REPLY TO SILVERMAN COMMENTS ON WMA JOURNAL PAPER ENTITLED "30+ WINTER SEASONS OF OPERATIONAL CLOUD SEEDING IN UTAH", GRIFFITH *ET AL.* 2009

Don A. Griffith, David P. Yorty, and Mark E. Solak

North American Weather Consultants
Sandy, Utah

1. INTRODUCTION

North American Weather Consultants (NAWC) published a peer-reviewed paper in the WMA 2009 *Journal of Weather Modification* entitled "30+ Winter Seasons of Operational Cloud Seeding in Utah", Griffith *et al.* 2009." That overview paper describes several operational winter cloud seeding programs being conducted in Utah (Griffith *et al.* 2009), hereafter referred to as Griffith. The paper included estimations of seeding effects using an historical target/control method to assess the ongoing non-randomized seeding projects. Silverman (2010) has submitted comments to the Editor of the WMA *Journal of Weather Modification* questioning the basis and accuracy of estimates of seeding effectiveness summarized in the Griffith paper.

Silverman has in recent years published four peer-reviewed papers in the WMA *Journal of Weather Modification* focused on evaluations of long-term winter operational cloud seeding programs utilizing a "ratio statistics" methodology (Gabriel 1999). The Silverman paper on the San Joaquin project also employs Monte Carlo permutation (re-randomization) statistics, a method he considers to be of "unquestioned validity." The four Silverman papers and the target areas that were analyzed are:

- Silverman, 2007, Kings River Drainage, Sierra Nevada, California
- Silverman, 2008, Kern River Drainage, Sierra Nevada, California
- Silverman, 2009a, San Joaquin Drainage, Sierra Nevada, California
- Silverman, 2009b, Vail Ski Area and Surrounding Areas, Colorado.

In his comments, Silverman is portraying himself as an unbiased, independent expert, wanting to apply his adaptation of the bias-adjusted regression ratio and re-randomization methods, as used in the four analyses listed above, to the Utah projects.

It needs to be emphasized at the outset that there are basically two types of weather modification programs: research programs and operational programs. One of the primary goals of a research program is to document the efficacy of the treatment with the associated confidence intervals and statistical significance of the indicated results. This is not the primary concern in the conduct of operational programs where the primary goal is to produce more water, reduce hail damage, etc. Operational program designs typically by necessity have less sophisticated (less robust) techniques applied to them to provide <u>estimates</u> of the potential results of the treatment. The Utah programs that Griffith reported on were all operational programs.

2. NAWC RESPONSE

Silverman, in his comment, states: "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."…. "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,

*Corresponding author: Mr. Don Griffith, North American Weather Consultants, Inc., 8180 South Highland Dr., Suite B-2, Sandy, UT 84093 ; e-mail: dgriffith@nawcinc.com

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?"

Silverman basically contends that a more robust statistical technique needs to be applied to the Utah data to provide statistically more significant results. As an aside, Silverman raises an objection to the use of point estimates, yet Smith (Smith 2009) criticized Silverman for doing the same thing in his Kern River paper (Silverman 2008).

As indicated in the Introduction, it needs to be stated that the Utah seeding programs are not randomized experiments. Quoting from Hess (1974), "The weather scientist recognizes the large natural variability of rainfall and cloud characteristics in space and time, and sees the need for appropriate statistical methods to cope with the problems of uncertainties, for, as expressed by F. Mosteller and J.W. Tukey in 1968, "One hallmark of the statistically conscious investigator is his firm belief that however the survey, experiment or observational program actually turned out, it could have turned out somewhat differently." Statistical methods designed to handle these problems were developed by R.A. Fisher (1960) in connection with the design and analysis of comparative experiments in biological and agricultural research, where large and only partly controllable variability is present. The basic ideas involve (1) *replication*, from which a quantitative estimate can be made of the variability of the response to treatment, and (2) *randomization,* a process of allocating treatments to the experimental material by tossing a coin (or equivalent procedure), which may make it possible for the experimenter to attribute whatever effects he observes to the treatment and the treatment only."

A reference that explains the ratio statistics methodology as adapted by Silverman in his various WMA *Journal of Weather Modification* evaluation papers has as its title "Ratio Statistics for **Randomized** (emphasis added) Experiments in Precipitation Stimulation (Gabriel, 1999)! NAWC does not have the Tukey (1978) reference (cited by Silverman) in-house but we suspect that it is directed at the evaluation of randomized programs since in Silverman's comment he applies the term response variable data when discussing this reference. Restating the obvious, the Utah seeding programs are not experimental in nature or design.

Based upon the above, the thought that more robust statistical analyses can be applied to the Utah data sets to derive ranges of effects and their statistical significance is open to some question. Silverman himself has alluded to this problem in the four recent referenced publications regarding his application of the "ratio statistics" to several non-randomized data sets. For example, quoting from Silverman 2007, "From a rigorous statistical standpoint, the suggested effects that are indicated must be confirmed through new, *a priori*, randomized experiments specifically designed to establish their validity." Similar statements are found in Silverman's other three WMA papers. In other words, Silverman goes back to the basic premises for application of statistical tests as summarized by Hess: 1) replication and 2) randomization. From Silverman's San Joaquin paper (Silverman 2009), "It is emphasized that this study is an a posteriori evaluation of a non-randomized seeding operation. In addition, this evaluation is an exploratory study that involves consideration of a multiplicity of hypotheses/analyses, some of which are suggested by the results of previous analyses. In view of these considerations, the results should be interpreted as measures of the strength of the suggested seeding effect and not as measures of **statistical significance** (emphasis added)." In light of this second statement, how can Silverman make the statement about NAWC's paper, "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? When Silverman uses the term basis in this context we infer he is saying what is the **statistical basis**. We interpret statistical basis as valid statistical significance tests applied to randomized data sets.

Based upon Silverman's own statements found in his four publications, he likewise has failed to provide credible statistical evidence that seeding increased the average streamflow in the Kings, Kern and San Joaquin River programs in California and the Vail program in Colorado. The tone of Silverman's papers implies that he has provided "proof" of the efficacy of cloud seeding in several non-randomized program areas that he has analyzed. NAWC disagrees that this is the case; rather, he has provided indications that these programs have been successful. The perception that ratio statistics provides the ultimate statistical analysis tool (even as applied to randomized data sets) is dispelled in Ruben Gabriel's 1999 paper which contains the following statement, "This paper does not argue that ratio statistics are best but presents tools for making correct inferences about them, given that they have been much used and are likely to continue being used." Further, application of Silverman's adaptation of the ratio statistics method has not been reanalyzed and verified by an independent statistics expert, so his call for its use widely, and his attendant request for the Utah data, are premature.

Silverman mentions concerns attributed to Dennis (1980) regarding the historical regression approach. NAWC offers the following comments regarding these concerns.

"Reliability of the results unless the underlying data sets conform to the normal distribution which, for precipitation data, requires an appropriate data transformation." Quoting from Dennis (1980) "Rainfall observations say for one hour or day at a point, tend to be highly skewed, with most observations near zero and a long tail extending to large amounts". NAWC utilizes longer-term data, either three or four-month cumulative values or April 1st snow water contents that are also cumulative values. Further, NAWC deals with averages of multiple sites (not point measurements) for the control and target average values. These data are not highly skewed, as demonstrated in Figure 1, which is a frequency plot of the average control area values for the Central/Southern Utah program for both the not seeded and seeded periods. We have chosen to not apply transformations in our basic estimations. Dennis states that rainfall distributions can be normalized by data transformations, but that caution is necessary in interpreting the results of experiments analyzed with the aid of transformations. Silverman notes that the distribution of the Utah target and control response variables are not highly skewed, but that they do not conform to the normal distribution.

"Unconscious bias in the selection of data in *post-hoc* evaluations." As described in Griffith, NAWC typically establishes target and control stations to be used in its evaluations at project outset or early in the lifetime of its operational programs. These target and control sites and the resultant regression equations are typically maintained without changes throughout the lifetime of the seeding program. Changes are typically made only if observations are discontinued at one or more target or control sites. As a consequence, NAWC evaluations should not be considered strictly *post hoc* evaluations as suggested by Silverman, rather they could be considered essentially *a priori*. Incidentally, most of Silverman's analyses as reported in the WMA *Journal of Weather Modification* would be considered *post hoc* evaluations, the possible exception being the San Joaquin which uses target and control stream gaging stations previously established by Henderson (2003).

"Difficulty in eliminating residual uncertainties." Dennis (1980), in discussing this concern, states, "A number of possible biases are dealt with rather simply. Agreements before a program begins as to which rain gages are to be included in calculating target and control rainfall, for example, go far toward eliminating both unconscious bias and any temptation to select data to demonstrate a desired result (Court 1960). As discussed in the previous bulleted item, NAWC typically follows this recommendation in evaluating its operational programs.

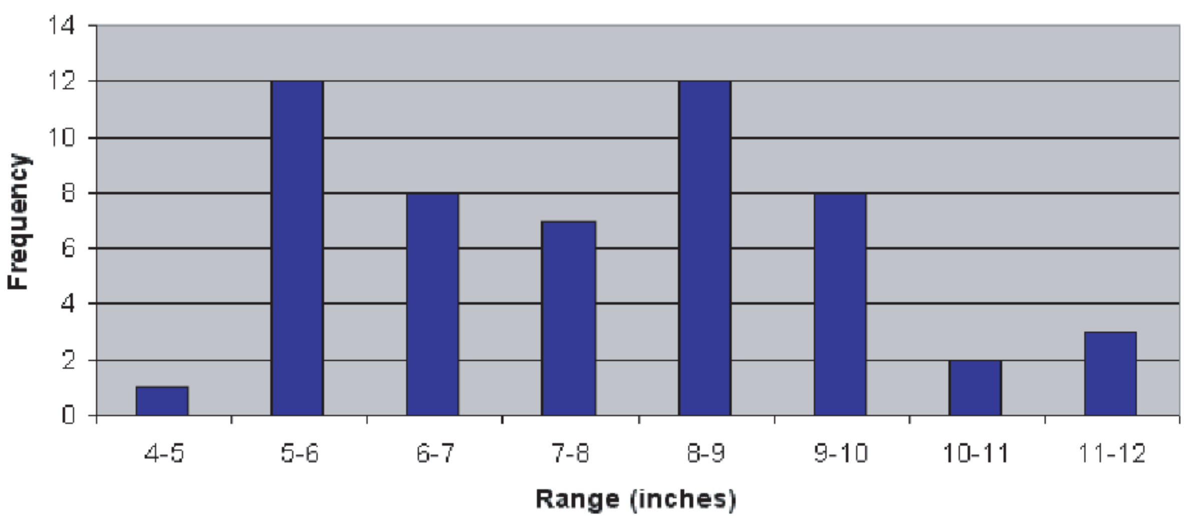

Figure 1. Distribution of Average Seasonal Control Site Precipitation for the Central/Southern Utah Program, Precipitation Evaluation, 1957-2009

"Representativeness of the target-control relationship and its stability in time." Quoting from Dennis (1980) "The most serious difficulty with the historical regression method has to do with the stability in time of the target-control relationship." "Neyman and Scott (1961) have hypothesized that the lack of stability in the target-control relationship is related to the occurrence of specific storm types, some of which favor the target area and some of which favor the control area." ... "The best that can be done appears to follow the criteria noted above for the selection of control areas and to be alert to any obvious changes in weather patterns that could distort the target-control relationship. One must not go to extremes in this regard; obviously, if one looked long enough, one could always find *something* that was different between the historical period and the operational period (Gabriel 1979)." NAWC has generally attempted to address this concern by careful selection of target and control sites (as described in Griffith) as recommended by Dennis (e.g., as close to target areas as possible, in areas typically upwind to avoid contamination, and selecting target and control sites at similar elevations). In fact, we often attempt to bracket the target area geographically (in a crosswind sense) with control sites in order to address the concern of storms favoring one area over another during specific storms or perhaps extending through an entire season. NAWC interprets the discussion contained in Dennis (1980) to be directed at short time intervals, e.g., individual storm periods. NAWC's evaluations utilize seasonal data that would be less subject to this concern since storm tracks change from storm to storm and any large differences between two areas are frequently dampened over longer time periods.

Silverman makes the statement "Silverman (2007) evaluated the Kings River operational cloud seeding program for seeding effects using both the historical target-control regression analysis method and the more robust bias-adjusted regression ratio." Silverman implies that these are different methods. NAWC contends that both methods are based upon the historical target-control analysis method. The bias-adjusted regression ratio method only contains some adjustments to the basic method that are primarily oriented at attempting to establish statistical significance of the results. That brings us back to our original argument, with which Silverman essentially agreed, that, in essence, one couldn't establish rigorous statistical significance from non-randomized programs.

Lacking randomization, any analyses of data can be subject to intentional or unintentional bias. Silverman states that Griffith refused to provide the Utah data sets to him. One of the reasons cited to Silverman for this decision was Silverman's statement in an e-mail that said "Based on the comparative results between my evaluations using the historical regression method in my Kings River evaluation paper (JWM 2007), I fear that the historical regression method may have yielded overly optimistic results for the Utah data." He further states in his comments, "Prompted by my doubts about the accuracy and statistical meaning of these evaluation results, I requested copies of the response variable data…" These comments indicate potential bias. Bias will always be a question when dealing with non-randomized data. Griffith questioned Silverman in this regard in his response to Silverman's e-mail "I have several concerns related to your request. 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 our results are wrong. This may or may not be the case. I believe an independent statistician should review the application of the standard historical regression techniques versus your and Ruben's double ratio method to determine the reasons for potential differences. I found it interesting that you apparently did not compare the results of the two methods in your Vail, San Joaquin and Kern analyses as reported in the WMA *Journal of Weather Modification*." Griffith asked Silverman several other questions in this e-mail none of which were answered. Silverman continued to demand that NAWC provide him with the Utah data. Since Silverman failed to respond to Griffith's questions, NAWC chose not to provide these data to Silverman.

Since the Utah seeding programs are not randomized, NAWC has typically chosen to not state statistical significance levels in our analyses. An exception to that can be found in Griffith *et al.* (1997). A re-randomization statistics method was applied to the longest-standing Utah seeding program. The results of 1,000 random draws indicated that the regression-indicated average seasonal precipitation excess of 14.6% in the target area is significant at better than the 5% level. NAWC more commonly uses the term "estimate(s)" when discussing the results of its seeding effectiveness efforts. Silverman's analyses likewise provide "estimates of effects" which should not be considered as conclusive "proof" of the confidence intervals or the statistical significance of these ranges of the non-randomized programs that he has analyzed. The question then of which "estimate" is best is then seen as a discussion of relative rather than absolute accuracy that an expert in statistics can best ascertain.

3. DISCUSSION OF RELEVANT ISSUES

With the background provided in the above, some of the ramifications of Silverman's comments may be examined. It is concluded from Silverman's reference to his Kings River paper that he apparently believes that the results using the historical regression technique may overstate the results for short periods of say 5-10 years but then seem to converge, giving very similar results to the ratio method once approximately 25 years of evaluation are achieved. This conclusion is based upon Figure 4 from Silverman (2007) reproduced here as Figure 2. Silverman states that, "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." Oddly, Figure 2 from Silverman (2007) only provides information after ten years of seeding, not for five years. **Of significance is the fact that the difference between the two types of evaluations as presented in Figure 2 only varies by an approximate 2% difference at most after ten years and declines to approximately a 1% difference in about fourteen years.** In Silverman's comments on Griffith, he provided a plot similar to Figure 2 but from Silverman's analysis of the San Joaquin cloud seeding program (this figure was not included in Silverman's 2009 paper that discusses this program). Silverman's figure, for the Mono Creek drainage, is reproduced as Figure 3. A couple of observations regarding this figure are as follows. It would appear both the historical regression and the bias-adjusted regression ratio methods "overestimated" the seeding effects in the early years of the San Joaquin program and then merged towards lower values over longer durations. This trend is the opposite of that in Figure 2 on an adjacent program in the Sierra (Kings) in which the apparent "seeding effect" started at lower values but increased over time. The maximum estimated seeding effect difference between the two methods in the early years of the San Joaquin program is approximately 4% declining to approximately 1% after 20 years. Due to the non-randomized nature of the data, NAWC does not endorse the apparent conclusion reached by Silverman that the bias-adjusted **estimate** is correct and the historical regression **estimate** is incorrect in the early years of these programs.

In order to examine this time evolution of seeding effects on some of the Utah data sets, NAWC selected two of the longest term programs referred to as the Northern and Central/Southern Utah programs in Griffith. NAWC used the same technique as Silverman in his Vail paper (Silverman 2009) in plotting the time evolution of the apparent cumulative seeding effect (expressed as a percentage

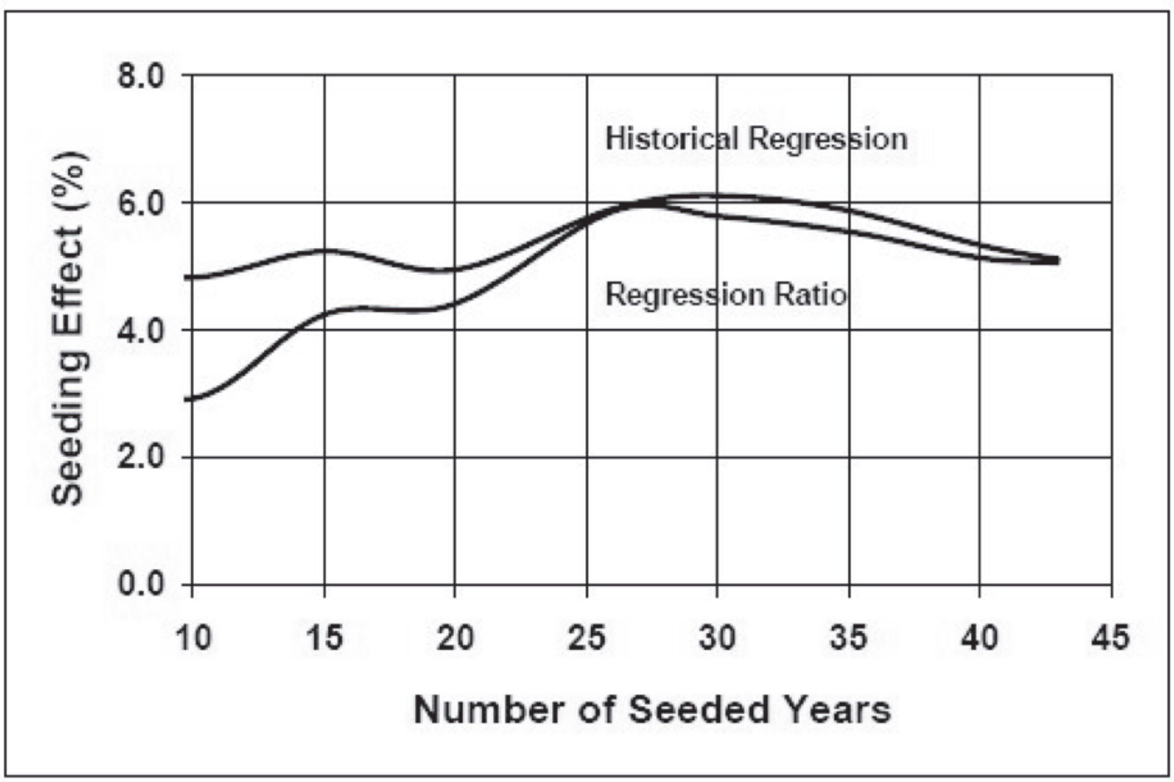

Figure 2. Kings River Program, Cumulative Year Effect of Seeding Estimated by the Historical Regression Method and the Regression Ratio Method (Silverman 2007)

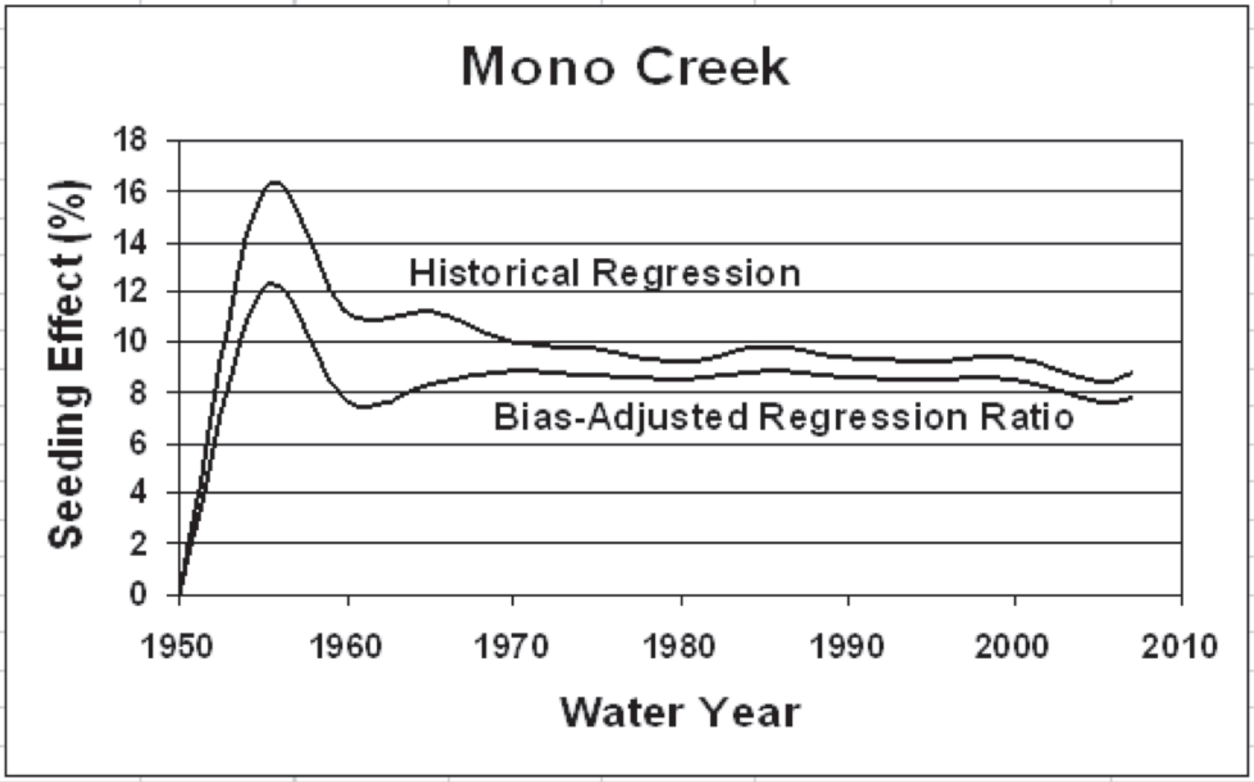

Figure 3. Mono Creek, Cumulative Year Effect of Seeding Estimated by the Historical Regression Method and the Bias-adjusted Regression Ratio Method (Silverman Comment).

increase) for these programs. The results are provided in Figures 4 and 5. Contrary to the supposition that the estimated seeding effects were high in the early years of seeding, these figures indicate that the apparent effects were lower in the early years of the programs but then stabilized at higher levels after 10-15 seasons of seeding. For comparison of the indications provided in Figures 4 and 5 with the Vail program, NAWC prepared a plot for one of the sub-basins in the Vail analysis as reported by Silverman (2009). Several of the target streamflow records used by Silverman in his analyses were rather short records (e.g. 11-14 years). One station did have a longer period of record, the Upper Gore Creek (GUP) station with records from 1948-2005. Silverman indicated the highest correlated control gage was one named the Frying Pan River below Ruedi (FRR), which had available data from 1909-2005. NAWC calculated the linear regression relationship between the two stations for the historical, not seeded period of 1948-1976. NAWC then used the resulting regression equation to calculate the annual indications of possible seeding effects during the seeded years of 1977-2005. NAWC prepared Figure 6 for Upper Gore Creek, which shows the evolution of apparent seeding effects on this sub-basin. This figure actually shows an opposite effect to those found on the two Utah programs (Figures 4 and 5) and on the Kings River program (Figure 2) but the same as that found for Mono Creek on the San Joaquin program (Figure 3). Figure 6 indicates the estimated seeding effects were higher in the early seasons of seeding then stabilized at lower levels after approximately 10 seasons. **Obviously, there is not much consistency in the trends of the indicated seeding effects in the early seasons of these long-term operational programs. The important factor in the comparison (Figures 2 and 3) of the two methods (historical regression and bias-adjusted regression ratio) is that the "indicated results" from the two methods merge with time to the extent that the differences are only about 1% after approximately a 20-year period. Silverman (2007), in discussing the Kings River program, contains the following statement: "Assuming that the relationship derived from the historical period is representative of the operational period, as is the case here, the historical regression method may, indeed, yield reasonably precise estimates of a multi-year effect of seeding provided that the natural variability is averaged over a sufficiently long period of years."**

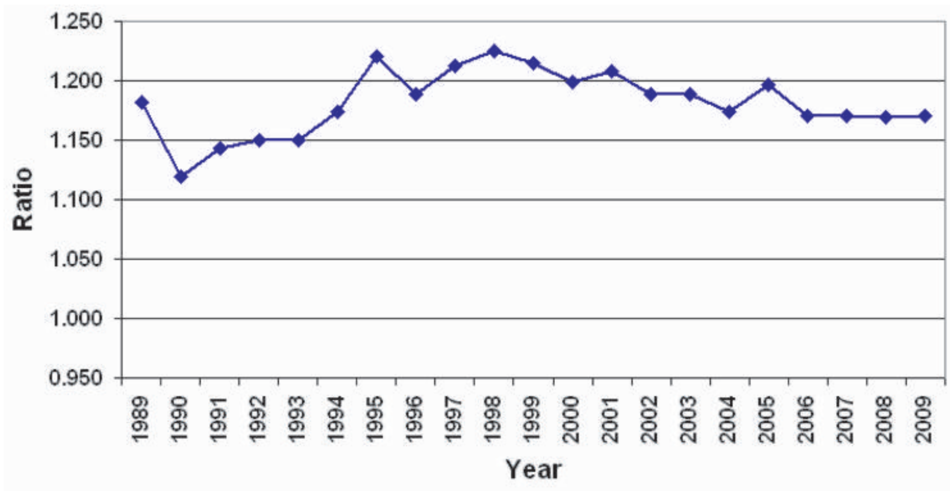

Figure 4. Northern Utah Program, Cumulative Mean Ratio (actual over predicted precipitation) for Seeded Years 1989-2009

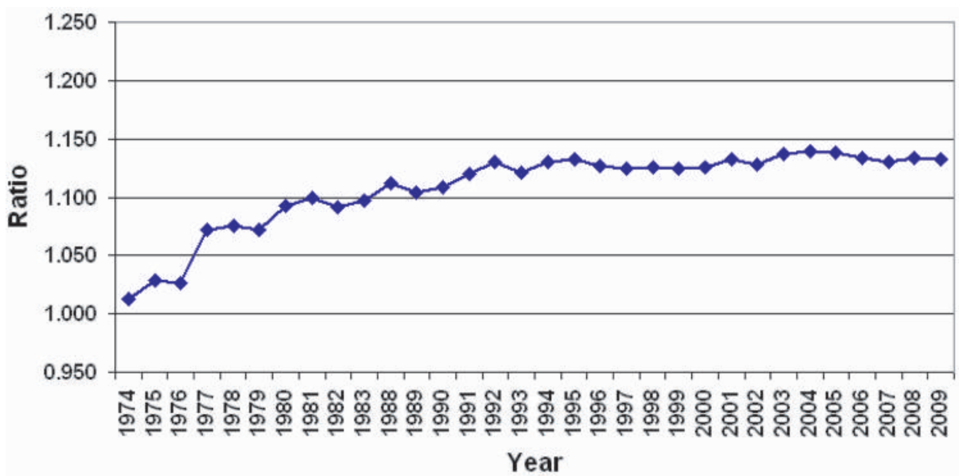

Figure 5. Central/Southern Utah Program, Cumulative Mean Ratio (actual over predicted precipitation) for Seeded Years, 1974-2009

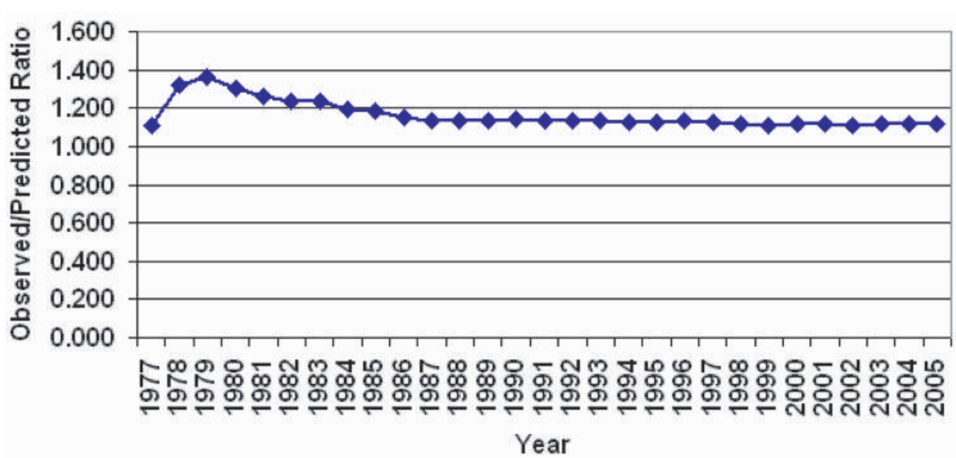

Figure 6. Vail Program, Cumulative Mean Ratio (actual over predicted stream-flow) for Seeded Years 1977-2005, for Gore Creek

Three of the six Utah programs as evaluated by Griffith are of durations from 19-29 years (Table 2 in Griffith). Two of the remaining programs are of 13- and 15-year durations, respectively. As a consequence it is concluded that the historical regression results from 4 of the 6 evaluated Utah programs would differ no more than 1-2% if the bias-adjusted ratio method were applied to these programs. It needs to be stated that NAWC normally indicates to its clients that it will take several seasons (on the order of 10 seasons) before the **estimates** of possible seeding effects begin to stabilize.

Of potential interest in this discussion is another type of NAWC analysis that has been applied to the longer term Northern and Central/Southern Utah seeding programs, an engineering technique commonly called a "double mass" plot. In this technique, two variables can be plotted in a cumulative fashion that will demonstrate how the two variables may be correlated. For the Northern and Central/Southern Utah programs, the average seasonal December through February or December through March values from the historical not seeded and the seeded periods are plotted for the control area averages versus the target area averages. Each successive season's data are added to the accumulated values for the combined prior seasons of data. If the two variables are well correlated, then a straight line can be drawn through the individual points. If there is a change in the relationship between the two variables with time, a "break" in the straight line will appear. Figures 7 and 8 provide that type of plot for the Northern and Central/Southern Utah seeding programs. There are obvious "breaks" upward in both of these plots, which coincide approximately with the beginning of cloud seeding programs in these target areas. Trend lines drawn through the data following these breaks appear as nearly straight lines, which imply that the apparent effects of seeding are rather constant over time. NAWC prepared a similar plot for the Upper Gore Creek site versus the Frying Pan site in the Vail program area (Figure 9). This figure contains a break upward in the plot (more streamflow at the target site compared to the control site) that is also approximately coincident with the beginning of the cloud seeding program in 1977. Interestingly, the plot in this figure suggests variability in the apparent seeding effects. For example, the upward break in the line seems to flatten out during the period of approximately 1983 to 1990. This implies a reduction in the seeding effect during this period for some unknown reason or reasons. No such prominent breaks are evident in the two Utah plots (Figures 7 and 8), which implies more consistent effects of seeding.

Silverman asks for an independent and unbiased analysis of the Utah seeding programs, citing a WMO statement. Since we are discussing WMA publications, we provide the following quotes from the WMA Statement on Standards and Ethics adopted in 2005 under the heading of Relationships with the Meteorological Profession:

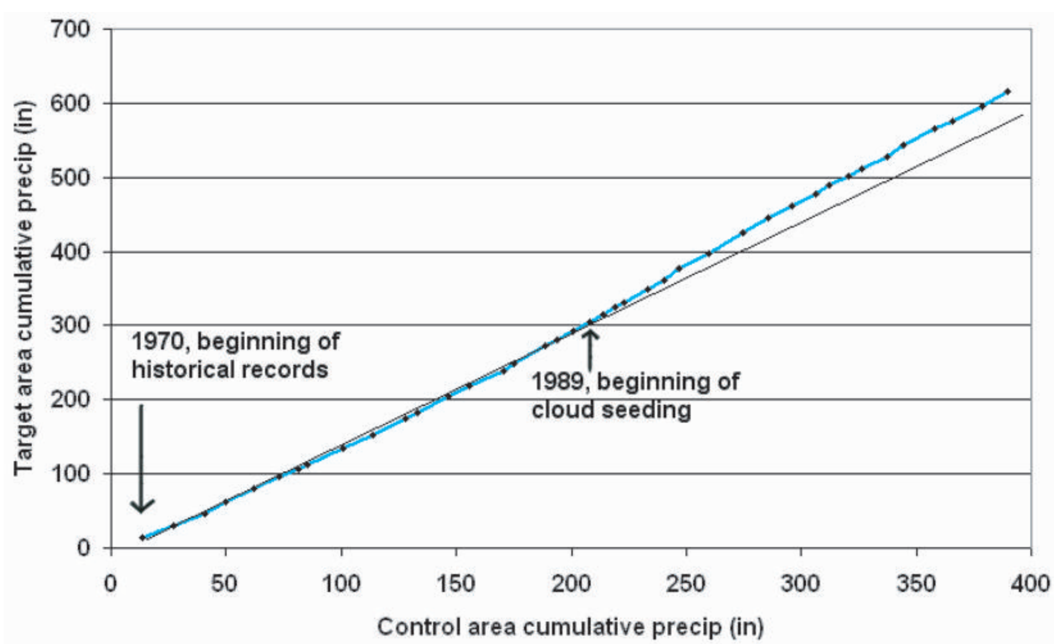

Figure 7. Northern Utah Program, December - February Precipitation, Double-Mass Plot, 1970-2008

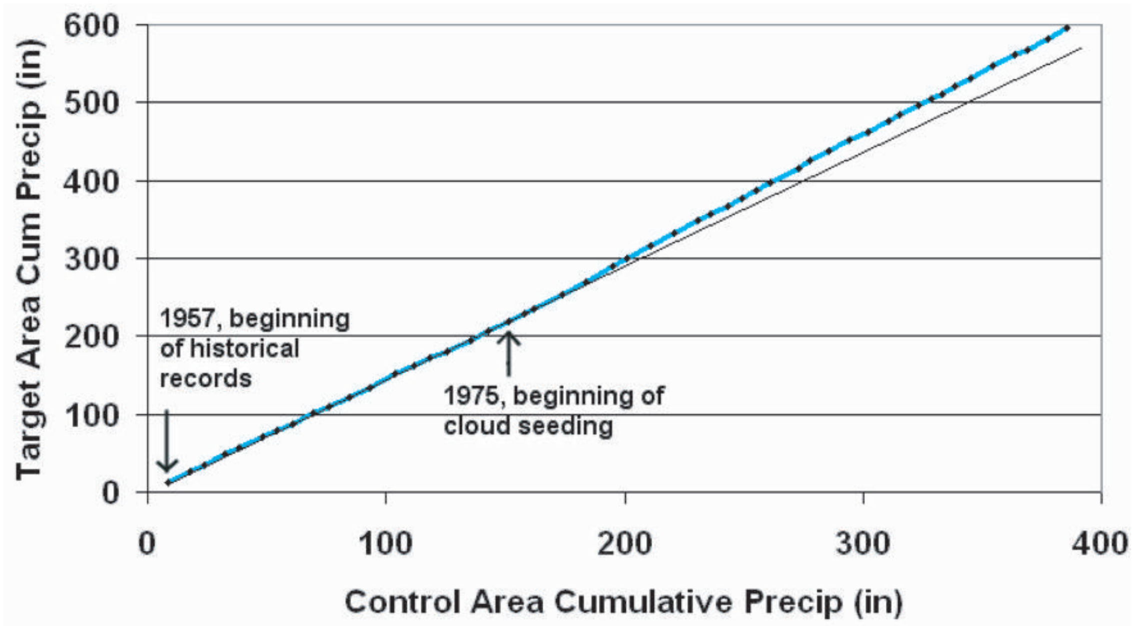

Figure 8. Central/Southern Utah Program, December – March Precipitation, Double Mass Plot, 1957-2008 (excludes water years 1985-1987)

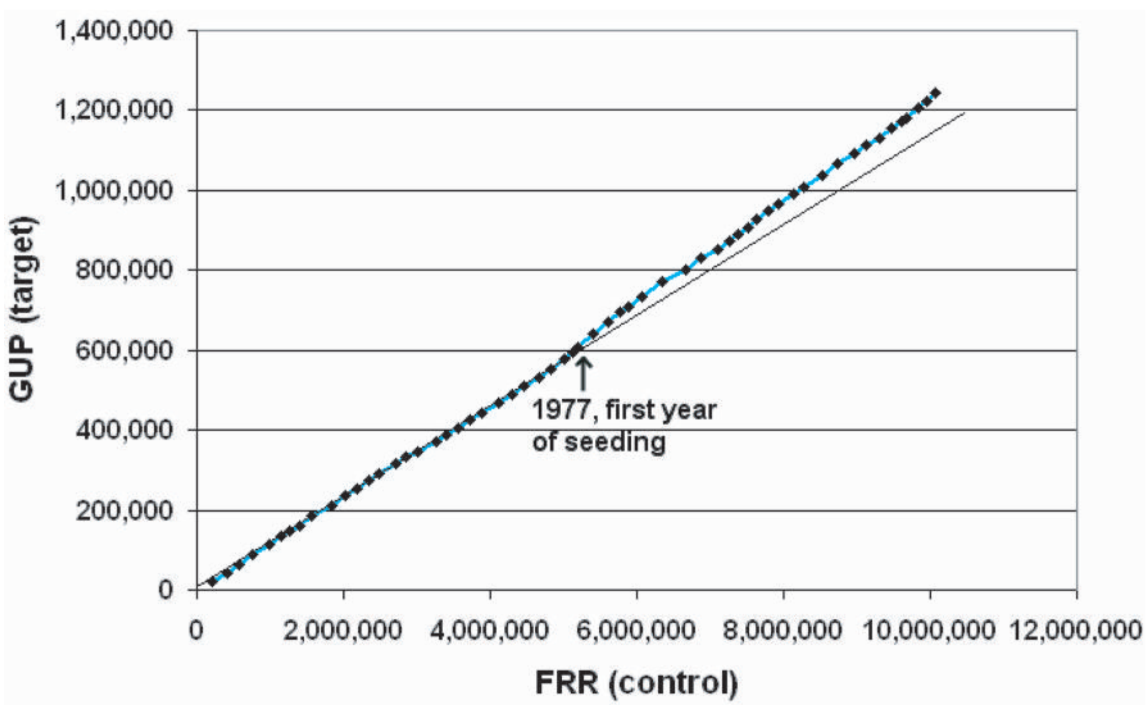

Figure 9. Vail Program, Double Mass Plot of Annual Streamflow, Upper Gore Creek versus Fryingpan River

"The **operator or manager** (emphasis added) will endeavor to contribute new knowledge to the profession by making known significant results from operational and research programs." ... "Evaluations of projects are strongly encouraged. Any limitations to evaluation will be reported to the client. Procedures to be used in evaluations should be specified in advance." Note that the evaluations are to be done by the weather modification operator, not a third party. NAWC routinely follows these requirements in the conduct of its programs. The Griffith overview paper is an example of NAWC following the first requirement and appropriately reporting the results of its evaluation efforts as estimates and indications, not as statistically significant proof of effectiveness. Recognizing the limitations of the evaluation technique applied to non-randomized data, NAWC does not state any measures suggesting statistical significance.

For the record, it perhaps should be stated that "independent" verification of some of the evaluations NAWC's Utah cloud seeding programs have been conducted by the Utah Division of Water Resources. A 2000 report (Stauffer and Williams 2010) when discussing NAWC evaluations states, "The data and analyses in NAWC evaluations have been reviewed and confirmed by the Division of Water Resources. In addition, target and control analyses have also been made for April 1 snow water content. The April 1 snow water content analyses are important because relationships can be developed to estimate runoff based on April 1 snow water content."

4. FINAL COMMENTS

Silverman's analyses of four long-term operational cloud seeding projects have provided some interesting insights into prospective techniques for estimating the effectiveness of winter orographic cloud seeding efforts. However, the four analyses are *a posteriori* and are applied to non-randomized projects, so the analyses and their indicated results carry the same caveats as similar analyses conducted by others over the years. They have not undergone unbiased, independent verification by a qualified (expert) statistician. Accordingly, the results must be viewed with caution and presented appropriately as indications, and certainly not proof, of seeding effects. "Proof" is not possible from operational programs, only indications. To illustrate this point, if Silverman's four papers had been published prior to the publication of the National Research Council 2003 report would there have been changes in any of the conclusions of this report regarding the efficacy of winter cloud seeding based upon Silverman's papers?

NAWC's clients and clients of other firms do not expect the type of "proof" or robust testing that Silverman seeks from these operational programs. The question becomes, whom are we trying to convince in the evaluation of operational programs? Certainly not the scientific community that will reject any evaluations not conducted on a research program with the main tent pole being a randomized design. We are then talking about providing "estimates" of effectiveness to program sponsors that include municipal water managers, irrigation district water managers, hydroelectric facility managers, farming organizations, and state regulators. These managers do not demand the 5% significance level "proof" of effectiveness as is demanded from research programs. These groups are also typically not interested in confidence intervals. Would such managers be concerned if the indicated point estimate from two different evaluation techniques indicated a maximum difference of 2% in the early years of a program but then became nearly the same after 25 years (as was the case in Silverman's analysis of the Kings River program)? Probably not!

One only needs to look at the large number of operational programs being conducted around the world without "robust" evaluations being applied to them as evidence of the above conclusion. This fact seemed to confound those that authored the 2003 NRC report. It almost seemed that the authors were asking: If there is not scientific proof of the efficacy of cloud seeding, why are all these operational programs being conducted?

NAWC believes at least some of the answers to this question regarding winter orographic cloud seeding programs are:

1. The potential for "new" water from precipitation augmentation programs, which may be used to offset the ever-increasing demands being placed on fresh water supplies due to expanding populations.

2. A perceived substantial return on investment. Various studies of U.S. programs indicate additional streamflow derived from winter snow augmentation costs on the order of a few dollars per acre foot to produce, often resulting in estimated benefit to cost ratios of ~5-10/1 or higher.

3. A lower expectation of "proof" that cloud seeding "works". The managers of water districts, municipalities, hydroelectric companies, irrigated agricultural districts, farm groups, etc. often do not have the luxury of demanding a 95% confidence level in making decisions in their day to day world so why should they demand this level of

confidence to fund a cloud seeding program?

Estimations of the effectiveness of non-randomized operational seeding projects are important but challenging. Such efforts must continue and will, no doubt, generate lively debate as they do.

REFERENCES

Court, A., 1960: Journal of the Irrigation and drainage Division. *Proc. Am. Soc. Civ. Eng.,* **86**, No. IRI, 121.

Dennis, A.S., 1980: Weather Modification by Cloud Seeding. Academic Press, New York, NY, 267pp.

Gabriel, K.B., 1999: Ratio statistics for randomized experiments in precipitation stimulation. *J. Appl. Met.*, **38**, 290-301.

Gabriel, K.R., 1979: *Commun. Statist. Theory Methods.* A8, 975-1015.

Griffith, D.A., J.R. Thompson, D.A. Risch and M.E. Solak, 1997: An update on a winter cloud seeding program in Utah. *J. Wea. Mod.*, **29**, pp. 95-99.

Griffith, DA., M.E. Solak and D.P. Yorty, 2009: 30+ winter seasons of operational cloud seeding in Utah. *J. Wea. Mod.* , **41**, 23-37.

Fisher, R.A., 1960: *The Design of Experiments*, 7th Edition. Hafner, New York.

Henderson, T., 2003: New assessment of the economic impacts from six winter snowpack augmentation projects. *J. Wea. Mod.*, **35,** 41-44.

Hess, W.N., 1974: *Weather Modification by Cloud Seeding.* John Wiley & Sons, New York, 842pp.

Neyman, J. and E.L. Scott, 1961: *J. Am. Stat. Assoc.*, **56**, p. 580.

NRC, 2003: *Critical Issues in Weather Modification Research*. Washington, D.C.: National Academy Press.

Mosteller, F. and J.W. Tukey, 1968: *Data Analysis, Including Statistics, The Handbook of Social Psychology,* 2nd Edition, Addison-Wesley, Reading, Mass., Vol.2, 819pp.

Silverman, B.A., 2007: On the use of ratio statistics for the evaluation of operational cloud seeding programs. *J. Wea. Mod.*, **39**,50-60

Silverman, B.A., 2008: A statistical evaluation of the Kern River Operational Cloud Seeding Program. *J. Wea. Mod.* , **40**, 7-16.

Silverman, B.A., 2009a: An evaluation of the San Joaquin Operational Cloud Seeding Program using Monte Carlo permutation statistics. *J. Wea. Mod.* , **41**, 15-22.

Silverman, B.A., 2009b: An independent statistical evaluation of the Vail Operational Cloud Seeding Program. *J. Wea. Mod.* , **41**, 7-14.

Silverman, B.A., 2010: Comments on the Griffith et al. report on operational cloud seeding programs in Utah. *J. Wea. Mod.,* **42**, 133-136.

Smith, P.L., 2009: Comments on Silverman's evaluation of the Kern River Program. *J. Wea. Mod*. , **41**, 161-162.

Stauffer, N.E. and K.S. Williams, 2000: Utah Cloud Seeding Program Increased Runoff Analyses. Utah Department of Natural Resources, Division of Water Resources, Salt Lake City, Utah.

Tukey, J.W., D.R. Brillinger, and L.V. Jones, 1978: The Management of Weather Resources, Vol. II - The Role of Statistics. In Weather Resources Management, Weather Modification Advisory Board, Statistical Task Force, Washington, D.C. , June 30, 1978, US Government Printing Office.