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Supplementary Notes

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which have been found by the same process in other records. To take one cycle alone, my figures show that if in wheat prices there is any periodicity at all, there is a periodicity of about 5.1 years having a particular maximum phase. Captain Brunt, using a totally different set of figures, but the same objective method of harmonic analysis in absolute independence, says in effect that if there is any continuous periodicity in Greenwich temperatures from 1850 to 1910, it is a period of about 5.1 years, with a phase which is found to agree very closely with mine. Mr. Baxendell again independently found the same period with the same phase. What conceivable possibility is there that my figures could yield this sort of result by chance? Exactly the same argument of almost the same strength arises from the 35-year period and others.

The last point I wish to make arises from what Mr. Flux said. I certainly do not wish to assert that the whole of the weather can be reduced to a series of cycles. The cycles which I have found may, I think, ultimately be found to account for 30 per cent., or 50 per cent., or possibly even 75 per cent. of the weather. To this extent the rainfall may be reduced to mathematics. But I do not in the least know what this proportion is, and I know still less what may be the law governing the balance. Until we know it is impossible to prophesy. That is the reason why I do not yet prophesy.

SUPPLEMENTARY NOTES.

Furnished by Sir William Beveridge after the meeting.

The following notes deal with one or two special points on which investigations suggested by the discussion of my paper have been carried a little farther.

Note A.—Synthetic Curve and Rainfall.

Since my paper was written I have seen Mr. R. H. Hooker's paper on the Weather and the Crops in Eastern England, 1885–1921 (printed in the *Quarterly Journal of the Royal Meteorological Society*, April, 1922). From this, the important fact emerges that in the area under review all the significant correlations between the yield of wheat and the rainfall are negative. In Mr. Hooker's own words "wheat never seems to want more rain than it gets," and again, wheat "never seems really to require an average rainfall at any period." This is in complete accord with the hypothesis on which the synthetic curve has been constructed and compared with rainfall, a maximum of prices, i.e. a poor crop of wheat being associated with excess of rain and never with deficiency. No other crop examined by Mr. Hooker has this peculiarity; no other crop would therefore serve equally well for my harmonic analysis.

The coefficient of correlation between the synthetic curve and the curve of actual rainfall in Europe from 1851 to 1905 (as drawn in the chart) is .384 and the probable error $\left(.674 \times \frac{1-r^2}{\sqrt{n}} \right)$ is .077. For the first differences, the correlation coefficient is .435 and the probable error .074. Thus the correlation coefficient is in one case five, in the other case six times the probable error. This would, I think, be accepted by most statisticians as satisfactory evidence of a close connection between the two curves.

In view of the importance attached to the synthetic curve, I give below its approximate ordinates as read off from the chart. The units of measurement are arbitrary.

Approximate Ordinates of Synthetic Curve.

1851		19.0	1870		12.7	1888		9.5	1906		21.5
1852		21.0	1871		15.4	1889		15.5	1907		18.0
1853		17.8	1872		17.8	1890		15.5	1908		12.2
1854		19.1	1873		12.4	1891		14.8	1909		11.0
1855		17.5	1874		10.9	1892		15.8	1910		14.8
1856		12.5	1875		18.6	1893		13.5	1911		20.0
1857		9.5	1876		27.0	1894		13.0	1912		27.0
1858		15.0	1877		20.5	1895		9.5	1913		21.5
1859		14.0	1878		19.0	1896		9.5	1914		18.0
1860		14.6	1879		17.2	1897		4.0	1915		19.5
1861		15.9	1880		12.4	1898		5.0	1916		28.0
1862		16.0	1881		11.8	1899		11.5	1917		24.5
1863		14.8	1882		13.0	1900		14.5	1918		18.0
1864		14.2	1883		8.3	1901		15.0	1919		11.9
1865		19.5	1884		8.5	1902		14.1	1920		12.1
1866		21.2	1885		10.0	1903		13.1	1921		8.6
1867		20.5	1886		7.4	1904		8.8	1922		10.6
1868		18.0	1887		5.5	1905		11.9	1923		16.5
1869		14.7									

Note B.—Complementary Periods.

The point raised by Mr. Yule as to the appearance, in harmonic analysis, of "complementary periods," *i.e.* of pairs of trial periods yielding identical amplitudes, is of considerable importance. I have examined it further, mainly in consultation with Mr. Yule. The particular period of 2.2 years to which he refers only appeared in my analysis through an arithmetical error, and has disappeared on revision. This does not affect the general argument.

From the formulæ used for evaluating a and b , it is easy to see that, for all values of p and s , the two trial periods $\frac{p}{s}$ and $\frac{p}{p-s}$ yield identical values of a^2 and b^2 and so of I . One of such pair of complementary periods is always less and one greater than twice the interval between the successive observations (t). The lengths of the two periods may also be expressed in other ways, as $\frac{Nt}{q}$ and $\frac{Nt}{N-q}$; or P and $\frac{Pt}{P-t}$.

From the first of these formulæ we see that, if the periodogram drawn in Chart A for values of q less than $\frac{N}{2}$ ($= 150$) were drawn also for values of q between N ($= 300$) and $\frac{N}{2}$ ($= 150$), this new portion would be an exact reflection of the old amplitudes at equal distances from $q = 300$ and $q = 0$ being identical. Every one of the peaks from 2.000 to 150.0 years would be repeated between 1.000 and 2.000 years. Since obviously there are not pairs of real periods corresponding to each of these pairs of peaks in the periodogram, only one of each pair can be treated as real, while the other is a mere mathematical reflection. So far as I know, there is no reason in the nature of the analysis itself for preferring one of any pair to the other; in the case of one pair the reality may lie between $q = \frac{N}{2}$ and $q = 0$, with the reflection between $q = N$ and $q = \frac{N}{2}$, and in the case of another pair the reality may lie between $q = N$ and $q = \frac{N}{2}$, and the reflection

between $q = \frac{N}{2}$ and $q = 0$. The ambiguity does not arise from the use of annual observations as such; and is not removed by comparison between two or more series of observations, however independent, so long as the observation interval (t) is the same in each.

The second of the foregoing formulæ shows that any true period of given length P will be reflected at $\frac{Pt}{P-t}$, that is to say, at a point varying with t . Accordingly, if two quite independent analyses show a significant peak for the same trial period, say, 2·200 years, then, if the observation intervals are different (say, a year in one case and a month in the other), the length of the real period is determined without ambiguity as 2·200 years. But if both sets of observations are annual, the true length of the period (though its reality be beyond question) may be either 2·20 or 1·83 years; if they are both monthly, the true length may be either 2·20 or about ·087 years. The difficulty arises not from the use of annual figures as such, but from using only one time interval, whether a year, three months, a month, and whether in the same or in different series of observations.

In the particular case now under consideration the ambiguity is in most cases resolved. Both Mr. Baxendell's and Captain Brunt's calculations are based on a time interval of a month; the lengths given by their analysis and mine for such periods as F (4·415), and G (5·100) are the true lengths, and not those of the reflections. The other parallels, such as those with the Bruckner period, do not rest on harmonic analysis at all. There remains the possibility, that some of the periods in my analysis for which no parallel has been found elsewhere may be mere reflections of true periods having lengths between one and two years. We cannot indeed explain the 15·225 year period in this way; that period appears unmistakably in the original observations. But the apparent period at 19·900 years may be a mere reflection of a true period of $\frac{19.9}{19.9-1.0} = 1.053$ year or 384 days, and the apparent period at 9·750 years (for which no real parallel has been found and which in no way fits the curve of actual rainfall) may be a reflection of a true period of 407 days. As it happens, much the most prominent peak in the periodogram of solar prominences from 1905 to 1912, made by Dr. Royds at Kodakainal, is at 13½ months or 406 days, and this result is confirmed by an analysis of Italian observations over 31 years. (*Bulletin No. XXIII of Kodakainal Observatory*, 1913.) This exact coincidence of length is striking, but of course not conclusive. To my mind it does not at present outweigh the inherent improbability that a true period of little over 13 months should be strong enough to show so emphatically in observations each of which represents an average price for 12 months. I should be surprised, therefore, though not at all sorry, to find the true explanation of some of my puzzling periodicities, such as 9·750, in much shorter "complementary" periods. For most of my cycles this explanation is already definitely excluded.

Note C.—Phases in Separate Half Sequences.

In view of the importance to be attached to agreement of phase as well as length between periods shown in the separate half sequences, I give below for each of the cycles in my table the phase angles ϕ_1 and ϕ_2 (distance of first maximum from the origin) in each half sequence and their difference ($\phi_2 - \phi_1$), showing the advance or regression of phase. The first half sequence, as stated, consists in all cases of a number of complete revolutions of the trial period under consideration, and the second follows on it immediately. It will be seen that in nearly all cases the values of $\phi_2 - \phi_1$ are either small or, when the true length is taken as lying between two trial periods, are of opposite signs for those periods, indicating in either case close agreement of phase for the true period. The most noteworthy exceptions are for the cycles K (7·417), O (12·050) and R (17·400), which have been left out of account in the synthetic curve. F (4·415) also shows irregularity. Apart from these cases, the agreement of phases is generally good and often remarkably close.

Phase in Separate Half Sequences.

Trial period.	Phase angles.		Difference $\phi_2 - \phi_1$.
	First half sequence ϕ_1 .	First half sequence ϕ_2 .	
2.737	Degrees. 24	Degrees. 23	Degrees. - 1
{ 3.412	278	303	+25
{ 3.417	330	316	-14
{ 4.400	127	85	-42
{ 4.412	104	41	-63
{ 4.417	97	38	-59
5.100	11	40	+29
5.667	337	343	+ 6
{ 5.933	341	40	-59
{ 6.000	223	151	+72
{ 7.400	243	207	-36
{ 7.417	322	260	-62
{ 7.429	237	164	-73
{ 8.000	146	195	+49
{ 8.091	348	347	- 1
9.750	323	322	- 1
11.000	226	236	+10
{ 12.000	206	273	+67
{ 12.143	263	272	+ 9
{ 12.667	64	68	+ 4
{ 13.000	355	294	-61
{ 15.000	278	14	+96
{ 15.200	247	305	+58
{ 15.500	209	197	-12
{ 17.333	251	228	-23
{ 17.500	236	193	-43
20.000	174	147	-27
{ 35.000	136	167	+31
{ 36.000	138	93	-45

The following candidates were elected Fellows of the Society :—

Ernest F. Bone.
 Alan Mitchell Chequer, A.C.I.S.
 George Cooper.
 Thomas James Cummins.
 William Richard Easson, F.F.A.

William King, M.A.
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