PIGGYBACK WEATHER EXPERIMENTATION:

SUPERIMPOSING RANDOMIZED TREATMENT COMPARISONS

ON

COMMERCIAL CLOUD SEEDING OPERATIONS

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1. EXPERIMENTAL VERSUS OPERATIONAL NEEDS IN CLOUD SEEDING.

The effectiveness of cloud seeding is still moot despite some 30 years of experimentation and operational seeding. Scientists generally perceive the need for further controlled and randomized experimentation. On the other hand, some farmers and other members of the public have been sufficiently convinced of the effectiveness of cloud seeding to pay for its operation to increase rain or snow or to decrease hail. When they do so, they usually insist that clouds be seeded on every available opportunity, especially if the seeding project was stimulated by drought conditions. Unlimited seeding, as demanded by the user public which pays for the operations, does not leave any precipitation opportunities unseeded. It therefore does not allow the unbiased scientific evaluation of seeding effects; that would require comparison of randomly selected unseeded opportunities with seeded ones. The consumers' demand for maximal effect precludes such randomized abstension from seeding and makes unbiased evaluation most difficult.

The situation is analogous to that of evaluating a new medical treatment. Clinical trials can be conducted with random choice of patients to receive the new treatment or the old, *as long* as the medical profession is in genuine doubt about the relative benefits of the two treatments. However, as soon as the evidence for the advantage of either treatment becomes overwhelming, such trials must be discontinued. It then becomes unethical to randomly provide the inferior treatment to any patient. However, if variants of the superior treatment exist, one may then experiment only with these variants to decide which of them is preferable. Analogous experimentation with treated clouds will be advocated here.

2. THE IDEA OF "PIGGYBACKING" EXPERIMENTS ON OPERATIONS

We are faced with the dilemma that evidence which farmers and other users consider adequate to justify investment in cloud seeding may not be considered adequate by scientists who have to appraise the "state of the art". The users and the public have to be the ultimate judges of the desirability of cloud seeding, but it would be unfortunate if their immediate needs were to prevent further study of the effectiveness of the available techniques. We are here proposing a way to ease this dilemma by experimenting within the framework acceptable to the farmers, i.e., without ever completely refraining from seeding. We are proposing experiments that can be carried out without disrupting the cloud seeding operations that are supported by the user public. These experiments would ride "piggyback" on existing weather modification operations, by superimposing scientific trials on ongoing cloud seeding. (Statistical Task Force, 1978).

"Piggyback" experimentation of this kind can obviously not compare seeding with no-seeding, since introduction of no-seeding occasions would interfere with existing operations. It can, however, compare methods and concepts of weather modification whose relative advantages are not known. Farmers and other users pay cloud seeders for using the "available technology" to increase precipitation (or reduce hail). This is taken to mean introduction of AgI, or some other agent, into suitable clouds by means of ground generators, airborne generators or rockets. There is no compelling evidence to show what amount of seeding yields optimal results, or, for that matter, what the best method of delivery is. Farmers should therefore not object to "piggyback" experimentation with alternative seeding rates, methods of delivery, etc., so long as all these alternatives are reasonable ones in terms of the present "state of the art".

3. METEOROLOGICAL ISSUES TO BE CONSIDERED FOR POSSIBLE "PIGGYBACK" EXPERIMENTATION.

Approaches to experimentation involving randomization during operational projects, without having a no seed option, could beneficially include the modification hypothesis and modification methods. The general approach to cloud modification is to affect either the microphysics or dynamics of the clouds and cause changes in the amount or type of precipitation at the ground. Thus, there are two basic cloud seeding hypotheses to consider for testing: microphysical (static) or dynamic. The basic goal of seeding under the microphysical hypothesis is to increase the efficiency with which cloud condensate is transformed to precipitation particles, and the basic goal of seeding under the dynamic hypothesis is to increase water vapor condensed by a cloud system.

Under either hypothesis, there are three ways to approach the desired goal. First, seeding could introduce particles of a type not naturally produced by the unseeded cloud. Second, seeding might introduce particles sooner than they would be produced naturally. Third, seeding could add more particles than would be produced naturally. Any or all of these three approaches could be tested in an operational project under either the microphysical or the dynamic hypotheses. The operation could further allow "piggybacking" of tests on different particles or varying times of delivery.

Elements of these and other modification approaches could be the subject of experimentation. These can include randomization tests on (a) seeding materials, such as dry ice versus silver iodide; (b) testing of seeding rates such as high or low release rates of silver iodide, and (c) actual techniques of delivery of seeding materials, such as delivery inside clouds versus delivery at cloud base. For example, the design of a hail suppression experiment in Illinois (Changnon et. al., 1976) recommended a 3-way randomization based on no seed, in-cloud seeding, and cloud base seeding.

Thus, one can conceive that modification experimentation, as a "piggyback" effort within an operational project, could be done on the basis of the total modification hypothesis or on any of the major approaches including seeding rate, seeding material, and means of delivery. For example, the dynamic seeding concept chosen might utilize silver iodide released inside the cloud at high rates, whereas the microphysical approach to be tested could be totally different and involve low seeding rates involving silver iodide released at cloud base. This discussion is offered to point out the range of elements that could be experimented with. The choice of elements should depend upon existing knowledge in the area of the operational project and should build upon prior experience. Following are two examples to explore in depth the approaches that could be employed.

4. AN EXAMPLE AND THE OUTLINE OF AN EXPERIMENT

An example of the kind of problem that could be addressed by piggy-back experimentation is the choice of chemicals to be mixed with the AgI seeding agent. To illustrate from the Israeli experiments, ground generators (which served as a secondary delivery system) used either (A) a 5.46% AgI solution with NAI in acetone, or (B) a 1% AgI solution with NH_4I in acetone (Gagin, 1980). The lower AgI concentration in (B) was thought to be as efficient as the higher one in (A) because NH_4I burns out completely in the acetone burning process whereas NAI does not (See also Saint-Amand, 1980 on the topic of mixtures with AgI). Since it is not clear which combination has greater effect on precipitation, it should not be objectionable to the user public if the two combinations were applied alternately for experimental comparison. AgI seeding would be carried out on every available seeding occasion but an experiment on the mixture could be "piggybacked" on these operations.

An outline of a design for a piggyback experiment will be illustrated with this (A) versus (B) treatment comparison as follows:

1. An operational protocol will be agreed on by farmers and cloud seeders. It will include the definition of seeding opportunities, which will be unequivocally delineated in time, with a beginning and an end. The simplest units are fixed time units, such as days for which the weather forecast predicts suitable conditions. (E.g., Grossversuch III in Switzerland, see Schmid, 1967). More refined units, such as convective bands, need specialized measuring devices to define them [Elliot, St. Amand and Thompson, 1971). Whatever units are used, the initial declaration of their occurrence and the final definition of their termination must be made in the absence of any knowledge of the treatment allocated to that unit - this is referred to as blindness in definition. (Biases could occur if, for example, the meteorologist on board the seeding plane were aware of the treatment allocated and might be influenced by it in recommending the time of terminating the experimental unit. In the latter part of the FACE-1 experiment, special placebo seeding flares were used to ensure that the aircraft crew would be blind to the treatment assignment - Woodley, et. al., 1981.)

2. A randomly chosen allocation of either treatment A or B will be provided in sequentially numbered envelopes. These envelopes will be accessible only to the seeding material supplier or store manager and their contents will not be revealed until the data are all in and ready for final analysis. As a result, all personnel, including the person defining will open an envelope in private and supply the allocated treatment solution in an unmarked container. No indication of the type of solution supplied may reach the seeding officer, flight crew, or collectors of data. As a result, all personnel, including the person defining the units, the cloud seeding pilots and technicians and the collectors of precipitation data, will be blind to the treatment allocated to ensure blindness in operation.

4. To ensure blindness in measurement, precipitation data must be collected for each seeding opportunity in ignorance of the seeding treatment allocations. This may be difficult to maintain, since it is not always feasbile to keep the treatment allocations confidential for a long period following seeding whilst the data are being assembled and edited for final analysis. (Thus, there have been fears that biases could have been introduced by the successive revisions of FACE-1 data which were made after the treatment allocation was known. Nickerson, 1981). To avoid the possibility of such biases it may be essential to rely on reasonably, objective measurements, such as raingauge readings, and allow only on-the-spot editing which would be done in blindness of the seeding treatments. Later revisions of data should be avoided.

5. In the final evaluation of the experiment, the assembled data will be compared with the treatment allocations to check for a difference in precipitation between the differently treated seeding opportunities. Statistical tests can then be applied to measure the significance of these differences.

Some comments on this design are in order. First, it has not excluded any seeding opportunity from being seeded -- thus satisfying the user public's demands. Second, treatment allocation was randomized, thus allowing valid probabilistic inference, i.e., significance tests, on the results. Third, double blindness has been ensured in that neither the seeders nor the observers of precipitation knew what treatment was allocated to any one opportunity; thus, they could not, consciously or unconsciously, bias the results.

5. ANOTHER EXAMPLE AND VARIATIONS IN DESIGN

Another example would be to "piggyback" rates of seeding AgI. In the two successful experiments in Israel (Gagin, 1980), a reasonable seeding rate was 600 gms of AgI per hours; AgI was distributed by airborne burners using a mixture of AgI, NAI and acetone -- the weight of the AgI being 5.46% of the total weight of the mixture. This rate was originally suggested by Australian CSIRO cloud seeders and was intended to introduce 10 particles of AgI per liter of air at -15° (centigrade) temperatures. There appears to remain considerable uncertainty about the suitability, or optimality, of this rate. A seeding rate five times this amount is also considered to be reasonable (Gagin, 1980). Perhaps a seeding rate half the amount used would also be considered satisfactory. One must, apparently, admit to uncertainty as to where, within the range of 300 gms per hours to 3000 gms per hour, the effect of seeding would be greatest. This range of uncertainty is a legitimate aim for experimentation -- researchers should address it in order to narrow down present ignorance. Users should not object to such experimentation since they cannot know, any better than members of the cloud seeding profession, what is the best seeding rate.

A piggyback experiment on seeding rates would mostly be quite similar to the mixtures experiment described above. (See also Mielke, 1980a). A point of difference would be that one might choose more than two levels of treatment. One might, for example, prepare four solutions (A), (B), (C), and (D), with percentage AgI weight varying in such a manner that, with standard operating techniques, they would output 300, 600, 1200 and 3000 grams per hours, respectively. The envelopes would then contain randomized allocations to A, B, C, or D. Otherwise the design would parallel that for the NAI vs. NH_4I experiment.

A further point has arisen in this design — the choice of experimental treatments within the reasonable range. Generally, the most sensitive design for a monotone effect is to concentrate half of the treatments at each end of the range. However, if the effects are not monotone, e.g., if there were "over-seeding" beyond a certain rate, such a design might fail to reveal such a property. A design with some intermediate levels of treatment (probably one or two such levels) would be called for in this case. Such a design would be sensitive to such "inversions" and might therefore be preferable to a "both-extremes" design.

6. FURTHER ISSUES IN DESIGN

Another issue is that of required sample sizes and power, i.e., how many seeding opportunities have to be included in an experiment to give a reasonable chance of discovering a precipitation difference between the treatments. This depends, of course, on what one expects the size of the difference to be. In general, "piggyback** sample sizes will be similar to those for seed/no seed rain stimulation experiments, with the difference between the treatments replacing the seed/no seed difference in the latter. Since the difference between variants of a treatment are likely to be smaller than between treatment and control - especially if one experiments with the most promising treatment - the power of a "piggyback" experiment is likely to be less than that of a seed/no-seed experiment of similar length and conditions. Consider, for example, a randomized seed/no-seed experiment which requires 8 years to ensure 95% power of discovering the expected 20% increase in precipitation. If a "piggyback" experiment were run under the same conditions and the expected precipitation difference between the two treatments were 14% (approx. $20\%/\sqrt{2}$), then 16 years would be required to ensure 95% power. Larger differences could be detected more quickly; fewer observations would be needed if concomitant observations were available from nearby areas. (For a "piggyback" design involving individual clouds see Mielke, 1980b).

"Piggyback" experimentation does not allow no-seed occasions and thus cannot provide direct evidence of the effects of seeding versus no seeding. However, indirect information would emerge. If "piggyback" experiments revealed significant precipitation differences between treatments, they would implicitly demonstrate that at least one of the treatments affects precipitation. If, on the other hand, repeated "piggyback" experimentation with a variety of treatment agents, rates, methods of delivery, etc., revealed no more significant results than expected by chance, this would justify skepticism about the existence of any effects of seeding at all.

A single negative finding of this kind might be due to a fortuitous comparison of two equally effective treatments. A series of such negative findings could not well be explained in this way: It is implausible that all variants of seeding technique should be equally effective, especially if the range of levels tried were chosen to be wide enough.

If the direct verification of the effect of seeding is thought to be crucial, it might be acceptable to farmers to allocate a small part of the occasions to be unseeded controls. Thus, for example, perhaps 20% of the opportunities could be randomly allocated to control, and the other 80% of the occasions would carry a "piggyback" experiment on seeding rates of 300, 600, 1200 and 3000 grams per hour - 20% of the occasions to be allocated to each rate. Such a design would combine the advantages of seed/no-seed experimentation with those of more detailed exploration of rate differentials. All this could be obtained with little sacrifice of opportunities which the users want seeded.

The idea of "piggybacking" experiments onto ongoing cloud seeding need not be regarded as a single-shot attempt. Where several commercial cloud seeding operations are being carried out in a region it might be feasible to plan to "piggyback" replicate experiments on them or to design a series of experiments to complement each other. The planning and co-ordination of such a series of trials might require some state and/or federal monitoring and might indeed develop into the kind of "evolutionary operation" Box and Draper (1969). have discussed as a strategy of industrial research.

7. SUMMARY

We have attempted to demonstrate that "piggyback" experimentation can be superimposed on commercial cloud seeding operations without sacrificing

any opportunities on which users want to have the clouds seeded. It allows randomization to be incorporated in experimenting with differential *

treatment effects and valid statistical analyses to be implemented. It should, however, be clear from our discussion that randomized allocation and double-blind execution of the treatment requires careful control by an independent authority of accepted scientific integrity. In planning any such "piggybacking", it would therefore be crucial to define protocols carefully and unequivocally assign responsibilities for allocation. Otherwise biases are likely to creep in or to be suspected to occur. Experience in experimentation in many areas suggests that when this issue is not addressed adequately at the planning state, misunderstandings tend to arise and result in biases, real or suspected, which destroy the credibility of the experiment. It should not be too difficult to persuade planners of such experiments to incorporate sufficient safeguards to ensure valid and valuable results from "piggyback" experimentation.

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