On Mr. Cowell's Discussions of Ancient Eclipses of the Sun.
By Simon Newcomb.

In his various papers discussing Greenwich observations of the Moon, Mr. Cowell has made so important a step in advance in the method of deriving results from a long series of observations, and in unravelling the intricate network of mutual relations between the quantities he has to investigate, that any conclusion he reaches in this field must carry great weight. This fact seems to render it all the more desirable to make a critical examination of his conclusions when his methods seem so open to question as do those adopted in his two papers, "On the Secular Acceleration of the Moon's Longitude and Node" and "On the Secular Acceleration of the Earth's Orbital Motion" (Monthly Notices, vol. lxv. p. 361 and vol. lxvi. p. 3).

I begin with a glance at the available material. Historical research has brought to light some forty or more records or statements, extending between B.C. 1069 to A.D. 200, which may be considered with a greater or less degree of probability to refer to eclipses of the Sun. In most cases the conclusion that an eclipse is referred to is undoubted; the other cases range through every degree of doubt from plausibility to absolute uncertainty. The date is frequently so doubtful that the identification of an eclipse is more or less uncertain, even when granted that an eclipse was referred to.

Several recent investigators, Airy, Hansen, Oppolzer, Ginzel, and now Cowell, have investigated various selections, generally from three to five, of these eclipses with a view of deriving corrections to the secular motions of the Moon's elements. But there is no agreement among any two of the authors except, perhaps, Airy and Hansen; and no one of the results agrees with conclusions derived from modern observations by the aid of gravitational theory. Yet there is no reason to impugn the correctness of the narrative when we measure correctness by the standard of the historian. As I have said, in the majority of cases the eclipse is so far identified as to show that the statement of the historian was well founded. But it does not follow from this that the eclipse can be utilised for any astronomical purpose. As I have repeatedly pointed out, there being no observations of times or phases, the only fact we can take as the base of a conclusion is that a well-identified eclipse was total at a known place. By a curious fatality there is always some weak point in each of the small number of cases in which this condition is presumably satisfied by the narrative. In other cases the (as it seems to me) gratuitous assumption has to be made that the eclipse was total at the point where the record was discovered, or where the historian was supposed to have lived.

In the Monthly Notices, vol. lxv. p. 61, Mr. Cowell discussed five of these thirty or forty eclipses, and found, on the assump-
tion that each was total at a given place, that the whole five could be represented by a correction to the secular acceleration of the Moon's node, the reality of which would involve the conclusion that, in ancient times, the annual motion of the node was about 1''3 different from the result of gravitational theory. As the correctness of gravitational theory at the present time is proved by Brown's researches, this conclusion would imply that a law of nature was different in ancient times from what it is now.

In the Monthly Notices for November 1905 Mr. Cowell accepts the inadmissibility of this conclusion, and shows that his eclipses also be represented by supposing a sidereal secular acceleration in the mean longitude of the Sun of +4''1. In view of the fact that no such acceleration is shown by 150 years of modern observations of the Sun, and that if it exist it must be due to some other cause than gravitation, it would seem to require a good deal of proving. To test the sufficiency of Mr. Cowell's proof we must consider his evidence in detail.

His first eclipse is one recorded as occurring at Babylon -1062 July 31. Although the record does not distinctly state that the phenomenon was actually observed at Babylon, the presumption that such was the case is fairly plausible. In affording even a plausible conjecture of totality at a definite place this eclipse seems to me exceptional.

In his No. 2, recorded at Nineveh, nothing is said in the record about the eclipse being total. If we accept the modern tables as correct the southern limit of totality passed about sixty miles north of Nineveh. I see nothing unlikely in the hypothesis that this eclipse, supposing it thus to have occurred as our modern tables show, should have been recorded at Nineveh, as it must have been very striking at that point.

His eclipse No. 3 is that of Archilochus in -647. Here we have nothing to base a conclusion upon except that a certain writer vaguely mentions what undoubtedly were the phenomena of a total eclipse, but without indication when or where the phenomena occurred. It seems to be doubtful even where Archilochus lived, as Ginzel says he probably lived either at Pharos or Thasos. The identification of the description with the eclipse of -647 seems to be little more than a plausible conjecture. The only conclusion which it seems to me can fairly be drawn from the record is that at some time in the seventh century B.C. there occurred a total eclipse at some point near enough to the residence of Archilochus to admit of his hearing about it.

Eclipse No. 4 is that of Thucydides -430. This was an annular eclipse by our tables; but I see no reason for inferring that the eclipse was fully annular at Athens.

The fifth and last eclipse is that of Tertullian at Utica. Of this eclipse the writer says that it was not quite total—extinctor pene lumine. This statement of the historian is quite in accord
with the tables if the latter indicate that the total phase passed anywhere near Utica. But in order to reach a conclusion Mr. Cowell assumes that the writer meant something different from what he says, and that the eclipse was total at Utica.

By these interpretations five of the six authorities are brought into unanimity. But how is this unanimity reached? In the most unambiguous record the interpretation put upon it is only a plausible one; the totality of the second and third eclipses at the assigned places is largely hypothetical; in the fourth, which was annular, the conclusion is doubtful; and the last witness was interpreted as meaning something different from what he said—that is to say, an eclipse which he distinctly describes as not total was assumed to be total. Unanimity reached in this way does not seem to me a sufficient basis for a conclusion so far-reaching as a secular acceleration of 4'' in the mean longitude of the Sun.

I may advert to another phase of the subject—the relation of the question to the tidal retardation of the Earth. The apparent acceleration arising from this cause results in a reaction of the Earth on the Moon causing a proportional retardation of the latter. The amount of this retardation is easily computed, and the result is found to be a diminution of the apparent increment of the acceleration of about \( \frac{1}{2} \) part of its entire amount. Nothing can therefore be based on this reaction.

Two phases of the subject are alluded to by Mr. Cowell in the Monthly Notices, vol. lxvi. pp. 5, 6. He there alludes to my non-use of the observed magnitude of the Ptolemaic eclipses of the Moon. On this it will suffice to remark that I should regard it as assigning an unwarrantable degree of precision to the observations to suppose the magnitude of the eclipses precise to \( \frac{1}{10} \) part of the Moon’s diameter. This uncertainty corresponds with one of 180” in latitude, or more than half a degree in the longitude of the node. In view of the fact that the longitude of the node at the time of these eclipses can probably be fixed from modern observations within 1’ it does not seem worth while to consider observations the probable error of which is 30’.

Mr. Cowell also alludes to the case of a Ptolemaic eclipse in which, as I said at the time, if my corrections be real, the Moon must have set at Babylon before the eclipse was total. In this he seems to be unaware that it was pointed out by Oppolzer that the record did not state that the observation was made at Babylon, as I had wrongfully supposed, but left open the easy inference that it was observed on Grecian territory.

On the whole Mr. Cowell’s conclusions seem to me in need of better evidence than that which he adduces; and one might also fairly inquire whether it is sound to confine the discussion to six out of twenty or thirty real or supposed eclipses.
June 1906.  Mr. Cowell, On Ancient Eclipses.  


Two papers have lately appeared in the Monthly Notices in criticism of the conclusions that I have drawn from ancient eclipses.  I have read Mr. Nevill's paper with the greatest possible interest; he takes the standpoint that the question is one to be settled by the evidence, and not by preconceived theory, and he brings forward a large number of additional eclipses with which he tests my conclusions.  I intend, therefore, to perform as soon as possible my own calculations upon these eclipses.  I shall supplement Mr. Nevill's results by giving the least distance between the centres of the Sun and Moon as seen from a specified place, with symbolical corrections for the unknown quantities whose values are to be determined.  At the present time, before these calculations are ready, I can offer only a few general remarks.

1. I did not choose the date —647 for the eclipse of Archilochus, but found it already assigned, and I adopted it without further inquiry.  I now admit the force of Mr. Nevill's reasoning, and the eclipse must therefore be relegated to the same category as the eclipse of Agathocles.

2. Siwan was the third Babylonian month.  If, therefore, July 31 was the end of Siwan, the year must have begun about May 4, and the month that began on April 4 was presumably an intercalary month.  The equinox fell about March 31, and the difficulty is, therefore, only one of a few days, and not of one or two months, and consists in the question, Why was April 4 not recognised as subsequent to the equinox? But in any case errors in the calendar which might be impossible a few centuries later are not necessarily inadmissible at an earlier time.  I understand that Mr. Nevill's alternative of relegateing the eclipse to the twelfth century does not find favour with Mr. King.  Mr. Maunder is sending an interesting note on this question to the July number of The Observatory.

3. Thucydides was an Athenian writing principally for Athenians.  I consider the probability great that he either saw the eclipse at Athens himself, or that—if the phenomenon was different at Athens from what he described—some of his friends would have told him so.

4. In the case of the first twelve eastern eclipses given by Mr. Nevill I find in M.N. xxiv. p. 42, the statement that the eclipses of —708 and —600 are alone described as total.  For these two eclipses, as well as for those of —180, —187, and —299, subsequently given by Mr. Nevill, my system does very well, and distinctly better than any of Mr. Nevill's three systems.  The eclipse of —351, as pointed out by Mr. Nevill, fits no system of corrections.

5. Mr. Nevill makes no allusion to the lunar eclipses of the Almagest, which support my conclusions.