COMMENTS ON THE GRIFFITH *ET AL.* REPORT ON OPERATIONAL CLOUD SEEDING PROGRAMS IN UTAH

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The report by Griffith *et al.* (2009) on operational cloud seeding programs in Utah states the following conclusion: "*The NAWC* (the Utah programs seeding contractor) *utilized an historical target/control regression analysis technique to estimate the effects of cloud seeding in the various target areas in Utah. These analyses suggest average seasonal effects ranging from 3-21%*." The quoted increases attributed to seeding are the range of point estimates from the evaluation of the various Utah target areas (their Table 2), point estimates that Griffith *et al.* have taken literally. Except for giving the correlation coefficients for the various target/control relationships, Griffith *et al.* do not provide any details about the specific evaluations that produced these results or their interpretation of them. Of particular importance, Griffith *et al.* do not provide a measure of the statistical certainty of each of the point estimates, i.e., a confidence interval and/or a P-value for each of the estimated seeding effects. **They did not provide it despite the fact that the description by Dennis (1980) of the historical target/control regression analysis methodology for evaluating operational (non-randomized) cloud seeding programs includes a statistical method of determining the statistical significance and/or the confidence interval of the point estimate of the seeding effect as well as a statistical method for determining the point estimate of the seeding effect.** The statistical significance of a point estimate of a seeding effect is determined by its P-value and/or its confidence interval. The World Meteorological Organization (WMO 2007) recommends that "*Confidence intervals should be included in the statistical analyses to provide an estimate of the strength of the seeding effect so informed judgments can be made about its cost effectiveness and societal significance*". Thus, Griffith *et al.* present no statistical basis for rejecting the null hypothesis that seeding had no effect on the average seasonal precipitation at any of the Utah operational program target areas. What then is the basis for the unsubstantiated conclusion by Griffith *et al.* that their historical target/control regression analyses suggest average seasonal effects from 3-21% for the various target areas of Utah?

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Complicating the interpretation of the evaluation of the Utah operational cloud seeding programs by the historical target/control regression analysis are the results of several studies that indicate this evaluation method is not robust for such applications and that this lack of robustness affects the reliability and accuracy of the estimates of the seeding effect that it produces. Nevertheless, Griffith *et al.* present the results and conclusions of their evaluation without any caveats. Consider the results of the following relevant studies:

1. Dennis (1980) stated "*Although the basic idea involved in the historical regression analysis is intuitively appealing, there are a number of difficulties with it*." He identified some of these difficulties, including:

a) reliability of the results unless the underlying data sets conform to the normal distribution which, for precipitation data, requires an appropriate data transformation. Since Griffith *et al.* use seasonal precipitation values averaged over multiple sites, the distributions of their target and control response variables are not highly skewed; however, they do not conform to the normal distribution,

b) unconscious bias in the selection of data in post-hoc evaluations;

c) difficulty in eliminating residual uncertainties; and

d) representativeness of the target/control relationship and its stability in time. Dennis stated that *"The most serious difficulty with the historical regression method has to do with the stability in time of the target-control relationship. This difficulty arose very early in the evaluation of operational cloud seeding projects. MacCready (1952) performed an evaluation of a winter cloud seeding project in central Arizona using the historical regression technique and reported*

indications of a significant increase in rainfall. Brier and Enger (1952) performed several tests of the same project using different controls and different historical periods for establishing the target-control regression line. Their results showed considerable variation in the apparent rainfall increase due to seeding".

2. Brier and Enger (1952) showed that the variation between sequences of years is different from (less than) the variation between random samples of years.

3. Gabriel and Petrondas (1983) examined whether evaluation methods based on operational/historical comparisons, like the historical target/control regression analysis method, is valid for precipitation data, i.e., whether it is robust to departures from the assumptions under which it was derived and does it allow valid inferences, at least approximately, when they are not. The study used annual precipitation data, the distribution of which was not skewed like hourly or daily precipitation tends to be but was still not Gaussian (normal). They concluded that operational/historical comparison methods tend to produce appreciably more significant results than they properly should. This prompted them to state, *"One cannot but wonder how many of the past findings of 'encouraging' results by cloud seeders may have been a consequence of the radical character of statistical tests when applied to precipitation data"*. Gabriel and Petrondas suggested that statements of significance made on the basis of operational/historical comparisons should be discounted; rather they should be augmented by a factor that is proportional to the number of years involved in the operational/historical comparison.

4. Silverman (2007) evaluated the Kings River operational cloud seeding program for seeding effects on annual streamflow using both the historical target/control regression analysis method and the more robust bias-adjusted regression ratio. It is important to note that Silverman made an adjustment to the regression ratio results to compensate for the bias introduced by using data from a non-randomized program in order to enable the ratio statistics method (Gabriel 1999) to yield valid inferences for operational/historical comparisons. As suggested by Gabriel and Petrondas (1983), the computed P-values from the regression ratio results were multiplied by a bias-adjustment factor, the magnitude of which was chosen to achieve confidence interval results with the bias-adjusted regression ratio evaluations that were statistically comparable to those obtained from re-randomization analysis. Silverman found that the historical target/control regression analysis method overestimated the effects of seeding. The estimate by the historical target/control regression analysis was greater than that estimated by the bias-adjusted regression ratio by almost a factor of two after 5 years of seeding; however, the difference in the estimates by the two statistical methods narrowed as the number of operational seeding years increased until they became comparable after about 25 operational seeding years. A similar comparison of the two evaluation methods was done for several other seeding targets in the Sierra watersheds and the results for those targets were consistent with those for the Kings River, i.e., the historical target/control regression analysis method overestimates the seeding effect, especially during the first 25 years of operational seeding. The result for the Mono Creek (MNO) sub-basin of the San Joaquin watershed (Fig. 1) is another example of what was found. It should be noted that the horizontal axis starts after 10 seeded years, the same as that shown for the Kings River, the reason being that it takes about that long before the statistical estimate of a possible seeding effect becomes unequivocally apparent.

Prompted by my doubts about the accuracy and statistical meaning of these evaluation results, I requested copies of the response variable data so I could independently check the results by repeating the evaluations using re-randomization (permutation) analysis. I had honored Griffith's request and provided him with the response variable data that I used in my Vail evaluation study (Silverman 2009) and I asked him to reciprocate by providing me with the response variable data that he used in his Utah report (Griffith *et al.* 2009). Griffith turned down my request. Originally, I intended to check the results by repeating the evaluations using the bias-adjusted regression ratio method. However, Griffith's e-mail response to my data request stated that *"If you were to do your ratio analysis and the results were different than ours, it appears you will believe your results are right and ours are wrong"*. I responded by saying *"I am now using Monte Carlo permutation (re-randomization) statistics in my evaluations as I did in my 2009 JWM paper on the San Joaquin evaluation. That is what I am planning to use in my re-analysis of the Utah programs in order to put the Utah evaluation results on a more robust statistical footing and not to imply that your results are not correct".* Tukey *et al.* (1978) stated that *"Re-randomization analyses can be applied to any (numerical) summary comparison of seeded results with unseeded ones"* and that it (re-randomization analyses) offers the most secure basis for drawing statistical conclusions about the effectiveness of weather modification programs. Re-randomization analysis is a non-parametric method of analysis that is based solely on the response variable data itself. It does not depend on any assumptions about

the distribution shape and its associated properties or about independence of the data from one time to another so robustness is not an issue. Nevertheless, Griffith refused to provide the Utah programs response variable data.

One cannot help but wonder why Griffith refused to provide the Utah data. If Griffith *et al.* believe that the historical target/control regression method yields reliable and accurate estimates of the seeding effect, then one would think that they would welcome an independent evaluation using re-randomization, a statistical method of unquestioned validity, since it would corroborate their results. My request for the data is consistent with the WMA's recommendation (Boe *et al.* 2004) that states *"We (WMA) recommend that evaluation techniques presently being applied to operational programs be independently reviewed and as necessary revised to reduce biases and increase statistical robustness to the extent possible. Recognizing that randomization is not considered to be a viable option for most operational seeding programs, we acknowledge there is much room for improvement in most present evaluations, many of which are presently done in-house".*

The results of the Utah programs will remain in doubt until an evaluation of the Utah target areas is done using a more robust statistical method than historical target/control regression analysis. To assure that the new evaluation is independent and unbiased, it should be carried out in accordance with the recommendation of the WMO Statement on Weather Modification (WMO 2007) which states *"Weather modification managers are encouraged to add scientifically accepted evaluation methodologies to be undertaken by experts independent of the operators".* I thought that Griffith agreed when he stated in his response to my data request that *"I believe an independent statistician should review the application of the standard historical regression techniques versus your and Ruben's double ratio* (regression ratio) *method to determine the reasons for potential differences"*. I suggest that re-randomization (permutation) analysis should be included in this study.

In requesting the data, I assumed that authors of published papers in the *Journal of Weather Modification* (*JWM*), as is the case with most scientific journals, were required as a condition of publication to provide the data they used to obtain their results and conclusions to interested/concerned readers who request it. However, it turns out that I was mistaken about the *JWM*'s publication policy with regard to this matter. I was disappointed to learn that the Weather Modification Association (WMA) encourages but does not require authors of *JWM* papers to provide their data to readers who request it. Since requiring readers to accept results and conclusions on faith is not consistent with the pursuit of scientific understanding, I strongly recommend that the WMA change the JWM publication policy with regard to this matter. The WMA should

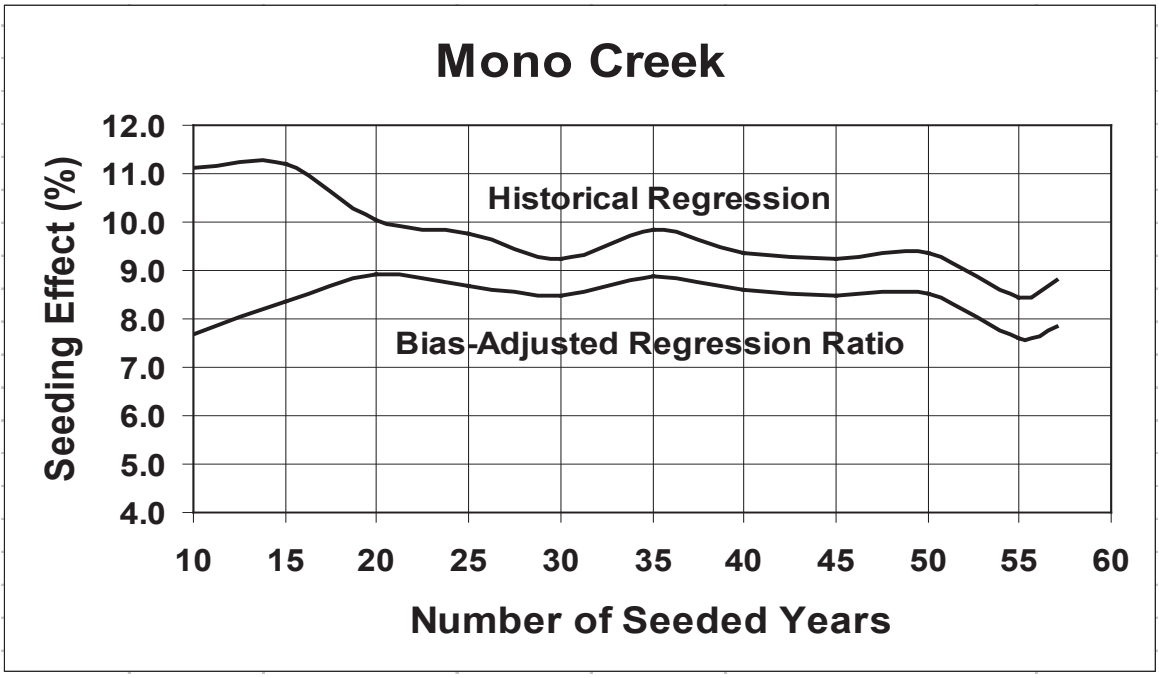

Figure 1. Cumulative year effect of seeding for Mono Creek (MNO) estimated by the historical regression method and the bias-adjusted regression ratio method.

establish well-defined (not optional) guidelines for authors and reviewers that cover all aspects of the processing and publication of manuscripts in the JWM, the aim being to publish in a timely manner high quality contributions to the advancement of the science and practice of weather modification.

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