REPLY TO A PAPER ENTITLED "REEXAMINATION OF HISTORICAL REGRESSION ANALYSIS APPLIED TO A RECENT IDAHO CLOUD SEEDING PROJECT"

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1. BACKGROUND

North American Weather Consultants (NAWC) conducted a winter cloud seeding project for the Boise Project Board of Control in the mid-1990's. The project was discontinued when some wetter winters impacted Idaho in the latter 1990's. In discussions with their Board of Directors in 2000 a question arose as to whether increases in streamflow that might be produced by a winter cloud seeding project would be lost to hydro generation at Lucky Peak Dam in the high runoff periods resulting from spring and early summer snow melt. Some board members indicated that the turbine capacities at Lucky Peak could be exceeded in some situations. This would obviously affect the value of the additional water from cloud seeding. We conducted an in-house study to determine if this might in fact be the case. We (NAWC) subsequently published a paper entitled "Economic Feasibility Assessment of Winter Cloud Seeding in the Boise River Drainage, Idaho" in the 2002 in the reviewed section of the edition of the WMA *Journal of Weather Modification.* That paper by Griffith and Solak will herein be referred to as GS. The intent GS was to explore the concerns of these board members and to produce some estimates of the potential economic benefits of the cloud seeding project, based upon estimates of increases in snowpack water content values on April 1st. Although the method used to produce the estimates of seeding effects was described briefly in GS as background material, the intent of that paper was not to present a definitive explanation of the historical target/control analyses that were used to establish the estimates of increases in April 1st water content. More comprehensive discussions on the target/control evaluations were provided in our annual reports to our client, but have not been formally published. The title of GS indicated that it was an economic feasibility assessment. We used the term <u>estimate</u> (or <u>estimates</u>) in discussions of the potential increases in April 1st snow water content. These numbers were never cited as exact, nor could they have been, based upon the type of analyses that were performed in the absence of a randomized data set.

2. CONCERNS

Subsequently, a paper was published by Super and Heimbach entitled "Reexamination of Historical Regression Analysis Applied to a Recent Idaho Cloud Seeding Project" in the reviewed section of the 2003 edition of WMA *Journal of Weather Modification* (Super and Heimbach, 2003). This paper will herein be referred to as SH. In this publication, the GS paper which had been reviewed and published in the 2002 Journal is criticized and characterized by inference as being gravely flawed. The criticisms presented in SH ignore the stated purpose of the original GS paper, which was by title and content a practical analysis of economic and operational project-specific issues. Instead, SH uses the data to disparage not only the GS paper, but in our view essentially any attempts to evaluate non-randomized operational cloud seeding projects.

We were not made aware of the SH paper prior to its publication, nor given the opportunity to respond to it when it appeared in the 2003 Journal. It has been our experience that the opportunity to reply to criticisms is often granted in situations such as these, e.g., in the Bulletin of the American Meteorological Society. The WMA Journal itself has used this approach recently (see the 2002 comment by Bigg on a paper written by Long, and also the subsequent reply by Long). A comment by Super and Heimbach on our original paper, followed by a reply from us in the same volume, from our perspective would have been fairer, providing the reader with all the relevant information and differing perspectives at once.

In the absence of the comment and reply scenario described above, another acceptable approach would have been for Super or Heimbach, or the Editor of the Journal to contact us directly to ensure complete understanding of GS before publishing SH. Some criticisms offered by SH are not entirely without merit, but misrepresent the intentions of GS, which as previously stated, were to provide some sense of the economic feasibility of the program.

To be sure, SH offers some valid advice as to how operational projects might be conducted and evaluated from the authors' viewpoints. From our perspective, it would have made more sense for SH to simply write and publish an original paper stating their positions on how operational projects should be designed, operated and evaluated.

3. AN IMPORTANT CONSIDERATION FOR THE WMA

The WMA Statement on Standards and Ethics was originally adopted in 1978 and recently updated (2003). Under the section *Standards of Conduct for Specific Projects* it states that "Evaluations of projects are strongly encouraged. Any limitations to evaluation will be reported to the client. Procedures to be used in evaluations will be specified in advance." Regarding this issue, NAWC has consistently attempted to perform annual evaluations of our operational projects. We indicate the difficulty in performing these evaluations and limitations in interpreting the results to our clients due to the non-randomized nature of these projects.

It has been NAWC's practice to develop a historical target/control evaluation (usually consisting of the development of a linear regression equation) following the first season of seeding activities. The target and control sites are then normally maintained in future seeded seasons unless any of the following happens: 1) stations are discontinued, 2) some of the control sites are subsequently included in the target area of another cloud seeding project, 3) data quality deteriorates. Should one or more of these situations arise, a revised regression equation is developed, retaining as many of the previous target and control sites as possible while maintaining a high correlation coefficient. The WMA statement says nothing about the importance of third parties performing these evaluations (as advocated in SH). Rather, we believe the implication is that the cloud seeding contractor is expected to perform these evaluations. We feel that it is ironic that we have attempted to comply with the WMA Statement on Standards and Ethics, and then have been criticized for doing so with the tacit approval of the Editor and Editorial Board of the Journal. We direct the reader's attention to the statement in SH regarding evaluations of non-randomized operational projects, in the context of statistical estimations. "The more valid alternative is to present ***no*** (our emphasis added) estimations of seeding effectiveness because such estimates without P-values must call into question ***any*** (again, our emphasis) interpretations of results. It is understood that sponsors expect some evaluation of whether seeding was successful. But they should be made to understand that evaluation of operational projects may provide suggestions but never scientific proof." These are strong statements. It appears to us that Super and Heimbach do not follow their own advice in some of their ensuing statements in their 2003 paper.

This type of critical paper and especially the manner in which it was published without any opportunity for comment by those criticized may well discourage others from publishing in the *Journal of Weather Modification.* This is especially true of papers containing any results derived to estimate the effectiveness of operational projects. In a sense, this is a very important precedent-setting situation since, to our knowledge, no previous papers of this (SH 2003) type have been published in the Journal. This negative outcome could be counter-productive to one of the stated purposes of the Association that the WMA, via its meetings and Journal, will serve as a clearing-house and dissemination agent for weather modification oriented literature and information.

4. ADDITIONAL CONCERNS AND OBSERVATIONS

It is our opinion that some of the conclusions stated in the SH paper cannot be accepted as proof. We submit that, just as it has been suggested by SH that *a posteriori* analyses do not provide an acceptable basis for conclusive statements regarding the effectiveness of cloud seeding, SH does not contain or cite any scientifically conclusive evidence "proving" that cloud seeding is ineffective. Thus, we believe that many of the negative points stated by SH as though conclusive are merely **opinion and conjecture**. Some examples of these negative opinions are as follows:

- Page 31, "When frequent and significant melt occurs prior to the sampling date, the historical regression relationship is no longer valid for evaluation of seeding effectiveness. Melt will be a particular problem when it affects the snow water content differently over target and control areas". This statement assumes that melt "problems" only occur during the seeded seasons. Is it not more reasonable to assume that melt also occurred during the 32 year historical not seeded period and that the regression equation(s) will compensate for this "problem"?

- Page 33, "April 1st SWE observations are generally unsuitable for historical regression analyses of winter orographic cloud seeding effectiveness in the climate of southern Idaho". Certainly this statement needs a qualifier. This is the authors' opinion, not a proven fact.

- Page 35, "Clearly the results of Table 4 and associated P-values provide no basis for considering the Boise Basin cloud seeding project to have been effective in snowfall augmentation up to March 1st. These results are totally at odds with the findings of GS who used April 1st observations". How about the "results" of Tables 5 and 6 in the SH paper which do show positive values? We are unconvinced that SH has proven anything by their "data mining" (16,383 re-randomizations!), based upon their own definition of scientific proof. Their initial position was that it was not possible to scientifically evaluate operational projects due to the lack of randomization. Our understanding of the idea behind re-randomization is to take an indicated result (preferably one obtained in an *a priori* fashion) and then test it through re-randomization to determine its significance. Re-randomization is not intended to find the "best" predictor after the fact.

  We believe that a double standard has been allowed here. Any positive claims about the efficacy of operational cloud seeding can be challenged and subjected to some high standard of statistical proof. It appears to us that any negative comments can be made without adhering to the same high standards of proof expected of the positive statements.

- Page 35, "….an apparently impressive but false suggestion of seeding effectiveness". Again, just an opinion, not a proven fact. (See again our comment above about page 31.)

- Page 36, "Including poorly associated control stations which marginally improve the mean control correlation is a suspect approach". Another opinion, not necessarily fact. What evidence was supplied that supports this claimed "poor association"?

- Page 39, "*….provides no reason to conclude that seeding was effective in snowpack augmentation.*" Italicized for emphasis by SH, this is probably the most troublesome statement in the entire paper. We are surprised that the Editor and reviewers accepted such a strong statement as if it were a proven fact and not just an opinion. In the same section under recommendations SH states "Choose and publish all measurement stations and statistical approaches to be used before data becomes available from the first seeded winter". This is another example of a double standard. Again quoting from the SH document (page 38) "…post project analysis results should be viewed with considerable skepticism.". Given this statement in SH, that paper <u>*should be viewed with considerable skepticism since its analyses were conducted after the fact.*</u>

Again, somewhat ironically, we did specify the target and control sites following the first season of seeding. The ensuing three seasons did provide positive indications (+10.7%). SH argue for this approach, but then proceed to conduct detailed analyses after the fact (*a posteriori*) while discounting the indications from our paper because of a purported "snow melt problem." First of all, the reason our evaluation utilized April 1st water content information was because it is standard practice in the prediction of streamflow runoff in the western United States to utilize April 1st water content values (the National Weather Service uses this approach). Since the goal of our 2002 paper was to investigate the potential impact of the cloud seeding project upon streamflow (at the request of our client), we utilized April 1st snowpack information. This same approach was utilized by Dr. Norman Stauffer of the Utah Division of Water Resources in performing a similar analysis (Stauffer, 2001) to estimate the potential increases in streamflow from winter cloud seeding projects being conducted in Utah (Stauffer, 2001). If we had been asked, Super and Heimbach may have been interested to know that we had performed a concurrent target and control evaluation based upon NRCS SNOTEL December through March <u>precipitation</u> data. The same control sites were maintained once they were selected after the first season of seeding (1992-93). Many of the target sites were at the same locations as the ones used in the snow water content analysis. The precipitation analysis indicated an average 8.5% increase for the four seeded seasons. Since precipitation measurements would be unaffected by any "snow melt" problems, these data support the positive indications (if not the absolute values) obtained with the snow pack evaluation. A break in seeding operations during water years 1998-2001 (water year 1997 was excluded since seeding was conducted over the Payette River drainage by Idaho Power) provided an opportunity to gain some additional insight Using the target and control stations that were established *a priori* and the identical analysis method used to evaluate the seeded seasons, the evaluation trials indicated an average ratio of actual to predicted April 1st snow water content of near unity (1.02) for the four non-seeded winter seasons that immediately followed the seeded seasons. If there really was a snow melt problem with April 1st data, why did the regression analysis of the non-seeded seasons not indicate such an effect? This argues against the concern that global warming or some other unidentified phenomena impact only the seeded seasons of the early to mid 1990's, but not thereafter. Further, seasonal analyses since the operational seeding project was resumed in the 2001-2002 winter season have shown a return to more positive indications (these analyses are discussed in a later section). This result is contrary to the statement in SH that "The important point is that this evidence of increased melt frequency in the last decade strongly suggests that the historical regression relationships were not constant with time" (page 32). Although we have argued that the snow melt issue does not seem to have produced a significant problem in multi-season analyses of the Boise project, we acknowledge that the potential of snow melt influences is an issue worthy of consideration in the evaluations of winter orographic projects, especially if performed on a single season basis.

Another concern with the SH re-examination is the *carte blanche* elimination of the month of March from any assessment of potential seeding effects, again justified only on the basis of a purported snow melt "problem". NAWC in-house analyses of monthly target/control evaluations of a long-term winter project being conducted in the State of Utah have indicated that, statistically speaking, the month of March has the highest indicated effects of seeding of the four months (December through March) that have been consistently seeded. One hypothesis that might explain such an outcome is the likely increase in embedded convection in springtime storms. The presence of embedded convection could increase the amount of supercooled liquid water that is available, plus assist the vertical transport of ground released seeding materials.

5. CLARIFICATION ON CRITIQUES OF OUR WORK

We certainly welcome unbiased and positive criticism of our work, such as one might expect from peer reviewers of technical papers (incidentally, there were two reviewers of our original paper, one with a large number of comments). We gave serious consideration to these constructive comments, made a number of changes to the draft paper and submitted the final version which we felt had benefited substantially from the reviewer's comments. The negative tone taken by SH can only serve to polarize the parties involved, and to heighten the debate about operations versus research level-of-proof issues.

6. DIFFERENT LEVELS OF ACCEPTANCE

In SH, Super and Heimbach are essentially asking for levels of scientific proof expected of research projects, yet GS reported on a non-randomized operational project. This is obviously a no-win situation, since without any randomization the application of statistical tests is always subject to a question of bias. A considerable number of operational projects have been conducted in the past and continue to be conducted without the requirement of the level of scientific proof sought by SH. Sponsors of operational projects are not naive. They are practical decision makers. As our original paper attempted to demonstrate, the potential benefit-to-cost ratios can be significant from properly designed and conducted seeding projects. Our original paper suggests a possible ratio of 10:1 when using the estimate of a 12% increase (considered a conservative estimate since only one half of the calculated increase in streamflow was used in this calculation to account for periods when turbine capacity could be exceeded). If one assumes that SH is closer to estimating the real effect of seeding with its estimate of an average 4% increase in the December through February period, the benefit-to-cost ratio would still be 3.6:1. We remind the reader that our December-March <u>precipitation</u> analyses indicated an average estimate of an 8.5% increase for the four seeded seasons, which would result in a ratio of 7.6:1. These numbers ***do not account*** for any increased hydroelectric power generation from the Anderson Ranch Dam, nor do they include the value of the additional streamflow to irrigated agriculture downstream of Lucky Peak Dam. In other words, the project could be justified from a benefit-to-cost standpoint based solely upon increases in hydropower production from Lucky Peak Dam, based upon what we consider to be conservative estimates of the increases from cloud seeding and power production. **This was the real intent of GS, our original paper, i.e., to demonstrate the potential economic impacts of an operational project** and why sponsors of projects like this one are willing to support such projects without the 5% or better significance levels demanded of research projects. It comes down to practical risk assessment and decision-making. For example, can the sponsors of a potential project accept the risk that there is perhaps a 20% possibility that there will be no effects from a cloud seeding project to potentially multiply their investment by a factor of 3 to 10? In the real world, decisions such as these are made routinely in the affirmative. We would all like a 95% or even better 99% confidence level that each decision we make in life would be correct. However, we almost never have this luxury. Silverman (1978) concluded "Users of weather modification are shrewd business people. They understand that they are, in many cases, taking a gamble when they use weather modification, but it is no greater risk than they take in other aspects of their business."

The opinions expressed above are not just our own. Dr. Roelof Bruintjes of NCAR in a cloud seeding review published in the AMS Bulletin (Bruintjes, 1999) makes the following observations:

"The fact that many operational projects have been going on and have increased in number in the past 10 years indicates the ever-increasing need for additional water resources in many parts of the world, including the United States. It also suggests that the level of proof needed by users, water managers, engineers, and operators for the application of this technology is generally lower than what is expected in the scientific community. The decision of whether to implement or continue an operational project becomes a matter of risk management and raises the question of what constitutes a successful precipitation enhancement project. This question may be answered differently by scientists, water managers, or economists depending on who answers the question. This difference is illustrated by the fact that although scientific cloud seeding experiments have shown mixed results based on the level of proof required by the scientific community; many operational cloud seeding projects are still ongoing. However, it also emphasizes that the potential technology of precipitation enhancement is closely linked to water resources management. It is thus important that the users of this potential technology are integrated into projects at a very early stage in order to establish the requirements and economic viability of any project (Ryan and King, 1997). In addition, the continued need for additional water and the fact that most projects currently ongoing in the United States and the rest of the world are operational projects emphasizes the need for continued and more intensive scientific studies to further develop the scientific basis for this technology".

The dichotomy we see is the desire of the "scientific community" to convert operational projects into research or quasi-research projects and, perhaps on the other side of the fence, for the operational groups to want to adopt new ideas being tested in research projects into operational projects too quickly. We, as a company, have in the past supported and continue to support additional research in this evolving technology. Our company has participated in the conduct of a number of prior research projects. We do not, however, accept the notion that the operational projects that we conduct for our clients have to fit into the research mode or be expected to individually produce unequivocal "scientific proof" of the effectiveness of cloud seeding.

7. A RE-START OF THE BOISE PROJECT

This Boise cloud seeding project was inactive for a five year period (water years 1997-2001) but was reactivated for the 2001-2002 winter season. It has continued for the 2002-03 and 2003-04 winter seasons. NAWC was awarded the contract to re-start this project through a competitive bid process. NAWC has used the same project design used in the conduct of the original four season (1992-96) project that served as the basis of the GS paper. The operational periods for the first two seasons of the re-activated project were November 15, 2001 - April 15, 2002 and November 1, 2002 – April 6, 2003.

Evaluations of the apparent effectiveness of the seeding were provided in our reports to the client for those two winter seasons. The same set of target and control sites used in the earlier snowpack evaluations discussed in GS (except Camas Creek Divide, a target site, which was dropped because the manual snow course observations were discontinued in 2000) were again used to determine if there appeared to be any effects from the seeding. The ratio of the observed to estimated natural April 1st snow water content for the 2001-2002 season was 0.91. This would indicate 9% less snow water content than expected from the regression equation prediction. The target and control sites used in a 1996 analysis of the December – March precipitation (with one station added to the target sites to achieve better representation of the mid-elevations of the target area and one dropped from the control sites due to poor data quality) were used to determine if any effects of cloud seeding were indicated. The resulting observed-to-predicted ratio was 1.07, suggesting a 7% increase in precipitation. This was an unusual winter season with a disproportionately large amount of precipitation occurring in the middle (approximately 3500 – 6500') elevations in central Idaho compared to the higher elevations (>6500'). In fact, we had concerns expressed by several of our seeding generator operators in the mid-elevation zone about the depth of the snowpack they were having to deal with and even questioning whether the cloud seeding should continue. It was theorized that the difference in average elevations between the snowpack control sites (6377') and target sites (7387') may have resulted in an overestimation of snow water contents in the target area for the 2001-2002 winter season. Nonetheless, the results from these two analyses (snow water content and December-March precipitation) were reported to our client.

The evaluation of the 2002-2003 seeding project provided some additional challenges. The Idaho Power company re-started a cloud seeding project on the Payette River drainage during this winter season. The Payette River drainage is the river basin immediately north of the Boise River drainage. Idaho Power had previously conducted a project in this area during the 1996-97 winter season. Unfortunately, four of the seven control sites used in the earlier snow water content evaluations for the Boise River project were located in the Payette River drainage. When the original regression equation was used (with one of the sites dropped from the target group since snow course measurements at this site were discontinued in 2000) the resulting actual over predicted ratio for the 2002-2003 winter season was 1.01, that is no indication of any effects of cloud seeding. This outcome would be expected if the Idaho Power project was successful in increasing snow water content in the Payette River drainage (i.e., more snow water produced through seeding at the four control sites in the drainage would artificially inflate the estimate of the natural amount of snow water in the Boise River drainage). We concluded that the project for the Payette drainage had been successful, so that some of our control sites had been contaminated and therefore we needed to establish a new set of control sites to be used in the 2002-2003 evaluation.

A set of nine sites was judged to be the best alternative set for a new control group, based upon 1) their correlation with the target area, 2) geographic bracketing of the target area, and 3) similarity to the target area in terms of elevation and meteorology. These nine sites were selected and a new linear regression equation developed **before** the equation was used to estimate the amount of April 1st snow water content in the target area. That is, the selections were made mathematically to achieve the best correlations prior to any estimation of the resultant evaluation indications. Adhering to this sequence removes any deliberate bias on our part (for example, this procedure precludes our deliberately selecting a set of control sites that yielded a positive result or tuning the group for a best result).

When the April 1 snow water content at the alternate grouping of sites was averaged for each historical year and compared to the average for the target area snow water content, the two groups were found to be strongly correlated with one another, a correlation coefficient (r) of .966 (compared to 0.978 for the original control group). This means that approximately 93% of the variance is accounted for in the regression equation developed from the historical (non-seeded) period. Somewhat lower correlation for the alternate control group than for the original group is not surprising, since the original group includes many sites in closer proximity to the target area. However, the correlation between the alternate control group and the target group is still very great. The average elevation of the alternate control sites is 6,898 feet MSL, somewhat higher than the 6,377 foot average for the original control group and closer to the target site average (7,387 feet) than the original. The lowest alternate control site is located at 6,240 feet, compared with 5,380 feet in the original control group.

When the new regression equation established through this procedure was used to calculate the natural snow water content for the 2002-2003 winter season, the observed-to-predicted ratio was 1.10 which would suggest a 10% increase in April 1st snow water content. If the indicated effects from the five seeded seasons (1993-1996 and 2001-2002) using the original control sites are combined with the indicated 10% increase for the 2002-2003 season using the alternate control sites, the average difference is +7.5%, with an average estimated increase in April 1st snow water content of 1.68 inches per season.

As was the case in the snow water content analysis, the observed-to-predicted ratio for the December-March precipitation was 0.87 for the 2002-2003 winter season. This again indicated a seeding effect on three of the control sites due to the Idaho Power seeding project in the Payette basin. A similar process was used to establish a new grouping of control sites for a December-March precipitation evaluation. A grouping of eight sites was judged to be the best alternative set for a new control group, based upon 1) their correlation with the target area, 2) geographic bracketing of the target area, and 3) similarity to the target area in terms of elevation and meteorology. These eight sites were selected and a new linear regression equation developed **before** the equation was used to estimate the average amount of December-March precipitation in the target area. Again, site selection prior to this analysis, as indicated in the snow water content analysis, removes any question of deliberate bias on our part. The historical years of 1982-1992, and 1998-2001, were used in the development of the linear regression equation. This period was selected in order to include only those years that data were available from SNOTEL observations (i.e. no estimated data), and excluded the water year of 1997, which was a seeded year in the Payette drainage. The resulting linear regression equation had a correlation coefficient of 0.94 (an r^2 value of .88). This equation was used to predict the amount of December-March precipitation in the target area for the 2002-2003 winter season and then compared to the observed precipitation. The resulting observed-to-predicted ratio was 1.13, which suggests a 13% increase in precipitation. When this information was combined with the prior five seasons of seeding using the original control sites, the average indicated increase was 6% with an average seasonal increase of 1.18".

We felt it was important to document that there were continued indicated increases in both snow water content and precipitation after the project was re-started. The average indicated increases are 7.5% April 1st snow water content and 6% for December-March precipitation. The 7.5% indicated increase in snow water content was used to estimate the average annual benefit from this project in terms of hydropower production only, as was done in the original GS paper. Working under the same assumptions and using the average runoff of the first four seeded years as representative of the six seeded seasons (official USGS data are not yet available for 2002 and 2003), the average annual value would be $584,598. Dividing this amount by the estimated cost of conducting this winter's project ($90,000) would result in an estimated benefit/cost ratio of 6.5:1. This analysis is considered conservative in nature since 1) only one-half of the estimated increases in annual streamflow were used, 2) it does not include any estimate of the value of additional electricity produced from the Bureau of Reclamation Anderson Ranch Dam facilities, and 3) the value of the additional irrigation water downstream of Lucky Peak Dam is not considered in this analysis.

8. SUMMARY

In summary we offer the following points for consideration:

- Ongoing evaluations of the Boise River project continue to indicate positive effects from cloud seeding that average from 6.0-7.5% for six seeded winter seasons.

- During a four winter non-seeded period between an earlier seeding project period of 1993 and 1996 and the restart of the seeding project during the 2001-2002 winter season, the regression equation developed after the first season of seeding (1992-1993) was used to estimate the target area snow water content. The average observed/predicted snow water content April 1st ratio was 1.02. This suggests no effect of seeding as would be expected, and also provides no indication of a "snowmelt" problem claimed in SH to have influenced the four seeded seasons.

- Any evaluations or reviews of non-randomized projects by "independent" parties need to provide the evaluation procedures, equations to be used, etc. **prior** to the beginning of the evaluations (preferably before any seeding is conducted). Otherwise, the intentional or unintentional biases of the "independent" reviewers are either likely to influence the conclusions reached through this "independent" evaluation or there may well be a suspicion of such biases coming into play. Repeated re-analyses using different procedures, control gauges, or time periods are subject to the same criticisms as would be multiple evaluations conducted *a posteriori* by a contractor. Thus, the "conclusive" statements in SH should be considered as opinions, not as unequivocal facts. The 2003 WMA Statement on Standards and Ethics states the following: "Evaluations of projects are strongly encouraged. Any limitations to evaluation should be reported. Procedures to be used in evaluations should be specified in advance."

- The evaluation of operational projects presents challenges first of all because they are not randomized. Less statistically rigorous evaluation techniques (e.g. target vs. control) are therefore necessary if we wish to attempt to evaluate operational projects at all. There always seems to be a segment of the weather modification community that says "your evaluation techniques are not good enough". Our question to this segment is as follows: What is your solution and is your solution an affordable and established technique? We seldom, if ever receive any alternative suggestions to the target vs. control evaluation approach other than randomization. Some of the other challenges of evaluating operational projects relate to the availability of both current and historical data that can be used in the evaluations. Ideally, we would like to find snowpack control sites at the same elevations as the target sites that are well correlated with the target sites. Oftentimes the unavailability of historical data precludes this desirable mix of control and target sites. Ideally, we would like to establish one set of target and control sites that would be used throughout the duration of the seeding project. However, in the real world, stations are retired, data quality declines, or a cloud seeding project may be initiated in an area that contains some of the "control" sites. Should we give up the evaluation attempts at that point, or do we develop new regression equations to address the new reality? We think the latter course of action is the obvious choice. Is it time for the WMA to attempt to develop a list of cost effective and acceptable techniques that may be used to evaluate operational seeding projects? Perhaps so.

- It is our opinion that the Editor and Editorial Board of the WMA need to develop standard procedures to be used in accepting or rejecting highly critical articles submitted for publication in the *Journal of Weather Modification.* For example, comment and reply procedures, and the grounds on which papers submitted to either the reviewed or non-reviewed sections can be rejected, should be established. How are papers that are critical of others' work to be handled in light of the phrase in the recently adopted WMA Statement on Standards and Ethics that says "The operator or manager will not unjustly criticize fellow workers in the profession?"

9. REFERENCES

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