CLOUD AND AEROSOL RESEARCH GROUP

Comprehensive Reply

by

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to

"Comments on 'A New Look at the Israeli Cloud Seeding Experiments'" by D. Rosenfeld*

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*Rosenfeld's Comments appeared in *J. Appl. Meteor.*, **36**, 260-271, 1997.

Rangno and Hobbs' brief Reply was in the same issue (*J. Appl. Meteor.*, **36**, 272-276).

May 1997

This Extended Reply is also available on the Cloud and Aerosol Research Group's home page:

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NOTE ON THE BACKGROUND TO THIS COMPREHENSIVE REPLY

The Israeli cloud seeding experiments hold a unique place in meteorology, because they have been widely viewed as one of the very few (perhaps the only) demonstration that precipitation on the ground can be significantly modified by artificial seeding.

In 1995 we published a critique of the Israeli cloud seeding experiments (Rangno and Hobbs 1995). Subsequently, D. Rosenfeld, a former student of the late Abraham Gagin (a leader of the Israeli experiments), who is continuing the work of his mentor, submitted for publication in the *Journal of Applied Meteorology* extensive Comments on our critique (Rosenfeld 1997). We prepared a comprehensive Reply to Rosenfeld's Comments. However, at the request and advice of the Editor of the *Journal* (R. Koenig), we published a considerably abbreviated Reply (Rangno and Hobbs 1997). In the first paragraph of that abbreviated Reply, we noted that we would make available our detailed rebuttal to Rosenfeld's Comments; the present report is that rebuttal.

Discussions of the pros and cons of the Israeli experiments, involving as they do both physical and statistical arguments and assessments of the relative roles of natural meteorological processes and artificially-induced effects, are necessarily detailed, often lengthy, and not always conclusive. However, as in a good detective story, attention to detail has its rewards. In carrying out our studies of cloud seeding over many years, we have certainly learned the wisdom of Richard Feynman's philosophy, as summarized by Gleick (1992): "He believed in the primacy of doubt; not as a blemish upon our ability to know but as the essence of knowing."

Peter V. Hobbs
Arthur L. Rangno

May 1997

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1. Overview

Rosenfeld (1997) (hereafter R) agrees with us (Rangno and Hobbs 1995-hereafter RH95) that earlier reports by A. Gagin and his associates[1] that the clouds of Israel contain few natural ice particles at cloud top temperatures above -21° C, do not contain droplets ≥23 µm diameter in the riming-splintering temperature zone (-2.5 to -8° C), and never form rain by the collision-coalescence process are all incorrect. As anticipated by Rangno and Hobbs (1988), it has now been observed that concentrations of ice particles of 20-300 per liter occur in clouds in Israel at cloud top temperatures ≥-14° C (Levin 1994; Levin et al. 1996), even though it was reported for many years that these clouds were virtually free of ice until cloud top temperatures fell to -14° C or less, and averaged only 3 per liter at -20°C (e.g., GN74, GN76, GN81, G75, G80, G81, G86, GG87). Also, precipitation does form in Israel from both the ice process and the collision-coalescence mechanism (e.g., Rangno 1988).

Even though R now agrees with us on these basic new facts about Israeli clouds, our views diverge sharply on two main points: (1) whether it has been demonstrated that the artificial seeding of clouds increased rainfall in Israel during two randomized statistical experiments, and (2) whether naturally high ice particle concentrations reduce seeding potential.

Rosenfeld believes that cloud seeding in *both* Israeli experiments caused widespread increases or decreases in rainfall, depending on the absence or presence of dust/haze. For example, R contends that seeding had effects on precipitation in the various target areas of the Israeli experiments, in the buffer zone (which was designed not to be seeded), and in Jordan and Lebanon. We, on the other hand, concluded that the HUJ investigators misinterpreted natural patterns of rainfall in both of the Israeli experiments as being due to seeding effects. We arrived at this conclusion because:

• Similar (in some cases even larger) anomalies in rainfall to those that the HUJ investigators attributed to cloud seeding occurred in both of the Israeli cloud seeding experiments in regions where any effects of seeding should have been minimal or non-existent.

• Although Rosenfeld and Farbstein (1992—hereafter RF92), Rosenfeld and Nirel (1996) and R agree with RH95 that warm rain and high ice particle concentrations are common in clouds in Israel with tops warmer than -21° C, R believes that such findings do not compromise, nor cast any doubt, on the contention that rainfall in Israel can be increased by cloud seeding. This is because R believes that the presence of warm rain and/or ice multiplication does not affect the static seeding potential of polar maritime clouds over Israel, *except* when those clouds are also affected by dust/haze from deserts to the southwest of Israel. Rosenfeld and Farbstein (1992) and Rosenfeld and Nirel (1996) believe that excessive ice formation caused by dust/haze makes the clouds of Israel unsuitable for seeding on about half the days with rain in northern Israel (north of Tel Aviv), and for the majority of the days with rain from approximately Tel Aviv southward (i.e., most of Israel). Rosenfeld and Farbstein concluded that seeding on dust/haze days may have *decreased* rainfall by about 10-25%. We believe the latter analysis is flawed, and that seeding had little effect on rainfall under any conditions (see Section 4).

Rosenfeld believes that the potential for increasing rainfall by cloud seeding is not compromised even when the targeted clouds naturally produce high concentrations of ice particles soon after cloud tops cool to below -10° C. We, on the other hand, as well as many others (e.g., Braham 1964, 1979; Dennis 1980, 1989; Sax et al. 1975; G75; Cotton 1986; Silverman 1986), including R in 1989, believe that high natural ice particle concentrations in moderately supercooled clouds severely compromise their *static* seeding potential.

Today we know that the clouds of Israel, even with tops warmer than about -14° C, have 10s to 100s per liter of natural ice particles (Levin 1994; Levin et al. 1996); that the onset

[1] Gagin and Neumann 1973, 1974, 1976, 1981—hereafter GN73, GN74, GN76, GN81; Gagin 1975, 1980, 1981, 1986—hereafter G75, G80, G81, G86; and Gagin and Gabriel 1987—hereafter GG87. We will refer to these workers collectively as the "Hebrew University of Jerusalem (HUJ) investigators."

of precipitation from clouds in Israel occurs at cloud top temperatures >-10° C (e.g., Rangno 1988; Rosenfeld and Gagin 1989); and, that the collision-coalescence process for rain formation is active in Israel (Rangno 1988). Therefore, by inference, the criteria for the production of high ice particle concentrations by the Hallett-Mossop mechanism are met in Israel. Consequently, the numerous claims by the HUJ investigators that ice particle concentrations are perpetually low in Israel, and therefore cloud seeding might increase rainfall in Israel when cloud temperatures are between -12° and -21° C, now lacks a physical foundation.

We (RH95) pointed out that, irrespective of the presence of dust/haze, cloud base temperatures in Israel (generally >-5° C) virtually guarantee high natural ice particle concentrations when cloud top temperatures are in the range where the strongest seeding effects were reported by the HUJ investigators (-12° to -21° C). Moreover, fetches of moist, unstable boundary layer air over the Mediterranean Sea virtually guarantees the addition of large salt and biogenic particles that can contribute to the broadening of the cloud droplet spectra. Thus, the days on which large cloud droplets, naturally high concentrations of ice particles, and warm rain can compromise cloud seeding to increase rainfall are likely more numerous than those days in which precipitating clouds are affected by dust/haze.

Rosenfeld's proposal that dust/haze causes immediate glaciation in moderately supercooled clouds is an interesting, but unproven, hypothesis. On the other hand, the profound effects of large cloud droplets ($\gtrsim$ 23 µm diameter) in producing high ice particle concentrations in maturing and aging and moderately supercooled clouds is an established fact (e.g., Koenig 1963; Braham 1964; Mossop 1970, 1985; Ono 1972; Hobbs et al. 1980; Hobbs and Rangno 1985, 1990; Rangno and Hobbs 1991, 1994).

• In the first Israeli cloud seeding experiment (hereafter Israeli I), seeding was carried out during only about 25-35% of the time that precipitation fell, or 65-70 h per entire rainy season per target area. Indeed, the number of *hours* of seeding per target is only slightly larger than the average number of *days* with rain (≃ 55)!

• Contrary to the assertion of R, the results of the second Israeli cloud seeding experiment (hereafter Israeli II) have not been fully reported. Both Gabriel (1967) and GN74 emphasized that all days, *including dry days*, must be included in the evaluation of Israeli II (as they were in Israeli I). Such an evaluation for Israeli II has not appeared. Second, RH95 noted that rain fell in northern Israel on days that were not included in the statistical analyses (e.g., GN81). Third, the statistical crossover results that have been reported for Israeli II do not replicate those reported for Israeli I, either *in toto* or in the details of the target areas that experienced apparent seeding effects (e.g., Gabriel and Rosenfeld 1990—hereafter GR90).

At first glance, it would appear that seeding in Israeli II decreased rainfall in the south target area (STA) but increased it in the north target area (NTA) by almost exactly the same amount, resulting in a null crossover result (GR90). However, it has been established beyond a doubt that heavy rainfall in the NTA on seeded days was also experienced on those *same* days in the south target area (e.g., GR90) and in central and southern Lebanon (RH95). Thus, rainfall was *not* lighter than normal in the STA on seeded days, rather it was much heavier than normal on *control* days (which were the days on which the NTA was seeded). These facts, first established by GR90, are a formidable refutation of RF92 and R's hypothesis that seeding decreased rainfall in the STA of Israeli II. Consequently, we concluded that a "false positive" (in the NTA) and a "false negative" (in the STA) occurred in Israeli II, one of the several explanations offered by GR90.

• Control and target stations were not specified in advance of either Israeli I or II, a condition that is necessary for proper statistical experimentation (e.g., Thom 1957; Court 1960; Dennis 1980). The control and target stations in Israeli II were unevenly distributed, with a clustering of control stations in zones where seed/no seed precipitation ratios were anomalously low from a regional viewpoint (Fig. 1); target and control stations also changed from one analysis to another. Nor were the target stations evenly distributed, except in Gabriel's (1967) analysis of Israeli I (Fig. 1a). More significantly, the target and control stations used by GN81 in their analysis

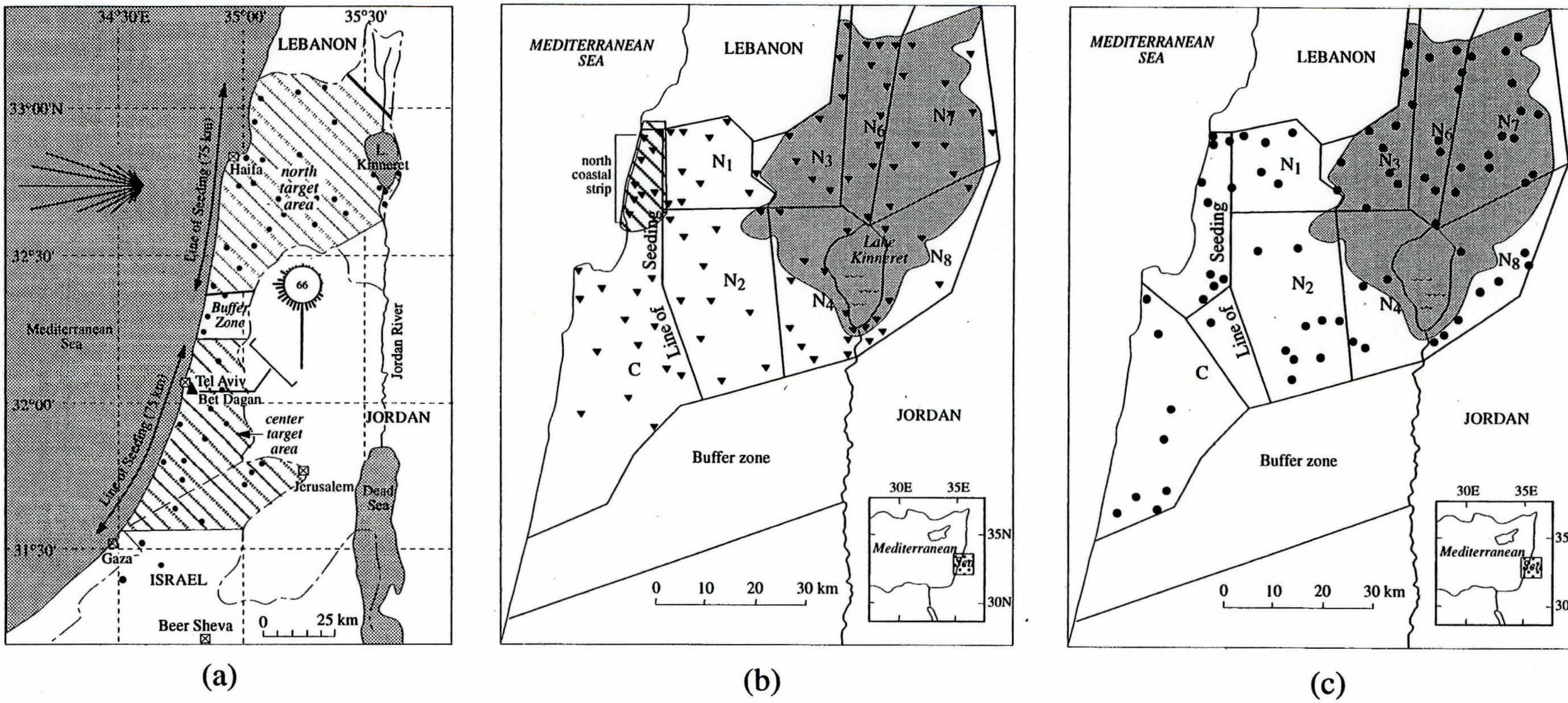


Figure 1. a) Rainfall stations (dots) used by Gabriel (1967). See Fig. 13 of RH95 for further information. b) Rainfall stations (triangles) used by GN81 in their target-control analysis of Israeli II. The shaded area represents the catchment area of Lake Kinneret (the primary target area for Israeli II). The hatched area denotes the extreme northern coastal strip from which GN81 chose nine control stations. The complete control area (from which a total of seventeen control stations were chosen by GN81) is marked by "C". The various target sub-areas analyzed by GN81 are denoted by "N". (c) The target (N) and control (C) rainfall stations used by GR90 (dots). A surface wind rose is shown in (a) for Bet Dagan, Israel, for those rawinsonde launches when rain was falling at or within 60 km of Bet Dagan, and at or within about 90 min of the launch time, for the period January-March 1978, November - March, 1978-79 through 1984-85, January - February 1986, and November through March 1986-87. The number in the center of the wind rose represents those cases with a calm wind.

of Israeli II are not the same as those used by Gabriel (1967) to evaluate Israeli I (Fig. 1b) in those regions where the target and control areas overlapped in the two experiments. Finally, we note that Gabriel and Rosenfeld (1990) were apparently not able to duplicate the results of GN81 when they used data from the same stations; they therefore used many different stations (compare Fig. 1b with Fig. 1c). Our concern in this matter is that there were more than 500 raingauges to choose from at the time of Israeli I (*Atlas of Israel* 1970), and 436 Israeli rainfall stations had complete records during the second Israeli experiment (personal communication, 1997, from R. Ben-Sarah, Chief of Climatic Verification Section, Israel Meteorological Service). The HUJ investigators used only 182 raingauge stations in the analysis of Israeli II (GR90).

In the remainder of this Reply, we respond in more detail to Rosenfeld's specific comments on our paper. For the convenience of the reader, our responses are grouped under the following headings: cloud microstructures, seeding and seeding logistics in Israeli I, and statistical evaluations of the Israeli experiments. In the final section, we make a few concluding remarks.

2. Responses to Rosenfeld's specific comments concerning cloud microstructures

In his Abstract, R states: "the existence of coalescence and ice multiplication in some of the Israeli clouds in no way precludes enhancement of precipitation (from cloud seeding) even from those clouds."

First, high ice particle concentrations are likely to occur in most (not "some") Israeli clouds. This is because warm (>5° C) cloud bases predominate, and the air masses that bring rain-bearing clouds to Israel generally have an appreciable fetch across the Mediterranean Sea. Both of these factors give rise to large cloud droplets, which are conducive to ice multiplication. In addition to these factors, R has postulated that dust/haze, which he asserts occurs on more than half of the days with rain in Israel, causes or amplifies ice multiplication (RF92). In fact, RF92 concluded that because of dust/haze, cloud seeding in Israel can actually decrease rainfall.

In none of the many analyses of Israeli II (GN76, GN81, G81, G86) was it found that the seeding of clouds that contained naturally high ice particle concentrations (previously believed to be confined to those clouds with echo tops <-21° C) produced any effect on rainfall. Therefore, in asserting that ice multiplication in Israeli clouds does not affect their cloud seeding potential, R contradicts what the HUJ investigators stated for many years and in numerous publications, namely, that the presence of high ice particle concentrations has a drastic effect on the potential for increasing rain in Israel by glaciogenic static seeding. Further, R (1989) himself claimed there was no static seeding potential in these cases.

In his Abstract, R states that we concluded that there is no physical basis for glaciogenic seeding in Israel.

Because of natural ice multiplication, we doubt that there is seeding potential for clouds with top temperatures in the range for which the greatest seeding effects in Israel were reported by the HUJ investigators (viz., -12° to -21° C). In Rangno and Hobbs (1988) and in RH95, we suggested that the onset of significant concentrations of ice particles in Israeli clouds occurs near -10° C. If this is correct, a static seeding potential might be present in some clouds with tops warmer than about -10° C. However, the HUJ investigators concluded that the rainfall produced by seeding such clouds is trivial (e.g., G86) because the seeding agent is ineffective at these temperatures. Moreover, in winter in Israel, clouds with top temperatures around -10° C are relatively thin (<3 km), it was argued, and the growth of precipitation embryos is limited (e.g., G81, G86). However, we noted that the results of seeding clouds in northwest flow at 850 hPa appeared promising, as reported by GN74, since such seeding might sometimes involve clouds with low cloud base temperatures and perhaps less likelihood of naturally high ice particle concentrations.

In summary, based on new physical insights and observations of the microstructures of clouds in Israel, we (RH95) suggested that a small seeding "window" might exist in Israel for clouds with top temperatures between about -5° and -10° C, as suggested by statistical analyses of Israeli I and II (e.g., GN74, GN81, G86, GG87).

In his Introduction, R acknowledges that (as first pointed out by RH88 and Rangno 1988) cloud structures and the processes leading to rain formation in Israel are more complicated than reported for many years by the HUJ investigators. However, R believes that seeding can still increase rainfall from wintertime clouds in Israel, even though ice multiplication and droplet growth by collision-coalescence occur in these clouds.

Rainfall in Israel is dominated by clouds in polar air masses in which ice multiplication and/or droplet growth by collision-coalescence are active. We believe that rainfall from such clouds is unlikely to be enhanced by cloud seeding because: 1) the clouds would normally contain high ice particle concentrations at relatively modest supercoolings; 2) these clouds have little capability of responding "dynamically" because of their relatively low liquid water contents; and, 3) the clouds are often capped by strong stable layers that restrict their vertical growth and therefore dynamic seeding effects (e.g., Druyan and Sant 1978; Rangno 1988).

In his Section 2a, R expresses his conviction that the potential for increasing rainfall through the seeding of clouds with naturally high ice particle concentrations (due to ice multiplication) is no less than for low ice-producing clouds that were previously thought to exist in Israel.

It was thought previously that clouds in Israel had low ice particle concentrations and were therefore ripe for seeding at cloud top temperatures $\geq$-21° C. Today, the evidence strongly suggests that clouds over Israel frequently contain high ice particle concentrations, except perhaps at temperatures >-10° C, and that they can develop rain solely by the collision-coalescence process. Rosenfeld's view that these new findings having little impact on cloud seeding potential in Israel is consistent with his refutation of the evidence for "lucky draws" in Israeli I and II. This is because it is not possible, on the one hand, to accept that the clouds in the primary seeding "window" defined by the HUJ investigators (i.e., cloud tops from -12 to -21° C) have little seeding potential (because of the high natural ice particle concentrations now known to be present in these clouds) and, at the same time, accept the hypothesis that artificial seeding increased rainfall in the Israeli experiments.

In his Section 2b, R questions the cloud top temperature range assigned by us to the clouds shown in Fig. 9a and 9b of RH95. He asserts that we deduced that haze was present from these photographs alone.

The cloud top temperature of "about -14° C" that we assigned to this cloud at 1600 LT was obtained as follows. From the height of the lifting condensation level (LCL), determined from the temperature and dew point measurements of IMS coastal stations at 12 and 15 UTC, the distance from the surface to the base of the cloud shown in Fig. 9a and 9b of RH95 was estimated to be 0.5 to 0.6 km ASL. To be conservative in our estimate of cloud top height, which we derived from the cloud base height estimate (e.g., Malkus and Scorer 1955), we increased the cloud base height to 0.7 km ASL. The cloud thickness in Fig. 9b of RH95 is 5.6 units of cloud base height, or 3.9 km thick. Adding the distance from the surface (0.7 km), results in a cloud top height of 4.6 km ASL. The IMS sounding from Bet Dagan, Israel, at 12 UTC (actually launched at about 1230 LT, or 3.5 h prior to the photographs in question), shows the temperature at 4.6 km to be -14° C (Fig. 11 in RH95).

Rosenfeld claims, erroneously, that his radar data is for the same times as the photographs shown in Fig. 9 of RH95. Our photographs were taken at 1556 and 1600 local time, which were 8 and 4 mins earlier than R's radar data, which was at 1604 LT (Fig. 1 in R).

Rosenfeld claims that the turrets associated with this cloud complex continued to grow (by 800 m in 5 mins) *after* 1604 local time, from 5200 to 6000 m ASL. At this rate of rise of the cloud top, it would have been 640 m lower, or at 4560 m, at 1600 LT. This is 60 m *lower* than the height we estimated in RH, but this would have a negligible effect on the temperature we estimated. Note that the top of this cloud was thus several degrees warmer at 1556 LST (Fig. 9a of RH95) when ice particles were undoubtedly already developing.

For the purpose of perspective on the behavior of this cloud relative to a radar climatology in Israel compiled by Rosenfeld and Gagin (1989), we note that the thickness of the cloud (≥3.9 km by either R's estimate or ours) is far greater, and the cloud top temperature is well below, the values for ice/precipitation formation in Israel (e.g., RH88; Rangno 1988; Gagin and Rosenfeld 1989; Levin 1994; RH95; Levin et al. 1996). Figure 2 shows our estimate of the height of this cloud above the freezing level relative to radar echo data in Israel from Rosenfeld and Gagin (1989). It can be seen that it would have been extraordinary if this cloud had *not* shown signs of converting to ice by the time it had reached -14° C.

Our statement that haze was present on this day was not based on our visual observations alone. Restricted visibility (10 km), which satisfies RF92's criterion for a "dust/haze" day[2], was reported on 15 January 1986 by the coastal stations of the Israeli Meteorological Service (IMS) at 1200 and 1500 UTC (1400 and 1700 local time)—also see Fig. 3.

In his Section 2c, R agrees that shallow clouds (with marginally supercooled tops) can produce precipitation in Israel. However, he asserts that these clouds are exceptions, because they have long lifetimes and tend to be more stratiform in character, and produce precipitation efficiently. He asserts that artificial seeding of cumuliform clouds with shorter lifetimes might result in enhanced rainfall by decreasing the time required for the formation of rain.

This is an old (and still unproven) cloud seeding hypothesis, which R now applies *ad hoc* to the Israeli experiments. On the one hand, there is little doubt that small, isolated clouds, which momentarily shoot turrets up to heights where the temperature is -5° C or lower, and which often do not produce precipitation naturally, can be seeded to produce spectacular results—*aloft* (e.g., Cooper and Lawson 1984). However, it is very unlikely that such clouds could produce the rainfall needed to explain the statistical results of the Israeli cloud seeding experiments, and, in particular, could not have produced the increase in runoff that Ben-Zvi (1988) has attributed to cloud seeding.

First, such clouds would rarely have been treated at the key moment in their life cycle (before the liquid water disappeared) by the aircraft line-seeding method used in Israeli I. Second, clouds with lifetimes too short to produce precipitation naturally are unlikely to reach temperatures as low as -12 to -21° C, which is the temperature range where strong seeding effects on precipitation were reported by GN76, GN81, G81, G86, and GG87.

In RH95 we showed that the development of precipitation in the clouds of Israel can be extremely rapid (~10 min). Therefore, any seeding "window" (assuming that it exists before high ice particle concentrations develop in ascending turrets that reach temperatures between -12° and -21° C) will be present only briefly. Is it likely that the aircraft line-seeding method used in the Israeli experiments, in which no attempt is made to target specific clouds, could have effectively treated clouds that naturally glaciate so quickly? We think not.

We agree with R that more airborne measurements, made in a variety of weather situations over a substantial period of time, will be needed to define the degree of "seedability" of Israeli clouds, to answer the questions we have raised concerning the efficiency of the aircraft line-seeding method used in Israeli I, and to establish the viability of the "corkscrew" seeding trajectories postulated by R to explain possible inadvertent seeding of the BZ in Israeli I (see Section 3 below).

In his Section 2d, R questions the representativeness of Levin's (1994) and Levin et al.'s (1996) measurements on Israeli clouds.

[2] Although the air carrying these clouds did not pass over deserts—see Figs 10a and 10b in RH95.

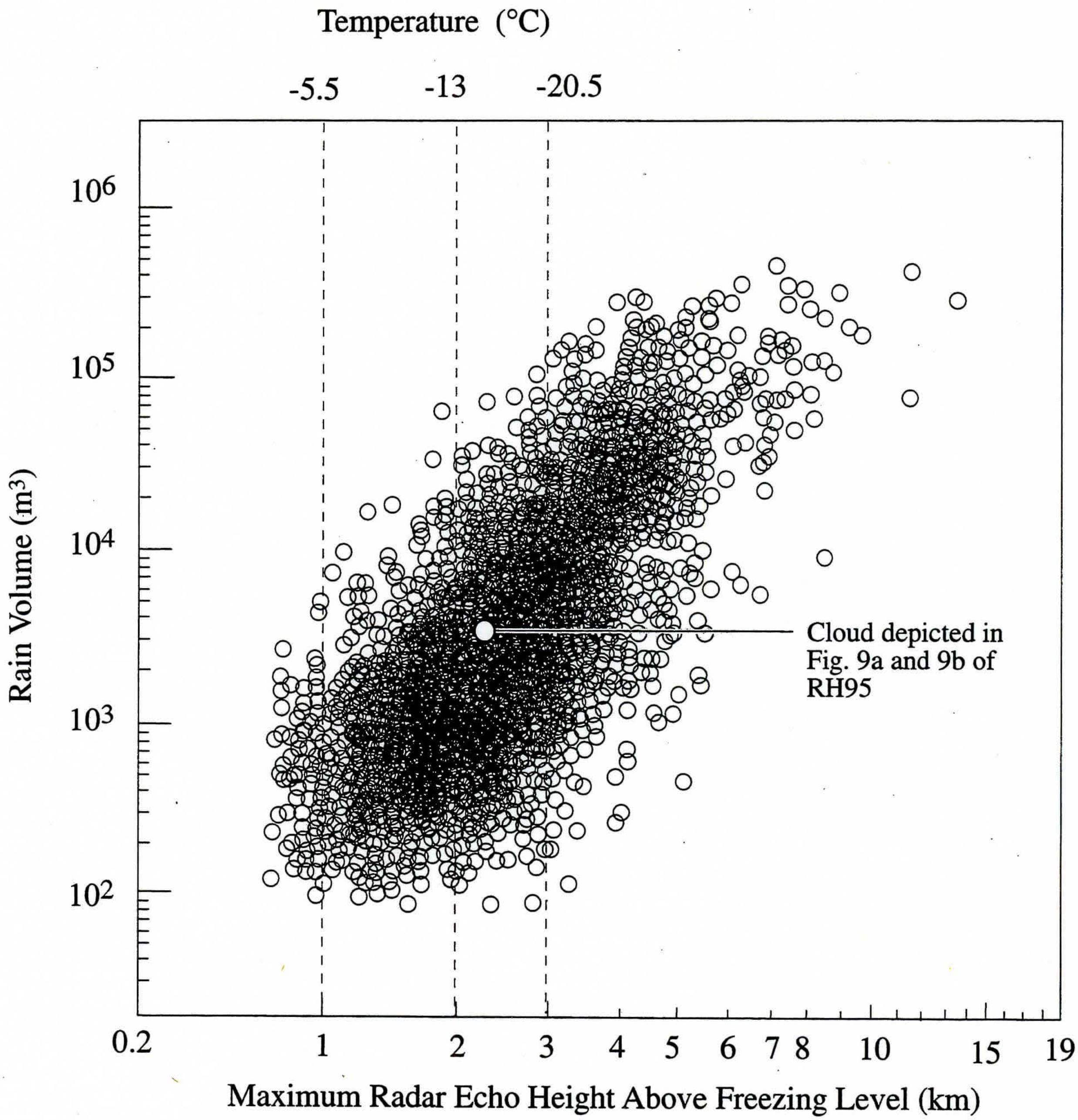


Figure 2. Volume of rain derived from radar measurements in Israel during the 1982-83 rainfall season as a function of maximum radar echo height above the freezing level (after Rosenfeld and Gagin 1989). The temperatures (shown at the top of diagram) were derived using the saturated-adiabatic lapse rate and a cloud base temperature of 8° C at 0.7 km ASL.

a) b)

Figure 3. Photographs taken on 15 January 1986 in windy conditions a) at 1458 LT from Tel Aviv beach looking to the south and showing a surface haze layer, and b) at 1600 LT from Tel Aviv beach looking to the west and showing crepuscular rays caused by aerosols below the bases of approaching stratocumulus clouds.

Apparently, Rosenfeld does not see the import of the recent airborne measurements of Levin (1994) and Levin et al. (1996). While R highlights how few measurements Levin obtained, he does not acknowledge how remote the chances would have been for Levin to obtain the measurements he did if the earlier reports by the HUJ investigators on the microstructures of clouds in Israel were correct. Had Levin sampled thousands of clouds, found only a few cases that differed from the earlier reports, and published those few cases, one would have reason to be concerned about their representativeness. But this is not what happened. In only a few measurements, Levin (1994) and Levin et al. (1996) found several key differences from the earlier reports. More importantly, Levin's few measurements are compatible with a global data set on "continental" cumuliform clouds (e.g., Rangno and Hobbs, 1988, 1995), and with the descriptions of Israeli clouds given by Rangno (1988).

In his Section 2e, R asserts that the profound difference between Gagin's cloud measurements and those of Rangno (1988) and Levin (1994) and (Levin et al. (1996) can be attributed to the fact that Gagin focused his airborne sampling on young clouds, and apparently had little regard for ice particle concentrations that may have developed later in the life cycle of the clouds.

Rosenfeld repeats the suggestion that we made in RH88, and again in RH95, to explain why Gagin and his colleagues did not detect ice multiplication in clouds in Israel. However, Gagin stated on numerous occasions that ice multiplication did not occur in Israeli clouds without any qualifications about cloud life history (e.g., GN74; GN76; GN81; G75; G81; G86). For example, G75, in discussing the results of the few aging clouds he did sample, wrote: "The probable absence of any significant time-dependent (ice) enhancement mechanisms...is further emphasized by the absence of any systematic or significant change of (ice) concentration with time."

In his Section 2f, R appears to contend that only dust/haze can produce ice multiplication and warm rain in Israel, and that dust/haze is limited to certain wind directions.

Many factors contribute to the large cloud droplets required for ice multiplication and warm rain, including high cloud base temperatures, low cloud condensation nucleus concentrations, large cloud condensation nuclei (which may or may not include dust/haze particles), and cloud depth. Once cloud droplets attain diameters in excess of about 23 µm, and reach slight to moderate supercoolings (>-20° C), ice particle production can be prolific (e.g., Mossop 1970; Mossop et al. 1967, 1968, 1972; Ono 1972; Hobbs and Rangno 1985, 1990; Blyth and Latham 1993), particularly when precipitation-sized drops are present (e.g., Chisnell and Latham 1976; Lamb et al. 1981).

With cloud bases averaging 5-10° C in Israel, there is no reason to believe that large drops, and therefore high ice particle concentrations, require the presence of dust/haze and certain wind directions. For example, we have not found in the literature a cloud droplet spectrum for a continental cumulus cloud with base temperature ≥5° C that did not have a droplet spectrum conducive to the production of high ice particle concentrations at 2-3 km above cloud base (which is typical of where the riming-splintering zone is located in Israeli clouds).

Some of the most spectacular cases of high ice particle concentrations that have been documented occurred in very clean air (e.g., Mossop *et al.* 1968; Hobbs and Rangno 1985, 1990). Further, Rangno (1988) described a case in Israel when rain was produced by the collision-coalescence process in clouds on a nearly calm day and ice formed in clouds with tops at -9° C embedded in a northwest flow. Levin (1994) reported ice particle concentrations of 10 per liter in clouds with tops at -10° C in northwest flow. In none of these cases was it likely that dust/haze from the deserts of southwest Israel affected the clouds.

In his Section 7a-c, R restates his belief in the dust /haze hypothesis of RF92 and its role in decreasing rainfall in the STA. He *cites several papers to support his assertion that dust/haze has a profound influence on cloud microstructures in Israel.*

If dust has a profound effect on ice formation in clouds (in essence, providing nearly instant glaciation and leaving no seeding "window"), why was not such a dramatic effect on clouds detected long ago, particularly in Israel (where measurements of clouds were made over many years by Gagin and his colleagues) or in the numerous measurements of ice nucleus concentrations and cloud structures that have been made throughout the world?

Are the apparent decreases in rainfall in Jordan, and throughout Israel and Lebanon, on STA seeded days in Israeli II (Fig. 4) really due to seeding on dusty days in the STA, as R would have us believe, or are they part of a widespread natural rainfall pattern that produced an illusion of decreases in rainfall due to seeding on STA days (and increases on NTA days)? We believe the answer is obvious.

In his Section 7a-c, R challenges our statement that there is no evidence in RF92 that dust was carried into the clouds in Israeli II.

In RH95 we questioned whether dust/haze observed at Elat in the extreme south of Israel meant that clouds that precipitated on Jerusalem, or on Lake Kinneret 12 h later, were affected by this dust. (In R's view, if any one of 13 IMS observing stations report dust/haze at any time of the day, that day qualifies as one in which precipitating clouds are affected by dust.) Neither in RF92, nor in subsequent papers, is there any evidence that the clouds on any particular day of Israeli II were affected by dust/haze.

On the other hand, we do not doubt that there are some days in Israel when clouds do contain dust particles. But do these particles have a significant effect on cloud microstructures and rainfall? Do they occur in rain-bearing clouds with long fetches across the Mediterranean Sea, which typifies most showery periods in Israel (e.g., Levin et al. 1996)? Are restrictions to visibility always caused by dust/haze? Are droplet concentrations in clouds on dust/haze days comparable to days without dust/haze? Does a few hours of seeding really cause significant decreases in rainfall from such clouds? Until answers are available to such questions, the dust/haze hypothesis will remain just that: an unproven, if interesting, hypothesis.

Also, consider the following implicit aspects of R's postulates about the clouds of Israel. While rejecting the pre-1988 reports on cloud microstructures in Israel by Gagin and his colleagues, R postulates (implicitly) that there are *two* processes that cause high ice particle concentrations in Israeli clouds. In the first type, which might be thought of a "good" ice multiplication (because, according to R, it leaves a "window of opportunity" for a rainfall enhancement caused by a dynamic-like response, albeit in clouds seeded via the static method!), there are high ice particle concentrations at cloud top temperatures <-12° C, but for a few minutes as these clouds ascend above the -10° C level, they have sufficiently low ice particle concentrations for silver iodide to be effective in accelerating and enhancing rain. For example, R believes that in Israeli I the seeding agent reached sufficient clouds during this narrow seeding "window" to have caused statistically significant increases in rainfall, including regions that were not targeted for seeding, and in which line seeding occurred on an average of only 4 h per day. We are not convinced.

The second type of ice multiplication postulated by R might be thought of as "bad" ice multiplication (because when it occurs R believes that seeding with silver iodide reduces precipitation, even in clouds with moderately supercooled tops). According to R, this type of ice multiplication occurs when precipitating clouds are present in northern and central Israel and dust/haze is reported in (usually) the dry portions of southern Israel. However, rainfall and dust/haze are usually not co-located either in time or space.

Rosenfeld envisions that dust/haze from the southern deserts of Israel affects clouds to the north in an extreme way: ice particles form with such rapidity and in such high concentrations, even in moderately supercooled clouds (tops ≥-21° C), that glaciation is virtually instantaneous. Therefore, there is no seeding "window" even in the building stages of these clouds. In fact, because so many more of these situations occur in central Israel, R believes that rainfall was decreased on seeded days in the CTA and the STA of Israeli I and II, respectively, due to "overseeding".

Rosenfeld and Farbstein (1992) did not reconcile their conclusions regarding overseeding of clouds with the fact that cloud tops in the STA average only -16° C (GN74), 3°C higher than in the NTA. Nor did they address why G75 asserted that the clouds of Israel were impervious to overseeding, regardless of cloud top temperature.

In his Section 7e, R states that several factors prevented the HUJ investigators from acknowledging or learning that the clouds of Israel frequently exhibit high ice particle concentrations and, on occasions, rain formation via the collision-coalescence process. He asserts that "the time available for glaciation", among other things, may have been why these researchers did not detect high ice particle concentrations and warm rain occurrences.

Rosenfeld states that because high ice particle concentrations tend to occur during the middle and later stages in the life cycle of a cloud, this may have prevented the HUJ investigators from observing that clouds in Israel precipitated at much shallower depths, and at significantly warmer cloud top temperatures, than they reported. Apparently, these researchers failed to notice the dramatic effects of high ice particle concentrations, even though they sampled more than 100 clouds with an instrumented aircraft (e.g., GN74; G71; G75), used an aircraft to verify vertically-pointed X-band wavelength radar measurements of the tops of precipitating clouds over Jerusalem (e.g., G80; Rosenfeld 1980), and used a powerful C-band radar beginning in the 1970s to assess cloud top heights (see Section 2.2 of R's Comment). Rangno (1988), on the other hand, detected this phenomenon visually from the ground and through the use of rawinsondes. Also, after just a few flights, it was readily apparent to Levin (1994) that high concentrations of ice particles were common in clouds over Israel. Further, Fig. 2 of this Extended Reply (which is adapted from Rosenfeld and Gagin 1989) reveals most of these phenomena.

In his Section 8, R discusses the radar-inferred cloud top temperatures in Israeli II and cloud seeding effects. He asserts that the consistency between those studies is a good argument for believing that stratifications of seeding effects in Israel by cloud top temperatures determined from radar are valid.

In RH95 we questioned the reliability of cloud top temperature data for Israeli II derived from radar by the HUJ investigators. Gagin and Neumann (1981), for example, stated that they measured the top of every cell in the NTA of Israeli II. In fact, they could not have done so because the radar was too far from the NTA to measure all of the cloud tops in that region (personal communication, 1994, from Karl Rosner, Chief Meteorologist for Israeli II). Also, as noted by GN76, a "modal" radar top temperature[3] of -15° C assigned to an entire day, can be misleading if, for example, all of the rainfall fell from a band of clouds with a top temperature of -24° C. This problem has never been satisfactorily resolved, nor is it now by R.

We also note that the radar used by the HUJ investigators was installed at Ben Gurion Airport in the late 1970s (e.g., G80; Rosenfeld 1980). Based in part on observations made with this radar, G80, G81, G86, and GG87 concluded that rain did not develop either by collision-coalescence or by the ice process in clouds in Israel with tops warmer than about -14° C, a conclusion now known to be incorrect. Perhaps this radar was improperly calibrated.

3. Responses to Rosenfeld's specific comments concerning seeding and seeding logistics in Israeli I

In his Section 3b, R postulates that the percentage of suitable clouds seeded in Israeli I was about 60% (rather than 25-35% estimated by RH95) and that this was enough to produce statistically significant modifications in rainfall. He bases his

[3]Modal cloud top temperatures are the radar-derived cloud top temperatures used by the HUJ investigators to stratify seeding effects in the NTA of Israeli II.

estimate on two claims: 1) that the hours of rainfall per season from clouds in Israel with tops warmer than -5° C (which are unresponsive to seeding) is significant, and the inclusion of these clouds in our estimates of hours of "showery weather" inflated our estimates of the clouds per season that were suitable for seeding; and, 2) that the HUJ investigators knew that rain fell from clouds with temperatures >-5° C and they did not seed these clouds.

Rosenfeld's claims raise several interesting issues. They are apparently based on Gabriel's (1967) statement: "The aircraft takes off whenever cloud conditions appear favorable, but seeding is carried out only after the cloud seeding officer has ascertained that cloud tops reach or exceed the -5° C level." As discussed by RH95, this statement could not have been true for many of the actual cloud seeding events in Israeli I. Consider, for example, the case of nighttime seeding. According to the Chief Meteorologist for Israeli I (personal communication, 1994, from Karl Rosner, Chief Meteorologist for Israeli I), the seeding aircraft was sent up and seeding took place at cloud base whenever rain was reported during nighttime hours. It was *presumed* that the precipitating clouds were appropriate for seeding. Clearly, it was not possible for a cloud seeding officer aboard the aircraft at night to ascertain whether or not some cloud tops rose to the -5° C level. Also, in convective situations, cloud top heights vary greatly, and undoubtedly some "unsuitable" clouds were seeded as the aircraft flew back and forth at cloud base along the seeding track. Further, the data of Rosenfeld and Gagin (1989) for the 1982-83 rainfall season suggests that rain from such clouds comprises a small fraction of convective rain events in Israel (see Fig. 2 of this Extended Reply).

In view of the many requirements for "suitable" clouds for seeding, we believe that our estimate that 25-35% of the seeding material was released in suitable cloud conditions is, if anything, generous, and that seeding, as carried out in Israeli I, was unlikely to have produced a statistically significant effect on rainfall in the target areas. Moreover, we believe that it is virtually impossible that on the few occasions when the Buffer Zone (BZ) may have been inadvertently seeded that this could have caused the high rainfall that occurred in the BZ, which showed the most significant statistical increases in rainfall on days when the CTA was seeded (e.g., Wurtele 1971).

In his Section 3c, R states that we ignored the paper by Gagin and Arroyo (1985) concerning the dispersion of seeding material in Israeli II. He claims that this paper shows that the seeding coverage in Israeli I was considerably greater than we estimated.

In the RH95 discussion of seeding efficiency, to which R refers, we confined ourselves to Israeli I for which sufficient data have been published to make an assessment. Gagin and Arroyo's (1985) paper (hereafter GA85) is concerned with Israeli II. The airborne line-seeding path used for the NTA of Israeli II was considerably shorter in length than the path used in Israeli I (54 km versus 75 km). Further, GA85's estimates of the dispersion of the seeding agent are based on several questionable assumptions. For example, they assume that the aircraft flew under a continuous updraft for the entire 54 km, and that this updrafts transported the seeding agent straight up to the -15° C level where it dispersed downwind across the target in an enormously wide plume relative to Gaussian model plume predictions. It is apparent from R's own radar studies of convection that it is unlikely that the seeding material was carried up in a continuous updraft along an entire seeding track (see Fig. 1 of R's Comment and Fig. 3 of the present Reply).

Even with these optimized assumptions concerning the dispersion of the seeding agent in Israeli II, GA85 found that line seeding in the NTA could have affected only *half* of the volume of air -15° C. Thus, even if the generous assumptions concerning dispersion made by GA85 are applied to Israeli I, the fraction of the air affected by the seeding agent when the aircraft was flying at -15° C is reduced to 25% simply due to the longer seeding track (by 50%) used in Israeli I.

We note also that, in apparent recognition of the poor efficiency of seeding in Israeli I, a second aircraft and a network of ground seeding generators was added in Israeli II (GN81) and, in today's operational cloud seeding program in Israel, two aircraft fly simultaneously in opposite directions.

Rosenfeld has also misunderstood what we did. There are two seeding scenarios in RH95: 1) when the aircraft is line seeding between cumulus cloud updrafts and the seeding material is not transported directly upward (i.e., the sky overhead is clear, or contains a stratiform cloud), and 2) when the aircraft is beneath the base of a cumulus cloud and releases silver iodide into the updraft feeding the cloud. Recall that the aircraft almost always flew in a straight, prescribed path, rather than performing "sorties" in search of cumulus updrafts.

Our first calculation was for the first scenario (no updraft). Therefore, we used a Gaussian plume dispersion model. To exaggerate the vertical dispersion, we assumed a slightly *superadiabatic* lapse rate. Rosenfeld has confused this calculation with our discussion of dispersion when a seeding agent is released in the updraft at the base of a convective cloud. We concluded that since the seeding aircraft flew under a cloud just once, not more than one or two turrets (or even portions of turrets) could have been seeded.

At night the seeding aircraft was directed toward regions of precipitation detected by radar (personal communication, 1995, from Karl Rosner, Chief Meteorologist for Israeli I). Yet, as R acknowledges, if a cloud is already precipitating it is probably too late for seeding to have any affect on rainfall.

In view of these many factors, we standby our conclusion that it is doubtful that enough "suitable" clouds were seeded in Israeli I to significantly affect rainfall.

In his Section 2c, R asks us to consider the case of line seeding from an aircraft flying upwind of orographic clouds in Israel.

Line seeding at cloud base is *potentially* a viable method for intermittently seeding standing orographic clouds, providing the aircraft track is short. However, even this scenario has problems in Israel. First, most of the hilly terrain is in the eastern portion of the country. Therefore, even if the stationary clouds that form over these hills were deep enough for ice formation to occur (tops >3-4 km ASL on most rain days), there would not be sufficient time for ice crystals nucleated at <-10°C to grow and fallout in Israel. A temperature of -10° C is referred to here because, according to G81 and GN81, clouds with tops warmer than -10° C, and certainly those clouds with tops warmer than -5° C, cannot be seeded effectively with silver iodide.

Standing orographic stratocumulus clouds do, in fact, form frequently over the Judean Hills and in the hilly regions in the north of Israel. However, since these clouds are formed by uplift over hilly regions that are <1 km in elevation, they are not very deep, and often topped by strong stable layers (Rangno 1988). Such clouds rarely reach -10° C.

Also, much of the rain that falls in the hilly regions in northern and eastern Israel derives from cumulonimbus complexes, which originate upwind over the Mediterranean Sea. When these clouds pass over Israel they are generally in their mature and decaying stages, and contain high concentrations of ice particles at relatively small supercoolings. These clouds may also overrun shallower orographic clouds, where accretion and riming can increase rainfall. While temporarily over the hilly regions, such dissipating clouds could be mistaken for simple orographic clouds.

In his Section 2d, R states that our estimate of the amount of rain that would have had to have been produced by seeding in Israeli I is too large by a factor of two because we did not consider control days. He claims that for Israeli I we calculated that about 1,000,000 m^{-3} of water would have had to have been produced by each gram of silver iodide.

Nowhere does the number 1,000,000 m^{-3} appear in RH95. We estimated that 500,000 m^{-3} of water would have had to be produced by seeding in Israeli I to explain the amount of rainfall attributed to seeding by GN74 in Israel and by Brier et al. (1973) in Syria and Jordan. Our number was derived by assuming that 1% of the seeding material reached "suitable" clouds, namely, those clouds with top temperatures from -12° to -21°C but not in their mature or dissipating stages. However, if one were to assume that RF92's report of decreases in rainfall of 10-30% due to seeding on dust/haze days actually occurred, even our estimate of 500,000 m^{-3} per gram of silver iodide would be conservative because the silver iodide that entered "suitable" clouds on days without dust/haze would have had to make up for the decrease in rain caused by seeding on dust/haze days!

In his Section 4b, R questions our characterization of the seeding efforts in Israeli I as "wasted".

The word "wasted" does not appear in RH95. It has to be recognized however that in any seeding operation forecasts can go awry: conditions that might be suitable for seeding are not forecast correctly, and forecasts of suitable conditions are not fulfilled; also, aircraft downtimes occur. These realities of life reduce the efficiency of any seeding project. Rosenfeld implies that the seeding operations in the Israeli experiments were perfectly executed, with the seeding material rarely released in unsuitable conditions.

In his Section 4e, R recounts some observations concerning cloud base heights over the Mediterranean Sea, and a shallow, offshore-flowing wind regime from the interior of Israel that can develop over the coastal plain. He posits that a zone of enhanced convection, which can occur offshore of Israel in some of these situations, could have been seeded. He believes this explains how the BZ was inadvertently seeded on a routine basis in Israeli I and that it accounts for the statistically significant greater rainfall in the BZ on CTA seeded days in Israeli I.

Because of the troublesome bias in rainfall on CTA seeded days in the BZ and along the coast of Israel in Israeli I, R has proposed a complicated scenario to explain why the BZ may have been seeded. By contrast, we attribute the bias in rainfall in the BZ on CTA seeded days to an uneven draw in natural rainfall that led to the misperception of a seeding effect.

Complicated scenarios, such as the one proposed by R, have a number of steps that have to be fulfilled to be valid. For example, each of the following nine steps would have to be realized before the scenario postulated by R could produce rain due to seeding in the BZ and other coastal locations in Israel:

1) Silver iodide is released at cloud base (about 0.8 km ASL) in westerly, onshore flow.
2) Turbulence at the release level causes the silver iodide plume to deepen rapidly downward (as well as upward).
3) The silver iodide plume reaches down to levels at or very near the surface before reaching the Judean Hills, about 40 km inland (otherwise it would be carried over those hills and not return to the west).
4) An ESE wind would have to exist at and just above the surface on the coastline, and offshore, for an appreciable distance; also, for an appreciable distance inland.

   (In the afternoon, and in strong pressure gradient situations in postfrontal periods when rain often occurs in Israel, there is rarely a wind with an easterly component at the coast (Neumann 1951). Also, of the 4 h per day that seeding was carried out in Israeli I, fewer hours occurred at night (Gabriel and Neumann 1978; RH95) when an ESE wind is more likely to be present. Wind rose data for the IMS rawinsonde site at Bet Dagan, which is located about 7 km from the coastline, is shown in Fig. 1a. It can be seen that when rain falls at this site, or within about 60 km as determined from IMS observing stations, there is a wind from the southeast quadrant less than 15% of the time!)
5) Rosenfeld assumes that most of the seeding agent affects clouds downwind. However, he also suggests that, to the east of the seeding line, a portion of the silver iodide plume filters downward into the shallow easterly flow at the surface, and then "corkscrews" to the west and northwest while rising back to cloud base. Silver iodide must not only be carried back to the coast by this shallow flow, it must be carried far

enough offshore to the west to enhance rainfall in the BZ.

(How far upwind of the BZ must the silver iodide be carried? Assuming a 5 m s^{-1} updraft, 15 min for precipitation formation after the silver iodide enters cloud base, and 15 min for the precipitation to reach the ground, silver iodide entering a cloud base at 0.8 km ASL would require about 40 min to rise, form precipitation, and fall to the ground. For a westerly wind of 15 m s^{-1} between cloud base and the 600 hPa level (where temperatures typically first go below about -10° C on showery days in Israel), the silver iodide would have to enter clouds that are moving toward the BZ at a distance of no less than 25-30 km west of the Israeli coastline. However, east winds at the surface, which might carry the silver iodide westward, are rarely greater than 5 m s^{-1}. At 5 m s^{-1}, it would take the silver iodide in this layer about 1 ½ hours to reach a location far enough upwind to have the possibility of creating ice particles aloft that could fallout as rain in the BZ.)

6) During its 1 ½ hour drift to the west, the shallow easterly flow must not be destroyed by convection currents arising from the warmer Mediterranean Sea.

   (Rosenfeld does not address why the "corkscrew" seeding phenomenon he postulates for Israeli I did not affect Israeli II, where ground generators were more likely to inject seeding material into the easterly surface flow.)

7) If the silver iodide arrives at a appropriate point upwind of the BZ, clouds suitable for seeding must be present; these clouds must be large enough to precipitate, but not so large that sufficient concentrations of natural ice particles are present for rainfall production. Cloud top temperatures upwind of the BZ must, according to the HUJ investigators, be between -12° and -21° C, a span of just 1 km in cloud top height!

8) The seeding plume that is carried into the upwind clouds must not only be ingested by clouds with appropriate cloud top temperatures but also, according to R, the clouds must be at an appropriate stage in their life cycle (i.e., the young, building stage), and be seeded with the optimum concentrations of silver iodide particles, to enhance rainfall.

9) Finally, according to R's dust/haze hypothesis, this inadvertent seeding must not have occurred on the majority of days in the CTA in which rain was decreased because of the presence of a super ice-nucleating dust/haze aerosol.

If we assume (generously) that there is an 80% chance of independently fulfilling each of the above requirements, the probability that all of the requirements would simultaneously be met on any one occasion (which R's scenario requires) is about 13%. Hence, we do not believe R's scenario is viable.

4. Response to Rosenfeld's specific comments concerning the statistical evaluations of the Israeli experiments

In his Abstract, R asserts that "the results (of the Israeli seeding experiments) show significant positive effects in northern Israel."

This statement is misleading. First, to recap the published results of the Israeli experiments to which R alludes: in Israeli I, increases in rainfall due to seeding were suggested in both targets, but with much greater increases in the CTA (e.g., Gabriel 1967; Wurtele 1971; GN74). In Israeli II, one can accept an unambiguous statistically significant increase in rainfall due to seeding in the NTA only if one ignores the well-established fact that naturally heavier rain fell regionally in Israel on NTA seeded days (as first reported by GR90), and that these heavier rains extended into Lebanon and Jordan (RH95). If there is a glimmer of hope that there was a seeding effect in Israeli II, it rests on the attempt by RF92 to use a numerical model, based on rawinsonde profiles, to account for the naturally heavier rainfall on NTA seeded days.

On the other hand, if the rainfall in the non-seeded target is used as the control rainfall in Israeli I (as GN74 recommended for Israeli II), the results for Israeli I (from Gabriel and Baras 1970 and Wurtele 1971) are strikingly different from previously published results, and they do support R's contention concerning Israeli I because the double ratio for the NTA is 1.28 (and clearly significant), while the result for the CTA is 1.03 and insignificant. However, if one accepts the large double ratio in the NTA as due to seeding, one also has to accept that seeding effects were produced at coastline locations that were situated virtually under the path of the line seeding, and were produced by very few hours of seeding. We believe that these statistical results are further manifestations of a Type I error (i.e., lucky draw) for the NTA of Israeli I.

In his Abstract, R states that in his view Israeli I confirmed Israeli II.

Rosenfeld does not state which result was confirmed: positive, negative, null, or all three, all of which have been suggested by the HUJ investigators! For example, R states that he now believes that seeding *decreased* rainfall in the center target area (CTA) of Israeli I due to dust/haze that affected the clouds on most CTA-seeded days. This is a new claim, which we find particularly interesting in view of the earlier analyses of Israeli I (e.g., Wurtele 1971; GN74) which suggested that the main effect of seeding was to *increase* rainfall in the CTA more than in the NTA.

In his Abstract, R states that a target/control analyses of the operational seeding program in northern Israel (which followed Israeli II and is still continuing) shows significant (~6%) increases in rainfall due to seeding.

While the results of a non-randomized cloud seeding experiment can be encouraging, they are rarely scientifically convincing. Little is known about the operational cloud seeding program in Israel. We do not know, for example, how much seeding material is used or for how long. Are one, two or three seeding aircraft used? Do they fly simultaneously? How many ground-based silver iodide generators are there? On what days has seeding occurred? What radar is used to infer cloud properties? What are the effects of storm types on the analysis? What are the criteria for a seeding operation? Why are the apparent effects of seeding on rainfall so small compared to those reported by GN81? However, perhaps what is most needed is a comprehensive and independent evaluation of the results of the operational cloud seeding program in Israel, along with comprehensive cloud measurements.

Other evaluations of the effects of the current operational cloud seeding program in Israel have produced differing results. Benjamini and Harpaz (1986) found no statistically significant evidence for increased runoff in the target area around Lake Kinneret. However, Ben-Zvi (1988) did. The findings of Ben-Zvi (1988), and those of Nirel and Rosenfeld (1995) who reported a 6% increase in rainfall due to seeding, are dependent, to varying degrees, on the use of control data from the northern coastal strip and from the plains regions of Israel, rather than on rainfall (or runoff) from nearby areas in similar terrain, such as the central hill region adjacent to the operational target area, or from southern Lebanon, both of which experienced heavier than normal rainfall on NTA seeded days and whose rainfall is highly correlated with the operational target.

Rosenfeld states in his Abstract that there is mounting evidence that desert dust is responsible for the apparent differences in the effects of seeding on rainfall in north and south Israel.

Rosenfeld describes the results of cloud seeding in Israel as though they are uniformly the same (*viz*, increases in rainfall in the NTA and decreases in the STA due to dust/haze). Rosenfeld believes that the statistical results of Israeli I were misanalyzed by Gabriel and Baras (1970), Wurtele (1971) and GN74 (i.e., heavier rain in the CTA and BZ were incorrectly attributed to seeding), and that his own interpretation is the correct one. Rosenfeld first raised this interesting hypothesis in 1989; we recommend that he submit his reanalysis of Israeli I for formal publication.

In his Introduction, R states that two "lucky draws" (for Israeli I and II) could not have occurred.

First, lucky or unlucky draws seem to turn up rather often in cloud seeding experiments (e.g., Brier and Enger 1952; Lovasich et al. 1971; Gelhaus et al. 1974; Hobbs and Rangno 1978; Rangno 1979; Mielke 1979; Grant et al. 1979; Nickerson 1979), particularly when target and control station are not specified in advance.

Second, had the original statistical designs for Israeli I and II been strictly followed, with all of the target and control stations listed in advance and adhered to, two "lucky" draws in a row would have, indeed, been unlikely, although not impossible. However, it is arguable whether two "lucky" draws in a row actually occurred in the Israeli experiments. From the standpoint of the CTA and STA of these experiments, R could just as well argue that there was a "lucky" draw in Israeli I (apparent increases in rainfall due to seeding) followed by an "unlucky" draw in Israeli II (apparent decreases in rainfall due to seeding). Such ambiguity in possible interpretations of these experiments is evidence of problems in the statistical draw.

Rosenfeld is not disturbed by the fact that in Israeli I the statistical analyses indicate that the greatest effect of seeding occurred in the buffer zone (BZ), even though the BZ was probably seeded for only a few hours each rainy season (e.g., Wurtele 1971; RH95). Further, the greatest statistical significance for Israeli I found by GN74 (to which R refers) was enhanced by the inclusion of the BZ, which was not only designed not to be seeded but has been stated on many occasions by the HUJ investigators not to have been seeded (Gabriel 1967; GN73; GN81; GG87). Yet, the addition of rainfall data from the BZ *improved* the statistical results that suggested seeding increased rainfall in Israeli I. We believe that this result, when combined with the other evidence presented by RH95, shows that Israeli I was compromised by natural rainfall patterns that led to the misperception of seeding effects. This supports R's (1989) own statement: "It is not likely that the buffer area (of Israeli I), only due to inadvertent contamination, will be positively affected twice as strong (1.32 and significant) as the actual center target area (1.16 and insignificant)."

A statistically significant result indicating increases in precipitation due to artificial seeding was achieved in Israeli II only when a large portion of the data was omitted (that for the STA) in the many analyses prior to GR90, and the published analyses were confined to the NTA using a target/control rather than a crossover evaluation (e.g., GN76; GN81; G81; G86; GG87). The seeding effect in the NTA disappears when the STA is used as a control on NTA seeded days, which was an original requirement of the statistical design in order to avoid meteorological biases (e.g., GN74).

There is no question that the target/control analyses for Israeli II shows that rainfall on the NTA seeded days (in which a seeding effect has been claimed by the HUJ investigators) was unusually heavy over a wide region (e.g., GR90; RH95). How could it have been concluded that seeding increased rainfall in Israeli II in the face of such clear evidence that the experiment was compromised by a lucky draw? As a starting point, exactly the same control and target stations should have been used in Israeli II as were used in Israeli I. Our analysis shows that the supposed effect of seeding on rainfall in the NTA (reported by GN81) may have been dependent, in part at least, on the *post-factum* choice of control stations in a small coastal strip that had anomalously low seed/no seed ratios (see Fig. 1b of this Reply and Fig. 17 of RH95).

In his Introduction, R emphasizes his belief that the statistical results of the Israeli experiments provide sufficient plausibility that seeding increased rainfall since they were "black box" experiments.

We have already pointed out that the various statistical analyses of the Israeli experiments have not yielded consistent results, and that they are subject to various interpretations. For example, in our view, the statistical results for the BZ of Israeli I alone make that experiment ambiguous. If one believes that the BZ, which exhibited the strongest statistical result in Israeli I, was largely unseeded when the CTA was seeded, then a lucky draw (false positive) did occur for the CTA. On the other hand, if one believes that the BZ was routinely inadvertently seeded, then seeding may have been responsible for the heavier rain in the CTA on seeded days. However, in doing so, one would have to: 1) accept that a very poor job was done of targeting seeding effects in Israeli I; 2) reject the assessment of the Chief Meteorologist for Israeli I that seeding could have affected the BZ only "5-10% of the time" (Wurtele 1971; RH95), together with a similar conclusion by RH95 based on a more detailed analysis of ten seasons of data; 4) reject the statements of the HUJ investigators that the BZ was not seeded (e.g., Gabriel 1967; GN81); 5) ignore the analysis of GN74, who found that the BZ had even more rain on CTA seeded days when seeding was *not* carried out; and, 6) explain why there was not a similar effect when the STA was seeded in Israeli II.

In his Section, 4c, R contends that the possibility of a lucky draw in Israeli I is as remote as the tests of statistical significance suggest. He further asserts that since we have accepted the null results of the crossover analysis of Israeli II, we should accept the crossover analysis of Israeli I, which suggested a 15% increase in rainfall on seeded days overall.

Were it not for the strong statistically significant increases in rainfall in the BZ (which was supposed not to have been seeded), a natural bias indicating heavier natural rainfall on seeded days in Israeli coastal zones too close to the line of seeding to have been affected by seeding, a similar natural rainfall bias that favored rainfall on seeded days in extreme southwest Jordan (Brier et al. 1973), and the lack of a sound physical basis for expecting that seeding might increase rainfall in Israel, there would indeed be no reason to doubt the crossover results of the Israeli I experiment.

Are we inconsistent in accepting the null results of Israeli II crossover evaluation while deducing that the crossover results of Israeli I were impacted by a storm bias? The problem, as we see it, is that the results of these two experiments are inconsistent. In Israeli I, seeding appeared to have increased rainfall the most in the southernmost target; in Israeli II, nearly statistically significant *decreases* in rainfall due to seeding appeared to have occurred in the southernmost target (GR90; RF92). Thus, even if one accepts *prima facie* the outcome of Israeli I (and ignores the warning signs presented by the BZ), one would conclude that the *crossover* results of Israeli I were not confirmed by the *crossover* results of Israeli II.

The types of storms that affected these two experiments were also different. In Israeli I, the rainfall patterns on seeded days were more localized and contained within small regions (Brier et al. 1973; Rosenfeld 1989). These patterns favored the perception of a strong positive seeding effect in the CTA of Israeli I, although heavy storms in the CTA had little effect in the NTA (Rosenfeld 1989).

Israeli II, on the other hand, was dominated by storm conditions on seeded days that affected larger regions. For example, when heavy rain fell on NTA seeded days, the rain was also often heavy on those *same days* in the STA (GR90). We extended this finding to as far north as Beirut, Lebanon, and southern Jordan (RH95). Thus, any increase in rainfall that might have been produced by seeding of the STA was masked by the heavy rain that fell on the control days for the STA (which were the seeded days for the NTA).

On the other hand, in Israeli II, R (mistakenly in our opinion) interprets vagaries in the random draw as increases in rainfall (in the NTA) and decreases (in the STA) due to seeding.

In his Section 4c, R claims that Wurtele's findings concerning the BZ are "irrelevant".

We disagree. Wurtele's enigmatic findings are as important today as they were when first published. The results she found for the BZ can now be seen as a "red flag," which should have raised further questions about the HUJ investigators' statistical analyses of Israeli I.

In his Section 4d, R posits a scenario that appears to resolve the vastly disparate results of Israeli I and II. He believes that in both experiments seeding decreased rainfall in the CTA and STA (contrary to several previously published analyses). He also claims that the increase in rainfall in *the NTA due to seeding in Israeli I was closer to 40% (instead of the 8% increase claimed by GN74) and that there was a 7%* decrease *in rainfall in the CTA (instead of the 22% increase claimed by GN74).*

Rosenfeld's speculations are interesting. He believes that the previously published results for the CTA of Israeli I are incorrect. Whereas, Gabriel (1967), Wurtele (1971), Brier et al. (1973), and GN74 all concluded that most of the apparent increases in rainfall due to seeding occurred in the CTA, R thinks that seeding produced an overall decrease in rainfall in the CTA.

However, R's speculations are based on a major flaw. As pointed out above, and by Rosenfeld (1989), the storms that affected Israeli I and II were quite different both in their type and regional extent. For example, in Israeli I the storms on CTA seeded days did not affect most of Israel, Jordan and Lebanon, as they did on NTA seeded days in Israeli II (Brier et al. 1973; Rosenfeld 1989).

It is worth repeating R's own conclusions in this regard (Rosenfeld 1989): "The occurrence of heavy rain days was much greater in Israeli 1 as compared to Israeli 2. Israeli 1 had 19 days with more than 40 mm in one or both of the target areas, as opposed to only 3 such days in Israeli 2. The nature of the daily rainfall is remarkably different between the two experiments." In his Table 10, Rosenfeld (1989) notes that for Israeli I the elimination of the heavy rain days causes little change in the apparent increases in rainfall due to seeding in the NTA, but it causes the apparent increase in rainfall due to seeding in the CTA to diminish by 23%. This substantiates our point that the heavier rains in Israeli I were not evenly distributed between the two target areas.

Hence, inferences about the rainfall in the NTA of Israeli I based on the rainfall in the CTA are misleading. For example, using a cluster of stations in extreme southern Lebanon, Brier et al. (1973) found no evidence that the rain was heavier in southern Lebanon on CTA seeded days in Israeli I. Hence, the stronger storms on CTA seeded days did not extend across the NTA to southern Lebanon, as would be required by R's hypothesis of translating results from the CTA to the entire NTA.

Were the upper-level troughs, or "cutoff lows", farther south on CTA seeded days in Israeli I, so that a (natural) rainfall maximum occurred in the south and decreased northward? Were NTA seeded (and thus CTA control) days affected by storms in Israel I that produced heavy rain in southern Lebanon? Were "coastal fronts", of the type described by Khain et al. (1993) and Rosenfeld and Nirel (1996), more numerous on CTA seeded days in Israel I? We do not know the answers to these and many other questions. However, several natural meteorological scenarios such as those mentioned above, which could have concentrated rainfall in the CTA, could also explain the increases in rainfall that were attributed to seeding in Israeli I. In this context it is illuminating to quote Wurtele (1971): "...according to M. G. Wurtele and A. Gagin, the meteorological characteristics of the days that are associated with moderate or strong (seeding) effects are descriptive of situations for which greater than average *natural* rainfall may be expected."

In his Section 4f, R asserts that we are premature to dismiss possible seeding effects when the airflow is from the northwest at 850 hPa.

We agree with R that one of the most promising statistical results in Israeli I was that indicating increases in rainfall due to seeding in northwest flow at 850 hPa (GN74; RH95). However, we questioned the practicality of increasing rainfall significantly by randomly seeding this flow regime, since in Israeli I the total number of experimental days with northwest flow was only about eighty in six and a half seasons (GN74), and many of those days had sporadic, light rainfall. Thus, the small sample size that would accrue for this regime in a random statistical experiment (about 6 seeded and control days each season), would make it difficult to arrive at a firm evaluation unless the experiment was carried out over many years.

In RH95 we pointed out that rain falls from shallow clouds with tops $\geq$-10° C in northwest flow. This conflicts with R's dust/haze hypothesis, since it shows that the presence of dust/haze from deserts to the southwest is not the major factor required for ice multiplication and/or collision-coalescence of rain in Israel, and it raises questions about how much potential there is for seeding effects even in these clouds.

In his Section 5, R objects to our characterization of Israeli II as being "widely viewed" as a confirmatory experiment. He describes the target/control evaluations for Israeli II as exploratory analyses.

We are puzzled by R's refutation of Israeli II being a confirmatory experiment. It was described as such by Tukey et al. (1978), Simpson (1979), Kerr (1982), Silverman (1986), Cotton (1986), Dennis (1989), and Cotton and Pielke (1992, 1995), without any objections from the HUJ investigators!

In his Section 5, R states Israeli II was designed first as a single target/control randomized experiment, and that the crossover design was subsidiary.

We appreciate R providing a summary from a 1969 meeting of the Israeli Rain Committee (consisting of J. Neumann, A. Gagin, A. Kali, M. Bitaron, and K. R. Gabriel). However, this document (originally in Hebrew) does not substantiate R's claim that Israeli II was *primarily* a target/control experiment. While the target/control design of the experiment was discussed by the Committee, and was clearly a *component* of Israeli II (which we never doubted), this document shows that the Committee voted to approve the full crossover design of Israeli II by a vote of three to two.

Second, based on information provided by the HUJ investigators, the National Academy of Science's Panel on Weather and Climate Modification (1973) described Israeli II as a "two-target, crossover, randomized; one area (N) has an adjacent control area."

Finally, in the physical descriptions of the Israeli cloud seeding experiments given by GN74, the description of the Israeli II experiment begins as follows: "As in the first experiment, there are two experiment areas, North and South. These areas are larger than those of the first experiment. Again, we can use the randomized crossover scheme." Concerning the early results of Israeli II, GN74 begin their discussion of the crossover design by stating: "At the time of writing this chapter (April, 1972), we are close to the end of the third season of seeding in the framework of the second experiment...the RDR (root double ratio) North versus South is about 1.10; the single area ratio for the North alone is over 1.2 while for the South it is less than 1. However, none of those results is significant as yet."

We refer R to the discussion of GN74 on the purpose and value of the crossover design, which we described in similar terms in RH95. While praising the attributes of the crossover design that they were later to drop, GN74 wrote: "The great merit of the crossover design and, especially that of the test statistic is that it eliminates to some extent, the troublesome and misleading effects of the *natural* (emphasis by GN74) fluctuations in rainfall."

No one can read GN74, and the emphasis that they place on the RDR results, and their lauding of the crossover design over target/control designs, and not clearly and unambiguously understand that the primary concern of GN74 in April 1972 (when GN74 was being written) was the result of the Israeli II crossover experiment. The target/control aspect of Israeli II was just as clearly subsidiary (not the other way around as R now claims). We urge the reader to study GN74, and the 1973 National Academy of Science's report, to confirm the emphasis that was placed on the crossover design.

Rosenfeld is correct on one point: the crossover results of Israeli II were downplayed when the results did not confirm Israeli I. In fact, by 1981, the results of the Israeli II crossover experiment not only became subsidiary to the target/control results, they were not mentioned at all (GN81)!

In his Section 5f, R states that the "experiment days" in Israeli II were predetermined as those days with measurable rain at three stations in the BZ.

A rainfall criterion for the *crossover* design of Israeli II is not mentioned in the Israeli Rain Committee meeting of 1969. However, such a criterion for determining, retrospectively, experimental days is not unreasonable for the Israeli II crossover experiment, because the occurrence of rain in the BZ is a reasonable test for the presence of suitable clouds in the target areas on either side of the BZ. However, for the target/control evaluations for northern Israel and its hilly regions, the BZ rainfall criterion is not a particularly good choice. For example, we found many days (about 50) when rain occurred in extreme northern Israel in or around the NTA, but these days were not included in the GN81 analyses because there was not rain in the BZ. Hence, the criterion of rain in the BZ to ensure the presence of suitable clouds in the NTA failed on numerous occasions in Israeli II.

In Section 6b of his Comments, R contests our conclusions that the analyses of Israeli II are, thus far, incomplete because numerous days with rain in the NTA have not been analyzed and only a fraction of the available IMS raingauge data has been used.

With regard to the completeness of the reporting of Israeli II, we quote Gabriel (1967): "It would be advantageous to restrict the evaluation of the experiment to those days on which seeding is feasible and to exclude the large number of days without rain clouds which cannot add to the sensitivity of the experiment. However, this is not permissible."

Also we quote GN74: "Yet statistical significance testing of the results of seeding is only meaningful if the data of *all* (emphasis by GN74) days randomly allocated to seeding, whether actually seeded or not, are considered together."

To date, there is no published analysis of Israeli II that meets the requirements specified in the above quotations from Gabriel (1967) and GN74. For example, in Israeli II a random selection was made to seed one of the targets every day between November 1 and April 30 for the years 1969-70 through 1974-75 (for a total of 1087 days). So far, with the exception of results mentioned by R in his present Comments, formal results have been presented for only 388 (or fewer) days (e.g., GN81; GR90). Hence, we repeat: the results of Israeli II have not been fully reported.

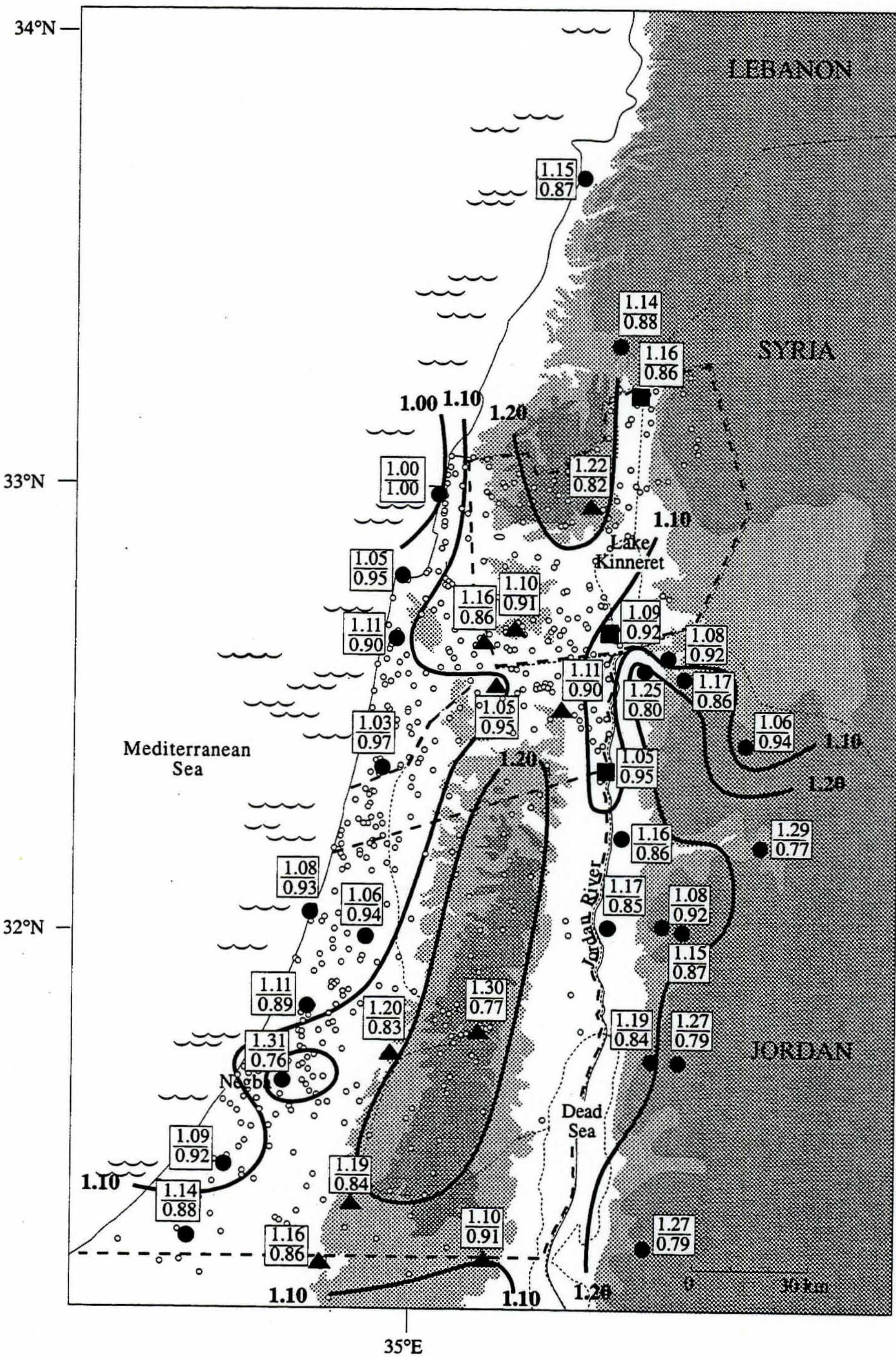
We appreciate the additional results regarding wider analyses of Israeli II now reported by R. While these results alleviate some of our concerns, we recommend that they be incorporated into a comprehensive description of Israeli II and that this be submitted for formal publication.

In his Section 6b, R states that the selection of the control gauges in Israeli II was based solely on "historical continuity and reliability".

First, R's statement is inaccurate. In RH95, we used the rainfall data published by the IMS in their *Annual Climatological Data* for Israel. To our astonishment, many of these main IMS rain gauges were not used by GR90 in their analyses, despite having continuous records throughout the Israeli II experiment, including, for example, the gauge at Jerusalem! Some of the gauges not used by GR90 are noted in Fig. 4. Hence, many questions remain about the choices by both GN81 and GR90 of target and control raingauges in Israeli II (see below).

On days when the NTA was seeded we expected to find an isolated anomaly in the seed/no seed ratios in the NTA, beginning some distance downwind of the line of seeding. Elsewhere we expected to find near unity values for the seed/no seed ratios. To our surprise, we found high seed/no seed ratios on NTA seeded days from central Lebanon to southern Jordan (RH95). In fact, the only anomaly in rainfall (from a regional viewpoint) on the NTA seeded days of Israeli II were relatively *low* (near unity), seed/no seed ratios in a narrow coastal strip north of Haifa (see Fig. 17 in RH95).

We then observed that the number of control stations that GN81 selected (sometime during or *after* the conclusion of Israeli II) from this narrow coastal strip was disproportionate relative to the density of control stations that they selected in other areas where the seed/no seed ratios were not so low. GN81 used nine control stations from the narrow coastal strip with low seed/no seed



Location and names of stations	(a)	(b)
Southern Lebanon *Beirut *Marjayoun	0.85	1.02
Israeli Northern Coast *Nahariyya Haifa/Qiraty Atta Nir Ezyon	0.78	1.11
Israeli Northern Hills Mishmar Ha-Emeq Nazareth Afula	0.87	1.05
North Target *Mt. Kena'an *Dafna *Degania Alef	1.00	1.16 (SAR)
Buffer Zone En Ha Horesh *Hefzi-Bah	0.77	1.09
Israeli Central and South Coast Tel Aviv Yavne/Kefar Mordecai Negba *Sa'ad *Lod	0.64	1.03
Israeli Southern Hills and Jordan Valley *Tirat Zevi *Jerusalem Beit Jimal Lahav	0.65	0.97

Figure 4. Figure 17 from Rangno and Hobbs (1995) with the names of the stations used by them for the calculation of seed/no-seed ratios and double ratios. See the caption to Fig. 17 of Rangno and Hobbs (1995) for further information. Stations listed on the right and preceded by a star are IMS meteorological stations not used by GR90.

ratios (shaded area in Fig. 1b of the present Reply), but only seventeen control stations from the rest of the control areas that together were about ten times larger in area than the narrow coastal strip. Also, the gauges used by GN81 were not the same as those used for Israeli I by Gabriel (1967) who employed a nearly uniform grid of gauges across Israel (compare Fig. 1a with Fig. 1b).

Rosenfeld states the control gauges were chosen by GN81 on the basis of "historical continuity and reliability". If this is true, then the operators of rain gauges in the extreme northern coastal strip of Israel exhibited a remarkable propensity for maintaining "historical continuity and reliability" compared to their colleagues who operated rain gauges in the remainder of the control area in Fig. 4–since GN81 used almost every available gauge from the extreme coastal strip as a control in their analysis of Israeli II, but not so in the other regions (compare Figs. 4 and Fig. 1b of the present Reply)! Also, if this is true, why did GR90 not use data from the same raingauges as GN81 (compare Fig. 1b with Fig. 1c).

We suggest that it would have been more appropriate for the IMS to have determined which gauges should have been used in the analysis of the Israeli experiments, both in the target and the control areas. Further, in view of the importance of these experiments, we urge that the IMS daily rainfall data, rawinsondes, surface observations, and the random decisions for each day of Israeli I and II, be made freely available to any interested party.

In Section 6c of his Comments, R asserts that the orographic rainfall bias in Israeli II, which is apparent on NTA seeded days (Gabriel and Rosenfeld 1990; RH95), has been sufficiently accounted for by using rainfall predictions from a numerical model based on rawinsondes launched twice-a-day by the IMS during Israeli II (RF92). He states that a) adding data from the Beirut, Lebanon, soundings is not necessary; b) sensitivity tests in which the number of rawinsonde profiles was doubled resulted in negligible improvements in the model predictions of rainfall; and, c) RH95 exaggerated the difficulty of predicting convective rainfall amounts.

Rosenfeld's statement that higher spatial and temporal data (i.e., using four rawinsondes per day instead of two) does *not* improve his model's predictions of rainfall, is surprising. We believe it reflects the crudity of R's model. Also, R does not indicate how he accounted for the large errors in relative humidity during daylight hours in the raw-insonde humidity elements that were used in the Israeli II era (Hill 1980). Airborne studies, in which humidity profiles measured with rawinsondes were compared with actual cloud fields, often showed little correlation between these profiles and the nearby presence of clouds (United States Air Force 1969).

We note also that in rejecting the suggestion in RH95 to use rawinsonde data from Beirut, Lebanon, in his numerical model, R also rejects the design for Israeli II recommended by the Israeli Rain Committee in 1969. That committee recommended that: "Temperature and upper level data (upper level wind, humidity, etc.). Interpolation between Beit Dagan and Beirut, twice a day" be used in evaluating Israeli II.

Although R's model results reflect some of the natural bias in rainfall on seeded days that favored the NTA of Israeli II, it is difficult to believe that with the use of twice-a-day rawinsonde data alone (and without using rawinsonde data from Beirut, which is closer to the target than Beit Dagan) that his model can account for differences in natural rainfall between seeded and control days, which were *less than 10 percent* in Israeli II, and could not be improved with more frequent sounding profile data.

Finally, only RH95 have used rainfall data from southern Lebanon as a control for the NTA, as was suggested by the Israeli Rain Committee in 1969.

Therefore, just as it is prudent to declare target and control stations prior to a cloud seeding experiment, it is also good practice to declare prior to a statistical experiment which meteorological variables will be used for evaluation (e.g., Flueck 1986).

In his Section 6d, R gives several reasons why he believes that the conclusions we *reached regarding the occurrence of a Type I statistical error ("lucky draw") in Israeli II on NTA seeded days are invalid. He contends that since our analysis was conducted post facto it is invalid compared to what he asserts is the a priori design evaluation of GN81. He points out the need for a high correlation between target areas in crossover experiments. Finally, he states that Fig. 17 of RH95 (Fig. 4 in the present Reply) includes seeded days in some regions as part of the regional analysis.*

Some statements by R must be corrected before we can respond to these comments. There is no evidence that the proper elements for an *a priori* design and evaluation were in place prior to Israeli II. For example, the control *stations* were not specified in advance, and an analysis using the same gauges as those used in Israeli I has still not been presented. Also, analysis of Israeli II is incomplete. With 436 raingauges available for analysis of Israeli II, virtually any result could be produced (e.g., Thom 1957).

Rosenfeld's discussion of our wider areal analysis for Israeli II, which included Lebanon and Jordan (RH95), highlights a serious deficiency in the design and previous evaluations of both Israeli I and II. The designs either did not include analyses of the wider regional distribution of precipitation/ runoff on seeded and control days, which would have alerted the HUJ investigators to potential problems, or, when the design did specify the use of regional data, such as from Lebanon (see meeting of the Israeli Rain Committee in 1969), these data were not used.

Rosenfeld believes that *post facto* regional analyses should be disregarded if they are not specified in the original design of a cloud seeding experiment. In fact, *post facto* analyses have a long and valuable tradition in cloud seeding experiments. For example, numerous *post facto* analyses were carried out for Project Whitetop that shed important light on that experiment (e.g., Braham 1979). Are these analyses invalid?

Rosenfeld is correct in stating that we used relatively few rainfall stations in our reanalysis of Israeli II. We wished we could have used more, particularly for Israel[4] and Lebanon[5]. However, we used *all* of the data we could obtain, all of the Israeli stations we used are contained in the IMS publication, *Annual Climatological Summary for Israel*, and we expanded the analyses of GR90 and RF92 by adding as many stations as we could from Lebanon and Jordan. Apart from some stations in the Israeli deserts that had zero median rainfall on experimental days, no station for which we had data was omitted in our analysis.

We agree with R that it is best to choose target and control regions that are highly correlated. However, when the bias in storms is as strong as it was on NTA seeded days in Israeli II, a consistent pattern of seed/no seed ratios among both highly correlated and moderately correlated stations with the NTA rainfall will be evident, as indeed it was.

By showing the regional values of seed/no seed ratios for the seeded days of the NTA and STA of Israeli II (see Fig. 17 in RH95 and Fig. 4 in present Reply), we demonstrated that Israeli II was dwarfed by natural events that produced a regionally consistent pattern of large seed/no seed ratios on NTA seeded days, and that this pattern was inadvertently interpreted as "increases" and "decreases" in rainfall due to cloud seeding by RF92. For example, low seed/no seed ratios (<<1) are present throughout the region of our analysis on STA seeded days, which one could erroneously interpret as seeding-induced decreases in rainfall over this entire region, not just in the STA and downwind in Jordan as does R.

Rosenfeld has also overlooked the finding of Gabriel and Rosenfeld (1990) who found that it was the *heavy rainfall* (25-45% higher than the climatological normal daily rainfall!) on STA *control* days (the NTA seeded days),

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[4]Geophysical data are now seen as a valuable resource by some countries and some charge large sums for such data. In 1987, the cost of obtaining the full data set for the Israeli experiments was estimated by the IMS to be $50,000 (personal communication, 1987, from A. Vardi, Deputy Director, IMS)!

[5]With the exception of data for Beirut and Marjayoun, the rainfall data for the dense Lebanon network had numerous missing months during Israeli I and II.

and not exceptionally light rain on seeded days in the STA, that produced the appearance of decreased rainfall due to seeding in the STA. It is worth quoting GR90: "Otherwise, *one would need to explain why there was so much more rain in the south when the north was being seeded* (our italics for emphasis); as of now, no explanation is available, especially as the prevailing wind direction is from the southwest." In view of this statement (which R co-authored), we find it difficult to comprehend R's sole reliance on his hypothesis that rain in the STA of Israeli II was decreased by seeding. How does R explain why rain was so much heavier in the STA on NTA seeded days?

To provide just an even draw, the *natural* rain on the STA seeded days would have had to have been 25-45% heavier than normal! Since the seeded and control days comprised all of the rain days in Israel, R is implying that during Israeli II these were six straight rain seasons with total rainfall 25-45% above normal! However, this did not occur either in the seeded or in the unseeded target areas of Israeli II (*Atlas of Israel*, 1985). This is why we disagree with RF92 and R's conclusion that seeding decreased rainfall in the STA (and increased it in the NTA) in Israeli II.

A test of the claim by RF92 that seeding increased rainfall in the northern interior of Israel, while decreasing rainfall in the central parts of Israel, during the thirteen and a half seasons of the two experiments can be checked in a rudimentary way by using historical rainfall data for a much longer period than that used by GR90. According to R, seeding in the north part of Israel should increase the gradient in rainfall from the northern coast (north of Haifa) to the interior hill region beginning near Mar Meron and Mt. Kana'an in extreme northern Israel and on eastward downwind of the seeding line. Also, according to RF92 and R, seeding should have *especially* increased rainfall gradients in the north-south direction in Israel, since it increased rainfall in the north interior and decreased rainfall in the south interior (e.g., near Jerusalem).

We have tested these hypotheses using the detailed isoheyts of rainfall for each rainfall season from 1932-33 through 1975-76, compiled and analyzed by the IMS, that appear in the *Atlas of Israel* (1985). Figure 5 shows the results of this survey of rainfall gradients using the departure from the medians of each gradient (computed for the full 44 seasons) as a measure of any seeding-induced effects in rainfall gradients. While the data are not sufficiently precise to be tested statistically, it is obvious that there is not even rudimentary support for RF92's claim that seeding increased rainfall in the NTA while decreasing it in the STA during Israeli II. Figure 5 indicates that the rainy seasons of Israeli II, on which RF92 based their "increases-in-the-north-and-decreases-in-the-south" hypothesis, run exactly counter to that hypothesis in both the east-west and north-south directions!

In conclusion, we believe that the limited areal scope of the rainfall analyses by RF92 (and by R in his present Comment) led them to confuse widespread natural rainfall events with seeding effects.

In his Section 7c, R claims once again that there is not a contradiction between the results of Israeli I and II and the dust/haze hypothesis of RF92.

We disagree. No one who is familiar with the several previous analyses of Israeli I (i. e., Gabriel 1967; Gabriel and Baras 1970; Wurtele 1971; GN74), all of whom concluded that the best indications of a seeding effect in Israeli I were in the CTA and in the BZ on days when the wind was from the southwest quadrant, could not be astonished at R's claim that the rainfall in Israeli I in the CTA was actually decreased by seeding.

As we have pointed out already, R's reasoning is flawed because it is based on the assumption that rainfall patterns in Israeli I and II were similar. As noted by RH95, and again in the present Reply, in Israeli I the heavier rainfall was more localized on CTA seeded days than it was on the seeded days of the NTA in Israeli II (e.g., Brier et al. 1973; Rosenfeld 1989; RH95).

Rosenfeld's view that in Israeli I rainfall in the CTA was decreased by seeding, but that in the NTA it was substantially increased (>30%) by seeding, is an interesting new idea. We reiterate our suggestion that R submit a paper for publication to substantiate this claim.

Rangno and Hobbs (1995) noted that regional values of the seed/no seed ratios

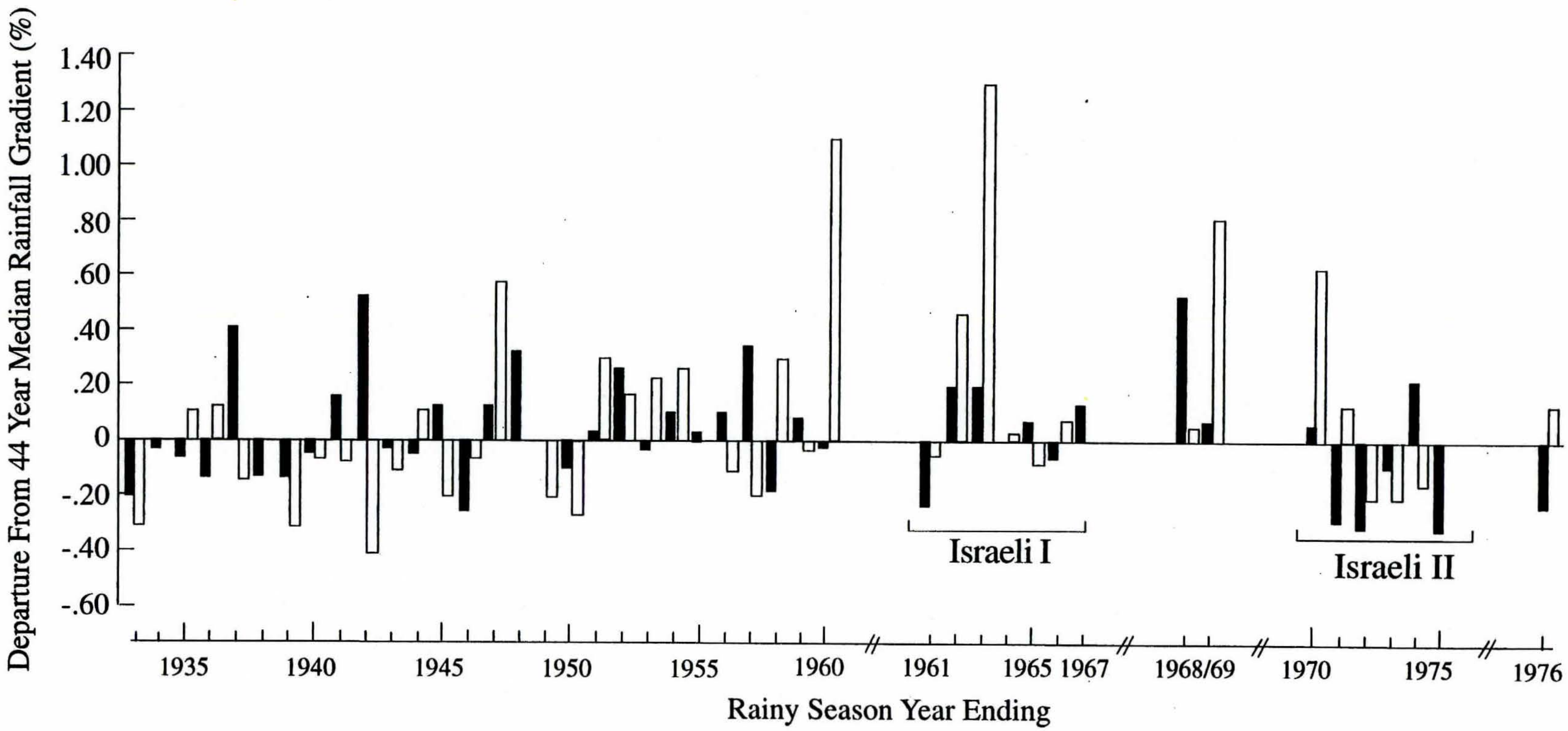

Figure 5. Departures from rainfall gradients in Israel for each rainfall season (mid-October through April) from 1932-33 through 1975-76. The open bars are the differences in average rainfall between the coast north of Haifa and the maximum in the northern hills around Mt. Kana'an. The black bars are the differences in rainfall between the rainfall maxima in the northern hills around Mt. Kana'an and those in the southern hills near Jerusalem. An upward/downward bar indicates that the difference in rainfall between the two regions is greater/less than the 44-year median. Therefore, for the periods 1961-1967 and 1969-1975 (which are the years of Israeli I and II), an upward/downward bar supports/refutes RF92's hypotheses that seeding 1) increased rainfall in the northern hill region compared with the northern coast, and 2) increased rainfall in the north target area but decreased rainfall in the southern hill region.

for Israeli II were not displayed by RF92 for the dust/haze and non-dust/haze days. We suggested that the display of such values would have helped readers understand RF92's evaluations of Israeli II. However, in his Section 7d R asserts that regional values of the these ratios are superfluous in the evaluation of cloud seeding experiments.

Seed/no seed ratios provide a first step in the evaluation of cloud seeding experiments, and this statistic has been a standard component of such evaluations for many decades. These ratios are vital in order to see if the expectation of an anomaly in them, produced by cloud seeding, is reasonably localized to regions downwind from where seeding took place. In our view, the failure to give proper weight to regional seed/no seed ratios accounts for the many of the erroneous conclusions that have been drawn for the Israeli cloud seeding experiments.

5. Concluding remarks

Many unanswered questions remain concerning the Israeli cloud seeding experiments, the clouds of Israel, and the intriguing new hypotheses about seeding effects and cloud seeding potential that have been postulated by R and Reisin et al. (1996). It is clear that the questions outnumber the answers, and that the results of the Israeli cloud seeding experiments are, at best, ambiguous.

We join with R in urging a comprehensive study of clouds in the Middle East with respect to their seeding potential, the efficacy of line seeding, and many other questions that we have raised. We recommend that such a study be carried out by a team of experts under the auspices of an international organization, such as the World Meteorological Organization.

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