The following communications were read:—

I. Some account of the Transit of *Mercury* over the Sun, observed by Mr. Abraham Sharp, in the year 1707. By Mr. Baily.

It is stated by Dr. Halley, in the *Phil. Trans.* for 1725, that the transit of *Mercury*, which took place on the 24th of April 1707, happened so near midnight, that it escaped unseen by all the astronomers of Europe, excepting Mr. Roemer of Copenhagen: who, in fact, caught only a slight view of it, just as it was passing off the sun’s disc, on the morning of April 25th (or May 6th, new style), at 4h 19m; the planet being then about a 1/3 of the sun's diameter above his lower limb, and to the left hand, in an inverting telescope. "Altus supra imum solis marginem 1/3 diametri solaris, et ad sinistrum in tubo (sc. invertere)." If this statement be literally correct, the planet must at that time have had north latitude; which Dr. Halley says was absolutely impossible: and hence he draws the conclusion that it was not Roemer himself that witnessed the transit, “but some person less acquainted with these matters.”

In a paper by MM. Cassini and Maraldi, published in the *Mémoires de l’Acad. des Sciences* for 1707, page 175, they state that they looked out for this transit the whole day of May 5th, till sun set, and on the morning of May 6th at sun rise, but could not discover any signs of its appearance. They also received letters from Rome, Bologna, Marseilles, and Montpellier, stating the same circumstances. In fact, Dr. Halley’s computation made it to commence at a 1/4 past 8 o’clock on the evening of April 24th (old style) and to end on the following morning at a 1/4 past 4. Therefore, allowing something for the inaccuracy of the tables, and for a hazy atmosphere at the horizon, it might possibly have passed unobserved by the most diligent astronomers.

I have recently, however, found amongst Flamsteed’s manuscripts at the Royal Observatory at Greenwich, a letter addressed to him by his former assistant Mr. Abraham Sharp, wherein he not only gives an account of his having observed this transit, but also subjoins a diagram of its appearance. The observation indeed was but of short duration, and nearly under the same circumstances as seen by Roemer; whose account (with some slight modification to which I shall presently allude) it evidently confirms. I shall give Mr. Sharp's description of it in his own words, as follows: the letter is dated “Horton, May 17, 1707.”

“*You mention nothing of your observation of Mercury on the sun, on the 24th of April, though you put me in mind of it in your former [letter]; and your mentioning only the 25th day had like to have occasioned my neglect of the 24th, and total loss of it: but,
accidentally reviewing Parker's Ephemeris, I waited for it the 24th [in the] evening; the air was cloudy and thick. The 25th at sun rise, a thick bank of clouds run along the east part of the horizon, to 3 or 4 degrees of altitude (the rest of the sky being very clear): as soon as the sun got above the clouds, I prepared to view it with a 10 feet tube, and fortunately got a transient sight of Mercury, just at its passing off the sun's limb; which here was April 24 m 16° 52° ½". Though I had got the micrometer ready, yet the time was so short I could not use it. Its motion seemed so quick (being retrograde, contrary to that of the sun), it was vanished, and gone ere I could well recollect myself, and get sufficient assurance that my sense was not deluded. However, though the time differ somewhat from Parker's computation, yet all circumstances duly weighed, it could I think be nothing else. It appeared as in the scheme [annexed] like a dark spot on the lower side of the sun's southern limb, not distinctly terminate, but seemed encompassed with a thick haze or atmosphere. It passed off the sun's limb, as near as I could guess, about 10' below his horizontal diameter; so that it had something more south latitude, which is agreeable to Parker's calculation. I shall be glad to hear that you observed it, and the particulars.'

In Flamsteed's answer to this letter, dated May 29, 1707, he says, "I waited for Mercury in the sun, April 24 last: it was clear here till 4 minutes past 7 at night, and there was not the least spot in the sun that I could perceive. Next morning, April 25th, a bank of clouds lay upon the sun, and hindered us from seeing him till 5½ h, when he got above it, shone clear, and there was no Mercury on him: by your observation he was passed off a ½ of an hour before." And in the same letter, alluding to the diagram sent by Mr. Sharp, he says, "I thank you for yours of the 17th instant, especially for your observation of Mercury passing off the sun: but whether it be that your paper was wet or otherwise, so broad a blot is made for Mercury, as could no ways agree with the appearance. For Gassendi makes the body of Mercury not above 20 h diameter; Hevelius, in an April transit, but 12": which is something less than it probably was, because opaque bodies, seen in lucid, have their diameters diminished. But yours of Mercury seems about one-tenth of the sun's diameter. Pray, clear me in this particular, and let me know how the planet appeared as he passed off the sun's limb. He might seem confused; but not more than the limb of the sun he was leaving, nor bigger than he really was."

Mr. Sharp's reply to this inquiry is very satisfactory: it is dated June 16th, and wherein he says, "I am sorry the clouds did not permit you to observe Mercury's transit through the sun; that what I saw might have been confirmed: which being so transient and short, could not be certain as to any thing but the time. The
scheme I drew was only designed to represent the posture of the planet's appearance: the magnitude was merely conjectural; or rather, done at random without any consideration of its due proportion. Through inadvertency it was made larger than it ought; though I must acknowledge it seemed to me to be much greater than I expected: which might possibly arise from the thickness of the atmosphere, being so near the horizon, and the air not clear in that part. Which might also occasion its appearing so indistinct and undeterminate: and this haziness or atmosphere surrounding it might represent it to my imagination larger than in truth it should be. However, having had no leisure to measure any thing, I hope you will pardon the errors of a figure drawn only by conjecture."

It is evident therefore that this transit was seen by Mr. Sharp: for the appearance which he witnessed could not have been a spot upon the sun, since it would not in that case have vanished so suddenly; and moreover Flamsteed states that on the preceding evening the face of the sun was quite free from any such appearance. The only point in which this account seems to differ from that given by Roemer is with respect to the position of Mercury as regards the ecliptic: Mr. Sharp stating in direct terms that it had south latitude, which in fact agrees with the computations; whilst the inference from Roemer's statement is, that it had north latitude. Now this is a fact on which no observer could possibly be mistaken, where the quantity was so large as that which is here acknowledged by both parties. I apprehend therefore that in the account given by Roemer, as above quoted, the words within the parenthesis must be restricted to those immediately preceding, namely to "ad sinistrum in tubo," and that they do not extend, nor were intended to be applied to "supra imum solis marginem." With this interpretation of the passage, the two accounts will agree as well as might reasonably be expected, from such a slight and transient observation: and in no other way can the difficulty be reconciled.

II. On the Calculation of Eclipses and Occultations. By J. W. Lubbock, Esq., Treas. and V.P.R.S.

Lagrange, in the Astronomisches Jahrbuch for 1782, first deduced the theory of eclipses from the general principles of analytical geometry. He employed as data the true longitudes and latitudes of the heavenly bodies. Mr. Lubbock is of opinion that had he possessed an Ephemeris as complete as the Nautical Almanac, he would have explicitly used right ascensions and declinations, both because the co-ordinates of the observer's place are thus more readily expressed in terms of known quantities, and because the manner in which the moon's places are now given dispenses with all interpolation except that which is easy and safe.

Mr. Lubbock, starting from the conditions of the problem in its most general form, has deduced general expressions which, when the data are longitudes and latitudes, may be made to furnish those of Lagrange above mentioned, and has developed them in a form for use, taking right ascensions and declinations as data. He has also given some equations, which he has not seen else-