of less than two standard deviations from the linear predictor. A correlation coefficient of 0.90 is found between the average hectares hit per farm and the average farm size for the historical period.

Extrapolation of such a linear predictor is always considered risky, but here, the extrapolated relationship for the Primary area provides a fair fit to the historical years in the Badplaas valley, where the average farm sizes are much larger (cf. Fig. 2). This surprising result suggests that the empirical relationship observed here might be found in other areas. There is no evidence in Fig. 4 that the average hectares hit per farm has increased during the Lear seed years.

Intuitively, the relationship makes sense. Visualize a fixed operational area surrounding a protected area of crops susceptable to hail damage. A storm occurring within the operational area has a certain probability of striking the protected crop. As the number of farms increase within the operational area this probability increases. As the size of these farms increase, the probability of greater areal damage also increases. Fig. 4 in this reply suggests that for a registered crop of fixed size, risk is minimized by splitting the crop into many small farms scattered about the operational area. This sounds very much like the military tactic of deployment (never put all your eggs in one basket).

One or two additional comments and I will end this lengthy reply to a lengthy comment. I do not understand Summers' statement "the seeding hypothesis cannot be verified by examining changes in the severity ratio alone, without at the same time, considering the concurrent changes in both the numerator and denominator." Fully one quarter of the paper under discussion is devoted to just this consideration.

Since my paper does not address the economic implications of the hail suppression program, I am at a loss to understand why this issue is raised in the comments.

The comparison of the two tables (Summers' Table 2 and my Table 1) really only serves to underline the futility of this type of analysis. It certainly renders meaningless any conclusions reached in the discussions section of the comments. I think that it is important to realize that it is unlikely that definitive and final conclusions on the efficacy of the Nelspruit hail suppression techniques will ever be reached based on the Nelspruit data alone. The primary reason for this, of course, is that the results have not been achieved from an experiment incorporating randomization. This does not mean that the Nelspruit results should not be published. It may be that what has been learned in Nelspruit (the development of operational techniques, the possible identification of new response variables, etc.) may increase the efficiency of a randomized test of hail suppression technology at some future date. The author and his associates are working very hard to arrange such an experiment in Kenya.

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Comments on "The Measurement of 1 min Rain Rates from Weighing Raingage Recordings"

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Bodtmann and Ruthroff (1976) presented a method for obtaining a smoothed distribution of the derivative with time of rainfall accumulation from weighing-bucket raingage charts. The method is a valuable addition to the methodology for obtaining rates from analog charts.

It should be pointed out, however, that the title of the paper and the use to which the derived data were put are misleading. Bodtmann and Ruthroff (BR) picked off the rainfall data at 1 min intervals and then applied a smoothing technique. This, in effect, averaged the 1 min values with their trend over an interval of 5–6 min. Although this procedure should come closer to a 1 min rain rate than would a simple average over a 5 min period, it cannot be assumed that it represents actual 1 min rates as their title suggests.

By coincidence, two of the stations analyzed by BR, Miami and Bismark, were analyzed by Sims and Jones (1973) in a study of "instantaneous" rainfall rates. The raingage record in the Sims and Jones' (SJ) analysis was located at Coral Gables in the Greater Miami area rather than the official Miami National Weather

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Service gage. The weighing-bucket gage at Coral Gables was fitted with a 6 h chart rotation rather than the usual 24 h and a 321.3 mm diameter collector rather than the usual diameter for standard United States gages of 203.3 mm. The increased collector size results in an amount magnification of 2.5, and the faster chart rotation expands the time scale by 4 over the standard gage used by BR. The Coral Gables data were recorded between August 1957 and August 1958. The records were digitized at singular points with the frequency of points dependent upon the complexity of the recording. One and 4 min accumulations were computed from the *x*-*y* points describing the rainfall curve.

The Bismark record analyzed by SJ was a microfilm copy of the National Weather Service charts for 1966, a year with near-normal precipitation for that station. The chart image was enlarged to the original size by projection for digitization. The reticule used to digitize the 1966 record appears to have had the same geometry as that used by BR. Four minute accumulations of precipitation were computed from the digitized replica of the chart traces.

Fig. 1 shows the curves for comparison of the filtered data of BR and the 1 and 4 min accumulations from SJ for the two stations. It will be noted that there is a close correspondence between the 4 min rates at Miami and Bismark and the curves of BR for those stations. There is less correspondence between the BR curve for Miami and the expanded-scale 1 min rates of SJ. This indicates that the 4 min rates and the BR analysis are approximately equivalent and cannot be assumed to be 1 min rates.

The limitations of the one-year sample may be the reason for the lack of rain rates above 95 mm h^{-1} at Bismark and 150 mm h^{-1} at Miami for the 4 min rate curves. This conclusion seems logical since there was no 1 min rate above 239 mm h^{-1} from SJ, whereas the filtered data of BR produced much larger rainfall rates.

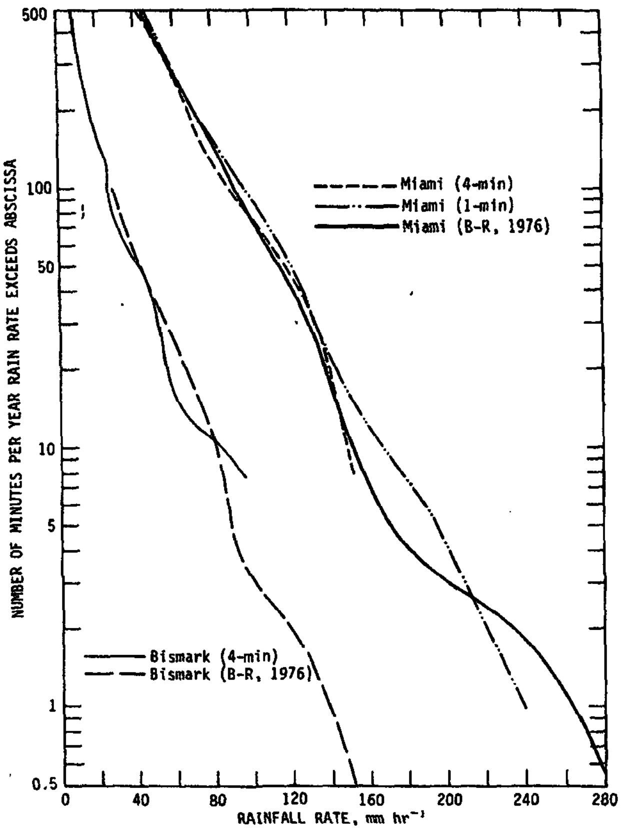

FIG. 1. Comparison of observed and computed rain rate curves.

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Reply

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We disagree completely with the conclusions stated in Dr. Jones' comments on our paper.

The method used by Sims and Jones in obtaining the Miami rain rates appears to be adequate; we disagree, however, with Dr. Jones' interpretation of the results. In the first place, he did not complete the figure. The text states that there were no 4 min rain rates above 150 mm h^{-1}, which means that the number of minutes for that year of rain rates above 150 mm h^{-1} is exactly zero. His 4 min curve can be completed by dropping a vertical line from the end of his 4 min curve to the abscissa for zero minutes per year. When this is done it becomes clear that the correspondence between his 4 min distribution and our 1 min distribution is any-

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