Reply

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ABSTRACT

An introduction section discusses aspects of the basic design of the CRBPP that are criticized by Rangno and Hobbs (RH). Individual sections reply to the following five aspects of our analysis discussed by RH: diffusion of seeding agent, anomalously high nucleus counts, statistical analysis based on 6 h blocks, duration alteration, and the question of multiplicity in the analysis. The concluding section states that the analyses support the concept that cloud-top nucleation dominates the water balance under stable orographic conditions.

1. Introduction

Two of the four separate parts of the five-winter Colorado River Basin Pilot Project (CRBPP) are criticized by Rangno and Hobbs (1980; hereafter referred to as RH): the original design of the experiment before it started and our detailed analysis after it ended. Not discussed were either the extensive and reliable collection of precipitation, wind, nuclei, ice crystals, and other data by Western Scientific Services, Inc., and most importantly, the day-to-day operation of the five-year project, including declaration of 147 experimental days and programming of 33 AgI generators, by meteorologists of EG&G, Inc. As the only such meteorologist on duty during all five years, Rangno had an unequaled opportunity to see all the difficulties of the project. Rangno (1979, pp. 499–609) has extended to the CRBPP his comments about its predecessor, the Wolf Creek Pass Experiment. Part of this reply is directed to those comments.

Criticism by RH of the basic design of the CRBPP is discussed separately by Professor Lewis Grant, principal author of both the preliminary design document (Grant *et al.*, 1969)[2] and of the final design report (Grant *et al.*, 1974)[3]—published less than a year before the five-year project ended. Actually,

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[2] Grant, L. O., C. F. Chappell, L. W. Crow, P. W. Mielke, Jr., J. L. Rasmussen, W. E. Shobe, H. Stockwell, R. A. Wykstra, 1969: An operational adaptation program of weather modification for the Colorado River Basin. Interim Report for Bureau of Reclamation, Contract 14-06-D-6467. Dept. Atmos. Sci., Colorado State University.

[3] Grant, L. O., C. F. Chappell, L. W. Crow, J. M. Fritsch, P. W. Mielke, Jr., 1974: Weather modification. A pilot project. Final Report, Colorado State University to Bureau of Reclamation (DAWRM), Contract 13-07-D-6467 [NTIS PB237 085661].

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TABLE 1. Evolution of experimental day criteria, and variations in decision time deadlines, in the Colorado River Basin Pilot Project.

	Year				
	1970–71	1971–72	1972–73	1973–74	1974–75
Precipitation 0.01 inch forecast					
Anywhere in target	×				
KH 1 Lemon Park (1-W)		×	×	×	×
JK 1 Northfork Sand Creek (1-E)			×	×	×
KO 3 Wolf Creek West (2-N)		×	×	×	×
MO 1/2 Nipple Mountain (2-S)			×	×	×
NP 2/3 Castle Creek (2-S)		×	×	×	×
Temperature at top of highest cloud		−23[b]			
Temperature at top of precipitating cloud		−23[b]	−8 to −23		
Temperature at 500 mb	−23[a]	[c]	[c]	−23	−23
Duration of temperatures within limits	12 h	3 h	3 h	3 h	3 h
Wind direction limits (deg)	150–300	170–300	170–300	170–300	170–300
Level or layer of wind limits	700 mb	SFC to 700	700 mb[d]	700 mb[d]	700 mb[d]
Decision time deadline (MST)	0910	0400	0400	0400	2300
Randomization decision source	envelope	computer	computer	computer	computer
Experimental days: treated/control	17/17	11/14	15/12	16/17	12/16

[a] −26 changed to −23 at cloud top (note c) on 17 April 1971.

[b] Until "late November", criterion same as previous spring, then highest cloud until 21 January 1972, thereafter "top of precipitating mass."

[c] "Cloud-top temperature or in the absence of this datum, 500 mb temperature." But the average cloud temperature over the San Juan is considered to be at 475 mb. (A temperature of −26°C at 475 mb would correspond to −23°C at 500 mb assuming a wet adiabatic lapse rate.)

[d] On 22 December 1971, changed from mean wind for layer to "representative wind, usually 700 mb."

the CRBPP was conducted primarily according to the Bureau of Reclamation's "Operational Design Aspects" pamphlets issued in October 1970 and October 1971, and oral and written directives from the contract monitor.

During the first three years of the CRBPP, the "cloud-top" temperature criterion was discussed frequently and revised repeatedly. Details appear in Table 1, taken from Table 1-3 of the CRBPP "Comprehensive Evaluation Report" (henceforth referred to as CER[4]) from which ESCH (Elliott, Shaffer, Court and Hannaford, 1978) was extracted.

Ten years ago, when the CRBPP was planned, the apparent relation between cloud-top temperature and seeding success, developed from the two Climax experiments, was incorporated into the design: the 1979 "Operational Design Aspects" prohibited seeding "when the cloud-top temperature is colder than −25°C." Thus, the CRBPP was the first randomized experiment to specify in advance a temperature "window."

The preliminary and final design reports and the "Operational Design Aspects" pamphlets of the CRBPP generally indicated the project's purpose was to determine whether precipitation could be increased at moderate cost, without undesirable side effects (such as increased avalanche threat). They also offered some loose criteria for declaration of experimental days, partly in terms of temperatures at cloud top or at 500 mb (CER, pp. 1–10, 1–11). But no guidance whatever on evaluation methods was offered in these documents, or in the specific contract. Hence the comprehensive evaluation used methods found to be most effective in preliminary studies (as documented in annual reports).

Our detailed evaluation of the CRBPP showed clearly the essential independence of cloud-top temperature from that at 500 mb, as emphasized in Rangno's (1979) Fig. 19. All our detailed analyses, therefore, used the Lifted Cloud-Top Temperature (LCTT) as extrapolated from the Durango radiosonde data by an advective-orographic model. No useful purpose would be served by presenting findings, as RH suggest, "based on the unadjusted sounding data." Rangno and the other meteorologists felt constrained to rely on the unadjusted Durango sounding, which may explain the CRBPP's major deficiency: seeding of clouds whose tops were much colder than −25°C by the time they rose 2 km over the San Juan Mountains.

Apart from the question of CRBPP design, largely relating to the 500 mb and cloud-top temperature difference, five aspects of our analysis (ESCH) are discussed by RH: diffusion of seeding agent, anomalously high nucleus counts on control days, statistical analyses of hourly days grouped into 6 h blocks, whether precipitation duration was affected by treatment, and possibilities for multiplicity in the analysis.

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[4] Elliott, R. D., R. W. Shaffer, A. Court and J. F. Hannaford, 1976: "CER": Comprehensive evaluation report, five winter seasons, 1970–71, 1974–75, Colorado River Basin pilot project. Rep. No. 76-1, Aerometric Research Inc., to Bureau of Reclamation (DAWRM), Contract 14-06-D-7332 [NTIS PB 262057-AS].

The comments by RH on these aspects of our analysis are replied to individually in the following sections. They do not object to the major conclusion that precipitation amounts on the 71 treated days did not differ significantly from those on the 76 control days, either at 18 to 26 recording gages in the 300 km^2 target or at 59 recording gages around it, chiefly to the west.

2. Diffusion of seeding agents

Diffusion of the nucleating AgI vapor upward under *upslope stable* conditions is questioned by RH, but they also seem to doubt that under *blocking* or *pooling* conditions the nucleant was transported westward to the Animas River or beyond, then carried eastward aloft perhaps a day later.

''Diffusion'' of nucleant in an orographic setting involves both transport and dispersion. Stable air approaching a barrier is deflected partially around the barrier, partially upward over it. The extent to which the local flow moves upslope depends on the ratio of the energy of the basic air flow (normal to the obstacle) to the air mass stability. Even stable air can be literally ''driven'' upslope.

The CRBPP had many days of severe flow deflection. Air carrying nucleant moved west then northwest, north and finally northeast. The depth and speed of this flow was clearly manifest by the network of telemetered anemometers at various elevations across the barrier. The Durango rawinsonde also detected at least partial deflection in these cases.

In the deflected cases a lower stable layer sometimes was capped by an inversion. Few generators, if any, would provide nuclei directly to the target area under these conditions. Data from the nuclei counter network confirmed that nuclei were flowing west and northwestward around the end of the barrier, extending even into Utah under these stable blocking flow conditions. This flow was slow, and often the trapped nuclei appeared to form a stagnant pool (hence the term ''pooling mode'' in Fig. 6 of ESCH).

This category was excluded entirely from the 6 h precipitation block analysis: ''When all generators being operated were below the top of the blocked layer, the period was specified as not seeded in the 6 h block analysis'' (ESCH, pp. 1307–1308). Sometimes the nuclei became entrained by an advancing frontal system and returned the next day over the target area, contaminating a no-seed experimental day. In these cases we considered as seeded the early 6 h blocks of the second experimental day. In establishing the roster of pooling mode cases we did not consider precipitation patterns, only the numerous other physical observations. Under the pooling mode, some nucleant can be dispersed into an inversion when the wind shear is great (ESCH, Fig. 6), but this was not considered to be effective in seeding the target.

Our stable category, on the other hand, includes the slightly stable to neutral cases in which the energy of the basic wind flow prevails over the stability. In this *stable upslope mode* the air blows up the barrier at all levels, as verified by the anemometer network and the Durango soundings. This class also includes about an equal number of cases in which soundings showed a positive thermodynamic area less than 75 mb thick, taken as the boundary between shallow and deep instability. The design prohibited seeding when deep instability existed.

In the stable and neutral cases, the plume centerline, which is at ground level, would often ascend to as high as the $-10°\mathrm{C}$ level; this, along with the additional vertical dispersion estimated through use of the Pasquill-Gifford neutral plume type (Turner, 1973)[5], would provide an adequate supply of nuclei for treating the cloud. Our reference to field and laboratory tests by Cermak *et al.* (1970)[6] was to suggest that dispersion in rugged mountainous terrain can be more rapid than indicated by the Pasquill-Gifford stability categories alone. This concept has been substantiated in field studies elsewhere.

Subdivision of the stable category into stable or neutral cases and those that are unstable (up to 75 mb depth) showed similar results as far as seeding effects are concerned. The Wolf Creek Pass group showed a treat-to-control precipitation ratio (for warm top clouds) of 2.43 for the slightly stable to neutral subcategory, and 3.18 for the more unstable subcategory.

Diffusion conditions on 19 February 1974, listed in Appendix D1 of CER, are discussed at length by RH. The detailed classification was stable pooling at low-level, stable above, with some generators above the pooling layer. Clearly, nuclei released in the low-level pool would not be expected to rise above the pooling layer, which seems verified by Hobbs' flight observations of no AgI nuclei. A semantic confusion has arisen in Section 3 of RH. They have referred to our classification of this case as *stable warm-topped* and identified it with our upslope stable conditions where warm top clouds (frequently called just stable warm top clouds) showed large treat-to-control ratios. The detailed classification of 19 February was a subclassification of the pooling condition and the case was therefore excluded. It did not fall into the stable upslope condition at all.

We said (ESCH, p. 1308, Col. 2): ''Aircraft measurements around the San Juan Mountains by the Universities of Washington (Hobbs *et al.*, 1975)[7]

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[5] Turner, D. D., 1973: *Workbook of Atmospheric Dispersion Estimates.* U.S. Public Health Service, HEW, No. 995-Ap-2-6.

[6] Cermak, J. E., L. O. Grant and M. N. Orgill, 1970: Laboratory simulation of atmospheric motion and dispersion over complex terrain as related to cloud seeding operations. *Preprints Second Nat. Conf. Weather Modification*, Santa Barbara, Amer. Meteor. Soc., 59–65.

[7] Hobbs, P. V., L. F. Radke, J. R. Fleming and D. G. Atkinson, 1975: Airborne ice nucleus and cloud micro-structure measure-

and Wyoming (Marwitz, 1976)[8] substantiated the major role of stability in dispersal of artificial nuclei." In CER we quoted the major findings by Hobbs *et al.* We see no major disagreement between us on the role of stability in dispersal of artificial nuclei.

3. Anomalously high nucleus counts

Anomalous nucleus counts at Piedra on a control day, which we considered evidence of contamination from the previous seeded day, RH feel may be 1) a random high count, 2) from blowing dust or 3) from a commercial seeding operation. Had this been the only such evidence of contamination from the previous seeded days, our conclusion might have been different. The overwhelming evidence time after time of westward drift of nuclei in the pooling mode and the detection of nuclei many hours after generator turnoff was the basis for the conclusion. On one such instant, Hobbs *et al.* (1975)[7] found a high nucleus count 18 h after generator turn-off. Contamination was attributed to external sources in those few cases where mesoscale analysis suggested probable transport from far away projects in New Mexico and Utah.

4. Statistical analysis based on six-hour blocks

Statistical analysis of any *a posteriori* stratification, such as in our diagnostic evaluation, are suspect, RH say, unless the investigator can "demonstrate that the stratification is unbiased". Our entire *a posteriori* analysis was an attempt to remove biases produced by mixing, within one experimental day, 1) treated clouds that were seedable under the rules, 2) treated clouds that were not seedable under the rules, and 3) clouds that had been contaminated from previous treatment under disallowed conditions.

To remove sources of bias we 1) broke the experimental day down into 3 h blocks fitting the sounding data, determining their acceptability as seeding candidates under the guidelines and their potential for contamination, 2) combined the original 3 h blocks into 6 h blocks to reduce serial correlation between units, and 3) partitioned the 6 h blocks into various physically meaningful categories, using sounding-derived parameters.

Multiple regression equations relating 6 h block precipitation to sounding-derived parameters explained about half of the variance. These were employed to test whether the weather patterns were biased in favor of or against a seeding effect due to the manner in which the seeded and not seeded cases fell into the various stratifications. No substantial and consistent biases were found. For example, a strong wind component normal to the barrier (southwest wind), a large saturated mixing ratio at cloud base, a large cloud depth and a cold cloud-top temperature all favor heavier precipitation.

More seeded than control cases had strong southwesterly winds, but more control than seeded cases had large saturated mixing ratios at cloud base. Other less significant biases also counterbalanced each other. Precipitation efficiency (ESCH Fig. 10) provides a physically meaningful device for correcting these biases. Use of 6 h rather than 3 h blocks reduced serial correlation between blocks, but also greatly reduced the sample sizes and thus the statistical significance of the results.

The western region could *not* serve as a valid control area by reason of its being contaminated. Return of stable pool mode air by advancing fronts was partially responsible for this, but in addition, blocking flow carried nucleant laden air to this area on some occasions when the air mass was even neutral or unstable. This area was treated as an extra-area effects study area in CER (p. 5-5).

RH express concern that elimination of contaminated days, or their conversion to treat days, might bias the comparisons since these are likely to be associated with large-scale troughs. Their concern is quite correct. However, if this indeed occurred, then it would have shown up in the sounding parameter comparisons discussed above. Four contaminated cases (out of 34 treat cases) were converted to seed cases on the basis of the various non-precipitation physical observations. All of these had southwesterly wind components < 10 m s^{-1}, the upper quartile limit of the seeded cases, and, therefore, do not seem to provide reason for concern on these grounds.

A sizable fraction of the 6 h blocks fell into the forbidden deep instability cloud category. These were not excluded from analysis; an attempt was made to determine seeding effects, which appeared to be adverse. Various partitions failed to show good support for any conclusions. More recent work suggests there may have been a mixture of negative and positive responses to seeding, depending on the degree to which convection remains embedded within or emerges from the orographic deck. In any event, it can be inferred that the cloud-top temperature (and implied cloud-top nucleation) did not play an important role in these clouds.

Another semantic confusion appears in Section 2 of RH where it is stated: "Curiously, many 6 h blocks excluded from analysis by E on the basis of being convective had cloud top temperatures > -26°C

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ments in natural and artificially seeded situations over the San Juan Mountains in Colorado. Report X, Cloud Physics Group, Atmos. Sci. Dept., University of Washington, to Bureau of Reclamation (DAWRM), Contract 14-06-D-6999.

[8] Marwitz, J. D., W. A. Cooper, 1976: Structure and seedability of San Juan storms. Rep. No. AS118 to Bureau of Reclamation (DAWRM), Contract 14-06-D-6801.

(even after subjected to E's model lifting); this contradicts the design limits cited by E." There is no contradiction; the design excluded 1) cold-top clouds and 2) deep convection.

5. Duration alteration

Duration changes in precipitation cannot be studied in an experiment where 24 h randomization is applied to storms which may last several days. Thus, we do not understand the surprise of RH over the lack of any investigation of whether the seeding acted "to increase the duration of precipitation rather than its intensity". We did, however, find generally "greater precipitation intensities (mm h^{-1}) on control days. Greatest hourly precipitation at the various target gages occurred 57 times on control (or suspect) days and only 14 times on treated days. Outside the target, where relatively few gages had records sufficiently complete for analysis, heaviest precipitation came on control days 16 times, on treated days 9 times" (CER, p 2–24). Any duration extension probably results from precipitation starting earlier with seeding. The CRBPP did not begin until precipitation at key stations was already under way, so any such effect would not show in the 6 h block analysis.

In his recent paper, Rangno (1979, p. 599) remarks that we "did not address the question of whether increases in precipitation were effected during the Pilot Project under the shifted day criterion under which the increases had been reported" in the previous Wolf Creek Pass Experiment. In that experiment, precipitation during the 24 h ending at 1000 MST was compared to the 500 mb temperature interpolated from raobs at either 1700 or 0500 LST, according to whether more precipitation fell in the first 12 h (centered on 1700) or the second (Rangno, 1979, p 581). With actual radiosonde data available just upwind at Durango, in 3 h intervals, such devious analysis seemed pointless, and we see no merit in the results given in Rangno's Fig. 18; is it based on interpolated data, or those from Durango? It corresponds very closely to Fig. 3 in ESCH.

6. Multiplicity in the analysis

Multiplicity problems are suggested by RH in their final paragraph of Section 4c. The possibility of finding a surprisingly large effect with strong statistical support merely by trying a large number of possible variables and stratifications has been of concern in the reanalyses of past projects (e.g., Whitetop), and in the Skagit River reanalysis cited by RH (Hobbs and Rangno, 1978). It is also of concern here, not only for the partitioning and stratifications we used, but also for the additional observations in the RH comments. The term "multiplicity" with Machiavellian connotations, has become popular since its use by Tukey (1978) in Volume II of the Weather Modification Advisory Board Report.

In their Section 2, RH note the importance of advance specification of cloud types, which is, of course, the standard design approach to avoiding multiplicity from any *a posteriori* decisions. The 24 h blocks of the CRBPP contained numerous cloud types which had to be isolated *a posteriori*. This was done objectively through sounding-derived parameters without reference to precipitation. The RH complaint (Section 2) about a "wide range of interpretation being possible in actual conduct and *a posteriori* analysis" clearly does not apply.

This leaves for consideration the question of choice in stratification boundaries. Initially, the division between cold and warm cloud-top temperatures was set at −26°C, as measured by lifting the sounding-derived cloud top above the crest to attain the Lifted Cloud-Top Temperature (LCTT). If the 500 mb temperature was indeed representative of cloud top, then a temperature division at −23°C, when lifted over the crest would, on the average, fall to −26°C. Later it became clear that the data did not support a good relationship between sounding-derived cloud-top temperature and sounding-measured 500 mb temperature, and that a colder boundary was desirable.

During the analysis the treated and not treated precipitation were compared for a spectrum of LCTT grouped by octiles. The range was from −9 to −46°C. The octiles of interest were two, with limits at −25.4, −27.6 and −29.8°C. The boundary chosen was at −29°C, and was to some extent influenced by looking at data in these two octiles. Since the treat-to-control ratio changes little in this whole range the choice was not crucial; however, to be conservative one could demand that the probabilities cited be multiplied by 2 since an approximate choice between two limits was available. Thus, warm top stable cloud probability at Wolf Creek Pass group (see Table 3 of ESCH) would be 0.004, not 0.002.

The choice of a boundary between stable and unstable (deep) conditions was initially expected, on physical grounds, to reasonably lie between 50 and 100 mb depth of positive area, easily measured by sounding plots on the thermodynamic chart. A later quartile analysis, showing a range from 0 to 410 mb, had limits of interest at 30, 75 and 130 mb. Conservatively, we might again multiply the p value by 2, ending up with 0.008 at Wolf Creek Pass.

7. Conclusions

Over 700 rawinsondes, and thousands of precipitation records, ice nucleus counts and surface anemometer measurements were employed to make a detailed *a posteriori* analysis on combined physical and statistical ground of what the true effect of seeding may have been on this important project. The resulting information should prove of value in better designing and conducting future orographic research projects. Unfortunately, the cloud physics flights (to which RH often refer) were added too late to add much to this information base. Their input, while interesting, is almost anecdotal in comparison with the mass of other numerical data collected and analyzed on the CRBPP.

The analyses support the concept that cloud-top nucleation dominates the water balance under the stable orographic conditions. When the LCTT was −29°C or colder, overseeding occurred. These few instances were heavy producers with large seeding effects, and their inclusion was partially responsible for spoiling the 24 h experimental units. Also responsible was the inclusion of contaminated cases, and cases with deep convection.

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