Comments on ''A New Look at the Israeli Cloud Seeding Experiments''

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As one of three reviewers of ''A New Look at the Israeli Cloud Seeding Experiments,'' I have been afforded the opportunity to comment on the paper by Rangno and Hobbs (1995) (hereafter referred to as RH). I intend to confine my remarks to the portion of the paper dealing with cloud microstructures in Israel.

My main continuing problem with the section of the paper by RH that deals with cloud microstructures in Israel is that the authors do not pay enough attention to the importance that cloud life cycle plays in determining internal cloud structure. From my perspective, time is the primary consideration when planning, making, and interpreting cloud microphysical measurements. The question to be addressed in any glaciogenic seeding experiment for rain enhancement is not whether a cloud ultimately develops ice at a particular cloud temperature, but when that ice forms in relation to the updraft and to the supply of cloud water that is needed for the growth of the ice crystals and/or graupel and, finally, precipitation. In my experience, young vigorous clouds contain much less ice and more cloud water than old and dying clouds, unless the young tower has been seeded naturally by ingesting debris from aged glaciated towers.

Rangno and Hobbs (1995) claim that the plot in their Fig. 12, relating cloud-base temperature to the cloud temperature at which ice concentrations of 1 L^{-1} were noted, is evidence that the Israeli measurements (Gagin 1975) are anomalous to measurements made elsewhere. If evidence were proffered that the measurements in all studies were made at the same relative time in the cloud's life cycle, I would be the first to agree with them. Without it, I cannot concur.

The plotted points for two other studies, Willis and Hallett (1991; point 31) and Heymsfield et al. (1979; point 10), are just as anomalous relative to the others as are the Israeli measurements reported by Gagin (1975; point 8). It is odd that RH said nothing about them. Why these two studies are anomalous is evident from their titles. That by Heymsfield et al. (1979) is entitled ''Ice Initiation in *Unmixed Updraft Cores* in Northeast Colorado Cumulus Congestus Clouds,'' and that by Willis and Hallett (1991) is entitled ''Microphysical Measurements from an Aircraft Ascending with *a Growing* Isolated Maritime Cumulus Tower'' (emphasis mine). These clouds remained supercooled to colder temperatures than would have been predicted from the plot of the other data points in Fig. 12 of RH because the observations were confined to the updraft regions of growing clouds. This is just the situation in which low ice crystal concentrations would be expected (Young 1974).

Having worked with Dr. Gagin while he served as an advisor to the Florida Area Cumulus Experiment (FACE) and later the Southwest Cooperative Project (SWCP) in western Texas, I am aware of the emphasis that he put on the study of young vigorous clouds. Such clouds were, in his view, the only suitable candidates for seeding, in conformance with the Israeli cloud seeding conceptual model. It seems obvious, therefore, as pointed out correctly by RH, that the Israeli measurements are ''anomalous,'' in part because they were made in a particular class of cloud that normally has little natural ice. Methods of analysis and interpretation may also have played a role, as suggested by RH.

Because of this life cycle uncertainty, I put no stock in Fig. 12 of RH as proof that the Israeli measurements reported by Gagin (1975) are any more anomalous than those made anywhere else in the world. Figure 12 is valuable, however, in demonstrating that clouds with warm cloud bases are more likely to contain ice crystals at warmer temperatures than those with colder bases. This is due presumably to the increased likelihood of drop coalescence in warm-based clouds and the production of ''secondary'' ice particles when these drops freeze.

Even if RH are correct in asserting that Israeli clouds contain more ice than believed previously, the question remains of whether the existence of this ice at some point in the cloud life cycles necessarily means that they have no seeding potential. I do not think that it does, any more than the existence of ice in the supercooled

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TABLE 1. Summary of the mean maximum microphysical measurements in the unseeded supercooled convective clouds of western Texas.

	Undraft cores				Downdraft cores			
Time interval (min)	*N*	Updraft (m s^{-1})	SLWC (g m^{-3})	Ice con. (L^{-1})	*N*	Downdraft (m s^{-1})	SLWC (g m^{-3})	Ice con. (L^{-1})
0	10	6.9	4.11	29	10	7.3	2.68	30
1–4	10	5.6	2.86	55	11	6.1	2.27	57
5–9	10	6.2	3.25	36	12	5.8	2.24	100
10–16	6	7.1	3.15	180	6	4.9	1.22	296
>16	4	6.2	1.78	236	5	6.5	2.09	308

convective clouds of Florida, Texas, or Thailand means that they have no glaciogenic ‘‘dynamic’’ seeding potential.

This is readily illustrated by recent measurements in supercooled convective clouds that were made in the TEXARC (Texas Experiment in Augmenting Rainfall through Cloud Seeding) project in August 1994 by instrumentation aboard the T-28 of the South Dakota School of Mines and Technology. TEXARC (formerly the SWCP) is the continuing effort by the state of Texas to evaluate the potential of dynamic cloud seeding to augment the natural rainfall in western Texas. The most recent results and the revised dynamic seeding conceptual model have been discussed by Rosenfeld and Woodley (1993).

The microphysical findings to be presented here are preliminary and are for purposes of illustration for this commentary. They are subject to revision as the analysis proceeds. Ultimately, our analyses and their interpretation must pass the same critical peer review as did RH in the long laborious process that led to the publication of their paper.

It is important at the outset to emphasize three points. First, internal cloud structures are highly variable in space and time. Consequently, high natural intercloud variability and high intracloud spatial and temporal variability make generalizations from a few measurements a highly risky business.

Second, the sample used in this illustration is highly skewed, consisting of some clouds that contained virtually no ice out to 10 min after the initial aircraft penetration, whereas at least two clouds contained substantial quantities of ice at and shortly after the initial pass by the aircraft. The clouds containing substantial quantities of ice appeared to have undergone a natural seeding process, whereby they appeared to ingest icy debris from clouds that had preceded them.

Third, cloud microphysics is not an exact science. The measurements themselves and their interpretation are fraught with uncertainty, as is obvious from a careful reading of RH and Gagin (1975). Add to this the possibility that the T-28 resulted in aircraft-produced ice particles, a problem identified first by Rangno and Hobbs (1983, 1984) and followed up by a number of scientists (e.g., Woodley et al. 1991), and one can begin to appreciate the difficulty of microphysical investigations.

The measurements in Texas, which were made on 8 days in August 1994 in which the cloud-base temperature ranged between 10° and 17°C, are summarized in Table 1. The entries represent mean maximum values in the updraft and downdraft cores encountered during aircraft penetration. The measurements, partitioned into time intervals relative to the time of the initial cloud pass ($t = 0$), were obtained at temperatures ranging between $-8°$ and $-10°$C. The estimates of cloud water and ice concentration were made from the measurements of FSSP and 2D-C probes, respectively. The inferences of ice crystal concentrations from the observations of the 2D-C probe were made from the ‘‘shadow counts,’’ after attempting to eliminate the shadow counts that were obviously due to water drops and not to ice particles. Crude estimates of cloud drafts were made in most cases using the rate of change of aircraft altitude with time while the pilot flew at constant attitude.

Based on the limited sample reflected in Table 1, the following comments are made.

1) The sample is uncomfortably small, although it is larger than the sample utilized by several of the studies referenced by RH in their Fig. 12.
2) The mean maximum updrafts and downdrafts decreased very little with the time interval after the initial cloud pass out to time intervals greater than 16 min. However, the mean maximum supercooled liquid water content (SLWC) decreased and the mean maximum ice content increased with time after the initial pass. These trends are in accord with our expectations.
3) The SLWC was higher and the ice crystal counts lower in the updrafts than in the downdrafts—again in agreement with our expectations.
4) The FSSP estimates of the SLWC are likely overestimates of the true values when ice was present in the cloud. In addition, although an attempt was made to remove the effect of false ‘‘streaker’’ and water drop counts in the 2D-C measurements, the ice concentrations may be too high on some of the passes, particularly the initial ones, that went into the calculation of mean values.
5) Adopting a dynamic seeding conceptual model and a desired ice crystal concentration of greater than 100 L^{-1}, the measurements suggest that the average window of opportunity for seeding intervention for

this sample is only about 10 min, after which the mean maximum ice crystal concentrations are greater than 100 L^{-1}. Those clouds that contained no ice initially had larger windows, whereas those that appeared to have been seeded naturally had no window at all.

6) Considering the rapidity of ice formation, a so-called static seeding conceptual model, requiring low natural ice crystal concentrations (e.g., < 10 L^{-1}), may not be relevant for this sample of clouds.

It should be noted that the observations that were used in the calculation of the means presented in Table 1 were highly variable from cloud to cloud and from day to day. (This variability will be quantified when this work is submitted to peer review.) If one looks hard enough, a subset of data could be found to support virtually any conceptual model of cloud development and strategy for seeding intervention.

The lessons to be learned from this example are many. First and foremost is the importance of life cycle in evaluating the suitability of clouds for seeding intervention. That, in my view, is the critical element that is missing from the critique of the Israeli experiments by RH. Even in west Texas one would be horribly misled as to seeding potential there without the benefit of cloud life cycle measurements. Certainly, one would reach a much different conclusion as to dynamic seeding potential if that conclusion were based on measurements made more than 10 min after the cloud moved through the level of sampling at its time of maximum vigor.

Some might even question whether a 10-min window is enough to effect a dynamic seeding response in the clouds. The theoretical work of Lamb et al. (1981) suggests that it is. Further, Gagin et al. (1986) found that only young clouds in Florida appeared to respond to dynamic seeding intervention. These Texas measurements provide insights as to why this should be the case.

In addition, our recent findings (Rosenfeld and Woodley 1994) that the apparent increases in mean cell height, area, duration, volume rain rate, and rain volume in Texas and Thailand are greater for warm-based (i.e., $T > 16°C$) clouds also indicate that the window is open long enough for seeding intervention. Ironically, it is the warm-based cloud with active coalescence, rapid natural glaciation through an ice multiplication process, and the smallest of time windows for seeding intervention that shows the greatest apparent response to seeding. The window may be small but the response is great, if seeding is done at the right time.

The second lesson to be learned from this exercise is the importance of knowing the size of the seeding window in Israeli clouds. Gagin (1975) would imply that the seeding window is large and always open in the clouds of Israel. Certainly *his* measurements suggest that is the case. If RH are to convince me otherwise, they must come up with *their* estimate of the size of the seeding window in Israeli clouds. Their Fig. 12 does nothing for me in this regard.

I suspect, but cannot prove, that the window is open long enough in Israeli clouds for seeding to be effective, but suspicions, feelings, and hunches don't count for much in the meteorological sciences. Addressing this uncertainty should be a major focus of the continuing Israeli program.

Rangno and Hobbs (1995) raise a valid point concerning the microstructures of Israeli supercooled convective clouds. Had they been able to show that young vigorous clouds contain ice particle concentrations of 1–10 L^{-1}, I would be the first to admit that something is awry with the Israeli experiments and their interpretation. But Rangno and Hobbs have not been able as yet to demonstrate that this is the case. Until they do, this matter remains open to debate.

A positive aspect of the RH challenge is the implicit emphasis that the challenge puts on the need for additional microphysical measurements in Israeli supercooled convective clouds. National and international funds continue to go into a number of worthy (and not so worthy) joint research efforts. The paper by RH underscores the importance of a joint international cooperative research effort to study the clouds of Israel. Since so much appears to be riding on the success of the Israeli program, such a joint effort cannot come one day too soon.

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