

Holon, Oct 18, 1986

Dear Art,

Sorry to have been so long in getting~~g~~ in touch with you after such a delay - we did not get lost in the glens of Scotland as you must have assumed. We returned only a couple of weeks ago and I haven't half caught up with accumulated chores which had been sadly neglected these past few weeks. The main reason for my not having caught up is of course that I am not a "retired" person, as I have started, since August, <sup>in</sup> a new job at a weekly magazine. The "Israel Review" is published in English, French and German editions and my official title is editor of the German edition. However, I do all the proof-reading and corrections in all three languages and have, as you can imagine, a very busy time. The magazine comes out sometime in November. I took this offer after I terminated my stint in the tourist business where I had been replacing their permanent representative (I didn't know this at the time). I still keep open an option for something similar in January, in case the new magazine does not make money - it is always a gamble - and is obliged to close down. I forgot to mention that my main occupation at the magazine is translations (Hebrew-English etc) and supervision of translators. Anyhow, it is interesting and it offers a challenge to me - all that in a field completely new for me and entirely removed from my experience as a meteorologist. By the way, we'll publish an article on the occasion of the 50th anniversary of the Meteorological Service. *(I'll send you a copy)*

This brings me to your letter with enclosed reprint. Your paper on Climax Re-Analyzed left me wondering about intentional or unintentional scientific skulldoggery in the field and struck - as you know - a responsive chord when I think about the reporting on and claims about our own cloudseeding program. I shall detail some of the points with which I disagree, I mean in the paper you sent me and which is quite familiar to me. These and others (not concerning you, as they relate to the overall conduct of the experiment) were detailed by me in several memoranda to the authorities concerned, but <sup>re</sup>action is slow in forthcoming, if at all. It may need a push from outside to get things moving and expose the practises of certain persons. I hear there has been some reaction to the project at the Boulder conference - could you sent me the relevant reprints of the proceedings?

These are some of my criticisms of the paper by G. and N. :

Para 1. No comment. The experiment was conducted in keeping with accepted scientific procedures

Para 2. The Israeli Experiment II.

Reference is made to ~~2~~"the primary purpose ...enhancing rainfall in the LKC" This is only partly true, as the experiment was conceived as a follow-up experiment to the preceding one, namely in two areas seeded according to a cross-randomization sequence."It was no longer possible to apply the cross-over scheme..." is a gross distortion of the truth, since the area S was at least as much seeded ~~x~~ as the N during the period 1969-75. It is true that a much longer period of randomized seeding would be required to detect a given effect (in view of the smaller CC between the two areas, referring to mean daily rain, of course); it was nevertheless decided at the outset to go ahead with cross-randomization. In fact, I think the experiment should have been continued until a definite trend in both areas could be established, rather than terminating it as soon as a positive result emerged in one area (the N in this case). I shall come back to this later.

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The "adoption of three hypotheses in Exp. II" was, contrary to the claim of the authors, not made at the beginning, but post factum in the light of analyses performed after the project had terminated. This is only a minor point perhaps, but it shows a way of thinking.

"Cloudtops ~~wt~~ warmer than  $-5^{\circ}\text{C}$  were not seeded". In fact, the threshold was  $-8^{\circ}\text{C}$ . There were many instances where the tops did not reach these levels and yet rained, sometimes heavily *from such clouds.*

The statement represents only a minor inaccuracy, but it contradicts the claim that such clouds produce no rain. However, I don't think that this would have any bearing on the outcome of the experiment.

It is entirely unnecessary to stress that the rainfall observer did not know the seeding sequence. If anyone - inconceivably - would have <sup>been</sup> interested he/she could have obtained the information easily (for example from the ground generator operators). It may, however, be a good idea to obtain independent rain data from the Met. Service to check whether all relevant data were used by the project statistician or a subjective selection. For the first experiment, all data were published in the proceedings of the 5th Berkley Symposium, for example. This should have been <sup>done</sup> ~~forthcoming~~ for the the Second Experiment, too. I am inclined to think that the data are reliable.

Another inaccuracy is the mention of "generators ... mostly on the windward sides of hills". Had there been a map of generator locations - after all, a network of 45 generators in the N and 18 in the S constituted an integral part of the seeding effort - this would have been apparent at once.

"From 1969 to 1975, the S had no control area and seeding .. was not experimental". The S could not care less whether it was seeded "experimentally" or otherwise as long as it was seeded randomly, which it was. This quibbling with words amounts to a serious distortion of the truth. In fig. 1 of the reprint, the S area was deliberately left out, although the "buffer" was shown - "buffer" between what? Compare this map with the <sup>enclosed</sup> map of fig. 13.2, p. 466 of the paper "Rain Stimulation and Cloud Physics in Israel", in the book "Weather and Climate Modification, (1974) by the selfsame authors - the discrepancies are glaring. The reason for this later omission, <sup>in contrast</sup> ~~contrary~~ to how the experiment was planned and begun in 1969, can only be that the S showed no or negative results which were then conveniently swept under the carpet. In the 1974 <sup>study</sup> paper, the authors the preliminary results of Exp. II: ~~RDR~~ <sup>RDR</sup> of 1,10 ~~xxxxxxx~~, but SAR less than 1 for the S. It may be that this negative trend continued in the S - if otherwise, the result should have been reported. In any case, the S data should have been analyzed and reported; in fact none of the <sup>foreign</sup> researchers invited to check the results of Exp. II was ever supplied with these data and no wonder they verified the (positive) results in the N. I doubt if Messrs Wodley and Changnon who visited the project and pronounced it a resounding success, were ever given all the information. The reason for not counting the S area as "experimental", namely the absence of a control area, is patently absurd: Expt. I with its positive results, did not have a control area and yet the results were accepted. In exp. II there were areas, such as the N control area, which were never seeded, and could have been used, in the post analysis, as control (although the correlation between it and the whole of the S is low; a higher correlation can be found when referring to only the northern part of the S, <sup>for</sup> ~~for~~ example). Another possibility would have been to declare a narrow, say 10-15 km wide, coastal strip between Tel Aviv and Ashkelon or Gaza as control, since the seeding aircraft, flying over the coast (or 2-3 miles) off the coast, could not have conceivably ~~been~~ influenced (although technically "contaminated") ~~the S~~ in this zone because of the time delay in view of the prevailing westerly winds on rain days. With some effort, all these suggested analyses could have been made in order to learn more about the effects (or their absence). In any case, in the 1974 paper, no hint is given that the S would eventually be excluded from the analysis! Some scepticism as to the effectiveness of the seeding in S was even then expressed, tentatively ascribed to a supposedly different temperature structure of clouds in the S. This assumption was not ~~borne out~~ <sup>borne out</sup> later (excepting the extreme S, near Beer Sheva) and I am sure that for a large part of the area, say in the Center, cloud characteristics do not differ

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\* My suggestions to that effect were flatly rejected at the time by G. and N.

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To sum up, the deliberate omission of an analysis of an experimental area contradicts all the tenets of fair scientific reporting. It goes without saying that any unfavorable results would have severely restricted the availability of funds - a well-known fact - but would have given a more unbiased picture of the true potentialities to the water planners. In fact, hydrological analyses (by TAHAL for example) find no evidence of increased ground water or spring flow, even in the North where statistical results were positive. No <sup>hydrological</sup> analysis was attempted in the south. As of today, the Kinneret, the principal water~~res~~ reservoir in ~~the~~ the N, is at its lowest level since 1936 in spite of the massive (operational, i.e. not randomized) seeding operations under taken in that area (LKC"). It makes me think...

Exp III is taking place in the S, from 1975, while the N was and is being seeded routinely on all rainy and suitable days. No analysis has been forthcoming (there were some interruptions in the randomization scheme during parts of drought years) for the S, although a control area (not a good one, as it lies to the south of the area and is therefore poorly correlated with it) has been introduced. I <sup>maybe</sup> suspect that no positive results <sup>show</sup> would appear and not even a preliminary analysis was attempted ~~resp.~~ resp. reported. (In expt. I, an annual statistical report was published and the lack of significance for this short period mentioned).

Turning now to Table 6 of the paper, the stratification given seems to me rather strange. It is nowhere mentioned that post hoc analyses of this kind should not be couched in probabilistic terms, strictly speaking. Note that the ~~ratio~~ ratio of 1.46 refers to only 42 days, or 17% <sup>of all days</sup>. I think it will always be possible to find a sub-group where any desired effect can be found. You may draw many more conclusions from this table.

There is no mention in the paper of a description of how the cloud top measurements were obtained, except with a 3 cm radar. The weather radar is situated at Beit Dagan (Met. Service) and thus some 100-120 km from the center of the N area. Cloud top measurements were made subjectively by radar observers and cannot be considered reliable in view of the distance involved - we are talking here of convective clouds. The number of cloud tops measured on an individual day depended strongly on the cloud cover for that day - it was ~~it~~ physically impossible to measure all ~~cells~~ cells on a day with a large cloud cover. An error of 1 km in estimating the echo top could be easily ~~made~~ made, amounting to 6 C. Also, there was no radiosonde in the N to arrive at an accurate estimate of top temperature.

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In addition, the effective radar coverage from Beit Dagan did not

comprise even half of the LKC area. It is unsound, therefore

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to relate rain data for this area to very incomplete and often unreliable radar data.

It may be of interest to note that the research aircraft flew only a small fraction of total flying hours (6-8%) and almost never under typical raining

Conditions, as it was always seeking out single clouds to make the measurements. Thus, even cloud top verifications were very rare and confined to clouds with tops  $< 16000$  ft. Moreover, the equipment was antiquated (lack of funds) and often quite unreliable (Compare the data <sup>to the table</sup> quoted in the 1974 paper).

I have raised here only some of the more salient points needing clarification. All these points were (ingenuously) refuted by G - I doubt if there is really anyone among the various institutions and committees who knows anything about the subject, so he gets away with it. The information I have given you could be used as a background to specific questions you may want to raise in this connection. I feel not competent enough to judge the cloud physics and rain physics adduced by the authors to provide a physically plausible explanation for the success claimed - you'll certainly be a better judge of that. Feel free to address to me further

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Let us hear from you soon - in the meantime good wishes and regards to you from Kerol & Rita.