Persistent Effects of Cloud Seeding with Silver Iodide

E. K. BIGG AND ENID TURTON

CSIRO Division of Atmospheric Research, Aspendale 3195, Australia.

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ABSTRACT

A statistical examination of precipitation records in and near areas where cloud seeding experiments have taken place in Australia strongly suggests the delayed effects of seeding. The most conspicuous effect is an increase in precipitation 1 to 3 weeks after a seeded day, which would have caused the conventional statistical estimates of success to have been misleading. Through comparison with earlier findings and some observations of prolonged increases in ice nucleus concentrations following the application of silver iodide to the ground, it is argued that secondary ice nuclei are involved in the persistent effects of seeding. If this is so, then they must have been more effective in enhancing precipitation than the silver iodide, itself; this leads to the possibility that there may be better ways of stimulating precipitation than those used so far.

1. Introduction

An observation by Grant (1963) that concentrations of ice nuclei remained very high for considerable periods after cloud seeding with silver iodide had ceased, led Bowen (1966) to postulate that the apparent decrease in effectiveness of Australian cloud seeding operations with the length of experiment was due to persistent and cumulative effects of the silver iodide reaching the ground. Rosinski (1966) described experiments providing an explanation in terms of the production of secondary ice nuclei by reaction of liberated iodine with plant terpenes.

A reexamination by Bigg (1985) of measurements of concentrations of ice nuclei made in conjunction with cloud seeding experiments also showed strong support for persistent effects of seeding on time scales of both weeks and months.

Bigg and Turton (1986) carried this aspect further by a statistical examination of the precipitation in target and controls during seeding in the Tasmania I experiment (Smith et al. 1979). They found, on average, a period of enhanced precipitation following seeded days, peaking on the tenth day after seeding but lasting in all for about three weeks. They also attempted to verify with a physical experiment that such an effect could occur by applying silver iodide to a totally enclosed grass plot and measuring the concentration of ice nuclei in the air above it. The positive result led to a further experiment, as yet unpublished, in which silver iodide in the form of an aqueous colloid was applied to 30 ha of wheat in an open field. Measurements of ice nucleus concentration in a network surrounding the field pointed to a release of ice nuclei about 13 to 22 h after application, with the effects dying away in the course of a few days, but recurring on the tenth day. The latter experiment failed to provide a clear indication as to whether Rosinski’s mechanism was correct, but at least confirmed that in open air conditions, applying silver iodide to the ground results in subsequent enhanced concentrations of ice nuclei.

The reason why our earlier statistical treatment was confined to one particular experiment was that its design and length made it the most favorable. All but one of the earlier Australian experiments were of the “crossover” design in which one of a pair of target areas was seeded on a random basis. Clearly, if persistent effects of seeding are a reality, both areas will become affected and the chance of detecting such effects (or, indeed, the effects of seeding) will be reduced.

In the present work we attempt to avoid this problem by treating each seeded area of crossover pairs as a separate experiment, while introducing new controls in order to use the same sort of analysis as in the Tasmanian experiment. As we pointed out in that work, there are possibilities of obtaining misleading results from the type of analysis used, and in this paper we will discuss them in more detail. But, always, the greatest problem is the “noisiness” of rainfall records and the difficulty of detecting small signals buried in that background. This, of course, is why it is so difficult to obtain a statistically satisfactory estimate of the rainfall increase due to seeding in an experiment. In this paper, we will attempt to reduce the problem by combining the results of all experiments. Fortunately, the relatively homogeneous design and meteorology of the Australian experiments makes this task possible.

Corresponding author address: Dr. E. Keith Bigg, 12 Wills Avenue, Castle Hill, NSW 2154, Australia.
2. The superposition method
   
   a. The superposition functions

   Suppose that the daily precipitation in the target area, $T_i$, is $P_i$, $(i = 1 \cdots m)$ and in the control area, $C_i$, is $q_i$ $(i = 1 \cdots m)$. Suppose that the seeding sequence is represented by a series $s_i$ $(i = 1 \cdots m)$ in which $s_i = 1$ for a seeded day and $s_i = 0$ for an unseeded day. Let $N$ be the number of seeded days; then:

   $$N = \sum_{i=1}^{m} s_i.$$

   The mean target precipitation on any day, “$d$” days from a seeded day is

   $$\bar{T}_s(d) = \frac{(\sum_{i=1}^{m} s_i P_i + d)}{N}$$

   and, similarly, for the control area:

   $$\bar{C}_s(d) = \frac{(\sum_{i=1}^{m} s_i q_i + d)}{N}.$$

   When $d = 0$ these values are the mean precipitations of all seeded days in the target and control.

   Control areas are always chosen so that their precipitation correlates well with the precipitation in the target when seeding is absent. If the correlation were perfect in the absence of seeding, the single-ratio superposition function

   $$S(d) = \frac{\bar{T}_s(d)}{\bar{C}_s(d)}$$

   would immediately yield both the change due to seeding and any subsequent changes, providing that these effects were simply additive. When seeding is relatively frequent compared to the span in $d$, which is being examined, the apparent effects of seeding, or delayed effects, are “smear.”

   The effect of seeding is best illustrated by a specific example in which we take the actual distribution of seeded days, $d$ days from a seeded day that was found when all Australian experiments were combined—this is the distribution with which we will subsequently be dealing. We assume a hypothetical target and control area in which the precipitation is 1 mm each day in each area, except on seeded days, when in the target it is 1.1 mm.

   Superposition produces curve A of Fig. 1. The peak is detected in its correct position and having its correct value, but $S(d)$ is also enhanced for all other values of $d$. The curve is symmetrical about $d = 0$ to the same extent that the frequency distribution of seeded days is symmetrical.

   Now let us suppose that in addition to the effect on day 0, the precipitation for $d = 10$ (the tenth day after seeding) is also 1.1 mm in the target, and if the day is both a “tenth day” and a seeded day, it is 1.2 mm. The result is shown as curve B on Fig. 1. All ratios are enhanced even more and the peak on day 10 is quite clearly defined, but the most conspicuous feature of the curve is the difference in mean levels for $d < 0$ and $d > 0$. If we could ensure that it was not due to other causes, the difference in means would represent a more sensitive test for the existence of any delayed effects of

   ![Fig. 1. Hypothetical example of the superposition method. The distribution of seeded days, $d$ days, from a given number of days before or after a seeded day is the same as in the composite of all Australian cloud-seeding experiments. Target and control rainfall were 1.0 mm on every day except curve A, target rain 1.1 mm for $d = 0$; and curve B, target rain 1.1 mm for $d = 0$ and $d = 10$; it is 1.2 mm for cases which are both $d = 0$ and $d = 10$.](Image)
seeding. Even if the effects are confined to one day, superposition causes their influence to be spread over several days and the mean makes use of all the available information. At the same time, the random fluctuations are reduced. Also, no prior knowledge of when delayed effects of seeding occur is needed in order to detect them, providing they lie within the viewing window.

b. Validity of the method for detecting delayed effects of seeding

In the real world, target and control precipitation are never perfectly correlated, which makes it difficult to assess the effects of seeding. There are several ways in which the function $S(d)$ can have systematic variations (as distinct from random fluctuations, which will always be present) even in the absence of seeding. The most common variation without seeding is when a seasonal trend, which is not symmetrical with respect to seeded days, exists in the precipitation ratio. Another variation is for periodicities, or quasi-periodicities, in precipitation appearing to a different extent or having a different phase in target and control. Both these circumstances produce effects that are usually visible for both positive and negative values of $d$. The first test of whether differences between mean levels for positive and negative $d$ could be related to after-effects of seeding, is to observe whether the variations in $S(d)$ for negative $d$ appear to be purely random.

It would still be possible to have no obviously suspect trend in the curve for negative values of $d$, and a detectably different mean value for positive values of $d$ that was not caused by seeding, but it would require special and unlikely circumstances. For this reason, in our earlier paper we used instead a double ratio. In the Tasmania I experiment, the analysis made use of an experimentally declared set of days, $u$, which were “suitable for seeding but unseeded.” We can form a second ratio:

$$U(d) = \frac{\bar{T}_u(d)}{\bar{C}_u(d)},$$

based on these experimental days as key days in just the same way as for the seeded, $s$, days. We define the double ratio superposition for day $d$ from a seeded day to be:

$$D(d) = \frac{S(d)}{U(d)}.$$  

Providing that the $u$ days have been chosen to be meteorologically similar to $s$ days and to be similarly distributed throughout the year, variations in $D(d)$ unrelated to seeding should be minimized; it could never be claimed that variations were eliminated. We have to judge whether they have or have not got a physical basis in view of the size and nature of the differences before and after day 0 and their consistency in divided data.

A price has to be paid for the added security of using this double ratio. Inevitably there are random fluctuations in both $S(d)$ and $U(d)$ and the double ratio enhances the fluctuations, making the signal (if any) harder to detect. Even in the best experiments, it has taken more than a hundred seeded days to detect with any confidence, a 10% increase due to seeding, simply because of these fluctuations in a comparable double ratio. The same considerations apply to detecting possible after-effects of seeding. It seemed that because the Australian experiments were all relatively homogeneous in design and execution, we could greatly reduce noise problems by combining the results of all experiments in an objective fashion. Such a procedure should also further reduce the risk of irrelevant effects, and at the same time, test the wider applicability of our demonstration that delayed effects of seeding occurred in the Tasmanian experiment.

3. Summary of cloud seeding experiments in Australia

There have been three scientific experiments using targets and unseeded controls, and four using pairs of areas in which only one area was seeded at a time (“crossover” design). All used airborne seeding burning silver iodide–sodium iodide–acetone solutions except for the most recent one (Tasmania II) where silver iodide–ammonium iodide–acetone solutions were used. Their locations are shown in Fig. 2. In addition, there have been many nonrandomized operations, with the two longest-running shown as shaded areas in Fig. 2. They have not been used herein because of occasional difficulties with the large target areas and nonrandomized operations; it has been verified that their inclusion makes no great difference to the conclusions reached.

Each of the three experiments using target and unseeded control appeared to be successful in increasing precipitation to a relatively high degree of confidence. The Snowy Mountains experiment has been described by Adderley and Twomey (1958) and by Smith et al. (1963a). Seeding was carried out from 15 March to 1 December in each of 5 yr, with a total of 173 days (13.9% of all days in the combined experiments). Seeding was randomized by periods, determined meteorologically by the passage of anticyclones, but with a minimum of 8 days. Double ratios were consistent with an increase of 19% due to seeding.

The Tasmania I experiment has been described by Smith et al. (1979). The final year’s work has appeared only as an internal report. The experiment was spread over 8 yr, 1964–71. Although only 5 were seeded: lengthy periods without seeding were included to reduce the supposed cumulative and persistent effects of seeding. Altogether, 260 days were seeded and, although some were excluded on the basis of the design plan, all have been used in this analysis. This sample represents 20.9% of the total of seeded days in the combined experiments. Seeding was again by periods, determined in the same way as for the Snowy Mountains experiment. Days “suitable for seeding but unseeded” were declared experimentally on the same basis as the
seeded days and used in the assessment. Double ratios were consistent with large increases due to seeding in autumn and winter, diminishing in spring and disappearing in summer. Overall, an increase in precipitation of about 11% was indicated.

The Tasmania II experiment (1979–83) was the only one to use liquid water content as a criterion of suitability for seeding. There were far fewer seeded days (58), and these were drawn only from April–October. Results have been published only as an internal report of the Division of Cloud Physics, CSIRO. Daily randomization was used, and again, days suitable for seeding but unseeded were selected. Double ratios (based this time only on seeded days, not periods) were consistent with seeding increases of over 30%.

None of the four crossover experiments suggested any significant increase in precipitation—in fact, in three of the experiments, double ratios were consistent with a decrease in precipitation. Also, in three of these experiments, the crossover periods were randomized in the same way as in the Snowy Mountains experiment, while in the fourth, Warragamba, daily randomization was used.

The New England experiment ran for 6 years, 1958–63, with 187 seeded days in the south area (15.0% of the total) and 164 in the north (13.2%). The experiment continued for about 10 months of each year, with operations being suspended during the wheat harvest period of November–December. This has been described by Smith et al. (1965).

The Warragamba experiment ran continuously for 4 years (1960–63) and has been described by Smith (1967). There were 121 seeded days in the south area, and 128 in the north, representing 9.7% and 10.3% of the total for the combined experiments.

The South Australian experiment continued from April through October for each of 3 years, (1957–59). There were 55 south-seeded, and 47 north-seeded days, 4.4% and 3.8%, respectively, of the total for the combined experiments. This has been described by Smith et al. (1963b).

In the Darling Downs experiment, the apparent reduction in precipitation was so large that the experiment was discontinued after 2 years (1960–61). A description has only been published in internal reports. were 24 south-seeded and 28 north-seeded days, amounting to only 1.9% and 2.2%, respectively, of the total.

4. Combination of the experimental data
   a. Choice of controls

Each seeded area of the crossover experiments was treated as a separate experiment and new controls ex-
established for each. We have pointed out that peculiarities in single target/control ratios, such as seasonal trends, can lead to false signals in the superposition analysis. The problem is likely to be reduced by using all possible control groups which, however, increased the possibility of detecting irrelevant results due to anomalous T/C trends in one or more of the controls or experiments.

We therefore used controls to the west, northwest, north, and south for each seeded area. The southwest was not used because insufficient rainfall stations existed for three of the experiments. Eastern controls were also used but kept separate; our hypothesis, that the delayed effects of seeding result from silver iodide deposited on the ground, would lead to a prediction of far less effect for this group because the predominantly westerly winds on all seeded days would have distributed much of the silver iodide to the controls also. The problem in considering eastern controls is that, if large apparent effects of seeding were found, they would probably be spurious and cast doubt on any effect found using the other controls.

In choosing the controls, all Bureau of Meteorology rainfall stations with sufficiently complete records were used in a sector of approximately 45°, 50–150 km from the target area. It was not possible to use unseeded north controls for the southern sector of crossover experiments, nor unseeded south controls for north areas. For these we had to use the companion area in its unseeded periods, a procedure which would probably reduce the ability to detect delayed effects if they should be present.

In order to avoid "end effects" in the data, area precipitation was calculated for at least one month before the first, and one month after the last seeded date. A difficulty arose with the Snowy Mountains experiment, because no daily measurements from the inner target were available. Instead, we used the five Bureau daily-read gauges in the outer target area, all lying on the eastern side.

b. Days suitable for seeding but unseeded: "u" days

In all experiments but the Tasmanian ones, "u" days were not declared at the time of the experiment, and it is not possible to select them now with any certainty. In all the earlier experiments a strict assessment of "seedability" was not available and it was generally assumed that most rain-bearing systems would contain suitable clouds. The instruction to the operators was, "When in doubt, seed." Consequently, if we choose unseeded days with approximately the same distribution of target rainfall amounts as on seeded days, the two groups are likely to be meteorologically similar, remembering that there was a great preponderance of westerly rain-bearing winds at seeding levels in all the experiments. If the u days are chosen from unseeded periods of seeded years there is the risk that delayed effects from preceding seeding will influence the result. If we choose them from totally unseeded years, we run the risk that the years could be climatically different, with different T/C seasonal trends. We opted for the latter procedure, but selected the u days from three separate groups of years in which there was no seeding to reduce the possibility of such climatic vagaries.

c. Combination of the data

Having selected all the target and control precipitation series, the superposition functions S(d) and U(d) were next calculated for d in the range -31 < d < 31. The means often departed substantially from 1, and so that equal weight could be given to all pairs that were normalized to the mean value of 1.0 for -31 < d < 0. The same normalizing factor can be used for the days in the range -1 < d < 31. The mean value for D(d) for each pair was therefore also 1 for -31 < d < 0.

To combine these and to form the averages, S(d), U(d) and thence, D(d) = S(d)U(d), each was weighted according to the number of seeded days on which it was based.

We believe that the use of a huge number of seeded days (1245), 11 different experiments, the multiplicity of controls, and double ratios using u days from unseeded years, makes the probability very slight of a significant difference occurring in the two halves of a superposition function spanning 60 days, unless its cause has some physical basis related to seeding.

5. Results

a. Combined results for all experiments, for the whole year using double ratios

Figure 3 shows the results of this combination of all available data. The broken lines, drawn more heavily for the days following a seeded day, are the double ratios D(d) for values of d from -30 to +30. The solid lines smooth these results with a smoothing function giving weight 4 to day d, weight 2 to (d + 1) and weight 1 to (d + 2).

We notice immediately the sudden transition from apparently random fluctuations about a mean value of 1 before a seeded day, to large and apparently non-random changes following it. The first half of the graph gives good reason to suppose that there are no peculiarities in the double ratio of artificial origin, the second half, that there is a real and prolonged aftereffect of seeding.

This obvious impression can be made more quantitative by considering the mean, B, of D(d) for -31 < d < 0, and comparing it with the mean, A, for 0 < d < 31. We assume that the fluctuations about the mean of the "before" days are normally distributed. Then the standard deviation σB of the mean is σ/301/2 where σ is the standard deviation of the 30 in-
d. Separation into two single ratios

If the signal is spurious, we need to find where it has crept into the analysis. In Fig. 4 we break Fig. 3 into seeded, \( S(d) \) and unseeded, \( \tilde{U}(d) \) components, using the same sort of presentation as before. We see at once that the only important contribution to the result comes from \( S(d) \) (except for \( d = 0 \), and that is an inevitable consequence of choosing the \( u \) days on the basis of a threshold rainfall in the target rather than in the control). We therefore have to examine this function in detail to ensure that the signal does not arise from causes unrelated to seeding. The absence of such large trends in \( \tilde{U}(d) \) using the same controls is, of course, a strong indication that they are related to seeding, but there is always the possibility that subtle differences, such as distribution of seeded days in relation to season, could have caused the discrepancy.

We can now ask by how many standard deviations does the mean, \( \mu \), of the 30 “after” days exceed \( B \). The answer is 17.7.

While ratios are not normally distributed about their mean, the bias is slight for the departures in question. Consequently, we are either dealing with a very large spurious signal, or an overwhelming significant physical phenomenon.

c. Comparison of summer and winter results

The most likely cause of spurious signals is seasonally related changes in target/control precipitation ratios. Those changes would have to be very large and to have finely tuned characteristics in order to produce such an obvious feature appearing only for values of \( d > 0 \) when results are combined from so many different areas and experiments. It is most unlikely that the changes would be equally present in an arbitrary division of the year, so that if \( S(d) \) is calculated for each portion separately, the similarity of the two curves can be regarded as a test of the reality of the apparent signal in Fig. 4.

As soon as we start to reduce the number of seeded occasions, noise problems increase, so in such a comparison it is essential to have approximately equal numbers of seeded days in the two portions. For this reason the year has been divided into winter seeding, defined as May through August, and a remainder.

The result is shown in Fig. 5. The prolonged high values for about the same positive values of \( d \) in each
makes it seem improbable that anything but a real physical phenomenon is involved.

d. Breakdown by season and by control area

We can easily postulate that some strange distribution of seeded days in relation to seasonal changes or periodic phenomena in one of the target/control pairs could have caused the observed “before–after” differences seen in Fig. 4. To disprove such an assertion would be very difficult. Figure 5 makes it seem unlikely, but when we are dealing with a totally unexpected happening, even small doubts of their reality may be sufficient for them to be dismissed.

If we could show that the same sort of differences were present in both seasons for all controls separately, the lingering doubts must surely become very small. Reducing the amount of data in each group by a further factor of 3 must, of course, reduce the significance levels if the signals are real, and it makes the assumptions of normal distributions somewhat worse.

Table 1 shows the control area, the season, and under "differences", the number of standard deviations of $B$ by which the mean $A$ exceeds the mean $B$. Note that the only groups in which no effect similar to those of Figs. 4 and 5 could be claimed are the eastern controls, which were previously excluded because we expected, on physical grounds, to find less aftereffects of seeding relative to the target areas. This observation lends further support to the reality of the differences in the remainder.

6. Experiments in other countries

We have obtained precipitation data from several experiments in other countries, either from publications or directly from the experimenters involved, in the hope of finding whether these unexpected effects

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**Fig. 4.** Seeded, $S(d)$, and unseeded, $U(d)$, components of the superposition function. Solid lines: smoothed; broken lines: unsmoothed.
of seeding are unique to Australia, or have a wider applicability. In view of the fact that it was observations of persistently high concentrations of ice nuclei in the United States which stimulated the original interest in the possibility, it seemed likely that we would find the same effect in experiments in that country.

Unexpected difficulties were encountered when examining data from the United States. For example, most experiments ran for limited seeding seasons and the experimental area precipitation data stopped at the end of seeding. This is unfortunate, because end-effects, when we use a 60-day window, can create all sorts of spurious results. We attempted to overcome this problem by obtaining tapes from the U.S. National Weather Service archives of daily precipitation for the states in which the experiments were located. Without exception, there were insufficient well-correlated stations having records in both targets and prospective control areas to make the sort of analysis feasible that we have described above. There were other problems, such as “floating targets” (Project Whitetop), lack of precipitation data for nonexperimental days (Florida Area Cumulus Experiment), seeded and nonseeded days in tied pairs (Arizona), randomization by storm systems instead of by period, and so on. These made the task unattainable of providing a homogeneous combination—such as in the above analysis—to overcome the problems of random fluctuations.

There were, indeed, some indications in most of these experiments that similar effects to those of Fig. 4 occurred but they were statistically unconvincing on their own.

**Table 1.** Differences in the means for $B$, $-31 < d < 0$, and $A$, $0 < d < 31$, of the superposition functions $S(d)$, expressed as the number of standard deviations of $B$, for different control areas and seasons.

<table>
<thead>
<tr>
<th>Control</th>
<th>Season</th>
<th>Differences</th>
</tr>
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<tbody>
<tr>
<td>West</td>
<td>Winter</td>
<td>3.0</td>
</tr>
<tr>
<td>West</td>
<td>Summer</td>
<td>9.3</td>
</tr>
<tr>
<td>Northwest</td>
<td>Winter</td>
<td>5.2</td>
</tr>
<tr>
<td>Northwest</td>
<td>Summer</td>
<td>12.2</td>
</tr>
<tr>
<td>North + south</td>
<td>Winter</td>
<td>5.5</td>
</tr>
<tr>
<td>North + south</td>
<td>Summer</td>
<td>14.5</td>
</tr>
<tr>
<td>East</td>
<td>Winter</td>
<td>-0.4</td>
</tr>
<tr>
<td>East</td>
<td>Summer</td>
<td>1.5</td>
</tr>
</tbody>
</table>
The only experiments for which we were given full experimental data, and which were similar in design and execution to those in Australia, were the Israel I and II experiments described by Gagin (1974) and Gagin and Neumann (1981). The first was a crossover experiment, for which no unconfounded control area precipitation was available, but the second was apparently better suited to our analysis. The main difficulty is that less than one-seventh of the number of days used in the combined Australian data were available, so that random fluctuation problems were necessarily much worse. A second difficulty was end-effects, with half the year being very dry. Another was the use of daily randomization, making the double ratio method of dubious value unless similar unseeded days can be drawn from unseeded years. These were not available. Using single ratios only, we obtained a result that was quite similar to that of Fig. 4. However, a trend during the days before a seeded one was immediately obvious, and destroys confidence that the apparent delayed effects of seeding were real. One cause of the trend was a large monotonic change in T/C precipitation during the seeding season. Allowing for the trend, there still appeared to be a residual effect, but again it was unconvincing without the availability of a double ratio based on u days unaffected by seeding.

7. Discussion and conclusions

a. Cause of the delayed effects of seeding

The analysis here gives no indication of the nature of the effect. It appears to have been very strong both in the Snowy Mountains and in Tasmania, in winter, where much of the precipitation was snow, but appears to have also been present in the remaining experiments. Because of one case described by Bigg (1985) where seeding was followed by a large enhancement of ice nucleus concentration with just the delays found here, and also in the more recent work by Bigg and Turton (1986), it seems reasonably certain that secondary ice nuclei are generated and later become airborne. So far, the mechanism has not been adequately confirmed.

b. Effectiveness of the secondary ice nuclei

One of the curious implications of the analysis is that the secondary ice nuclei must be more effective than the silver iodide in enhancing precipitation. Despite consideration of the fact that the artificial anomaly in \( U(d) \) at \( d = 0 \) causes the effect of the silver iodide to be underestimated, \( D(d) \) is still much higher for 9 < d < 18 than for d = 0.

If the reality of the secondary nuclei is accepted, then the further implication is that there must be better ways of seeding than have been used so far. Is this because of their ice nucleating efficiency, the fact that they could be present at all times and hence miss no opportunities for seeding, or because they may be large and hygroscopic and aid coalescence rain formation processes? If sufficient confirmation of the reality of delayed effects can be found, a very wide field of research will be opened up.

c. Evidence against delayed effects of seeding

It is very easy to find an absence of evidence for delayed effects of seeding by considering insufficient data. We could (and did) come up with statistically inconclusive results by taking separately most of the experimental areas used in this study. This result is not “evidence of absence” and it was only the tantalizingly similar results from the individual experiments that stimulated us to try to devise methods to suppress the random fluctuations that make it so hard to find whether an apparent effect is real. The degree of success surprised us considerably.

We have been asked by one reviewer to state all the evidence against delayed effects of seeding. We know of no contrary evidence other than the apparent improbability that delayed effects should occur. When the possibility was first raised, over 20 years ago, we did not believe it, so we cannot blame others for having the same reaction now. It may be of interest that the present work arose from renewed public statements by Dr. Bowen in 1983 that cloud seeding had been very successful but persistent effects of the seeding had falsified the statistics. We decided that a report commissioned by CSIRO to test this statement failed to refute the possibility, and that the method described herein might do so more successfully. The outcome has certainly been unexpected.

d. Future work

We hope that others with access to more complete precipitation records will continue this study in other countries. We recommend that double ratio methods should be used initially to check the validity of the results but that single ratio methods may subsequently be more useful. Perhaps the most promising sources of data are very long sequences of operational seeding, providing adequate controls can be established.

One notable feature of weather modification has been the large discrepancy between the optimistic results claimed for many nonrandomized operations and the commonly pessimistic results from randomized experiments. Instead of assuming that the former have been inspired by commercial motives, it might be instructive to inquire if the latter resulted from degradation of the statistics by delayed effects of seeding. It is only when we make the assessment by comparing the precipitation in a target on seeded days with that in intervening unseeded days that such degradation can occur. Comparison with historical records may run into trouble with climatic shifts but, at least, it avoids the problems discussed in this paper.
Acknowledgments. We should like to thank the Director of the Anglo-Australian Observatory for the use of the Observatory’s computer, without which this work would not have been possible. We should also like to thank those investigators who provided us with data from their experiments. We regret that the problems discussed above prevented us from obtaining conclusive results.

REFERENCES


