at the north end of the Observatory domain; he also added smaller and less permanent buildings in other parts of the grounds, as they were required by the addition of branches of work to the Observatory scheme. It was left for Mr. W. H. M. Christie to replace these by a building more worthy the nation and more suitable for the conduct of the daily work. It would be idle and unfair to compare the work of the present day with that of the time of the earlier astronomers. The main difference, apart from the increase in the number of branches dealt with, is that the observations are now reduced and published shortly after they are made, whereas it had to be one of Airy's first tasks to reduce the lunar and planetary observations of Bradley, Bliss, and Maskelyne. One of the annual Greenwich volumes of to-day is bigger than the two volumes which contain Flamsteed's work for forty-two years.

H. P. H.

E. W. Brown's Theory of the Motion of the Moon.

By this time Professor Brown's researches in the lunar theory require no notice from the point of view of those interested in the mathematical difficulties of a numerical lunar theory. The class is a small one, and every member of it is probably pretty well acquainted in detail with the work of Dr. Hill and Prof. Brown. We here attempt, however, to sketch an outline of this work, prefacing it with a popular account of the history of the subject, starting from very early times.

From the time of Copernicus the Sun has been recognized as the centre of the planetary system, and in some way responsible for its motion. Before Newton the prevalent opinion was that the Sun was responsible directly for the velocity of the planets in their orbits. In other words, given the Sun and the planet, it was inconceivable that the planet should not have its actual velocity. The mental attitude may best be conceived by the comparison of a vortex motion in liquid. Stir a basin of water until a cylindrical hollow appears in its centre of a certain shape and a certain velocity of rotation. Then if a match be floating in the water between the vortex and the rim of the basin, its velocity is entirely determined by the circumstances of the moment. In the case of a falling stone, on the contrary, the velocity is determined by the time that the stone has already been falling. It is easy to conceive the same stone occupying the same position and moving in a different way. The match in the basin could not move in a different way. Newton demonstrated that the Sun is only responsible for the change of motion of the planets, and identified the cause that keeps the planets in their orbits with the cause that is responsible for the path pursued by a falling stone. Of course the philosophers who thought that the planets could only have their actual velocities, could conceive that no such condition was imposed on the stone. They did not see any analogy between the two problems. It was the nature of planets to move in circles, they thought, and it was the nature of fire to mount upwards, and it was the nature of a stone to seek the spot where the centre of the Earth was. There was no idea of the force of gravitation residing in matter. Dante, for instance, evidently supposes that all matter seeks to approach as nearly as possible to a certain spot, which the centre of the Earth has come to occupy in pursuance of this law. Dante would have considered that, if the whole Earth were pushed aside, it would come back again to the old position. The Earth was attracted, or rather had an innate desire to approach a certain spot; it was not itself the seat of attraction. Hence it is that Dante does not make the attraction fade away into nothing, as the poets approach Lucifer at the centre of the Earth; but, on the contrary, it is as strong as ever, but suddenly changes its direction as the centre is passed.

These ideas passed away on the discovery by Newton of the law of gravitation. Newton showed that the consequence of his law is that the planet or satellite would describe an ellipse round its primary except in so far as it is prevented by the attraction of third bodies. He also traced to the Sun the irregularities from elliptic motions in the Moon's orbit about the Earth.

Newton completely solved the problem of two bodies, or the motion of one body under the attraction of a second. The problem of three bodies, or the motion of a body under the attraction of two others, has never yet been solved, and probably cannot be. The recent researches of Prof. Darwin *, dealing only with a fringe of the whole subject, wonld almost warrant the belief that there is no path, however fantastic, that could not be pursued by a body under the attraction of two other bodies under proper initial conditions. Attempts at classification have been made by pure mathematicians, and the lines on which to proceed pointed out, but it must be acknowledged that the results are meagre in the extreme. Meanwhile it has been found possible to trace, with a high degree of universal accuracy, the consequences of Newton's law of gravitation in the actual cases that occur in the solar system. The labour is immense, but it has been performed in several different ways. Of all numerical calculations in celestial mechanics, the problem of the motion of the Moon is the most tedious, and the lines on which it is being attacked by Prof. Brown by far the best.

Tables have been constructed at various times by which the place of the Moon at any time can be found. These tables have been formed after long numerical calculations based on the special conditions known to obtain in the case of the Moon. In view of the extreme complexity the problem of three bodies is capable of assuming, it must be regarded as good fortune that tables are

* See 'Periodic Orbits,' vol. xxi. p. 121.

possible. It is true that, with Newton's law of gravitation and with sufficient labour, the Moon's place could in any case have been predicted in advance by a sort of dead reckoning. To illustrate this, consider a piece of driftwood in the sea. If we knew a great deal more than we do know, it might become possible to predict where that piece of wood will go to in a week, and then starting again with the new position, the prediction could conceivably be extended to a second week, and so on; but tables could never be made, for two reasons. One is that prediction would soon cease to be accurate. An error of an inch at the end of a week would become two inches at the end of the next week, and would soon make all the difference whether, for example, the log went up the English Channel or up the Irish Channel. The second reason is that in general such a log would not find itself back near some definite point at regular intervals. To calculate a place for any time we must then calculate for all intermediate times. Supposing, however, we could describe some course that we could prove that the log never got very far away from, and supposing that we could always predict how far away from this fictitious course the log would be at any time, then tables would be possible. Now it happens that the Moon is so close to the Earth that the disturbing action of the Sun is never very large in comparison with the attraction of the Earth, and hence we conclude that the Moon will for a time not get very far away from the ellipse that it would describe in the absence of the Sun. In some respects, however, the action of the Sun is not wholly oscillatory in its effects, and the result is that no fixed ellipse will continue to approximately represent the Moon's path. The difficulty was generally got over by considering an ellipse rotating in a convenient manner. Such an intermediary orbit, as it is called, suffered from one great inconvenience. Without the Sun the Moon could describe any fixed ellipse. It cannot, under any conditions, admitting a physical interpretation, describe a rotating ellipse. Hence the idea of the ellipse merely got over the difficulties of making tables, but it did not contribute much towards the solution of the problem of the Moon's motion.

Dr. Hill's great contribution to the lunar theory lay in this. He proposed to abandon the rotating ellipse as the intermediary orbit and substitute another curve, which is called the variation curve, to play the same part. The Moon never gets far from this latter curve, so that it shares all the advantages of the rotating ellipse. It is equally simple in its geometrical properties, and it has, ove and above, this immense convenience. It is a curve that the Moon could actually traverse if the actual conditions were slightly modified. It is true that the Moon does not actually describe the curve. It oscillates about it in four distinct ways. One oscillation is due to the fact that the Sun, though a long way off, is nevertheless at a measurable distance; a second is because the Sun is nearer in winter than in summer; these causes give rise to what may be called forced oscillations. Besides this there are two free oscillations; for these no cause can be assigned; they happen to exist and that is all. The Moon, for instance, might have moved in the plane of the ecliptic, but it does not do so. It also might have moved in a curve much more nearly circular than it does.

The lunar theory is reduced then to a calculation of oscillations about a state of steady motion, and only differs in being more laborious from those problems with which every student of advanced dynamics is familiar, such as, for example, the rising and falling of a top, or the wobbling of a hoop or a bicycle when not going quite smoothly. The nature of the conclusions is at once anticipated, and the embarrassments disappear. Clairaut gave his name to an artifice for expressing symbolically that the nodes of the Moon's orbit regrede. The artifice seems simple enough to those to whom it has been explained, but it evidently was a great discovery once. Considering the Moon's motion in latitude as an oscillation, no one is surprised that the nodes regrede. It merely means that the Moon does not take the same time to oscillate from side to side of the ecliptic that it takes to go round the Earth. No one expects the period of a bicycle's wobble, for instance, to be exactly the period in which the wheels or the pedals go round.

It is not easy to describe, in popular language, the stage in the computations that Professor Brown has reached. Roughly speaking, he has perhaps done the smaller half. The whole will take five years at least. By former methods it has taken twenty to get a less degree of accuracy. The leading idea, as has been explained, was due to Dr. Hill, but Professor Brown's papers are full of ingenious methods for simplifying the work; but perhaps what strikes the reader most is the orderly arrangement so that no term in long calculations should be forgotten, so that every stage of the work should be tested for accuracy, and so that every set of numbers should be ready to hand as it is wanted. No muddle could probably be so bad as a muddle in the middle of a lunar theory, and it is no small achievement merely to have avoided one.

P. H. Cowell.

The Observatories of India.

By the courtesy of the Secretary of State for India, we are in possession of the Report of the Observatories Committee of the Royal Society, who have been considering, for the Indian Government, the Reports made by the Astronomer Royal and Sir Norman Lockyer, mentioned in our August number.

1. The Committee consider that the work of the Indian Observatories should be directed to the following subjects (for some of which provision is already made):—