Transactions of the Astronomical Observatory of Yale University

THE EVIDENCE FOR CHANGES IN THE RATE OF ROTATION OF THE EARTH AND THEIR GEOPHYSICAL CONSEQUENCES, WITH A SUMMARY AND DISCUSSION OF THE DEVIATIONS OF THE MOON AND SUN FROM THEIR GRAVITATIONAL ORBITS

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LANCASTER PRESS, INC.
LANCASTER, PA.

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I. INTRODUCTION AND SUMMARY [1]

1. This work was undertaken as a fresh attempt to see whether further light could be thrown on the question of the constancy of the earth's rate of rotation, that is, of our measure of time. The chief evidence for changes of this character is derived from observations of the moon. I am not here mainly concerned with the secular changes due to tidal friction, but with the considerable fluctuations which are exhibited in the difference between the observed and calculated longitudes of the moon when all known causes of variation have been eliminated. The numerous investigations into ancient eclipses culminating in the results of Fotheringham, and the work of Taylor and Jeffreys on Tidal Friction in shallow seas, are in substantial agreement as to the amount of the frictional effect, so that it may be regarded as known. The tabular place of the moon due to gravitational theory is therefore first corrected for this effect, the fluctuations referred to being the differences between this corrected theory and observation.

Since all classes of observations of the moon in all places show these deviations, there is practically no doubt that they are real, that is, that they are not due to errors of observation, the latter being in general small compared with the magnitude of the deviations. Nevertheless, the differences between the results obtained from various methods of observation and at different places show some peculiarities which are brought out below for examination in order to clear the ground.

The main issue is whether these fluctuations are due to forces, not hitherto recognized, acting on the moon so as to alter its motion, or whether they are changes in the rate of rotation of the earth. Either hypothesis will account for the observed data so far as the moon is concerned. Distinction between them can only be made by observations on other bodies, the most important of which, on account of the material available, is the sun. The conclusion reached here is that the evidence favors the latter hypothesis. This being granted, a discussion is made of the possible sources of changes in the rate of rotation of the earth with such numerical evidence as could be obtained.

2. The following brief account of the history of the discovery and of the attempts at explanation of these fluctuations will assist in forming an estimate of the degree of reliability of the evidence.

Newcomb first came across traces of the fluctuations in Part I of his Researches on the motion of the moon,[2] in which he discussed various observations made before the year 1750. By 1903, in a brief paper [3] he was able to show that they had a real existence, and six years later he gave a summary [4] of the second part of his Researches [5] which finally appeared in 1912. He there showed that the chief fluctuation appeared to be well represented by a harmonic term with a period of

[1] This Introduction has been printed in Proc. Nat. Acad. for June, 1926.

[2] Washington Observations, App. 2, 1875.

[3] M. N. R. A. S., volume 63, page 316.

[4] M. N. R. A. S., volume 69, page 164.

[5] Papers American Ephemeris, volume 9, part 1.

some 270 years and an amplitude of about 14″. Superimposed on this were minor fluctuations of some three or four seconds amplitude but irregular in period. Newcomb discussed the origin of these terms and, in view of the attempts to determine the complete gravitational solution of the lunar problem, leant strongly towards the hypothesis that they represented changes in the earth's rate of rotation. He mentions that he tried to find confirming results from the transits of Mercury but that he was not successful.

The next extensive investigation was that of Cowell, which was published in a series of papers in the Monthly Notices from 1903–5. In these he analyzed the Greenwich meridian observations of the moon from 1750 to 1900. Newcomb's results were confirmed and Cowell attempted an analysis of the fluctuations into two or three harmonic terms. Shortly after, in 1913, my theory of the gravitational problem of the moon's motion being complete, and the new tables being under way, I took Cowell's results, and corrected them so as to make them correspond as far as necessary with the new theory, mainly in order to get the best possible values of the principal constants. Incidentally, a comparison with Newcomb's results was made and a detailed determination of the minor fluctuations from both sources resulted. In general the agreement was good except in the interval between 1815 and 1845 when there were divergences which I was unable to trace.

In 1923, Dyson and Crommelin took the results of Cowell and myself, carried them to the end of 1922, corrected them in certain particulars, and made a full analysis of the minor fluctuations. Shortly after, H. Spencer Jones published his analysis of the Cape occultations from 1880 to 1922, mainly with a view of testing the adopted constants of the theory, but also obtaining the run of the fluctuations during that period. A further short but useful series is that deduced by Russell from photographs taken at Harvard College Observatory during the years 1911–17.

3. In the meantime attempts were made to see if these outstanding fluctuations could be explained as deviations of the moon's motion. The detailed theory of the sun's action on the moon and searching investigations into planetary and other gravitational effects made it finally clear that all such sources must be excluded. In 1910, I examined [6] some hypotheses designed to explain them, postulating forces other than gravitational, but with little success. The answer to a question sometimes asked as to whether the Einstein effect might be responsible is clearly in the negative. One reason is that the resulting perturbation is too small. A second, which applies to many similar hypotheses, is that small disturbing forces which are functions only of the configuration of the system will not in general produce new periods, but only combinations of periods already present, and the periods which might be present in the fluctuations do not seem to have any such property.

4. It appears therefore that we must fall back on the hypothesis that the fluctuations are due to changes in the earth's rate of rotation. In 1914, I was able to throw some light on the question by exhibiting evidence of similarity between the minor fluctuations exhibited by the moon's motion and the deviations of the sun and Mercury from their theoretical orbits during the period 1750 to 1900.[7] The following year, Larmor [8] examined the hypothesis that changes in the earth's rate of rotation might be caused by vertical oscillations of limited areas but found that the amounts

[6] Amer. Jour. Science, volume 29, page 529.

[7] Brit. Ass. Report, 1914, page 311.

[8] M. N. R. A. S., volume 75, page 211.

to be postulated were not in accordance with geophysical evidence. The same year, H. Glauert [9] showed that a marked similarity between the fluctuations for the interval 1865–1915 existed in the case of material derived from Greenwich observations of the sun, Venus, and Mercury, and he also showed that their order of magnitude was not very different from that to be expected on the hypothesis of changes in the earth's rate of rotation. In 1916, Ross [10] showed that Mars during the nineteenth century exhibited fluctuations very similar to those of the sun. In 1925, Innes [11] gave additional evidence from the Transits of Mercury (see § 27).

5. Thus evidence has gradually accumulated which seems to justify the hypothesis. In view of the new material on the moon's motion which has been assembled during the past few years, it seemed worth while to examine the material once again in a collected form. This first demanded a detailed comparison of the observations of the moon with theory, which, as already stated, was in large measure already available, and with this an intercomparison of the various series of observations to determine if possible the sources of systematic differences. To test the hypothesis, the fluctuations should be visible in the motion of the sun, but only to one thirteenth of the amount. It is necessary to combine with them the secular acceleration of the sun discovered by Cowell in 1905 and later verified by Fotheringham from a discussion of ancient and modern observations; the results were in large measure confirmed by Jeffreys in his calculations of the tidal friction in shallow seas. This demanded an examination of the sun's deviations from its tabular positions, and here again most of the material had been made available by Newcomb and Ross. While in the previous attempts the effect of the minor fluctuations or of the secular acceleration had been examined, both effects, namely, the total fluctuations as determined from the moon, added to the sun's secular acceleration, do not seem to have been simultaneously applied to the observations of the sun.

6. The results obtained from the present examination may be briefly summarized as follows.

(*i*) Observations of the sun since 1750 are consistent with the hypothesis that the observed fluctuations of the moon are due to variations in the earth's rate of rotation, when these are combined with the known effects of tidal friction. In particular, the hypothesis accounts for the principal part of the sudden change in the deviations of the sun from its tables which began in 1900 and which is at present greater than $1''$. The previously obtained correlations between these differences and those deduced from observations of the planets are also consistent with the hypothesis but are partly to be explained by common systematic errors of observation.

(*ii*) Granting the hypothesis, arguments resulting from astronomical data, mechanical laws, and geophysical evidence point to the conclusion that such variations can only arise from vertical oscillations of the whole crust of the earth, the source of these oscillations being due to oscillatory changes taking place in the isostatic layer or below it. The maximum change of the average radius needed by the astronomical data is between five inches and twelve feet, according to the hypothesis made as to the depth of the source.

(*iii*) Evidence in favor of the following correlations appears to be sufficiently good for an explanation of them to be needed.

[9] M. N. R. A. S., volume 75, page 489.

[10] A. J., volume 29, page 149.

[11] A. N., volume 225, page 109.

The differences between the Greenwich meridian observations of the moon and the occultations as reduced by Newcomb are affected with a fluctuating error which has the same principal maxima and minima as the minor fluctuations themselves, but the ratio of the amplitude of the former to that of the latter diminishes from about unity at the beginning of the nineteenth century to 1/8 at the present time.

(*iv*) A fluctuation with a shorter period in these same differences appears to be related to the frequency of occurrence of British earthquakes.

(*v*) During the whole run of the Washington observations of the sun (1846 to date), the differences between them and the tabular values follow the same major fluctuations as the differences between the Greenwich observations of the sun and the tables. The change appears to have a period of about forty years.

(*vi*) The large deviations of the Greenwich observations of the sun from the tabular values between 1805 and 1825, taken in conjunction with the similar large deviations of the Greenwich meridian observations of the moon from the occultations, point to the conclusion that the meridian observations are defective in this interval and that it is unsafe to make use of them with full weight for the discussions of tabular differences, unless the source of the apparent errors can be found and the errors, if existent, corrected. The indications are that for discussion of variations of the earth's rate of rotation the occultations rather than the meridian observations of the moon should be used in this period. It follows that harmonic analyses based on the meridian observations before 1850 are of doubtful reality and that the minor fluctuations are much more irregular in character than had previously been suspected.

(*vii*) It is doubtful whether attempts at harmonic analysis of the fluctuations will give any clues as to their origin.

(*viii*) A somewhat doubtful correlation is that between the Greenwich (and Washington) tabular differences for the sun and the differences between the Greenwich meridian observations of the moon and the occultations since 1870. Another, which may be accidental, between the same series appears before 1840, but it requires a lag of ten years in the former in order to make it correspond with the latter.

7. In my discussion on the forces which can change the earth's angular velocity, in which I have attempted to prove that the only hypothesis which can account for the facts is an oscillatory change in the earth's mean radius, two calculations have been made. In one the earth is supposed to expand and contract throughout its whole mass uniformly. In the other, all the expansion and contraction is supposed to be due to changes of pressure in a layer 80 km deep, changing the height of the crust above it. These are set down merely as outside limits which are in accordance with the knowledge we have at present concerning the earth. The limits can possibly be narrowed, but I am not concerned here with attempts to do so, nor have I tried to make any suggestion as to the physical or chemical mechanism by which such expansions or contractions can be produced. As I view the problem, the argument depends on a chain of reasoning which appears to require the existence of some such force well below the crust of the earth. The strength of the chain depends on the strength of its weakest link. The chief steps are as follows.

(*a*) The astronomical data. There can be little doubt that the principal part of the apparent variation in the moon's motion is real and not due to errors of observation.

(*b*) The evidence that the observed variation is a variation in the rate of rotation of the earth and not in the rates of revolution of the moon, sun, and planets about the earth. The greater part of this paper is devoted to setting forth the evidence for this, involving attempts to distinguish between systematic errors of observation and real differences between gravitational theory and observation in the motions of these bodies, especially in that of the sun.

(*c*) The arguments by which sources for these variations are traced to forces well below the outer crust of the earth. The evidence is largely negative, that is, it consists of attempts to show that, unless matter acts on matter in some way at present quite unknown, the forces cannot proceed from any other source.

(*d*) Assuming the last result as provisionally established, the collateral evidence which can be brought forward to support it in the form of correlations between geophysical and astronomical data, and simplified explanations of geodynamical questions.

An oscillation with a period between 200 and 300 years in the frequency of Chinese earthquakes brought forward by H. H. Turner in 1920, and compared by him with the principal fluctuation in the moon's motion, may be significant in this connection.

8. As the main argument must ultimately depend on the numerical data, the latter have been set out in detail as far as possible with full references to their sources, and with an exact account of what changes or corrections have been made. The treatment of these data has necessarily been such as to bring out as prominently as possible the facts needed for the argument. That it also brings out other results for which explanation is not forthcoming is inevitable. The question as to whether the latter are relevant to the main issue is discussed in some detail.

9. While the discussion of the effect of changes in the external radius of the earth would lead to the expectation of correlation between geophysical and astronomical data, the mechanism which is responsible for a correlation between the errors (if existent) of the Greenwich observations and the frequency of certain seismic data is not apparent. Owing to the lack of uniformity of the earth's crust at least as far down as the isostatic layer of compensation, we may expect differential vertical motions of the crust superimposed on the main vertical oscillation common to the whole earth. The magnitude of that oscillation (exhibited by the moon), which is correlated below with the frequency of occurrence of British earthquakes, seems to be rather greater than can be attributed to changes of the vertical at Greenwich. Such an explanation would seem to demand corresponding changes in the sun's motion, and there is not much, if any, evidence of them. If, however, as seems likely, the effect on the latter is only 1/13 of that on the former, their absence would be explained. The well-established correlation of the differences between the Greenwich meridian observations and the occultations, with the differences between the observations and theory, would seem to result from the same cause, but it is difficult to suggest any reasonable hypothesis to account for the manner in which it is brought about.

Attempts were made to connect these differences with those obtained by Sampson [12] in the time determinations at different observatories. His proof that, at least at Edinburgh,[13] they were connected with level errors seemed to hold out some promise and I made attempts at correlation between his results and those simultaneously furnished by observations of the moon and sun, but obtained nothing of sufficient significance to reproduce.

[12] M. N. R. A. S., volume 81, page 89.

[13] M. N. R. A. S., volume 85, page 560.

10. A theory by Joly,[14] reported in the Observatory for 1926, February, also postulates vertical oscillation of the whole crust. The basis appears to be its value in explaining the phenomena of dynamic geology. He believes that the cause is to be found in the thermal effects of radium acting in a substratum of basalt at a depth of some 30 kilometers and that it is measured in time by geological periods. This basalt was alternately liquid and solid; at the present time it is solid. In this paper I deduce from astronomical data, secured during the past two centuries, oscillations measured by years and by one or two centuries, and place the origin at least at a depth of 80 or 90 kilometers, but make no hypothesis as to their cause. The nature of the oscillation which he deduces is therefore quite different from that of this paper.

[14] The Halley Lecture for 1925.

II. THE MOON'S LONGITUDE

11. The principal material available to determine the path of the moon is as follows:

(G.) The Greenwich meridian observations from 1750 to 1922. These were partially corrected for errors of theory by Cowell in a series of papers published in the Monthly Notices during the years 1903–5, further corrected by myself in 1913,[1] and finally by Dyson and Crommelin in 1923.[2] The last named memoir will be adopted here as the best representation of the Greenwich meridian tabular minus observed errors of the moon's place.

(N.) Newcomb's great work [3] on the occultations extending from 1621 to 1908, with a correction given in my longitude paper; the results from 1750 in this paper will be used below, those given in Newcomb's paper for dates before 1750 being retained without further change except to make them consistent with the remainder.

(Cp.) The Cape occultations from 1880 to 1922 [4] reduced by H. Spencer Jones.

(H.) The Harvard photographic results[5] from 1911 to 1917. My comparison with the Greenwich observations will be used.[6]

(1923–5) Results obtained at Greenwich, Washington, Johannesburg, and New Haven from the beginning of 1923, when the new tables of the moon came into use, to date, references to which are given below.

12. In order to compare the results from the different series of observations with one another and with gravitational theory (including the effects of tidal friction) the material must, as far as possible, be made uniform. Most of the work for this purpose has already been done in the published results, either by correcting the observations individually or in groups. While some lack of uniformity probably still exists, the actual errors in the annual means due to different methods and sets of corrections are probably not large and can be regarded as accidental rather than systematic.

The additions required in order to obtain the differences between observation and gravitational theory are as follows:

(G.) The Greenwich residuals do not include an empirical term which has for its expression $13''.60 \sin (139^\circ T + 104^\circ.2)$. We must therefore add to the tabular minus observed residuals the term

$$\text{G.E.T.} = 13''.60 \sin (139^\circ T + 284^\circ.2),$$

where T is the number of centuries from 1800.0, and the letters G.E.T. stand for 'great empirical term.'

[1] M. N. R. A. S., volume 73, page 692. This will be referred to below as my "longitude" paper.

[2] M. N. R. A. S., volume 83, page 359, and App. G of the Greenwich observations for 1920.

[3] Papers American Ephemeris, volume IX, part 1, 1912.

[4] M. N. R. A. S., volume 85, page 21, 1924; Cape Observatory Annals, volume 8, part 8, 1925.

[5] Harvard Annals, volumes 76, 80, 81.

[6] A. J., volume 36, page 153.

TABLE I

THEORY — GREENWICH MERIDIAN OBSERVATIONS = TH. — G

Per.	Date	G.E.T.	Th. — G	Per.	Date	G.E.T.	Th. — G
		″	″			″	″
1	1751.3	− 8.12	− 5.85	45	1801.2	−13.10	−13.06
2	2.4	− 8.42	− 9.22	6	2.3	−13.00	−12.28
3	3.6	− 8.71	− 9.39	7	3.4	−12.88	−12.27
4	4.7	− 8.99	− 9.26	8	4.6	−12.75	−11.26
5	5.8	− 9.26	−10.31	9	5.7	−12.61	−10.73
6	7.0	− 9.53	− 9.66	50	6.8	−12.45	−11.69
7	8.1	− 9.80	− 9.31	1	8.0	−12.30	−11.36
8	9.2	−10.06	− 8.46	2	9.1	−12.14	−11.92
9	1760.4	−10.30	− 9.08	3	1810.2	−11.97	−11.67
10	1.5	−10.53	− 9.79	4	1.4	−11.78	− 9.79
1	2.6	−10.76	−10.11	5	2.5	−11.58	− 9.51
2	3.8	−10.99	− 9.22	6	3.6	−11.39	− 9.24
3	4.9	−11.21	−12.33	7	4.8	−11.19	− 7.46
4	6.0	−11.41	− 9.92	8	5.9	−10.97	− 8.96
5	7.2	−11.60	−11.00	9	7.0	−10.74	− 8.25
6	8.3	−11.79	−11.48	60	8.2	−10.50	− 8.23
7	9.4	−11.97	−12.95	1	9.3	−10.25	− 6.60
8	1770.6	−12.15	−14.02	2	1820.4	−10.00	− 7.07
9	1.7	−12.31	−12.47	3	1.6	− 9.74	− 7.43
20	2.8	−12.46	−11.62	4	2.7	− 9.48	− 7.49
1	4.0	−12.61	−12.76	5	3.8	− 9.20	− 8.03
2	5.1	−12.75	−13.69	6	5.0	− 8.93	− 8.18
3	6.2	−12.89	−14.33	7	6.1	− 8.65	− 7.72
4	7.4	−13.00	−12.93	8	7.2	− 8.35	− 7.54
5	8.5	−13.10	−13.43	9	8.4	− 8.06	− 6.87
6	9.6	−13.20	−15.93	70	9.5	− 7.75	− 7.58
7	1780.8	−13.29	−14.42	1	1830.6	− 7.45	− 6.50
8	1.9	−13.36	−13.29	2	1.8	− 7.13	− 5.50
9	3.0	−13.41	−15.34	3	2.9	− 6.81	− 4.59
30	4.2	−13.46	−15.49	4	4.0	− 6.48	− 4.88
1	5.3	−13.50	−14.73	5	5.2	− 6.14	− 4.56
2	6.4	−13.54	−14.69	6	6.3	− 5.81	− 4.14
3	7.6	−13.57	−14.41	7	7.4	− 5.47	− 3.81
4	8.7	−13.59	−16.54	8	8.6	− 5.12	− 3.38
5	9.8	−13.60	−16.05	9	9.7	− 4.77	− 3.94
6	1791.0	−13.59	−16.35	80	1840.8	− 4.42	− 3.20
7	2.1	−13.57	−15.94	1	2.0	− 4.06	− 3.55
8	3.2	−13.54	−16.22	2	3.1	− 3.70	− 3.80
9	4.4	−13.50	−16.09	3	4.2	− 3.34	− 3.75
40	5.5	−13.46	−14.86	4	5.4	− 2.97	− 4.29
1	6.6	−13.41	−12.02	5	6.5	− 2.61	− 3.04
2	7.8	−13.36	−12.99	6	7.6	− 2.24	− 3.37
3	8.9	−13.29	−14.03	7	8.8	− 1.87	− 4.11
4	1800.0	−13.20	−12.95	8	9.9	− 1.51	− 3.40
				9	1851.0	− 1.14	− 3.13

Following Cowell, the results by Dyson and Crommelin are given for 'periods' of approximately 400 days. Table I above contains the number of the period, the date of the middle of the period, the value of G.E.T. for this date, and the result of adding G.E.T. to the residuals given by Dyson and Crommelin in the last column of the table on page 362 of their paper in the Monthly Notices. This table includes the observations from 1750 to 1850.

After 1850, the residuals given by Dyson and Crommelin in the last column of the table on page 363 of the same paper have been interpolated to years and the term G.E.T. added. The results are placed in the third column of Table III below, the values of G.E.T. being shown in the second column, the epoch in each case being the middle of the year.[7]

(N.) Newcomb's occultation residuals before 1750 [8] have so little weight that I have only corrected them for the difference between the mean motion used by him (l. c., page 210) and that used here.[9] After changing the signs of his 'total fluctuations,' I add to them this difference, namely,

$$\delta_1 T = +1''.1 - 0''.8T - 2''.8T^2,$$

the results being given in column 4 of Table II below.

TABLE II

THEORY – OCCULTATIONS (1621–1850) = Th. – N

Date	$\delta_1 T$	Th. – N	Date	$\delta T - ''.2$	G.E.T.	Th. – N
				″	″	″
1621	−7″	−22″	1755	−0.2	− 9.1	− 9.2
35	−5	+ 4	71	+ .1	−12.2	−13.3
39	−5	+30	84.7	+ .1	−13.5	−15.1
45	−5	+13	92	0	−13.5	−13.8
53	−4	+21	1801.5	− .1	−13.0	−12.5
62	−3	+16	09.5	− .2	−12.1	−12.0
81	−1.9	+12.8	13.0	− .2	−11.4	−11.0
1710	− .5	+ 3.7	21.0	− .3	− 9.8	− 9.9
27	+ .2	− 1.5	22.5	− .4	− 9.5	− 9.8
37	+ .5	− 4.4	29.5	− .5	− 7.8	− 6.7
47	+ .7	− 7.5	33.5	− .6	− 6.4	− 6.2
			38.5	− .7	− 5.0	− 5.1
			40.5	− .7	− 3.8	− 4.7
			46.5	− .7	− 2.7	− 3.9
			48.5	− .7	− 2.0	− 4.7
			49.5	− .7	− 1.6	− 2.9
			50.5	− .7	− 1.3	− 3.8

[7] The value for period 143 has been changed from − 354 to − 230 (communicated by Sir Frank Dyson). In my longitude paper, I unfortunately combined the equinox corrections on page 700 with the tabular correction to the Hansen series. This seems to have been overlooked by Dyson and Crommelin in their reductions of the annual means to my theory on page vi of their memoir in Appendix G of the Greenwich observations for 1920, but was pointed out and corrected in their Monthly Notices paper. It seems also to have been overlooked by Jones in his comparisons with Hansen and Greenwich, but of course does not affect the results by him used here.

[8] Papers American Ephemeris, volume IX, part 1, page 211.

[9] A. J., volume 34, page 53.

TABLE III

THEORY MINUS OBSERVATION, 1850–1925

Date	G.E.T.	Th. – G	Th. – N	Th. – C	N – G	C – G
	″	″	″		″	
1850.5	− 1.32	− 3.26	− 3.8		+ .5	
1	− .99	− 3.58	− 3.5		− .1	
2	− .66	− 3.83	− 3.4		− .4	
3	− .33	− 3.43	− 2.8		− .6	
4	.00	− 3.46	− 3.0		− .5	
5	+ .33	− 3.37	− 3.1		− .3	
6	+ .66	− 3.16	− 2.3		− .9	
7	+ .99	− 2.76	− 1.8		−1.0	
8	+ 1.31	− 2.35	− 2.5		+ .1	
9	+ 1.63	− 2.05	− 2.3		+ .3	
1860.5	+ 1.96	− 1.82	− 1.7		− .1	
1	+ 2.29	− 1.61	− .6		−1.0	
2	+ 2.62	− 1.46	− .8		− .7	
3	+ 2.94	− 1.46	+ .3		−1.8	
4	+ 3.27	− .80	+ .4		−1.2	
5	+ 3.59	.00	+ .8		−0.8	
6	+ 3.81	+ .82	+ .6		+ .2	
7	+ 4.12	+ 1.62	+ 2.2		− .6	
8	+ 4.52	+ 2.45	+ 2.4		− .1	
9	+ 4.83	+ 3.10	+ 1.8		+1.3	
1870.5	+ 5.13	+ 3.75	+ 3.0		+ .8	
1	+ 5.43	+ 4.81	+ 5.2		− .4	
2	+ 5.74	+ 5.33	+ 6.1		− .8	
3	+ 6.05	+ 6.52	+ 6.6		− .1	
4	+ 6.34	+ 7.71	+ 7.7		0	
5	+ 6.63	+ 8.55	+ 8.4		+ .2	
6	+ 6.92	+ 8.80	+ 8.9		− .1	
7	+ 7.21	+ 8.95				
8	+ 7.48	+ 9.56	+ 9.6		− .3	
9	+ 7.75	+ 9.65	+ 9.1		+ .6	
				″		″
1880.5	+ 8.01	+ 9.91	+10.4	+10.45	− .5	− .54
1	+ 8.27	+10.06	+10.8	+10.87	− .7	− .81
2	+ 8.53	+10.43	+10.8	+10.75	− .4	− .33
3	+ 8.78	+10.85	+11.6	+12.16	− .8	−1.31
4	+ 9.02	+11.36	+11.5	+10.41	− .1	+ .95
5	+ 9.26	+12.14	+11.8	+12.30	+ .3	− .16
6	+ 9.50	+12.63	+12.1	+11.52	+ .5	+1.11
7	+ 9.74	+12.59	+11.9	+11.73	+ .7	+ .86
8	+ 9.98	+12.99	+12.9	+12.43	+ .1	+ .56
9	+10.20	+13.04	+13.0	+12.97	0	+ .05
1890.5	+10.41	+13.20	+13.2	+12.90	0	+ .30
1	+10.62	+14.05	+13.4	+13.70	+ .7	+ .35
2	+10.83	+14.89	+13.4	+14.22	+1.5	+ .67
3	+11.03	+15.24	+14.0	+14.32	+1.2	+ .92
4	+11.22	+15.35	+15.0	+15.09	+ .4	+ .26

TABLE III (*continued*)

Date	G.E.T.	Th. − G	Th. − N	Th. − C	N − G	C − G
	″	″	″	″	″	″
1895	+11.40	+15.44	+14.8	+14.85	+ .6	+ .59
6	+11.58	+15.73	+14.3	+15.06	+1.4	+ .67
7	+11.75	+15.76	+14.7	+15.11	+1.1	+ .65
8	+11.91	+15.36	+15.5	+15.21	− .1	+ .15
9	+12.06	+15.21	+15.9	+14.94	− .7	+ .27
1900.5	+12.20	+15.03	+14.6	+14.66	+ .4	+ .37
1	+12.34	+14.72	+14.7	+14.39	0	+ .33
2	+12.50	+14.30	+14.2	+13.75	+ .1	+ .55
3	+12.62	+13.85	+13.9	+13.16	0	+ .69
4	+12.73	+13.50	+13.2	+12.65	+ .3	+ .85
5	+12.84	+12.98	+12.7	+12.85	+ .3	+ .13
6	+12.95	+12.58	+12.8	+12.73	− .2	− .15
7	+13.05	+12.48	+12.1	+11.99	+ .4	+ .49
8	+13.14	+12.32	+12.1	+11.75	+ .2	+ .57
9	+13.21	+12.22		+11.11		+1.11
1910.5	+13.28	+11.80	Th. − H	+11.05	H − G	+ .75
1	+13.35	+11.27	+11.63	+10.40	− .36	+ .87
2	+13.41	+10.93	+10.93	+10.43	0	+ .50
3	+13.46	+10.46	+10.36	+ 9.70	+ .10	+ .76
4	+13.50	+ 9.80	+ 9.81	+ 9.81	− .01	− .01
5	+13.54	+ 9.71	+ 9.65	+10.06	+ .06	− .35
6	+13.57	+ 9.30	+10.45	+ 9.59	−1.15	− .29
7	+13.59	+ 9.16	+ 9.50	+ 9.03	− .34	+ .13
8	+13.60	+ 9.45		+10.30		− .85
9	+13.59	+ 9.56		+ 9.09		+ .47
1920.5	+13.58	+ 9.41		+ 9.88		− .47
1	+13.57	+ 9.42		+ 9.90		− .48
2	+13.56	+ 9.17	Th. − W	+ 9.94		− .77
3	+13.53	+10.73	+10.70		J − G	
4	+13.49	+10.80	+10.82		+ .8	E − G
5	+13.44	+11.14			+ .6	+ .6

From 1750 to 1850, the corrected group results in my longitude paper are used. To them are added G.E.T. and $\delta T - 0''.16$, where δT is tabulated in my paper just referred to and $-0''.16$ is an addition to it made by Dyson and Crommelin. The values of G.E.T. for the epoch dates of Newcomb's groups and the resulting residuals are shown in Table II.

As I pointed out in my longitude paper, the extent of Newcomb's groups is not quite known, so that the proper values of $\delta T - 0''.16$ to be applied are slightly doubtful, but the possible errors due to this cause are small compared with the probable errors of the residuals.

After 1850, Newcomb's annual values are taken from the corrected means given on page 707 of my longitude paper. To these have been added $\delta T - 0''.16$ and G.E.T., the results being placed in the fourth column of Table III.

(Cp.) The Cape occultations as given by Jones in the second column of his Table VI require their signs to be changed and the addition of G.E.T. The results are given in the fifth column of Table III.

(H.) The results from the Harvard photographs were obtained as annual mean differences in right ascension from the Greenwich meridian observations. After multiplication by 15 to reduce to seconds of arc, these were considered as differences of longitude and the results given in column 4 of Table III were deduced directly from the values for the corresponding years in column 3.

(G.) For the years 1923, 4, the Greenwich mean annual results are taken from the Annual Reports, that for 1925 being kindly communicated by the Astronomer Royal.

(W.) The Washington results for 1923, 4, were published[10] as differences of right ascension and declination. I have transformed these to differences of mean longitude in a paper to appear shortly.

(J.) The Johannesburg reductions of occultations for 1923, 4 have been published by Innes.[11]

(E.) The results from the eclipse of 1925 January 24 are from a paper to appear shortly in the Astronomical Journal.

All of these require the addition of $\delta T - 0''.16$ and G.E.T. The results after receiving these additions are shown in Table IV, the last line of the sixth column giving results obtained during the first two lunations of 1925, and that in the last column the result obtained from the eclipse.

In the later columns of Tables II and III are shown differences between the results of the separate series. To obtain the differences in Table II, means were found of the results obtained in Table I to cover what appeared to be the extent of the groups of Table II, also using the epoch date of each group. The remaining difference columns require no explanation.

13. In order to bring out certain features of the results which are not very evident from the numerical tables some graphs are shown.

Figure 1 contains the results in the last column of Table I, and the fourth column of Table II, that is, the complete residuals as deduced from the Greenwich meridian observations and Newcomb's occultations from 1662 to 1850. The first five groups of the occultations have been given such small weight by Newcomb that they must be regarded as indications rather than values, and they are not shown in the figure. After 1850, the Greenwich results from the third column of Table III alone are plotted since, on the scale of the figure, the differences between them and other observations are not easily perceptible. The inclined straight lines are merely inserted as guides to the eye.

The principal results in Tables II, III are plotted in Figure 3. The full line upper curve shows the 'minor fluctuations' from 1750 to date as deduced from the Greenwich series, i.e., column 4 — column 3 of Table I and column 3 — column 2 of Table III, plotted by averages of four periods before 1850 and of five years after 1850. The dotted line shows the occultation results from 1750 to 1850, as similarly deduced from Table II.

The middle curve exhibits the difference, occultations minus Greenwich, obtained from Table II before 1850 and by overlapping five year means from Table III after 1850. The letters C show the positions of the overlapping five year means of Cape minus Greenwich, the letters H, three and four year means from the Harvard minus Greenwich series, while W, J, E show the means deduced from two Washington years, the Johannesburg occultations, and the eclipse series of occultations, all minus the corresponding Greenwich values.

[10] A. J., volume 36, page 77.

[11] A. J., volume 36, page 119.

III. THE SUN'S LONGITUDE

14. The greater part of the material used to find the deviations in longitude of the observed positions of the sun from its tabular place was collected by Newcomb and published in the first chapter of his "Astronomical Constants" which appeared in 1895. In 1916, this material was reduced by F. E. Ross [1] to Newcomb's system and Newcomb's tables of the sun, and it is the results of this latter paper which I adopt here.

The tentative hypothesis of the present memoir is that the total apparent deviations of the moon from its gravitational orbit (apart from observational errors) are due to changes in the rate of rotation of the earth. If so, we should find similar changes in the apparent position of the sun. In the comparisons of observation with gravitational theory by Ross the effect of the acceleration produced by tidal friction is not included and must therefore be added to the fluctuations deduced from the moon's orbit in order to test the hypothesis.

15. The first astronomical evidence of this acceleration was obtained by Cowell [2] who deduced it from an attempt to reconcile ancient solar eclipses with modern gravitational theory. The latest calculation by Fotheringham [3] probably represents the best that can be deduced from the material available. The terrestrial evidence was furnished by H. Jeffreys, who has also given an excellent summary of the whole matter in Chapter XIV of his volume "The Earth."

Fotheringham deduces a term $1''.5T^2$ but states that any value between $1''.0T^2$ and $1''.7T^2$ will satisfy the observations.

Jeffreys shows that, accepting the figure $4''.5T^2$ for the apparent effect of tidal acceleration [4] on the longitude of the moon—an amount consistent with both astronomical and terrestrial evidence—the effect on the longitude of the sun will be $0''.9T^2$ or $0''.4T^2$ according as we attribute the whole frictional effect to the moon or the sun. Fotheringham's lower limit is therefore somewhat larger than Jeffreys' upper limit, as the latter points out.

However, since, as far as the observations of the sun are concerned, a difference of $0''.2$ cannot at present be detected, I shall adopt the round number $1''.00T^2$ which takes care of the principal part of the effect. Since the mean motion and value at epoch of the longitude are in any case deduced from observation I add $0''.80T - 0''.65$ (epoch 1800). This last addition is an approximate estimate derived graphically. In the present state of the observational material, as will be seen from Figure 2 and from the discussion below, a more systematic method of deriving this correction has no real meaning.

[1] A. J., volume 29, page 152.

[2] M. N. R. A. S., volume 66, page 3.

[3] M. N. R. A. S., volume 81, page 104.

[4] The unfortunate frequent astronomical use of the word 'acceleration' to represent what is really half the acceleration has led to occasional error. If a be the real acceleration, the effect on the longitude is of course $\frac{1}{2}at^2$. The value of $\frac{1}{2}a$ rather than that of a is often quoted. Ross, in his paper, has applied twice Fotheringham's value in an attempt to find whether the evidence from the sun's motion favors the hypothesis of tidal friction. In the present text I have given the actual addition to be made to the longitude in order to avoid misconception.

16. With the present hypothesis, I suppose that the fluctuations of the earth's rate of rotation are derived from observations of the moon. In order to obtain the effect on the apparent position of the sun we must add to the tabular position of the latter an amount equal to .075 of that added to the tabular position of the moon, this number being the ratio of the mean motions of the two bodies. The question as to whether the lunar fluctuations shall be derived from the meridian observations or from the occultations is of no significance since, where the two sets sensibly differ, the observational material for the sun differs from the tables to a much greater extent. Actually, I have used the occultations for the purpose.

The total addition to the tabular minus observed results given by Ross is, under these hypotheses,

$$.075(\text{T.F.M.}) + 1''.00T^2 + 0''.80T - 0''.65, \qquad \text{(S.E.)}$$

where T.F.M. (total fluctuation of the moon) represents the results in column 3 of Table II and column 4 of Table III, and T is reckoned in centuries from 1800.0. These terms will be referred to as the 'sun effect' (S.E.). The total fluctuations before 1850 were plotted from Newcomb's groups and a curve drawn through them. From this curve means for the different groups of observations of the sun were formed and the results are shown in the columns headed S.E. below. In each case the third column is that given by Ross (with changed signs) and the fourth the sum of this and the column S.E. For the later years Ross gives the annual errors in right ascension. I have assumed that these may be taken to be the annual errors in longitude, with sufficient accuracy, and have combined them into groups of a few years each.

The Greenwich material available since the appearance of the paper by Ross has been extracted from the annual reports except that for the years 1924, 5, which was communicated by the Astronomer Royal. The additional material for Washington was communicated by Professor Eichelberger. A reference to the Washington results with the nine-inch transit circle (volume IX, part I, Ap. CXLVIII) shows that from 1903 to 1906 a correction of $-0^s.067$ and from 1907 to 1911 one of $-0^s.072$ is to be applied to the tabular minus observed Washington residuals, as given by Ross, on account of personal equation. This has been applied in the results which follow.

17. The last columns of the Table IV, except that for Palermo, have been plotted in Figure 2. The full line represents the Greenwich values, those for the other observatories being indicated by letters placed approximately in the positions which the residuals would occupy. The letters (W) in brackets are the Washington residuals before the corrections for personal equation, mentioned above, had been applied.

TABLE IV

RESIDUALS OF THE SUN'S LONGITUDE

Greenwich

Date	S.E.	Th. – O	G
	″	″	″
1750–62	–0.10	+0.63	+0.53
65–71	+ .09	+ .28	+ .37
72–78	+ .19	+ .35	+ .54
79–85	+ .32	–1.94	–1.62
86–92	+ .35	– .69	– .34
93–97	+ .33	–1.13	– .80
98–02	+ .31	– .3	0
1803–06	+ .31	+ .4	+ .7
07–10	+ .31	+1.0	+1.3
11–14	+ .31	+3.98	+4.29
15–18	+ .31	+2.19	+2.50
19–22	+ .31	+1.38	+1.69
23–26	+ .28	+1.91	+2.19
27–30	+ .24	+ .77	+1.01
31–34	+ .20	– .64	– .44
35–38	+ .19	–1.02	– .83
39–42	+ .20	– .27	– .07
43–46	+ .22	+ .16	+ .38
47–50	+ .27	– .10	+ .17
51–54	+ .29	– .29	0
55–58	+ .31	– .08	+ .23
59–62	+ .31	– .17	+ .14
63–66	+ .25	– .48	– .23
67–70	+ .20	– .45	– .25
71–74	– .02	– .06	– .08
75–78	– .12	– .09	– .21
79–82	– .12	– .35	– .47
83–88	– .12	+ .13	+ .01
89–92	– .09	+ .15	+ .06
93–96	– .08	+ .27	+ .19
97–00	– .01	+ .33	+ .32
1901–04	+ .17	– .04	+ .13
05–08	+ .42	– .49	– .07
09–12	+ .63	– .88	– .25
13–16	+ .86	–1.21	– .35
17–20	+1.01	–1.59	– .58
21–23	+1.10	–1.63	– .53
24–25	+1.08	–1.63	– .55

Paris

Date	S.E.	Th. – O	P
	″	″	″
1801–07	+0.31	+2.5	+2.8
08–15	+ .31	+1.17	+1.48
16–22	+ .31	+ .49	+ .80
23–29	+ .26	+ .44	+ .70
37–44	+ .20	+ .13	+ .33
45–52	+ .27	– .17	+ .10
53–59	+ .31	– .44	– .13
60–65	+ .27	– .59	– .32
66–70	+ .21	–2.42	–2.21
71–79	– .07	+ .06	– .01
80–89	– .12	– .60	– .72
90–93	– .09	– .81	– .90
97–99	– .04	– .25	– .29
1900–03	+ .12	– .64	– .52
04–06	+ .34	– .83	– .49

Washington

Date	S.E.	Th. – O	W
	″	″	″
1846–52	+0.27	+0.74	+1.01
61–65	+ .26	+ .27	+ .53
66–73	+ .13	+ .17	+ .30
74–81	– .12	+ .07	– .05
82–91	– .11	+ .73	+ .62
94–96	– .09	+1.46	+1.37
97–00	– .01	+ .77	+ .76
1901–04	+ .18	+ .11	+ .29
05–08	+ .42	– .20	+ .22
09–11	+ .58	– .56	+ .02
11–14	+ .75	–1.42	– .67
15–18	+ .93	–1.65	– .72

TABLE IV (*continued*)

Date	S.E.	Th. − O	C′
Cambridge			
	″	″	″
1828–34	+0.20	+0.34	+0.54
35–40	+ .19	+ .40	+ .59
42–47	+ .23	+ .14	+ .37
50–58	+ .30	+ .36	+ .66
Königsberg			K
1816–23	+ .31	+ .37	+ .68
24–30	+ .25	+ .43	+ .68
31–38	+ .19	− .10	+ .09
39–45	+ .21	− .35	− .14
Oxford			O
1840–49	+ .23	−2.39	−2.16
60–68	+ .25	−2.16	−1.91
69–76	+ .01	− .95	− .94
80–87	− .12	+ .15	+ .03

Date	S.E.	Th. − O	P′
Pulkowa			
	″	″	″
1842–50	+0.24	−1.15	−0.91
61–70	+ .27	+ .20	+ .47
Dorpat			D
1823–30	+ .26	+ .09	+ .35
31–38	+ .19	− .32	− .13
Cape			C
1884–90	− .11	+ .26	+ .15
Strassburg			S
1883–88	− .12	+1.58	+1.46
Palermo			
1791–96	+ .33	+ .86	+1.19
97–01	+ .31	+3.0	+3.3
1802–05	+ .31	+3.8	+4.1
06–12	+ .31	−5.2	−4.9

IV. DISCUSSION OF THE RESULTS

18. The main question at issue here is the effect of the hypothesis that the fluctuations exhibited in the moon's motion are due to variations in the rate of rotation of the earth. When these fluctuations are multiplied by the proper factor and combined with the effects of tidal friction, we obtain the term S.E. which is to be applied under the hypothesis to observations of the sun.

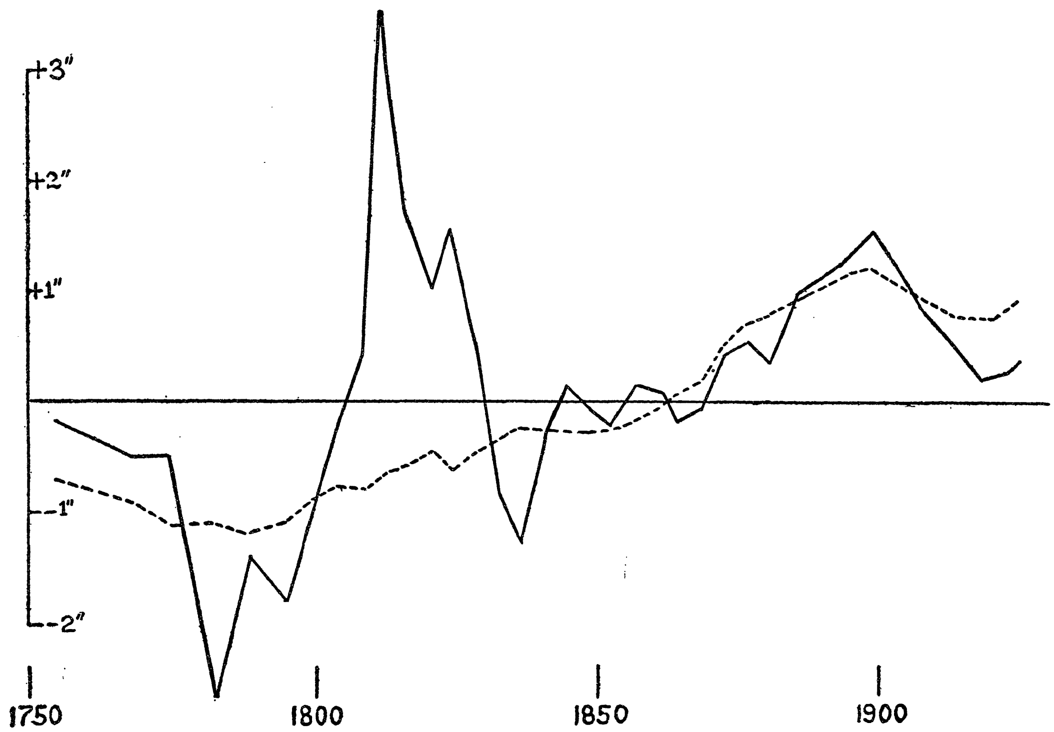

FIG. 4. Full line curve: the Greenwich tabular minus observed errors of the Sun including the secular acceleration. Dash line: 1/13.3 of the lunar fluctuations.

If we regard the secular acceleration of the sun due to tidal friction as established, we can combine it with the observed deviations. The comparison of the result with the fluctuations brings out more graphically the evidence that the latter are needed to explain the deviation of the sun from its complete gravitational orbit. The full line curve in Figure 4 in the text shows the Greenwich residuals combined with the secular acceleration, and the dotted curve, the lunar fluctuations multiplied by .075. It is seen at once that the agreement is good during the last century. Especially to be noticed is the sudden change in slope about 1900 in both curves.

Up to this date the secular acceleration and the fluctuations nearly cancel one another—a reason perhaps why the phenomena have not hitherto been detected in the solar observations. In fact it is only after 1900 that the combined effect begins to become large; its increase is rapid and amounts to 1''.2 in 1925.

A reference to Figure 2 shows that the Washington and Greenwich curves agree in their main features. The Paris curve differs to some extent but not sufficiently to invalidate the general conclusion.

19. But there are other differences between observation and theory which need some mention. In the first place the large deviations in the Greenwich series from 1810 to 1825 appear to be due to systematic errors of observation in this period. Partly or wholly within these fifteen years we have results from Paris, Königsberg and Dorpat, and, except for the first Paris group, none of them show any such large differences, especially towards its close. These also are within the period when the differences between the Greenwich meridian observations of the moon and the occultations become large. Hence it would seem that it is scarcely safe to use the Greenwich observations with full weight during these years in any discussion which involves the theory. Before this time, both sun and moon indicate that the observations are sufficiently good for use in obtaining the mean motion and epoch of the sun but are of doubtful value for any other purpose. The Palermo series between 1791 to 1812 is omitted from the diagram altogether for reasons which are evident from inspection of Table IV.

After 1830 the differences become much smaller in general. One group of the Paris series, the Oxford series and the short Strassburg series exhibit rather large deviations but the remainder are well clustered above and below the zero line. The three long series each exhibit traces of systematic change. Both Greenwich and Washington after 1850 give evidence of an oscillation of about forty years period, but the Washington amplitude is greater than the Greenwich amplitude and the curve also differs in the mean from Greenwich by about half a second of arc.

20. A somewhat different view must now be taken of the correlations, referred to in the introduction, between the deviations of the sun and planets from their tables and those shown by the moon. Those which I showed in 1914 (l. c., paragraph 4) are undoubtedly partly due to errors of observation common to the sun, Mercury and the moon in the earlier part of the range from 1750 to date. The still closer correlations of the planets with the moon, exhibited by Glauert from 1865 to 1915, do not seem to exist before that time. In fact, though the Washington and Greenwich curves for the sun between these dates roughly follow the minor residuals of the moon shown in the upper curve of Figure 3, the resemblance ceases in the earlier range. The conclusion follows that any given observatory will have a systematic error which slowly varies and that this error may or may not be common to observations of other objects. It is thus difficult to disentangle errors of observation from what may be real tabular errors. Ross's diagram III (l. c., paragraph 4) for the deviations deduced from the observations of Mars is undoubtedly of the same character. In the earlier part the deviations are similar to those of the sun and there is the sudden rise after 1900, which disappears after the application of the term S.E.

21. The whole deviation of the moon from its gravitational orbit is exhibited in Figure 1, the full line being the Greenwich results and the dotted line those of Newcomb. Its most remarkable features are the sharp turns about 1785 and 1898 and the straight line effects here and there extending over a number of years in several cases. The former feature has not been hitherto brought out, mainly because it has been usual to plot the 'minor fluctuations' after the great empirical term has been taken out. Newcomb indeed (l. c., paragraph 11) plotted the whole curve, but his scale was such that this feature did not stand out prominently, and apparently was not

noticed. The straight line effects which I have pointed out on previous occasions referred to the 'minor fluctuations.'

This diagram raises the question as to whether we have not actually clouded rather than clarified the situation by our mode of representation of these residuals in using a single harmonic term for the principal oscillation and then discussing the deviations which remain after this term has been taken out. The curve indicates that changes occur suddenly rather than gradually, and these sudden changes happen several times, the slopes being nearly constant in between, thus indicating something more than the usual errors of observation. While an analytic expression can of course always represent the changes, it is doubtful whether in this case such an expression is of assistance in searching for physical causes.

22. This question of the mode of analytic representation has a bearing on the remarkable correlation which is exhibited between the two upper full line curves of Figure 3. I recall (paragraph 13) that the top curve represents the 'minor fluctuations,' while the middle curve, on four times the scale, shows the difference between the occultations and the Greenwich meridian observations. The principal maxima and minima agree up to the end of the Newcomb series and the agreement is maintained up to the present time if we continue this series by a curve drawn through the mean of the Cape, Johannesburg, and eclipse series of occultations. The ratio of the amplitudes, however, varies from unity in the early part of the nineteenth century to about 1/8 in the latter part of the curve. It is not easy to see why a curve which represents the difference between two classes of observations should be correlated with one which represents the difference between one class of observations and theory. The first conclusion would be that the Greenwich meridian observations are responsible for the error and that this error has been diminishing with increased care in making the observations. Even so, why should this error of observation be correlated to a certain portion of the deviations between observation and theory when the latter difference is established by all classes of observations?

This apparent change of amplitude, however, is deceptive. I recall that the minor fluctuations are obtained by subtracting from the residuals a single harmonic term of long period. If we had adopted for the principal oscillation some other Fourier series rather than a single term, the minor residuals would change their character.

Suppose, for example, we adopt a periodic series which gives a nearly straight line between the principal maxima and minima and consider the deviations from this curve as the minor fluctuations. Then it is evident from Figure 1 that, while the dates of the principal maxima and minima of the smaller variations of the latter might not be much altered, their amplitudes would undergo considerable changes. It follows that while we can perhaps consider the correlation between the positions of the maxima and minima of the two upper curves as having some physical significance, the apparent change of relative amplitude may be due to nothing more than our mode of representation of the principal part of the fluctuation. I believe that this correlation contains in some way the key to the whole question and that it is therefore worth a more detailed study. But I have not been able to find any approach which will throw further light on the matter. It is to be remembered in such a discussion that, unless the correlation is accidental, which seems very improbable, it needs an explanation whether it has an instrumental, observational, or an external physical source.

These differences between the different classes of observations must be considered in the light of the probable errors. Each annual mean of the Greenwich series has a probable error of about 0″.1 as deduced from a smooth curve drawn through the results from any given year. The probable error for an annual mean of the Newcomb series varies but averages about 0″.2, while that for the Cape series is about the same. There cannot therefore be much doubt about the systematic character of the observational differences.

23. A reference may be made here to the numerous attempts that have been made to analyze the 'minor fluctuations' into two or three harmonic terms, a summary of the results being given by Dyson and Crommelin.[1] The upper curve of Figure 3 shows that the terms will be very different according to whether we use the occultations or the Greenwich observations between 1800 and 1840. The appearance of the former does not suggest that such analyses will lead to a clue to the physical causes of the fluctuations or that they will enable us to predict their future course. The latter curve appears to be more hopeful but if the tentative conclusion reached here that the Greenwich observations between 1800 and 1840 are of doubtful accuracy, such an analysis has little value. Perhaps the best answer to the question is contained in the results of Dyson and Crommelin who constructed the periodogram with great care and in much detail, using the Greenwich data up to the end of 1922. Their remark, that they were prepared for failure in the matter of prediction in view of my own experience in a similar effort on an earlier part of the series, seems to be justified by the present difference of about 2″ from their curve.

24. The question of simultaneous errors of observations at Greenwich in the sun and moon must also be discussed. Between 1750 and 1840 there is a general resemblance between the full line curve in Figure 2 and the middle full line curve in Figure 3. But a closer examination makes the matter less simple. In fact, the resemblance can only be substantiated if we move the former curve forward about 10 years, and it then becomes quite close. Is this correlation accidental or is there some physical explanation of it? There seems to be no evidence of it after 1850. Nor does there seem to be any further correlation between the curve of Figure 2 and either of the upper curves of Figure 3.

25. A feature of the middle curve of Figure 3 since 1850 is an oscillation of some twenty years period superimposed on the principal oscillation. The eighteen-year period of the node at once occurs to one's mind together with the fact that there was at one time some doubt about the actual corrections required for the principal terms depending on this angle in Hansen's tables and in Newcomb's occultation reductions, and that in spite of the efforts made to avoid this source of error it may still be present. A closer inspection of the curve seems to indicate, however, that the period is not so short as eighteen years. The Cape results show little trace of it. In view of the data given in the lowest curve, I believe that the approach to the period of the node is accidental.

26. The lowest curve in Figure 3 is that of earthquake frequency in the British Isles from data compiled and classified by Davidson.[2] I made a count of the number of days on which earthquakes occurred, counting days with more than one earthquake as one day, and averaged them in three-year groups so as to exhibit long period variations and have placed them in Table V. Owing to the fact that the earlier records were not kept in a systematic manner, but little stress can be

[1] L. c., paragraph 11.

[2] A History of British Earthquakes. Camb. Univ. Press, 1924.

laid on the heights of the maxima. Their positions, however, are probably not very different from those which would have been obtained from a full record. After 1800 there appears to be a considerable degree of correlation between the maxima and minima of the two lower curves. The maxima at 1808, 1820, 1869, 1890, 1911 fall together within the probable errors of the results. Davidson's record stops at 1916 and I have not been able to secure data gathered on the same plan after that time. Following the previous argument, this correlation would imply that the Greenwich observations of the moon have errors which are in some way connected with the frequency of crustal disturbances in Great Britain. But if so, why does not the sun show similar errors?

TABLE V

FREQUENCY OF BRITISH EARTHQUAKES (DAYS PER 3 YEARS)

Date	d	Date	d	Date	d	Date	d
1749.5	2	1794.5	31	1839.5	103	1884.5	8
52.5	6	97.5	11	42.5	108	87.5	18
55.5	10	1800.5	10	45.5	48	90.5	35
58.5	6	03.5	8	48.5	16	93.5	26
61.5	1	06.5	2	51.5	9	96.5	8
64.5	1	09.5	11	54.5	5	99.5	8
67.5	7	12.5	7	57.5	4	1902.5	24
70.5	4	15.5	7	60.5	4	05.5	36
73.5	1	18.5	24	63.5	6	08.5	35
76.5	4	21.5	16	66.5	8	11.5	63
79.5	2	24.5	5	69.5	13	1914.5	3
82.5	3	27.5	6	72.5	10		
85.5	2	30.5	3	75.5	7		
88.5	11	33.5	9	78.5	8		
1791.5	7	1836.5	8	1881.5	5		

A similar count for the earthquake frequency in California from 1850 to 1906 was made from a paper by H. O. Wood.[3] In this case I counted separately the number of days in which weak earthquakes occurred and also the number for strong earthquakes, as distinguished by Wood, and averaged the two for each three years. Table VI contains these latter averages. Most of the maxima are not very different in time from those of the British earthquakes, and they are therefore not shown in Figure 3.

TABLE VI

FREQUENCY OF CALIFORNIA EARTHQUAKES

Date	d	Date	d	Date	d	Date	d
1851.5	16	1866.5	32	1881.5	30	1896.5	44
54.5	34	69.5	54	84.5	37	99.5	87
57.5	38	72.5	33	87.5	42	1902.5	79
60.5	26	75.5	15	90.5	53	05.5	v.g.
63.5	29	78.5	18	93.5	63		

[3] Bull. Seis. Soc. Amer., volume 6, page 55.

In this connection mention should be made of a period of some 240 years deduced by Professor H. H. Turner in 1919 from a study of Chinese earthquakes.[4] In his second note he attempts to reconcile it with the period of 260 years deduced from the observations by Fotheringham. But if my view of the lunar data is correct, namely, that the observations before 1660 have little value as far as this term is concerned, and that the minimum about 1785 and the maximum at 1898 (see Figure 1) furnish the principal evidence, the period should be rather less instead of greater than Turner's original 240 years. In other words, the attempt to analyze into one long and two or three shorter period terms a phenomenon which may be irregular, at least in details, may have led to an incorrect estimate of where the maxima and minima actually occurred. But the sudden upward turn of the curve after 1922 may indicate that the maximum of 1898 will not turn out after all to be the principal maximum.

27. As this paper was about to be sent to the printer Circular 65 (1925 November 14) of the Union Observatory, Johannesburg, arrived with the full discussion of the Transits of Mercury 1677–1924, by Mr. R. T. A. Innes. He there compares (page 313) the results from these transits with the fluctuations of the moon, but apparently has not included the secular change due to tidal friction. If this latter be added and suitable changes be made to the mean motion and epoch of the orbit of Mercury, the agreement appears to be considerably strengthened.

[4] M. N. R. A. S., volume 79, page 531; volume 80, page 617.

V. GEOPHYSICAL CONSEQUENCES

28. The curve of Figure 1 exhibits the apparent variations which the moon's longitude has shown during the past 260 years referred to the earth as a clock having a constant change of rate (the frictional retardation). If we regard these variations as due solely to further changes in the rate of the clock, the ordinates are proportional to the errors of the clock at any time; the slope measures the rate of the clock, and the curvature measures the change of rate.

The greatest average slope over any considerable number of years is that between 1863 and 1875; this can be regarded as known independently of errors of observation; there may be shorter intervals in which the slope is greater but possible errors of observation make them of uncertain value. In this interval the average slope is not less than $0''.7$ per annum. Since the moon travels $13.3 \times 1296000''$ a year, this slope corresponds to a difference of rate of one part in 2.6×10^7 from the mean rate.

The rapidity with which the rate can change is more difficult to gauge because it is affected to a much greater extent by errors of observation. The curve of Figure 1 indicates that the most rapid change of slope for which we have good evidence is that near 1898. Table III shows that the maximum values of the ordinates in the Greenwich, Newcomb, and Cape series occurred in 1897, 1899, 1898, respectively. The ascending rate of $0''.4$ became zero within five years and changed to a descending rate of $0''.4$ within two more years. The whole change took place at most within seven years and probably within a much shorter interval.

The straight line effects which Figure 1 exhibits and which I have pointed out as apparent in the minor fluctuations on previous occasions correspond to intervals in which the rate did not change beyond that due to tidal friction. In fact, the impression conveyed has been that of changes taking place per saltum when the unit of time is a year, rather than gradually.

In any case, the curvatures produced by these rapid variations are much greater than that produced by tidal friction—a point which is of importance in the argument which follows. In other words, the angular acceleration of the earth, assumed to be the cause of these variations, is at times much greater numerically than that produced by tidal friction.

29. The argument by which the variations are traced a step further back towards their source is one which is always open to objection, namely, that of the exclusion of hypotheses other than that finally reached. Its provisional adoption required such quantitative or qualitative evidence as could be produced. In the present state of our knowledge of the earth, the principal question is whether any fundamental objection against it can be made.

Let us first consider the earth as a single solid body acted on by body forces from without. It is very nearly a solid of revolution which revolves very nearly about its principal axis. The theory of the motion of such a body about its center of mass shows that with such external fields of force as are known to exist, any couple which could sensibly change its angular velocity cannot be present without showing other phenomena which would be easily observed. It has, however, two movable layers on its surface, the atmosphere and the ocean, and these are differentially attracted by the moon and the sun, producing differential motions of the layers with respect to the solid crust. Such motions can produce motion in the latter only through frictional forces.

We know the total average retardation of the angular velocity over long periods of time by astronomical observation. We also know the approximate amount of tidal friction produced by the seas and oceans through the work of Taylor and Jeffreys, and this amount substantially agrees with that derived from astronomical observation. Any other average frictional effects must therefore be small compared with those that are known.

(Incidentally, this argument appears to dispose of the hypothesis that there is any observable relative circulation of the atmosphere as a whole round the earth. A simple calculation of the frictional effects of air passing over a surface, the orders of magnitude of which are known, indicates that any general circulation greater than a small fraction of a mile per day at the equator cannot exist.)

30. If we next turn from the average or secular effect of tidal friction to periodic changes, two lines of argument can be followed both of which lead to the conclusion that the observed variations cannot be produced in this manner. The first is concerned with the periods of the variations, which would have to be the same as the periods present in the motions of the moon and sun or be combinations of these periods.[1] All the evidence obtained from analysis is against the existence of such periods. The second line of argument demands that the numerical values of the accelerations in the periodic effects shall always be smaller than the numerical value of the secular acceleration. This latter argument applies against a further hypothesis of the same character, namely, that the variations take place mainly in the crust of the earth which might be assumed to slip as a whole over the nucleus to an observable extent.

Let I_c, I_n be the respective moments of inertia of the crust and the nucleus about the earth's polar axis, ω_c, ω_n their angular velocities, L_c the frictional couple caused by tidal action on the outside of the crust, and L_n that induced between the crust and the nucleus by motion of the crust relative to the nucleus. The equations of motion are:

$$I_c \frac{d\omega_c}{dt} = L_c - L_n, \qquad I_n \frac{d\omega_n}{dt} = L_n.$$

Let us first suppose that the frictional effect is that due solely to the lunar tides, neglecting that of the solar tides. If a, m, ω be the mean distance, mass, and mean angular velocity, respectively, of the moon around the earth, the moon being supposed to move in the plane of the earth's equator, the conservation of angular momentum gives

$$I_c \frac{d\omega_c}{dt} + I_n \frac{d\omega_n}{dt} + ma^2 \frac{d\omega}{dt} = 0.$$

Astronomical observation gives f, where

[1] This argument appears to be also conclusive against the hypothesis that the variations are produced in the motions of the moon, by changes in the law of gravitation similar to that of Einstein. Small forces which depend only on the configuration of the system will not introduce new periods in general; they will only slightly alter existing periods. Exception to this will only occur where two or more periods in the system are equal or nearly equal, that is, in cases of internal resonance.

$$f = \frac{d\omega}{dt} - \frac{\omega}{\omega_c} \cdot \frac{d\omega_c}{dt}.$$

From these four equations we easily deduce

$$\frac{\omega}{\omega_c}\frac{L_n}{I_c} - \frac{\omega}{\omega_c}\frac{L_c}{I_c}\left(1 + \frac{I_c}{ma^2} \cdot \frac{\omega_c}{\omega}\right) = f.$$

Since $I_c\omega_c$ is very small compared wth $ma^2\omega$, we have approximately

$$\frac{\omega}{\omega_c}\frac{L_n - L_c}{I_c} = f.$$

The hypothesis requires that f shall be able to change its sign. But since the frictional couple $L_n - L_c$ obviously cannot change its sign, the hypothesis must be rejected. The method, which refers to variations of long period relative to those of the tides, is obviously applicable to any moving layer whether solid or fluid. It therefore eliminates the atmosphere and the oceans from being the direct or indirect source of the deviations.

31. In the absence of an external source it is necessary to consider an internal one. For all internal sources the angular momentum of the earth is unaltered. Since the total mass is unchanged, a change in the angular velocity can only come from a change in the distribution of the mass, and this must on the average be radial.

If the earth be supposed to be spherical with external radius r and with uniform distribution in every spherical layer, the angular momentum is

$$Mkr^2\Omega = \text{constant},$$

where Ω is the angular velocity, M the mass, and k a numerical constant which is not far from $\frac{1}{3}$.

Suppose that the radial changes are uniform expansions and contractions. We then have

$$\frac{1}{\Omega}\delta\Omega + \frac{2}{r}\delta r = 0.$$

Since the least possible numerical maximum or minimum of $\delta\Omega/\Omega$ is $1 \div 2.6 \times 10^7$ or 4×10^{-8}, that of $\delta r/r$ is 2×10^{-8}, which gives an oscillation of r of 5 inches above and below its mean value. This hypothesis appears to give the least possible change at the surface.

The greatest change which appears to be admissible in view of our present geophysical knowledge is that which would be produced by expansions and contractions taking place in a layer about 50 miles below the surface, raising and lowering the crust but not affecting the nucleus below it. If we denote quantities referring to the crust by the suffix c and to the nucleus by the suffix n, we have

$$(M_c k_c r_c^2 + M_n k_n r_n^2)\Omega = \text{const.}$$

The hypothesis gives

$$(M_c k_c r_c^2 + M_n k_n r_n^2)\delta\Omega + 2M_c k_c r_c \Omega \delta r_c = 0.$$

Since we have, approximately,

$$k_n = \tfrac{1}{3}, \qquad k_c = \tfrac{2}{3}, \qquad r_c = r_n,$$

and since the density of the crust is approximately half that of the nucleus, we obtain a value of δr about 30 times that deduced from the previous hypothesis, or about 12 feet.

32. Some examination of possible surface changes has been made by Larmor (l. c., paragraph 4) who found them much larger than geologists could admit and he used a smaller maximum value of $\delta\Omega$ than that which I have found it necessary to adopt here. If these changes in the radius are to be invoked at all, it appears that their source must be at least as deep as the isostatic layer of compensation (taken here at a lower limit of 50 miles) and that their effect must be a widespread raising and lowering of the crust extending at least over the greater portion if not over the whole of the earth's surface. Local changes on this scale would cause larger alterations of the apparent sea level than observation permits, while a nearly uniform world-wide change will produce little or no alteration.

This statement does not, however, exclude the smaller local changes superimposed on the principal oscillation, discussed below.

33. We have next to consider what effects expansions and contractions of the radius, between the limits of 5 inches and 12 feet, would have on the outer surface of the crust. These limits correspond to maximum changes of one part in 5×10^7 and one part in 1.7×10^6 respectively in expansion and contraction of distances measured along the surface. Young's modulus for ordinary metals and close-grained solids lies between 5×10^{11} and 20×10^{11} in dynes per square centimeter. Let us take 10^{12} as the value for the average material constituting the earth's crust. This is equivalent to an expansion ratio of 1 to 1.4×10^7 for a tension of 1 lb. per square inch. Thus under the two hypotheses the maximum strain set up in the crust would lie between a third of a pound and twelve pounds per square inch. Since the breaking strains are of the order of 1000 or more of these strains, it follows that if the crust of the earth were uniform no external evidence in the form of fissures would appear. It is evident that no probable changes in the numerical data will alter this conclusion.

The outer part of the crust is, however, far from uniform in respect of density. It is also fissured everywhere to a greater or less extent. Further, erosion and the consequent necessity for isostatic compensation tend to maintain the continuance of cleavage planes. Changes of pressure of the order obtained above would seem to be sufficient to permit of adjustment wherever the loads require it, and the intervals are measured, under the hypothesis, not by geologic periods, but by years. Prima facie evidence in favor of the hypothesis of rapid alterations of tension and pressure should be afforded by frequency of seismic phenomena.

34. In view of the last deduction, I attempted to obtain correlations between the fluctuations and the occurrence of earthquakes. In spite of the inadequacy of the data for furnishing reliable evidence, it seems worth while to exhibit what I have been able to find. The main difficulty has been to find earthquake data over a sufficiently long interval and of a sufficiently uniform character to furnish comparisons of any value at all.

The best for the purpose seemed to be that given by Davidson for British Earthquakes. The method of dealing with his data so as to obtain Table V and the lowest curve of Figure 3 is described in paragraph 26; such correlation as there is (paragraph 26) can be seen from a comparison of the lowest curve in this figure with the middle and upper curves. If the minor correlation between the two lower curves there suggested is not accidental, it points to local disturbances at Greenwich which in some way affect observation, perhaps in the manner pointed out by Sampson (l. c., paragraph 9). The California data (Table VI) are also tabulated but add little to the evidence.

35. The applications of the hypothesis to the problems of dynamic geology can only be properly made to phenomena which have taken place during the past two or three centuries, because it is only in that interval that we have any knowledge of the existence of the oscillations. If, however, such oscillations have existed through geologic periods in the past, the assumption opens a wide field for speculation and calculation as to its bearing on the formation of the surface features of the earth. Much geologic evidence points to the former existence of horizontal changes of pressure and perhaps of tension in many areas. With the hypothesis of alternate contraction and expansion of an irregular outer crust from deep-seated changes of pressure below it, such changes appear to be more easily accounted for than by a slow secular cooling and consequent contraction of the whole earth. The latter has of course occurred, but the slow rate at which heat can be radiated through the crust has always presented difficulties in this connection.

36. There are, however, certain considerations which indicate that the rate of loss of interior heat may be greater than that given by radiation alone. If we have two layers of compressible material separated by one of less compressible substance, a change of temperature causing a change of pressure in one may be almost instantaneously transferred as a change of pressure causing a change of temperature in the other. That two if not three such layers exist in the interior of the earth has been deduced from the velocities of seismic waves. It is thus possible that a large part of the energy which is exhibited in earthquakes and vulcanicity on the surface may come from the interior heat of the earth, through rapid oscillations.

37. The horizontal changes of pressure caused by the oscillations, as calculated in paragraph 33, are small and are easily taken care of by bodily expansion and contraction at any considerable distance below the surface. Hence, apart from isostatic adjustments due to erosion, any local relative earth movements such as are exhibited in great earthquakes must be little more than surface effects if they are due to these interior oscillations. The occasional considerable engulfments of material which sometimes accompany volcanic phenomena (e.g., that shown by Jaggar to have occurred in the pit of Halemaumau in the crater of Kilauea in 1924) would be explained if we remember the existence of a certain degree of residual rigidity of the crust.

38. A summary of the general effect is that most of the processes of change and adjustment can take place more easily when (geologically speaking) very rapid oscillations are taking place than when the motion is a slow secular change alone.

39. It has seemed necessary to assume the existence of these deep-seated oscillations in order to account for the astronomical evidence and to satisfy mechanical arguments. If they exist, their cause will have to be sought in some form of chemical, as distinguished from mechanical, action taking place in the interior of the earth. But discussion on this subject is outside my province.

May, 1926

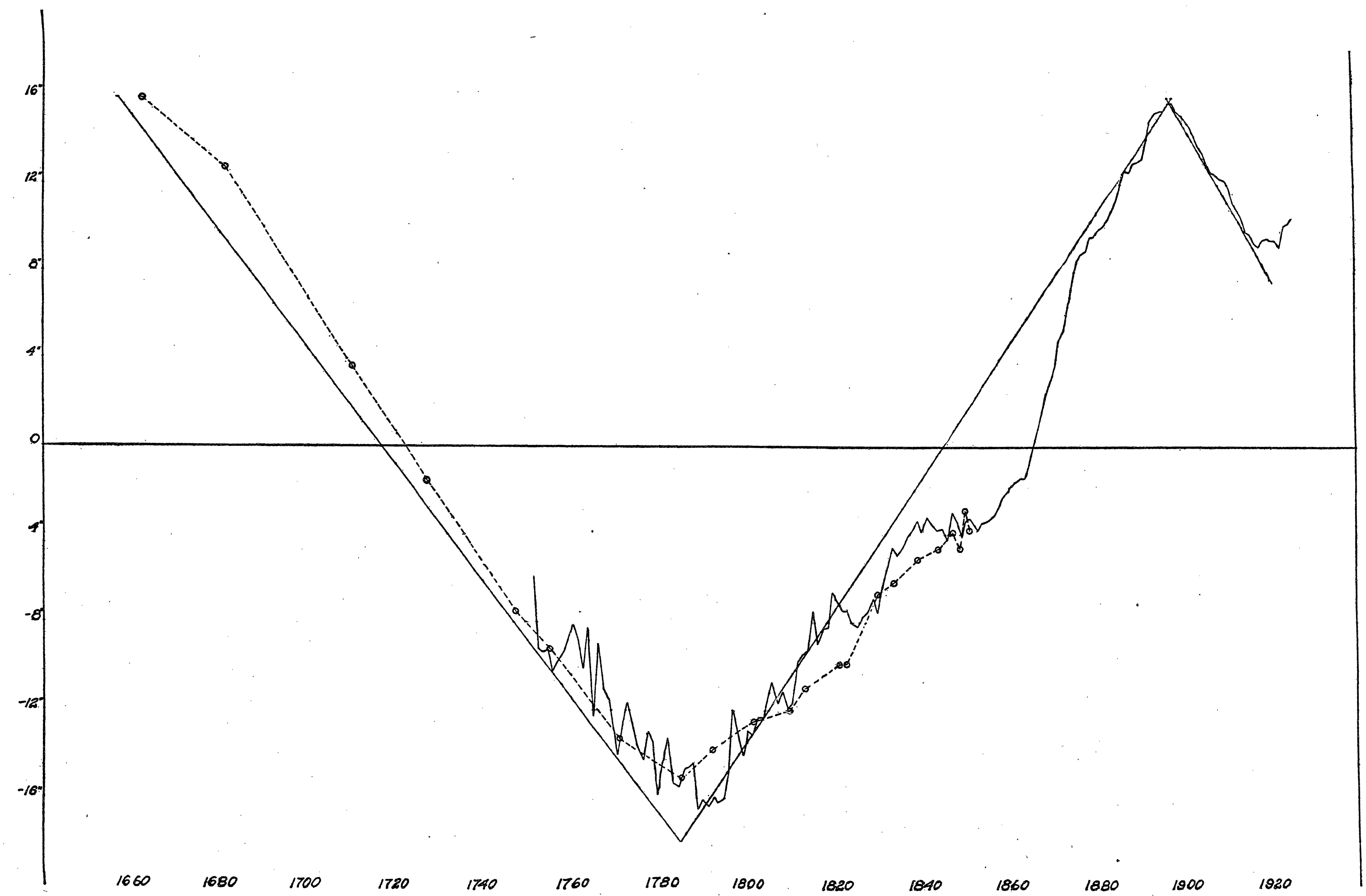

FIG. 1. Fluctuations of the Moon's Mean Longitude.

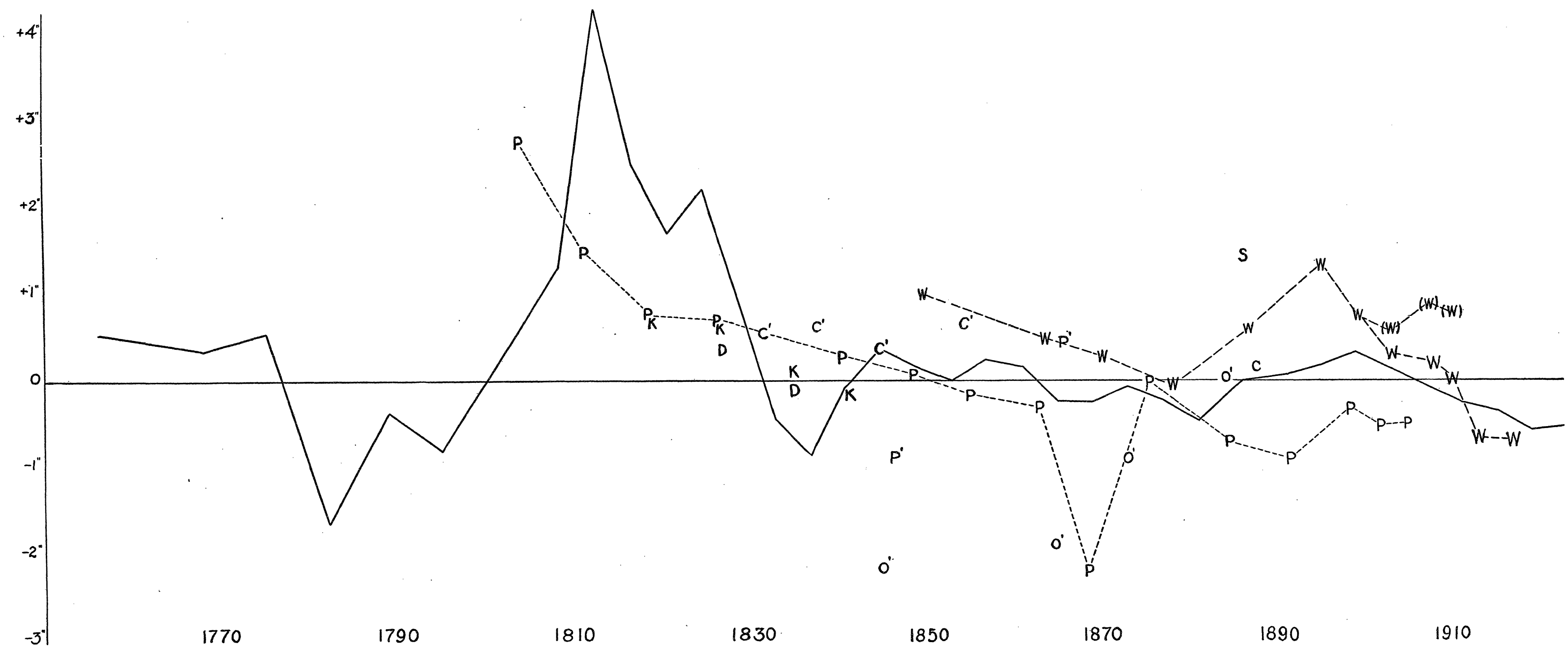

FIG. 2. Tabular minus observed errors of the Sun's Longitude, after application of the secular acceleration and of 1/13.3 of the lunar fluctuations.

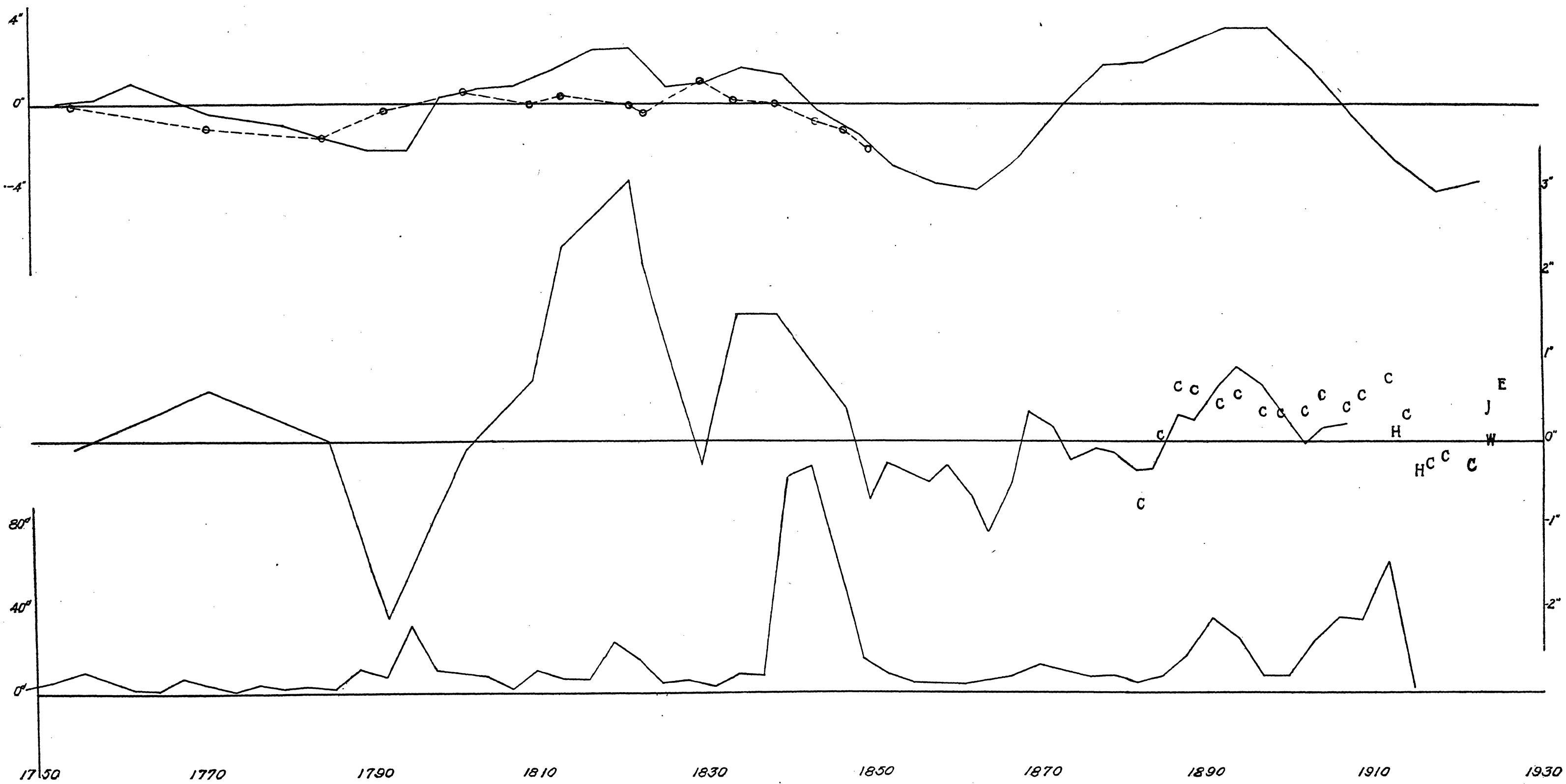

FIG. 3. Upper curve: the minor fluctuations of the Moon. Middle curve: Newcomb's occultations minus Greenwich meridian observations. Lower curve: the frequency of British Earthquakes.