The Relations between Sunspots, Terrestrial Magnetism, and Atmospheric Electricity.

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THE existence of a relation between sunspot frequency, or area, and the phenomena of terrestrial magnetism and atmospheric electricity is a question on which a reasoned statement of opinion may be opportune.

Two elements may be connected and yet not stand in a linear relationship. When nothing is known, there are advantages in a graphical method, such as that employed by W. Ellis [1] when comparing diurnal magnetic ranges at Greenwich with Wolf's sunspot frequencies. But when there is reason to anticipate a linear relationship, it is better to use Wolf's formula

$$R = a + bS,$$

where S denotes sunspot frequency, and R is a quantity such as the diurnal range of a magnetic element. a and b are constants which can be determined by least squares. Obviously a is the value of R when $S = 0$. As the average range of S between sunspot maximum and minimum approaches 100, $100b/a$ is a convenient measure of the importance of sunspot influence. In the case of magnetic daily ranges $100b/a$ usually exceeds 0·5, and not infrequently 1·0. If $100b/a$ is a small fraction, the sunspot influence, even if real, is unimportant. The closeness with which Wolf's formula fits the observations is measured by the correlation coefficient r. As b shows the sign of the correlation, whether positive (*i.e.* element increasing with S) or negative, we shall treat r as a numerical quantity. It cannot exceed 1, which represents a perfect fit. A low value such as 0·3 implies that the sunspot relation is very doubtful.

In the case of terrestrial magnetism, a linear sunspot relation seems fairly established for the range of the regular diurnal variation, whether of declination (D), horizontal force (H), or vertical force (V). Further claims have been made. Thus Leyst [2] believed the secular change of D to be considerably faster at sunspot maximum than at sunspot minimum, but further investigation has not confirmed this. Declination may be east or west, and numerically increasing or diminishing; thus acceleration of the secular change signifies different things at different places. *A priori* the force components H and V seem more likely to possess an 11-year period. Unfortunately, with ordinary instruments, H and V determinations are less reliable than those of D, and it is doubtful whether the annual values available have the accuracy necessary for determining the reality of a sunspot influence, which is certainly not large.

Diurnal range may signify the range of a diurnal inequality based on hourly values, or the difference between the extreme instantaneous values of the day, usually called the absolute range. In most if not all of the earlier work by Wolf, Ellis, and others, range meant the diurnal inequality range, or some analogous quantity. Older data were mostly from eye readings taken at two fixed hours, at Milan, for example, at 8 A.M. and 2 P.M. If the observation hours are the hours of maximum and minimum in the mean diurnal inequality for the year, they will give in the case of the whole year the same range that hourly readings give. If the hours of maximum and minimum vary in different months, the range derived from hourly readings may in some months sensibly exceed that from readings at two fixed hours. But these are minor differences, and the older observations may be regarded as establishing the validity of Wolf's formula for the mean diurnal inequality of the year. In general, D was the element considered, but Ellis also included H. It is not claimed that Wolf's formula with invariable values of a and b agrees closely with observation in every year of, say, 50 years. But, so far as is known, whenever Wolf's formula has been applied to any 11 years, b has proved to be plus, and $100b/a$ has been substantial. This has been true whether the diurnal inequality has been derived from quiet days or from ordinary days. At Kew the value obtained for $100b/a$ from the quiet days of 1890 to 1900 was 0·71 for D, and 1·07 for H. Ordinary days gave very similar results. Fairly similar results have been obtained at many stations, the value of b/a being usually decidedly higher for H than for D. The fit of Wolf's formula is generally good, and sometimes extremely close. For the period 1911 to 1921, in the case of the mean diurnal inequality at Kew, r was 0·96 for D, and 0·95 for H.

Instead of the range of the mean diurnal inequality for the year, we may take the arithmetic mean of the ranges of the diurnal inequalities for the 12 months. The two quantities usually differ, but similar results are obtained.

Instead of considering the whole year, we may apply Wolf's formula to different seasons, or even individual months of the year. The fit for an individual month, *e.g.* the Januaries of an 11-year period, may be indifferent, but it is usually good for a 4-month season, *e.g.* May to August (summer), or November to February (winter). We may calculate a and b from the range of seasonal diurnal inequalities, or we may accept as the seasonal values of a and b the arithmetic means of the a's and b's calculated for the included months separately. The most outstanding result is that, at least in higher latitudes, b/a is considerably larger for winter than for summer. Thus at Pavlovsk, 1890 to 1900, the value of $100b/a$ for H from all days was 1·77 in winter, as against 0·98 in summer.

The absolute daily range is larger than the inequality range, and is more affected by disturbance. It, too, shows the sunspot influence clearly, but with a less close fit of Wolf's formula. An interesting example [3] is afforded by the years 1892, 1893, and 1894, with sunspot frequencies of 73·0, 84·9, and 78·0 respectively. The ranges of the mean diurnal inequalities in D at Kew for ordinary days were 9′·85, 10′·7, and 9′·8; while the mean absolute ranges from all days were 17′·7, 15′·6, and 16′·5. Thus 1893, the year of sunspot maximum, had the largest inequality range, but its mean absolute range was distinctly inferior to those of the adjacent years. The result appeared in H as well

[1] Phil. Trans., 171, p. 541.
[2] *Bull. de la Société Impér. des Naturalistes de Moscou*, 1909, p. 160.
[3] Phil. Trans., A 208, pp. 215 and 226; A 216, p. 261; and Chree, "Studies in Terrestrial Magnetism," pp. 177, 178.

as D, and the order of the inequality ranges was the same for quiet as for ordinary days.

The difference between sunspot maximum and minimum years is partly a matter of disturbance, but both classes of years contain days practically free from disturbance, and the quiet days from sunspot maximum years tend to have larger ranges than the quiet days from sunspot minimum years. The relation between sunspots and magnetic disturbance is much less definite than that between sunspots and the regular diurnal inequality. In a general way, disturbance is least at sunspot minimum. In the 11 years 1890 to 1900 at Kew there were 29 (Greenwich) days with H absolute ranges not less than 250γ, but none of these occurred in the three years nearest to a sunspot minimum. On the other hand, some years of many sunspots are also quiet. For example, 1893, a year of sunspot maximum, had no H range so large as 250γ, while 1892 and 1894 between them had 24 such ranges. Again, some of the very largest magnetic storms have occurred in years of comparatively few sunspots. Thus 1921, with a sunspot frequency little more than half that of the average year, had a succession of highly disturbed days during May, to which the previous 60 years afforded only one parallel.

The existence of a specific relation between individual sunspots and individual magnetic storms is a vexed question, on which a general agreement cannot be claimed. The magnetic character of an individual day certainly cannot be inferred from the sunspot area or frequency for the day. The 660 selected quiet days of the 11 years 1890 to 1900 had 41·15 as their mean provisional sunspot frequency, as compared with a mean of 41·03 from all days of the year. When the 5 days of highest sunspot area from each month of these 11 years were considered, the corresponding mean daily H range at Kew exceeded the average from all days by only 3 per cent. Most magnetic storms last only one or two days, few so much as four days; but a large sunspot is seldom so short-lived as this. The natural inference is that if the sunspot is the immediate cause of the magnetic storm, its effectiveness must be largely restricted to one particular stage of its development, or else to a very limited range of position relative to the earth. The investigation above referred to suggested an enhanced diurnal range in H for some days subsequent to the attainment of a maximum sunspot area on the sun, the largest range appearing 4 days subsequent to the maximum area.

The phenomenon most suggestive of an influence associated with specific small solar areas is the 27-day interval in the sequence of magnetic storms. The interval seems well established, and its most natural explanation, as suggested by Mr. Walter Maunder, is the existence for a number of solar revolutions of a comparatively narrow cone of radiation which sets up a magnetic storm whenever it crosses the earth. A difficulty is that the 27-day interval seems as well established for quiet as for disturbed conditions. A curious phenomenon, some cases of which were recently discussed by Father Cortie,[4] is that after a number of recurrences of disturbed conditions at 27-day intervals, quiet conditions intervened, to be succeeded by further sequences of disturbed conditions. On the other hand, when we proceed by 27-day steps from a selected quiet day, we sometimes hit on a highly disturbed day. These phenomena suggest the possibility that a limited solar area may emit a radiation, which at one stage enhances and at another stage diminishes the ionisation of the upper atmosphere. If the radiation consisted at one stage of free ions, and at another stage of ejected matter which loaded up the ions naturally present in the upper atmosphere, the 27-day interval in quiet conditions would be intelligible. This suggestion was made originally in a frivolous spirit, but it may be more worthy of consideration than was originally supposed.

The possibility of a sunspot influence in atmospheric electricity seems to have occurred independently to several people, including the present writer.[5] It has recently been the subject of two papers by Dr. L. A. Bauer.[6] In the first of these, which dealt with the potential gradients recorded at the Ebro Observatory, Tortosa, he concluded that mean yearly values of potential gradient (P) and its diurnal range both increased with sunspot frequency. As a check, the writer[7] applied Wolf's formula to five sets of Kew potential gradient data, from two periods 1898 to 1909, and 1910 (or 1911) to 1920 (or 1921). The results were so far favourable to Dr. Bauer's conclusions in that a positive value of b resulted in four cases. But the fifth case gave a negative value, and the values of $100b/a$ were all small, the four positive values averaging only $+0·17$, and the corresponding values of r averaging 0·49.

In his second paper (*l.c.* p. 186) Dr. Bauer expresses somewhat modified views, including

(*a*) "The probability is high that . . . potential gradient and its diurnal and annual ranges . . . are subject to sunspot influence."

(*c*) "During 5 of the past 7 sunspot cycles the potential gradient and ranges . . . generally increased with increasing sun-spottedness. For the remaining 2 sunspot cycles, the potential gradient and ranges . . . apparently decreased with increasing sunspot activity."

The epochs in which the relation is supposed to have been negative seem to be 1845–1855 (station Brussels) and 1886–1897 (stations Perpignan, Lyons, and Greenwich).

Some knowledge of the observational uncertainties is a desirable prelude to a consideration of the results. Suppose we take a water-dropper, the most efficient type of "collector." When the electrograph is working, the tube discharging the jet is connected to the needle of a quadrant electrometer. One pair of quadrants is maintained at a constant potential $+v$, and the other pair at $-v$, the sensitiveness varying with v. Suppose we break the connexion to the jet, and connect the needle to a variable source of potential. Raising the potential step by step, put marks on the paper on the recording drum answering to the voltages 100, 200, etc. Now remove the source of potential, and connect the needle to the discharge tube. For simplicity, suppose the air surrounding the jet to remain for some time at $+100$ volts. On turning the jet on, the electrometer reading gradually rises, reaching a stationary

[4] Proc. Roy. Soc., vol. 106, p. 19.

[5] Phil. Trans., A, vol. 206, p. 303.

[6] *Terrestrial Magnetism*, vol. 27, p. 1; vol. 29, pp. 23 and 161.

[7] Proc. Physical Society, vol. 35, p. 129.

position in, say, 30 seconds. But the reading answering to the stationary position will be sensibly less than 100 volts, unless the insulation of the water tank and discharge tube is really good, and the deficiency is greater the poorer the insulation and the less efficient the collector.

Again, the potential at the site of the collector is usually a good deal lower than it would be at the same height above level ground remote from buildings. Thus, however good the insulation, the readings require multiplication by a factor to give true potential gradients, and unless the position of the collector and its environment (and the insulation of the water-dropper) are invariable, the factor varies with time.

The necessity for a reduction factor was generally unrecognised until comparatively recently, while changes calling for alteration in the factor are practically certain to have occurred in the older installations. Insulation is very hard to maintain good, especially with the older types of insulators. It generally suffers from damp weather, and some years are much damper than others. The outcome is that unless a reduction factor is regularly determined and applied, the absolute potential gradient and its diurnal and annual ranges, as deduced from the curves, may fluctuate as insulation is better or worse. They will also naturally alter with the growth of trees or shrubs, or modification of buildings near the collector.

Kew has probably a longer record from a fairly modern electrograph than any other observatory, and Dr. Bauer has suggested the utilisation of the earlier records. The curves prior to 1898 were unfortunately not tabulated, with the exception of one or two years, and the heavy labour required to do so now has not appeared justifiable in the absence of determinations of a reduction factor before that date.

To gauge the probability of Dr. Bauer's conclusions, his notation and methods must be understood. He employs two formulæ:

$$P - P_m = s'(S - S_m) \quad . \quad . \quad . \quad . \quad (A);$$

and

$$P - P_m = s(S - S_m) + t(T - T_m) \quad . \quad . \quad (B).$$

Here S represents Wolf's sunspot frequency, P the absolute value or the range of potential gradient, T the year, and the suffix m denotes the mean for the period considered. s', s, and t are constants determined by least squares. (A) is simply a variant of Wolf's formula, with s' written for b. (B) differs through the addition of a term varying linearly with the time. The correlation coefficient is called r'_s in the case of (A) and r_s in the case of (B). Dr. Bauer attaches most weight to (B), but gives no adequate justification for its use. The question is important because r_s is usually larger than r'_s, so if (B) is admissible the case for a sunspot influence appears stronger than it otherwise would. Cases are conceivable in which (B) would be justified, *e.g.* if besides an 11-year period there were a much longer period, say 100 years, or if some gradual change had been in progress in the apparatus or its environment, which might reasonably be supposed a linear function of the time. But some positive justification seems called for in each specific case. The fact that the t-term usually improves the agreement with observation is no sufficient argument in its favour, because, with two constants at our disposal instead of one, that is only to be expected. When t is small, s and s' differ but little, but when it is large, they usually differ considerably in size and sometimes even in sign. Large values of t appear more especially in the older series of observations used by Dr. Bauer, especially those for St. Louis, Perpignan, Brussels, and Greenwich. But they also occur in the case of some of the more recent data, including some from Kew. In 4 out of 7 cases at this station in Dr. Bauer's Table 5, p. 169, *l.c.*, s is positive, while s' is negative. In fact, the adoption of the two-term formula converts a vote against to a vote for a positive correlation.

One most important modern station, the uncorrected data from which might have called for a t-term, is dismissed by Dr. Bauer in the following words (*l.c.* p. 24): "Unfortunately as regards the Potsdam observations, various changes . . . especially during the period 1914–19 when no control observations . . . could be made, have introduced discontinuities . . . so as to make unsafe the utilization of the observations." This view has recently been controverted by Dr. Kahler, of Potsdam,[8] who claims that the mean annual values of potential gradient now published are satisfactory. Dr. Kahler adds that they do not support a sunspot influence, but he gives no figures. As the Potsdam series is longer than most, and the station has a high reputation for staff and equipment, it has seemed desirable to apply a Wolf's formula to the mean yearly values given by Dr. Kahler, treating them on parallel lines with data from Kew and Eskdalemuir. The results from the three stations are as follows, the unit for a and b being 1 volt per metre:

Quantity.	Period.	Station.	*a*.	*b*.	100 *b*/*a*	*r*.
Mean annual value	1904–23	Kew	309	+0·303	+0·098	0·37
,, ,, ,,	,,	Potsdam	211	−0·216	−0·102	0·34
,, ,, ,,	1911–21	Kew	312	+0·444	+0·142	0·56
,, ,, ,,	,,	Potsdam	205	−0·143	−0·069	0·46
,, ,, ,,	,,	Eskdalemuir	236	+0·440	+0·186	0·83
Range mean annual	1911–21	Kew	152	−0·114	−0·075	0·23
Diurnal inequality	1912–21	Eskdalemuir	110	+0·115	+0·105	0·25

The year 1911 was omitted in the final case at Eskdalemuir owing to some special uncertainties.

One of the outstanding things was the opposition between the Kew and Potsdam data from 1911 to 1921. At Kew the departures of S and P from their mean values agreed in sign in 9 of the 11 years, while at Potsdam they differed in 10.

Referring to Dr. Bauer's own tables of results, there seem to be only three of his stations—Tortosa and Eskdalemuir with at most 14 years' observations, and Kremsmunster with only 8—which supply in all the cases considered positive values for s and s', and several of these values are quite small.

The data from Kew and Potsdam are suggestive either of no direct sunspot influence, or of a comparatively trifling influence liable to be masked by weather effects. Less conflicting results may be obtainable from regions having a less variable climate, but lower latitudes have been as yet very poorly represented.

A sunspot influence acting in different directions at

[8] *Ergeb. der met. Beob. in Potsdam in den Jahren* 1921, 1922, *und* 1923, p. viii.

different places at the same time, and affecting the results at one station in opposite directions at different epochs, is something quite unlike the sunspot influence recognised in the diurnal variation of terrestrial magnetism. Its acceptance does not seem justified without much more observational support than it has yet received.

Some remarks in § 47 of Dr. Bauer's second paper (*l.c.* p. 184), if they stood alone, or even if they followed the conclusions on p. 186, might be interpreted as not inconsistent with the conclusions reached here. If, however, he really regards the existence of a sunspot influence in atmospheric electricity as quite an open question, a more explicit statement is desirable, as several recent references to his work assume a relation to have been established.

Note added June 3.—Since the above was in print, a further paper by Dr. Bauer has appeared in the March number of *Terrestrial Magnetism*, in which he dissents from Dr. Kahler's conclusions.

An International Campaign against Sleeping Sickness.

AMONG the limited number of post-War changes that it is possible to regard with satisfaction, not the least is an enhanced sense on the part of representatives of European nations of their responsibilities towards what are known as native races. Conspicuous manifestations of this new spirit have been shown by the British Government, and recent action on the part of the League of Nations is evidence of similar breadth of view. In 1922 the Provisional Health Committee of the League appointed a Committee of Experts, under the chairmanship of Dr. Andrew Balfour, for the purpose of collecting information as regards sleeping sickness and tuberculosis in equatorial Africa, and making certain recommendations with reference to these diseases. The members of the Experts Committee, in addition to the chairman, are Dr. E. van Campenhout, Director of the Public Health Service at the Belgian Ministry of the Colonies; Prof. Gustave Martin, formerly head of the French Sleeping Sickness Mission in French Equatorial Africa; and Dr. A. G. Bagshawe, Director of the Tropical Diseases Bureau, London. This Committee, which met for the first time in November 1922, submitted to the Health Committee of the League two most valuable Reports; and the outcome of the recommendations included in the second of these was an International Conference on Sleeping Sickness, which assembled last month in London, and was presided over by Mr. W. Ormsby-Gore, Under-Secretary of State for the Colonies. All of the countries interested in tropical Africa, namely, Belgium, France, Great Britain, Italy, Portugal, and Spain, were represented at the Conference by well-known authorities on tropical disease.

As most people are already aware, thanks to the prominence given to the matter by the daily press, among the recommendations that the delegates to the Conference have unanimously decided to make to the Council of the League of Nations, and to their respective Governments, is the formation of a small international commission for the investigation of sleeping sickness problems in Africa itself. It is suggested that this Commission shall consist of a few specialists in tropical disease furnished by the Powers interested in Africa, with the addition of a biochemist and an entomologist with local knowledge; and that it shall also include the well-known authority Dr. K. Kleine, the value of whose recent researches in Northern Rhodesia and elsewhere on the therapeutic effect of "Bayer 205" is widely recognised.

Since Uganda and the regions adjacent to Lake Victoria furnish the most suitable field for the study of the problems selected, it is proposed to make Entebbe the headquarters of the Commission, and to place the latter under the presidentship and control of Dr. H. Lyndhurst Duke. The necessary expenditure, to which the respective Governments, the Health Organisation of the League of Nations, and scientific research institutions of certain countries, are to be invited to contribute, is estimated at some 10,000*l*. It is suggested that the Commission shall assemble at Entebbe at the end of next December, or in January 1926, and that it shall work for twelve months, after which it will submit a special report to the League of Nations Experts Committee. Guided by Dr. Duke, the Commission will apply itself in the first instance to a study of "the research methods and laboratory technique at the Entebbe Institute and its field laboratories, as well as the field work and measures taken against sleeping sickness in the Protectorate of Uganda and the infected districts of Tanganyika."

After inspecting the methods and highly promising results of Mr. C. F. M. Swynnerton's experiment in the control of *nagana*-carrying tsetse-flies at Shinyanga, Tanganyika Territory, the Commission will settle down to "joint laboratory investigations as to the methods of work which are most suited for research into the several problems referred to it" by the Conference. It is understood that these problems include, among others: the question of the existence, nature, and determining factors of any human immunity to trypanosomiasis; the comparative value from various aspects of trypanocidal agents; the function of wild and domestic animals as breeding grounds for the virus; and the possibility of *Trypanosoma gambiense*, the causal agent in trypanosomiasis as conveyed by *Glossina palpalis*, assuming the form known as *Trypanosoma rhodesiense*, and so becoming capable of dissemination by *Glossina morsitans*.

In addition to its proposals for the Commission, the International Conference has also advised the adoption of a number of highly important administrative measures, including arrangements for periodic official conferences, and frequent interchange of information between administrative and medical officers on both sides of boundaries between infected countries; the devising of means for giving legal effect to recommendations of the medical service engaged in the campaign against sleeping sickness; and methods for the control and reduction of trans-frontier native traffic in infected areas.

It must not be thought that, at the present time, before the proposed International Commission is yet in being, little or nothing is being done by the governments and medical services concerned to combat