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Israeli experiment I

20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

As stated by the Editors of the Special Issue, it was compiled with reference to two of my statements: (1) that much of the current weather modification literature is slanted and unreliable, and (2) that progress in the building of a reliable weather modification technology requires an interdisciplinary study of as many completed cloud seeding experiments as possible. The material published in the Special Issue relates to two completed experiments, the Tasmania and the Israeli experiments. It is shown that a realistic appraisal of a

20. (Cont. from reverse)

completed experiment requires a prolonged effort, including an examination of quite a few relevant publications and, on occasion, some numberical work on published raw data. It appears that an interdisciplinary reanalysis of the Tasmania experiment can contribute to the development of a reliable cloud seeding technology.

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1. INTRODUCTION

The contents of the special issue of Communications in Statistics (Vol. A8, No. 10, 1979) edited by J.J. Wiorkowski and P.L. Odell are:

(i) Preface by the Editors, pp. 953-954.

(ii) E.J. Smith, "An experimenter's view of the application of statistics to cloud-seeding experiments," pp. 955-973.

(iii) K. Ruben Gabriel, "Some statistical issues in weather modification," pp. 975-1015.

(iv) A.J. Miller, D.E. Shaw, L.G. Veitch and E.J. Smith, "Analyzing the results of a cloud-seeding experiment in Tasmania," pp. 1017-1047.

As correctly stated by the two Editors, the idea of a special issue originated from their conversations with Donald B. Owen and myself. At the time, I expressed my concern about inconsistencies abundant in the cloud-seeding literature. However, the nature of these inconsistencies is not illustrated in the special issue and the reason for my concern is not explained. I note that the "special issue was compiled so that...the statistics community could get relevant information and insight into the very difficult..." domain of "weather modification experiments."

The purpose of my present "comments" is to help to achieve this insight. A few illustrative examples are likely to be useful.

2. NATURE OF "INCONSISTENCIES" AND REASONS FOR MY CONCERN

The following passages are quoted from the first column of my review article (Neyman, 1977a). The title of the article is "A statistician's view of weather modification technology." My focus was on the following three questions:

"Question i. Is the present cloud seeding technology reliably confirmed as a means of alleviating drought?

Answer: No.

Question ii. Is there evidence that cloud seeding affects pre-

cipitation and, if so, what are the indicated effects?
Answer: It appears established that cloud seeding does affect precipitation and does so over areas far in excess of the intended targets, occasionally up to distances on the order of hundreds of kilometers. In some cases the effects are large increases and in some others large decreases in precipitation. The several hypothetical mechanisms advanced to explain these effects and to predict them vary in their empirical support and convincingness. In particular, much of the existing literature, some of it stemming from official sources, is SLANTED AND UNRELIABLE. [Here caps are added. The emphasized words correspond to the "inconsistencies" mentioned by Wiorkowski and Odell.]
Question iii: What are the means of advancing the development of a reliable weather modification technology?
Answer: Establishment of at least two philosophically different interdisciplinary research groups, including statisticians versed in experimental work....with a special mission to reevaluate the data of as many already performed cloud seeding experiments as possible, and continuation of properly planned experimentation. The suggested research groups should have unlimited access to the same data and have facilities for personal meetings to exchange ideas. They should be funded from sources other than those engaged in funding cloud seeding."

I should have added that reevaluations of completed experiments are tedious and time-consuming. Thus, to achieve reliable results, the work of the recommended at least two interdisciplinary research groups should be planned for several years.

My point is that, soon after the publication of my review there appeared a two-volume document, The Management of Weather Resources (Cleveland, 1978). To my regret, the work summarized in this document was done in a hurry and was not quite "interdisciplinary." The first volume of the document represents a report of the Weather Modification Advisory Board appointed by the Secretary of Commerce and the second that of the Board's Statistical Task Force. The two groups worked under pressure to complete the studies within about a year. The lack of harmonious interdisciplinary effort is reflected in the FOREWARD to Vol. II written by Harlan Cleveland, Chairman of the Advisory Board. Here the following statement appears relevant: "The Board's own judgments do not always follow the statistical findings to their ultimate inconclusiveness..."

The following quotation from page B-8 of the Statistical Task Force report seems to reflect both the lack of interdisciplinary effort and the time pressure:

> Tasmania
>
> This study depended very heavily on control areas to provide precision of results. Seeding was interrupted -- or aborted -- whenever wind directions suggested that AgI nuclei might be conveyed directly toward a control area. (Since such days were still counted as "seeded days," no false significances would result.) In view of the limited area offered by the interior of this large island, remote-effect issues must, we feel, be taken seriously. We do not feel competent to adequately evaluate them here.

In the following pages there are described "inconsistencies" of two different kinds, both occuring in the Wiorkowski-Odell specil issue No. 10. The focus is on two experiments, the Tasmanian and the Israeli experiments.

3. INTERFACE BETWEEN THE WORK OF WEATHER MODIFICATION EXPERIMENTERS AND THAT OF STATISTICIANS

The title of this section is based on a sentence in the Introduction to the article by E.J. Smith (pp. 955-973), under the inspiring title "An experimenter's view of the application of statistics to cloud-seeding experiments." The spirit of the article seems to be properly summarized by the dictum that "both the statistician and experimenter are searching for the truth as to what the cloud seeding does."

I fully agree with this spirit, but must suggest a change in the formulation of the dictum. My preference is for the following: "both the statistician and the experimenter OUGHT to search for the truth as to what the cloud seeding does."

Unfortunately, such an interdisciplinary cooperation does not always occur with the result that "much of the contemporary literature...is SLANTED AND UNRELIABLE."

While the motivation for cheating is, clearly, some personal gain, the means used by particular experimenters may be complicated. In fact, it may be subconscious. From the point of view of the development of a reliable weather modification technology, the following passage from (Neyman, 1977a) is relevant:

> RELIABLE INFORMATION ON EFFECTS OF CLOUD SEEDING MAY RESULT FROM STRICTLY RANDOMIZED EXPERIMENTS
>
> With reference to precipitation augmentation, the essence of a *randomized* experiment is, briefly, as follows:
>
> First, "potential experimental period" (or "seeding opportunities") and the "response variable" are clearly defined. In the simplest case, the potential experimental period may be of fixed duration, say 24 hr from 0730 of a given day to 0730 of the next. In this case, the response variable might be the precipitation measured by specified gauges (defining the "target"), say from 0800 of the given day to 0800 of the next.
>
> A special organizational unit, to be called the "randomization center" (RC), must be established. At an appointed time before the beginning of a potential experimental period, the experimenter reports to the RC whether the approaching potential experimental period is suitable for inclusion in the experiment -- that is, whether it is to become an "experimental unit." In the affirmative case, the experimenter communicates to the RC certain other information deemed important, such as the nature of the prevailing weather (type A, type B, or type C, etc.). In response, the RC provides the experimenter with a randomized decision, either a permissive "seed" or a categoric "do not seed." It is emphasized that the randomized decision must be communicated to the experimenter AFTER his declaration as to the approaching experimental unit, not before. In fact, it would be best to arrange that even the personnel of the RC have no advance information on the nature of the forthcoming randomized decision. Perhaps, a computerized random number generator could be adjusted for this purpose....

> The primary evaluation of the experiment must be based on all the experimental units (some seeded and others not) and no others, and it must use the originally defined response variable. The supplemental information about the type of weather ought to be used for stratification purposes and is useful by providing the experimenter with means to verify his ideas.

The reader will realize that the spirit of the above passage is fully consistent with Dr. Smith's idea that "both the statistician and the experimenter are searching for the truth as to what the cloud seeding does."

As emphasized by Smith, the experimenter has a specific field of competence "in subjects such as physics, meteorology" [perhaps, also] "aviation and engineering." The type of experiments contemplated by Smith is exemplified by the Tasmania experiment analyzed by Miller et al, p. 1017 in the same issue of Communications in Statistics. The object "of the experiment is to find out if seeding clouds with ice nuclei can increase the rain MEASURED OVER A DESIGNATED TARGET AREA [emphasis added]. *Time is divided into periods* [which I shall assume to be one day] on some of which selected at random, seeding takes place..." The statistician is expected to analyze the data reliably, in order to answer the questions of the experimenter and, possibly, also those of his "customer" such as a hydro-electric authority.

Quite rightly, Smith is uneasy about the assumptions underlying the statistician's work. "The objective ought to be to avoid the use of any assumption unless there is good evidence that it is acceptable." I fully agree. Specifically, a statistician's "reliable assumptions" are proved theorems, such as the law of large numbers, the central limit theorem, etc. Provided there are no mistakes in the deductions, the conclusions drawn using such theorems will be reliable (Neyman, 1979a).

My own concern is that, in addition to hypotheses underlying the work of the statisticians, there are also hypotheses adopted by the experimentalists. Are they always realistic? Here is an example.

At the end pararaph of Smith's Introduction there is the following outline of a proposed method of analyzing the data of a randomized experiment:

> For each seeding day the measured target area rainfall is compared with an estimate of the rain that would have fallen in the same period if seeding had no effect. This estimated rain is derived from covariates which usually include rain in adjacent control areas in both seeded and unseeded periods. (In simple experiments the measured rain in a control area may be used as an estimate).

It is here that I see a contradiction between the opinion of Dr. E.J. Smith and my own. It seems to me that the methodology indicated by Smith depends upon an unverified hypothesis, a "meteorological" hypothesis, that seeding over the designated target does not affect the rainfall in the adjacent control area. Otherwise, "the measured rain in a control area" during a seeding period would not be an appropriate estimate of the target rainfall "that would have fallen in the same period if seeding had no effect."

In full conformity with Smith's own suggestion that "the objective ought to be to avoid the use of any assumption unless there is good evidence that it is acceptable," I feel in need of an effort at verification.

How can one check, at least tentatively, whether the assumption that the rainfall in a control area is not affected by seeding over the target? How can one do so without relying on any other unverified assumption? The only answer seems to be: examine the totality of published experimental data and produce a summary.

The data on the Tasmania experiment we have were published two years ago (Smith <u>et al</u>, 1977). There are two sets of data. Both give average rainfall amounts in the intended "target" and in four other areas in Tasmania. Three of them are described as "controls" and the fourth as the Eastern Subsidiary Area, see Figures 1 and 2. The precipitation amounts are means per raingage, per "period."

The particular periods varied in length, from about ten to eighteen days. As indicated in the above quotation from the report of the Statistical Task Force, there were some irregularities.

One set of data refers to four years 1964, 1966, 1968 and 1970 which were the experimental years, with seeding over the target during randomized periods. The other set of data refers to years 1965, 1967 and 1969 during which rainfall amounts were measured in all the five areas but there was no seeding. We may label these years, the "control years." The purpose of the three "odd" control years intermingled among the four "even" experimental years was to check the hypothesis, again a meteorological hypothesis, that the seeding during a particular year can affect the precipitation during the subsequent year, the so-called "presistence" hypothesis.

The attempt at an objective summary of all the published precipitation data resulted in Figures 1 and 2. Both show the map of Tasmania, including the target and four other areas mentioned. The Stat. Lab's contribution is limited to percentages attached to each of the areas of interest. They represent what I like to label "percent effect" of seeding, namely 100[(S-NS)/NS]. Here S and NS represent the mean measured rain in the given area that fell during the periods with and without seeding, respectively.

Figure 1 corresponds to the four experimental years, 1964, 1966, 1968 and 1970. It is seen that the percent effect in all the five areas is negative. The least seed period deficiency of rain, namely 8%, was found for the target and for the "South Control" area. The greatest seed period deficiency was found for the North Control. It amounts to 21%, more than double that for the target! The deficiency of seed period rain for all the areas combined is 15%, a finding likely to be of interest to the hydro-electric authority.

What is convincing and what is not is a subjective matter. In my own opinion, Figure 1 fails to support the assumption that seeding over the target does not affect the rain over the North Control. If anything, it seems to support the idea that seeding by methods comparable to those in Tasmania can have far-away effects that are stronger than those in the target. See Section 2 above.

LOCATION OF TARGET
AND CONTROL AREAS

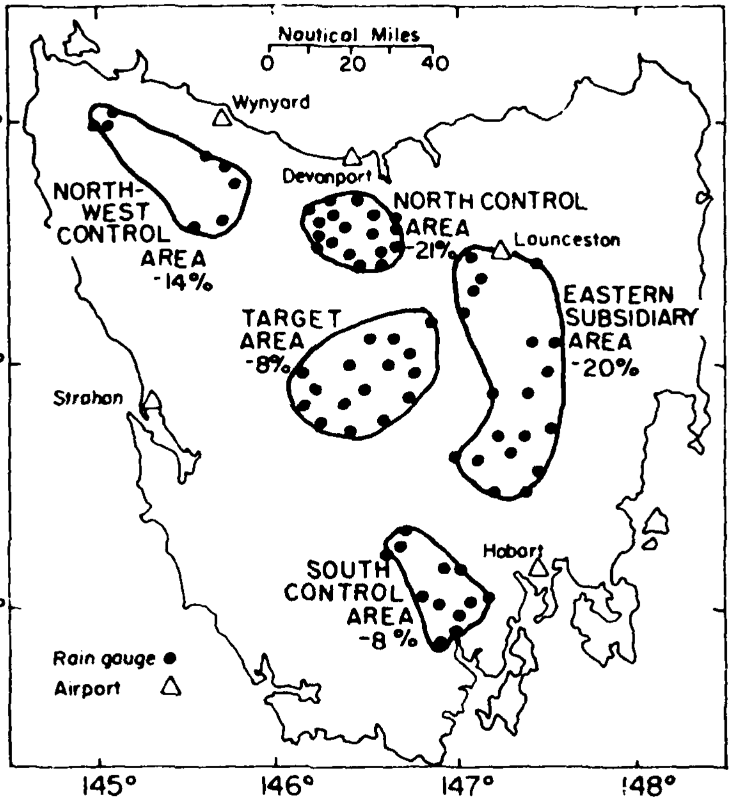

FIG. 1. Relating to experimental years.

LOCATION OF TARGET AND CONTROL AREAS

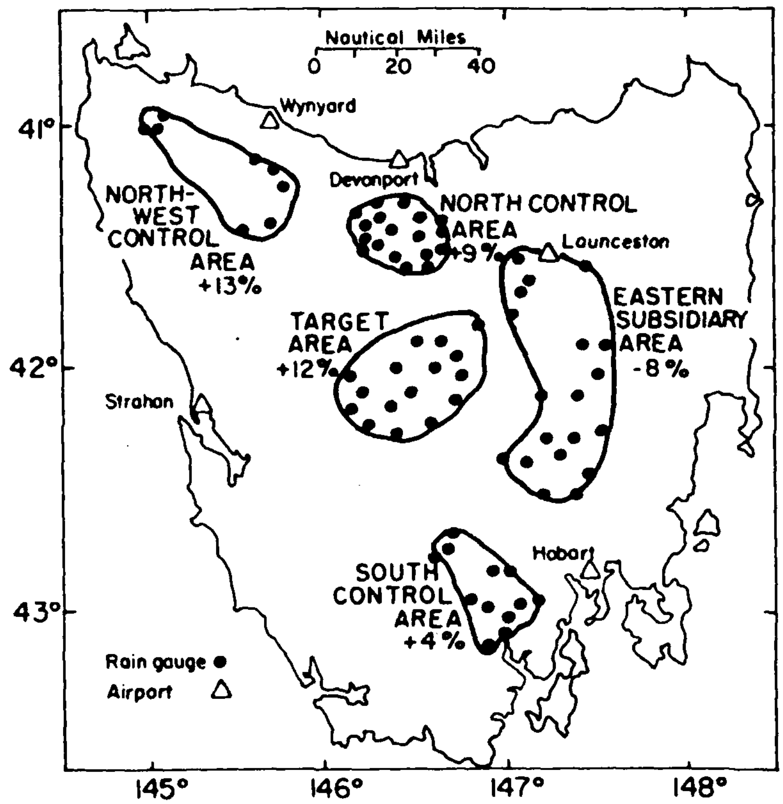

FIG. 2. Relating to control years.

Here, with some feeling of regret, I must mention a point relating to the statistical team that cooperated with Dr. Smith in the evaluation of the Tasmania experiment. As indicated on p. 1021, the team used the "double ratio statistic," because it "has an intuitive appeal." The reader will have no difficulty in noticing that this "intuitive appeal" depends very much on the unverified "meteorological" hypothesis that the seeding over the target does not affect the rain in the control. In particular, if the effects of seeding on rain in the target and in the control are both negative and if the negative effect in the control is stronger than in the target (as in Figure 1), then the double ratio statistic will lead to a misleading conclusion.

It seems to me that the selection of a test statistic merely because of an "intuitive appeal" is not an inspiring procedure. The more challenging way is to formulate some intelligible optimality of the needed test criterion and to DEDUCE the needed formula (Neyman, 1979a).

Figure 2, analogous to Figure 1, illustrates the precipitation pattern in Tasmania that prevailed during the three control years, when there was no seeding. Here, the "percent effects" were calculated for what may be labeled "placebo seeding." As mentioned, the published data give the rainfall amounts for consecutive periods. The succession of "placebo seeding" was arranged to coincide with the succession of real seeding for the experimental years. It is seen that the patterns of rainfall exhibited in Figures 1 and 2 are very different. Is this difference causally related to seeding during the experimental years and to the absence of seeding during the controls? Not necessarily. It may be due to my mistaken allocation of "placebo seeding" during the control years.

The essence of the presistence hypothesis seems to be that, whatever the effect of seeding in a given year may be, this effect "persists" over several months of the next year with a gradual decay. Here we face a difficulty that seems even greater than that in the establishment of the phenomenon of the far-away effects of local seeding. The point is that the possible "persistence" effect is confounded with familiar seasonal variation in precipitation. In very general terms, the Berkeley area has two rainy periods, of about one month duration. One, a mild one, occurs in October-November, and the other, stronger one, begins about the middle of January. Naturally, the intensity of these rainy periods varies considerably from year to year.

4. ILLUSTRATIONS OF FAR-AWAY APPARENT EFFECTS OF LOCAL SEEDING FOUND ELSEWHERE

During our more than a quarter of a century of interest in weather modification, it was natural for our Berkeley group to study the reliability of experimental designs that were commonly used. This included the cross-over design and the design using control areas. We found them both unreliable. The relevant question was whether the seeding over the designated target can affect the rainfall over a distant area.

Experiments suitable for studying this question are those of long duration, with a properly randomized design of seed/no seed over a designated target and with a "natural" experimental unit of 24 hrs, morning to morning. Here, the word "natural" refers to the periodic changes in temperature, etc., connected with the irradiation from the sun. The additional requirements refer to the availability of data on wind directions and on hourly precipitation data.

Our studies included three experiments: (1) the Swiss experiment Grossversuch III, (2) the Whitetop experiment of Professor R.R. Braham performed in Missouri, and (3) the Arizona experiment performed by Professor Louis J. Battan. To my regret (Neyman, 1979b), a closer study of the Whitetop experiment indicated some difficulties with randomization. For this reason, the findings described below refer to two experiments only, the Grossversuch III and the Arizona experiments. The details of the work are somewhat voluminous (see references quoted below) and the following brief summaries must suffice.

The far-away effects of local cloud seeding were first studied for the Swiss experiment Grossversuch III (Neyman *et al*, 1969).

Here, the target was the canton Ticino on the southern slopes of the Alps. Th᷉ studied far-away localities included two areas in Switzerland in which we had reliable data from 20 gages each. One area was near Zürich (some 80 miles away) and the other near Neuchâtel (some 120 miles away). The average apparent effects of seeding on all the 190 days with "stability layers" were as follows:

Table I

Apparent effects of seeding at Grossversuch III on days with stability layers

Area	Percent Effect	Two-tail Significance Probability
Ticino	+64	0.031
Zürich	+67	0.012
Neuchâtel	+57	0.037

As mentioned, Table I refers to all the 190 days, irrespective of the prevailing wind directions. The following Table II gives similar data for the 94 days when the published winds had a southerly velocity component. This stratification was performed because of the information that the primary source of atmospheric humidity in Switzerland is the Mediterranean, in the south.

Table II

Apparent effects of seeding at Grossversuch III on days with stability layers and southerly winds

Area	Percent Effect	Two-tail Significance-Probability
Ticino	+102	0.018
Zürich	+116	0.004
Neuchâtel	+ 64	0.060

In interpreting this table one must remember that Zürich is almost directly north of Ticino while Neuchâtel is substantially to the northwest. Here, then, the degree of "downwindedness" was crude, which stimulated the development of a new methodology, the moving grid methodology (Lovasich et al, 1971).

Curiously, the apparent effects of seeding on days with uninhibited updrafts were all negative, but none significant by customary standards. The subsequent use of the moving grid methodology clarified the situation. The original measurement of the degree of downwindedness was too crude. Our final finding was a 61% average deficiency of seeded day precipitation in localities 90 to 180 miles downwind from Ticino, with a two-tail significance $P = 0.002$.

The above findings for Grossversuch III were unexpected and stimulated our interest in the question of the generality of the phenomena observed. It is this question that motivated our persistent studies of the Arizona experiment performed by Professor L.J. Battan. The experiment, with two "programs," included 212 experimental days. Professor Battan's target was an isolated body of Santa Catalina Mountains. The seeding was conducted over 2-4 hours beginning at about 12:30 p.m. Battan's own evaluation was based on rainfall during 5 hours only, from 1 p.m. to 6 p.m. Our reevaluation used 24 hour rainfall from noon to noon. It included not only the Santa Catalina Mountains but also a locality, Walnut Gulch, about 65 miles to the south southeast from the Santa Catalinas. Here, the Water Conservation Research Division of the Agricultural Research Service maintains a very tight set of recording rain gages. The person in charge is Dr. Herbert B. Osborn. It appeared that during the Arizona experiment there were 26 gages that operated reliably. Table III summarizes the results obtained (Neyman et al, 1972).

Table III

Apparent effects of seeding at the Arizona experiment

Locality	All Days		Walnut Gulch Downwind	
	Percent Effect	Two-tail Probability	Percent Effect	Two-tail Probability
Santa Catalina	-30	0.06	- 9	0.78
Walnut Gulch	-40	0.02	-73	0.01

It is seen that both parts of Table III indicate the apparent effects of seeding over the Santa Catalinas on the 24 hour rain at

Walnut Gulch are stronger than in the intended target. Also, the significance of these effects is more impressive.

The timing of these apparent far-away effects is of interest (Neyman, 1977a). It is illustrated in Figures 3 and 4.

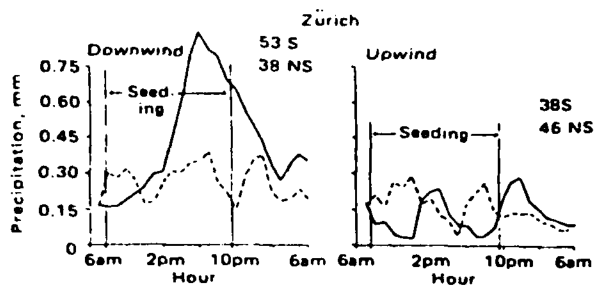

FIG. 3. Diurnal variation in hourly rainfall in Zürich when it was approximately downwind and when it was approximately upwind from Ticino. Solid lines correspond to days with seeding; dashed lines to control days.

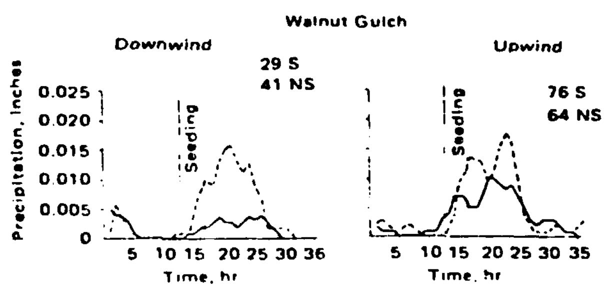

FIG. 4. Diurnal variation in hourly rainfall in Walnut Gulch when it was approximately downwind and when it was approximately upwind from the Santa Catalinas. Solid lines correspond to days with seeding; dashed lines to control days.

It would be most interesting to see whether the "downwind/upwind" differences illustrated in Figures 3 and 4 were also observable during the Tasmania experiment. The difficulty is that in Tasmania the experimental periods were rather long, presumably with very variable wind directions, with varying wind velocities, etc.

5. COMPARISON WITH THE ISRAELI EXPERIMENT I

It is my opinion that the Tasmania experiment, as described in the Wiorkowski-Odell special issue of Communications in Statistics, and especially as described in the Final Report (Smith et al, 1977), is a very valuable contribution to the weather modification literature. The reason is that the material published includes many details about the facts that happened, the facts relevant to Dr. Smith's question about "what the cloud seeding does." I wish I could express a similar opinion on the Israeli experiment. This applies both to the original evaluation (Gabriel, 1967) and to Professor Gabriel's article in the Wiorkowski-Odell special issue now discussed. The following passages, marked A and B, are quoted from this article.

A. Page 983.

> Perhaps one reason for the surprising success of the two Israeli experiments is that expertise in cloud physics was closely involved in all stages of design, evaluation and analysis (Gabriel, 1967; Gagin and Neumann, 1976). *In the existing uncertain state of the art, we cannot afford to do without the very best available experts.*

B. Page 977.

> In the first Israeli experiment (Gabriel, 1967), the experimental unit was a 24-hour period, STARTING AT 8 AND ENDING AT 8 on the next day. (1) Randomization was applied to calendar dates and...

The reader will notice that the above capitalized description of the experimental unit is not complete. The hour 8 may be 8 a.m. or 8 p.m. The following passages, marked C and D, are quoted from Professor Gabriel's article, described as (Gabriel, 1967).

C. Title Page.

> THE ISRAELI ARTIFICIAL RAINFALL
> STIMULATION EXPERIMENT.
> STATISTICAL EVALUATION FOR THE
> PERIOD 1961-65
>
> K.R. Gabriel
> Hebrew University, Jerusalem

1. Introduction

A rainfall stimulation experiment is being carried out in Israel by silver iodide seeding from an aircraft in a randomized crossover design. The operations are directed by Electrical and Mechanical Services (Mekorot, Ltd.), Mr. M. Cohen, Director, and are financed by the Israeli Ministry of Agriculture. The experiment is conducted under the guidance of the Rainfall Committee whose chairman is Professor E.D. Bergmann, and the related research work is performed at the Hebrew University, under the direction of Professor J. Neumann. THE AUTHOR IS RESPONSIBLE FOR THE STATISTICAL DESIGN AND EVALUATION. Daily rainfall data are provided by the Israeli Meteorological Service from its regular network of raingage stations. [Emphasis added.]

D. Page 94. Table specifying the experimental units on which the evaluation was based.

TABLE II
UNITS EMPLOYED IN THE EXPERIMENT

Season	Date	Period	Unit of Time
1961 half	19. 2.61-15. 4.61 15.10.61- 5.11.61	weekly	0800 to 0800 hrs
1961-62	7.11.61-15. 4.62	daily	2000 to 2000 hrs
1962-63	16.10.62-15. 4.63	daily	2000 to 2000 hrs
1963-64a	1.11.63- 8. 1.64	daily	2000 to 2000 hrs
1963-64b	9. 1.64-30. 4.64	daily	0800 to 0800 hrs
1964-65	16.10.64-15. 4.65	daily	0800 t0 0800 hrs

Here the specification of the experimental units is complete. However, it is indicated that the actual units varied. For a brief period the unit was 8 a.m. to 8 a.m. Then, for a longish period, it was from 8 p.m. to 8 p.m. Then there was a return to the original 8 a.m. to 8 a.m. This description generated some literature.

I felt impressed by the variability of the experimental units, and when writing a historical review (Neyman, 1977b), expressed the opinion that the design and the evaluation used are "unprecedented." Next year there appeared two protests (Gabriel and Neumann, 1978) and (Mielke, 1978). Here it is relevant that Dr. Mielke, a Professor of Statistics, is at least partly responsible for the evaluation of the cloud seeding experiment known as Climax I. This evaluation is also mentioned in (Neyman, 1977b).

Here, I wish to refer to the statement by the Editors Wiorkowsi-Odell that their Special Issue was complied so that the statistics community could gain insight into the complexities of weather modification studies. My suggestion is that interested members of the statistics community examine the publications mentioned in the preceding paragraph. In fact, I wish to suggest one more paper (Hobbs and Rangno, 1979). This paper ends with the following sentence:

> "In view of the importance that has been placed on the Climax results, an independent evaluation of the statistical results of these experiments is urgently needed."

My question is: Why only of Climax?

ACKNOWLEGEMENTS

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