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THE WEATHER AND CYCLICAL FLUCTUATIONS

FROM time to time various attempts have been made to obtain reasonably accurate forecasts of weather tendencies for long periods in advance. Ordinary forecasts are made from day to day by extrapolation from recorded conditions and tendencies, but even these are not invariably correct, and are, in temperate latitudes at any rate, limited to a very few days at a time. Seasonal forecasting, such as that of the Indian Monsoon, is a further extension of the same principle. Past experience shows some sort of correlation between certain foregoing data and the following seasonal weather, and from these a seasonal forecast can be made with a considerable probable error. When the period desired is measured in years only one resource remains available, and that is periodicity. All attempts to forecast the weather of future years thus reduce to an analysis of a more or less long series of past registers. The most obvious and powerful source of weather conditions is clearly the sun, and, apart from the diurnal rotation of the earth by which the sun is enabled to impose a twenty-four-hour period on the temperature variation, and an important semi-diurnal period on that of atmospheric pressure, the most important period in temperate latitudes is that of the earth's revolution round the sun, which governs the seasons, besides modifying the diurnal variations. The rotation of the sun itself has not hitherto yielded much evidence of variable control, and the only other solar period remaining is that of the sun-spot cycle. It would seem, then, that for long-range forecasting the phase of the sun-spot period must be of far greater importance than any other consideration. It cannot be denied that the tide-raising efficiency of the moon is so much greater than that of the sun that any indirect effect of tides on weather might be expected to vary in sympathy with the longer periods of the moon, and the period of between eighteen and nineteen years, covering the changes of the moon's latitude with respect to the ecliptic, has often been suggested. In this connection a few figures may be interesting: 1841 was a very cold winter at Greenwich; so also were 1860 and 1879; but 1898 was quite warm. On the other hand, 1846, 1857 and 1868 were decidedly warm, while 1879 was the coldest in eighty years.

Investigations over wide areas attribute to years of sun-spot maximum an effect of lower temperature over the world in general, in 1893 in particular, but that year was noted for a specially hot summer in Britain and France. It may be safely said that in our latitude the effect of the sun-spot phase, *i.e.*, the variation in solar radiation, on the weather is not sufficiently direct to enable a reasonable forecast to be made. Lately, in tropical and sub-tropical regions of South America, the effect of rapid short-period changes in solar radiation has been studied, and a possibility of forecasting from these is asserted; but these are short-range forecasts, and do not affect the point we are now discussing.

The question arises: "Does the variation of the solar radiation in the sun-spot cycle have a sufficiently direct effect in the tropics to give assistance to forecasters?" And the answer appears to be that both directly and indirectly this has been used by Dr. Walker in forecasting the Indian Monsoon by his method of partial correlation coefficients, but the coefficients obtained were not large enough to be of great value. Much larger coefficients were found connecting the Monsoon rainfall with some barometric data, but not one of these reaches Sir Napier Shaw's limit of 0.7, which he considers (*Computer's Handbook*, Section V.) the minimum for practical forecasting.

Bearing these things in mind, it is rather surprising to find in *THE ECONOMIC JOURNAL* for 1920 a paper by Sir W. Beveridge on "British Exports and the Barometer," in which he alleges that a period of fifteen and one-third years governs the price of wheat, and that bad harvests and high prices recur at not quite regular intervals of approximately this length. He even ventures a guarded prediction of unseasonable weather, bad harvests, and high prices, with possible famine in one or more of the years 1924, 1925, and 1926. I am not a student of economics, and I do not profess to know what would be, to a wheat-grower, the practical value in 1923 of even a cast-iron certainty that one of the next three years would answer this description. If the date were definitely given as 1924 it might be a simpler problem; but I do not know the solution. My concern is simply with the process by which the prophecy is made possible, with a strong presumption against its validity.

Many of us have from time to time analysed long series of meteorological data, in the fascinating pursuit of periodicity, and the first great difficulty encountered is the choice of time-unit. For such a purpose as crop forecasting, the unit ought apparently

to be a year, and yet the year is a singularly useless period to study in bulk. There is well-marked correlation between low pressure and high rainfall, but this does not show very much from year to year. Taking three-month periods is better, and for monthly periods the agreement is close. In sixty years of Greenwich figures seventy-eight months show mean barometer readings differing from normal by twice the mean residual. Of these, high barometer is associated with low rainfall and *vice-versâ*, except in two months. Of these two months, one July had a small deficiency of rain with low mean barometer, but there were twenty rainy days in the month. The other exception is a November with high mean barometer and high rainfall, but in that instance all the rain fell in the first ten days, after which the barometer rose and remained high with hardly a spot of rain. Thus even the month may be too long for time-unit. Sir W. Beveridge, however, is satisfied to take the year as his unit, so that a year of drowned harvests may be a dry one, and a year of dry summer may be a wet one. Even in the months the same objection may be found, as a single thunderstorm may make a really dry month come out with excess of rainfall.

Let us look further into the figures given. Accepting the barometer data, I have examined those from 1873 to 1904 (India, etc.), and 1873 to 1903 for North America, each being a homogeneous series. I computed the mean residuals, and found fifteen of the first set and twelve of the second, which differ from normal by more than the mean residual. In only seven years do both series stand out. In two of these the residual is of the same sign, both being high in 1888 and both low in 1893. In 1873, 1882 and 1892 the Southern barometer is low and the American high, the reverse being the case in 1885 and 1902. The greatest residuals in the respective series are 1877, high, in Southern area, and 1878, low, in the North American. Assuming that the very small residuals are of no consequence, it is an easy inference that the barometer figures show no evidence of any such period as fifteen and one-third years.

Presumably the total atmospheric pressure over the earth is practically constant, so that an excess in one hemisphere would be compensated in the other; so that the five cases noted are quite plausible. The areas considered are not large enough to supply perfect compensation; so we may assume that the Eurasian figures might have compensated the high residuals in 1888 and 1893. This particular compensation might be expected to prevent the occurrence, except at very long intervals, of such

a phenomenon as a world-wide failure of harvest from meteorological causes; having regard, for instance, to Lockyer's "Barometric See-saw" (see *Proceedings of Royal Society*, Vol. LXX., and elsewhere).

Curiously enough, the Argentine region, which on this showing would come to the rescue in case of a bad season in India, especially as it is an important source of wheat supply, seems to have been ignored by Sir W. Beveridge.

A practice that I have known to be followed in some periodicity investigations is to obtain an approximate period from one set of data, and to "correct" it by reference to other sets of data which by themselves would give rather different periods, but it must be borne in mind that a period of x years in one place and of $x+2$ years in another cannot be regarded as strong evidence of a period of $x+1$ years in both.

There remains one important consideration. Periodicity as an aid to forecasting depends for its justification on its definiteness and on its accuracy in prediction. It is not enough to derive series of sine-curves from long-continued observations, and make them fit more or less all the known data. An example of this can be found, very pertinent to the present question, in the sun-spot analysis of Professor Kimura. The sun-spot period is known to be irregular, and Professor Kimura analysed the sun-spot numbers from 1750 to 1911 and found a curve which fitted fairly well through that long period. He continued his curve by way of prediction to 1950, but the actual figures disagreed the first year and got worse and worse year by year. Another case, when greater accuracy could be expected, since, after all, very little is really known about sun-spots, is that of what is called the moon's error. The moon's motion is modified by the attraction of planets, the want of sphericity of the earth, and so on; but when allowance is made for all known causes the moon's observed place still goes on differing more and more from the computed place. Professor Brown, the greatest living authority on lunar theory, analysed the observed residuals for 150 years, and yet failed to predict them for the next few years. Considering how many causes, which are so apparently arbitrary as to be commonly called "accidental," affect the weather and the crops, it does not seem likely that the time has yet come for long-range forecasting to become a practical factor in the regulation of the world's food supply.

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