I was interested to see Mr. Lawrence’s comments on the relationship between meteorological factors and poliomyelitis, and I must apologize for misquoting his 1956 paper. I seem to have confused the work of the Meteorological Office with that of Bradley and Richmond (1953) which he also cites. In my paper (Spicer, 1959), I should have been more explicit about the effects of relative and absolute humidity. I found the following second-order partial-correlation coefficients:

- Polio with vapour pressure; temperature and relative humidity held constant:
  \[ r = + 0.191 \]

- Polio with relative humidity; temperature and vapour pressure held constant:
  \[ r = - 0.294 \]

- Polio with temperature; relative humidity and vapour pressure held constant:
  \[ r = + 0.362 \]

All these coefficients are based on 96 degrees of freedom. The correlation with vapour pressure is not formally significant and, as it accounts for less than half as much of the variation in polio incidence as relative humidity, I felt justified in concentrating on the other two variables. It would be interesting to see the calculations extended, as Mr. Lawrence suggests, to include some kind of interaction term between absolute vapour pressure and temperature but, in view of the low correlation with vapour pressure when temperature is held constant, it is difficult to see how this could affect my original conclusions.

The functions of the meteorological variables suggested by Mr. Lawrence as being possibly more closely related to polio incidence do not seem to me to give much support either to the drying hypothesis or to that of Armstrong (1952). Taking the saturation deficit as a measure of drying power will, as Lawrence points out, give a relation between polio incidence \((P)\), relative humidity \((R)\), and temperature \((T)\) of the form

\[ P = a - bR + cT \]

where \(a\), \(b\), and \(c\) are constants. If temperature is held constant, this also implies a negative correlation with absolute vapour pressure but, in fact, the observed partial correlation coefficient is +0.11 and is not statistically significant.

On the other hand, if Armstrong’s hypothesis holds, there will be a relation of the form

\[ P = a - bE \]

when \(E\) is the rate of loss of water from the nasopharynx. Lawrence suggests that \(E\) might be expressed as

\[ E = m(n - V)/T \]

in terms of absolute vapour pressure \((V)\) and temperature \((T)\). Substituting this expression for \(E\) leads to a positive correlation between polio incidence and relative humidity when temperature is held constant; the observed partial correlation is -0.25 and is statistically significant.

Neither hypothesis therefore is wholly satisfactory but, such as it is, the evidence seems to favour the saturation deficit as being the operative meteorological factor.

Mr. Lawrence makes three main criticisms of the statistical aspects of my paper:

(a) That the meteorological data are averaged over a wide geographical area which may conceal important local effects.

(b) That the limited range of variation of meteorological factors in England and Wales may fail to reveal the existence of thresholds beyond which conditions become unfavourable for the virus, this being especially the case for absolute vapour pressure.

(c) That . . . “using monthly averages (masks)
important meteorological variations the effect of which on polio incidence may be further complicated by such non-meteorological influences as ice cream consumption and bathing”.

As regards (a), I agree that there may well be a loss of sensitivity in the analysis arising from the use of a wide geographical average. On the other hand there is also a good deal of smoothing of local variations unconnected with the weather. The analysis suggests that only about 36 per cent. of the variation in polio incidence in any given month can be ascribed to meteorological factors. The remainder must be due to other causes, such as local variations in immunity, and transmission of the virus. Under these circumstances only a consistent and general relation with the weather will show up over an extended time and area.

Point (b) was touched on in my paper, though I did not realize that it was especially relevant to vapour pressure. In any case, what may or may not apply elsewhere does not invalidate the conclusions for England and Wales.

Criticism (c) seems to cover two points: That a monthly average misses important short-term effects, and that it introduces seasonal effects due to non-meteorological factors. As regards the first of these, similar considerations apply as to the geographical averaging: some real effects may be missed, but on the other hand unimportant ones, such as the influence of holiday periods, are smoothed out.

Another objection to the use of shorter time intervals, such as the week, is that, as the interval is reduced, the autocorrelations between the neighbouring time units are increased, and they are already fairly high between months. A rigorous solution of this problem would probably require some kind of spectral analysis of the auto- and cross-correlograms of the variables. In the present example, with a comparatively small number of degrees of freedom and a manifestly non-stationary situation, it did not seem justifiable to use the methods at present available.

The objection that the monthly averages introduce non-meteorological seasonal effects seems to me to apply to any time unit less than a year; certainly weeks are as much affected as months. The statistical technique I used was adopted specifically to try to eliminate the main seasonal trends and did not use the monthly averages but the deviations about them. It did not exclude the possibility that the effects observed were due to non-meteorological causes correlated with temperature and humidity, but it was less likely to be affected in this way than would be the case were the full seasonal variation not eliminated. This point, and the related difficulty that high correlations will often occur between cyclically varying quantities whether they are really related or not (cf. Yule, 1926), seem to me to be crucial in any statistical study of the relationship between weather and disease. The latter in particular vitiates much of the published work on this subject. The use of deviations from the averages is a useful technique, since a true effect of weather variables should show up both in the seasonal trend and in the deviations; if this effect does not appear in the latter it is very doubtful whether meteorological factors have any direct influence on the disease.

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Comments on Mr. E. N. Lawrence's Paper on Poliomyelitis and Meteorological Factors
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