ART. XLVII.—*On the Mean Motion of the Moon;* by SIMON NEWCOMB.

FOR some time after the appearance of Hansen's Lunar Tables, it was very generally considered that the theory of the moon, after occupying the attention of the mathematicians and astronomers of every century for two thousand years, was at length complete, and that the motion of that body could now be predicted with entire confidence. That Hansen's computation of the inequalities of short period produced by the sun not only far exceeded in accuracy any before made, but fulfilled all the requirements of modern astronomy, I conceive can hardly be doubted. But in the number of this Journal for September, 1870, I showed that this improvement did not extend to the inequalities of long period in the mean motion. While it was true that Hansen by an empirical term had secured a very good agreement with observations from 1750 to 1860, it was there shown that this agreement had been obtained by sacrificing the agreement before 1750, and that the moon had then begun to deviate from the tables at such a rate that they could not continue satisfactorily to represent the observations. During the seven years which have since elapsed, this suspicion has been entirely confirmed. So far as can be judged by the most recent observations, the error of the tables now exceeds ten seconds, and is increasing at a rate of not less than half a second a year.

Shortly after the publication of the short paper to which I have alluded, it was made a part of my official duty to investigate this question. In accordance with this arrangement, I have aimed at the complete discussion of all recorded observations of any astronomical value before the year 1750. These researches now being brought substantially to a close, so far as the observations are concerned, the object of the present article is to give some account of them, and of their results. The material consists in brief, of every observation of an eclipse or an occultation previous to 1750, which appears to be worthy of confidence, and calculated to throw any light upon the question of changes in the moon's mean motion. The available data may be classified as follows:—

I. Accounts of ancient historians from which it has been inferred that the shadow of the moon passed over certain points of the earth's surface during certain total eclipses of the sun. The celebrated eclipses of Thales, of Larissa, and of Agathocles have been very carefully discussed by Professor Airy in two papers which have appeared, the one in the Philosophical Transactions, and the other in the Memoirs of the Royal Astronomical

Society. After a careful examination of the six or eight eclipses in question, I was led to the conclusion that none of them could be safely relied upon as furnishing data for the error of the Lunar Tables at the times when they were observed. It is impossible, within the limited space of the present article, to enter into any details of the considerations which led me to this conclusion. It may be remarked, however, that among the eclipses in which I can feel but little confidence is the celebrated one of Thales. To prevent misapprehension I may say that I do not deny either that Thales predicted eclipses or that the shadow of the moon passed over Asia Minor, B.C., 585 as indicated by the Lunar Tables, or that a battle was stopped by some real or fancied advent of darkness, as described by Herodotus a century afterward; but I fail to see any good reason for maintaining that the extremely obscure account of Herodotus really refers to the total eclipse in question, or, in fact, to any eclipse whatever. Consequently, while these eclipses may be useful in throwing more or less of evidence on the question of the moon's secular acceleration, I do not think they can be considered reliable enough to be used for determining that quantity.

II. The second class comprises the nineteen eclipses of the moon quoted by Ptolemy in the Almagest, on which he founded his theory of the moon's motion. These eclipses appear to be worthy of some confidence, making due allowance for the very considerable errors of observation with which they are necessarily affected. The mode of treatment was this: from a very careful study of the account of each eclipse as given by Ptolemy, and without any knowledge of how it compared with the tables, I sought to make an estimate, first, of the most probable time of the phase described, and second, of the probable error of that time. These estimates I shall publish without any alteration suggested by the subsequent comparison with the tables. When this comparison was made, it was found that the general deviations of the tabular from the recorded times did not indicate a probable error essentially greater than that estimated, except in two cases.

There are five eclipses in which Ptolemy does not say to what phase the time which he gives refers. It has very generally been considered that in these cases the phase was that of the middle of the eclipse; but in all other cases the time which he gives is that of commencement; and there would be a certain probability in favor of the times where no phase was given being also those of commencement. The errors in question were systematically different from those of the other eclipses, and seemed to indicate that in these eclipses also, the beginning was referred to. Owing, however, to the uncertainty of this entire hypothesis, I judged it best to reject these eclipses entirely, and confine the discussion to the fourteen remaining ones.

Among these fourteen, which, in some cases, include the end of the eclipse as well as the beginning, there was a single one, that of B.C., 382, December 22, which was in contradiction with all the others. The other thirteen all agree in the most remarkable manner in assigning a correction of more than half an hour to the tabular times; while this one indicated a negative correction. This discordance, however, is not the most perplexing circumstance. It happened that this eclipse commenced just before sunrise, and therefore just before the moon set; and if the other eclipses were accurate, this one could not have been seen at all. If this one really was seen, it would almost necessitate a negative correction to the tabular times. We have then this dilemma: either the whole thirteen eclipses recorded by Ptolemy are, with a single exception, half an hour or more in error, or there is some mistake about this eclipse having been actually observed. Deeming the latter the more probable of the two hypotheses, I threw out this eclipse entirely. Of the twelve remaining eclipses, sixteen phases were observed, which were divided into four groups, and the mean result, by weight, of each group was taken. The mean corrections to the tabular times given by the several groups, are as follows:—

Epoch,	-687	$\delta t = +20^{\mathrm{m}}$	$\delta\varepsilon = -11' \pm 4'$	3 phases.
	-381	$\delta t = +50$	$\delta\varepsilon = -27 \pm 5$	3 phases.
	-189	$\delta t = +36$	$\delta\varepsilon = -20 \pm 3$	8 phases.
	$+134$	$\delta t = +30$	$\delta\varepsilon = -16 \pm 4$	3 phases.

III. The next observations in order are the eclipses observed by the Arabian astronomers between the years 829 and 1004, which are published in the work entitled Le Livre de la Grande Table Hakémite, traduit par le C$^{\mathrm{en}}$. Caussin, Paris, 1804. This work is a translation of the Arabic manuscript belonging to the University of Leyden. A few of the observations were known to Tycho Brahe and were published by him in his Historia Cœlestis. As a slight indication of the value of these eclipses it may be remarked that the two or three given by Tycho Brahe furnished the first data from which the secular acceleration of the moon was deduced. It is therefore a singular fact that no comparison of them with modern tables has ever been seriously attempted.

There are, in all, in this book, observations of twenty-five eclipses including thirty-four phases of beginning or ending. They were all reduced and compared with the tables of Hansen. Three of them were so far discordant that they had to be rejected entirely. This ratio of three out of thirty-four will not appear great if we reflect that the manuscript from which the observations were translated, was frequently very difficult to decipher or to translate, owing not only to the fading of the writing, but to the uncertainty of some of the terms which the author used. Besides these three discordant observations, there were two which could not be used because the altitude assigned to the moon at the time of the observation actually exceeded its meridian altitude. Here it was evident that there was something wrong, in recording, transcribing or translating the observation. The general result was that each observation of a phase gave the mean longitude of the moon with a probable error ranging from three to five minutes of arc. The results were divided into three groups, each made by a separate observer or set of observers, and therefore worthy of being considered as entirely independent. The mean result of each of these groups was as follows:—

Epoch, 846 $\delta\varepsilon = -4'{\cdot}4$
926 $\delta\varepsilon = -1{\cdot}1$
986 $\delta\varepsilon = -4{\cdot}8$

IV. Observations made after the revival of science in Europe and before the invention of the telescope. These observations were made by various astronomers from Regiomontanus to Tycho Brahe. But after a careful and laborious examination of all their observations I could find, I was led to the conclusion that none of them would throw any light on the problem. Before Tycho Brahe the observations were no better than those of the Arabs, while the time elapsed was one half that which has elapsed since the Arabian observations. No doubt the observations of Tycho Brahe are more accurate; but the records are so confused that it is impossible to obtain any definite result from them. In fact they preceded the invention of the telescope by so short an interval that it can hardly be supposed that they would throw much light on the question under consideration, however carefully they had been made. I searched carefully to find whether Tycho Brahe had ever observed an occultation, especially of Aldebaran; but could find no trace of any such observation.

V. Observations of occultations and eclipses made with a telescope but without a clock, the time being determined by the altitude of the sun or of some star observed with a quadrant. This class comprises the observations of Bullialdus and Gassendus, as well as some of the earlier of Hevelius. Bullialdus seems to have been the first one who actually observed the occultation of a star by the moon, but he does not appear to have been a skillful observer. The observations of occultations have the great advantage that the only error to be feared is that of the determination of time, always supposing that the phenomenon was actually seen. The disappearance of the star behind the moon's limb is, in fact, a sudden phenomenon which does not require any measure of distance to be well observed.

VI. Observations of eclipses and occultations made by Hevelius with a very imperfect clock regulated by altitudes taken with a quadrant with pinnules. It is well known that Hevelius would never use a telescope with his quadrant; so that the results to be derived from the observations of this most indefatigable observer do not correspond to the labor which he spent in making them. His observations are much better than those of Gassendus, but far more inaccurate than those made with the telescopic sights.

VII. Observations of Flamsteed at Greenwich, and of the astronomers of the French school, from 1672 to 1750. Flamsteed's observations were published in the Historia Cœlestis. Those of the French astronomers are not only for the most part unpublished, but seem to have been totally forgotten from the time they were made until I was fortunate enough to find them in the archives of the Paris Observatory in 1871. Not only were they wholly unreduced, but in many cases not even the name of the occulted star was given. The reduction of these observations has been the most laborious part of my work. The observers have left no explanations whatever of their mass of observations, and it was necessary to learn this by induction from the observations themselves; and from the calculations scattered here and there through the books. The errors of the instruments and of the clocks had to be investigated from modern data; and the observations have proved to be well worthy of the pains which were taken with them. Thereby, the motion of the moon has been traced back to 1675, an epoch seventy-five years before observations upon it have heretofore been supposed to commence. In the same class with these Paris observations are to be included those of DeL'isle at St. Petersburgh, with which I was furnished by Struve.

The following are some independent mean corrections given by the observations of Bullialdus, Gassendus, Hevelius, Flamsteed, and the French astronomers. The list is incomplete, as the discussion of the solar eclipses has not been finished; but it will suffice for the purposes of the present discussion:—

1621	+ 77″	1661	+ 37″
1630	+ 30	1666	+ 24
1633	+ 53	1680	+ 30·4
1635	+ 55	1682	+ 28·5
1639	+ 23	1715	+ 13·8
1645	+ 51	1725	+ 7·0
1652	+ 38		

The investigation is terminated at the epoch of 1750 so far as the reduction of observations is concerned, because there is reason to believe that Hansen's tables are not greatly in error from 1750 to 1865. We may, therefore, in this preliminary discussion consider the tabular errors zero between these epochs. For the epoch 1875 the correction given by some good observations of occultations is −8″0, a result 1″·7 less than that indicated by the observations at Greenwich and Washington. This discrepancy is quite surprising. It is, however, worthy of remark that Captain Tupman from a discussion of all the meridian observations made in Europe about the time in question obtained a mean result somewhat less than that given by Greenwich and Washington alone. It is well known that Hansen's term depending on eight times the mean motion of Venus minus thirteen times that of the earth is almost entirely empirical, being adjusted so as to satisfy the observations between 1750 and 1850. And since this term fails to satisfy the observations outside of these limits, in fact making the tables worse than they would be without it, it ought to be rejected from the comparison of theory with observation. Its effect upon the ancient results is, however, so small in comparison with the necessary error of the observations that its effect need not be taken into account.

From the individual corrections to the moon's mean longitude which have been given for the modern dates I have sought to obtain by a rough interpolation the actual corrections for every quarter of a century from 1625 to 1725. The general results are shown in the following table, of which I shall explain the several parts:

Table of residual corrections to the several theories of the mean motion of the moon.

	(1)	(2)	(3)	(4)	(5)
Epoch.	Hansen.	H′.	s=8″·8.	s=6″·18.	Δt.
−687	−11′	−11′	+16′	+39′	-70^m
−381	−27	−27	− 7	+10	−18
−189	−20	−20	− 4	+10	−17
+134	−16	−16	− 6	+ 4	− 6
846	− 4·4	− 4·4	− 2·4	− 0·2	0
926	− 1·1	− 1·1	+ 0·3	+ 2·1	− 4
986	− 4·8	− 4·8	− 3·8	− 1·3	+ 2
1625	+50″	+33″	+ 6″.1	− 6″·6	$+12^s$
1650	+39	+18	− 6 ·9	−19 ·0	+ 3
1675	+32	+15	− 7 ·4	−18 ·6	+34
1700	+21	+16	− 3 ·6	−13 ·5	+25
1725	+ 7	+16	− 0 ·3	− 8 ·6	+16
1750	0	+19	+ 6 ·4	0 ·0	0
1775	0	+21	+12 ·5	+ 8 ·4	−15
1800	0	+15	+11 ·1	+ 9 ·5	−17
1825	0	+ 2	+ 3 ·0	+ 4 ·4	− 8
1850	0	−11	− 4 ·6	0 ·0	0
1875	− 8	−28	−15 ·8	− 7 ·6	+14

In column (1) we have the mean correction indicated by observations to Hansen's tables of the moon without any modification whatever. In column (2) these corrections are modified by the effect of Hansen's empirical term, so as to show the corrections to the pure theory after this term is subtracted from the tables. If the theory is perfect, these numbers ought to be represented by corrections to the mean longitude and mean motion of the moon and the secular acceleration.

The following are the several corrections given by the method of least squares:

$$\left.\begin{aligned}\delta e &= +19''{\cdot}57\\ \delta n &= -12\ {\cdot}31\\ \delta s &= -\ \ 3\ {\cdot}36\end{aligned}\right\}\text{Epoch, 1700.}$$

The value of the secular acceleration adopted by Hansen is 12″·17. Subtracting the correction it seems that the acceleration to which we are led by observation alone, is 8″·8.

Column (3) shows the outstanding corrections which remain after subtracting the result of the corrections we have just found. It is evident that the theory does not represent the observations, and that the most recent observations indicate a value of the secular acceleration much less than that indicated by the older ones. If we investigate the uniform variation of the acceleration which would best satisfy the whole of the observations, we shall find it to be −0″·9 in a century. The hypothesis of such a uniform variation is, however, too improbable to be admitted; and moreover, it still fails to represent the modern observations, although the ancient ones are thus greatly improved.

In recent times it has been generally considered that the difference between the theoretical acceleration and that given by observations arises from a change in the length of the day. It is worthy of remark that by supposing this change itself subject to variations, all the apparent changes in the mean motion of the moon can be accounted for. This is a hypothesis which I have suggested in former numbers of this Journal, as one by which the changes in question may be explained. Let us now see what the actual variations in the rotation of the earth must be to account for the difference between observation and theory. In the first place, the secular acceleration must be supposed to be uniform and equal to 6″·17. Two epochs at which we may suppose the time given by the rotation of the earth to be correct, being entirely arbitrary, we shall take 1750 and 1850 for these epochs. Having thus formed a theory of the moon's mean motion founded on gravitation alone, column (4) shows the apparent corrections indicated by observation. In column (5) these corrections are changed into time. The times here given are hypothetical errors of the earth's rotation which it is necessary to subtract from the times given by astronomical observations in order to reduce them to a perfectly uniform measure of time. The sign + indicates that the earth is ahead of its mean rotation, and the sign − that it is behind it. For some years past it has seemed to me that this was the most probable hypothesis on which to explain the deviations in question. It was evidently a most unwelcome one; for, granting its truth it would be no longer possible to predict the apparent motion of the moon, since the changes in the rotation of the earth could not be expected to follow any determinate law. It is therefore extremely gratifying to find that the comparisons we have just given lead to the hope that these deviations may, after all, be due to the action of some of the bodies of the solar system. A very cursory examination of the residuals given in column 3 shows that they have apparently a period not very far from 260 years. Now, it is remarkable that this differs very little from the period of Hansen's first inequality, which is 273 years. The question therefore arises whether the deviations in question may not be explained by a change in the constants of this inequality. The result is very surprising. By merely diminishing the argument of Hansen's first inequality by 60° 48′ without changing the co-efficients at all, the observations from 1625 to 1875 may all be represented within the limits of error. In fact, we see that the numbers in column (3) may be very nearly represented by the formula

$$-5''\!\cdot\!04 - 10''\!\cdot\!14 \left(\frac{t-1800}{1800}\right) - 15''\!\cdot\!50 \cos A,$$

in which we have placed,

$$A = 18V - 16E - g,$$

V being the mean longitude of Venus counted from the equinox of 1800, E that of the earth counted in the same way, and g the mean anomaly of the moon. The comparison in question is shown in the following tables; the fourth column of which is taken from the corresponding column of the preceding table. The residuals still outstanding are shown in the last column.

Epoch.	A.	Computed terms.	Observ.	Diff.
1625	−47°·0	+2″·2	+6″·1	+3″·9
1650	−14 ·0	−4 ·7	−6 ·9	−2 ·2
1675	+19 ·0	−7 ·1	−7 ·4	−0 ·3
1700	52 ·0	−4 ·5	−3 ·6	+0 ·9
1725	85 ·0	+1 ·2	−0 ·3	−1 ·5
1750	118 ·0	+7 ·3	+6 ·4	−0 ·9
1775	151 ·0	+11 ·0	+12 ·5	+1 ·5
1800	184 ·0	+10 ·4	+11 ·1	+0 ·7
1825	217 ·0	+4 ·8	+3 ·0	−1 ·8
1850	250 ·0	−4 ·8	−4 ·6	+0 ·2
1875	283 ·0	−16 ·0	−15 ·8	+0 ·2

Correcting Hansen's term by this empirical addition, we find that instead of

$$15''\!\cdot\!34 \sin (A+30^\circ\!\cdot\!2),$$

the value given by Hansen, we shall have

$$15''\!\cdot\!3 \sin (A-30^\circ\!\cdot\!6),$$

as the result of observation.

As a test of this result, the sum of all the corrections here found to Hansen's tables has been taken and compared with the corrections given in column 1. It is to be remarked in the first place that the diminution of 10″ a century in the mean motion of the moon involves a further correction of −0″·4 to the value of the secular acceleration in order that the ancient observations may still, on the average, be best represented. Thus the secular acceleration reduces to

$$8''\!\cdot\!4\,;$$

and the total correction to the acceleration of Hansen is

$$-3''\!\cdot\!76.$$

We put V_2 for the empirical term of Hansen,

$$21''\!\cdot\!47 \sin (8V-13E+274^\circ\ 14'),$$

the existence of which appears to have been entirely refuted by the researches of Delaunay; and T for the time counted in centuries after 1800. Then the total corrections to the tables of Hansen are as follows:—

$$-V_2-1''\!\cdot\!14-29''\!\cdot\!17\,T-3''\!\cdot\!76\,T^2-15''\!\cdot\!5 \cos A.$$

The following are the values of these corrections for the principal epochs from 1625 to 1900. The computation and comparison with observation is given so fully that any explanation of the table appears to be unnecessary.

Epoch.	$-V_2$	−15″·5 cos A.	− 1″·14 − 29·17 T − 3·77 T^2.	Sum.	Observation.	Diff.
1625	+17″·1	−10″·6	+38″·4	+44″·9	+50″	+5″
1650	21 ·4	−15 ·0	34 ·1	40 ·5	39	−1·5
1675	16 ·9	−14 ·7	29 ·4	31 ·6	32	+0·4
1700	+ 5 ·2	− 9 ·5	24 ·3	20 ·0	21	+1·0
1725	− 8 ·6	− 1 ·4	18 ·6	8 ·6	7	−1·6
1750	−18 ·9	+ 7 ·3	12 ·5	+ 0 ·9	0	−0·9
1775	−21 ·2	13 ·6	+ 5 ·9	− 1 ·7	0	+1·7
1800	−14 ·7	15 ·4	− 1 ·1	− 0 ·4	0	+0·4
1825	− 2 ·1	12 ·4	− 8 ·7	+ 1 ·6	0	−1·6
1850	+11 ·4	5 ·3	−16 ·7	0 ·0	0	0·0
1860	15 ·7	+ 1 ·8	−20 ·0	− 2 ·5	+1·5	+4·0
1870	19 ·0	− 1 ·7	−23 ·4	− 6 ·1	−5·5	+0·6
1880	20 ·9	− 5 ·2	−26 ·9	−11 ·1	----	----
1890	21 ·4	− 8 ·4	−30 ·4	−17 ·4	----	----
1900	+20 ·6	−11 ·2	−34 ·1	−24 ·7	----	----

The only case in which the difference exceeds the possible error of the comparisons is at the epoch 1860. As an explanation of this I can only suggest that the term found by Mr. Neison as due to the action of Jupiter is at that time added to the result of a possible error in Hansen's value of the term which depends upon the ellipticity of the earth. The comparison may therefore be improved when the theory is suitably corrected.

The great question which now arises is this. Is it possible that this correction to the term produced by the action of Venus can really be a result of the attraction of that planet? We are struck by the fact that the proposed change can be expressed by a mere change of the algebraic sign of the constant term of the argument, leaving the value of the co-efficient unchanged. It may therefore be inquired whether it is possible that the sign of this quantity is erroneous in Hansen's formula. This question must be answered in the negative. I have found by an investigation still unpublished, substantially the same result as Hansen; while the researches of Delaunay published in the Connaissance des Temps for the year 1862 show that the approximate expression of the constant term in question, is

$$180°-2h'',$$

h'' being the longitude of the node of Venus, which does not differ much from 75°. It is, therefore, a mere chance that the change of Hansen's term can be expressed in this way.

Although Hansen, Delaunay, and myself have all arrived at the same result for the value of the term in question, I cannot confidently say that that result is complete. In all three computations the terms of the second order due to the mutual attraction of Venus and the earth are neglected. It is evident that in consequence of this mutual attraction, the direct action of Venus on the moon is different from what it would be if each planet moved in its elliptic orbit. It may be that this difference is sensible in terms of so high an order as those under consideration. I have actually computed the additional terms in $\frac{1}{\Delta^3}$ (Δ being the distance of Venus from the earth) which arise in this way and which depend upon the argument $18V-16E$. The result is that the values of the several parts which make up this term are quite comparable with those of the elliptic terms which depend on the same argument; but these co-efficients destroy each other in taking the sum. I have, however, always regarded my computations on this subject as incomplete, and have, in consequence, never published them.

As the case stands, the marked agreement between theory and observation which is produced by the introduction of this empirical term, seems to me such as to warrant its provisional use until a more careful investigation of the subject can be made.

Washington, Oct. 3, 1877.