Reply

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Blanchard's reasons for doubting the plausibility of my suggestion that a relation exists between the condensation nucleus concentration and the drop-size distribution of continuous warm-front rainfall can be summarized as follows: He questions the validity of the assumptions that:

1) The spectra examined were all from continuous warm-front rainfall;
2) The most probable ranges of nucleus concentrations were properly chosen; and
3) The condensation nucleus concentration may be the physical parameter causing the observed variations in raindrop spectra.

In addition, Blanchard disagrees with the hypothesis because

4) Spectra measured in orographic rainfalls in Hawaii do not show the presumed relation between n and the nucleus concentration.

I would like to comment briefly on each of these objections.

1) That the spectra examined were all from continuous frontal type rainfall can be inferred directly from Best's (1950) paper. Referring to the constants appearing in his distribution function, he states,

> "It is probable, however, that the mean values of the parameters will not be appropriate either in purely orographic rain or in rain which is essentially of the showery type."

This is borne out by the values Best obtained for n when he attempted to fit the spectra of orographic and shower type rainfalls to his distribution function. In the first instance, Best computed an n of 4.48, while in the second, n varied between 2.05 and 6.10. Using only the six sets of data considered in my paper, Best computed a mean n of 2.25, with a standard deviation of 0.28.

More recently, Mason and Andrews (1960) found that the distribution function suggested by Marshall and Palmer (1948) was applicable *only* to continuous warm-front rain showing no structural features. Commenting on the types of rainfall considered in his paper, Marshall[1] indicated that samples with intensities of 2 mm per hour or less were all from continuous warm-front rainfall. It is exactly at this range of intensities that the Marshall-Palmer distribution provides the closest fit to measured data. Samples from showery type rain (intensities greater than 8 mm) differ markedly from the theoretical curves, and those between 2 and 8 mm per hour (given by Marshall as a rough transition range between purely continuous and purely showery rainfalls) show some scatter. Fig. 1 of Marshall and Palmer's paper illustrates this.

The values of the constants entering the distribution functions, in particular the mean values of a and n in Best's distribution, computed for each set of measurements, are characteristic of the average spectra of continuous warm-front rainfall at the measurement site. Inclusion of shower spectra would perhaps affect the standard deviation of these quantities but not the means themselves.

2) As stated in my paper, the classification of each set of data into country, town, or city locales was based

[1] Personal communication, 8 August, 1963.

on a consideration of the location of the measurements *and* the most probable trajectory of the rains being measured. It was assumed that all the rains examined had their origin to the south or southwest of the measurement site. Population was considered only insofar as man-made nuclei might increase the nucleus concentration. For example, both the Washington and the Shoeburyness measurements were classified as originating from air masses having "town" concentrations under the assumptions that the air over Kent and Essex, England has approximately the same nucleus concentrations as that over Fairfax County, Virginia and Prince Georges County, Maryland; and that these concentrations lie somewhere between those classified by Junge (1951) as "city" and those he classifies "open country." Similarly, the Canadian spectra (Blanchard is correct in his assertion that the measurements were made at Ottawa, not Montreal) were classified as emanating from air masses containing nucleus concentrations representative of Junge's "city" values under the assumption that the storm originated in and travelled over areas having high nuclei concentrations before reaching the measurement site. In all cases, again as stated in the body of my paper, an attempt was made to estimate the most probable range of nucleus concentrations characteristic of the air masses feeding the rainmaking system, not merely that of the place where the measurement was made, as Blanchard seems to assume (misled, perhaps by the unfortunate wording of the abstract). In effect, the term "measurement site" refers to a wedge-shaped area extending south and west of the point at which the measurement was made.

3) It is quite possible, as Blanchard suggests, that in the case of rain from melting snow, the factor causing the observed differences in the final spectra was the ice nucleus concentration, not the concentration of condensation nuclei. However, more precise knowledge concerning the process by which the snowflakes were formed in each individual measurement would be required before the effect of condensation nuclei could be ruled out completely.

The basis for the suggested relationship is simply that there are observed differences between rainfall spectra, that these differences seem to vary in the same way as the condensation nucleus concentration; and that there may be a link, however tenuous, from the nucleus concentration, through the cloud droplet spectrum; to final raindrop spectrum. Because the sample used is small, and the histories of the rainfalls probably varied in regard to the turbulence and evaporation experienced between the cloud base and the ground, it is impossible to say that the initial concentration of condensation nuclei was the *only* factor causing the observed differences in the spectra. Nevertheless, in the absence of further data, the suggested relationship remains a possibility.

4) Spectra of orographic showers can hardly be expected to show the dependence on condensation nucleus concentration postulated in my paper. Best considered one set of data obtained by Anderson (1948) in orographic rains at Hilo, Hawaii. He computed an n of 4.48, which according to Fig. 1 of my paper, would indicate an extremely low nucleus concentration, agreeing with the results obtained by Blanchard and Spencer (1957). However, the variations in the other distribution parameters required to fit Anderson's data to Best's—equation p, q, A, C—render any speculation of this sort meaningless. For the same reason, it does not necessarily follow that Blanchard's (1953) measurements, if fit to Best's distribution function would necessarily yield a small n. Even if they did, the result would be to disprove any relation between nucleus concentrations and raindrop spectra in *orographic* rainfall. Such a relationship has never been suggested.

Blanchard's letter illustrates the futility of trying to either prove or disprove a hypothesis by analyzing data taken for another purpose entirely. My own reasons for not accepting as established the suggested relationship between initial condensation nucleus concentration and final raindrop size spectrum all stem from an awareness of the lack of information concerning the storm's history and the nucleus concentration of the air masses entering the system. If future publications of raindrop-size spectra include notations of the wind direction, and nucleus counts upwind from the measuring site, then more definite statements concerning the validity of the hypothesis can be made. Until then, the whole question remains highly speculative.

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