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CONSIDERATIONS

ON THE

APPARENT INEQUALITIES OF LONG PERIOD

IN THE

MEAN MOTION OF THE MOON.

BY SIMON NEWCOMB.

[FROM THE AMERICAN JOURNAL OF SCIENCE AND ARTS, VOL. L, SEPT., 1870.]

New Haven

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CONSIDERATIONS ON THE APPARENT INEQUALITIES OF LONG PERIOD IN THE MEAN MOTION OF THE MOON.

By SIMON NEWCOMB.

[Read to the National Academy, April, 1870.]

THE problem of determining the motion of the moon around the earth under the influence of the combined attraction of the sun and planets has, more than any other, called forth the efforts of mathematicians and astronomers. Nearly every great geometer since Newton has added something to the simplicity or the accuracy of the solution, and, in our own day we have seen it successfully completed in its simplest form, in which the earth, the moon, and the sun are considered as material points, moving under the influence of their mutual attractions. The satisfactory solutions are due to the genius of Hansen and of Delaunay. Working independently of each other, each using a method of his own invention more rigorous than had before been applied, they arrived at expressions for the longitude of the moon which, being compared, were found to exhibit an average discrepancy of less than a second of arc. No doubt could remain of the substantial correctness of each.

The solutions here referred to exhibit only inequalities of short period in the motion of the moon. But, it has long been known, from observation, that the mean motion of the moon is subject to apparent changes of very long period, and especially to a secular acceleration by which it has been gradually increasing, from century to century, since the time of the earliest recorded observations. If we inquire into the problem of these inequalities of long period, we shall find it seemingly no nearer a final solution than it was left by La Place, observation having since added more anomalies than theory has satisfactorily shown to exist.

The first inequality in the order of discovery was the secular acceleration. This was discovered about the middle of the last century by a comparison of ancient eclipses with modern ob-

* See Buff's paper in the Annalen d. Chem. u. Pharm., 4th supplement vol., 1865–6. Or, see his "Grundlehren der theoretischen Chemie."

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servations. Its cause was first discovered by La Place, who showed that it was due to the effect of the action of the planets in changing the eccentricity of the earth's orbit.

The results of his computations agreed substantially with observations, and was therefore received with entire confidence until less than twenty years ago. The question being then taken up by Mr. John C. Adams, this eminent mathematician was led to the conclusion that La Place's result was nearly twice too large.

The same conclusion was reached independently by Delaunay, and gave rise to a remarkable discussion, the history of which is too familiar to be now recounted. It is now conceded that the value found by Adams and Delaunay is theoretically correct.

The new result no longer agreeing with observation, the difference is now accounted for by an increase in the length of the day. That this length is increasing is also known from theoretical considerations, but the data for its accurate determination are wanting.*

In the third volume of the *Mécanique Céleste* (Seconde Partie, Livre vii, Chapitre v) La Place discusses an apparent inequality of long period in the motion of the moon. The discussion is mainly empirical. The existence of the inequality is inferred from observations, these showing that the mean motion of the moon during the half century following 1756 was less than during the half century preceding. He then assumed that the inequality was due to the fact that twice the mean motion of the moon's node, plus the motion of its perigee, minus that of the sun's perigee was a very small quantity, less than two degrees per annum, and determined the coefficient of the varying angle solely from the observations. The result was that these might be satisfied by supposing the inequality of mean longitude

$$\delta l = 47''\cdot 51 \text{ [or } 15''\cdot 39\text{]} \sin (2\,☊☽ + \pi☽ - 3\pi☉)$$

If, in this expression, we substitute Hansen's values of the elements, it becomes

$$\delta l = 15''\cdot 39 \sin [173° \; 26' + (1° \; 57'\cdot 4)\,(t - 1800)].$$

When in 1811 Burckhardt constructed his tables of the moon,

* The time and place when the discordance referred to was first distinctly attributed to the tidal retardation of the earth having been a subject of discussion, the following extract from an article on "Modern Theoretical Astronomy" in the North American Review for October, 1861 (vol. 93, p. 385), may not be devoid of interest.

"It seems to be well established that the new theory is inconsistent with the observations of ancient eclipses, and if it should prove to be correct, we may be driven to the conclusion, that a portion of the acceleration proceeds from some other cause than the attraction of gravitation, or that the length of the day is gradually increasing to an extent which has become perceptible from the cause to which we have already referred [the tidal retardation, p. 374]. If, as centuries roll by, the day should gradually increase, the moon would move a little farther in the course of a day than if no such increase should take place. Since, in our calculations, we suppose the day constant, the apparent acceleration would be greater than the real—precisely the effect observed. The difference can be entirely accounted for by supposing an increase of something less than one thousandth of a second per century in the length of the day, and a corresponding diminution in the lunar month."

he omitted the sun's perigee from this argument by the authority of La Place, himself, who now attributed the inequality to a difference of compression between the two hemispheres of the earth. The function was also changed from *sin* to *cos* and the coefficient altered. The adopted term thus became

$$\delta l = -12''{\cdot}5 \cos\left[291^\circ\ 57' + (2^\circ\ 0'{\cdot}45)(t-1800)\right]$$
$$= 12''{\cdot}5 \sin\left[201^\circ\ 57' + (2^\circ\ 0'{\cdot}45)(t-1800)\right]$$

Succeeding investigators have regarded the theoretical coefficients of both of these terms as insensible. It does not seem likely that there is any such difference between the two terrestrial hemispheres as could produce the second, but I am not aware that the coefficient of the first has ever been shown to be insensible by any published computation. This coefficient is of the ninth order and the argument is,

In Delaunay's notation, $3D-2F-l+3l'$;
In Hansen's, $w-3w'$.

The period is 184 years, and the large value of the ratio of this period to that of the moon itself might render the coefficient sensible. Both Hansen and Delaunay pronounce it insensible, but neither publish their computations of its magnitude.

These terms have ceased to figure in the theory of the moon since Hansen announced that the action of Venus was capable of producing inequalities of the kind in question. So far as I am aware, Hansen's first publication on this subject is that found in No. 597 of the Astronomische Nachrichten (B. 25, S. 325.) Here, in a letter dated March 12, he alludes to La Place's coefficients, and says he has not been able to find any sensible coefficient for La Place's argument of long period. But on examining the action of Venus on the moon he found, considering only the first power of the disturbing force, the following term in the moon's mean longitude:

$$\delta l = 16''{\cdot}01 \sin(-g-16g'+18g''+35^\circ\ 20').$$

g, g' and g'' being the mean anomilies of the moon, the earth and Venus respectively. As this expression still failed to account for the observed variations of the moon's longitude he continued the approximation to the fourth power of the disturbing force, and found that the terms of the third and fourth order increased the coefficient to $27''{\cdot}4$, the angle remaining unchanged, so that the term became

$$27''{\cdot}4 \sin(-g-16g'+18g''+35^\circ\ 20'),$$

But this increase made the theory rather worse, and the term depending on the argument of Airy's equation between the earth and Venus was then tried with the result—

$$\delta l = 23''{\cdot}2 \sin(8g''-13g'+315^\circ\ 30').$$

The introduction of this term seemed to reconcile the theory with observation.

Hansen finally remarks that these values of the coefficients are still subject to some uncertainty from his not having employed decimals enough in his computation.

In a letter to the Astronomer Royal, published in the Monthly Notices of the Royal Astronomical Society for Nov. 1854, Hansen gives a statement of the elements employed in his tables of the moon, and refers to the subject of these inequalities in the following terms:—

"The accurate determination of these two inequalities by theory is the most difficult matter which presents itself in the theory of the moon's motion. I have on two occasions and by different methods sought to determine their values, but I have obtained results essentially different from each other. I am now again engaged with their theoretical determination by a method which I have simplified, and hope to bring the operation to a definitive close. I have also applied to my tables some coefficients which are not free from empiricism but which I can justify by the circumstance that they represent the ancient as well as the modern observations with great exactness, and it may be expected that they will represent the future observations equally well."

Hansen's lunar tables were published in 1857.

The terms of long period finally adopted are

$$15''\cdot34 \sin(-g-16E+18V+30^\circ\ 12')$$
$$+21\cdot48 \sin(8V-13E+274^\circ\ 14'),$$

V and E representing the mean longitudes of Venus and the earth. Changing them to mean anomalies the terms become

$$15''\cdot34 \sin(-g-16g'+18g''+33^\circ\ 36')$$
$$+21\cdot47 \sin(8g''-13g'+4^\circ\ 44').$$

It appears that while the first term has been restored to what was substantially its original value, when only the first power of the disturbing force was included, the argument of the second term has been changed by 50°, the coefficient being but slightly changed.

In a letter to the Astronomer Royal, dated 1861, Feb. 2d, found in the Monthly Notices for March, 1861, Hansen again refers to this second term with the statement that its coefficient is one of those somewhat empirical. At the same time he has found the coefficient, by his last theoretical determination of it, by no means insensible, like Delaunay. He adds that in the comparison with observation he has never gone beyond Bradley, nevertheless his tables satisfactorily represent the ancient observations.

A well marked feature of Hansen's published works is the copiousness and perspecuity with which his theoretical calculations are laid down. But, so far as I am aware he has never published any computation of these inequalities except that part of the first inequality which depends on the first power of the disturbing force. This computation is found in vol. xvi of the Memoirs of the Royal Astronomical Society. In the second part of his "Darlegung" we find a general method of treating inequalities of long period, but—unless I have overlooked it—no computation of any particular inequality. Nor do we find any statements of the numerical results of Hansen's various computations except those already quoted.

The only geometer besides Hansen who has attacked the problem of these inequalities is Delaunay. His researches are published in full in the Additions to the Connaissance des Temps for years 1862 and 1863. For the first approximation to the first inequality his result is

$$16''{\cdot}02 \sin(-l-16l'+18l''+35^\circ\ 20'{\cdot}2)$$

a result practically identical with that of Hansen. The ulterior approximations change it to

$$16''{\cdot}34 \sin(-l-16l'+18l''+35^\circ\ 16'{\cdot}5),$$

so that they increase the coefficient instead of diminishing it as in Hansen's theory. The difference is however so small that the results may be regarded as identical.

But, in the case of the second inequality instead of reproducing the result of Hansen, he finds a coefficient of only $0''{\cdot}27$, a quantity quite insignificant in the present state of the question. We have thus an irreconcilable difference on a purely theoretical question.

I propose to inquire whether we have in either theory an entirely satisfactory agreement with observation. As a preliminary step to this inquiry I have prepared the following table of the mean longitude of the moon from the tables of Burckhardt and of Hansen respectively, for a series of equidistant dates, the interval being 3652·5 days, and the epoch 1800 Jan. 0, Greenwich mean noon. These dates are marked by the year near the beginning of which they fall. Column L_0 gives Burckhardt's mean longitude on the supposition of uniform motion, from the data given on the fifth page of the introduction to his tables. Next is given the acceleration of the mean longitude deduced from Table XLVIII. The inequality of long period is from Table XLIX. The sum of these three quantities gives the corrected mean longitude.

Hansen's mean longitude and secular acceleration are deduced in the same way from the elements given on page 15 of his *Tables de la Lune.* His terms of long period are deduced from Tables XLI and XLII, the constants being subtracted and the remainder reduced to arc by being multiplied by the factor $0''{\cdot}004703$. The last column of the table gives the correction to Burckhardt's mean longitude to reduce it to that of Hansen. That this difference is really the mean difference between the longitudes of the moon deduced from the two tables is shown by its agreement with the known difference at particular epochs. At the end of the British Nautical Almanac for 1862 is found a comparison of the two tables, from which it appears that Burckhardt's mean longitude was then greater than Hansen's by about 14″·2. The general agreement between 1750 and 1800, when both tables agreed with observations, shows that the difference of mean motion is certainly affected with no sensible error.

Year.	Burckhardt. L'_0	Sec. Var	Long Period.	Corr. Mean Longitude.	Hansen. L_0	Sec. Var.	Long Period.	Mean Longitude.	H.–B.
	° ′ ″	″	″	° ′ ″	′ ″	″	″	° ′ ″	″
1630	100 19 28·0	+ 4·9	− 8·0	100 19 24·9	16 14·4	+38·5	−21·4	100 18 31·5	−53·4
40	347 6 45·4	+ 3·6	−10·8	347 6 38·2	5 36·8	+34·1	−20·0	347 5 50·4	−47·8
50	233 94 2·7	+ 2·5	−12·3	233 53 52·0	52 58·3	+ 30·0	−17·2	223 53 11·1	−31·8
60	120 41 20·1	+ 1·6	−12·3	120 41 9·4	40 20·3	+26·1	−13·1	120 40 33·3	−36·8
70	7 28 37·4	+ 0·9	−10·8	7 28 27·5	27 42·2	+22·5	− 8 1	7 27 50·6	−30·9
80	254 15 54·8	+ 0·4	− 8·0	254 15 47·2	15 4·2	+10·2	− 2·3	254 15 21·1	−26·1
90	141 3 12·1	+ 0·1	− 4·2	141 3 7·8	2 26·1	+16·1	+ 3·9	141 2 40·1	−21·7
1700	27 50 29·5	+ 0·0	+ 0·2	27 50 29·7	49 48·1	+13·3	+10·0	27 50 11·4	−18·3
10	274 37 46·8	+ 0·1	+ 4·4	274 37 51·3	37 10·0	+10·8	+15·6	274 37 30·4	−14·9
20	161 25 4·2	+ 0·4	+ 8·3	161 25 12·9	24 32·0	+ 8·5	+20·5	161 25 1·0	−11·9
30	48 12 21·5	+ 0·9	+11·0	48 12 33·4	11 59·9	+ 6·5	+24·2	48 12 24·7	− 8·7
40	204 59 38·9	+ 1·6	+12·4	294 59 52·9	59 15·9	+ 4·8	+26·4	294 59 47·1	− 5·8
50	181 47 56·2	+ 2·5	+12·2	181 47 10·0	46 37·9	+ 3·3	+26·9	181 47 8·1	− 2·0
60	68 34 13·6	+ 3·6	+10·6	68 34 27·8	33 59·8	+ 2·1	+25·7	68 54 27·6	− 0·2
70	315 21 30·9	+ 4·9	+ 7·8	315 21 43·7	21 21·8	+ 1·2	+22·9	315 21 45·9	+ 2·2
80	202 8 48·3	+ 6·4	+ 3·9	202 8 58·6	8 43·7	+ 0·5	+18·5	202 9 2·7	+ 4·1
90	88 56 5·6	+ 8·1	− 0·4	88 56 13·4	56 5·7	+ 0 1	+12·8	88 56 18·6	+ 5·2
1800	335 43 23·0	+10·0	− 4·7	335 43 28·4	43 27·7	0·0	+ 6·1	335 43 33·8	+ 5·4
10	222 30 40·4	+12·1	− 8·3	222 30 44·2	30 49·6	+ 0·1	− 1·1	222 30 48·6	+ 4·4
20	100 17 57·8	+14·4	−11·0	109 18 1·2	18 11·6	+ 0·5	− 8·4	109 18 3·7	+ 2·5
30	356 5 15·2	+16·9	−12·4	356 5 19·7	5 33·5	+ 1·2	−15·4	356 5 19·3	− 0·4
40	242 52 32·5	+19·6	−12·2	242 52 39·9	52 55·5	+ 2·1	−21·6	242 52 36·0	− 3·9
50	129 39 49·9	+22·5	−10·6	129 40 1·8	40 17·5	+ 3·3	−26·5	129 39 54·3	− 7·5
60	16 27 7·2	+25·6	− 7 6	16 27 25·2	27 39 4	+ 4·8	−29·8	16 27 14·4	−10·8
70	263 14 24·6	+28·9	− 3·8	263 14 49·7	15 1·4	+ 6·5	−31·3	263 14 36·6	−13·1

Burckhardt's tables have been selected for this comparison because they have been extensively compared with observations made before 1700. The additions to the Connaissance des Temps for 1824 contain a paper by Burckhardt himself giving a comparison of his tables with observations of occultations made by Flamstead, Hevelius and others, between 1637 and 1700. The general result of this comparison is that the mean longitude of his tables could hardly have been more than a very few seconds in error in the year 1670. But, the preceding table shows that for this epoch Hansen's mean longitude is 30″ less than Burckhardt's. Therefore, unless we suppose Burckhardt's investigation to be affected with some egregious systematic error we must admit that the mean longitude of Hansen's tables for the epoch 1670 is about 30″ too small.

Desiring an independent test of this conclusion I have selected certain observations which, with the data available, seemed well fitted to answer this purpose and compared them directly with Hansen's Tables.

They are

1. Occultation of Aldebaran, 1680, Sept. 13, observed at Greenwich by Flamstead.
2. Occultation of the same star 1680, Nov. 7, observed at Greenwich by Flamstead, and at London by Halley.
3. Total eclipse of the sun 1715, May 3, observed at London, Greenwich and Wanstead by Halley, Flamstead and Pound.

To compute the occultations of Aldebaran the mean position for 1680·0 was derived from Le Verrier's Tables (Annales de l'Observatoire, Tome II) correcting the right ascension by $+0^s{\cdot}01$, and was as follows:

$$\alpha(1680) = 4^h\ 17^m\ 37^s{\cdot}01$$
$$\delta\ \ldots\ldots +15^\circ\ 49'\ 11''{\cdot}8$$

The corrections for reduction to apparent place are

for Sept. 13, $\Delta\alpha = +2^s{\cdot}90$; $\Delta\delta = +1''{\cdot}1$
Nov. 7, $\Delta\alpha = +4{\cdot}18$ $\Delta\delta = +2{\cdot}4$

The following geocentric positions of the moon were derived from Hansen's Tables.

Date (Julian Cal.)	Sept. 13.						Nov. 7.					
	h	m	s	h	m	s	h	m	s	h	m	s
Gr. Mean Time,	15	0	53	16	12	53	7	50	39	8	48	15
☽'s Longitude,	64°	54′	24″·3	65°	37′	20″·4	64°	33′	11″·6	65°	9′	49″·6
" Latitude,	–4	45	29·8	–4	48	10·6	–4	39	26·9	–4	40	48·0
" Parallax,	0	59	30·0	0	59	28·8	1	1	18·5	1	1	17·8

From these data we derive the following times for the immersion and emersion of Aldebaran for the dates in question. The observed times have been concluded from the observed altitudes and clock times given by Flamstead in the *Historia Celestis*, kindly furnished me by Prof. Winlock. They differ but little from the results of Flamstead himself, when the latter are corrected for the equation of time.

		Computed.			Observed.			C—O.
		h	m	s				s
Sept. 13,	Immersion,	15	2	49	15	0	53	+116
	Emersion,	16	10	5	16	9	12	+ 53
Nov. 7,	Immersion,	7	51	47	7	50	43	+ 64
	Emersion,	8	48	16	8	47	12	+ 64

The great difference between the results of the two phases of the first occultation gives rise to a suspicion of error in the observations or the data of reduction. The second observation is confirmed by that of Halley in London, he having observed the immersion at $7^h\ 50^m\ 9^s$, and noticed that the star was "newly emerged" at $8^h\ 47^m\ 1^s$. His place of observation was probably twenty-five or thirty seconds west of Greenwich, and therefore his observation agrees well with that of Flamstead. The discordance between the observed and computed times, of this second occultation indicates a correction of about $+32''$ to Hansen's mean longitude at the epoch 1680, and the first may be considered as confirming this correction in direction, if not in amount.

For the eclipse of May 3, 1715 we have the following computed and observed times. I have assumed Halley's station to be in latitude 51° 31′ and longitude 25ˢ west. Pound's is taken in accordance with his own statement to be in latitude 51° 34′, and longitude 8ˢ west. These agree pretty well with Flamstead's statements that Wanstead is seven or eight miles N. by E. from Greenwich,* and that Crane Court is half a minute of time West of Greenwich.

Halley at London.

	Computed.			Observed.			C—O
	h	m	s	h	m	s	s
First contact,	20	2	35	20	2	37	— 2
Beginning of Totality,	21	5	52	21	5	39	+13
End of "	21	9	3	21	9	2	+ 1
End of Eclipse,	22	16	55	22	16	37	+18

Pound at Wanstead.

	Computed.			Observed.			C—O
	h	m	s	h	m	s	s
Eclipse first perceived,	20	3	18	20	3	15	+ 3
The total immersion,	21	6	38	21	6	6	+32
The emersion,	21	9	48	21	9	26	+22
The just end of the eclipse,	22	17	42	22	17	10	+32

The only information I have respecting Flamstead's observations is contained in a letter of his found in Baily's 'Life and Correspondence of Flamstead, p. 315, from which it appears that his times differ only a few seconds from Halley's, instead of differing by the half minute required by the difference of meridians. An obvious slip of the pen, (*later* being written instead of *earlier*) makes it doubtful in which way the "few seconds" are to be counted. It can, however, be fairly inferred from his statement that his observations diverge from the tabular times as much or more than Pound's.

The discordance of the results of first and last contact may be attributed to this cause: that with their imperfect telescopes the observers did not begin to see the moon until several seconds after the actual commencement of the eclipse, and lost sight of it a few seconds before the actual end. The discordance in the duration of totality indicates with a high probability that the computed shadow path falls a few miles too far north. In this case the mean of the results for beginning and end of totality

* **Baily's Flamstead, p. 316 p. 328.**

will be about right, and we have for the excess of computed times

Halley's observations,	$+ 7^s$
Pound's,	$+ 27$
Flamstead's,	$+ 30 \pm$

I infer from these results that the correction to Hansen's mean longitude at the epoch 1715 is about $+11''$.

Comparing the corrections thus found for the epochs 1680 and 1715, we find they are substantially those required to reduce Hansen's mean longitude to Burckhardt's. I conclude, therefore, that no egregious systematic error has crept into the researches by which Burckhardt sought to show that the epoch of his tables was substantially correct during the latter half of the seventeenth century, and that the difference between the mean longitude of Hansen and Burckhardt during that period represents approximately, at least, errors of Hansen's mean longitude.

The observations of the moon made at the observatories of Greenwich and Washington during the last ten years, indicate a tabular deviation of a remarkable character. From 1850 to 1862 we find the moon slowly running ahead of the tables, until the latter required a correction of plus two seconds in longitude to make them agree with observation. But this correction, instead of continuing to increase as all analogy would have led us to anticipate, suddenly began to diminish, so that since 1862 the moon seems to have been falling behind the tables at the rate of a second a year. This is shown by the following exhibit of the corrections to Hansen's mean longitude, or right-ascension, deduced from the meridian observations of the two observatories.

Year.	Correction given by Greenwich.	Correction given by Washington.	Mean.	Corr. mean.
	$''$	$''$	$''$	$''$
1850	+0·3	−1·3	0·0	+1·0
51	+1·5	+0·6	+1·3	+2·7
52	+0·9	----	+0·9	+2·4
56	+1·0	----	+1·0	+1·4
57	+1·5	----	+1·5	+1·4
58	+2·0	+1·5	+1·8	+1·3
62	+2·4	+2·4	+2·4	+0·9
63	+2·2	+1·2	+1·7	+0·5
64	+0·1	−1·0	−0·4	−1·2
65	−1·1	−2·4	−1·7	−2·1
66	−2·2	−2·5	−2·4	−2·4
67	−3·9	−4·1	−4·0	−3·6
68	−4·4	−4·5	−4·5	−3·6
69	----	−5·5	−5·5	−4·3

The corrections here given as those of Greenwich are, previous to 1859, derived from the comparison found in the Greenwich observations for 1859. From 1863 forward they are derived from a paper by Mr. Dunkin in the Monthly Notices of the Royal Astronomical Society for April, 1869. The work of only the four principal observers is therefore included in the comparison. The object of this comparison being not so much to determine the absolute correction to the epoch of the tables as to show the changes of this correction, it is better to reject the results of the observers whose labors were discontinuous. In the case of the Washington observations, such a selection could not be made: the results given are therefore an indiscriminate mean of all. The systematic personal differences are however found to be very small.

That these corrections are real will not, I conceive, be disputed. To suppose them due to errors of observation, would be to suppose that six or eight long practiced observers divided between the two hemispheres, all progressively changed their habits of observing in the same way, and to nearly the same amount, through a period of seven or eight years.

A portion of the observed discordance may arise from a small error in Hansen's value of the coëfficient depending on the ellipticity of the earth, which is more than a second greater than the values derived by previous investigators, either from theory or observation. The last column of the preceding table shows what the correction would be if Hansen's coëfficient were 1″·5 smaller than it is.

From all these comparisons it would appear that the problem of the inequalities of long period in the moon's mean motion is really no nearer such a solution as will agree with observation, than when it was left by La Place. By a partially empirical correction, Hansen has succeeded in securing a very good agreement during the period 1750–1860, but, if the results of the preceding examination are correct, this has been gained only by sacrificing the agreement for the century previous to 1750, and for the years following 1860. This failure to reconcile theory with observation must arise from one of two sources. Either:

(1) The concluded theory does not correctly represent the mean motion of the moon. Or:—

(2) The rotation of the earth on its axis is subject to inequalities of irregular character and long period.

The first hypothesis admits of two explanations. We may suppose either that the mean motion of the moon is subject to change from some other cause than the gravitation of the known bodies of the solar system, or that the effect of this gravitation is incorrectly calculated, and that theory and observation will be reconciled by a correct calculation.

There are difficulties in the way of accepting either of these explanations. In reference to the first it may be remarked that anomalies of mean motion cannot be accounted for by a deviation from the received law of gravitation inversely as the square of the distance, because the anomalies produced by such deviation would be regularly progressive, and would be most sensible in the secular motion of the moon's perigee. The comparison of the theoretical and observed values of this motion is, perhaps, the severest test to which the Newtonian law has yet been subjected. That the anomalies proceed from the attraction of unknown bodies passing through the system seems extremely improbable, since, if they were distant, they would affect the earth and planets more than the moon, while the closer passage of bodies could scarcely escape detection. Still, this explanation does not admit of being mathematically disproved. If we attribute the deviation to the impact of meteoric matter, we must suppose the moon to have encountered such matter in quantities nearly incredible.

These three causes exhaust those on which we can base the first explanation, unless we invalidate the third law of motion. For, by that law, matter moves only by the influence of other matter. Other matter can affect the motion of the moon only by impact and gravitation. The gravitation of known bodies. the gravitation of unknown bodies, and the impact of matter is therefore an exhaustive enumeration.

We pass now to the second explanation of the first hypothesis, namely, errors or omissions in the theoretical computation of the effect of gravitation. The wide difference between the conclusions of Hansen and Delaunay suggests the possibility that there may be inequalities still overlooked. We have however the assurance of Hansen that there are none, and we shall find it extremely difficult to introduce any periodic terms whatever which will represent the observed deviation of the moon from the tables during the past ten years, without discordance during the century previous, when the agreement of Hansen's tables with theory is believed to be quite close. It is however hardly worth while to dwell upon this explanation until we have a more rigorous theory of the inequalities of long period produced by gravitation.

Considering that the reconciliation of theory and observation is not very probable, the second hypothesis may become worthy of serious consideration. If we accept it we must admit that between the years 1860 and 1862 the rotation of the earth was so accelerated that our reckoning of time is already eight or ten seconds ahead of what it would have been had the day remained invariable. Such an acceleration could proceed only from a change in the arrangement of the matter of the earth. The possibility of this effect being produced by changes in the quantity of ice accumulated around the poles has, I believe, been pointed out by geologists. But the effect of this cause could scarcely be sensible. But, if we admit that the interior of the earth is a fluid, and also admit that general changes in the arrangement of this fluid are possible, we have all that is necessary to account for considerable changes in the rotation of the outer crust. That this fluid, admitting its existence, is not in a state of entire quiescence is rendered probable by the phenomena of volcanoes and earthquakes. If we suppose a large mass of it to move from the equatorial regions to a position nearer the axis, a mass from the latter position taking its place, the following effects will follow:—

1. A diminution in the angular velocity of the surface of the fluid, accompanied by a corresponding increase in the velocity of the axial portion. The velocity of the outer crust will then be gradually retarded by friction.

2. The gradual transmission of the increased rotation of the central mass to the surface by friction and viscosity. The motion of the crust will then be gradually accelerated. The velocity of rotation finally attained will be greater or less than the original velocity, according as the radius of gyration of the fluid mass is diminished or increased by the change in the arrangement of the fluid.

I conclude, from this discussion, that we have reason to suspect that the motion of rotation of the crust of the earth is subject to inequalities of an irregular character, which, in the present state of science, can be detected only by observations of the moon. This suspicion can be neither confirmed nor removed until we have more positive knowledge than we now have of the possible inequalities which may be produced in the mean motion of the moon by the action of gravitation.

The operation of calculating these inequalities, though complicated and difficult, is certainly within the powers of analysis. When it is completely and thoroughly done, we may ascertain whether the result can be made to represent observations. If so, well; the length of the day is not variable, and the future positions of the moon can be safely predicted. If not, it will follow either that the motion of the moon is affected by other causes than the gravitation of the known bodies of the solar system, or the day is irregularly variable.

By the end of the present century, if not sooner, we shall have an independent test of the latter hypothesis, in the agreement of the observed and theoretical times of the transits of Mercury and Venus. If the hypothesis is a true one, the irregularities may range over half a minute of time in the course of a century, and this quantity might be detected even by meridian observations of the planets in question.

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