Comprehensive review of

"Cloud microphysical background for the Israeli-4 cloud seeding experiment"

by Freud, E., H. Koussevitsky, T. Goren and D. Rosenfeld,

published in 2015 by Atmospheric Research

and "White Paper" concerning the history of the reporting on cloud seeding by the Hebrew University of Jerusalem

by A. L. Rangno[1]

Opening remarks

The review of this 2015 published article is organized by topics; it also contains a brief history of cloud seeding in Israel for potential future reviewers of those additional manuscripts in cloud seeding that might emanate from the Hebrew University of Jerusalem's "cloud seeding unit" (hereafter, "HUJ-CSU" and refers to those HUJ authors over the decades that have authored papers on cloud seeding). This history may also be of interest to young graduate students within the HUJ science department who may not be informed about it.

The major portion of this critique of Freud et al's (2015, hereafter "F2015") paper are due to the authors' claims in the abstract and "Introduction" sections. The former chief editor of Science magazine says it all:

"The difficulty is that positive claims are sometimes made against a background of unrevealed negative results."

----------------- Donald Kennedy**,** 2004 *Science* editorial: "The Old File Drawer Problem."

However, F2015 improves measurably after the "Introduction" section, though there are still significant, but fewer, "reviewer" comments. There are also a few minor editing suggestions.

The word "***independent***" is highlighted in this review due to the remarkable number of times that outside, ***independent*** researchers, when examining the findings published by the HUJ-CSU in peer-reviewed articles, beginning with Rangno (1988-hereafter R88), could not substantiate them. This may give the HUJ-CSU one of the highest rankings of published "unreliable, non-confirmable" results.

HUJ are you listening?

[1] Retiree, Cloud and Aerosol Research Group, Atmos. Sci. Dept., University of Washington, Seattle.

"One-sided citing", is often practiced by the HUJ-CSU, and is seen again in F2015.

"One-sided citing" has recently been condemned in the American Meteorological Society book, *Eloquent Science,* by David Schultz (2015). Here's what Schultz (2015) had to say about one-sided citing:

"One-sided reviews of the literature that ignore alternative points of view, however, can be easily recognized by the audience, leading to a discrediting of your work as being biased and potentially offending the neglected authors (who might also be your reviewers!)."

Moreover, there is material damage to your fellow scientists when they are not cited when they should be, as by the HUJ-CSU in this and other cloud seeding articles. The impact in one's field; promotions, awards, impact and status, is usually determined by an impact metric, such as the number of citations of your work. Too, there is implicit damage to the reputation of the home department and institution from which such one-sided citing emanates.

HUJ, are you listening? You're the university of Albert Einstein, Sigmund Freud, and more.

One-sided citing, as in F2015 also establishes evidence of a poor peer review process prior to publication, perhaps by seeding partisans ("one-sided reviewing"?) The obvious poor peer review of this article in the manuscript stage is why I have troubled to spend time on it.

It is strongly recommended that the HUJ-CSU authors read Schultz' book, and also that a course in scientific writing and ethics be taught at the HUJ. (Perhaps many research institutions would benefit from such courses.)

-----------------------------Organization of this review----------------------------

This review is organized in sections:

1) Reviewer disclaimers, baggage, conflicts of interest, background that qualifies him for this review, etc., as should always be mentioned.

2 Overall assessment of this paper.

3) Who should be assessing seeding potential in Israel? Ans. Not the HUJ-CSU, as told by their history.

4) The shifting cloud microstructure reports over the decades from the HUJ-CSU: how did it go so wrong? (Ans. "We don't know yet.")

5) The wrongful scientific consensus on Israeli cloud seeding: how did it go so wrong? Ans. "We don't know yet."

6) A list of mandated, publically-available data from the HUJ-CSU field program described in this paper, and why.

7) "Filling in the blanks": critiques of the HUJ-CSU's incomplete descriptions of Israeli 1, 2, and 3, and operational seeding in this paper (in separate, titled "modules.")

8) If you want to go farther: The original article itself (in black type) with "inline" reviewer's commentaries following highlighted statements is found here. Repetition is inevitable and left in place, since readers may also jump around and otherwise miss points.

Line numbers have been added so that rebuttals or support of this review can be easily accomplished.

I don't apologize for the length of this review, virtually identical to the one I would have done if I had been asked to do one by the editors of *Atmospheric Research* (hereafter, *AR)*. The decision from here on a manuscript version would have been "Accept, but ONLY upon the authors fulfilling the required major revisions and corrections to their statements."

About the excessive length: The HUJ-CSU has "earned it"; it can't be trusted in the seeding domain under its current leadership IMO. If this sounds overly provocative, or even outrageous, read on…

"It didn't come out of a vacuum."

This "review" will contain compliments and "condemnations." Expect to get mad at someone, maybe me. The language is sometimes strong. But, I have done my *best* with the brain God gave me in this review. Let us begin…..

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1) Reviewer "baggage" (and disclaimers): The reviewer's 1986 11-week cloud investigation in Israel

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I have worked on both sides of the seeding fence, having participated in operational seeding projects in South Dakota twice, India, Washington State, and in the Sierras of California. I was a forecaster for the Colorado River Basin Pilot Project, a large randomized cloud seeding experiment in Colorado, and following that, worked in airborne studies of clouds and the origin of ice in clouds in the University of Washington's Cloud and Aerosol Research group for the next 28 years. Subsequently, he participated in cloud studies for NCAR during the 2006-07 winter as a "flight scientist" in their Saudi Arabia seeding potential study.

This review is motivated by the absence of post-publication comments on F2015 over the past three years. And it comes from the emotional reaction after I read it recently for the first time: "Someone has to do something about this!", the same feeling I had before I went to Israel in 1986 as a skeptic of ripe-for-seeding cloud claims emanating from the HUJ-CSU.

The reviewer's *a priori* convictions concerning cloud seeding:

1) cloud seeding works in limited situations, principally via the seeding of non-precipitating, supercooled clouds. Whether there is economically viable return from such seeding, I don't know. I begin every talk I give with, "Cloud seeding works!", and show a couple of examples of effects of seeding (those resembling Schaefer's dry ice seeding experiments in 1947)

2) In late 1985, I resigned my job at the University of Washington Atmos. Sci. Dept. over issues of credit, and went to Israel in January 1986 to investigate their clouds. I was skeptical that those many descriptions of them in the peer-reviewed literature and in conference presentations were correct. I did not go to Israel without "baggage,", but had done a considerable amount of "homework."

A note-sized paper asserting that the clouds were not as they were being described in journal articles and elsewhere, had been rejected in 1983 (B. Silverman, personal communication). That rejection was instrumental in my 1986 trip. The several reviewers' negative takes on my "Note", including that of the leader of the HUJ-CSU at that time (A. Gagin, 1984, who lectured me at the Park City weather mod conference about how wrong I was), had no effect whatsoever about what I thought about the clouds of Israel. To repeat, I had done my homework, had scrutinized all of their cloud reports in great detail.

While in Israel in 1986, that same leader of the HUJ-CSU prevented this writer from visiting the HUJ-CSU-controlled radars to evaluate cloud top heights during storms. I wanted to see what top heights were, and indirectly via Israel Meteorological Service (IMS) rawinsonde soundings, obtain top temperatures. ("AG" did allow a brief "show and tell" visit to his Ben Gurion AP radar as a storm approached[2] as part of our third and last meeting.)

To disallow a *bona fide* worker in the field of cloud microstructure and weather modification access to data/measurements to test claims about the Israeli clouds was demonstrable scientific misconduct[3]. The clouds of Israel can *only* be studied in Israel, and as scientists we must be open to cross-checking of our results; having our findings tested. It's what we do in science (e.g., see Blyth and Latham's 1998 criticisms of the glaciation papers of Hobbs and Rangno and our Reply as a great example of open criticism).

But this was not what the leader of the Israeli experiments understood about science. If he was deluded about his clouds, or was correct about them, I would have been welcomed, I thought. If he had "contrary knowledge", I would be blocked. Q. E. D.

My findings, in lieu of radar data, were based on Israel Meteorological Service's (IMS) four-times-a-day rawinsondes, were published in 1988. They strongly suggested that there was a serious problem with the HUJ-CSU's ultra "ripe-for-seeding" cloud descriptions. Rosenfeld and Farbstein (1992) within the HUJ-CSU, belatedly discovered that "dust-haze" when present, produced efficiently raining clouds in Israel, findings that supported R88. Dust, with haze has been around for quite awhile in Israel.

Later ***independent*** aircraft reports by Tel Aviv University (Levin 1992, 1994, Levin et al. 1996, hereafter

[2]Marked by a mid-level overcast of Altostratus and some lower Altocumulus with dust.

[3] After being refused access the HUJ-SU's radars, I wrote to several scientists around the world at near the time of the refusal asking them to intervene with the leader of the HUJ-SU so that I could gain access during storms. The author is not aware of what action, if any, those scientists took beyond encouraging replies (P. V. Hobbs and Lawrence Radke, U of Washington; Gabor Vali, U of Wyoming; Roscoe Braham, Jr., North Carolina State U.; and S. C. Mossop, CSIRO. No reply was received from Hobbs or Radke, who were involved in the Genesis of Atlantic Lows project.

L96) also supported R88 and further exposed the faults in the HUJ-CSU cloud descriptions.

Today, and at *last* in this paper by the HUJ-CSU in 2015, itself finally acknowledges, implicitly, why the leader of the Israeli experiments refused this visitor access to radars in 1986: the high precipitating efficiency of Israeli clouds was going to be obvious in radar cloud top imagery.[4] At the same time, F2015 offer additional confirmation of the those long ago findings in R88. Lahav and Rosenfeld (2000), and Rosenfeld et al. 2001 had also found some clouds unsuitable for seeding, but did not state that explicitly as do Freud et al.

The high precipitation efficiency of Israeli clouds, too, is not limited to "dust-haze"; it never was. The belief that the HUJ-CSU attribution of divergent seeding effects to dust-haze was unreliable, was the "acorn" of conviction that led to the "oak" of RH95, also done on "own time, own dime"; not on grant monies.

Rather than "dust-haze, that high efficiency is due relatively low droplet concentrations in Mediterranean clouds, perhaps due to "sea spray " from whitecaps derived from the five million year old Mediterranean Sea that provide large aerosol particles on which cloud droplets first form on at cloud base (as asserted in this paper and previously by Levin 1992)[5]. Larger droplets in clouds accelerate the precipitation forming process.

2. Overall assessment of F2015

This article, due to its "Jeckyl-Hyde" properties, presents a unique review challenge; some of the best conscientious scientific writing by the HUJ-CSU is in this very article; the kind of writing marked by caveats and qualifications made in a circumspect manner. After all, "the Hebrew University of Jerusalem is Israel's premier academic and research institution" as it states on its web page. (Thus, it should be held to a high standard of research reporting.)

On the positive side, too, it was pleasant and unexpected to read that the HUJ-CSU has finally agreed with long standing work, first published by Rangno 1988-hereafter R88, that the Israeli clouds are overall unsuitable for seeding with silver iodide due to their propensity to form precipitation/ice readily, a finding reported again in Rangno and Hobbs (1995-herafter RH95). Due to an oversight, or small-mindedness, however, F2015 do not cite the original altruistic[6] work from my 1986 11-week field investigation of Israeli clouds, work accomplished on my own initiative, time, and dime—namely, R88

[4] Those within the HUJ-SU had about ten years of viewing and recording storms on their Enterprise 5-cm radar by the time of my 1986 visit. Was no one "minding the store"?

[5] One can speculate that the clouds of Israel have been "highly efficient" rain producers for most of that five million years! (Maybe with a further contribution to efficiency by "dust-haze"?)

[6] Altruistic? I went to Israel after I became convinced that the people of Israel were likely paying for the seeding of unsuitable clouds, and that the cloud reports by the HUJ-SU were in substantial error. "Someone has to do something about this!", a "do-gooder" thought for Israel, and I thought I COULD do something due to my background in airborne cloud sampling and as a storm chaser, cloud photographer. I spent 11 weeks in Israel and lived off my savings in 1986 while preparing that manuscript (R88).

was “self-funded.”

In contrast to the positive writing mentioned, there are elements of writing in this paper that cast a dark shadow on the authors, their home institutions, and ultimately, the journal that their article appeared in, *AR*.   Partial, and therefore, misleading descriptions of the prior Israeli experiments are found in several places, along with omissions of important references to work that was critical of HUJ-CSU findings, thus representing a stunningly “gorgeous” exhibition of “one-sided citing” in Freud et al. 2015, as is often practiced by the HUJ-CSU.

3) Who should evaluate cloud seeding potential in the Golan? Not the HUJ-CSU

Perhaps the most surprising and troubling aspect of this paper is that the assessment of seeding potential in the Golan is being undertaken by HUJ-CSU; “troubling” due to that organization’s prior reporting history in that domain, i.e., its inability to detect the nature of Israel’s clouds for decades with so many tools at its disposal, and the inherent conflict of interest such a new study represents.

Philosopher George Santayana said it: "Those who cannot remember the past are condemned to repeat it."

Moreover, If the present “background” paper by the HUJ is the primary reason why a new randomized cloud seeding experiment, Israeli-4, was undertaken, then yes, the failed past of the HUJ-CSU’s seeding experiments (Israeli 1, 2 and 3) will be repeated providing that Israeli-4 is evaluated by ***independent***, non-HUJ-CSU statisticians and scientists!

The evaluation of the clouds in this article in *AR* is not complete enough to provide confidence that seeding is going to increase rain by an economically worthwhile amount in the Golan.   A new randomized experiment, or operational seeding, both appear to this reviewer to be unwise or at least, premature undertakings if based on this paper.

The sad history of the HUJ-CSU reporting in the cloud seeding domain is reprised for the reader or reviewer who is/was likely unaware of it so that the faults in this *AR* paper can be seen in the context of “damage control.” These faults also represent the “baggage” carried by the HUJ-CSU and their efforts to recoup the HUJ-CSU’s damaged credibility when they re-write and minimize key elements of that history in Freud et al (2015).

Or, perhaps it’s just the HUJ-CSU’s style of presenting scientific findings on cloud seeding?

The appearance of this article shows that the HUJ-CSU has yet one more chance (after Israeli 1,2, 3, and in their evaluations of operational seeding) to skew findings to impress an apparently still naïve Israel Water Authority (IWA) about seeding potential in the Golan Heights. This is the perfect example of the “fox guarding the hen house,” analogous to the Phelps-Dodge Mining Corporation being solely responsible for the environmental impact statement of its next mine. That Phelps-Dodge assessment *could* be right, but it would never be trusted as such.

Why doesn’t the IWA recognize this inherent, and, obvious to all familiar with the history of seeding in

Israel, HUJ-CSU conflict of interest?

Why weren't outside ***independent*** groups, such as one of those at the University of Wyoming, Stratton Park Engineering Company, NCAR, Tel Aviv University, etc., brought in by the IWA to make this assessment? The people of Israel and their media should be asking this question. The HUJ-CSU forfeited its right to do such an assessment decades ago.

Lastly, because of my experiences with the HUJ-CSU and their unreliability factor, I URGE the Israel Water Authority to have outside, ***independent*** groups experienced in airborne work collect data and report on the seeding potential of Israeli clouds in conjunction with critical supporting ground measurements at Mt. Hermon.

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4. The shifting Israeli cloud microstructure reports over the decades and their shifting implications on cloud seeding

Prior to the appearance of this HUJ-CSU article in *AR*, ***INDEPENDENT*** assessments (this cannot be overstressed) of the statistical results of seeding in Israel found them questionable at best (RH95, Silverman 2001[7], Levin et al 2010, 2011, the latter in reply to Ben-Zvi et al). Those ***independent*** assessments are strongly supported by clouds that we know are unsuitable for seeding due to the copious natural formation of ice at modest supercoolings. Indeed, the Israeli clouds have never been suitable for seeding in spite of the numerous published reports by the HUJ-CSU to the contrary.

Conversely, it was those erroneous ultra "ripe-for-seeding" cloud descriptions by the HUJ-CSU in the 1970s and 1980s that gave the statistical results of the Israeli 1 and 2 experiments credibility that seeding *really* had increased rain (e.g. Kerr 1982, Mason 1982, Dennis 1989).

Thus, HUJ-CSU authors cannot reasonably believe that *actual* rainfall increases occurred in Israeli 1 and 2 although they inform the *AR* reader of "positive effects" in the abstract. Seeding clouds whose high natural efficiency to form ice is as high as that anywhere in the world, cannot lead to statistically-significant results in a "static-style" cloud seeding experiments, as the Israeli experiments were. End of story.

5) The wrongful scientific "consensus" created by the HUJ-CSU

That the HUJ could not discover the true nature of their clouds until recently, and did not report results experiments in a timely manner also produced an erroneous "consensus" view in the science community that cloud seeding had been "proven" in Israel (e,g, Tukey et al. 1978, Mason 1980, 1982, Kerr 1982, American Meteorological Society 1984, Dennis 1989, World Meteorological Organization

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[7]Whom also goes uncited (strangely) since one of the current authors commented on his *BAMS* review. Small-mindedness here as well? Or is it the desire by the authors to hide alternative views from your funders and readers?

1992, Young 1993).

So what?

A wrongful consensus damages all of science!   This false "consensus" was published in numerous textbooks, and in countless popular articles along the lines of, "Israelis make it rain in the desert."

The essential question that should be asked by all of us is, "Why did it take so long for HUJ-CSU to discover the true nature of their clouds, given all the tools they've had available to them since the later 1970s?   The tools at their disposal included multiple radars, including a vertically-pointed 3-cm one (Gagin 1980), aircraft, IMS rawinsondes, and the skill of the IMS forecasters who were well aware of the efficiency (shallowness) of Israeli clouds when they begin to precipitate[8].

The inability of the HUJ-CSU to detect the nature of their clouds can be either seen as an example of astounding incompetence or one of investigable misconduct *if* those within the HUJ-CSU knew that their clouds were not as they described them in journals so many times; but rather, hid that knowledge from their peers and funders to keep their jobs, their seeding programs, and misbegotten prestige intact.

Donald Kennedy (2003), in a Science editorial on research fraud:

"In the real instances of research misconduct we know about in biology and physics, the motive appears to have been career enhancement, pure and simple. It is, after all, a competitive world, and the incentive to gain reputation can be powerful. But other motives may appear in those social sciences that bear upon major policy issues." (Always was and always will be—reviewer comment)

You decide.

The Israeli government is now attempting in Israeli-4 in the mountainous north of Israel to once again see if the clouds of Israel can be made to produce *economically useful* amounts of water via cloud seeding in an orographic setting. This because *none* of the prior Israeli experiments have credibility, results due to the belated discovery of efficiently raining clouds.

Gagin 1986: "While it is important to record the effects of seeding on rainfall at the ground, the statistical evaluation of this parameter ***alone*** cannot constitute an acceptable result of a successful seeding effect."   Q. E. D.

6. Mandated publically-available data requirements from the HUJ-CSU's recent cloud sampling program

[8] The several IMS forecasters I spoke with when I worked within the Israel Meteorological Service (IMS) in 1986 were well aware of the "efficiency" that Israeli clouds exhibited. One stated, "We get good rains out of clouds with tops at -10°C."

Due to the past history of partial reporting by the HUJ-CSU; namely, their inability to address flaws in their own experiments, omission of experimental results, and the gross errors in prior cloud microstructure assessments in which far *lower* than actual ice particle concentrations were reported, extra scrutiny of their cloud reports is mandatory to protect the Israeli people and its government from further abuses by the HUJ-CSU, some of which are demonstrated in this *AR* article.

For these reasons, this reviewer would have mandated before publication of this article and before any cloud seeding experimentation takes place that the HUJ-CSU provide online the following:

1) A table of flight data with dates and times of flights, linked to synoptic maps and satellite imagery for those flight days, and the ability to access flight videos from this program.

2) A table detailing some of the microstructural measurements, following Hobbs and Rangno (1985), Table 1:

- a) maximum ice particle concentrations found in each sampling zone on each day over widths of of 300 m and 1-km,
- b) cloud top temperatures and heights of sampled clouds
- c) cloud base temperatures and heights of sampled clouds
- d) flight level temperature at which sample was obtained,
- e) height of the sample below cloud top,
- f) widths[9] near cloud top of the clouds that were sampled,
- g) average and maximum liquid water content in each "study" cloud,
- h) sizes of droplets ≤13 µm diameter and ≥23 µm diameter within the H-M temperature zone of -2.5° to -8° C clouds,
- i) Large size tail of the FSSP droplet spectrum ("threshold diameter", after Hobbs and Rangno 1985)
- j) average and maximum droplet concentrations in study clouds,
- h) stage that the cloud was in when sampled

The mandated data elements for outside researchers to examine may seem like an undue burden to the HUJ-CSU since some of them require care to obtain. But these requirements have to be put in the perspective of how much the prior erroneous and incomplete reports by the HUJ-CSU cost not only the people of Israel in ineffective cloud seeding but also by neighboring Arab countries that undertook similar ineffective seeding operations, and finally, by the cost of the failed attempted replication of the ersatz Israeli results in Italy (List et al. 1999).

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[9] Width (and implicitly, cloud top lifetime) has a direct bearing on the production of precipitation (e.g., Schemenaur and Isaac 1984; Rangno and Hobbs 1991.

Nonetheless, the HUJ-CSU's idea that inland clouds reforming over the Golan Heights/Mt. Hermon region *might* have seeding potential has merit and is worthy of further investigations and corroboration of the results in this paper by ***independent*** groups. However, seeding potential there is not demonstrated in this paper as will be shown in subsequent commentaries.

Factors to consider in the extreme northern Israel seeding scenario: Cloud tops in northern Israel are usually significantly colder than those in the central and southern regions of Israel (GN74; RH95).

GN74 reported modal radar tops on rain days in the north target of Israeli 2 near -19°C, whereas in the south target of Israeli 2, they were -16°C. GN74 (written in 1972) in preliminary analyses, and used those temperature differences to explain why seeding was seemingly effective in the north target (more activation of AgI), but not in the south target due to less activation of AgI[10].

Today, however, due to R88, Levin 1992, 1994, Levin et al. 1996, and in this paper, we know that *both* cloud top temperatures given in GN74 would be associated with high ice particle concentrations as turrets mature; they would be highly unsuitable for seeding.

7. The Israeli 1 experiment as described by the authors and the counter evidence to that description; "filling in the blanks"

Abstract: "These clouds were seeded….producing statistically-significant positive effects…"

This partial description of results for the first two experiments in the authors' abstract should have been removed. Right from the start the HUJ-CSU authors began misleading less-informed *AR* readers, some of whom will likely not go beyond the abstract.

There are MANY reasons why the Israeli 1 rain increase results are suspect, all known by the authors of this paper. Nevertheless they deflected the full story about those reasons to, "positive effects." We reprise those reasons why no one any longer believes that Israeli 1 nor Israeli 2, have credibility as successes in rain production for *AR* readers who might have taken the faulty abstract *prima facie*:

1) The clouds of Israel have been found largely unsuitable for seeding as the authors themselves finally have discovered and now report in this paper, in agreement with several other studies, initially by R88. If there are no suitable clouds, then the statistical results are ones that cannot be due to seeding effects, but rather *must* be due to "lucky" random draws, or other mischief such as omitting data, cherry-picking controls, etc.

It is worthwhile to reprise GN74 concerning statistical results without a cloud foundation, a well-known principle within the weather modification community that has not been appreciated by the HUJ-CSU:

"…no statistical evaluation will be of real value unless these results are substantiated by detailed physical considerations."

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[10] This was the last mention of a numerical result of seeding in the south target of Israeli 2.

The Israeli statistical results no longer have any viable physical foundation. The clouds they previously described, so ripe with seeding potential, existed only in the experimenters' imaginations. It's the clouds that drive the credibility of the statistical results, not the other way around.

2) Evidence for a Type I statistical error in Israeli 1 was reported by the Chief Meteorologist of that experiment, Mr. Karl Rosner, who pointed out to Wurtele (1971) that the region that exhibited the highest statistical significance on Center seeded days, the "buffer zone" (BZ) between the two targets, could barely have been seeded ("5-10%" of the time). The HUJ seeding unit did not give this "red flag" enough attention; that is, they did not attempt to reconcile the wind analysis by Shimbursky (in GN74) with their own Chief Forecaster Rosner's evaluation.   In contrast to their Chief Meteorologist's view, they considered the BZ inadvertently seeded (GN74).

3) The BZ seeding issue was examined ***independently*** and in more detail by Rangno and Hobbs (1995-hereafter RH95). In RH95 was concluded, based on their wind analysis when rain was falling in Israel (when seeding would have been expected to be taking place). It was concluded that it would take a very bad pilot to have inadvertently seeded the BZ when instructed not to do so. This is due to the very narrow low-level wind envelope *concurrent* with rain in Israel.

The Shimbursky wind analysis in GN74, on the other hand, had only a once a day IMS rawin launches, which may or may not have been associated with clouds and rain, and thus, could not address the direction of winds solely coincident with rain falling in Israel at the time of the launch as did RH95.

4) Additional evidence for a Type 1 error in Israeli 1 was presented in RH95 due to more rain on seeded days in the immediate coastal zone of Israel over which the seeding aircraft virtually flew, a conclusion that was reached earlier by Gabriel et al 1967, also due to logistical considerations.

Rosenfeld (1997) in a grand series of speculations concerning Israeli 1, offered an alternative to RH95 and Gabriel et al's conclusions: that some airborne-released AgI dispersed downward in southwesterly or westerly flow, was caught in thin offshore flowing layer below the flight level and near the ground, went offshore and was ingested by seedable clouds (which we know do not exist; never did) at the right distance upwind for the AgI to rise up into the those offshore clouds, nucleate at appropriate levels (usually above 3.5 km ASL), grow into precipitation-sized particles that then fell out just in time on the Israeli coast, thus "explaining" the indications of a bias in storms on Center seeded days on the Israeli coast.

5) Too little seeding (an average of but 4 h per day and about 970 grams total of AgI, Gabriel 1967) was carried out in Israeli 1.   RH95 demonstrated that the area claimed to have had increased rainfall (under the seeding line, sidewind, downwind, in the target) was not commensurate with the amount of seeding material released in Israeli 1, reinforced by unsuitable-for-seeding clouds.

6) The line seeding carried out by a single aircraft flying 75 km up and down the Israeli coastline was deemed ineffective in having seeded enough clouds to have produced an effect (RH95); corroborated in

modeling studies by Levin et al. 1997.[11]

8. The Israeli 2 experiment as described by the authors and the counter evidence to that description; filling in more blanks

In Israeli 2, the HUJ-CSU, realizing the poor seeding strategy it had used in Israeli 1, added a second aircraft, and 42 ground generators in Israeli 2 (NRC-NAS, Panel on Weather Modification, 1975). In Israeli 1 there had been but a single aircraft, and four ground generators located in the far NE of Israel (GN74).

The authors omit for the *AR* reader, the fact that the north "positive" effect in Israeli 2 was found to be the product of an astoundingly one-sided random draw[12] for heavy storms throughout Israel (Gabriel and Rosenfeld 1990[13]), and also one that affected Lebanon, and Jordan (RH95). The extraordinary random draw saw the home of HUJ-CSU in the south target experience the most rain of all the stations available to RH95 *on north target seeded days*! Ignoring the report of extreme rain in the south target on its control days by Gabriel and Rosenfeld (1990), Rosenfeld and Farbstein (1992) proposed that dust-haze had caused overseeding of clouds in the south target when combined with AgI and rain had actually been decreased the rain. This report gained wide traction for a time.

Levin et al 2010 examined Israeli 2 in more detail than did RH95. They attributed the apparent seeding-induced extra rainfall in the interior of the north target as due to a bias in strong synoptic systems on north target seeded days, one that created a misperception of seeding effects (and of decreases in rain on south target seeded days (see also, Levin et al 2011, reply to Ben-Zvi et al.).

Ironically, the Levin et al. (2010) findings are strengthened in F2015 when the authors observed that the intrusion of large aerosols inland from the Mediterranean on windy days in the north increased rain efficiency of inland clouds.

The Israeli 2 experiment had several design options (GN74), the first of which was the "crossover" evaluation, the same evaluation mode as was used in Israeli 1. The true tragedy of Israeli 2, however, was in the omission of numerical results of random seeding in the south target and of the crossover result by the HUJ-CSU after 1974. It was an omission that kept the scientific community in the dark about how the full Israeli 2 had actually turned out.

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[11] Not cited by the HUJ-CSU in this paper. We expect studies that offer counter views to their findings not to be cited by these authors. Perhaps they are taught that at the HUJ?

[12] The reviewer believes that it is *critical* that the IWA or other organization certify that the list of random decisions provided prior to each season for Israeli 2 by statistician Ehud Shimbursky are, in fact, the ones that were used in Israeli 2. This reviewer is doubtful, to add yet another layer of darkness to this analysis. It's just too tempting for those in the cloud seeding realm to say that, "Yes, we seeded that heavy rainstorm."

[13] The authors omit the indication of the remarkable biased random draw of Israeli 2 experiment in which the rain in the south target was "statistically significant" in terms of standard error from average amounts on rain days (Gabriel and Rosenfeld 1990).

While it was true, as GN81 pointed out, that the larger area of seeding in the south in Israeli 2 reduced the correlation with the north, thus making seeding effects harder to detect compared with the Center/North crossover comparisons in Israeli 1, a crossover analysis based on the prior Center target gauges could have been presented with whatever caveats the authors wished to add.

Furthermore, rain gauges in the prior Center region could have been used to "replicate" the crossover analysis of Israeli 1. (The prior Center target gauges were in zones S1 and S3 of Gabriel and Rosenfeld (1990). However, it is clear from the SAR's of those regions in Gabriel and Rosenfeld (1990) that the crossover result of Israeli 1 was not going to be replicated in Israeli 2. At this point, if the GN81 had done what they should have, they could have, after displaying that null result, provided some thoughts on why it was.

It took 15 years after Israeli 2 ended, and that spurred by a letter writing campaign begun in the winter of 1986 by the Israeli experiments' own Chief Meteorologist, Mr. Karl Rosner, to "out" both those "crossover results (-2%) and the SAR for the south target. And this was ONLY after the lead HUJ-CSU experimenter passed in 1987.

We have to assume that without Mr. Rosner's public call for the exposition of the south target results, they would still be hidden within the HUJ-CSU. Why weren't authors, Gabriel and Rosenfeld, and others at the HUJ, troubled by this omission over the years after Israeli 2 ended in 1975[14]? (We don't know why.)

Furthermore, the early reports of a "confirmation" of the results of Israeli 1 due to the partial reporting of an Israeli 2 "success," limited to the north target in a target-control evaluation, spurred the decision by the IWA to begin an "operational seeding" program in 1975 that produced no viable results for more than 30 years.[15] And to begin Israeli 3 in the south target where seeding had not been effective. Did the IWA know about the south target results of Israeli 2 before they began Israeli 3?

Where, too, was the outside cloud seeding community, one that failed to raise post-publication questions about the results of random seeding in the south target of Israeli 2? (I count myself in this oversight…and blame.) Statistician Jerzy Neyman, who closely monitored cloud seeding publications and had previously commented on them, would surely have caught the Israeli 2 omission had he not passed in 1980. There is a lot of blame to go around.

Not reporting the south target results of Israeli 2 that suggested decreases in rain on seeded days also suppressed the inevitable questions that would have arisen: "How could there be a suggestion of decreased rain on seeded days with such ultra "ripe-for-seeding" clouds (with warmer tops than in the north) as have been described by the HUJ-CSU over so many years?

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[14] From the Weather Modification Association's Code of Ethics: "Falsification: changing or not reporting appropriate data or results (i.e. the purposeful omission of conflicting data or information with the intent to falsify results; deceptive selective reporting). Isn't that the purpose of "one-sided" citing as well? Of note: The American Meteorological Society does not have a Code of Ethics, but rather "suggestions"or"guidelines" for professional conduct.

[15] See the discussion of operational seeding below for details.

Without doubt, the wheels would have come off the HUJ-CSU's seeding train with full reporting of Israeli 2 in a timely manner.

We reprise the speculation of Gabriel and Rosenfeld (1990) near the end of their statistical analyses, one that was to be confirmed by Levin et al. in 2010:

"The most plausible explanation (for the statistical results of Israeli 2) is one of random variation, with the north-seeded days being more rainy inland, especially towards the northern and southern edges of the experimental region."

9) The reported effect of seeding on Israeli 2 clouds: where it stands today

Almost the *entire* supposed seeding effect claimed in the Israeli 2 experiment was due to a greater duration of rain, not greater intensity or more frequent rain events (GN81, Gagin and Gabriel 1987). Seeding with AgI, they reported, caused cold-topped (-15°C to -21°C), *non-precipitating* clouds to precipitate exactly like natural clouds, thus extending the duration of rain on seeded days (north target evaluation only).

That duration finding no longer makes sense in the face of the highly efficiently precipitating Israeli clouds. It never really did upon close ***independent*** inspection.

10. The Israeli 3 experiment: its delayed reporting, and its significance

The first interim report about the progress of Israeli 3 randomized experiment from the HUJ-CSU was in 1992 (Rosenfeld and Farbstein), 15 years after it began[16]. Decreases in rain were suggested in that experiment year after year. The final result, reported by Rosenfeld (1998) at conference, was that after 19 winter seasons and nearly 1000 random decisions there was an indication of a decrease in rain of 8% on seeded days (non-statistically significant).

In F2015, the authors, following in the footsteps of GN76, cannot divulge for the reader the indication of decreased rain on seeded days in Israeli 3 as did GN76 for the Israeli 2 south target with devastating consequences, but rather obscure that important fact for Israeli 3 with the word, "inconclusive."

The 8% suggested reduction in rainfall on seeded days after 19 winters and nearly 1000 random decisions were important on two accounts: that 1000 random decisions after 19 winter seasons could lead to a result far from a null one, assuming there really was no seeding effect as Rosenfeld (1998) asserted.

Second, Israeli 3 also demonstrated that the clouds of Israel are unsuitable for glaciogenic cloud seeding. There is nothing particularly different between the clouds that affect the central and southern regions of Israel from those that affect the north in terms of microstructural behavior except that the clouds in

[16] Sound familiar? We repeat a WMA Code point for your consideration: falsification: changing or not reporting appropriate data or results (i.e. the purposeful omission of conflicting data or information with the intent to falsify results; deceptive selective reporting).

northern Israel are overall generally colder from top to bottom.

And, who would undertake a randomized seeding experiment for 19 years knowing you might have a natural draw so far from zero that can't even be overcome by an actual 10% seeding-induced increase in rain[17]?

11. Operational seeding: the descriptions by the authors and the counter evidence to those descriptions, filling in still more blanks

HUJ-CSU: "Subsequently operational seeding in the north of Israel was carried out between 1975 and 2013."

The above quote by the HUJ-CSU authors', writing in 2015 in *AR*, shows that they cannot bring themselves to report that the "operational seeding" program in the north of Israel, as it was originally formulated, was terminated in 2007 (Sharon et al 2008[18]). We quote Sharon et al. 2008 for the reader:

"3b. A revision and elaboration of possible future seeding activities
In view of Kessler et al.'s initial finding in Section 2a and the ensuing controversy, a joint forum of the national Water Authority and other professionals involved, met early in 2007. After 50 years of uninterrupted seeding activities, the forum decided to discontinue the program at the end of that rainy season (April 2007) and instead, consider the initiation of a new updated experiment, Israeli IV."

Why was the original program terminated by the IWA[19]?

Answer: The ***independent*** panel of experts (Kessler et al 2002, Kessler et al. 2006, distilled by Sharon et al 2008) could find no additional runoff due to seeding into the target, Lake Kinneret (aka, Sea of Galilee) over the 30 plus years of seeding. They also found that the small increases (6%) reported by Nirel and Rosenfeld (1994); Rosenfeld and Nirel (1996) could not be substantiated when more data from later years were used. This null finding for the operational program by Kessler et al. was corroborated by ***independent*** scientists at Tel Aviv University (Levin et al. 2010, 2011, the latter a reply to Ben-Zvi et al 2011).

At this point, one wonders who the reviewers of an article in the respected journal, *AR*, were? How

[17] We can only imagine how these results would have been spun around if this had been a suggestion of an 8% increase in rain. How many statistical tests would be tried? Would the authors really just say, "no significant effect"?

[18] Sharon et al (2008), and the reports that it was distilled from by an ***independent*** panel (Kessler et al. 2002, 2006), are nowhere to be found in the HUJ-SU authors' paper. Should we be surprised at this point? I don't think so. This is not science as we know it. HUJ, are you listening?

[19] While the original IWA program was terminated, some additional experimental-operational seeding did carry on, beginning with the 2007-08 winter.

could they let such partial statements and an article absent vital references appear? One must also assume that the reviewers for *AR* were woefully ignorant of the history of the Israeli experiments to have allowed the HUJ-CSU distortions reach the print stage of a major journal. Truthfulness and a full exposition of events is not part of the HUJ-CSU's understanding of how to compose a manuscript for a scientific journal. Their manuscripts and publications, in essence, need to not just be reviewed, but "policed" for accuracy.

HUJ-CSU: "Additional statistical analyses showed that the orographic precipitation responded most sensitively to the seeding experiments (Givati and Rosenfeld 2005)."

The HUJ-CSU authors' claim in the above publication, that air pollution had decreased precipitation *exactly* by the amount that seeding was increasing it in the interior of Israel (thus explaining the lack of operational seeding effects reported by the ***independent*** panel) could not be ***independently*** substantiated by either (Halfon et al 2009a[20], b) nor by Levin et al. 2010. The pollution claim by Givati and Rosenfeld (2005) was found to be ambiguous at best.

It is useful to quote Thom (1957) on how Givati and Rosenfeld came up with their findings (as was demonstrated by Halfon et al 2009):

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

That the results of operational seeding as reported by the HUJ-CSU could not be validated by ***independent*** researchers on four occasions (Kessler et al 2002, Kessler et al. 2006, Halfon et al 2009, and Levin et al. 2010, 2011) is one of the *most* important aspects of the seeding history in Israel. For those who know this story, we again see one-sided citing; the HUJ-CSU in F2015 cannot bring themselves to cite those publications, damaging those ***independent*** workers, themselves, and whether they realize it or not, their own home institutions. HUJ, are you listening?

12. A critique of the cloud sampling and effects on the data reported in F2015

Average concentrations in "hard", newly risen turrets over the Mediterranean, as the authors show in Figure 16 from their Mediterranean cloud targets, produces a bias toward low ice particle concentrations and higher liquid water contents. The authors surely know this. The authors are to be commended for adding the description of the stage that the Mediterranean clouds were in when they were sampled. This really helps the experienced reader weight those "averages" in the Figure 16.

But, did the authors' also sample "hard" but meteorologically inconsequential, slender, "chimney" Cu as well in Figure 16? These are ones we know produce little ice/precip compared to their wider brethren

[20] Halfon et al 2009, and the exchange between Givati and Rosenfeld, also go uncited, strengthening a pattern of deception by the HUJ-SU; such actions by the authors that can no longer be attributed to oversights, but are meant to keep readers in the dark about the quality of the HUJ-CSU research.

even if they are at the SAME cloud top temperature (e.g., Schemenauer and Isaac 1984, Rangno and Hobbs 1991).

That kind of sampling error of penetrating only taller, narrow clouds instead of the wider complexes that produce appreciable rain in Israel was one that was made in the original reports coming out of the HUJ-CSU in the 1970s, ones that misled the HUJ-CSU on Israeli clouds-ice content, and ultimately their journal audience (e.g., GN74, Gagin 1975).

Cloud top width, in particular, is an extremely important metric and should have been divulged by the authors.

A bias toward low ice concentrations can still result if the authors sampled wider, high ice-producing complexes, but only penetrated the very newly risen turrets in them in which the explosion of ice had not yet occurred.

Lastly, another critical metric for understanding the quality of the HUJ-CSU authors' measurements in Fig. 16 is the height of sampling below cloud top. Sampling too close to cloud top, say, tens of meters instead of a few hundred meters, also leads to a low detection of precipitation-sized particles (and also why R_e satellite derivations can mislead in newly risen turrets, and ice-spewing water-topped layer clouds common in orographic settings.

The HUJ-CSU: trouble with ice

But the HUJ-CSU has carried out several airborne sampling programs with modern hydrometeor probes such as a 2DC probe over the past 25 years, since the middle 1990s (Rosenfeld and Lensky 1998, Lahav and Rosenfeld (2000), Rosenfeld et al. 2001), and here in F2015. Ice particle concentrations were not divulged by the HUJ-CSU in the first three publications. In F2015, we have at last "average" concentrations obtained in "hard" turrets over the Mediterranean Sea. Ice multiplication in the clouds of Israel, except for the few observations of Levin et al. 1996, remains an unknown even after so many flights by the HUJ-CSU. While F2015 are to be congratulated as the first paper generated by the HUJ-CSU to report ice particle concentrations in Israeli clouds since Gagin (1975), "average" concentrations are unsatisfactory in the study of ice multiplication, a staple of cloud microstructural studies over the past 60 years. Why can't they report the degree of ice multiplication in Israeli clouds, a focal point of those reports when ice particle concentrations WERE reported by the HUJ-CSU from airborne studies in the 1970s?

This discussion above begs the question about why F2015 didn't target mature and dissipating portions of Mediterranean clouds *after* high ice particle concentrations had formed? Its inexplicable. Due to the authors' mode of sampling and reporting, one can't relate cloud top temperatures to the amount of ice that was produced in the clouds they sampled, and, therefore, the degree of ice multiplication in the clouds cannot be calculated. Ice multiplication values have been a staple of airborne programs since Koenig (1963), but the HUJ-CSU seems to have trouble honoring this standard.

Nor can their measurements be compared to the concentrations of ice in Israeli clouds reported by Levin

1992, 1994, L96).

Reporting ice particle concentrations seems to be an anathema for the HUJ-CSU when they go airborne.

Rosenfeld and Lensky (1998) flying on shower days in pursuit of a comparison between satellite and aircraft measurements of R_e,wrote that they did not carry a probe for hydrometeors on their aircraft (!). Lahav and Rosenfeld (2000), in a ten flight sampling program having a 2-DC probe, nevertheless refrained from reporting ice or other hydrometeor concentrations, while titling their paper, "Microphysical Characterizations of the Israeli Clouds from Aircraft and Satellites." But that's not what they did.

Rosenfeld et al. 2001 had a 2-DC on their research aircraft, but once again, refrained from reporting concentrations of hydrometeors in the clouds they sampled, only noting that there was a "large number", or that they were" increasing" or "decreasing." What's going on here?

Reporting maximum ice particle concentrations relative to cloud top temperatures to evaluate the degree of ice multiplication, has been a staple of airborne sampling since the first planes took off to sample clouds (e.g., Koenig 1963). Ice particle concentrations, and their origin has been one of the continuing mysteries of cloud microstructure (e.g., Mossop 1970, 1985); in particular secondary ice that forms in clouds with top temperatures warmer than about -15°C. Surely *AR* readers know this, and are aware of the 2017 *Amer. Meteor. Soc. Monograph* (Field et al.[21]) focused on the origin of secondary ice.

But the omission of maximum concentrations associated with cloud top temperatures by the HUJ-CSU does not permit us to evaluate ice multiplication for Israeli clouds beyond that of L96. Israel is in a region of confluence for various aerosols that affect clouds, and reports of ice multiplication in them are completely absent. What a shame.

This reviewer's guess, from his 1986 Israeli field project, is that the HUJ-CSU finds that they have an embarrassment of ice particle "riches" and wants to keep those concentrations and the degree of ice multiplication away from readers and funders who might consider cloud seeding. Ice multiplication has always been considered an impediment to successful "static" glaciogenic cloud seeding.

Is there another motive by the HUJ-CSU for omitting maximum concentrations from us?

In fact, from this reviewer's experience in Israel, and from the reports of Levin (1992, 1994, L96), 10s to 100s per liter of ice particles would be expected to form explosively in Israeli clouds at modest top supercoolings in less than 10 minutes, and not "tens of minutes" as the authors assert.

Gagin (1975) claim that ice particle concentrations do NOT increase with time could have been evaluated, too.

Moreover, those very same ice-filled clouds over the Mediterranean are going to be swept downwind

[21] "Cast of thousands!"

and into the Golan within ~30 min during rainy spells, making it doubly important to have reported those *maximum* concentrations and their evolution downstream in route to the Golan.

In sum, yes, you CAN mislead with an aircraft, even one with a full complement of cloud microstructure instruments via omission and sampling biases.

The points raised in the foregoing section concerning the airborne sampling carried out by the HUJ-CSU emphasizes all too well why in-flight videos must be made publically available since they are crucial to understanding exactly what was sampled, and how, by the HUJ-CSU over the Mediterranean and elsewhere in Israel. We hope the IWA will **mandate** that this be will done by the HUJ-CSU in a timely manner.[22]

To summarize the dark history of the HUJ-CSU: over a period of several decades, it misled their own people and the world repeatedly about their clouds, withheld statistical results for Israeli 2 that would have raised so many questions, delayed for 15 years reporting results from the Israeli 3 randomized experiment that, too, would have raised numerous questions, and can't seem to publish reliable results in their cloud seeding work. Every paper they have published in that domain, when examined by outside, ***independent*** investigators, has been found to be unreliable. Those faulty reports by the HUJ-CSU cost the people of Israel millions of dollars wasted on ineffective "operational" seeding (as evaluated by ***independent*** researchers). One can predict confidently that in the future the HUJ-CSU will, without major changes to its leadership, and without a more skeptical IWA concerning the claims coming out of that group, "repeat history."

[22] The University of Washington's Cloud and Aerosol Group's flight videos from 1986 through 2001 are available through NCAR and the University of Washington upon request.

The following is a reproduction of the 2015 article that appeared in *AR* with highlighted statements by the HUJ-CSU authors followed by this reviewer's comments. Some repetition is present from the above segment, but, what the HECK…

Abstract

The modest amount of rainfall in Israel occurs in winter storms that bring convective clouds from the Mediterranean Sea when the cold post frontal air interacts with its relatively warm surface.

Minor: shorten to, "Most of the rainfall in Israel…."

These clouds were seeded in the Israel-1 and Israel-2 cloud glaciogenic seeding experiments, which have shown statistically significant positive effect of added rainfall of at least 13% in northern Israel, whereas the Israel-3 experiment showed no added rainfall in the south. This was followed by operational seeding in the north since 1975.

(Repeated from p8)

This partial description of results for the first two experiments in the authors' abstract should have been removed. Right from the start the HUJ-CSU authors began misleading less-informed *AR* readers, some of whom will likely not go beyond the abstract.

There are MANY reasons why the Israeli 1 rain increase results are suspect, all known by the authors of this paper. Nevertheless they deflected the full story about those reasons to, "positive effects." We reprise those reasons why no one any longer believes that Israeli 1, nor Israeli 2, have credibility as successes in rain production for *AR* readers who might have taken the faulty abstract *prima facie*:

1) The clouds of Israel have been found largely unsuitable for seeding as the authors themselves finally have discovered and now report in this paper, in agreement with several other studies, initially by R88. If there are no suitable clouds, then the statistical results are ones that cannot be due to seeding effects, but rather *must* be due to "lucky" random draws, or other mischief such as omitting data, cherry-picking controls, etc.

It is worthwhile to reprise GN74 concerning statistical results without a cloud foundation, a well-known principle within the weather modification community:

"…no statistical evaluation will be of real value unless these results are substantiated by detailed physical considerations."

The Israeli statistical results no longer have any viable physical foundation. The clouds they previously described, so ripe with seeding potential, existed only in the experimenters' imaginations. It's the clouds that drive the credibility of the statistical results, not the other way around.

2) Evidence for a Type I statistical error in Israeli 1 was reported by the Chief Meteorologist of that experiment, Mr. Karl Rosner, who pointed out to Wurtele (1971) that the region that exhibited the highest statistical significance on Center seeded days, the "buffer zone" (BZ) between the two targets, could barely have been seeded ("5-10%" of the time). The HUJ seeding unit did not give this "red flag" enough attention; that is, they did not attempt to reconcile the wind analysis by Shimbursky (in GN74) with their own Chief Forecaster Rosner's evaluation. In contrast to their Chief Meteorologist's view, they considered the BZ inadvertently seeded (GN74).

3) The BZ seeding issue was examined ***independently*** and in more detail by Rangno and Hobbs (1995-hereafter RH95). In RH95 was concluded, based on their wind analysis when rain was falling in Israel (when seeding would have been expected to be taking place). It was concluded that it would take a very bad pilot to have inadvertently seeded the BZ when instructed not to do so. This is due to the very narrow low-level wind envelope *concurrent* with rain in Israel.

The Shimbursky wind analysis in GN74, on the other hand, had only a once a day IMS rawin launches, which may or may not have been associated with clouds and rain, and thus, could not address the direction of winds solely coincident with rain falling in Israel at the time of the launch as did RH95.

4) Additional evidence for a Type 1 error in Israeli 1 was presented in RH95 due to more rain on seeded days in the immediate coastal zone of Israel over which the seeding aircraft virtually flew, a conclusion that was reached earlier by Gabriel et al 1967, also due to logistical considerations.

Rosenfeld (1997) in a grand series of speculations concerning Israeli 1, offered an alternative to RH95 and Gabriel et al's conclusions: that some airborne-released AgI dispersed downward in southwesterly or westerly flow, was caught in thin offshore flowing layer below the flight level and near the ground, went offshore and was ingested by seedable clouds (which we know do not exist; never did) at the right distance upwind for the AgI to rise up into the those offshore clouds, nucleate at appropriate levels (usually above 3.5 km ASL), grow into precipitation-sized particles that then fell out just in time on the Israeli coast, thus "explaining" the indications of a bias in storms on Center seeded days on the Israeli coast.

5) Too little seeding (an average of but 4 h per day and about 970 grams total of AgI, Gabriel 1967) was carried out in Israeli 1. RH95 demonstrated that the area claimed to have had increased rainfall (under the seeding line, sidewind, downwind, in the target) was not commensurate with the amount of seeding material released in Israeli 1, reinforced by unsuitable-for-seeding clouds.

6) The line seeding carried out by a single aircraft flying 75 km up and down the Israeli coastline was deemed ineffective in having seeded enough clouds to have produced an effect (RH95); corroborated in

modeling studies by Levin et al. 1997.[23]

So, how did such descriptions by the HUJ-CSU authors of past Israeli statistical significance without the full story get into the abstract of a peer-reviewed journal?

HUJ-CSU, Abstract: "Subsequently operational seeding in the north of Israel was carried out between 1975 and 2013."

The above quote by the HUJ-CSU authors', writing in 2015 in *AR*, shows that they cannot bring themselves to report that the "operational seeding" program in the north of Israel, as it was originally formulated, was terminated in 2007 (Sharon et al 2008[24]). We quote Sharon et al. 2008 for the reader:

"3b. A revision and elaboration of possible future seeding activities
In view of Kessler et al.'s initial finding in Section 2a and the ensuing controversy, a joint forum of the national Water Authority and other professionals involved, met early in 2007. After 50 years of uninterrupted seeding activities, the forum decided to discontinue the program at the end of that rainy season (April 2007) and instead, consider the initiation of a new updated experiment, Israeli IV."

Why was the original program terminated by the IWA[25]?

Answer: The ***independent*** panel of experts (Kessler et al 2002, Kessler et al. 2006, distilled by Sharon et al 2008) could find no additional runoff due to seeding into the target, Lake Kinneret (aka, Sea of Galilee) over the 30 plus years of seeding. They also found that the small increases (6%) reported by Nirel and Rosenfeld (1994); Rosenfeld and Nirel (1996) could not be substantiated when more data from later years were used. This null finding for the operational program by Kessler et al. was corroborated by ***independent*** scientists at Tel Aviv University (Levin et al. 2010, 2011, the latter a reply to Ben-Zvi et al 2011).

At this point, one wonders who the reviewers of an article in the respected journal, *AR*, were? How could they let such partial statements and an article absent vital references appear? One must also assume that the reviewers for *AR* were ignorant of the history of the Israeli experiments to have allowed the HUJ-CSU distortions reach the print stage of a major journal. Truthfulness and a full exposition of events is not part of the HUJ-CSU's understanding of how to compose an manuscript for a scientific

[23]Not cited by the HUJ-CSU in this paper. We expect studies that offer counter views to their findings not to be cited by these authors. Perhaps they are taught that at the HUJ?

[24] Sharon et al (2008), and the reports that it was distilled from by an ***independent*** panel (Kessler et al. 2002, 2006), are nowhere to be found in the HUJ-SU authors' paper. Should we be surprised at this point? I don't think so. This is not science as we know it. HUJ, are you listening?

[25] While the original IWA program was terminated, some additional experimental-operational seeding did carry on, beginning with the 2007-08 winter.

journal. Their manuscripts and publications, in essence, need to not just be reviewed, but “policed” for accuracy.

HUJ-CSU Abstract:

The lack of physical evidence for the causes of the positive effects in the north caused a lack of confidence in the statistical results and led to the Israel-4 randomized seeding experiment in northern Israel.

The authors’ abstract should have begun here, with the claim, “positive effects in the north” removed.

It begs the question about why the HUJ-CSU authors would cite statistical results if no one believes in them? At least here, they acknowledge a common truth; without a cloud foundation, statistical results are not credible as true rain increases via seeding (as noted so long ago by GN74, and is a standard requirement for credible cloud seeding results).

In Israeli 2, the HUJ-CSU, realizing the poor seeding strategy it had used in Israeli 1, added a second aircraft, and 42 ground generators in Israeli 2 (NRC-NAS, Panel on Weather Modification, 1975). In Israeli 1 there had been but 4 ground generators located in the far NE of Israel (GN74).

The authors omit for the *AR* reader, the fact that the north “positive” effect in Israeli 2 was found to be the product of an astoundingly one-sided random draw for heavy storms throughout Israel (Gabriel and Rosenfeld 1990[26]), and also one that affected Lebanon, and Jordan (RH95). The extraordinary random draw saw the home of HUJ-CSU in the south target experience the most rain of all the stations available to RH95 *on north target seeded days*!

Levin et al 2010 examined Israeli 2 in more detail than did RH95. They ascribed the apparent seeding-induced extra rainfall in the interior of the north target as due to a bias in strong synoptic systems on north target seeded days, one that created a misperception of seeding effects (see also, Levin et al 2011, reply to Ben-Zvi et al.).

Ironically, the Levin et al. (2010) findings are strengthened by the HUJ-CSU authors’ in this paper when the authors observed that the intrusion of large aerosols inland from the Mediterranean on windy days in the north increased rain efficiency of inland clouds.

The true tragedy of Israeli 2, however, was in the omission of numerical results of random seeding in the South target by the HUJ-CSU after 1974 that kept the scientific community in the dark about how the full experiment had actually turned out.

The Israeli 2 experiment had several design options (GN74), the first of which was the “crossover” evaluation, the same evaluation mode as was used in Israeli 1. However, after the first initial reports of seeding results in each target in GN74 (but no crossover result), the HUJ-CSU decided not to report the

[26]The authors omit the indication of the remarkable biased random draw of Israeli 2 experiment in which the rain in the south target was “statistically significant” in terms of standard error from average amounts on rain days (Gabriel and Rosenfeld 1990).

crossover result of Israeli 2 after that time (i.e., GN74, Gagin and Neumann 1976; 1981, hereafter GN76 and GN81). Nor did they report the numerical "single area ratio" (SAR) for the south target, one that suggested rain had been decreased on seeded days in Israeli 2 (in fact, an artifact of the Israel-wide heavy rains on north target seeded days; those days were the control days for the south target's SAR).

It took 15 years after Israeli 2 ended, and that spurred by a letter writing campaign begun in the winter of 1986 by the Israeli experiments' own Chief Meteorologist, Mr. Karl Rosner, to "out" both those "crossover results (-2%) and the SAR for the south target (Gabriel and Rosenfeld 1990). And this was ONLY after the lead HUJ-CSU experimenter passed.

We have to assume that without Mr. Rosner's public call for the exposition of the south target results, they would still be hidden within the HUJ-CSU. Why weren't authors, Gabriel and Rosenfeld, and those at the HUJ, troubled by this omission over the years after Israeli 2 ended in 1975[27]?

Furthermore, the early reports of a "confirmation" of the results of Israeli 1 due to the partial reporting of an Israeli 2 "success," limited to the north target in a target-control evaluation, spurred the decision by the IWA to begin an "operational seeding" program in 1975 that produced no viable results for more than 30 years.[28]

Where was the outside cloud seeding community, one that failed to raise post-publication questions about the results of random seeding in the south target of Israeli 2? (I count myself in this oversight…and blame.) Statistician Jerzy Neyman, who regularly commented on cloud seeding, would surely have caught the Israeli 2 omission had he not passed in 1980.

Not reporting the south target results of Israeli 2 that suggested decreases in rain on seeded days also suppressed the inevitable questions that would have arisen: "How could there be a suggestion of decreased rain on seeded days with such ultra "ripe-for-seeding" clouds (with warmer tops than in the north) as have been described by the HUJ-CSU over so many years?

Without doubt, the wheels would have come off the HUJ-CSU's seeding train with full reporting of Israeli 2 in a timely manner.

Abstract: This experiment started in the winter of 2013/14. The main difference from the previous experiments is the focus on the orographic clouds in the catchment of the Sea of Galilee. The decision to commence the experiment was partially based on evidence supporting the existence of seeding potential, which is reported here.

In retrospect, concerning this statement, how sad for the IWA and the Israeli people if they relied on the results of this inadequately reviewed article to make a "go" or "no go" decision on renewed

[27] From the Weather Modification Association's Code of Ethics: "Falsification: changing or not reporting appropriate data or results (i.e. the purposeful omission of conflicting data or information with the intent to falsify results; deceptive selective reporting). Isn't that the purpose of "one-sided" citing as well? Of note: The American Meteorological Society does not have a Code of Ethics, but rather "suggestions"or"guidelines" for professional conduct.

[28] See the discussion of operational seeding for details.

seeding. It can only be fervently hoped that the evaluation is by certifiably ***independent*** statisticians…

Abstract: Aircraft and satellite microphysical and dynamic measurements of the clouds document the critical roles of aerosols, especially sea spray, on cloud microstructure and precipitation forming processes. It was found that the convective clouds over sea and coastal areas are naturally seeded hygroscopically by sea spray and develop precipitation efficiently. The diminution of the large sea spray aerosols farther inland along with the increase in aerosol concentrations causes the clouds to develop precipitation more slowly. The short time available for the precipitation forming processes in super-cooled orographic clouds over the Golan Heights farthest inland represents the best glaciogenic seeding potential.

A short time available for precip development also mitigates against seeding since it would normally take at least 30 min for a seeding effort to be realized at the ground. Seeding at the first sign of a supercooled layer cloud having a temperature below -5°C above the west ridge of the Golan would then travel between about 18 and 36 km for 700 mb winds between 10 and 20 $m\ s^{-1}$. Surely, as GN74 wrote, the seeders in Israeli-4 would target lower temperatures where crystal growth is more rapid (-15° to -20°C). But lower temperatures would mean higher altitudes, near 5 km stronger winds and longer times for to crystals to reach the ground.

This seeding experiment, unless they are measuring precipitation in Syria, seems infeasible.

The authors' "Introduction" section

Glaciogenic cloud seeding has been conducted in Israel since the early 1960s by seeding silver iodide (AgI) from acetone burners both from aircraft that fly along seeding lines upwind of the target areas and from ground generators. The seeding efficacy was evaluated by three randomized experiments (Israel-1: 1961–67, Israel-2: 1969–75 and Israel-3: 1975–95). Subsequently, operational seeding in the north of Israel was carried out between 1975 and 2013.

This statement contradicts Sharon et al. 2008, who wrote:

"3b. A revision and elaboration of possible future seeding activities
In view of Kessler et al.'s initial finding in Section 2a and the ensuing controversy, a joint forum of the national Water Authority and other professionals involved, met early in 2007. After 50 years of uninterrupted seeding activities, the forum decided to discontinue the program at the end of that rainy season (April 2007) and instead, consider the initiation of a new updated experiment, Israeli IV."

The knowledgeable reader will want to know, which statement is true: that by Sharon et al (2008), and why it was terminated, or what is stated above in Freud et al 2015, with no mention of termination?

Are Freud et al. unaware of the Sharon et al 2008 paper? They do not cite it anywhere so it would appear so. Or, is this still another example of deliberate “citing misconduct” by the HUJ-CSU?

The statistical analyses of the first two randomized experiments (Israel-1 and 2) showed precipitation enhancement of 15% and 13%, respectively.

Prior to the parenthetical “Israel 2”, insert, “the partial reporting of Israeli 2”). This is painful for everyone, but it’s what happened.

That partial reporting by the HUJ was critical in misleading the weather mod community and the world into believing a replication of Israeli 1 had occurred in Israeli 2. It also suppressed the inevitable questions that would have arisen with suggestions of decreased rain on seeded days in the south target. How could there be failed seeding in the south target with such ripe for seeding clouds.

Furthermore, results in the north have been attributed to synoptic factors, not ones caused by cloud seeding, by Levin et al. 2010. Ironically, Levin et al’s findings are supported further by the intrusion of large aerosols inland that led to increased rain efficiency as reported in this very paper.

Text: The indicated effects were larger at a distance of 25–50 km from the seeding line (Gabriel and Rosenfeld, 1990; Gagin and Neumann, 1974, 1981).

When the synoptically produced bias in the coastal zone single ratios are removed, the slightly higher inland ratios are no longer statistically-significant. Those single ratios in the coastal zones of the north and south are as high as those calculated for each targets (RH95).

The third experiment (Israel-3) was conducted in the south of Israel and showed no significant effect (Rosenfeld and Farbstein, 1992).

. The authors’ failed to mention the sign of the non-significant result; some readers will be misled and might believe it was an indication of increased rain that wasn’t large enough to reach statistical significance.

The first interim report about the progress of Israeli 3 randomized experiment was in 1992 (Rosenfeld and Farbstein), 15 years after it began. Decreases in rain were suggested in that experiment year after year. The final result, reported by Rosenfeld (1998) at conference, was an indication of a decrease in rain of 8% on seeded days after 19 winter seasons and nearly 1000 random decisions.

The 8% suggested reduction in rainfall on seeded days after 19 winters and nearly 1000 random decisions were important on two accounts: that 1000 random decisions after 19 winter seasons could led to a result so far from a null one, assuming there really was no seeding effect as Rosenfeld (1998) asserted

Second, Israeli 3 demonstrated that the clouds of Israel are unsuitable for seeding purposes. There is nothing different between the clouds that affect the central and southern regions of Israel from those that affect the north in terms of microstructural behavior except that the clouds in the north are generally colder in spite of so-called dust influences cited in this paper.

It has never been shown in any of those papers cited that the dust got high enough into Israeli clouds to effect nucleation at temperatures below -5°C to -10°C where it MIGHT trigger ice. Assumptions won't do as proof here. More definitive airborne work is needed.

Second, who would undertake a randomized seeding experiment for 19 years knowing you might have a natural draw so far from zero that it can't even be overcome by a 10% seeding-induced increase in rain[29]?

These results have been debated and a few studies have claimed that the reported enhancement in precipitation was actually a result of meteorological bias or statistical errors (Levin et al., 2010; Rangno and **Robbs** 1995), despite the statistically significant positive results in the first two experiments.

The portion of the statement "…or statistical errors" were debated is not clear. I think what they mean is that in ***independent*** reanalyses it was found that a meteorological biases *produced* Type 1 statistical errors.

The authors omit the ***independent*** review of the Israeli experiments by Silverman (2001) who concluded that the Israeli experiments should no longer be cited as having proved cloud seeding, as had RH95 earlier, Levin et al. (2010) for Israeli 2.

Furthermore, the authors do not give all of the factors why the indications of seeding results on rainfall, as reported by the HUJ-CSU in Israel 1 and 2, are to be doubted. They omit (again) the primary reason; unsuitable clouds for glaciogenic "static" seeding. With unsuitable clouds (as reported by R88, Levin 1992, L96) and in this paper, there can only be experimenter or random draw biases that produce an appearance of rainfall increases.

The ultra "ripe-for-seeding" Israeli clouds, lacking in much or having no ice until their tops reached -21°C as described by the HUJ-CSU on numerous occasions (Gagin 1975, 1980, GN74, 1976, 1981, 1986) existed only in their imaginations.

Its too bad the HUJ's Department of Earth Sci. did not have competent scientists within their domain to correct those early faulty reports, ones taken so seriously by leading scientists (e.g., Hallett and Mossop 1974, Mason 1982, American Meteorological Society 1984, Dennis 1989).

Minor: The misspelling of second author in RH95, Hobbs as "Robbs", is yet another sign of a poor manuscript review and/or of *AR* editing.

These claims have to be weighed against the mathematical fact that the likelihood of this

[29] We can only imagine how these results would have been spun around if this had been a suggestion of an 8% increase in rain. How many statistical tests would be tried? Would the authors really just say, "no significant effect"?

to happen twice in a row for experiments with significance level b 5% is b $0.05^2 = 0.0025$ (Ben-Zvi et al., 2011).

"These claims", should have been written, "The evidence presented in those papers…." (They weren't just "claims"…

Later in the same sentence: "…the likelihood of this to happen twice in a row for experiments with significance level <5% is $0.05^2 = 0.0025$ (Ben-Zvi et al. 2011)."

This is a bogus claim when it's an experimenter-created statistical significance. The considerable counter evidence is completely ignored as described in the evaluation of the authors' misleading abstract. They do not acknowledge that **IF** GN81 had performed the cross-over analysis that GN74 liked so much, THEN we would be talking about two Type I statistical errors in a row. But GN74 jettisoned the crossover analysis as soon as they saw it didn't replicate Israeli 1, and hid everything else except the target/control evaluation, raising the question about whether rain gauges were cherry-picked after Israeli 2 started, or were named in advance.

It is interesting in this regard that the HUJ-CSU actions fulfilled a "prophesy" for their own experiments made by the Israeli statistician, Gabriel in 1967, who observed:

"An interesting speculation [18] 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"

GN76,81, and thereafter, made a choice not to tell us about the "unsuccessful numerical results" in Israeli 2 and 3 to the detriment of their country and the scientific community.

This sequence of (experimenter-created) TWO Type I errors in a row was reported by Mielke (1979) for the once benchmark Climax, CO, randomized experiments. In this reviewer's experience, it is common in the weather mod community to create post-facto "Type I statistical errors" via exhaustive searches and/or cherry-picking controls after experiments have started or before they have ended rather than beforehand, and then present those results as reliable seeding increases (e.g., Brier and Enger 1952, Hobbs and Rangno 1978, Rangno 1979, Rangno and Hobbs 1993).

The seeding, in the first two experiments, was focused on convective clouds over the Mediterranean Sea and the coast of Israel. The heat flux from the relatively warm sea energizes these convective clouds. The main target area, however, was the hilly areas inland, where orographic clouds also form. Subsequent studies found that the convective clouds over the Mediterranean Sea are naturally seeded hygroscopically by sea spray and hence not very prone to enhancement by seeding silver iodide (Lahav and Rosenfeld, 2000 (preprint); Levin et al., 1996).

Insert a reference to R88. R88 was the very first report that something was seriously amiss with the HUJ-CSU's cloud assessments, that they could be seeding inappropriate, highly efficient in rain production, clouds. "Stand on the shoulders of those who came first"; it's a science paradigm.

Here's a quote from R88[30]:

*"If precipitation routinely falls from clouds over Israel when cloud top temperatures are at ≥-10°C, and even if these clouds contain only modest ice particle concentrations (say, 1-10 per litre), far higher ice particle concentrations (tens to hundreds per litre) would be expected in clouds with cloud top temperatures from -15to -21°**C.** If this is in fact the case, it would be difficult to explain Gagin and Neumann's (1981) report that seeding produces* **>40%** *increases in rainfall for clouds with top temperatures between-15° and -21°C."*

Isn't this exactly what the present authors are concluding?

The explanation given to the lack of positive seeding effect in Israel-3 was the frequent presence of desert dust in southern and central Israel that seeded the clouds naturally by serving as ice nuclei (Levi and Rosenfeld, 1996; Rosenfeld and Farbstein, 1992; Rosenfeld and Nirel, 1996).

Given the true, efficiently-raining nature of Israeli clouds rolling in off the Mediterranean first reported three decades ago by R88, recently re-discovered by the HUJ-CSU, dust would seem to have little influence on ice formation in Israeli clouds compared with the riming-splintering process of Hallett and Mossop (1974), Mossop (1978).

Dust is not required for high ice particle concentrations in clouds meeting the criteria of H-M riming and splintering. The "dust hypothesis" postulated by Rosenfeld and Farbstein (1992) is no longer viable explanation for "divergent" seeding effects as was proposed.

Furthermore, those authors had no evidence that dust actually got into the clouds and high enough in them to nucleate ice on the days they called "dust" days, or that days that they did not categorize, were, in fact, "dust free." This hypothesis requires ***independent*** airborne verification.

But, a more serious question is, how could the phenomenon of efficiently precipitating, ice-enhanced clouds escape the HUJ-CSU (e.g., Gagin 1980, hereafter, G80) who measured the tops of precipitating clouds with a 3-cm vertically-pointed radar, and cloud top heights, he wrote, that were often confirmed by his aircraft? Those measurements were made over at least two rain seasons in the late 1970s.

This may be one of the more troubling aspects of the HUJ-CSU studies. One of the co-authors of this *AR* article was there, studying the clouds and storm patterns of Israel for several years at the same time

[30] This reviewer's "Note", submitted to *J. Appl. Meteor.* in July 1983 in which it was concluded that the clouds of Israel were not being described accurately was rejected (B. Silverman, personal communication). Think of how differently all this would have turned out had that original "Note" been published! Would there have been enough outside pressure at that time to overcome the HUJ cloud seeding unit's stone-walling of outside, ***independent*** airborne cloud measurements?

that G80 was being published (Rosenfeld 1980, 1982)[31]. And yet descriptions of deep, cold-topped clouds, low in ice concentrations continued to be reported by the HUJ-CSU (GN81, Gagin 1986).

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Additional statistical analyses showed that the orographic precipitation responded most sensitively to the seeding experiments (Givati and Rosenfeld, 2005).

The Givati and Rosenfeld findings were reanalyzed by Halfon et al. (2009) and found to be ambiguous, dependent on which control station group was chosen upwind of a hilly region in Israel. In fact, it was so dependent that even the opposite finding could have been reported! Givati and Rosenfeld presented their conclusions as though there was no questions about them; the findings absolute. But, in fact, they were not.

Promoting results that are, in fact, knowingly ambiguous misleads the scientific community. Givati and Rosenfeld (2005) were perhaps trying to account for why there was no increased rain due to operational seeding into the Lake Kinneret watershed, as reported by Kessler et al (2002, interim report)?

Later, in the full-fledged report, that finding of no detectable runoff by the **independent** panel was fleshed out (Kessler et al. (2006), distilled by Sharon et al (2008). One would suppose that the HUJ-CSU went into a panic and needed a reason to counter Kessler et al's early findings by positing that air pollution *exactly* countered the seeding effect by selecting a an "optimized" group of control stations with which to do that.

The people of Israel and the weather mod community must thank Halfon et al. (2009) for digging into the Givati and Rosenfeld claim. Imagine the consequences if they had not!

Had Givati and Rosenfeld (2005) pointed out the ambiguity in their findings in the first place, instead of hiding that aspect of their work, work that we now know clearly resulted from an extensive search for the "right answer," no one would have needed to have checked into it. Or, if they had used *all* of the upwind controls so that choice was not involved, the Halfon et al. 2009 embarrassment need not have happened.

These hilly areas receive higher rainfall than the coastal plain. They constitute the main catchment for the Sea of Galilee, which serves as a major water reservoir for Israel. Therefore, presently, seeding is carried out with a similar technique as was done since 1961, but with additional seeding lines farther east and augmented with ground generators targeting the orographic clouds over the upper Galilee and the Golan Heights (see Fig. 1).

"1961" should be "1969."

[31] Was this another example of the "Code of Silence" in cloud seeding? This reviewer participated in one during the Colorado River Basin Pilot Project, 1970-1975. It wasn't until 1979 that he "outed" discrepancies in prior work (Hobbs and Rangno 1979) that so many published scientists knew about but remained silent anyway.

There was virtually no seeding by ground generators in Israeli 1; seeding was carried out by a line-seeding aircraft with the exception of several ground generators in the far northeast (GN74). In Israeli 2, however, the HUJ-CSU then added a vast network of ground release sites of AgI. This represented a significant heterogeneity in the execution of the two Israeli experiments, a heterogeneity which has largely gone unappreciated, and its ramifications unaddressed. For example, no one knows how much *more* AgI was released in Israeli 2 than in Israeli 1.

Israel-4 has a significant network of ground seeding sites and, thus, more closely resembles Israeli 2.

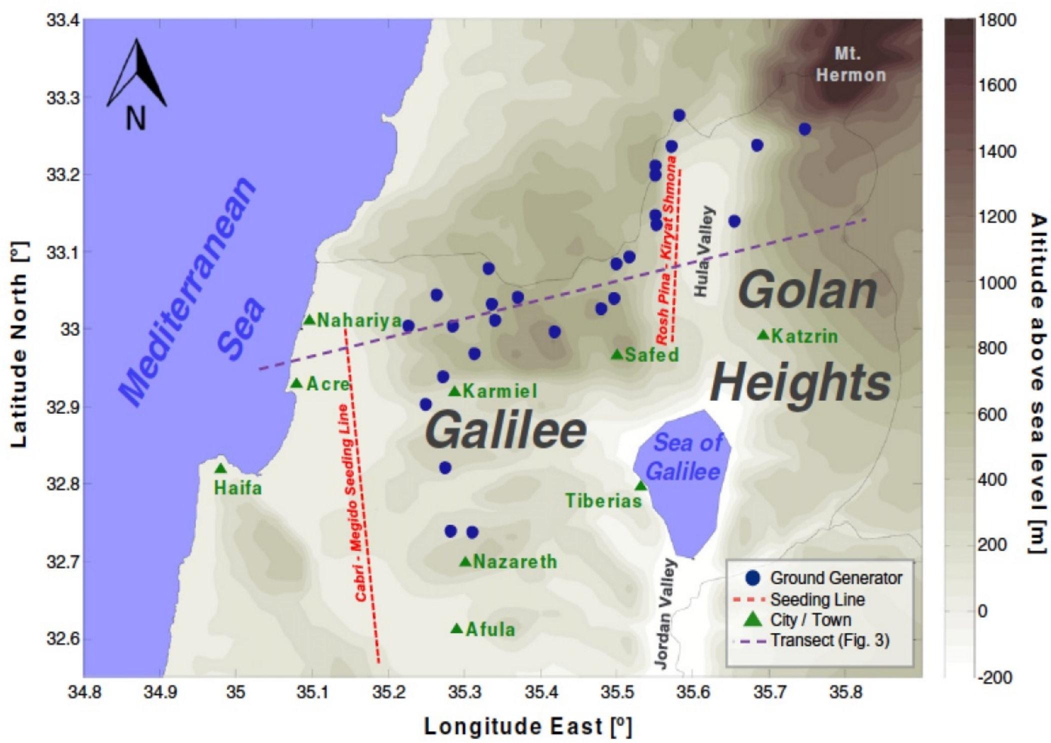

Fig. 1. A physical map of northern Israel showing the spatial distribution of the silver iodide ground generators (blue filled circles) as well as the seeding tracks of the aircraft of the Israeli rain enhancement program (red dashed lines). The purple dashed line denotes the approximate location of the topographic cross-section shown in Fig. 3.

The relatively short life time of cloud droplets in orographic clouds makes them more sensitive to aerosol effects (Givati and Rosenfeld, 2005, 2009; Rosenfeld and Givati, 2006). Cloud physics research flights have been conducted in Israel for further elucidating the hypothesis that orographic clouds might be more suitable for seeding than the convective clouds, and that previous indicated enhanced rainfall might have been contributed mostly by them (Givati and Rosenfeld, 2005).

Here again we see one-sided citing. Givati and Rosenfeld need to tell us what science text told them, "only cite your own works, or only those with whom you agree." Is that what they teach at the HUJ?

Once again they cannot mention the ambiguity in their findings, as demonstrated by Halfon et al. (2009). This seems to be HUJ-CSU's mantra, and shames the HUJ to have scientists representing that distinguished institution writing in this manner.

The main objective of these measurements was to characterize the Israeli winter clouds and the environment in which they develop, in order to evaluate their seeding potential. The cloud microstructure was documented with an instrumented research aircraft. The insights from aircraft measurements were expanded to the regional scale by satellite retrievals of cloud microphysical properties. The results, which are reported in the next sections, were sufficiently encouraging to support a fourth randomized cloud seeding experiment.

Perhaps the most surprising and troubling aspect of this paper is that the assessment of seeding potential in the Golan is being undertaken by HUJ-CSU; "troubling" due to that organization's prior reporting history in that domain, i.e., its inability to detect the nature of Israel's clouds for decades with so many tools at its disposal, and the inherent conflict of interest such a new study represents.

Philosopher George Santayana said it: "Those who cannot remember the past are condemned to repeat it."

Moreover, If the present "background" paper by the HUJ is the primary reason why a new randomized cloud seeding experiment, Israeli-4, was undertaken, then yes, the failed past of the HUJ-CSU's seeding experiments (Israeli 1, 2 and 3) will be repeated providing that Israeli-4 is evaluated by ***independent***, non-HUJ-CSU statisticians and scientists!

The evaluation of the clouds in this article in *AR* is not complete enough to provide confidence that seeding is going to increase rain by an economically worthwhile amount in the Golan. A new randomized experiment, or operational seeding, both appear to this reviewer to be unwise or at least, premature undertakings if based on this paper.

The sad history of the HUJ-CSU reporting in the cloud seeding domain is reprised for the reader or reviewer who is/was likely unaware of it so that the faults in this *AR* paper can be seen in the context of "damage control." They also represent the "baggage" carried by the HUJ-CSU and their efforts to recoup the HUJ-CSU's damaged credibility when they re-write and minimize key elements of that history in Freud et al (2015).

Or, perhaps it's just the HUJ-CSU's style of presenting scientific findings on cloud seeding?

The appearance of this article shows that the HUJ-CSU has yet one more chance (after Israeli 1,2, 3, and in their evaluations of operational seeding) to skew findings to impress an apparently still naïve Israel Water Authority (IWA) about seeding potential in the Golan Heights. This is the perfect example of the "fox guarding the hen house," analogous to the Phelps-Dodge Mining Corporation being solely responsible for the environmental impact statement of its next mine. That Phelps-Dodge assessment *could* be right, but it would never be trusted as such.

Why doesn't the IWA recognize this inherent, and, obvious to all familiar with the history of seeding in Israel, HUJ-CSU conflict of interest?

Why weren't outside ***independent*** groups, such as one of those at the University of Wyoming, Stratton Park Engineering Company, NCAR, Tel Aviv University, etc., brought in by the IWA to make this assessment? The people of Israel and their media should be asking this question. The HUJ-CSU forfeited its right to do such an assessment decades ago.

Furthermore, one or more of those outside, ***independent*** groups mentioned above should now be brought in to validate the assertions about seeding potential over the Golan as claimed in this *AR* paper *BEFORE* any experimentation begins. (Note to reader: it's too late.)

The Israel-4 experiment is focused at evaluating the seeding effect over the catchment of the Sea of Galilee. Most of the water running to the lake comes from precipitation falling over the Golan Heights and Mount Hermon (Fig. 1), which receive considerable amounts of orographic precipitation.

The assessment of seeding potential in this paper is seriously lacking without ground data from Mt. Hermon. Crystal habits, graupel concentrations, and the expected high concentrations of sheaths or needles due to riming splinter processes, and the aggregates of those habits in later stages of H-M production, or other mechanisms of ice multiplication such as fragmentation of drops, are critical measurements that are needed to back up the HUJ-CSU's many hypotheses about seeding potential.

Why wasn't this considered?

Mt. Hermon is often in-cloud and in the Hallett-Mossop temperature zone of -2.5° to -8°C, and in droplet spectra conditions that produce splinters. It is likely that hordes of new splinters themselves, could be observed. What a great research "oppo" for an ***independent*** researcher!

Also, the observation of a substantially deep orographic and supercooled (below -5°C) clouds that do not precipitate at Mt. Hermon need to be documented. This would be a *crucial* measurement that supports cloud seeding potential there.

Why?

The *entire* supposed seeding effects claimed in the Israeli 2 experiment was due to seeding with AgI that caused cold-topped (-12°C to -21°C), but somehow, naturally *non-precipitating* clouds to precipitate at exactly the same rate as natural clouds (Gagin and Gabriel 1987). This enhanced duration[32] finding on seeded days can't be stressed enough in view of the continuing assertions by the HUJ-CSU authors

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[32] This duration finding, of course, was compatible with the "static" seeding mode employed in the Israeli experiments (as the same ersatz finding was in the once benchmark randomized seeding experiments at Climax, CO). It vastly improved the credibility of both sets of experiments.

about statistical significance in prior Israeli experiments now that they have finally understood their own clouds and their high precipitating efficiency. How do they now explain the statistical significance in Israeli 1 and 2?

We suspect the current authors would now question a duration seeding claim due to their now clearer understanding of the copious ice that forms naturally in Israeli clouds. Perhaps they might once again think about those statistical results they quote in the abstract and elsewhere?

Furthermore, the degree of riming on crystals at Mt. Hermon, could also be evaluated. Riming increases snow and rain rates; seeding, by reducing riming, can reduce precipitation, or shift it downwind as we know from seeding experiments in the Pacific Northwest (Hobbs 1975).

It is remarkable that during the years of airborne sampling, ground measurements at Mt. Hermon were not made. This should be a future priority, and observations, of course, made by ***independent*** observers, not by the HUJ seeding unit!

The randomized experimental temporal unit of Israel-4 is 24-hours, delimited at 08:00 (local time), in accordance with the definition of a rain-day. A set of criteria for definition of a suitable experimental unit is defined, and when they are fulfilled a random decision at 50% probability is taken whether seeding should take place during the day or whether it would be a control unit. The qualification criteria are based on the forecast of a regional numerical weather prediction model. They require that the 850 hPa wind would blow from a sector confined by azimuths of 210–290°, that the clouds tops would be colder than −8 °C and that the predicted precipitation in the target area would accumulate to at least 2 mm. A readiness of the seeding aircraft is also required for a qualification of an experimental unit. All the details are available in the Israel-4 experiment book (Givati et al., 2013).

It appears to be in Hebrew. Can a copy in English be obtained?

--------End of "Comments" on Abstract and authors' Introduction sections-------

From this point on, this paper improves demonstrably, is written extremely well in places, representing the best in what we think of as scientific writing. However, there are still a few lapses and required "clarifications" that will be addressed as they appear.

The objective of this paper is to present the available knowledge on the cloud properties in northern Israel, which supported the decision to commence with the Israel-4 experiment, as briefly described above. Section 2 describes the typical synoptic conditions during the rainy days and the dynamics of the clouds as they interact with the sea and the topography. Section 3 describes the methodology of the physical experiment, the cloud physics aircraft instrumentation, flight pat- terns and methodology of data analysis. The methodology of supporting satellite microphysical retrievals is also given in this section. The results of the measurements with respect to aerosols and the way they determine the microstructure at cloud base are given in Section 4.1. The subsequent vertical evolution of cloud microstructure with height above cloud base and initiation of rain are described in Section 4.2. The mixed-phase processes and availability of super-cooled cloud water are presented in Section 4.3. Finally, a summary of the results and a discussion of the suitability of the clouds over the Golan Heights to glaciogenic seeding are given in Section 5.

2. Synoptic, dynamic and macro-physical considerations

2.1. Meteorological conditions

The synoptic systems that are responsible for more than 90% of the annual precipitation in northern Israel occur with cyclones passing through the north-eastern part of the Mediterranean Sea, these cyclones are referred to as Cyprian Cyclones (Goldreich, 2003). A rain event typically starts with the passage of a cold front followed by a thermal low that develops in the cold air-mass behind the front due to the relatively warm sea and the lee effect of the Turkish mountains to the north. An upper trough with relatively cold air aloft is associated with the cyclone, which increases the thermal instability and favors thunderstorm formation over the sea. First precipitation over land typically starts with the arrival of the cold front, as the air ahead of it is characterized by dry and often dusty air from the Sahara desert. Fig. 2 shows the synoptic conditions on a typical rainy day in Israel, at the time of the cold front arrival. As the winds veer from southerly to westerly, the low-level air becomes moister and the cloud-base elevation lowers to a typical level of 500-1000 m (all absolute heights are given above sea level). The thermal instability reaches its maximum in the thermal low after the passage of the cold front. The average wind speeds are often greater than 10 m/s and they contribute to the orographic component of the precipitation. When the low-pressure system moves to the east the instability is gradually reduced. However, due to the long trajectory of the cool low-level air over the warm sea the instability and moisture supply supports continued rainfall for another day or two.

Minor: The IMS refers to periods of rain as “rainy spells.” This is because they are often consist of several consecutive days with recurring showers. In Israeli 1, there was a period of 17 consecutive days with rain. The authors’ sentence should be revised to reflect the occurrence of “rainy spells.”

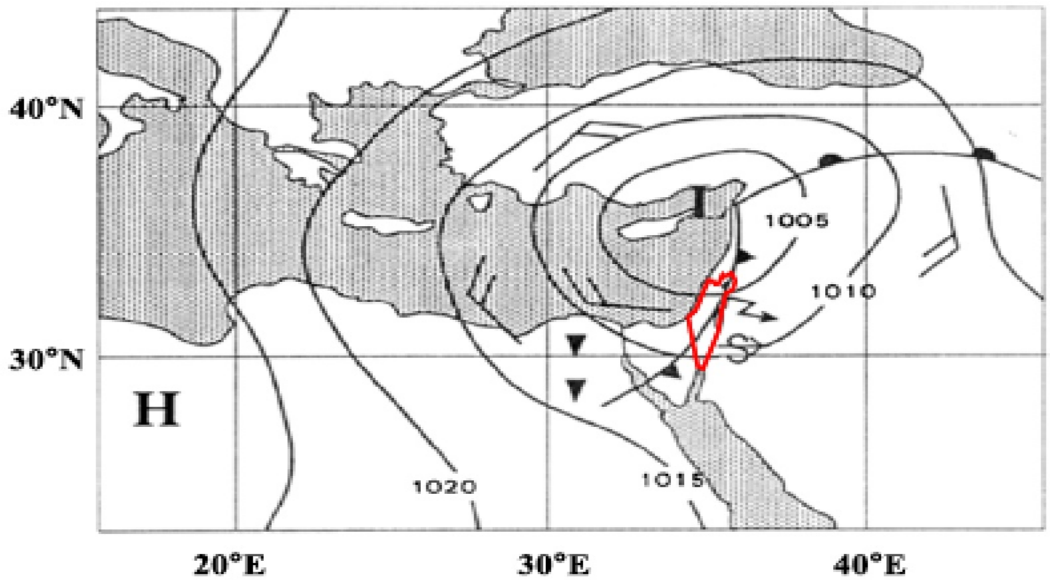

Fig. 2. A synoptic map for a typical rainy day in Israel (marked in red), at the time of the cold front passage. The low-pressure system is referred to as Cyprian Cyclone and the flow is from the western sector in the entire country.

2.2. Cloud dynamics

Fig. 3 illustrates the cloud and precipitation characteristics on a typical rainy day in the air-mass behind the cold front. It is a west–east cross-section across northern Israel, and it is based on our observations and impressions from the physical experiment. The schematic figure is intended to illustrate the main features of the cloud and precipitation processes that often take place as the air-mass travels eastwards across the land and over the mountains.

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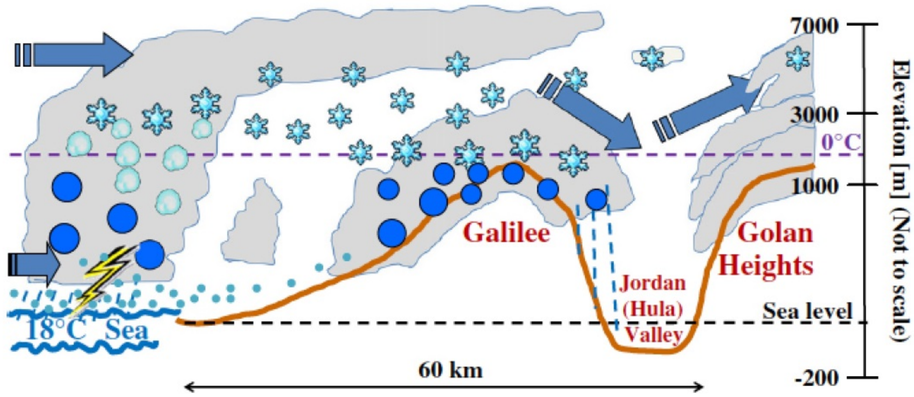

Fig. 3. Illustration of the clouds in a typical rain day on northern Israel, as a west–east cross section from the Mediterranean Sea to the Golan Heights. Rain originates in convective clouds over the sea as graupel and hail. The sea spray from the rough sea seeds the clouds hygroscopically. Orographic clouds over the Galilee are often seeded by the remnants of the convection. The clouds evaporate into the Jordan Valley and reform over the Golan Heights.

Figure 3 and its caption: "…clouds in a typical rain day on northern Israel, as a west-east cross section from the Mediterranean Sea to the Golan Heights. Rain originates in convective clouds over the sea as graupel and hail. The sea spray from the rough sea seeds the clouds hygroscopically. Orographic cloud over the Galilee are often seeded by the remnants of convection. The clouds evaporate into the Jordan Valley and reform over the Golan Heights."

This figure is over-simplified, perhaps out of necessity due to the many scenarios that would have to be presented to reflect stages in Israeli storms as troughs come and go (e.g., R88, RH95).

First, some of the rain that falls in the coastal areas during rainy spells is also due to aggregates from original high concentrations of ice that have formed in stratiform regions as debris clouds and or from vast stratiform rain areas that are remnants of convective complexes. The authors show a higher level stratiform region extending from a line of convection in Figure 3, but do not show the precip reaching the ground, as it sometimes does even at the coast and downwind (which they do indicate).

Satellite imagery, during major events (lines of convection organized by strong upper level troughs) consist of huge clusters of Cumulonimbus clouds and stratiform rain areas that barge into Israel *en masse*. While the roots of the of convection are lessened by moving inland in wintertime, as the authors note, these mesoscale systems march across Israel with much of the deep, precipitating cloud system intact. They do not separate completely, as a rule, into an upper layer and lower layer as shown here on "typical" rainy days over the Galilee and Golan districts.

However, as a *snapshot* view of an advancing storm, Figure 3 with its organized line of convection approaching the coast, with anvil material spreading out ahead of it, with shallow Stratocumulus over the hill regions; yes, this might be accurate for that specific moment of an approaching storm. Drizzle, and mist-like rain (due to collisions with coalescence) often fell from such low, hill-topping overcast low clouds during this writer's 11-week stay in 1986, events in which embedded, much taller clouds sprang forth as the upper trough approached.

But many times, as shown in examples of this, the shallow clouds topped out over hill regions at temperatures where seeding them would not be effective (tops above -5°C, e.g., R88).

In later winter and spring, these shallower Stratocumulus clouds tend to lift off the lower hills as temperature rises during the day, and by later afternoon can sprout into small Cumulonimbus clouds (glaciated clouds) under cold trough situations. Also, due to the local strengthening of the onshore flow during the afternoons, incoming convection weakens and may disappear altogether under the coastal divergence zone that develops during the afternoons.

The deepest clouds are then, in springtime, over the inland hill regions, not immediately offshore. Figure 3 is really much closer to a depiction of the coastal convergence zone that tends to develop in the later nighttime and morning hours (e.g., Neumann 1951, and the occurrences of lighting shift inland—Alcaratz et al 2003).

Though journal space is always limited, a few more schematics of a full storm sequences would have been helpful for the reader in understanding the progression of clouds on Israeli rain days, including that time after the trough core has passed and shallow precipitating clouds are present (as sequenced in R88) until they wither and scatter under subsidence.

Minor: Figure 3 also indicates that the bases of the clouds reforming over the Golan are virtually as low as those on the hills of Galilee. This seems implausible under the loss of precip water and downslope motions in prevailing westerly flow. Bases should be higher over the Golan, and probably closer to the freezing level? Or?

As mentioned in the previous section, the lower troposphere during a typical rainy day is quite moist and unstable due to the relatively warm sea (sea surface temperature never drops below 17 °C) and colder air aloft (typically below +2 °C at 850 hPa during the rainier days).

The cycle of Mediterranean Sea temperatures from fall to late winter is large. It starts out at around 22°C and descends gradually to 17° C as noted above. Cloud base temperatures, hence, water content in clouds and the propensity to form warm rain and copious ice, also are impacted, likely further reducing seeding opportunities in the warmer fall/early winter period.

This favors the formation of fairly deep convective clouds over the sea. This is also why hailstorms and lighting activity are more probable near the coastline and less common further inland (Altaratz et al., 2003; Goldreich, 2003).

Correction: Lightning activity is equal over the Mediterranean Sea and inland areas during November and March (Alcaratz et al. 2003).

When these convective clouds move inland, they become separated from their main sensible heat and moisture source and weaken quickly.

The roots of convection may weaken, but these major complexes usually don't separate with a clear zone between upper and lower clouds as described here. There may be a lower level Stratocumulus-like layer over the mountains with tops too warm to seed glaciogenically. Ergo, the situation is over-simplified and constrained to one conceptual model by the HUJ-CSU authors. They authors give no frequencies concerning what they show as a single cloud/storm conceptual model of many possible ones. How often does this exact scenario in Figure 3 occur? How does it compare with the frequencies of other storm scenarios such as those described by this reviewer?

However, the moist air that is pushed inland by the strong westerly winds is forced upwards by the topography of the western Galilee, and therefore an orographic component is added to the weakening convective clouds. The annual precipitation amount in the upper Galilee peaks at 1000 mm, as compared to the ~600 mm at the coast.

When the air continues eastwards it descends about 800-1000 m to the Jordan (Hula) valley, the clouds, unless they are synoptically forced, tend to dissipate and break up. This leaves the valley with an annual precipitation amount of only ~500 mm. This is the area where often visual flight rules can apply and below-cloud aerosol measurements can be made safely.

The slopes of the Golan Heights force the air to rise again and to produce new orographic clouds with smaller droplets.

Check+

The crest of the Golan Heights rises gradually from a height of 400 m in the south to 1200 m in the north. Further north, Mount Hermon rises up to an elevation of 2800 m. The Golan Heights are 45–70 km away (shortest distance) from the coastline and the clouds there are normally less convective than the clouds over the Galilee and over the sea, because much of the instability is consumed upwind.

"Normally less convective." True except maybe for November and spring where convection over inland regions is apparently more evenly distributed if lightning occurrences are an indicator (Alcaratz et al 2003). What are the seeding implications for enhanced convection? Likely less potential if clouds glaciate at modest supercoolings....

The terrain over the Golan Heights is also less complex than over the Galilee and therefore the flight through the clouds over the Golan Heights is often relatively smooth. The top of the ridge is only about 10-15 km downwind from the foothills where the clouds form. This leaves the clouds little time to convert their water into precipitation before they start to dissipate and lose their water content back to the atmosphere.

Isn't this region too small for seeding to result in a fallout of precip on the Golan and the Lake Kinneret watershed? GN74 estimated 30 min for precip fallout from a line seeding. This estimate, if approximately accurate, would not allow seeding-induced precip to fall into the watershed of Lake Kinneret, considering average flow speeds at the AgI targeting levels of between -5°C and -20°C, except perhaps in NW-N flows?. Authors, please answer.

In case there are synoptically-forced or mature clouds above, which survived the descent to the Jordan (Hula) valley (Fig. 1), they often precipitate through the orographic clouds and seed them naturally from the top. The annual precipitation amount over the ridge increases from 600 mm in the south to more than 1200 mm over Mount Hermon to the north, mainly depending on the surface elevation (Goldreich, 2003).

2.3. Aerosol dynamics

The trajectory of the air mass that is associated with the Cyprian Cyclone passes various regions on its approach to Israel. Each of these regions leaves its signature on the aerosol properties. One of the common features of the warm sector of the approaching low-pressure system is the reduced visibility due to increasing loads of desert dust in the air. The winds with the southerly component get stronger as the cold-front comes closer, lift the dust particles from the surrounding deserts and keep them airborne. The low-level convergence raises the dust particles higher up, so they can travel greater distances. The dust concentrations are normally higher in central and southern Israel than in the northern part. This is due to a number of reasons: 1) Northern Israel is farther away from the main dust sources; 2) The south-westerly flow ahead of the cold front travels some distance over the south-eastern Mediterranean before arriving to northern Israel, and is occasionally associated with pre-frontal rains that can wash out some of the dust particles; and 3) The Israeli deserts in the south, where sand and dust storms are common, can also contribute to the aerosol population in central and southern Israel. After the passage of the cold-front with the arrival of the cool air, the visibility tends to quickly improve.

Dust particles can act as cloud condensation nuclei (CCN) and may also serve as natural ice nuclei (IN). The seeding potential of the clouds is therefore expected to depend on the presence and the concentrations of the dust particles in the boundary layer and in the free troposphere. This was the reason, according to Rosenfeld and Farbstein (1992), to why they found a positive seeding effect both in northern and in central Israel when they stratified the experimental data based on dusty and non-dusty days. Consequentially, one can assume that the release of silver iodide may have the intended effect of accelerating the precipitation formation only when and where natural IN supplies are limited and ice does not readily form in the super-cooled clouds.

Another common and important aerosol type that is quite abundant when strong westerly winds are prevailing is the sea salt aerosols. The breaking waves and rough seas trap bubbles of air in the sea water. As the bubbles float back and reach the surface of the water, they burst the thin film of the seawater and release small drops into the atmosphere — the sea spray. The largest drops may quickly fall back to the sea, but those that stay airborne just long enough have the chance to remain in the air for a much longer time period as they evaporate and get smaller and more concentrated. The vertical mixing in the boundary layer together with the convergence associated with the low- pressure system assist in raising these sea salt particles to the cloud base level. These fairly coarse and hygroscopic particles are the first to act as CCN and make the largest droplets in the cloud, which serve as embryos for subsequent raindrops.

Concerning "sea spray" and large droplet formation in Israeli clouds.

The discussion of sea spray and bubbles strongly resembles that of Woodcock (1953). These researchers stand upon the shoulders of Woodcock but do not cite him. This again points to weak reviewing of the pre-publication manuscript. We quote Woodcock (1953) below:

"It is suggested that bursting air bubbles in "white caps" on the open sea are a major source of the salt nuclei, and that a greater portion of the sea surface may act as a source of these particles during average winds than might be judged from the relatively small area usually covered by white caps".

We note with interest that Woodcock was likely wrong in his initial finding (e.g., Woodcock et al. 1971). Woodcock's finding that large particles are more numerous on "average wind days" rather than days associated with a large number of whitecaps, supports the occurrence of warm rain on more days than just those with numerous whitecaps and stronger winds in Israel. More work is needed on this.

Levin (1992, 1994, L96) should be cited here as well. In Levin's studies it was found that large CCN comprised of sulfate-coated desert particles led to large droplets in Israeli clouds starting at cloud base.

However, rather than strong winds and "sea spray", the occurrence of large droplets in clouds are more likely to be related to cloud base temperature over Israel and the Mediterranean. In an R88 case study, clusters of Cumulus congestus clouds moving in from the Mediterranean Sea on a nearly calm day and produced light rain showers with cloud tops only near 0°C. Cloud base temperatures on that day were above normal, about 11°C. Cloud base temperatures vary substantially in Israel, not only changing as the air mass trajectory changes, but also due to the warm to cool cycle of the eastern Mediterranean from fall to late winter and spring where the temperature can start at 22°C at the start of the rain season, and ends up at 17°C in mid-late winter.

In RH95, it was noted that cloud base temperatures varied from 12°C to 5°C. There would be approximately 40% more water available for condensation with the highest cloud base temperatures compared to the coolest ones, given the average cloud base altitude of about 800 m above sea level. Furthermore, updrafts at cloud base are likely weak and only the largest CCN would be activated.

We note that the authors are aware of this effect of cloud base temperatures, but they do not present data related to it.

Herut et al. (2000) analyzed the chemical composition of nearly 600 samples of rain water collected all around Israel during five rainy seasons. They found that the sea-salt fraction of the rainwater composition is influenced mainly by the distance from the Mediterranean Sea, with a decrease from 73% of sea salt fraction in the coastal samples in the north to 55% in the samples from the Golan Heights. They also reported that the contribution of non-sea-salt precursors to the salinity of the rainwater was much greater in the south due to higher input of continental components and lower annual precipitation there.

3. Methodology of the microphysical measurements

3.1. The research aircraft

A twin-engine turboprop Beechcraft King-Air C90 aircraft was instrumented for the cloud physics measurements during the rainy seasons (November through April) between 2009 and 2013. At the start of every season the airplane was fitted with the aerosol and cloud-microphysics instrumentation as well as the data acquisition and displaying hardware and software. The entire system was tested on the ground periodically while the airplane was standing-by ready for suitable weather to arrive.

3.2. Instrumentation

For measuring the concentrations and properties of the aerosols we used a CPC (condensation particle counter), a cloud condensation nuclei (CCN) counter and an aerosol spectrometer.

The CPC (TSI model 3781) is a water-based condensation nuclei counter that measures the total concentration of particles larger than 6 nm in diameter, at a 1-Hz temporal resolution (Hering et al., 2005). The simple design, fast response and continuous measurement help detecting variations in aerosol concentrations that could be related to the atmospheric thermodynamic structure, pollution sources and/ or aerosol nucleation events. It is an instrument that is fairly easy to handle and maintain and is considered reliable.

The CCN counter that was purchased from Droplet Measurement Technologies (DMT) for the experiment is a continuous-flow streamwise thermal-gradient counter (Lance et al., 2006). It measures the concentrations of the particles that were activated into small droplets at a set super-saturation, as well as the sizes of the activated droplets. The CCN counter can measure continuously at a constant super-saturation or alternatively cycle though user-defined super-saturations for measuring the CCN spectra. The downside of changing super- saturations is that it takes a couple of minutes for the temperatures to stabilize and the actual super-saturation to settle around the required super-saturation. This also happens when the sample temperature or the pressure changes due to changes in flight altitude. We were therefore flying most of the time with a constant super-saturation (typically 0.5%), except during the dedicated time for measuring the CCN spectrum at a constant flight level below the cloud base (typically two-thirds of the way from the surface to the cloud base). The effects of the sample pressure on the actual super-saturations as well as the temperature changes within the instrument were accounted for during the data analysis and quality control.

During the rainy season of 2009–2010 we had a DMT manufactured aerosol spectrometer (PCASP-X2) onboard (Tan et al., 2010). As opposed to the CPC and the CCN counter, it does not expose the aerosols to any super-saturation, but actually does the opposite; it dries the air sample for avoiding aerosol swelling due to absorption of water vapor. The PCASP-X2 measures the diameters and concentrations of aerosols in the range 100 nm to 10 µm. The instrument was mounted inside the cabin. The air intake was isokinetic only up to aerosols of about 2 µm, thus truncating the sampling of much larger aerosol than 2 µm.

Another DMT instrument on the plane was the Cloud, Aerosol and Precipitation Spectrometer (CAPS) (Baumgardner et al., 2000). It consists of two spectrometers (CAS and CIP) and sensors for measuring the temperature, relative humidity, static and dynamic pressures as well as a hot-wire for measuring cloud liquid water content (LWC). The CAS (Cloud and Aerosol Spectrometer) measures particles and droplets at the diameter range of 0.5 to 50 µm. The instrument is mounted on a pylon under the wing and measures directly the airstream from the cloud. The measured aerosol spectrum is therefore sensitive to the relative humidity. Accounting for this effect is not possible without knowing the chemical composition of the aerosols, so we mainly used this probe as a second cloud spectrometer for particles larger than 2 µm, as a backup for the main cloud droplet probe (CDP) and for quality control.

The CDP is a DMT-made cloud droplet spectrometer that measures the concentrations and sizes of the cloud droplets in the 2–50 µm diameter range (Lance et al., 2010). This range is divided into 30 bins, which are much narrower than the bins of the CAS (in the cloud droplet size range). Both probes size each droplet that crosses their sampling volume, based on the amount of light that is scattered forward when the laser beam hits the droplet.

The DMT-made Cloud Imaging Probe (CIP) (Baumgardner et al., 2000) provides 2-D images of precipitation particles based on their shading pattern on a 62-element array of photo-diodes. The CIP that has been used had pointy tips, to minimize error due to shattering of large particles, and a resolution of 15 µm so the nominal width of the array corresponds to a length of 930 µm. The CIP allows identifying the different habits of the ice particles as well as distinguishing them from rain/ drizzle. It is not possible to directly derive the mass of the precipitation particles when ice is present due to their complex form and sensitivity to their orientation. However, the number concentration of the particles (after software partial removal of splinters from shattered particles) along with particle images can be useful for identifying different microphysical phases in the clouds.

Details of the exact methodology used to remove “splinters from shattered particles” by the authors is mandatory here due to the HUJ-CSU’s prior excess removal of “splinters” from in-cloud measurements that misled them about ice formation in their clouds (e.g., Gagin 1975). We also note that they used a 2-D probe with “pointy-tips” to minimize artifacts in the first place. So why can’t concentrations be reported, outside of Figure 16

Other parameters that were being recorded during the flights were the air temperature, the relative humidity and the navigation data from the GPS system.

3.3. Flight patterns and execution

During the four rainy seasons of the physical experiment, 27 research flights were conducted. Each flight lasted two and a half to three hours on average.

How different were the synoptic settings? A table of dates and times of flights is mandatory as are related synoptic maps (surface and 500 mb maps) preferably those embedded with satellite IR imagery, along with IMS and, if available, Beirut rawinsonde profiles Radar imagery should also be posted online for these flights. These items should be posted online to help corroborate the authors' findings; that cherry-picking of a few particular synoptic situations hasn't been done to "improve" the magnitude of seeding potential.

Fig. 4 displays a typical flight track. The black curved line shows the ground path, while the colored line is projected and colored according to the flight altitude. The numbering relates to the geographical locations that are mentioned below.

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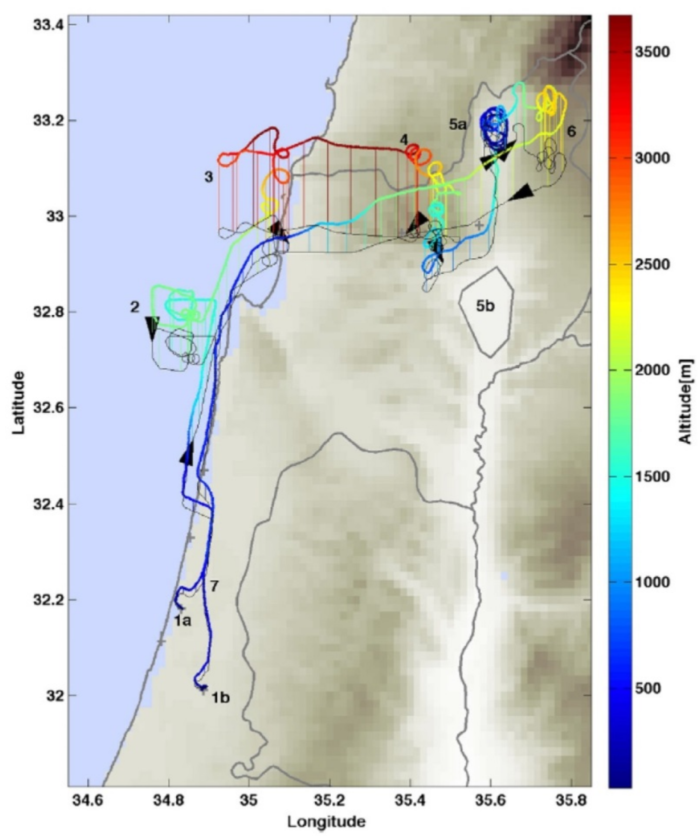

Fig. 4. The track of the research flight on 2 Jan 2012. The ground track is marked in black, while the color-coding and the offset from the ground track denote the flight elevation. The brown shading marks the topography (darker is higher). The numbers along the track mark locations that are referred to in the text.

The take-off was either from Sade-Dov airport in Tel Aviv (denoted as point “1a” in Fig. 4), or from Ben-Gurion International airport which is ~20 km to the southeast (point “1b”). The flight typically started with flying out to the Mediterranean Sea at low level in order to assess the roughness of the sea and to safely measure the aerosols below cloud base (point “2” in Fig. 4). This was followed by profiling the deepest clouds in the region from bottom to top, away from local pollution sources over land, while heading north (to point “3”). The profiling was done either at a constant climbing rate of ~500 ft/min in case the cloud layer was continuous or by flying horizontally through clouds and climbing stepwise 500–1000 ft in the cloud-free air, in the case of well-defined convective clouds. After reaching and sampling the tops of the convective clouds over the sea we were normally able to look eastwards towards the hilly regions of northern Israel, and identify new clouds over the Galilee. We would then fly to these clouds and profile them from their tops to the lowest safety flight level (6000 ft) in a spiral (point “4”).

The sampling height over the Galilee district is about 3500 feet above the bases of the Mediterranean Sea and coastal clouds.

Sampling “new” clouds might bias ice concentrations to lower values than actually developed a little later. Perhaps the authors don’t mean “new” in the sense of cloud stage?

The next step was to descend to below the cloud bases over the Jordan valley between the western and eastern mountain ranges (i.e., between the Upper Galilee mountains and the Golan Heights) in order to measure the aerosols inland.

Heights of cloud bases and temperatures should have been given here; preferably listed in a comprehensive table for all flights and different sampling zones.

This was either done over the Hula Valley (point “5a”) or over the Sea of Galilee (“5b”), depending on the weather and/or the air traffic control directions. The CCN counter was set at this point to cycle through three super-saturations (normally 0.3, 0.6 and 0.9%) for approximately 15 min, while we were flying in circles at a constant altitude, and trying to avoid areas with rain. The third cloud profile was done over the Golan Heights, starting with the cloud bases over the eastern edge of the valley and above the slopes (point “6”). Due to the proximity of the Syrian border, the rest of the climb over the crest was mostly done either in spiral ascent or in a number of north–south legs, each approximately 5 min long, perpendicular to the westerly wind direction, until reaching the cloud top or the height where the cloud was fully glaciated. At that point we typically started heading back south, unless we had a chance to complete another profile or measurement that we were not able to complete earlier. Finally we landed at Herzliya airport (point “7”).

The maneuvering was reasonable in consideration of the realities of the area. However, as the authors know, sampling on the upslope side of mountains leads to more LWC and less ice than would be found farther downwind. Again, is radar coverage of the sampling area on the fight days available? Did the aircraft have recorded radar imagery? If so, can it be made available for each zone that sampling took place?

3.4. Data analysis and quality control

The main software onboard the research aircraft for real- time data acquisition was PADS (Particle Analysis and Display System). PADS has been developed and is maintained by DMT. This data was subsequently processed by our own procedures for merging PADS and non-PADS datasets, extending the analysis from the research aircraft measurements, as shown in Section 4.2.

4. Results and discussion

4.1. Aerosols and cloud base microstructure

The research aircraft was not equipped with instruments to study the chemical composition of the cloud and rain water. However, we noticed that often on windy days, after flying through a cloud, there were white streaks of salt left on the windshield of the aircraft after the evaporation of the cloud water streamers. Fig. 5 shows what the aircraft windshield looked like after passing through a cloud over the Sea of Galilee on 3 Feb 2010, as an example. This is one of the expressions of high salinity of the cloud/rain water in Israel, as had been studied by Herut et al. (2000) and mentioned in Section 2.3.

Nice photo.

Fig. 5. White streaks of salt from evaporated cloud water streamers on the aircraft wind shield. The picture was taken over the Sea of Galilee on 3 February 2010.

Another and more quantitative expression of the abundance of sea spray is achieved by comparing the aerosol size distributions (ASD) that were measured by the PCASP-X2 below cloud bases (Fig. 6).

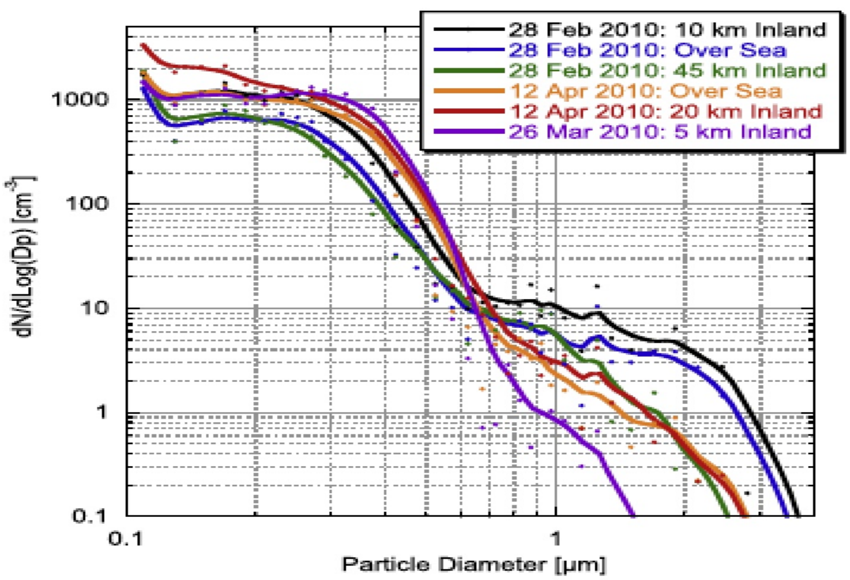

Fig. 6. One minute averaged aerosol size distributions as measured by the PCASP-*X2*, showing the role of sea spray in creating large concentrations of aerosols larger than 1 μm on windy days. The aerosols sampling is cut-off above 2 μm due to loss in the air tubes. The legend contains information on when and where the measurements were made.

Each curve shows a 60-second averaged ASD at elevations between 400 and 700 m. The black, blue and green curves show the ASD measured on 28 Feb 2010, which was a fairly windy day (mean wind of 10.3 m/s at Haifa Port during the flight time). The sea was rough and full with white caps — and hence we would expect an extensive discharge of sea spray.

We note again that Woodcock et al (1971), in later studies of the chemical composition of rain, did not find the expected association between salt in rain. Also, the largest micron plus particles are often only a few hundred meters above the surface. In Cloud and Aerosol Research Group flights over the Pacific coastal waters and rough seas, we did not see particles of more than a few microns in diameter much above the surface. Check pubs

It can be seen that these three curves have the greatest concentration of super-micron aerosols — even when the measurement was made approximately 45 km inland (the green curve) over the Hula Valley. These largest aerosols are normally the first to activate into droplets at cloud base at rather low super-saturations, as they act as giant CCN (GCCN). There were probably more GCCN than what is shown in Fig. 6, but the experimental setup and the inlet of the PCASP-X2 caused the truncation of the ASD at aerosol dry diameters greater than 2 µm, as mentioned in Section 3.2. However, despite the slight undercounting of the super-micron particles, it may still be useful to look into the geographical differences in their concentrations.

Fig. 7 presents the statistics of the concentrations of super- micron (diameter N 1 µm) aerosols in the marine boundary layer and at the foothills of the Golan Heights measured during four different flights. The dataset comprises the 60-second averaged PCASP-X2 concentrations measured at a maximum elevation of 1000 m above the ground. In-cloud and noisy measurements were filtered out, so each box in Fig. 7 is based on 5 to 20 one-minute averaged aerosol distributions. The green text above the boxes denotes the mean wind speed and direction that were measured at Haifa Port at the time of the flights.

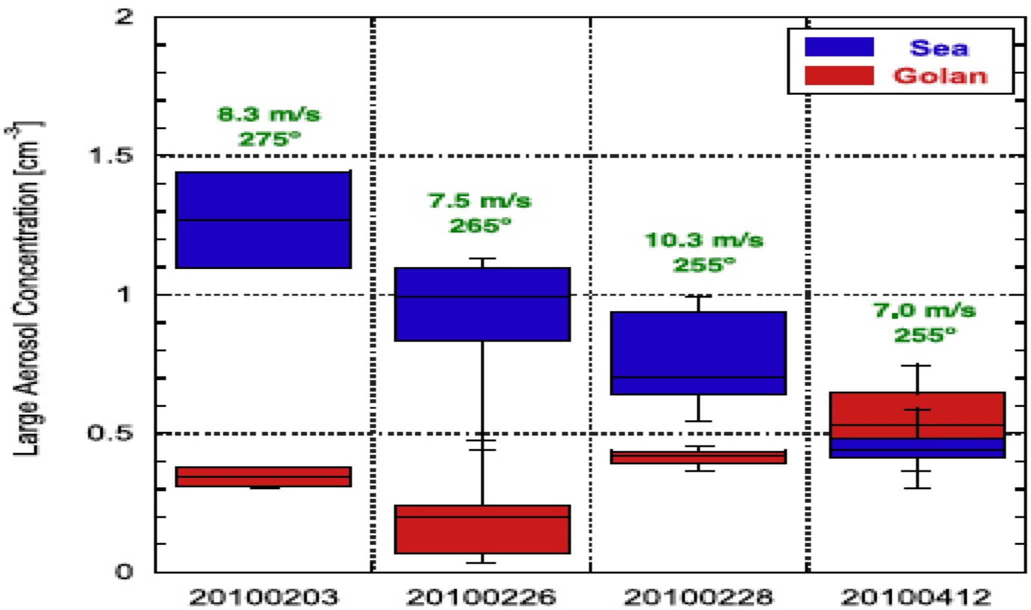

Fig. 7. Statistics of large aerosol (diameter > 1 μm) concentrations in the boundary layer over the sea (in blue) and near the foothills of the Golan Heights (in red) on four different flights, as measured by the PCASP-X2. The lower and upper limits of each box mark the first and third quartiles, while the horizontal line marks the median. The whiskers mark the full range of the data. The green text above the boxes denotes the mean wind speed and direction measured at Haifa Port during the time of each flight.

One can see that the last flight (12 Apr 2010) stands out in the sense that this was the only flight where the super-micron particle numbers at the foothills of the Golan Heights were not considerably smaller than in the marine boundary layer. On the other three flights the large aerosol concentrations over the sea were 2–5 times greater than ~45 km inland. The smallest difference and the greatest inland concentrations (excluding the last flight) were on 28 Feb 2010 — the day with the strongest winds. This implies that relatively little time was available for the large particle to settle on their way inland. The greatest difference and lowest inland concentrations was on 26 Feb 2010, with the weakest winds out of the three earlier flights.

Levin et al. 2010 attributed their finding of synoptic bias largely to stronger synoptic systems with strong winds that drove the precip max farther inland for the north target during Israel 2. The authors' finding seems to support the idea that large, Mediterranean Sea-derived aerosols also played a role in creating an ersatz seeding effect in the interior regions of that experiment.

The wind speed at the shore is not the only factor that determines the absolute concentrations of the super-micron particles and certainly not their rate of removal through cloud and precipitation processes. Furthermore, there could be additional sources of super-micron particles, other than sea spray, such as desert dust and local pollution. The PCAPS-X2 does not distinguish between the different particles, this allows only a qualitative estimation of their relative contribution to the observed values.

The flight notes and pictures from the flight of 12 Apr 2010 indicate hazy conditions over central and northern Israel, with desert dust being the probable main constituent of the super-micron aerosols. That is also the day with the weakest winds recorded at the shore and lowest concentrations of large aerosols near the coast, so the sea spray production may be the lowest on that day, compared to the three others. Low sea spray production and high regional dust loading can explain the relatively low concentration of large aerosols over sea, and the small differences between the marine boundary layer and the foothills of Golan Heights, as were observed on that day (Fig. 7).

What were the offshore cloud droplet concentrations for 12 April 2010?

Both desert dust and sea salt have the potential of accelerating the precipitation formation, but through different microphysical processes. The sea salt is an efficient CCN/GCCN, which can produce large cloud droplets and drizzle particles, and hence speed up the warm rain processes. The larger droplets are also more likely to freeze and accelerate the rate of secondary ice splinter production (Mossop and Hallett, 1974). Desert dust particles, on the other hand, may not be as effective CCN as sea salt, but they tend to be more efficient ice nuclei that might make cloud seeding with silver-iodide redundant on dusty days. This may be something to account for in operational cloud seeding.

The sub-micron particles contribute much more to the aerosol total number concentrations than the super-micron particles. A large source of small aerosols is local pollution, especially when the air-mass travels inland over densely- populated areas, such as in central Israel. This is demonstrated by comparing the blue and green curves in Fig. 6 with the black curve — all from 28 Feb 2010. The first two curves demonstrate the fairly low background aerosol concentrations on that day because the green curve, despite showing an inland distribution, is from the rather sparsely-populated north. The black curve, on the other hand, shows the aerosol size distribution downwind from the heavily-populated Tel Aviv area where the concentrations of both sub- and super-micron particles increase. But it is the sub-micron population that dominates the number concentrations, thus being responsible for doubling of the PCASP-X2 concentrations from ~ 350 cm^{-3} over the sea to~700 cm^{-3} about 10 km downwind of Tel Aviv.

The remaining three curves in Fig. 6 show the aerosol size distributions on relatively calm days; one with hazy skies — due to dust that was transported from SW (12 Apr 2010) with mean winds of 7 m/s at Haifa Port; and one with no haze (26 Mar 2010) and mean winds of only 4 m/s. The ASD of 26 Mar 2010 (in purple) shows the least super-micron particles despite being sampled only 5 km inland. With weak winds and calm seas the production of sea spray was very limited, but local air pollution probably caused the high concentrations of the sub-micron particles. The red ASD in Fig. 6, which was measured downwind from the heavily industrial area near Haifa, had the highest total aerosol concentration of all other ASDs. There were about 1000 cm^{-3} particles in the 0.1 to 2 µm size range. These particles make the largest part of the CCN population.

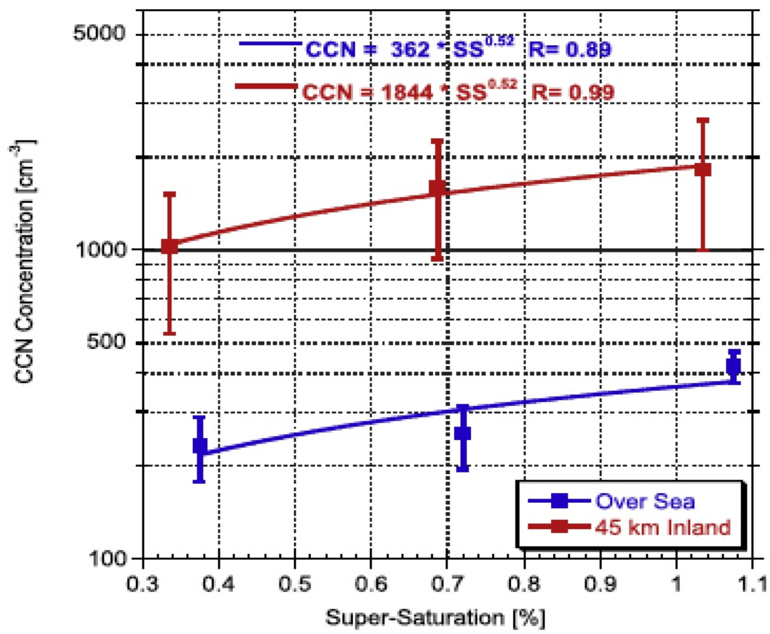

Fig. 8. The CCN concentration at different super-saturations over the Mediterranean Sea (in blue) and over the Hula Valley, about 45 km inland (in red), on 2 Jan 2012. The measurements were taken around noon at a constant level, two-thirds of the way from the surface to the cloud base. The error bars mark the standard deviation around the mean CCN concentration during a five minute period. Note the large difference in the CCN concentration between the two locations.

Fig. 8 presents a comparison of two CCN spectra from another flight. The blue curve shows the CCN concentrations at three different super-saturations (nominal 0.3, 0.6 and 0.9%), as measured over the sea on 2 Jan 2012 at ~600 m.

What were cloud base heights, if any? What time of day was this measurement made? Onshore or offshore flow? Bet Dagan sounding should be presented so we know this case is not a fair weather day with the aerosols clamped down by stable layers. Also, over what distances, length of time were these data collected?

The CCN concentration at 1% super-saturation is estimated at 360 cm^{-3}, while the total aerosol concentration measured by the CPC was 700–1000 cm^{-3}. The red curve, on the other hand, shows the higher CCN concentrations ~45 km inland. The low-level winds on that day had a clear southerly component, which could have brought the pollution from the industrial area near Haifa to the Hula Valley. The CPC concentrations below the clouds were 3000–3500 cm^{-3} while the CCN concentrations at 1% super-saturation were close to 2000 cm^{-3}. The actual cloud base super-saturation was probably lower, as the highest cloud droplet concentrations in that area barely exceeded 1000 cm^{-3} on that day.

The cloud drop concentration and size distribution at cloud base are determined primarily by the properties of the aerosol population (concentrations, sizes and chemical composition) as well as the cloud base updraft. Greater CCN concentrations and stronger updrafts lead to nucleation of more and smaller cloud droplets at the cloud base.

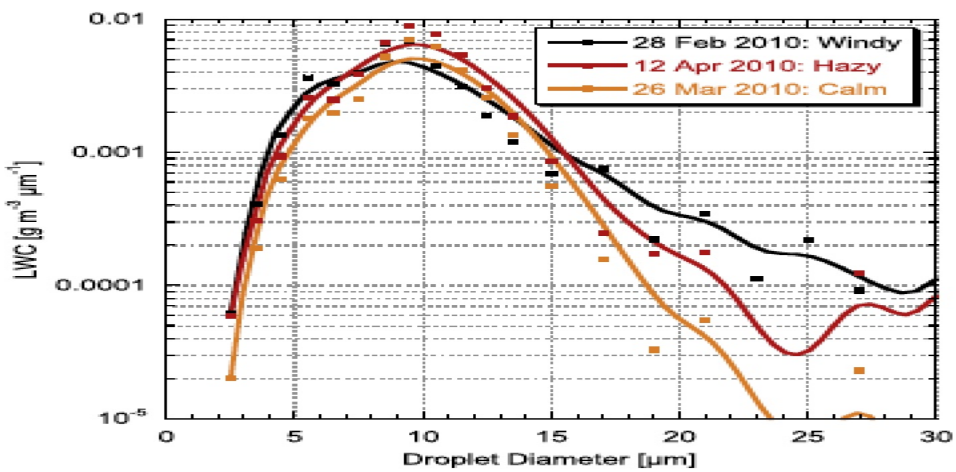

Fig. 9. A comparison between the shapes of the cloud droplet size distributions (DSD) slightly above the cloud base in a windy, a hazy and a calm day over the Mediterranean Sea. Note the differences in the skewness of the DSDs, caused by the "tail" of the largest droplets.

Fig. 9 shows a comparison between the shapes of the cloud droplet size distributions (DSD) slightly above the cloud base for a windy, a hazy and a calm day — all over the sea. All DSDs have a mode near 10 µm, a total droplet concentration of ~120 cm^{-3} and a liquid water content of ~0.04 g/m^3. However, the difference that stands out between the DSDs is the "tail" of the largest droplets. While on the calm day of 26 Mar 2010, the DSD is quite symmetric around the mode, it becomes more and more skewed with increasing concentrations of super-micron aerosol concentra- tions (see matching colors in Fig. 6). On the windy day of 28 Feb
2010, the concentration of the ~20 µm cloud droplets is about an order of magnitude greater than on the calm day, with the concentrations of the hazy day in between.

Would like to see more November – December data, that when the Mediterranean Sea temperatures are warmer; cloud bases, too!

A comparison of near cloud-base DSDs of more than 20 clouds sampled during 12 different flights over the Mediterranean Sea and the Golan Heights, is presented in Fig. 10.

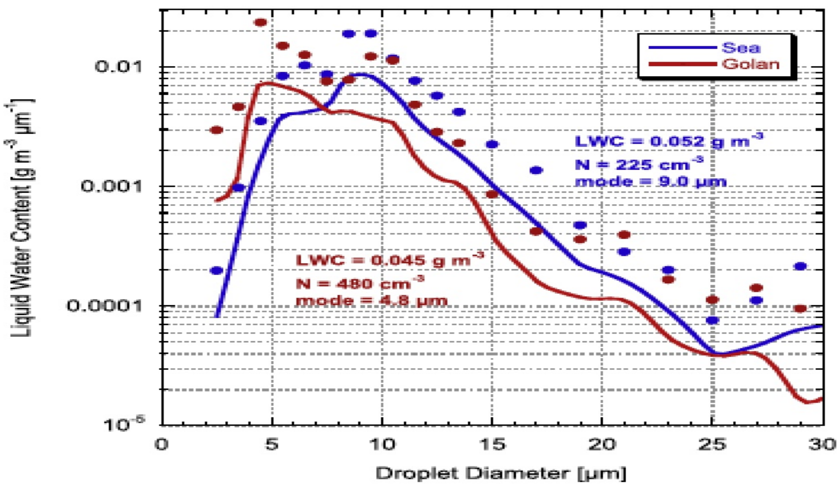

Fig. 10. The mean cloud drop size distribution near cloud base for clouds developing over the Mediterranean Sea (in blue) and over the Golan Heights (in red). Each curve is based on 9 and 13 different DSDs from different flights with various weather conditions and aerosol characteristics over the Sea and The Golan Heights, respectively. The filled circles above each curve mark the 90th percentile in each size bin. The integrated LWC, droplet concentration (N) and the mode of the mean distributions is indicated with the corresponding color. Note the shift of the mode of the DSD over the sea by ~4 µm compared to over the Golan Heights.

There is no separation between windy, hazy or calm days because the emphasis here is on the geographical differences. Because the cloud droplet initially grow (in diameter) rather quickly, only clouds within a hundred meters from the cloud base were considered — as long as the mode of the volume-weighted distribution was below 10 µm and the mean LWC was between 0.01 and 0.1 g m^{-3}. These filters were applied in order to avoid including DSDs from clouds whose bases could not be documented due to safety and air traffic control restrictions, as well as in order to exclude highly diluted clouds. In addition, the cloud base altitudes had to be within 300 m in both locations on the same day. This is to prevent large variations in cloud base temperatures that determine the amount of condensable water vapor near the cloud base.

Superlative considerations, but where are the ACTUAL cloud base temperatures? Nowhere to be found! The reader will want to know what they were to compare with "historical" reports of cloud base temperatures. Also, without care, low LWC's and small droplets can be found in clouds with bases that are evaporating upward. Here's where flight videos would be helpful to outside researchers evaluating these claims.

The integrated LWC of the mean cloud base DSD over the sea and over the Golan Heights, as indicated in Fig. 10, is comparable (0.052 vs. 0.045 g m^{-3}, respectively). This facilitates the comparison of the shapes of the observed distributions because it suggests that the differences in the distance of the compared samples from the actual cloud base and/or different exposures to entrained cloud-free air, are probably not the main contributors to the non-overlapping DSDs in Fig. 10. This leaves the different mean aerosol properties and cloud base updrafts as the probable causes for the different DSDs. However, the greater instability over the sea is expected to produce stronger updrafts that would result in a larger number of CCN to activate there — assuming a common CCN spectra. This would shift the blue DSD in Fig. 10 to the left, i.e. to smaller sizes. But the mode of the blue DSD is actually ~4 µm larger than the red DSD (9.0 vs. 4.8 µm). So apparently the different mean aerosol properties and their resulting CCN spectra, as the example in Fig. 8 shows, are the main factors in shaping the mean near cloud base DSD.

The droplet number concentrations that corresponds to the DSDs in Fig. 10 are 225 and 480 cm^{-3} over the sea and the Golan Heights, respectively.

Thank you. I know it had to be hard to report these numbers in view of the many descriptions of ultra-continental, high droplet concentration, ripe-for-seeding clouds that preceded this paper. It took courage. These are very credible concentrations from the author's experience in Israel, except on some "fair weather" Cu days (Cu mediocris, even Cu congestus) in the Golan, droplet concentrations would likely be higher than 480 cm^{-3}.

The larger number of activated droplets over the western slopes of the Golan Heights results in the smaller droplet sizes near the cloud bases there, compared to over the sea.

Furthermore, the proximity of the Mediterranean clouds to the source of the sea spray tends to result in a tail of larger droplets, as the comparison of the mean near cloud base DSDs in Fig. 10 shows. This is due to the activation of the giant CCN there (which are much fewer than the rest of the CCN). The 90th percentile DSDs in Fig. 10 (filled circles above the curves) indicate that the cloud base DSD over the Golan Heights can also have a considerable tail of large droplet. This is probably caused by activated sea spray, which has been transported inland with strong winds, because the dust particles, as discussed above, are less efficient as CCN.

Here is another finding about large aerosol particles that supports the Levin et al 2010 reanalysis of Israeli 2 and the finding that strong synoptic situations led to the misperception of a seeding effect in the interior of the north target.

The largest cloud drops at the tail of the DSD have greater fall speeds than the smaller cloud droplets, and therefore they can collide with and collect the small droplets and grow further. Those droplets also have higher probability of freezing in sub-freezing temperatures and hence help producing precipitation-sized particles more effectively. The clouds over the Golan Heights, however, most often have a smaller tail and hence convert their cloud water into precipitation-sized particles by warm processes less efficiently.

4.2. Vertical evolution of cloud microstructure

The convection that occurs from synoptically or topographically-induced updrafts raise the cloud droplets that nucleate at cloud base to higher elevations and colder temperatures.

Its hardly worth pointing out, this is such a common descriptor combination, but a temperature cannot be warm or cold. Peter Hobbs: “A cup of coffee can be warm or cold, but not a temperature.” A temperature refers to an object that is warm or cold. It itself cannot be warm or cold, it tells one aspect of the physical state of an object.

The droplet condensational growth is determined by the number of activated CCN and the height above cloud base (Freud et al., 2011). The rate of droplet coalescence is determined mainly by droplet size and is related to the 5th power of cloud drop effective radius (r_e) (Freud and Rosenfeld, 2012). This rate depends also on cloud droplet spectrum width and concentrations. When r_e exceeds 14 µm rain tends to initiate. This process takes normally a few tens of minutes and requires that the typical convective cloud in Israel would exceed a vertical extent of 2 to 3 km (Freud and Rosenfeld, 2012).

Tens of minutes? RH95 show incontrovertible evidence of the rapid glaciation of Israeli clouds, within a few minutes, once tops have passed the -5°C level. But let us assume a Cumulus cloud forms (becomes visible) above the Med., and contains a 5 m s^{-1} updraft. From an 800 m typical cloud base height, it would reach the freezing (typically about 2500 m above sea level on rainy days), in 340 s, and the 700 mb level (3000 m) in 440 s. The 700 to 600 mb temperatures are typically near the level where ice is initiated in Israeli clouds (-5°C to -10°C), and at still lower cloud top temperatures, the ice increases. (We specify that this is NOT a short-lived chimney cloud whose top may rapidly collapse after reaching these levels!)

To reach 4000 m ASL, about 600 mb, would take 640 s, where the temperatures are now typically -10°C or lower. Ice would be appearing rapidly in such a cloud in just over ten minutes, and certainly, if it reached the -12°C to -15°C, would contain hundreds per liter of ice particles from its first appearance, and a few minutes after surpassing the freezing level.

But why do we have to go through this simple example? The authors, or at least one of them, is well aware of this fast-glaciating behavior of Israeli clouds and has been for at least 30 years (e.g., Rosenfeld 1997; Rangno and Hobbs, 1997, Comprehensive Reply to Rosenfeld.) If one merely walked along the Tel Aviv beach during a showery episode one, would know the how the quickly the clouds of Israel glaciate. I would love to do this with the authors on some occasion!

Perhaps the phrase, “tens of minutes” was a careless entry in the original ms?

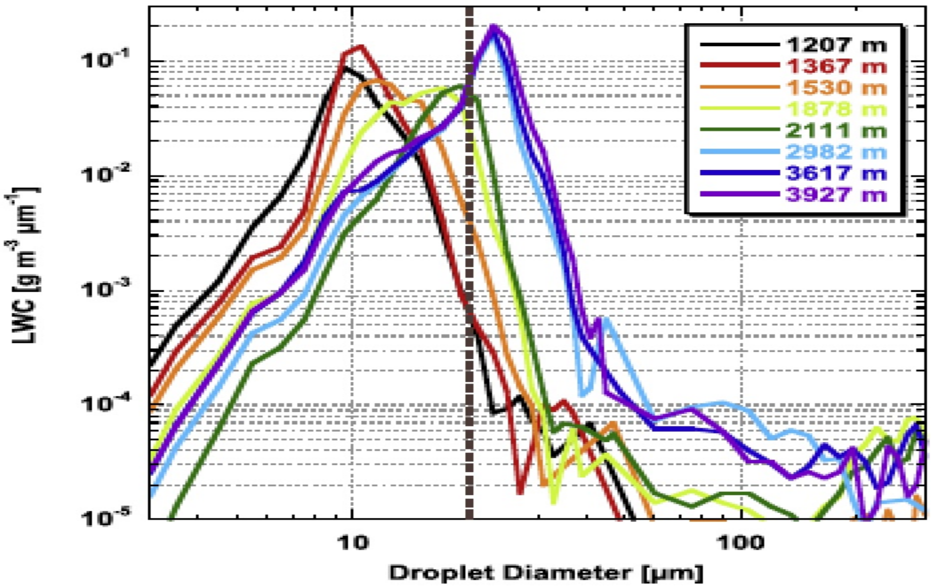

Fig. 11. Vertical evolution of the combined size distributions of the cloud droplets and precipitation embryos — as measured by the CDP and CIP on 12 January 2012, respectively. Each curve indicates the mean DSD of a single horizontal cloud traverse, with the mean altitude indicated in the legend. Note the increase of the modal cloud drop size with height, and the development of small precipitation embryos between the altitudes of 2 and 3 km.

Fig. 11 shows the evolution of the DSD with increasing height in convective clouds over the Mediterranean Sea on 12 Jan 2012. The mean DSD of every cloud pass is given as a single curve in the plot. The clouds were repeatedly penetrated close to their tops as they grew, until small precipitation particles (diameter ~100 µm) were detected by the CIP at the altitude of 3000 m, on that day.

How close to cloud top? Too close to cloud top misses the formation of precip because they lag the smaller droplets at the tippy-top.

These cloud-top “precipitation embryos” become much larger (N1 mm) as they fall through the cloud and collect other cloud droplets and precipitation particles. On other days, precipitation embryos were detected in the growing convective clouds at other altitudes depending on the specific conditions (Freud and Rosenfeld, 2012). However, most of the time that happened when the volume-weighted mode of the DSD exceeded ~20 µm.

For comparing the vertical evolution of the DSD for clouds growing in different aerosol environments, it is easier to represent the entire DSD with a single number, such as the cloud drop effective radius. This was derived from the binned

"EQUATION"

The values of r_e, the mode of the volume-weighted DSD and the droplet mean volume radius (r_v) are linearly correlated as most DSDs have a generally similar shape (Freud et al., 2008). Freud and Rosenfeld (2012) showed that these relations are independent of geographical location. They also showed both theoretically and empirically that the mode of the DSD and r_v have values that indicate the initiation of rain. This is why vertical profiles of r_e, derived from satellite retrievals (Rosenfeld and Lensky, 1998) can be used in real time to estimate the droplet sizes at cloud top, as well as their potential for forming precipitation even before it is detectable by the precipitation radars. This concept is already in use in the operational cloud seeding program in Israel. An example of the retrieval of the cloud microstructure for 10 Dec 2012, at 10:56 GMT (12:56 LT), in the form of re vs. cloud top temperature, is presented in Fig. 12. This retrieval is based on NPP/VIIRS high-resolution data, which allows effective analysis of differences in the microstructure of clouds developing in rather small user-selective areas (Rosenfeld et al., 2014).

What is the depth of cloud from which these retrievals are comprised? One meter? 10 m? 100 m? 1-km? This should be stated here for the *AR* reader.

There are three T–re profiles for different regions in this figure; the Mediterranean Sea (area 3), the Galilee (2) and the Golan Heights (1). The median re (the bright-green curve in each sub-plot) in areas 3 and 2 reached 15 µm at a temperature of about -5 °C. In addition, the T–re profiles in these two areas indicate that cloud glaciation has occasionally occurred at temperatures as warm as -10 °C.

Can the authors supply numbers to quantify this statement? Also, isn't this old news, about 30 years old, about clouds glaciating/raining when their tops are ≥-10°C? (See R88).

Glaciation is indicated by spikes of high r_e and by a red tone of the cloudy pixels.

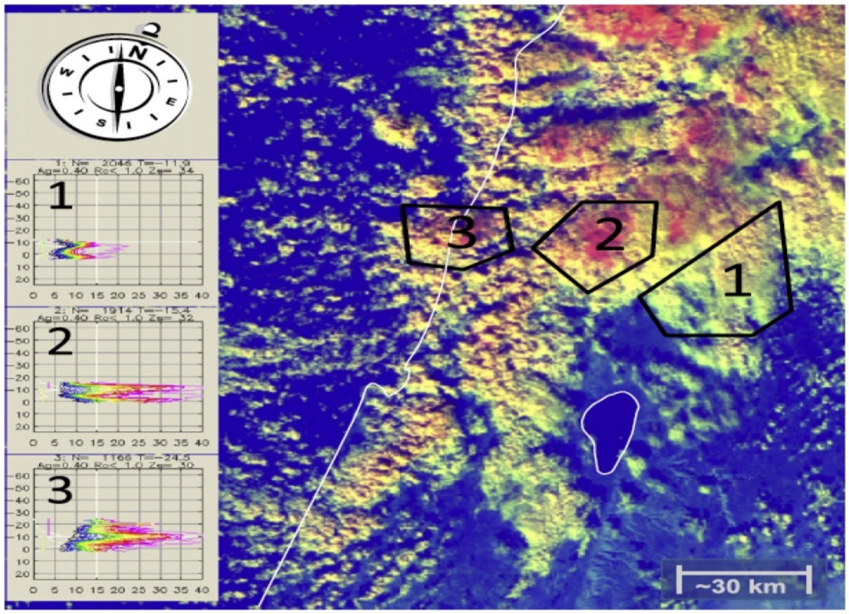

Fig. 12. NPP/VIIRS composite image of clouds and their microstructure over northern Israel, the adjacent areas in the neighboring countries and the Mediterranean Sea. The image is based on a retrieval from 10 Dec 2012 at 10:56 GMT. The coastlines of the Mediterranean Sea and Sea of Galilee, as well as an approximate scale are marked for easier orientation. The RGB color scheme is modulated by visible reflectance (red), the 11.8 μm brightness temperature (blue) and the 3.7 μm solar reflectance (green), which is inversely proportional to cloud particle size (Rosenfeld and Lensky, 1998). The three subplots at the left show the relations between cloud top temperature and particle effective radius over the Golan Heights and into Syria (1), upper Galilee and southern Lebanon (2) and the Mediterranean Sea (3). The colored curves in each sub-plot mark the different effective radius percentiles (every 5%) for one-degree temperature intervals. The bright-green represents the median r_e.

In contrast, the median r_e of the clouds over the Golan Heights (area 1) did not reach the precipitation threshold of 14 μm (Rosenfeld and Gutman, 1994) and did not show any indication of glaciation. The cloud tops there appear to contain mostly super-cooled drops despite developing above the -10 °C isotherm.

In more stratiform clouds, the top of ice-producing clouds is often supercooled droplet clouds, as reported on several occasions (e.g., Cunningham 1957, RH85, and Rauber and Tokay 1991). Thus, the R_e reported here is likely to be misleading. Ground confirmation is required. I believe the authors are aware of this problem in using R_e.

Is radar imagery or hourly rainfall data available for this date to confirm the authors' interpretations?

While Fig. 12 presents a snapshot of a single case, as an example of how the different r_e profiles look in clouds developing in different areas, it is useful to use a statistical approach and examine additional cases. Fig. 13 shows how the cloud droplet effective radius varies with the cloud top temperature. It is based on all high-resolution satellite retrievals from the rainy season of 2012–2013 that were suitable for obtaining the T–r_e profiles for clouds over the Mediterranean Sea and over the Golan Heights. In total, seven retrievals from days with seeding potential, according to the criteria of Israel-4, contained clouds with varying tops in both areas. The cloudy pixels were grouped according to their 11.8 μm brightness temperature to clusters with the same temperature ±1 °C, and then different percentiles of r_e were calculated for each group. Fig. 13 displays the first and third quartiles for each cluster and area as dashed lines, and the median r_e in each area as a solid line. Only clusters containing more than 100 data points are displayed for more robust statistics.

All the curves in Fig. 13 show an increasing r_e with colder cloud tops, but the change tends to be more substantial over the sea. The median r_e over the sea (blue solid curve) crosses the precipitation threshold of 15 µm already at −3 °C, even before the silver iodide can have any effect, compared to −12 °C over the Golan Heights (red solid curve).

I applaud the authors for this information, highlighted above. It gives one hope that we may not travel down the same path that the HUJ-CSU has traveled before.

However, not citing R88, one that was really for the people of Israel whom I thought might be funding ineffective cloud seeding due to inappropriate clouds, represents the act of small minds, not that of disinterested scientists only interested in truth. Omitting relevant work is devious, harms conscientious workers, and degrades the HUJ.

Is this really how the HUJ-CSU authors want to represent their home institutions?

This means that on average, the clouds over the Golan Heights have to acquire a greater vertical extent than the clouds over Sea to start precipitating. The probable cause for that is the greater aerosol and CCN concentration (e.g. Fig. 8) and less sea salt particles further inland (Fig. 7). But the different characteristic dynamics and thermodynamics of the clouds may contribute to the differences as well.

As in Fig. 12, Fig. 13 also shows indications of early glaciation over the sea at rather warm temperatures of around −10 °C, where the 75th percentile of r_e is close to the saturation value of 40 µm. Furthermore, the curves indicate that the clouds over the sea tend to have a greater vertical extent and reach colder temperatures.

The temperature bugaboo strikes again. A temperature, to repeat, cannot be warm or cold; it is not a thing, but rather one measure of the state of a thing.

This is probably caused by the stronger convection over the sea that encourages the formation of deep clouds, while the clouds over the Golan Heights normally have a more orographic nature and a layered structure, according to our subjective observations.

This statement fails to note that in November, and in later winter and spring, thunderstorms are as common at inland hill regions as over the Mediterranean (e.g., Alcaratz et al. 2003), so the idea of perpetual stratiform clouds over the Golan as suggested by the authors is flawed and needs to be re-written to draw this out. Too, clouds may not be ripe for seeding in these deeper convective situations that occur in November and later in the spring. More airborne work is needed.

This is also supported by the differences in the patterns of rain durations and intensities between the coastal and hilly stations in Israel (Goldreich, 2003), where rainfall at the coastal plain is much more intense and has a shorter duration.

It is important to keep in mind that the colored curves in the subplots of Fig. 12 can be considered as vertical profiles in the convective clouds because the cloud base temperature do not tend to vary much within the same confined area. r_e also shows little variance in a given area and altitude (Freud et al., 2008) and the cloud top r_e at a given temperature does not vary much during the lifetime of a convective cloud (Lensky and Rosenfeld, 2006). Fig. 13, however, is based on seven different profiles, not necessarily with the same cloud base temperature and probably with varying aerosol properties and atmospheric thermodynamics. This is why the curves in Fig. 13 cannot really be referred to as vertical profiles that represent the development of individual deep convective clouds. They should be treated as a general statistical view of the T–r_e relations in the indicated areas.

These are excellent qualifiers, as scientific writing should contain when necessary.

The large number of flight hours and the penetration of clouds at various heights and in different geographical areas facilitate a similar statistical analysis to what is presented in Fig. 13 for satellite data, with the in-situ data.

The flight hours are really not so large at <81.

What is far more important, to repeat, is the synoptic setting in which the flights actually took place. As a reviewer, I would have mandated a supplemental appendix with synoptic maps and satellite imagery for each flight be supplied by the authors prior to publication.

I am sorry to say that the HUJ experimenters long ago forfeited the right to make general statements without comprehensive backup material that ***independent*** researchers can investigate for reliability. Furthermore, and I repeat for emphasis, videos of flights should be made available on request or made available online. Most of us "senior researchers" who know well the trail of the prior reports from the HUJ-CSU, understand why more evidence is required from them in their cloud seeding reports than might be the case for another institution. "Fool me once, shame on you; fool me twice, shame on me."

The 1 Hz measurements inside the clouds represent a spatial averaging along a ~100 m long path. These measurements from 15 research flights that were conducted during the physical experiment were grouped according to the measured temperature at the flight elevation. The percentiles of the CDP-derived r_e were calculated for each group of measurements and no other filtering or normalization was applied except the geographical separation between the clouds over the Mediterranean Sea and the Golan Heights. Fig. 14 shows a comparison between the r_e values of the clouds sampled over these areas. Here too the apparent trend of increasing r_e with colder temperatures is more pronounced over the sea than over the Golan Heights, as in Fig. 13.

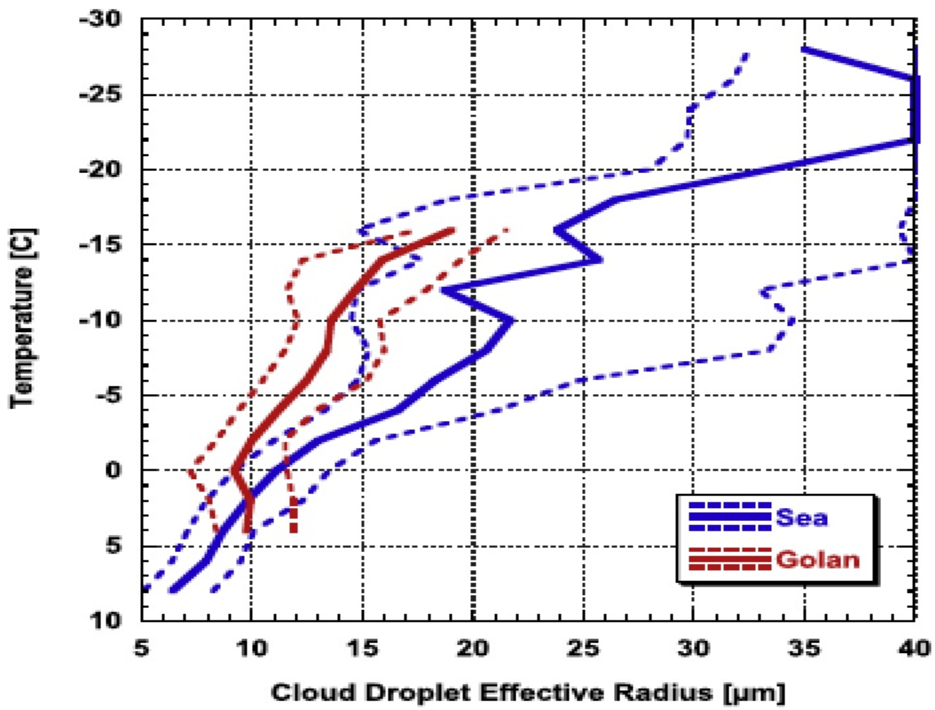

Fig. 13. Comparison between the vertical profiles of the cloud droplet effective radii (r_e) for clouds developing over the Mediterranean Sea (in blue) and over the Golan Heights (in red). The solid curves denote the median r_e for blocks of 2 °C, and the dashed curves indicate the 25th and 75th percentiles for the same blocks. The profiles are based on satellite retrievals of seven different rainy days between December 2012 and March 2013. Notice that r_e in a given temperature block tends to be greater over the sea than over the Golan Heights. Also note that the blue profile extend to colder temperatures than the red profile.

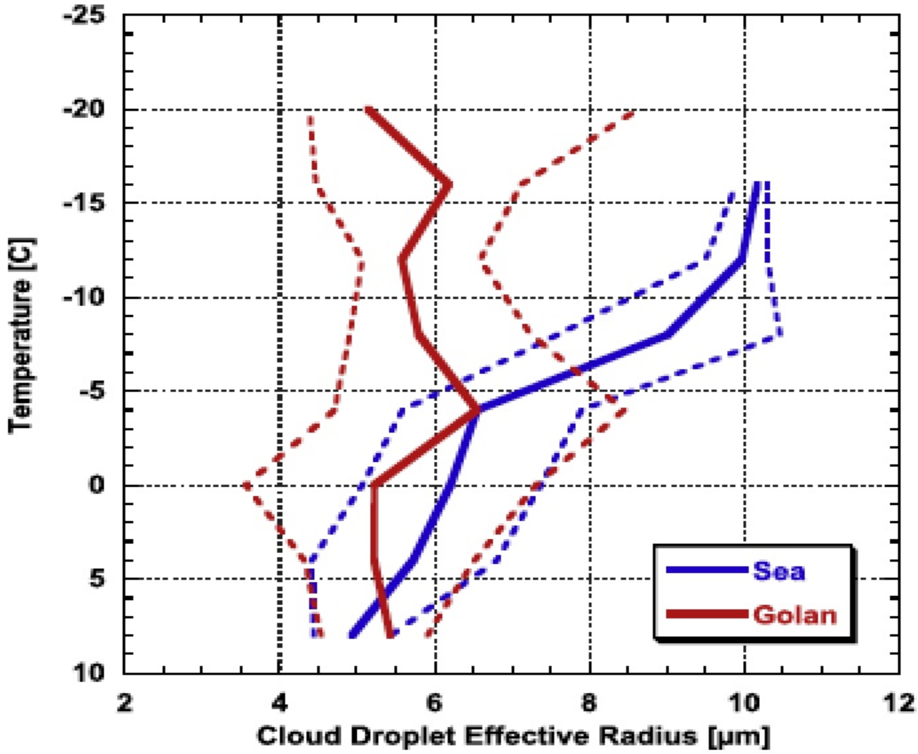

Fig. 14. Similar to Fig. 13 but this plot is not based on satellite retrievals but rather on airborne in-situ measurements from a 15 flights between 2009 and 2012. Notice the divergence of the profiles at temperatures colder than −5 °C, and the small absolute values here in relation to the satellite-derived r_e.

The two profiles appear to separate around the temperature of −5 °C, where the marine cloud seems to maintain its convective profile, i.e. increasing r_e with colder temperatures. Over the Golan Heights, however, the effects of elevated cloud layers with embedded convection and/or secondary droplet nucleation, may explain the offset of the peak between r_e and LWC (see Fig. 17). This is supported by the statistics of the droplet number concentrations around those peaks (not shown here). The differential likelihood of freezing of larger cloud droplets, elevated cloud layers, cloud-top entrainment and precipitation, may all contribute to maintaining a low median r_e at temperatures below −10 °C.

References supporting the contention highlighted above could have included HR1985, among many others. HR85 used the large end tail of the cloud droplet spectrum (“threshold diameter”) as measured by a FSSP-100 in young, low ice containing Cumulus turrets as a predictor of later ice particle concentrations. It worked pretty well except in short-lived narrow “chimney Cu”.

Can the authors provide that measurement in their mandated table (s)?

In situations that are truly orographic, it is common to have a liquid supercooled cloud layer at the top with ice falling out underneath, even at low supercoolings. To repeat for emphasis, deriving R_e in these situations would be misleading and not representative of what was happening underneath. Ground truth data are required.

The percentile-profiles of the satellite-derived r_e of the convective clouds over the sea exhibit greater variance than those of the orographic clouds over the Golan Heights (Figs. 12 and 13). This is partly caused by the so-called 3D effects — the variable illumination of the cloud tops by the sun due to their inhomogeneous physical structure (Marshak et al., 2006). The cloud top r_e of cloud surfaces that incline away from the satellite, i.e. in a direction that increases the sun–cloud–satellite angle, would be overestimated (even more in shaded cloud surfaces). The cloud surfaces that incline towards the satellite would have their r_e underestimated due to the opposite effect, resulting in a falsely wide r_e distribution for clouds with a non- horizontal cloud top surface. This, of course, does not apply for in-situ derivation of r_e, as in Fig. 14.

Outstanding distinterested writing by the authors describing issues, both above and below! Here the paper has the feel of real science! I'm excited.

Furthermore, the absolute r_e values that were calculated from the CDP measurements, are typically smaller in Fig. 14 than the satellite-derived r_e shown in Fig. 13. Possible sources for this apparent discrepancy include

1) Our focus was on documenting the evolution of the cloud DSD as it grew and on measuring the formation of precipitation embryos and the initiation of effective precipitation.

What is "effective precipitation"?

In addition, we tried to avoid flying higher than ~5 km or in heavy precipitation from mature clouds due to flight safety and performance reasons. This is why our sampling strategy did not cover the area uniformly but instead favored clouds in their early growing stages — unlike the satellite retrieval. The aging of the deep convective clouds allows more time for the collision and coalescence process to produce large droplets and increases the chances for ice formation. Therefore the underrepresentation of the mature clouds in the in-situ measurements and in Fig. 14 reduces the r_e values.

Qualifying one's measurements doesn't get any better than this! Thank you, authors! Just for the record, this reviewer believes that GN74—written in 1972) also represents one of the finest examples of objective writing within the often murky domain of weather modification/cloud seeding, where "confirmation bias", monetary considerations so abound that they deflect true science into something else.

2) The CDP measures only droplets with a diameter smaller than 50 µm. If larger drops or even small ice particles are present at the cloud top or slightly below it (or even as a thin cloud layer above it) then the satellite-derived r_e might be overestimated Point (1) above also helps explaining why the blue profile in Fig. 13 is deeper than the red profile, but in Fig. 14 it is shallower. We basically stopped climbing when cloud water was mostly converted to precipitation or glaciated. This happened on average at a slightly warmer temperature over the sea, and hence the shallower profile in Fig. 14. The clouds over the sea often reached much higher altitudes than over the Golan Heights (e.g. Fig. 12) due to the more unstable conditions.

The situation highlighted is often reversed early winter, and certainly during the spring in major troughs when thunderstorms are as common inland than over the sea. This needs to be re-written to reflect this reality.

The satellite retrievals reflect this, but the in-situ measurements do not because of the sampling strategy

4.3. Formation of hydrometeors

Supercooled cloud water tends to be quite abundant in northern Israeli rain clouds.

This does not represent the writing of disinterested scientists when they insert the word, “abundant”. They do not provide specific data on this “tend to be abundant”claim anywhere in this paper, nor do they seem to realize that supercooled water is often short-lived in Israeli clouds due to the rapidity of ice formation. The phrase, “quite abundant” in this sentence should never have appeared in a peer-reviewed publication without substantiation and definition.

Most of the rain that falls in northern Israel during the winter initiates as mixed phase hydrometeors in clouds with tops that are well above the 0 °C level.

Omit the word, “well”; “above” is sufficient. Furthermore, in low stratiform clouds that are thickening upward as an upper trough approaches, drizzle was found to often form in those clouds before the ice phase kicked in (from the reviewer’s 1986 experiences in Jerusalem as storms moved in. One suspects drizzle will also occur in hill locations in the far north of Israel as well. This again speaks to the importance of having ground obs at Mt. Hermon; maybe Har Kanaan, as well.

This has been documented with the help of the CIP that provided 2D images of the hydrometeors in its sampled volume of cloud/ air. Although the CIP does not allow distinguishing between spherical super-cooled liquid drops and slightly irregular frozen drops at diameters smaller than ~100 μm — due to its 15 μm resolution, riming of frozen drops and graupel particles can be easily identified. These particles provide clear indication of ongoing mixed-phase processes in the clouds. Quantifying the ice content in the cloud with a 2D imaging probe is highly uncertain, however we were still able to see and document the increasing concentrations and sizes of the ice particles as the clouds matured and the water content decreased.

Apparently this statement is the reason that ice particle concentrations are limited to "averages" in Figure 16 in this paper. This would appear to be a first in a paper asserting to be evaluating cloud seeding potential in any location in this writer's experience that doesn't have a full disclosure of ice particle concentrations. Are 2-DC concentrations now considered to be unreliable? They have been reported over the decades? They may not be accurate to within whole numbers per liter, but they are certainly accurate enough to be reported here with error bars, if the authors so choose.

The authors may not know that the Amer. Meteor. Soc. issued a Monograph in 2017 on secondary ice particles, and the effort to explain them, authored by our leading scientists in that domain. Most of those observations that are discussed are due to measurements with a 2-DC probe! So, which is correct, that a 2-DC probe results in "highly uncertain" concentrations (translate as "high ice particle concentrations that we, the HUJ-CSU authors of this seeding potential paper, do not want to reveal)?

Or that data from a 2-DC probe is accurate enough that the authors of the AMS Monograph felt comfortable enough to discuss its concentrations?

Since the 1970s, there have been ways of reducing 2-DC artifacts caused by, say, shattering on probe tips. Indeed, the authors tell us that they used 2-DC probes with "pointy tips" to reduce shattering artifacts, and now they tell us they can't report what they found from those probes. This is sad, because it appears to be another omission of important data by the HUJ-CSU.

Maybe we shouldn't be surprised at such an omission, except that it appears in a peer-reviewed journal. What reviewers allowed an omission of such critical data for the reader as a full disclosure of ice particle concentrations, in particular, their maxima, in a so-called evaluation of cloud seeding potential?

If this type of editing of findings was what convinced the IWA to proceed with rudimentary operational seeding or "Israeli IV", then the HUJ-CSU has misled them once again. It is likely, in such an experiment with an unbiased draw, that no statistically-significant results from seeding will accrue.

Elevation [m]	Temperature [°C]	Max LWC [g m^{-3}]	CIP 2D Images
3920	-14.7	1.4	
3610	-12.4	1.4	
3190	-9.6	1.2	
2990	-7.9	1.6	
2310	-4.5	0.5	
2020	-2.9	0.3	
1530	0	0.4	
1210	+1.9	0.4	

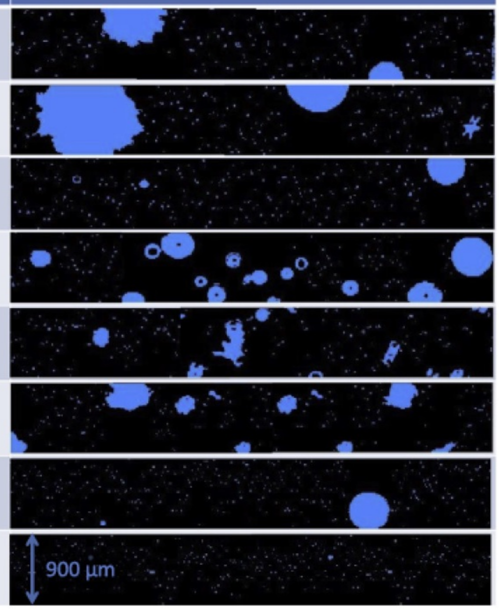

Fig. 15. CIP 2D images of the hydrometeors in a deep convective cloud that grew above the sea on 12 Jan 2012. Each horizontal slice shows some of the CIP 2D images that were recorded during the cloud traverses as well as the elevation and mean temperature of each traverse, and the maximum liquid water content. Note the scale at the bottom.

Fig. 15 shows some 2D images of the hydrometeors that were observed at different altitudes over the sea on 12 Jan 2012, as an example.

The images are quite nice; shows what we can do today.

However, a single image chosen by the HUJ authors, does not suffice. They have omitted (sound familiar?) those images of sheaths and needles, crystal habits that form at temperatures > -10°C that would document and confirm the prolific operation of secondary ice particle production mechanisms that surely operates in the clouds of Israel. The aerosol environment of Israel, with its large CCN and often high concentrations of smaller droplets due to pollution in the Hallett-Mossop riming/splintering zone creates the "perfect storm" for ice multiplication in Israeli clouds (as the authors know when they cite Mossop 1978). Figure 12 is not acceptable to this reviewer without extensive 2-D imagery, along with the "best possible" concentrations, being made available online, as shown in Figure 16, to support the claims concerning Figure 12.

The CIP particle size distributions for the same cloud/area were shown in Fig. 11. According to the combined CDP/CIP particle size distributions, the mode of the cloud DSD reached ~20 µm at an elevation of slightly below 3000 m. This is also where the CIP water content of ~100 µm particles became more significant. Fig. 15 shows that these precipitation embryos are spherical, i.e. probably still in the liquid phase, although the temperature is close to −8 °C. The LWC at that elevation reached 1.6 g m^{-3} and there was no indication for ice. Higher up in the cloud the particles became even larger and started having a more irregular shape. The irregular particles seen at temperatures warmer than −5 °C are not likely to have frozen at the observed elevation, but rather have fallen from above while riming smaller particles. This leads to another small mode in the far right of the CIP size distribution (Fig. 11) rather than a more continuous decline in LWC with increasing particle diameter in the CIP range.

Documenting a similar process of cloud droplet growth, the formation of precipitation embryos and their freezing over the Golan Heights was much more challenging due to a number of reasons. It was difficult to follow the same cloud element through its lifetime due to the more orographic nature and layered structure of the clouds.

The authors recover from the omissions discussed previously and write an outstanding couple of qualifying sentences, the type that normally accompany a great science piece! I'm excited again!

And even if distinct convective cloud tops were identified, they often quickly drifted eastwards into Syria, where it was not possible flying to. The impression was that often the clouds there would consist only of supercooled water with no precipitation, unless having embedded convection or ice particles falling from above.

The above is good, and the use of the word, "impression" makes it even better. Thank you authors!

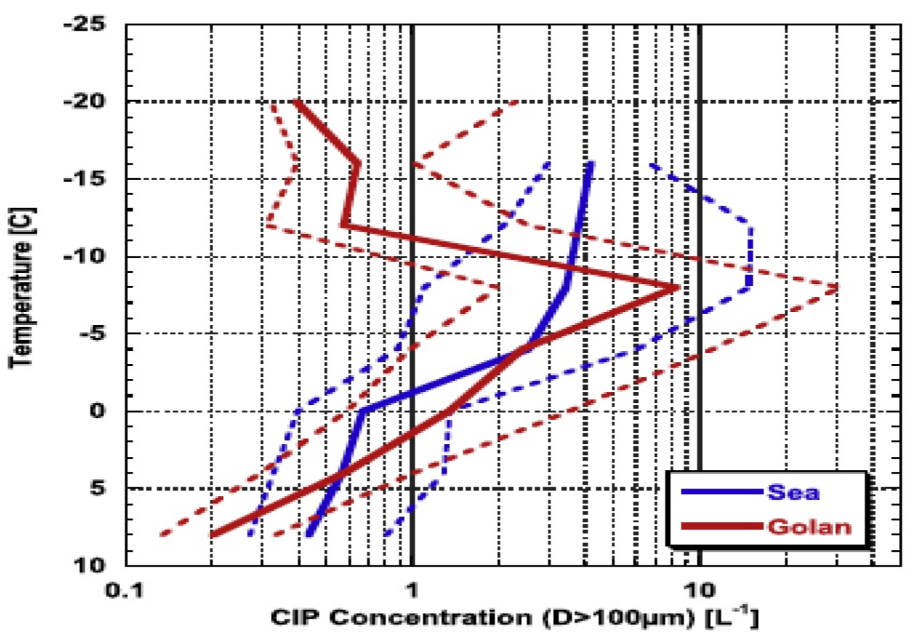

Fig. 16. Comparison between the CIP concentration of hydrometeors larger than 100 μm at different temperature blocks over the Mediterranean Sea (in blue) and the Golan Heights (in red), based on 15 flights between 2009 and 2012. The solid curve marks the median value in each temperature range, while the dashed curves mark the 25th and 75th percentiles. Note the peak of the red profile around −8 °C.

Fig. 16 presents the profile of the concentrations of hydrometeors with diameter greater than 100 μm in clouds over the Sea and the Golan Heights — as measured by the CIP. It is based on the same 15 flights used for the analysis in Fig. 14 and both figures have some common features. As with the cloud droplet effective radius, the concentrations of the large hydrometeors increase with decreasing temperatures in the developing convective clouds over the sea. This is because as the cloud grows and its top becomes colder, the cloud droplets and the hydrometeors that do not fall become larger, and also because the freezing of the largest droplets, which may push them over the 100 μm cutoff size used in Fig. 16, is more likely.

The statistics of the hydrometeor concentration over the Golan Heights, on the other hand, shows a maximum concentration between −5 and −10 °C, with median concentrations close to 10 L^{-1}. The inclusion of cloud layers with colder bases, smaller droplets and less LWC might explain the decreasing hydrometeor concentrations at colder temperatures over the Golan Heights. This might not, however, explain the finding that the CIP concentrations in the −5 to −10 °C temperature band are considerably higher over the Golan Heights than over the sea.

There is an obvious reason for the lower concentrations in the -5° to -10°C temperature range over the Mediterranean where ice multiplication is rampant; the sampling of young, newly risen “hard” turrets as the authors describe. Why they didn’t sample maturing and dissipating portion of Cumulonimbus clouds to get a handle on the degree of ice multiplication in Israeli clouds is a mystery.

It is worth noting that the temperature of maximum hydrometeor concentration over the Golan Heights coincides with the activation temperature of the silver-iodide which was released upwind during most of the sampling.

WHY would one sample, at great operating cost, clouds when seeding was underway upwind??? Isn’t that a waste of precious resources? Was this unintentional? Are there no data from your flights that were gathered in the Golan without seeding taking place? Did you measure ice nucleus concentrations, ones that could have alerted you to this problem? This, unfortunately, raises the issue of incompetence unless there were mitigating circumstances.

It is also close to the temperature of maximum Hallet–Mossop ice multiplication rates, which may be suppressed in strong convection (over the sea) due to relatively short residence time of the graupel in the optimal temperature band for ice splinter production (Hallett and Mossop 1974).

This is non-sense. These authors need to go outside and watch there incoming Cumulus congestus clouds as they convert to ice (and small Cumulonimbus clouds) within minutes after reaching much above the -5°C level. With proper aircraft sampling, the authors would have found a ton of H-M generated (and/or that by other processes) ice crystals (needles and sheaths) in that same temperature zone (-3° to -10°C) as over the Golan.

One wonders why the analysis goes so badly here? Who was directing the aircraft? Who wrote this segment?

But this process may be enhanced by the embedded convection that often occurs over the Golan Heights. This is because the mixing of the convective cloud element with the stratiform cloud with a colder base, can result in a broad, or even bi-modal, droplet size distribution, which is favorable for ice splintering (Mossop, 1978). It is not possible, however, to quantify the relative contribution of each factor to the observed hydrometeor concentrations without a very detailed case by case analysis, because the variation from cloud to cloud, and even within a cloud is rather high.

Send me your videos from these flights, your ice and droplet concentrations and spectra, and I promise to help you understand what you did!

4.4. Availability of supercooled cloud water

The supercooled liquid water content of the clouds over the Mediterranean Sea and over the Golan Heights, at different temperatures, is presented in Fig. 17. The water content was integrated over the binned CDP data and, if necessary, slightly adjusted to match the measured content of the Hot-wire probes. Fig. 17 is based on the same dataset as Figs. 14 and 16. Note that the abscissa in Fig. 17 is in the log-space due to the large possible variation in LWC. At the initial stages, as the clouds develop, their tops get cooler and the LWC values increase both over the Mediterranean Sea and over the Golan Heights. At −4 °C, 25% of the measurements that were taken over the Golan Heights had a LWC greater than 0.4 g m^{-3}, while over the Mediterranean Sea that number, at the same temperature, was slightly smaller.

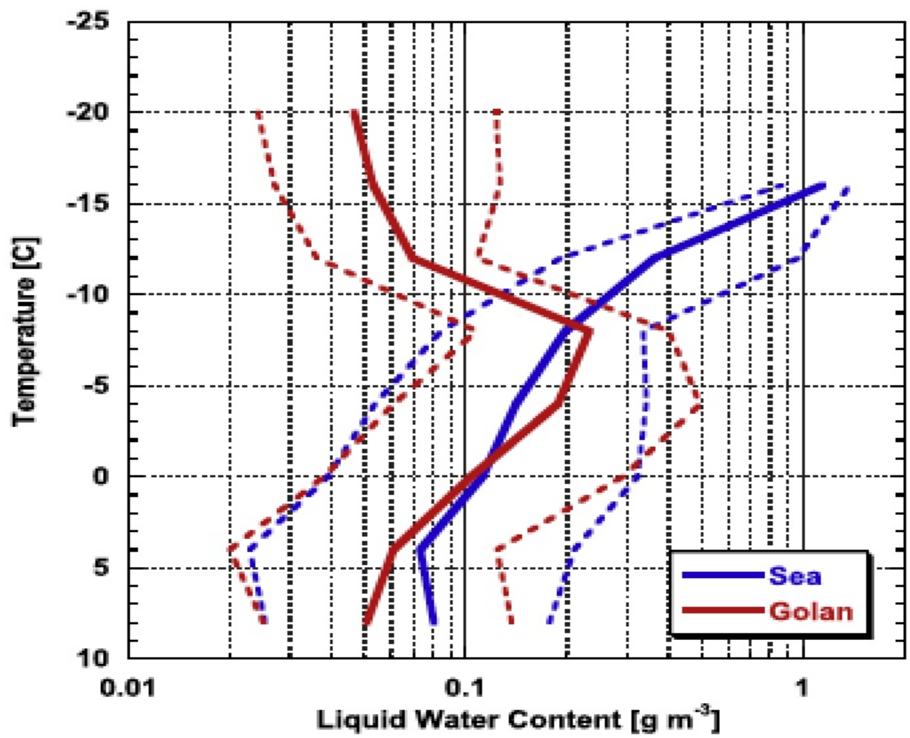

Fig. 17. Comparison between the cloud liquid water content at different temperature blocks over the Mediterranean Sea (in blue) and the Golan Heights (in red) from a variety of flights between 2009 and 2013, as calculated from the CDP measurements. The solid curve marks the median value in each temperature range, while the dashed curves mark the 25th and 75th percentiles. Note that the red curve reaches a lower maximum LWC and at a warmer temperature than the blue curve.

As the authors noted previously, they have no idea what happened to the LWC in the clouds at the upwind side of the Golan. It should be re-iterated here, lest the reader be misled that this is unambiguously seeding potential water. That potential remains to be seen without more comprehensive measurements, such as ground measurements of ice crystal concentrations, habits, riming characteristics at the top of Mt. Hermon, which is often in the H-M temperature zone during precip events in Israel.

Hermon crystal data would make a stupendous paper of itself. But could we trust the HUJ-CSU, with so much seeding baggage, to do it objectively? I really don't think so. What a shame that a "senior" researcher, who is extremely familiar the output of the HUJ-CSU over several decades, feels this way. Convince me I'm wrong.

Further up the LWC values over the Golan Heights started decreasing, while the LWC over the sea continued to increase as the cloud tops developed further.

As mentioned in Sections 3.3 and 4.2, the sampling strategy over the Mediterranean Sea and over the Golan Heights was often different because of the different typical cloud structure/ dynamics as well as flight safety restrictions. While over the sea "hard" tops of deep convective clouds were more abundant and favored, over the Golan Heights the maneuvering was more restricted and the sampling covered the area more homogeneously.

Here and below, the authors divulge a critical pieces of information about sampling that represents the highest form of objective, scientific writing for their readers!

Preferential sampling of developing cloud tops with relatively little history of mixing over sea, versus measuring mostly orographic layer clouds over the Golan Heights, can explain the greater LWC values over the sea and the positive trend all the way up. More than half of the samples over the sea at a temperature around −16 °C had a LWC greater than 1 g m^{-3}.

The quote above about 1 gm m-3 at -16°C concerns liquid water that would be very short-lived, indeed, as I believe the authors know, or should know. Would it last even five or ten minutes? Maybe----this estimate from the writer's 11-week wintertime experience in Israel watching clouds roll in off the Med.

The likely explanation for the LWC "bump" only over the Golan Heights is the sampling of cloud layers with bases at ~~colder~~ lower temperatures and hence reduced LWC. As Fig. 3 illustrates, the clouds over the Golan Heights tend to have a structure of layers (on the days that we chose to sample on) and a more homogeneous coverage of the area would include sampling of those layer clouds. This explanation is supported by Fig. 14, as new high cloud layers that are separated from the clouds below, are expected not only to have low values of LWC, but also small r_e (Fig. 14). The LWC in those elevated cloud layers would increase little with height due to the ~~cold~~ low temperatures, but r_e might increase rapidly due to the low concentration of CCN and hence also cloud droplets, even in the undiluted parts of the clouds.

This doesn't make any sense to this reader; "re might increase rapidly.." Where?

Another possible contributor to the changing trend of LWC over the Golan Heights at temperatures below −8 °C is the conversion of liquid cloud water into ice (which is not measured by the CDP or by the Hot-wires). The concentrations of the hydrometeors (Fig. 16) are at maximum at that temperature and that might not be just coincidence — as discussed in Section 4.3. In order to determine the causes and the effects of the observed relations, as well as the contributions of the seeding operations versus the natural processes, other methods are required.

Again, why spend so much on research targeting natural clouds when seeding is going on? Its crazy.

So, within the scope of this study, it is practically impossible to quantify the relative impacts of the explanations above to the observed different trend of LWC versus the temperature over the sea and over the Golan Heights without analyzing a larger dataset and a more dedicated planning and filtering.

Yes! Thank you, authors, for this candid assessment. (I would love to take part in those additional studies!)

However, despite the reduction in LWC with altitude, and regardless of its true explanation, there are still non-negligible amounts of super-cooled LWC over the Golan Heights.

The HUJ-CSU authors seem to spin this sentence toward their possible funder, the IWA. The end result of the supercooled water they observed at the upwind side of the Golan is not known, and contrary to this partial description, the seeding potential would also not be known.

As far as cloud seeding is concerned, the relevant question is the precipitation efficiency of the orographic layer clouds. Obviously, this type of cloud is inexistent over sea. Fig. 18 provides the probability distribution and the cumulative distribution function of the supercooled water content over the (upwind side of the) Golan Heights at temperatures ~~colder~~ lower than −8 °C.

You can't emphasize the sampling bias contained in this diagram enough;otherwise it will likely mislead the IWA or other organizations into funding cloud seeding with no real return, as we saw the HUJ-CSU caused the IWA to do in the past at great expense to the people of Israel.

While some of the water might have been converted to ice due to the seeding operations, the remaining liquid water might mark the potential of additional precipitation enhancement. It shows that 55% of the measurements in the clouds had less than 0.2 g m^{-3} of super-cooled water, but it means that 45% had more. 20% of the data had more than 0.5 and ~4% even more than 1.0 g m^{-3} of liquid water with a potential to freeze with silver-iodide.

"Spin on", authors, for your potential funders. This sentence is absent the very nice qualifiers you incorporated in this piece at other points. Were these questionable portions written by only one of the authors? One wonders.

Since the flight pattern over the Golan Heights was more or less fixed, this represents the true areal fractional coverage of these clouds during the flights there. Therefore, this represents significant amount of super-cooled cloud water and indicates that ice nuclei may still be a limiting factor in the conversion of LWC into ice and further to precipitation. So there might be room for extending the seeding efforts that target the clouds over the Golan Heights to realize more of their seeding potential.

5. Summary and conclusions

A key factor that determines the seeding potential of the clouds is the quantity of super-cooled water in the clouds, the time that the water droplets remain in the super-cooled region and the temperatures at which supercooled water persists.

Add this sentence after "persists": "We were unable to do all the necessary maneuvers to establish these parameters."

More super-cooled water, longer time and ~~colder~~ lower temperatures, all lead to greater seeding potential as there is a possibility to convert more of the super-cooled water into ice and precipitation.

After analyzing 27 research flights during four rainy seasons (2009–2013) we see that although the natural processes of converting cloud water into precipitation is often quite efficient,

Yep.

there are still occasions with good potential for precipitation augmentation by glaciogenic cloud seeding.

Remains to be seen. "Good potential" was not demonstrated in this paper. There were too many unanswered questions. This sentence appears to have been written for funders, not for those of us senior researchers who know the paper trail of the Israeli literature. One wonders how the present authors, who wrote so eloquently about the drawbacks of this study, and the questions that remain, could have written that sentence? Here the questioning and objectivity have disappeared.

One natural seeding process is the hygroscopic seeding by the abundance of sea salt aerosols in the boundary layer close to the coast due to the strong winds and rough seas. Another natural seeding process is called the "seeder–feeder" mecha- nism, where hydrometeors from more mature and higher clouds fall through the clouds at lower levels and collect or rime their water content.

In the scenarios, there well might be an underlying, often shallow orographic Stratocumulus layer that is present before the rainy/icy complex of Cbs arrive. If precipitation is falling over mesoscale regions, downdrafts can be present that weaken of dissipate the lower Stratocu layer as it barges into the hill regions and the Golan. I just don't think we have enough information to know what exactly happens, how much supercooled water there is when weakening Cb clusters/lines organized by upper troughs arrive in the northern mountains.

The results of this study have been already applied for the design of the Israel-4 cloud seeding experiment.

I find this statement sad, because this study was not reviewed properly, and those who assembled it have so much cloud seeding baggage, as is demonstrated in this review. I feel sorry, too, for the Israeli people who paid millions based on prior faulty interpretations of cloud seeding results and seeding potential based on fictitious cloud descriptions. The HUJ-CSU for too long has prospered on the backs of the Israeli people with their biased presentations of clouds and seeding potential.

Finally, why wasn't I sent this manuscript in the first place? One of the authors, DR, knows I am "above ground", and I do not oppose seeding in proper circumstances.

This was done by an addition of an eastern seeding line for targeting the clouds developing over the Golan Heights and by positioning the ground generators accordingly. The main objective of the Israeli rain enhancement program is to increase the amount of water that reaches the Sea of Galilee, therefore adding an eastern seeding line contributes to fulfilling this objective in four ways: 1) The Golan Heights contribute most of the water to the Sea of Galilee; 2) The clouds over the Golan Heights typically undergo less natural seeding by mature clouds and sea spray;

There is quite a bit of conjecture in the first claim. This study didn't quantify how much "less natural seeding by mature clouds" happens in the Golan. Such a study would take several winters in this reviewer's opinion, of combined satellite and surface observations, not just from a relatively small number of flights. To repeat, such a study shouldn't be done by the seeding unit at the HUJ. They had their chances over so many years to get it right, and couldn't do it.

Cloud tops are colder in extreme northern Israel and offshore region compared with those areas to the south (GN74; RH95). Decreased cloud top temperatures lead to more ice and deeper clouds. Presumably more Cbs, organized by troughs and the Cypress low, would move onshore and into the Golan in most rainy day situations. Whether the underlying orographic forcing would supply enough water for seeding purposes in the context of these mesoscale systems remains known. This is another situation where surface obs on Mt. Hermon would help by addressing the character of the precip and how much riming takes place.

3) The clouds over the Golan Heights occasionally have relatively high concentrations of supercooled water at temperatures in which silver iodide can serve as efficient ice nuclei;

Add this sentence: "Due to flight limitations, it is not known whether this liquid water persists long enough to be a viable seeding target."

4) There is a greater seeding potential for the less convective clouds (King and Ryan, 1997), such as those clouds over the Golan Heights.

Who knows? Don't count on it, Israel. Be cautious.
A further indication of citation sloppiness, and a poor manuscript review of this article; "King and Ryan" should be Ryan and King (1997).

This whole article has degraded *AR* in my opinion.

The Israel-4 randomized seeding experiment started in November 2013. The expected duration of the experiment is about 6 winters, during which the randomized seeding will be done in the same consistent way, and further accompanied by the cloud physics measurements by aircraft and satellites.

Acknowledgements

This study was funded by the Israeli water authority for supporting the rain enhancement project.

Very sad to read that this article, or some other version of it, led to IWA to seed.
Doesn't the IWA have any cloud expertise within it, or consultants to rely on? It doesn't seem like it.

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