The paper of Luna and Church (1974) deals, among other things, with the distribution of speed in the full wind in given directions, whereas my paper, commented on by Luna and Church above, treats a somewhat more complex distribution (Neumann, 1977). The nature of this somewhat more complex distribution is made clear in ¶2, Section 1, of my paper where it is stated that the paper is concerned “either with the mean wind or the component of the mean wind in a given direction.” Thus, the set of data of concern in my paper may be regarded to be constituted by two subsets: 1) the type of data that is studied in the Luna-Church paper, that is, the speed of the full wind in a given direction (with an allowance for an angular opening), and 2) the speed in the component in the same direction if the full wind points in a different direction.

In their 1974 paper Luna and Church study a large body of data and find that the distribution of speed of the full wind in given directions is well represented by the log-normal distribution. Now, there is no assurance a priori that the more complex set which I considered obeys a similar distribution. Given two random variables $X_1$ and $X_2$, say, both of which are assumed to follow the same type of distribution, there is no certainty at all that the sum variable $X_1+X_2$ will be similarly distributed. Only under special conditions is this so-called “reproductive property” made sure. This problem is discussed in a meteorological context by Kotz and Neumann (1963, 1964). Thus, if the distribution of speed in the full wind in a given direction is of a log-normal type and if the same assumption is made with respect to the component distribution (where we assume that the components do not include the case of the full speed) in the same direction, we cannot be sure that the composite distribution also will be log-normal. In fact, it will be shown later in this Reply that the component data I used for the purposes of illustration in my 1977 paper is not fitted well by a log-normal distribution.

Before writing up my 1977 paper, I had made an intensive search for data on the distribution of speed in components of the wind in given directions. The only set of data that I was able to find is the one due to the Norwegian meteorologist Hesselberg whose figures are reproduced on p. 93 of Brooks and Carruthers (1953; henceforth we shall refer to that text as BC). This set consists of the distribution of speed in westerly and easterly components of the wind at Aas, Norway, in January 1929 and it was that set that I had used to illustrate the main point of my paper (which was not the matter of distribution but, rather, the problem of averaging the factor $1/\nu$ in the concentration equation of pollutants). Since the above set was the only one available to me, such a statement in the comments of Luna and Church as, e.g., in their ¶1), “… If this is not the case, justification for the arbitrary (italics mine) selection of two components of vector winds for analysis is necessary,” is without foundation. In the same paragraph, Luna and Church say “The suggestion that such components could be used in developing a pollutant ‘rose’…” Nowhere in my paper do I make any reference to pollutant roses. In ¶2, Luna and Church say, “Four component points… were calculated.” A reference to my Fig. 2 will show that the westerly component has five, the easterly six points (class interval data). A reference to Table 30 in BC (p. 93) will show that my study made use of all the data points published.

We shall now show that Hesselberg’s set does not lend itself to representation by a log-normal distribution and hence, the estimate put forward by Luna and Church in their comments concerning the ratio of the average of the inverse wind to the reciprocal of the average wind is not valid.

In fitting the log-normal distribution, I shall follow a method worked out by the British statistician Finney. The method is described on pp. 102–105 of BC. Their notation will be followed here.

In order to fit the log-normal distribution it is necessary for the quantity denoted $z$ in column 4 of Table 37 (p. 104 in BC) to be positive. This must be so because we have to take its logarithm. Now, line 2, p. 105 in BC, shows that $z=1+(\omega/\sigma_z)x$, where $x$ is the deviation of the variate (in the present case: the speed in the easterly component of the wind at Aas in January 1929) from its mean, $\sigma_z$ is its standard deviation and $\omega$ is given in Eq. (78), p. 102. The equation reads

$$\omega = [[\gamma_1+(\gamma_1^2+1)^{1/2}][[\gamma_1-(\gamma_1^2+1)^{1/2}]].$$

Here $\gamma_1$ is the coefficient of skewness which is equal to $\mu_3/\sigma_z^3$, $\mu_3$ being the third moment of the distribution about zero. We can compute the average, $\sigma_z$ and $\mu_3$ from the data in Table 30 of BC and then correct the moments for grouping (e.g., Kendall, 1958, p. 76) and find the average=1.03 m s$^{-1}$, $\sigma_z=0.95$ m s$^{-1}$ and $\mu_3=5.94$ m$^3$ s$^{-3}$ or $\gamma_1=0.63$, $\omega=1.37$, $(\omega/\sigma_z)=1.44$. Hence for $z=1+(\omega/\sigma_z)x$ to be positive, we require

$$1+(\omega/\sigma_z)x>0, \text{ or } 1+1.44x>0, \text{ or } x>-1/1.44=0.7,$$
which means that the log-normal distribution would have to start at $x = -0.7 \text{ m s}^{-1}$ and since the average of the easterly component is $1.03 \text{ m s}^{-1}$, the foregoing result means that speeds $\lesssim 0.33 \text{ m s}^{-1}$ are not admitted. The conclusion, therefore, is that the log-normal distribution does not fit the Hesselberg data. True, this set of data is an extremely small set, but, as was pointed earlier, it was the only one available. Since small wind speeds are an important factor in increasing the difference between the average of $1/u$ (the average that is required according to my 1977 paper) and $1/\bar{u}$, clearly, we have to look for a better fitting distribution.

I agree with most of what is said in §3 of Luna and Church's comments.

**REFERENCES**


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**Comments on “An Analysis of a Possible Crop Response to Hail Suppression Seeding: The Nelspruit Hail Suppression Project”**

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**1. Introduction**

The paper by Mather (1977) is an important one in the history of hail suppression. It is the first analysis to be published in the scientific literature that describes the results of using a new technique for seeding hailstorms—one of direct injection of the seeding material into the supercooled portion of the clouds that comes close to duplicating the technique used in the Soviet Union. Also, the analysis introduces a new response variable and some new statistical techniques resulting in claims of a very substantial apparent suppression effect. However, I feel compelled to comment on several aspects of Mather's analysis and to take issue with some of the critical assumptions made and the results claimed, because his paper once more illustrates the great difficulty involved in attempting to evaluate an operational weather modification program "after the fact."

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**2. Selection of the control data set**

**a. The control period**

It is stated by Mather that, "Although historical records prior to 1970 do exist, abrupt increases in the registered crop and changes in planting patterns seriously mar the homogeneity of these data." These historical data are given in a previous report (Colorado International Corporation, 1976), and the yearly values of registered crop for the primary target area are plotted in Fig. 1. Apart from a drop in registered crop between the 1971–72 and 1972–73 hail seasons there is an almost linear increase in registered crop since the 1966–67 season to the present time. Why then were only two years preceding the seeding chosen as part of the control data set? Why not a period equal to the seeding period, or why not the whole historical period since the damage can easily be normalized to the risk as determined by the registered crop? It is well known that fluctuations occur in the mean annual values of such meteorological parameters as temperature and rainfall, and that selecting any two adjacent years as being representative of the climatic norm for a given area can be very misleading. The same danger clearly exists in selecting two adjacent years as an indication of expected hail damage for control purposes.

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