TRAVELLING this evening between Plymouth and Exeter, I pulled the screens over the light in my compartment to enjoy the moonlight, and was rewarded by seeing a fine display of aurora borealis, which was, I hope, witnessed by some other of your readers.

Between 9 p.m. and 9.30 p.m., when near Totnes, there was a bright flattened arch near the northern horizon, with white streamers rising from it at intervals, and very bright patches of rose-red, extending from north-east to north-west, and passing nearly overhead. At 9.15 one of these patches, on the right of the Great Bear, was a veritable "pillar of flame," and was more remarkable because of its contrast to the moonlight, which was very brilliant.

I think I am right in saying that a similar display has not been seen in the south of England for twenty-five or thirty years, and the last "rose-red" display that I can remember was in 1870. R. LANGTON COLE.

November 15.

A LUNAR THEORY FROM OBSERVATION.

ON June 3, visitation day at the Royal Observatory, Greenwich, the editor, who is a member of the board of visitors, asked me to write an account of my researches on the moon for NATURE. I delayed doing this for a few months in order to render my account more complete.

The moon's longitude contains about 150, and the latitude about 100, inequalities over $0''.1$. The arguments of these inequalities, and the mean longitude of the moon, require a knowledge of three angles connected with the moon, viz. the moon's mean longitude, the mean longitude of perigee, and the mean longitude of the node. The other angles involved in the arguments define the position of the sun, planets, the solar perigee, &c., and their values are to be determined from other observations than those of the moon.

The problem that I have had in view, therefore, is to determine the values of three angles as functions of the time, and to give a list of some 250 inequalities in all as accurately as possible.

Before the time of Newton, this was clearly the only way the problem of the moon's motion could be attacked, only the limit worked to was then more nearly $500''$ than $0''.1$. Since the time of Newton, the method has been almost entirely abandoned. Many mathematicians have attempted to calculate how the moon ought to move; the comparison between its observed and theoretical course has been rough in the extreme. No attempt has been made to verify from observation the coefficients of those inequalities for which a theoretical value had been calculated; observation has merely been required to furnish values for those constants which are theoretically arbitrary, and, as I shall show, the determination of these constants has often been rendered less accurate than was necessary by the tacit assumption that all theoretical terms had been accurately computed.

My point of view, as I have said, is that which was necessarily the only one before the time of Newton. Let us consider the application of this most ancient of all methods to the time when no observations were possible except a record of eclipses.

The two principal inequalities of the moon's longitude are

$$22640'' \sin g + 4586'' \sin (2D - g),$$

where g is the mean anomaly and D the mean elongation of the moon. Whenever the moon is either new or full, $2D = 0$; at such times, therefore, the two inequalities are indistinguishable from a single inequality

$$22640'' - 4586'' = 18054'' \sin g.$$

The "evection," as the smaller inequality is called, could evidently not have been discovered until the moon was observed near its quarters; moreover, a correct value of the eccentricity of the moon's orbit could never have then been obtained. On the other hand, so long as the sole object of astronomers was to obtain places of the new and full moons it did not matter whether the two inequalities were separated or not. Roughly speaking, material of a limited class is always good enough for generalisations confined to the same class; it is unsafe to extend the generalisation to a wider class, as in this instance it would be wrong to predict for the quarters of the moon from the formula $18054'' \sin g$.

When we have an extended series of observations and wish to determine whether a term $x \sin \alpha t$ runs through the errors, and, if so, to determine x, the theory of least squares directs us to multiply each error by $\sin \alpha t$ and add. But before equating

$$x\Sigma \sin^2 \alpha t = \Sigma \epsilon \sin \alpha t$$

we must pause and consider whether there may not be some other error $y \sin \beta t$ running through the observations such that

$$y\Sigma \sin \alpha t \sin \beta t \text{ is not zero.}$$

Now an interfering term of this sort may arise in two ways:—(1) β may differ so little from α that throughout the whole series of observations the difference between αt and βt does not take indiscriminately all values from $0°$ to $360°$; (2) the difference between αt and βt may be exactly equal to the mean elongation of the moon, in which case, since the observations are not uniformly distributed round the month, the two inequalities are liable to be confounded, just as the elliptic inequality and the evection were confounded in the early days of astronomy. Interference of the first kind can be eliminated by sufficiently extending the series of observations, but no amount of observations will obtain a correct result in the second case if the mathematical point is overlooked.

As a result of attending carefully to these considerations, I have succeeded in obtaining practically the same value of the eccentricity of the moon's orbit from two different series of observations compared with two different systems of tabular places. Hansen and Airy have given values of the same quantity differing by more than one second of arc. For the same reason, the value of the parallactic inequality of the moon obtained by me corresponds closely with the value of the solar parallax obtained in other ways. The consideration neglected by Airy in this case was the possibility of error in the tabular semi-diameter.

I have determined from the observations the coefficient of every term the coefficient of which was known to exceed $0''.1$. This constitutes, as I have said, the solution of the problem of the moon, as it presented itself before the time of Newton. It forms, too, the proper basis for comparing observation with theory. Previously the only thing known about the vast majority of terms was that whereas the apparent errors of Airy's tabular places frequently exceeded $20''$, those of Hansen's seldom differed from the mean of neighbouring observations by so much as $5''$, a quantity that might be attributed to errors of observation entirely. When, however, Newcomb in 1876 came to re-determine the value of the moon's eccentricity (in his immediate object he was not particularly successful owing to the neglect of the considerations I have just set down), he brought to light a term the coefficient of which is one second, and the argument of which was at the time unknown. The discovery of this term shows how unsafe it is to test the tables by the mere inspection of the series of errors of individual observations. However, in all my far more exhaustive search I only brought to light one fresh inequality that runs through the errors, and that is to all appearance due to an error in the adopted parallax of the moon. My analysis, however, enables me to say that the solution of the problem of three bodies, as recently completed by E. W. Brown, is final. This might fairly be inferred from its agreement with Hansen and Delaunay, and from the numerous equations of verification employed throughout by Brown. But on my analysis a further remark may be based; not only are Brown's expressions a correct solution of his differential equations, but those differential equations do really represent, with all necessary accuracy, the problem of three bodies as presented by nature. The problem has been solved. If in the future a method as much superior to Hill's as Hill's is to Hansen's were to be invented, it would no doubt be worked out numerically, but no matter how ingenious it might be, the test of its accuracy would be—does it agree with Brown?

Another inference may be drawn from what I may call my empirical lunar theory. As the coefficients of solar terms are verified by Brown's calculations with a probable error of about $0''.04$, that is presumably a measure of the accuracy of the constants. Moreover, on comparing the planetary and figure of earth terms with theory, larger discordances are found, especially in the figure of earth terms and in the Jupiter evection term. There is no special difficulty in obtaining these terms from observation; they are presumably determined as accurately as the others. Consequently, appreciable errors still exist in the theoretical values of the figure of earth terms and the Jupiter evection term.

Two suppositions of Hansen's on which he founded alterations of his tables have also been disproved, a mechanical ellipticity of the moon and an eccentricity in the face that it exhibits to the earth.

I come now to another class of investigations. The theory of the moon is deficient in that it does not explain the cause of a term of period of about 300 years and coefficient $15''$ which observation shows to exist. This deficiency of theory is an inconvenience in many ways. It renders the determination of the secular acceleration of the moon, and the resulting measurement of tidal retardation, impossible from modern observations. It will be years, possibly two centuries, before from observation alone a really accurate estimate of the missing term can be given, unless, as is much to be hoped, theory accounts for it in the meanwhile. This unknown term renders difficult also the determination of the motion of the node and perigee. The position of the perigee is found by measuring an arc equal to the mean anomaly back from the mean position of the moon, and it is fairly clear that the unknown term is also an inequality of the anomaly. Hence the motion of the anomaly contains a periodic part that it is difficult to allow for accurately. I have determined the motion of the node and perigee over a period of 150 years, and I get small differences from the theoretical values recently published by Brown. Possibly the cause that produces the term of long period also produces a small motion of the node and perigee. Hansen assumed an empirical term of 240 years' period for this unknown term, but before Hansen's tables had been in the Nautical Almanac for twenty years, Newcomb found it necessary to change the period assumed to 273 years. Each assumption was associated with an argument in the hope that it would turn out to be the correct argument, but both in turn have been disproved. My own idea as to the term is that its period is more nearly 350 years, and I have no suggestions to make as to its argument. There are also smaller terms of 40 and 70 years' period approximately, or possibly the errors assume a more complicated form still. The periods are so long that the uncertainty is great.

The last section of my investigations deals with the ancient solar eclipses and the value of the secular accelerations. The three angles mentioned at the outset of this paper as requiring measurement contain terms proportional to the square of the time. It is evident that these terms become of considerable importance at remote epochs. Also on their accurate determination depend (1) the degree of assistance that astronomy can extend to chronologists; (2) a numerical estimate of the tidal retardation of the earth's diurnal rotation.

I have succeeded in showing that the alteration of two of the secular terms renders total, or at any rate central, five ancient eclipses which are partial according to the existing tables. This may, of course, be an extraordinary coincidence, but it seems more natural to suppose that records of the eclipses have come down to us because they really were striking phenomena worth recording—in one case the account says "fire in the midst of heaven," which seems to indicate the corona, and therefore totality. There is also the further fact in favour of these corrections that one of them is confirmed and the other supported by the ancient lunar eclipses. It may be of interest to mention that the most ancient eclipse of the five was communicated to me from the British Museum after I had deduced corrections from the other four, and that the corrections already found satisfied the condition of totality for the newly discovered eclipse. To such an extraordinary piece of luck the words of Virgil seem applicable:—

> "Turne, quod optanti divom promittere nemo
> Auderet, volvenda dies en attulit ultro."

It had occurred to me to wonder whether it was worth while to write to the British Museum, but the chance seemed so small that I was letting the days slip by without doing so.

Ancient eclipses, therefore, give an accurate measure of the relative distances of three points, the positions of the node, the sun, and the moon. The next question is, "Where is the equinox relatively to these three points?" My first interpretation of my results proceeded thus:—The position of the sun relatively to the equinox has never been called in question. We may be assumed to know it. Therefore my calculations determine the distance of the node from the equinox. This view of the matter, I now am glad to say, was found on examination to be untenable. In the words of Dante, what I spun in October did not last until the middle of November (the date of the first meeting of the Royal Astronomical Society):—

> "a mezzo novembre
> non giunge quel che tu d'ottobre fili."
> Purg., vi., 143.

The position of the node, in fact, may be inferred with certainty from the gravitational calculations of Prof. Brown. Hence my eclipse results determine the position of the sun as well as of the moon. The conclusion is that the sun's motion is being accelerated.

The most obvious hypothesis to account for this observed fact—it does not follow that it is the only hypothesis—is that the æther has a sensible retarding effect. It may seem curious that the resistance of the æther should accelerate the earth's orbital motion, but that undoubtedly would be the effect. The total energy must be diminished, and this implies that the planet falls in towards the sun and consequently revolves faster in its orbit. P. H. COWELL.