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