just watching the showers rolling
211 in off the Mediterranean as this writer did in 1986?⁸

212
213 Rangno and Hobbs 1988, *Atmos. Res.*) examined the early (G71, GN74, G75)
214 publications on Israeli clouds. We were able to discern why the Israeli clouds
215 stood out at the temperature at which ice first appeared (had to be much colder
216 than comparable clouds). This, as we explained, was due to flaws in sampling
217 and data analysis. Too, the droplet spectra that Gagin and Neuman (1974)
218 reported in those early cloud reports made it appear that droplets were too small
219 to activate the Hallett-Mossop (1974, *Nature*) riming and splintering process in
220 the -2.5°C to -8°C temperature zone⁹.

221
222 However, upon closer inspection, the flaws in the droplet spectra (likely due to
223 poor sampling of clouds and/or sampling clouds unrepresentative of ones that
224 produce rain in Israel) were clear (e.g., Rangno 1983).¹⁰

225
226 The Israeli clouds were, and are, in fact, *primed* for ice multiplication due to the
227 Hallett-Mossop riming-splintering mechanism.

228
229 We can pardon the experimenters for those early flawed cloud reports. It's what
230 happened *after* those early airborne sampling days and the circumspect articles
231 by GN74, G75 in the later 1970s into the 1980s, that's troubling.

232
233 Background:

234
235 In the later 1970s the experimenters obtained an Enterprise 5-cm radar for their
236 use on the grounds of Ben Gurion AP to monitor clouds and storms¹¹ as well as a
237 3-cm vertically-pointed radar which they located near the HUJ. (DR had just
238 joined their group as a grad student under the lead experimenter, A. Gagin, at
239 this time).

240
241 Gagin (1980) reported that aircraft flew over the 3-cm vertically-pointed radar to
242 validate cloud tops he measured during two rainy seasons.

243
244 What do we make of the lack of reporting of shallow precipitating Israeli clouds
245 during this radar era that should have caused the experimenters to rethink their
246 cloud sampling reports? Can anyone truly believe such clouds, ones that would

⁸ Maybe we don't teach how to visually assess clouds in our atmospheric science programs? We should, if one is going to be a cloud physicist!

⁹Those early Israeli cloud reports are discussed in *both* benchmark Hallett and Mossop (1974), *Nature*; Mossop and Hallett (1974), *Science*, articles. Did the experimenters feel "locked in"? Pure speculation, but you do wonder...

¹⁰Rejected by three of four reviewers, one of whom was A. Gagin. *JAM* (B. Silverman, Ed., personal correspondence, 1983). Available on request.

¹¹Rosenfeld (1997) used that 5-cm radar to critique a 1986 Rangno and Hobbs (1995) cloud height assessment in his "Comment."

247 have overturned the early cloud reports, were not detected during two rainy
248 seasons? I'll leave it to the reviewers and BAMS editors to answer.

249
250 Too, that relatively shallow clouds (beginning at thicknesses of 2.5 to 3 km with
251 tops $\geq -10^{\circ}\text{C}$) rained was well-known by the several Israeli Met Service
252 forecasters I spoke with. This was also confirmed by two former seeding pilots I
253 met at Sade Dov AP in 1986 who told me that Israeli clouds began to rain when
254 they reach altitudes of "8,000 to 10,000 feet" above sea level. Examples of such
255 precipitating clouds are also shown in R88.

256
257 Mr. Rosner, the Israeli experiments' Chief Meteorologist also told me in 1986 that
258 he, too, knew that clouds with tops at -10°C or warmer also rained, though he
259 added that he thought it was due to a "warm-rain" process.

260
261 At this point in my 1986 cloud investigation, it seemed the only people that did
262 not know that it rained from relatively shallow clouds with top temperatures $\geq -$
263 10°C in Israel, were the HUJ experimenters and the scientific community abroad
264 that read their papers. Is it plausible that shallow precipitating clouds were not
265 known about?¹²

266
267
268 When I joined the Hebrew University as an Assistant Professor in 1988, I
269 "inherited" the HUJI component of the research of the Israeli cloud seeding
270 experiments and decided to find out for myself (and the for the community) what
271 was really going on.

272
273 Fact enhancement: DR has been continuously affiliated with the HUJ beginning
274 in the late 1970s. DR produced assessments of the clouds that affect Israel
275 using radar, synoptic and satellite data in his Master's (1980) and Ph. D. theses
276 (1986) (both in Hebrew; these are mentioned by Goldreich 2003, *Climate of*
277 *Israel*), and in RF92.

278
279 Why weren't the problems in the descriptions of the Israeli clouds apparent in
280 DR's studies? He had so many chances to find out for himself "and for the
281 community" what was "really going on" so long ago. Is it possible that someone
282 of the brilliance of DR really didn't know what the Israeli clouds were like?

283
284 The data from research flights that I conducted were open and shared with Tel
285 Aviv University. While having documented substantial amounts of supercooled
286 water, there was a great difficulty to determine the concentrations of ice crystals
287 due to lack of suitable instruments. [The 2DC at the time was with round tips and](#)
288 [created occasional non-physically high concentrations due to breakup of ice](#)

¹² Unless we believe that "sea spray cleansing" of clouds, described by Freud et al. (2015) by the Mediterranean Sea, is a new phenomenon.

289 hydrometeors. Zev Levin rushed to publish these high concentrations as
290 evidence for lack of suitability of the clouds for glaciogenic seeding, as R19 cited
291 (Levin, 1992; Levin, 1994; Levin et al., 1996).

292

293 It is ironic that DR invokes the very same reasoning (highlighted above) to reject
294 high ice particle concentrations in Israeli clouds (which do exist) as did G and GN
295 so long ago before him, a rejection of data that misled the original experimenters
296 into believing the clouds of Israel were filled with seeding potential that was
297 ersatz. Perhaps we don't learn from the past.

298

299 The possibility that Levin et al.'s (1996) ice particle concentrations are too high
300 due to shattering artifacts is a valid point. Due to this factor, the ice particle
301 concentrations reported by Levin et al. 1996 in Figure 3 of BAMS-D-18-0260
302 have been reduced by half, far more shattering than has been indicated to
303 produce in laboratory experiments. These "halfway" points are marked by
304 squares in Figure 3.

305

306 It will be seen that this (overdone) reduction of the concentrations reported by
307 Levin et al. 1996 does not reduce by much the observation of "ice multiplication"
308 in the clouds of Israel. They still exhibit, "ice multiplication" and support the
309 findings of R88.

310

311 We don't know why DR himself did not challenge Levin's results and published
312 corrections immediately after Levin et al.'s report appeared in *JAM*.

313

314 The flights "conducted by DR" in 1990 but whose results were reported by Levin
315 (1992; 1994; and Levin et al. 1996) were vital, not only in buttressing the findings
316 of R88, but alerting the scientific community and the Israeli government that
317 something was seriously wrong with the prior HUJ cloud reports on which cloud
318 seeding programs were based.

319

320 Perhaps in view of the likely lack of reporting of the high ice particle
321 concentrations measured on those 1990 flights by DR and his HUJ colleagues
322 caused Levin to act quickly in getting those findings out to the scientific
323 community? Levin should be thanked for getting those data out.

324

325 Had I not spent so much time on the Tel Aviv beaches watching rapidly
326 glaciating, modest Cumulonimbus clouds roll in from the Mediterranean Sea as
327 an experienced airborne researcher, as a cloud photographer and storm chaser
328 who traveled all over Israel during storms, DR's assertion that Levin et al.'s ice
329 particle concentrations are seriously compromised by artifacts might have some
330 credibility.

331

332 It doesn't.

333

334 In view of DR's expressed concern in 1988 to find out, "what was really going
335 on", I point out that his HUU group has not provided the science community with
336 concentrations of ice particles vs. cloud top temperatures of the type seen in G75
337 for more than 40 years. Yet the HUU has flown many flights into Israeli clouds
338 between 1990 and 2013 (e.g., Rosenfeld and Lensky, 1998, *BAMS*; to Freud et
339 al. 2015, *Atmos. Res.*, among several other separate airborne research missions
340 largely reported in conference preprints). Not one of those have ice particle
341 concentrations with cloud top temperatures with the exception of Freud et al.
342 (2015) who reported a *single* modal value for newly risen Cumulus turrets over
343 the Mediterranean.

344
345 The omission of ice particle concentrations continues, I believe, a pattern of
346 omission of critical findings related to cloud seeding by DR and his HUU
347 colleagues over the years.

348
349 *Independent* measurements of ice particle concentrations vs. cloud top
350 temperatures are *desperately* needed in Israel for the good of the INWA and,
351 yes, even the people of Israel. If the HUU and its scientists cannot measure ice
352 particle concentrations accurately with the best instruments available, then we
353 need to bring in those who can.

354
355 In the 1966 (1996) paper Levin associated the high concentrations of ice crystals
356 with desert dust, which in fact supported the desert dust hypothesis (Rosenfeld
357 and Farbstein, 1992). R19 states in line 378 that the dust hypothesis was
358 abandoned, but it is far from being the case. In fact, dust has been shown by
359 numerous papers around the world to serve as good ice nuclei and incurring
360 warmer glaciation temperatures in clouds. I certainly did not abandon it.

361
362 It's not about dust per se, as DR imputes above, and BAMS-D-18-0260 makes
363 that clear. Its about whether the dust *hypothesis* proposed by RF92 to explain
364 the rainfall distribution in Israeli 2 is credible. Its not; the remarkably biased
365 random draw in Israeli 2 obscured any dust effects.

366
367 Synoptics trump dust (e.g., Levin et al. 2010).

368
369 For lack of suitable instruments, it was **never possible** to establish the
370 concentrations of ice crystals in the Israeli clouds. But we (Freud et al., 2015)
371 finally were able to portray the microphysical and dynamical structure of clouds
372 during winter storms in Israel. The conclusions were that the convective clouds
373 over sea and the coastal areas were naturally hygrscopically seeded by sea
374 spray during winter storms, thus developing precipitation quickly.

375
376 DR returns to the topic of ice particle concentrations in Israeli clouds and why
377 HUU researchers have "never" been able to report them from their many research
378 flights.

379

380 What will DR do when outside researchers eventually come in and measure and
381 report those high ice particle concentrations in Israeli clouds and know that he
382 and his colleagues had been hiding them from us? Or will the HUJ
383 experimenters block attempts by outsiders to investigate the clouds of Israel as
384 their predecessors did for so many years in the later 1970s and 1980s?
385

386 Furthermore, virtually every group using 2-D probes has been aware of the
387 "artifact" problem for several decades and they have implemented various
388 software measures to reduce their impact. It is a certain that DR and his group
389 could have obtained such software and done likewise. Instead, they claim that
390 measuring ice in clouds is a lost cause for them.
391

392 But the orographic clouds inland were **not much affected** by sea spray and had
393 supercooled water that was consistent with the galciogenic seeding hypothesis.
394 R19 in line 32 describe this in these words: "A hoped-for exception to this overall
395 "unsuitability" is in the Golan Heights where Israel is now conducting Israeli-4".
396 This is an example for the inappropriate language that distorts the actual
397 findings. There is no overall "unsuitability", as most of the clouds inland are
398 orographic, as illustrated in Fig. 3 of Freud et al. (2015).
399

400 I have reviewed Freud et al. 2015 post publication with great thoroughness (as
401 required) and as an expert in Israeli clouds and storms. I was not sure it even
402 had been reviewed after reading it.
403

404 While some of the best, circumspect, qualified writing to come out of the HUJ
405 "cloud seeding unit" was in that article, it also had a Jeckyl-Hyde aspect. It was
406 as though different authors with very different views of how science should be
407 reported had written different sections¹³.
408

409 Overall, however, after reviewing it, I was saddened to think that Freud et al.
410 2015 may have been behind the INWA's decision to execute a new, and very
411 expensive, randomized cloud seeding experiment. What I found in Freud et al.
412 does not bode well for a successful outcome in my opinion¹⁴.
413

414 I addressed DR's claim of "suitable orographic clouds" and Fig. 3 in my review of
415 Freud et al. 2015. Perhaps if I had not spent 11 winter weeks in Israel chasing
416 clouds and storms all over Israel, working within the IMS, I might have swallowed
417 whole what is shown in Figure 3 in Freud et al. (2015). Figure 3 is not a
418 representative schematic of a standing pattern of clouds in Israel.
419 -----

¹³ I have been trying for more than a year to get Elsevier, that destroyer of libraries with their rapacious fees, to grant me permission to send my comprehensive review of Freud et al. 2015 to Freud et al. among others, including the INWA. Elsevier has not responded to my request, nor to that from the Copyright Clearance Center.

¹⁴ It is critical that this experiment NOT be evaluated by those at the HUJ!

420 Second, the Israeli 2 north target has been the subject of extensive *independent*
421 re-analyses by RH95, with “Comments” and Replies in 1997, and again by Levin
422 et al 2010 with “Comments” by Ben-Zvi et al. and “Replies” by Levin et al. (2011).
423

424 These *independent* re-analyses found no evidence of seeding having affected
425 rainfall in a detectable way in an orographic target.
426

427 Those findings are compatible with the *independent* finding of Kessler et al. 2006
428 (reprinted by Sharon et al. 2008) the latter describing the termination of
429 operational seeding in Israel due to a lack of evidence that runoff had been
430 increased by seeding).
431

432 Thus, we have three *independent* evaluations of cloud seeding in Israel that
433 found no viable results. These findings were acted on by the final arbiter, the
434 INWA. How many more *independent* examinations of seeding do we need?
435

436 R19 claims that the seeding agent was insufficient for inducing the seeding effect
437 (lines 280-285). He cites Levin et al. (1997) who reported AgI nucleating activity
438 of $1.4 \times 10^{15} \text{ L}^{-1}$ at -20C which decreased to 10^{13} L^{-1} at -15C. The exact
439 number is a decrease to 5×10^{14} at -16C. The optimal indicated seeding effect
440 was reported by Gagin and Neumann (1974) to occur for clouds with top
441 temperatures between -16 -21C. So R19 has no basis for claiming that seeding
442 material could not have affected the clouds according to the reported seeding
443 effects.

444 In any case, the activity of present formulation of the AgI with added Cl
445 compound is $1.4 \times 10^{15} \text{ L}^{-1}$ crystals at -10C.
446

447 DR has misconstrued this element of BAMS-D-18-0260.
448

449 The discussion is about the seeding methodology and the little AgI that was
450 released in Israeli 1; it's **not** about the activation characteristics of the AgI. Not
451 enough AgI was released to create statistically-significant results considering the
452 few clouds that were actually seeded in view of the total hours of rain-producing
453 clouds in 50-70 average number of rain days that affect Israel during the Oct 15-
454 April rain season.
455

456 Concerning seeding effectiveness, we note that DR did not “Comment” on Levin
457 et al.’s 1997 modeling study that showed the airborne seeding technique
458 employed in Israeli 2 to be inefficient.
459

460 The statistical experiments

461 The Israeli-1 project started as a black box experiment, which its analysis was
462 published in accordance to its statistical design as a success. I can't see any
463 reasonable reviewer that should have prevented such a publication.
464

465 I am *general* agreement with DR here, considering the era it was published in
466 where the idea of storm types ruining the interpretation of experiments was only
467 just beginning to be appreciated (as in the Santa Barbara randomized
468 experiments, e.g, Neyman 1977). Wurtele (1971) the first peer-reviewed article
469 describing results of the full Israeli 1 experiment, was extremely complete, as
470 was the followup by GN74.

471
472 Nevertheless, below is what a rigorous reviewer, perhaps an exceptional
473 reviewer, would have “demanded” as a reviewer of Israeli 1, one aware of how
474 “confirmation bias” afflicts and distorts this field:

475
476 First, more evidence should have been required to explain the critical finding that
477 the Buffer Zone exhibited the highest statistical-significance in the experiment,
478 particularly when their own Chief Meteorologist, Mr. Karl Rosner, asserted that it
479 couldn’t have been seeded more than “5-10%” of the time (“and probably less”).
480

481 This raised a red flag. The experimenters countered from their own wind
482 analysis in GN74 that it was, indeed, likely due to inadvertent seeding.

483
484 Thus, a sharp dichotomy arose that needed to be resolved by outside interests.
485 If the claim by the Chief Meteorologist was found to be correct it would have
486 ended this burgeoning “success” story immediately with its ultimately sad ending
487 for Israel. (And I would not be debating with DR at this very moment.)
488

489 Second, reviewers could have asked to see soundings when rain was falling at
490 the Bet Dagan launch site to check the nature of Israeli precipitating clouds (as
491 was done in R83 and R88).
492

493 Area-wide analyses should have been required by reviewers on seeded days to
494 see how widespread the apparent seeding effect was. The findings of Brier et
495 al. (1974) of regional seeding effects really suggested a Type I statistical error,
496 though Brier et al. themselves did not see it that way. Perhaps they were
497 unaware of how little AgI was released in Israel-1?
498

499 Lastly, a point that has been made on several occasions by the experimenters
500 and by RH95, the indications of a large seeding effect in the coastal zone of
501 Israel-1 are not supported by reasonable logistical arguments. Rather, they
502 support lucky draws on the seeded days as inferred in RH95 and RH97.
503

504 The analysis of the Israeli-2 experiment as a stand-alone experiment was also a
505 reasonable one. Here a reviewer could have done a better job with Gagrin and
506 Neumann (1981) and require that the full cross over analysis would be done also
507 for Israeli-2 experiment, as it was done for Israeli-1.
508

509 Thank you.
510

511 The daily correlation of 0.69 between North and South target areas is still quite
512 useful for such an analysis, in contrast with the claim made by Gagin and
513 Neumann (1981). [When I joined the Hebrew University in 1988](#), I had questions
514 about that and embarked on redoing the full analysis of the Israeli experiments,
515 and published by Gabriel and Rosenfeld (1990). This resulted in the zero effect
516 on the cross over analysis for Israeli-2. This led to a series of exploratory
517 analyses that tried to understand the causes.

518

519 DR was affiliated with the HUI for ten years prior to 1988 (please examine his
520 publication affiliations). His Master's Thesis on the clouds and storm types for
521 Israel using radar, satellite and synoptic data was published in 1980, his Ph. D.
522 dissertation in 1986. He did not just "drop in" like yesterday's rain in 1988 and
523 suddenly find problems.

524

525 Moreover, the Israeli experiments' Chief Meteorologist, Mr. Rosner's campaign to
526 "out" the South target results began in January 1986 if not earlier. Mr. Rosner
527 began a letter writing campaign to various government agencies about those
528 omitted results¹⁵. The leader of the experiments and the operational seeding
529 program, his staff, however, continued to rebuff Mr. Rosner's requests until the
530 leader passed in September 1987.

531

532 Only after the leader passed was Mr. Rosner's demands acted upon by Ruben
533 Gabriel and DR. Is it possible that DR did not know that Karl Rosner was
534 blowing the whistle on the South target omission in 1985-1986??? Mr. Rosner
535 should be considered a hero by the Israeli people.

536

537 The dust hypothesis showed that indicated positive effects occurred only in
538 conditions where clouds were not visibly affected by dust (Rosenfeld and
539 Farbstein, 1992; Rosenfeld and Nirel, 1996). This remains (at least in my mind)
540 the most plausible explanation to date. However, R19 presents incorrectly this
541 hypothesis as abandoned.

542

543 The word, "abandoned" has been deleted and this discussion has been altered.

544

545 R19 cites Levin et al. (2010), stating that "The stronger lower level winds created
546 a pseudo-seeding effect by pushing the maximum rainfall field eastward toward
547 the hilly regions of the target, the rain amplified by orographic effects". But they
548 neglect to mention that Farbstein et al. (1992) had already formally tested this
549 question 18 years earlier by using the low level winds and other synoptic
550 predictors for the target rainfall and as a surrogate for the control area. The
551 surrogate control rainfall had a correlation of 0.72 with the daily rainfall in the

¹⁵ It was personnel within the Israel Met Service itself (A. Manes) that alerted me to Mr. Rosner's campaign and provided his contact information when I worked within the IMS climate group in 1986.

552 North target area. Using that instead of the target rainfall as a control still left a
553 positive effect of 1.08 in the North target area.

554

555 I thank the reviewer for pointing out a study this writer has not seen referenced.
556 However, I cannot find that Farbstein et al. (1992) has ever been cited. It was not
557 cited by Ben-Zvi et al. in their challenge of the results of Levin et al. 2010, the
558 logical place to have referenced it. Where is it? Is the year correct?

559

560 Another bias of R19 is demonstrated by his dismissal of the relevance of the
561 formal control area of Israeli-2 North in favor of undefined control areas in
562 Lebanon and Jordan (lines 386-387; 395-398), while the correlation of the North
563 Control with North Target was 0.91 (0.87 in GN74). For example the correlation
564 of North vs. the South target was 0.61. Because Jordan is further away than the
565 South, its correlation is likely even lower. Yet, R19 used that argument for
566 asserting that the statistical outcome of Israeli-2 Experiment was invalid as
567 having Type I statistical error, or "lucky draw".

568

569 The control zone mentioned above, while highly correlated beforehand, did not
570 respond to the region-wide heavier storms on the north target's seeded days as
571 shown in RH95. There are more than 500 standard rain gauges in Israel and
572 nearly 100 recording gauges (A. Vardi, IMS Deputy Director, personal
573 communication, 1987).

574

575 What is critical here is learning whether the exact gauges to be used in the
576 control/target analyses by GN76/81, were specified BEFORE the Israeli 2 began.
577 The large number of gauges available to cherry pick a small increase in rain due
578 to seeding makes this information critical.

579

580 We note, too, that the gauges used in the initial analyses were not the same as
581 the ones used in Gabriel and Rosenfeld (1990). DR himself selected different
582 gauges. Fortunately, the north target/control results *were* examined by
583 independent researchers outside the HUU: Levin et al. 2010 (with, as noted,
584 Comments and Replies by and to Ben-Zvi et al. 2011).

585

586 Re Lebanese data: In 1969 during the design of Israeli 2 the Israeli Rain
587 Committee recommended the use of Lebanese data as a control for the North
588 target, much of which is downwind from Lebanon gauges. The experimenters
589 did not, or were not able to do this, but some data (just two stations) was
590 obtained from Lebanon, all that could be gotten, was used in RH95.

591

592 Jordanian data: The regional analysis by RH95 that included Jordan and
593 Lebanon showed that the heavier rainfall on North target seeded days extended
594 into Jordan, as one would expect given the synoptic findings of Levin et al. 2010.
595 If, in fact, there was no extension of those stronger storms into Jordan, the seed
596 ratios shown would be "helter-skelter", would not showing a consistent pattern of
597 heavier rain on North target seeded days as they do.

598

599 A note about high target/control historical correlations:

600

601 The Stehekin River runoff, a control for the Skagit River cloud seeding project,
602 exhibited a correlation of 0.98 (!) with the target Skagit river runoff over a period
603 of more than 30 years. But that historical correlation proved useless when the
604 storm type problem reared its head and produced an extraordinary snow year full
605 of not previously seen gradients in precipitation and runoff across the Cascade
606 and Olympic mountains of Washington State. These extraordinary gradients in
607 the winter of 1963-64 suggested a stunning, but ersatz, cloud seeding success
608 for the Skagit Project (see Hobbs and Rangno 1978, *JAM*). Nature can be cruel
609 to cloud seeding experiments.

610

611 This is the same argument that R19 used for the statistical outcomes of the
612 Israeli-1 experiment.

613 R19 highlights that the buffer zone between the two targets of the Israeli-1
614 experiment had 31% more rainfall in south-seeded days than in north-seeded
615 days, which was unlikely affected by seeding (lines 105-106). Because the buffer
616 zone is correlated well with both the North and South target areas, if it is used as
617 a control area for separating the effects of seeding for the North and Center of
618 Israeli-1 Experiment,
619 then we should subtract 31% from the seeding effect in the center and add 31%
620 to the single ratio seeding effect in the north, obtaining a corrected effect of 1.08
621 X 1.31=1.41 for the north, and $1.22/1.31 = 0.93$ for the center. The overall effect
622 (RDR, root double ratio) remains unchanged: $(0.93 \times 1.41)^{0.5} = 1.15$
623 (Rosenfeld, 1997). This makes the results of Israeli-1 and Israeli-2 very similar,
624 i.e., the relative effect in the north is much larger than in the south. Unfortunately,
625 R19 elects to ignore this critically important piece of information and prefer the
626 explanation of a "lucky draw".

627

628 DR's discussion above fails to note the difference in storms between Israeli 1 and
629 2. A reading of Gabriel (1967), Gabriel and Baras (1970), Brier et al (1974) will
630 make it clear that the random draw in Israeli 1 was very different in storm types
631 than in Israeli 2. Too often we neglect storm types and their influence on
632 experiments.

633

634 DR in his comment above, *assumes* in his analyses above that country-wide
635 storms of the type that compromised Israeli 2, was what the random draw also
636 produced in Israeli 1. It did not.

637

638 The sharp gradients in rainfall across Israel in Israeli 1 due to more localized
639 rains have been called out on several occasions as below:

640

641 Gabriel (1967): "In other words, $N_n - N_c$ has had the same sign as

642 $C_n - V_c$. Apparently, in any one season, the random differences in country
643 wide precipitation between North seeded days and Center seeded days were
644 much larger than any possible seeding effect.”

645

646 **Gabriel (1967)** “Seeding is more effective when buffer-South differences are
647 unusually large or small, that is, either very large or negative. Could it be
648 that when rainfall conditions deviate from the mode in the sense of an
649 unusual distribution over different parts of the country, there is some
650 atmospheric instability which furthers seeding effectiveness?”

651

652 **Brier et al. 1974:** “Figure 4 shows that precipitation increases at these three
653 stations on “north seeded” days tend to occur when precipitation is high at
654 X_4 (southern Lebanon) relative to X_1 , X_2 , and X_3 (in southern Israel and
655 Jordan). This suggests that cloud seeding is more effective when the storm
656 center is near the seeded target. Figure 5 shows a similar analysis for the
657 center target and three downwind areas. Here precipitation increases on
658 center-seeded days tend to be associated with increased precipitation at
659 controls X_2 and X_3 compared with X_1 (coastal southern Israel) and X_4
660 (southern Lebanon).”

661

662 We thus defer to the Israeli Chief Meteorologist’s interpretation and that in RH95
663 that the BZ was barely seeded on Center seeded days. Thus, a natural bias in
664 the draw been benefited the Center target. Those storms on Center seeded
665 days, diminished in rain production to the north as the quotes above indicate.

666

667 Furthermore, we’ve now known for decades that the clouds entering Israel are
668 unsuitable for seeding--the HJ scientists have apparently just discovered this in
669 Freud et al. 2015, *Atmos. Res.*

670

671 Map 2 below is from Gabriel and Baras (1970, Final Statistical Tables and
672 Evaluation) as it was received at the University of Washington from Ruben
673 Gabriel, with annotations on the map in place).

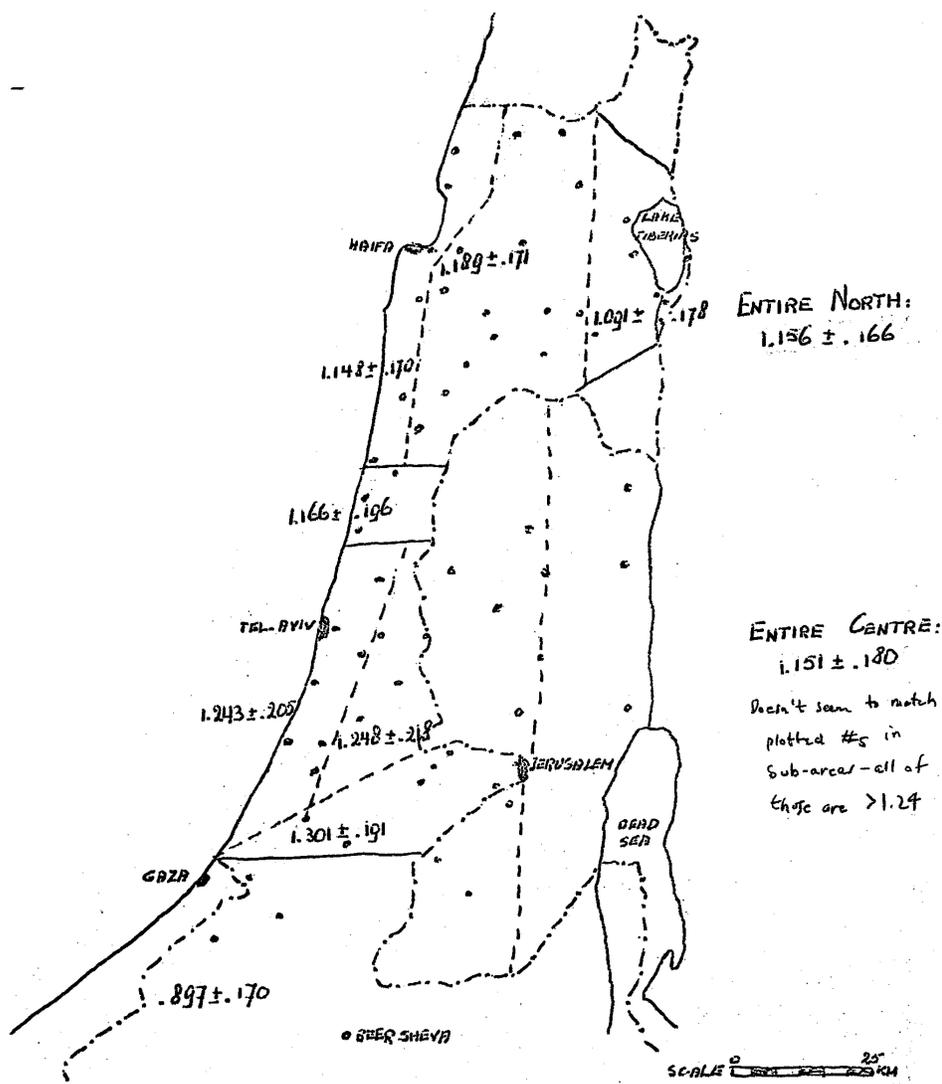
674

675 Note the large Seed/No Seed ratios in the coastal zones on target days, raising
676 the question, “Why are we tilting uselessly over a seeding effect on coastal
677 rainfall in Israeli 1 that can’t have been?”

678

679 Isn’t this “pathological science,” as described by Langmuir (1953)?

680



681
682
683
684
685
686
687
688
689
690
691

R19 invokes the "lucky draw" twice in a row for randomized experiments with statistically significant results. The chance for this to happen 2 times in a row is exceedingly small, but R19 argues that this is what actually took place and caused the "fall" of the Israeli experiments.

Concerning two "lucky" draws: The experimenters changed the game to get a second "lucky draw" while omitting the several other evaluation methods planned for Israeli 2 that would not have produced a "lucky draw."

692 Had exactly the same statistical tests, the same target regions and the same rain
693 gauges been used for each Israeli experiment DR's arguments would have merit.

694
695 When the experimenters abandoned the method used by Gabriel (1967), Gabriel
696 and Baras (1970) for the evaluations of Israeli 2, they also weakened their
697 results: Enter "multiplicity" which goes with changing the control stations, the
698 control areas, the target gauges.

699
700 It's not as simple as DR wants us to believe without these subordinate details (in
701 which the devil resides).

702
703 If the experimenters wanted to truly test the idea that there had been two similar
704 outcomes of Israeli 1 and 2, with the BZ remaining constant in each one, they
705 could have performed exactly the same statistical tests that Gabriel (1967)
706 employed for the as much of the same interior subregions of Israeli 1 as possible
707 in Israeli 2 using the same gauges in a crossover evaluation.

708
709 But, given the remarkably bad draw in Israeli 2, we know that would not have
710 turned out well.

711
712 In fact, there would have been no replication of Israeli 1, no two "lucky draws" in
713 a row without changing the game¹⁶.

714
715 Furthermore, analysis of the continued operational seeding in the North showed
716 significant positive effects (Nirel and Rosenfeld, 1995) once more, further
717 narrowing the basis for R19 claims.

718
719 DR omits contrary findings to his own in this comment, those by Levin et al.
720 2010, again practicing "one-sided" or "selective" citing. The results quoted above
721 by DR were not sustained in *independent* evaluations by Kessler et al. (2006),
722 reprised by Sharon et al. (2008), and by Levin et al. (2010).

723 Signs of data dredging to find a result are suggested in Nirel and Rosenfeld 1995
724 when once the methodology and controls were chosen, no further seeding effect
725 in runoff was observed according to the independent panel.

726
727 Second, there is no evidence that the methodology and controls used by Nirel
728 and Rosenfeld (1995) were documented before the start of the operational
729 seeding period in 1975, a mortal flaw.

730
731 Please see Rangno and Hobbs (1993, *JAM*) for another example of ersatz
732 "increased precipitation due to seeding" that terminated **after** the experimenters
733 selected their controls, the classic sign of cherry-picked controls among many
734 control possibilities to elicit the result the experimenters wanted to exhume from

¹⁶ Two "lucky draws" in a row have been reported in another benchmark set of experiments, those at Climax, CO (Mielke 1979, *J. Amer. Stat. Assoc.*)

735 the data while hiding its ambiguity.

736

737 The operational seeding

738 R19 cites reports that find no enhancement of the runoff and replenishment of
739 water at the Sea of Galilee with the operational seeding and ascribe it to lack of
740 seeding effectiveness (lines 440-458).

741 Although Givati and Rosenfeld (2005) separated between the decreasing general
742 trend of precipitation (due to air pollution) and the positive seeding effect on top
743 of it,

744

745 DR practices “one-sided-selective citing” by omitting the contrary evidence about
746 his pollution findings. Is this the HUU’s *modus operandi* in the cloud seeding
747 arena, to cite only part of a science story?

748

749 Alpert et al 2008, 2009, Halfon et al. 2009 found evidence of cherry-picking of
750 controls to find an “air pollution” explanation for the lack of detectable cloud
751 seeding/runoff effects found by Kessler et al. (2006; Sharon et al. 2008 and Levin
752 et al 2010¹⁷ The findings of Givati and Rosenfeld (2005) were, in fact,
753 ambiguous, and not the proof that they claimed.

754

755 We reprise Thom’s (1957) aphorism concerning “cherry-picking” of controls:

756

757 *"If one takes the liberty of choosing among minimum distance controls, he can*
758 *often find any result for seeding that suits his purpose, either positive or*
759 *negative."*

760

761 And Gabriel’s (1967a) ten years later:

762

763 “After all, by posterior selection of suitable subareas one could have “proved”
764 almost anything one might have wished to prove.”

765

766 Halfon et al’s reply to the “Comments” of Givati and Rosenfeld supported that
767 what Thom and Gabriel described above was exactly how Givati and Rosenfeld
768 (2005) generated their results in a vain attempt to salvage a lost operational
769 seeding cause that cost their country so much.

770

771 R19 cite others who failed to account for trend in decreasing precipitation due to
772 climatic changes. Please see the following four references:

773

774 Givati, A. and Rosenfeld, D., 2013. The Arctic Oscillation, climate change and the
775 effects on precipitation in Israel. *Atmospheric Research*, 132, pp.114-124.

776

777 Peleg, N., Bartov, M. and Morin, E., 2015. CMIP5-predicted climate shifts over

¹⁷An interim report by Kessler et al. (2002) concerning this had already appeared.

778 the East Mediterranean: implications for the transition region between
779 Mediterranean and semi-arid climates. International journal of climatology, 35(8),
780 pp.2144-2153.

781

782 Shohami, D., Dayan, U. and Morin, E., 2011. Warming and drying of the eastern
783 Mediterranean: Additional evidence from trend analysis. Journal of Geophysical
784 Research: Atmospheres, 116(D22).

785

786 Tal, A., 2019. The implications of climate change driven depletion of Lake
787 Kinneret water levels: the compelling case for climate change-triggered
788 precipitation impact on Lake Kinneret's low water levels. Science of The Total
789 Environment.

790

791 Yes, there is reason to believe that Israel will become drier in the decades
792 ahead. One hopes that Israel-4 can come up with some viable water.

793

794 (But where are the results, preliminary or otherwise, after five or more winter
795 seasons of random draws?)

796

797 Lessons to be learned

798

799 R19 determines that the Israeli cloud seeding experiments failed. This is far from
800 being the case, as I demonstrated above, (writers note: by omitting contrary
801 publication references) although they did not reach the level of proof that is
802 required confidently for accepting that cloud seeding did add rainfall.

803

804 The Israeli experiments were not failures in any way except in proving the
805 viability of cloud seeding, let me make that clear. Silverman's (2001)
806 independent review of the Israeli experiments corroborates DR's statement
807 above that they fell short of providing the necessary evidence of viable rain
808 increases due to seeding.

809

810 In view of the millions the Israeli government wasted on operational cloud
811 seeding due to lack of full reporting by the HUU experimenters, but most of all,
812 due to their inability to detect the efficiently precipitating nature of their clouds
813 over the many decades they had chances to do so, it means that HUU scientists
814 will have difficulty acknowledging the full range of their errors and the
815 consequences they had on their own people.

816

817 The HUU scientists do not seem to appreciate that acknowledging error
818 enhances one's standing among scientists.

819

820 The experiments are not a total failure nor a smashing success.

821 R19 claims the position of ivory of tower and pontificates his lessons from up
822 there.

823
824
825
826
827
828
829
830
831
832
833
834
835
836
837
838
839
840
841
842
843
844
845
846
847
848
849
850
851
852
853
854
855
856
857
858
859
860
861

Yes. 😊

I have expressed my view of these lessons at the outset in the overview of this review. R19 blames the researchers of the Israeli experiments for being biased, not being refereed carefully, and having poor scientific judgment. As I have shown above, R19 suffers from these weaknesses in his assessment of the Israeli experiments. So, in fact, he blames others for his own faults. In summary, this submission is not publishable in any journal, let alone in BAMS.

Thanks a lot, Danny... 😊

How about inviting me, a seasoned expert in airborne cloud studies, as a guest to go in your research aircraft with that DMT 2-DC probe with “pointy-tips”? I’ll find you the ice you’ve been missing in those slightly supercooled clouds (cloud tops $\geq -12^{\circ}\text{C}$) all these years... We’ll have Daryl and his group recover “accurate” ice particle concentrations from your DMT probe for you...

In the event of publication of BAMS-D-18-0260, as SHOULD happen but probably won’t, I would RELISH the open debate with DR and anyone else in BAMS! This is what science is about. Danny and I make good adversaries, and our colloquy would be enormously interesting to readers, as would those of other scientists who “jumped into the fray.”

I am so glad I went to Israel to see those beautiful clouds for myself.

The “comments” of the reviewers and my responses to them should be published along with the article.

Open the door to “openness.”

Sincerely,
Daniel Rosenfeld

Sincerely as well,
Mr. Arthur L. Rangno
