LVII. *Aſtronomical Obſervations made at the Iſland of St.* Helena, *by* Nevil Maſkelyne, *M. A. Fellow of* Trinity College, Cambridge, *and F. R. S.*

TO

THE RIGHT HONOURABLE

The Earl of MORTON, PRESIDENT,

AND

The FELLOWS of the ROYAL SOCIETY,

THE

Following Obſervations made, when I was employed, by their Appointment, at ST. HELENA,

ARE

Moſt reſpectfully preſented, by,

His Lordſhip's,

And the Royal Society's,

Moſt obedient,

Humble Servant,

Nevil Maſkelyne.

THE

Read Dec. 20, 1764, and Jan. 20, 1765.

THE following obſervations were taken with a reflecting teleſcope, of two feet focal length, made by Mr. Short (of a ſimilar ſize and conſtruction to thoſe uſed in the obſervation of the Tranſit of Venus, by himſelf at Saville Houſe, by Mr. Green at Greenwich, and by Meſſ. Maſon and Dixon at the Cape of Good Hope), with an equal altitude inſtrument made by Mr. Bird, and a clock, with a gridiron pendulum, made by Mr. Shelton, an account of whoſe going, at Greenwich, before my departure for St. Helena, and immediately upon my arrival there, is contained in Phil. Tranſ. Vol. LII. Part II. Page 434. and the difference of gravity between thoſe two places thence deduced.

The almoſt continual cloudineſs of the ſkies, at the Iſland of St. Helena, renders it a very inconvenient place for the making of aſtronomical obſervations, which I had the mortification to experience in loſing the ſight of the exit of the planet Venus, from the ſun's diſc, on the 6th of June 1761, to obſerve which was the primary motive of my going thither. I ſhould have thought myſelf, in a great meaſure, compenſated for this miſchance, if I had been enabled, by the help of the ten foot ſector, provided me at the expence of the Royal Society, either to prove or diſprove the exiſtence of a ſenſible annual parallax of the ſtar Sirius, ſome reaſons for the probability of which I laid before the Royal Society, in a paper ſince publiſhed in Phil. Tranſ. Vol. LI. P. II. p. 889, and, at the

the ſame time, offered a propoſal for the diſcovery of the ſame, by obſervations of the zenith diſtance of that ſtar, to be made at the iſland of St. Helena. But, unfortunately, when I came to ſet up the ſector there (which, through the tardineſs of the workman in finiſhing it, I had not had an opportunity of proving, as I had wiſhed, before my departure from England) I ſoon found a ſtrange irregularity in the obſerved zenith diſtances of the ſtars, amounting to 10, 20, and ſometimes even 30 ſeconds. After having ſatisfied myſelf, by various trials, that theſe great differences in the obſervations did not ariſe from any bending of the tube of the teleſcope, which conſtitutes the radius of the inſtrument, or from any looſeneſs in the object-glaſs, or inſtability of the wooden three-legged ſtand, which ſupports the ſector, I, at laſt, found the cauſe of error to lie, where I had leaſt ſuſpected it, in the imperfection of the ſuſpenſion of the plumb-line (which is a fine ſilver wire) from the neck of the central pin; for, upon taking the loop of the plumb-line off the pin, and putting on again, after turning it half round, or putting on a new one, I found the plumb-line would apply itſelf to a different part of the limb of the ſector, commonly by 10, and frequently by 20 ſeconds. This experiment, with the ſame event, I had the honour of exhibiting before a committee of the Royal Society, for their ſatisfaction, as to the cauſe of the failure of my intended obſervations, September 11, 1762, at the Britiſh Muſeum.

The irregularities in queſtion evidently aroſe from the friction of the loop of the plumb-line againſt the neck of the central pin; a fault, to which moſt of the

the ſectors, made before mine, have probably been liable. Indeed the fault became more glaring here, by the workman's having made the diameter of the neck of the central pin ſo large as $\frac{1}{20}$th of an inch; but that the errors cannot be entirely removed by leſſening the neck of the pin, I can aſſert from my own experience, having cauſed a pin to be made with the neck only $\frac{1}{70}$th of an inch in diameter (and beyond that it cannot well be reduced) by which I ſtill found an irregularity in the ſuſpenſion of the plumb-line, to the amount of 3″, a quantity, though ſeemingly ſmall, yet of great conſequence in the nice obſervations to which this inſtrument is generally applied, and which it is capable of taking to a prodigious exactneſs, when the ſuſpenſion of the plumb-line is accurately provided for. Mr. Bird has contrived one of ſix foot length, for ſettling the limits between Pennſilvania and Maryland, in which the plumb-line is adjuſted ſo as to paſs over againſt, and biſſect a ſmall point at the centre of the inſtrument.

I cannot, on this occaſion, omit remarking that the late learned Abbé de la Caille's ſector, with which he made his principal obſervations, from ſome of which I inferred the probability of an annual parallax of Sirius, ſeems to have had a like fault with my ſector, as may be inferred not only from the differences in the obſervations themſelves, but alſo from the brief account of the ſuſpenſion of that inſtrument, contained in a letter with which I have been favoured by M. Delalande from Paris, an extract of which I preſented to the Royal Society. Vide Phil. Tranſ. Vol. LII. Part 2. Page 607.

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Let me obſerve alſo, that the 9 foot ſector, made in London by Mr. Graham, with which the gentlemen of the Royal Academy of Sciences at Paris meaſured a degree of the meridian, at the polar circle, and afterwards a degree between Paris and Amiens, had, at the time of their making thoſe obſervations with it, the ſuſpenſion of the plumb-line contrived after the ſame manner as mine (whatever alterations may have ſince been made in it) as appears from M. De Maupertuis's minute and accurate deſcription of the ſaid inſtrument, in his account of the meaſure of the degree of the meridian between Paris and Amiens: for he there ſays, that the part of the central pin, on which the loop of the plumb-line was hung, reſembled the meeting of two oppoſite cones at their points; which is an exact deſcription of the form of the neck of the central pin of my ſector. But, though this capital defect in the ſuſpenſion of the plumb-line of my inſtrument (which I could not correct, at St. Helena, for want of workmen and tools) prevented me from deciding the queſtion concerning the annual parallax of Sirius; yet, as I am conſcious, the want of ſucceſs did not ariſe from any fault of mine, I ſhall endeavour to conſole myſelf for my diſappointment, by the reflexion, that I may, at leaſt, have contributed ſomething to the benefit of aſtronomy, by having diſcovered, by my experiments, the imperfection of the above-mentioned method of ſuſpenſion of the plumb-line in ſectors, which no one ever ſuſpected before, and ſo may be the means of preventing any more inſtruments of this kind being conſtructed in the like faulty

faulty manner, and consequently any future astronomers being deceived in their observations.

There still remained one object worthy of attention, which I had also proposed to the Royal Society, and received their encouragement to proceed in it. This was the observation of the horary parallaxes of the moon, by the difference of right ascension in time between the moon's enlightened limb, and stars near her parallel of declination: a kind of observation never before made to my knowledge, by any astronomer, in a latitude so near to the equator, as St. Helena; which, by determining the mean horizontal parallax in that latitude, infers also, by a proportion, which will come out sensibly the same upon any probable hypothesis of the figure of the earth, the mean equatorial parallax, which hath never yet been deduced in any manner so nearly direct.

For the purpose of making these observations, I was provided with a polar axis, suitable to the latitude of the place, on which my reflecting telescope was mounted, and a particular additional eye-piece, having fine silver wires stretched in the focus of the nearest eye-glass. The cell containing the wires being moveable round about, by means of a screw, it was easy to cause any star near the moon's parallel of declination to run exactly along one of the wires, which may be called the directing wire, from the centre to the extremity of the field of the telescope. The exact instants of the stars passing three wires placed perpendicular to the former, which may be called the horary wires, representing small portions of horary circles, were noted by the clock

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to the exactneſs of $\frac{1}{4}$th of a ſecond of time; as were alſo the inſtants of the moon's enlightened limb paſſing the ſame wires. It is manifeſt that the difference of time, obſerved by the clock, between the ſtar and the moon's limb paſſing the horary wires, reduced to ſidereal time, and from thence into parts of the equator, is the apparent difference of right aſcenſion between the ſtar and the moon's limb paſſing the horary wires. The ſame obſervations repeated after an interval of ſome hours gave the preſent difference of right aſcenſion between the ſtar and the moon's limb; whence the moon's apparent motion in right aſcenſion, or the difference of theſe differences is known; which ſubſtracted from the moon's motion in right aſcenſion, in the given interval of time, owing to her proper motion in her orbit, computed, in the moſt exact manner, from the beſt tables, leaves the remainder for the change of the moon's parallax in right aſcenſion between the two times of obſervation; the ratio of which to the horizontal parallax at that time being alſo computed, the horizontal parallax of the moon is known: and conſequently, by the help of a proportion borrowed from the tables, the mean horizontal parallax of the moon in the latitude where the obſervations are made. The mean horizontal parallax being deduced in this manner from a great many obſervations on different nights, the mean of all the reſults may be taken, as approaching very near to the truth: for the advantage is ſo great from taking a mean of a great number of aſtronomical obſervations, that any degree of exactneſs required, may be thereby obtained, provided they are not liable to any conſtant and uniform cauſe of error: as has been clearly ſhewn by my late worthy and learned friend Mr. Thomas Simpſon, Phil. Tranſ. Vol. XLIX. Part I. Page 82; and alſo in his Miſcellaneous Tract, Page 64. Therefore I cannot but think, that, from a conſiderable number of ſuch obſervations, the mean horizontal parallax, and thence the mean equatorial parallax of the moon might be deduced certainly to a ſingle ſecond, or ever nearer if required.

In hopes of attaining ſuch a degree of exactneſs, I endeavoured to multiply my obſervations as much as poſſible: yet, through the great cloudineſs of the iſland, could not obtain more than three nights complete obſervations. If theſe ſhould appear too few to attain the exactneſs propoſed, yet they may contribute, in a good meaſure, thereto. However, I have ſince had an opportunity, during my reſidence at the iſland of Barbadoes, in the latitude of 13° north, which 3° degrees nearer the line than St. Helena, to repeat theſe obſervations to a very great number, from which, I doubt not, the mean equatorial parallax of the moon may be accurately determined.

I ſhall here deſire to remark, that, if the like obſervations were repeated in different latitudes, they would probably afford the beſt means yet propoſed for aſcertaining the true figure of the earth; as they would determine the ratio of the diameters of the parallels of latitude to each other, the horary parallaxes being proportional thereto: and, after all the experiments and obſervations that have been made

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on this ſubject, we ſhall probably, at laſt, be indebted to obſervations of the moon's parallax for the beſt determination of it: for though the earth affords but a ſmall baſe at the moon, yet, by repeating theſe trials, and comparing their reſults, we may hope to attain that degree of exactneſs, which we could never expect from fewer obſervations.

As I look upon the ſpecies of obſervations here ſpoken of to be of very important uſe for the improvement of aſtronomy and geography, and as ſuch deſire to recommend the practice of them, eſpecially to thoſe who may have occaſion to viſit countries of diſtant latitudes; I ſhall briefly mention ſuch further particulars, which the experience I have had, and my attention have ſuggeſted to me, the obſervance of which may conduce to the greater accuracy, as well of the obſervations, as of the conſequences to be deduced from them.

I apprehend the uſe of a polar axis to be very neceſſary for rightly managing the teleſcope, as well for finding what ſtar it is proper to compare the moon with, as for preſerving the poſition of the wires unvaried, after their adjuſtment. A very nice and exact polar axis is not requiſite; but a cheap one, and ſuch an one as may eaſily be provided, will ſuffice. Mine was formed by a braſs ſocket, making an angle with the horizontal top of the ſtand equal to 16° or the latitude of the place, receiving the braſs cylindrical ſupport of the teleſcope, inſtead of the perpendicular ſocket of the common ſtand: and the teleſcope was firmly confined in the ſocket by a pointed ſcrew which paſſed through one ſide of the

the ſocket into ſome of the holes, wich were drilled in the ſupport of the teleſcope.

The polar axis may be ſet near enough to the direction of the meridian, by a magnetic needle, allowing for the variation; or, even by the ſight, provided the walls of the obſervatory be built nearly north and ſouth. This being done, and the directing wire being brought into ſuch a poſition, that the ſtar may run exactly along it from the centre to the extremity of the field of the teleſcope; then if the ſcrews of the rack work be turned, and the ſtar be brought back to the interſection of the wires, it will be found to run exactly along the directing wire again; and this I generally found would be the caſe, even for a very conſiderable ſpace of time, though the ſtar had, in the mean time, advanced a conſiderable way from eaſt to weſt by the diurnal rotation; ſo that it is not always neceſſary to re-adjuſt the wires after each ſett of obſervations, though it may be proper to examine whether they require it or not. Hence it follows, that there can be no danger of diſturbing the poſition of the wires after their adjuſtment, by bringing the ſtar back to the entrance of the teleſcope, in order to obſerve its paſſage acroſs all the horary wires.

Sometimes it ſo happens that a proper ſtar cannot be found that precedes the moon, to compare her with; in ſuch a caſe, the obſerver muſt compare her with a ſtar following her, and adjuſt the wires by making ſome bright point of the moon run along the directing wire, which is a more exact method than by making the directing wire a tangent to the moon's north

north or ſouth limb. Here, indeed, the directing wire cannot repreſent a parallel to the equator, on account of the moon's continual change of declination, but will make a ſmall angle therewith; which may be computed, and the obſervations corrected accordingly. But the correction may be eaſier made, as follows, let *a* expreſs the moon's apparent angular motion about the pole of the world in four minutes of time, being the difference of her proper motion in right aſcenſion, and the change of her parallax in right aſcenſion: *d* her apparent motion and declination in the ſame time, *h* the difference of the apparent declination of the moon, and that of the ſtar, *r* the radius, and *c* the coſine of the moon or ſtar's declination; the correction to be applied to the moon's right aſcenſion, or the difference of right aſcenſion of the moon and ſtar is $h \times \frac{d}{a} \times \frac{r^2}{c^2}$. If the moon is approaching the ſtar's parallel of declination, ſhe will come to the horary wire relatively too late for the ſtar, and her right aſcenſion, deduced immediately from that of the ſtar, will be too great, and muſt be diminiſhed by the correction here mentioned; but, if the moon is receding from the ſtar's parallel of declination, ſhe will come to the horary wire relatively too ſoon for the ſtar, and her right aſcenſion, immediately deduced from that of the ſtar, will be too ſmall, and muſt be increaſed by the above-mentioned correction.

There is another attention, which the nice obſerver will not think too trifling for his notice, namely, to examine whether the wires of his teleſcope are placed at exact right angles to each other (which they seldom are) and, if they are not, what the quantity of deviation is, in order to make an allowance for it in the reduction of the observations. This may be tried several ways. I examined the wires of my telescope at Barbados, by a great many observations of the difference of right ascension of stars, which differed considerably from each other in declination, namely Arcturus, and the little star accompanying it, and the virgin's spike, and a small star preceding it; first with the wires in the common position, and next when turned a quarter round, making the middle horary wire serve as a directing wire; for, if the wires do not cut each other at right angles, the difference of right ascension of the stars will come out too much one way, and as much too little the other way, and half the difference will be the correction in this case, whence it may be inferred in all other cases. Or, the angle of the deviation of the wires from a right angle being hence found, the correction of the difference of right ascension of the moon and star, is to the difference of their apparent declination; as the sine of the angle of the deviation of the wires is, to the cosine of the moon or star's declination.

I have determined the deviation of the wires of the telescope, which I used at St. Helena, by comparing them with a right angle, formed by two silver wires on a brass plate, fixed up in a window at the distance of 30 feet from the telescope. The extent of the compasses, with which the intersecting arches were struck, for finding the perpendicular lines

lines on the plate, being no leſs than ſeven inches, thoſe wires may be ſuppoſed to differ inſenſibly from a right angle to each other. The teleſcope being adjuſted for ſeeing them diſtinctly, I brought that wire of the teleſcope, which in celeſtial obſervations repreſented a parallel of declination, to be exactly parallel to one of the wires on the plate, with the ſmalleſt interval poſſible; and, at the ſame time, made the middle perpendicular, or horary wire, to paſs through the interſection of both the wires in the window: when I plainly diſcerned, that the wires of the teleſcope were not exactly perpendicular to each other, the ſuperior angle to the right being manifeſtly acute, and the ſuperior one to the left obtuſe. This I further verified by applying the acute angle to the left hand ſuperior angle of the plate, turning the wires in the teleſcope a quarter round, from right to left, by the ſcrew adapted for this purpoſe, when the ſame difference appeared as before. This proved alſo that the wires on the plate made exact right angles with each other; otherwiſe the acute angle of the wires of the teleſcope could not have appeared to differ equally from both of them. To find the exact difference of the angle made by the wires from a right angle, I had a third wire placed exactly parallel to one of the former on the plate at the diſtance of $\frac{1}{87}$th of an inch; when by applying the angle of the wires of the teleſcope to the right angle on the plate, the deviation of the former from the latter appeared to be equal to half the interval of the parallel wires at the extremity of the field of view; but the ſemi-diameter of the field of the teleſcope at the diſtance of the wires in the window being

being meaſured $\frac{925}{1000}$th of an inch; whence the angle of deviation of the wires, from a right angle, is 21 minutes. But, by a mean of 11 trials, the quantity of the deviation came out 28′ $\frac{1}{2}$, the extreme reſults being 21′ and 36′. This is the deviation of the ſouth part of the middle horary wire, from a perpendicularity, to the directing wire towards the eaſt, in the obſervations at St. Helena; a ſtar, that paſſed ſouth of the centre of the teleſcope, coming to the horary wire too ſoon, and a ſtar that paſſed north of the centre coming later to the horary wire than it ought to do.

In order to determine whether the two other horary wires were parallel to the middle one, or, if not, what angle they made with it, I compared the tranſit of 13 ſtars acroſs the three horary wires, with thoſe of as many more ſtars differing conſiderably in declination from the former, all obſerved at St. Helena; and from the differences of right aſcenſion at the ſeveral wires, after making an allowance for the convergence of the meridians, which however is not 2′, I found the ſouth part of the firſt or eaſtern wire to deviate from a paralleliſm with the middle one towards the weſt by 9′,8, and the weſtern wire to deviate towards the eaſt by 5′,4; hence it appears that the ſouth parts of the eaſtern, middle, and weſtern horary wire differed from a perpendicularity to the directing wire towards the eaſt, in the obſervation of St. Helena, by 18′,7, 28′ $\frac{1}{2}$, and 34′, the mean deviation of all the three being 27′ or only 1 $\frac{1}{2}$ different from that of the middle horary wire. This quantity of the mean deviation of the wires is alſo con-

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firmed by a comparifon of fix differences of right afcenfion of ftars obferved at St. Helena, with the fame obferved, at my defire, fince my return, at the Royal Obfervatory, by the tranfit inftrument, which gives 27′ $\frac{1}{4}$, agreeing exactly with what has been here found in a more certain manner.

Sometimes, in making thefe obfervations, it fo happened, that feveral ftars lay near the moon's parallel of declination; when I obferved all of them, that came within the field of the telefcope; as well to obviate the hazard of miffing to obferve the right ftar again after an interval of feveral hours, as to obtain a greater number of comparifons of the moon's motions in right afcenfion, and fo reduce the unavoidable errors of the obfervations as much as poffible.

It may be proper to remark, that the moft convenient time for making thefe obfervations, is when the moon is ftationary at her greateft declinations; when fhe may be compared with the fame ftar, with a telefcope having a moderate field of view, for feveral hours. The change of the moon's parallax in declination is then alone to be feared; but if the obfervations are made nearly at the fame diftance from the meridian, both on the weftern and eaftern fide, the parallax returning to the fame quantity, will occafion no difficulty. Sometimes, when the moon is not exactly at, but only near, her greateft declination, by obferving her on the proper fide of the meridian, the effect of parallax may be found to be contrary to, and confequently counteract, her change of declination arifing from her proper motion.

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Through the great cloudineſs of the ſkies at St. Helena, I could obſerve the moon only one night, namely January 8, 1762, near her limits of declination; on the other nights, I endeavoured to compare her with as many ſtars as came within the field of the teleſcope, truſting to determine afterwards the difference of right aſcenſion of the ſtars, with which ſhe was compared in the former and latter obſervations of the ſame night. This deſign I have ſince completed, by procuring a great many tranſits of theſe ſtars, to be obſerved on the meridian, at the Royal Obſervatory, in the latter end of the year 1762, and beginning of the year 1763; many alſo I made there myſelf. An account of them is given at the end of the other obſervations.

An uſeful remark here offers itſelf to our notice; that the moon's parallax may be very well determined, in a fixed obſervatory, at any period of her declination, by obſerving the difference of right aſcenſion of her limb, and any ſtar near her parallel, at a conſiderable diſtance from the meridian, either to the eaſt or weſt, with the parallactic teleſcope, and alſo on the meridian with the tranſit inſtrument.

But, in purſuing this method, the parallactic teleſcope ought to be nearly of equal goodneſs with the tranſit teleſcope; elſe the moon's diameter might appear greater by ſome ſeconds through one and the other, and conſequently the parallax ſo deduced would not be exact. It is true, that, by a proper method of comparing the obſervations, this ſmall error might be obviated, though the teleſcopes differed ever ſo much in the degree of diſtinctneſs, namely, by taking a mean of the reſults found by the obſervations in the firſt and laſt half of the moon, or on the eaſtern and weſtern ſides of the meridian; for it is manifeſt the errors would be of contrary tendency in theſe different caſes.

As it may ſerve the more to recommend the practice of theſe obſervations to aſtronomers, I think it proper to mention, that I ſeldom failed of finding ſome proper ſtar or ſtars near the moon, of ſufficient brightneſs, to compare her with, even when there were none ſuch marked down in any catalogue, or any charts: the number of zodiacal ſtars proper for comparing the moon and planets with in a teleſcope, and not inſerted in any printed catalogue, ſeeming much to exceed the number of thoſe marked down.

I have but one more remark to add on this ſubject, that, as it is neceſſary to know nearly the apparent difference of declination of the moon's centre, and the ſtars obſerved, in order to correct the obſervations for the deviation of the wires, and the moon's change of declination, ſo this may moſt readily and conveniently be done by ſome oblique wires fixed in the focus of the eye-glaſs of the teleſcope, as I had in that I uſed at Barbadoes. Not having ſuch a contrivance adapted to the teleſcope I uſed at St. Helena, I always eſtimated by my eye how many minutes each ſtar, and alſo the moon's centre, paſſed north or ſouth of the directing wire, which I had an eaſy method of doing, by comparing their diſtance from the directing wire, with the interval between the directing wire, and one of the two wires ſtretched parallel to it, at the exact diſtance of 10 minutes on each ſide. In this manner the numbers

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Fig. 1.

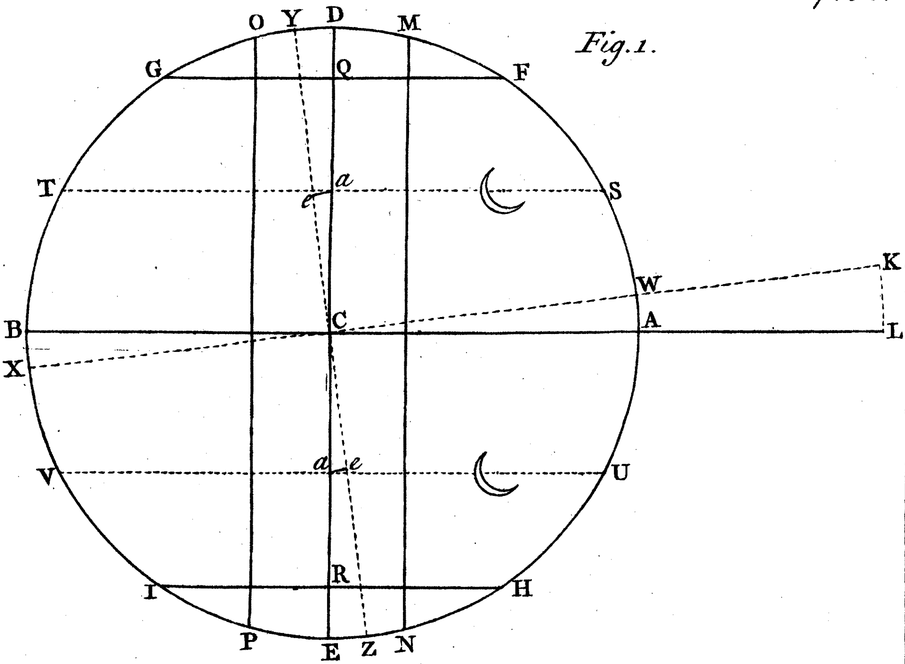
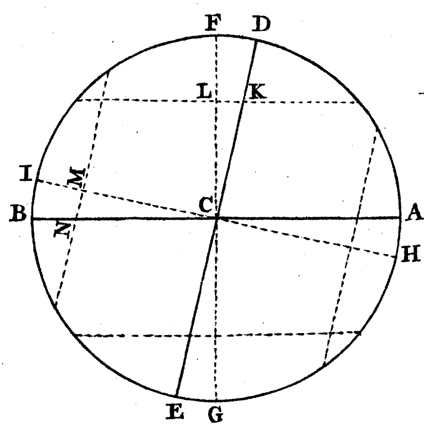


Fig. 2.



J. Mynde sc.

ſet down by the ſides of moſt of the obſervations were deduced, expreſſing the number of minutes which the ſtars paſſed north or ſouth of the moon's centre. They may be depended on to a minute, or two at moſt, which is ſufficient for the reduction of the obſervations.

To make the foregoing account of the method of obſerving the moon's horary parallaxes more clear, let ADBE, Tab. XXI. fig. 1. repreſent the cell containing the ſilver wires ſtretched in the focus of the neareſt eye-glaſs of the teleſcope, of which A B is the directing wire, M N, D E, O P, three wires perpendicular to A B repreſenting portions of horary circles; and, F G, H I, two other wires parallel to the directing wire A B, at the equal diſtances, C Q, C R, on each ſide, equal to 10 minutes. The ſemi-diameter of the field of the teleſcope C A is 14 minutes; A is the eaſtern ſide of the field of view, B the weſtern, D the ſouthern, and E the northern ſide. In order to adjuſt the wire A B parallel to the equator, ſo that M N, D E, O P, may repreſent horary circles, the ſtar, whoſe difference of right aſcenſion from the moon is to be obſerved, is brought by means of the ſcrews to the interſection of the wires A B, D E, at the centre of the teleſcope C, and when the ſtar is near paſſing out of the teleſcope at B, the cell A D B E is turned round, by means of a ſcrew, till the ſtar is again brought upon, and biſſected by the wire A B; this being done, if the ſcrew be turned to make the teleſcope follow the ſtar towards the weſt, and the ſtar be again brought to the interſection of the wires at C, it will be generally found to run exactly along the wire C B, biſſected by it all the way from C to B; which is then a proof that the wire A B is rightly adjuſted; but, if the ſtar

runs

runs not exactly along the wire A B, the poſition of that wire muſt be altered a little, till the ſtar runs exactly along it from the centre C, to the extremity of the field of the teleſcope at B. Then turn the ſcrew to carry back the ſtar from B a little to the eaſt of the firſt or eaſtern wire M N, and obſerve the exact minute, ſecond, and quarter of a ſecond, of the ſtars paſſing the three horary wires M N, D E, O P; the teleſcope remaining unmoved and undiſturbed, obſerve, in like manner, the tranſit of the moon's enlightened limb acroſs the ſame wires, whether ſhe paſs ſouth of the ſtar, as along the line S T, or north of the ſame, as along to the line U V; and the obſervation is completed. The like obſervation being repeated, after an interval of ſeveral hours, we ſhall have the apparent motion of the moon in right aſcenſion in this time; whence the moon's horary, and thence her horizontal parallax may be computed.

If the moon precedes the ſtar, and the wire A B is adjuſted by making ſome bright point in the moon run along it, and W X is ſuppoſed to be the true parallel of declination, it is manifeſt that the ſtar will paſs the horary wire at *a*, to the ſouth of the centre of the teleſcope ſooner, and to the north of the centre of the teleſcope later, than it paſſes the true horary circle Y Z at *e*, by the time it takes to deſcribe *a e* parallel to W X. Let C L be the apparent motion of the bright point of the moon, in four minutes of time; draw K L perpendicular to W X produced, and C K is the apparent motion reduced to a parallel of the equator in four minutes, and K L the apparent motion in declination in the ſame time; and, the right-angled triangles *a e* C, K C L being ſimilar, *a e* is to *e* C, as K L to C K; but the error of the right aſcenſion anſwering to *a e* is to *a e*, as radius to the coſine of the ſtar's declination; and C K, is to the moon's apparent angular motion about the pole, in four minutes, as coſine of moon's apparent declination, to radius. Whence, by compoſition of ratio's, and by equality, the correction of the moon's right aſcenſion is to *e* C the apparent difference of declination of the moon and ſtar, in a compound ratio of the moon's apparent motion, in declination in four minutes, to her apparent motion about the pole in the ſame time, and of the ſquare of the radius, to the product of the coſines of the ſtars and moon's apparent declinations.

Further it appears from the ſcheme, that the moon comes later to C than to the horary circle paſſing through the point of the wire (*a*) cut by a ſtar between C and D, whoſe parallel of declination ſhe is approaching, and that ſhe comes ſooner to C than to the horary circle paſſing through the point of the wire (*a*) cut by a ſtar between C and E, whoſe parallel of declination ſhe is receding from, by the time the ſtar takes to deſcribe *a e*; and, therefore, the right aſcenſion of the moon deduced immediately from that of the ſtar muſt be too great in the firſt caſe, and too little in the ſecond caſe, by the ſpace *a e* meaſured upon the ſtar's parallel of declination.

Laſtly, to explain the manner of examining the deviation of the wires, from a perpendicular to each other, by obſervations of the ſtars; let A B, fig. 2. repreſent the directing, and E D the middle horary wire, deviating from C F, ſuppoſed perpendicular to A B, by the ſmall angle D C F. Let any ſtar be made to run along the wire A B, from A to B, any other ſtar following it will paſs the wire D E, at K ſooner, than the horary circle F G at L, by the time of its deſcribing the ſmall ſpace L K, and conſequently the difference of right aſcenſion will appear too little; now let the wires be turned a quarter round, that the wires A B, D E, may change places, D coming into the place of A, and E into that of B, which is done by making the firſt ſtar run along the wire D E, from C to E. Now the wire A B, deviating from C I perpendicular to D E, by the angle B C I, the ſecond ſtar will paſs the wire B C at N, later than the horary circle I C at M, by the time it takes to deſcribe the ſpace M N = L K; and conſequently the difference of right aſcenſion of the two ſtars will appear as much too great, as it before appeared too little, when the wires were adjuſted in their uſual poſition: and half the difference will be the correction in this caſe, to be added to the firſt, or ſubtracted from the ſecond difference of right aſcenſion; whence the correction may be eaſily inferred for all other obſervations.

Celestial Observations.

Eclipse of the Moon, May 18, 1761.

Appt Time. H	′	″	
7	48	38	Penumbra plainly entered upon the Moon's disc.
7	56	23	Beginning of the Eclipse.
7	19	16	Shadow begins to touch Kepler.
7	37	8	Shadow bissects Manilius.
7	40	8	Shadow begins to touch Julius Cæsar.
10	39	23	Emersion out of total darkness.
11	46	52	End of the Eclipse.

Immersions and Emersions of Jupiter's Satellites, observed at the Observatory on the Alarum Hill.

1761 Day of the Month.	Appt Time H	′	″	
☽ July 20	17	44	31	Imm. 1 Sat. The Moon near Jupiter.
☿ July 22	12	12	29	Imm. 1 Sat.
☿ Aug. 5	14	49	50	3 Sat. almost immerg'd. Then clouds.
	14	50	30	3 Sat. certainly immerg'd.
	16	0	40	Imm. 1 Sat.
♃ Aug. 27	11	13	56	Emers. 4 Sat.

Immerſions and Emerſions of Jupiter's Satellites, obſerved in James's Valley.

1761. Day of the Month.	Apparent Time. H ′ ″	
♄ Oct. 10	12 48 38	Em. 2 Sat.
♃ Oct. 15	13 36 10 :	Em. 1 Sat.
♀ Oct. 16	10 3 40	Em. 3 Sat.
	10 3 45	Ditto by Mr. Maſon (who arrived here to-day from the Cape of Good Hope) with a two foot reflecting teleſcope.
☽ Nov. 9	8 19 54	Em. 1 Sat. Inſtantaneous both to myſelf and Mr. Maſon, exactly at the ſame ſecond, in different houſes. Air very clear, and Satellite increaſed its light very faſt.
☿ Nov. 11	12 40 19	Em. 2 Sat. by Mr. Maſon. I miſſed the inſtant of Emerſion by moving the ſtand of my teleſcope. Air very clear.
☽ Nov. 23	12 9 52	Em. 1 Sat.
♄ Nov. 28	7 39 18	Imm. 3 Sat. Air a little hazy.
	7 39 13	Ditto by Mr. Maſon.
	10 8 17	3 Sat. had not em. Then clouds.
	10 10 13	Ditto plainly out, though not near arriv'd to its full luſtre.
☉ Nov. 29	7 9 48	2 Sat. had emerg'd. Its light was ſo weak, it probably had not been out above 15 ſeconds.
☉ Dec. 6	9 43 36	Em. 2 Sat.
	9 43 33	Ditto by Mr. Maſon.
1762.		
☉ Jan. 10	7 37 42	Imm. 3 Sat.

N. B. This mark (:) affixed to any obſervation ſignifies that it is a little uncertain.

Obſervations

Obſervations of the difference of right aſcenſion, by time, between the moon's enlightened limb and ſtars, taken by the help of the parallactic wires adapted to the reflecting teleſcope; deſigned for determining the horary parallaxes of the moon. They may alſo ſerve to deduce the longitude of the place.

♃ Sept. 3. Compared the moon's weſtern and preceding limb, with α Libræ in right aſcenſion, at the Obſervatory on the Alarum Hill. The times are by the clock, which is 12 ſeconds too ſlow for ſidereal time, and keep the rate of going of ſidereal time, exactly. The ſtar paſſed ſouth of the moon's centre.

		1ſt hor. w.		2d hor. w.		3d hor. w.		☽'s Limb at middle wire. Apparent Time.		
	H	′	″	′	″	′	″	H	′	″
α Libræ	17	42.	9	42	23	42	37			
☽'s W. Limb	17	43	45	43	59½	44	13½	6	52	49
α Libræ	18	2	10	2	24	2	38			
☽'s W. Limb	18	4	14	4	28½	4	42½	7	13	15
α Libræ	18	16	3	16	17	16	31			
☽'s W. Limb	18	18	27½	18	42	18	56	7	27	26
α Libræ	18	23	6	23	20	23	34			
☽'s W. Limb	18	25	41−	25	55	26	9+	7	34	38

Thurſday September, 24 I removed the clock down to James's Valley, and keeping the ſame length of the pendulum as before, fixed it up ſtrongly againſt the wall of a houſe, in an upper room, whence I could make my obſervations through openings made in the roof of the houſe. I fixed the equal altitude inſtrument, for regulating the clock, againſt a ſtrong poſt, let deep into the ground, in a little room eight foot ſquare, built for this purpoſe, in a convenient open place, at a little diſtance from the

the houſe where the clock was. When I obſerved the ſun's equal altitudes, I firſt adjuſted the inſtrument; then I went to the room where the clock was, and ſet my watch, having a ſecond hand, exactly with it; then I returned to the equal altitude inſtrument, and obſerved the paſſage of the ſun's limb acroſs the horizontal wires of the inſtrument, according to the time ſhewn by the watch; and, immediately after the obſervation, went again to the clock, and compared the watch with it, noting how much it had got or loſt, whence the obſervations were eaſily reduced to the time of the clock.

			1 Wire		2 Wire		3 Wire		Apparent Time		
		H	′	″	′	″	′	″	H	′	″
♃ Oct. 8	☽'s W. L.	11	34	2	34	17	34	31½	10	45	14
	ε Capricorni	11	Clouds		36	30½	Clouds		Star 10′ N. of M.		
♄ Oct. 10	☽'s W. L.	14	19	57	20	12–	20	25	13	23	7
	ψ Aquarii	14	24	57	25	11	25	24½			

October 8th and 10th the clock got 7″,3 upon 24 hours in one revolution of the fixed ſtars.

The foregoing obſervations were all made with the teleſcope placed upon the common ſtand, without the polar axis. The obſervations of December 4th, that follow, were made with the teleſcope fixed upon a new and heavier ſtand, which was rendered more ſteady, by two broad feet reſting upon ſeveral of the boards at once. The ſocket for receiving the teleſcope was cut obliquely in the ſtand, ſo that it had partly the effect of a polar axis,

October 28th, I took down the clock, packed it up, and ſent it on board a veſſel going to the Cape of Good Hope, to return again ſoon, committing it to the care of Mr. Jeremiah Dixon, who had obſerved the tranſit of Venus over the ſun at the Cape. He took his paſſage on board the ſaid veſſel, in order to ſet the clock up at the Cape of Good Hope, and examine the difference of its going between that place and St. Helena, for determining the proportion of the force of gravity at thoſe two places.

The ſame day Mr. Maſon fixed his clock up, for my uſe, againſt a large maſſy poſt, let deep into the ground, near the equal altitude inſtrument, at the little Obſervatory. This clock was made by Mr. John Ellicott, F. R. S.

I ſtill

I ſtill continued, for ſome time, to make my obſervations in the upper room, as before. For this purpoſe I fixed up a little clock there, which may be called a journeyman, or ſecondary clock, having a pendulum ſwinging ſeconds, which after being well adjuſted, would keep time very regularly for ſeveral hours. It had only a minute and ſecond hands, and ſtruck every minute exactly as the ſecond hand came to ſixty, which, was very convenient for the counting of ſeconds; more eſpecially in the obſervations made with the parallactic teleſcope, it being improper, on account of the inſtability of the floor, to get up from one's ſeat, or to alter the poſition of the body conſiderably even to catch the ſecond, till thoſe obſervations were completed. I reduced the times to that of the obſervatory clock, by means of my watch, with the ſecond hand. The little clock, as well as the larger clock, which I ſent with Mr. Dixon to the Cape of Good Hope, was made by Mr. John Shelton.

♀ December 4^{th}, I compared the moon's weſtern limb with the three ψ of Aquarius, with reſpect to right aſcenſion, and obſerved her occult to the ſouthern one. The time is ſet down according to the little clock, and the difference between that and the obſervatory clock is ſet down by the ſide; the latter loſt 59 ſeconds upon 24 hours, in one revolution of the fixed ſtars, and the little clock kept very nearly the rate of ſidereal time. In theſe obſervations the directing wire was adjuſted by the ſtars.

Wire

	1 Wire	2 Wire	3 Wire	Lit. clock slow. than obs. clock	☽'s L. at mid. Wire by observatory clock.	Appt Time
	H ′ ″	′ ″	′ ″	′ ″	H ′ ″	H ′ ″
☽'s W. Limb 3d ψ Aquarii	23 21 0 23 23 32	21 13½ 23 46–	21 27 23 59—	8. 4	23 29 17½	7 19 5
☽'s W. Limb 3d ψ Aquar.	23 38 31 23 40 46	38 45 40 59½	38 59 41 13	8. 4	23 46 49	7 36 34
☽'s W. Limb 3d ψ Aquar.	0 47 17 0 48 18	47 31– 48 32	47 44 48 45½	8. 0	0 55 31	8 45 6
☽'s W. Limb 3d ψ Aquar.	0 58 0 0 58 50	58 15– 59 3½	58 28 59 17	8. 0	1. 6. 15	8 55 48
2d ψ Aquar. ☽'s W. Limb	1 4 10 1 4 30	4 24 Clouds	4 37 4 58+	8. 0	1 12 44	9 2 17
☽'s W. Limb 3d ψ Aquar.	1 17 33 Clouds	17 47 18 13½	18 1 Clouds	7 59	1 25 46	9 15 17
2d ψ Aquar. ☽'s W. Limb	1 25 7½ 1 25 54	25 21½ 26 8	25 35 26 22	7 59	1 34 7	9 23 37
1st ψ Aquar. 2d ψ Aquar. ☽'s W. Limb	1 31 27 1 33 33 1 34 30	31 40 33 47 34 44	31 54 34 0 34 58	7 58	1 42 42	9 32 10
1st ψ Aquar. 2d ψ Aquar. ☽'s W. Limb	1 45 31½ 1 47 37 1 48 53½	45 45½ 47 52 49 7½	45 59½ 48 5+ 49 21	7 58	1 57 5	9 46 31
1st ψ Aquar. ☽'s W. Limb	1 58 57 2 2 37½	59 11 2 52	59 24½ 3 5½	7 57	2 10 49	10 0 13
1st ψ Aquar. ☽'s W. Limb	2 9 16 2 13 11	9 30 13 25	9 43 13 39	7 57	2 21 22	11 10 45
1st ψ Aquar. 2d ψ Aquar. ☽'s W. Limb	2 20 6+ Clouds 2 24 17	20 20 22 26 24 31½	20 33½ Clouds 24 45	7 56	2 32 27	10 21 48

At

At 23 H. 49 M. 48 S. by little clock, or 23 H. 57 M. 51 S. by obſervatory clock, which is 7 H. 47 M. 34 S. apparent time, the 3d ♆ of Aquarius vaniſhed inſtantaneouſly, clouds coming over the moon at the ſame time. Therefore it remains a little dubious, whether this was the very inſtant of the ſtar's occultation by the moon, or whether it was obſcured by the clouds, though I rather ſuppoſe the former from the manner of its vaniſhing, and alſo becauſe when the clouds cleared away preſently the ſtar was gone.

December 4th, by equal altitudes, the ſun paſſed the meridian at 16 H. 9 M. 11,3 S. and December 5th at 16 H. 12 M. 33,4 S. by the obſervatory clock, whence the obſerved times are eaſily reduced to apparent time, as above.

Finding the above obſervations of December 4th (though they may be depended on to half a ſecond of time) to be ſtill incommoded by a ſmall trembling of the teleſcope, owing to its reſting on a floor; I determined, for the future, to make theſe obſervations, at the little obſervatory, on the ground, which I cauſed to be altered, to make it more convenient for this purpoſe. Here I conſtantly made uſe of the polar axis, which I found to afford conſiderable advantages with reſpect to the facility and exactneſs of making the obſervations.

♀ January 8th 1762. Compared the moon's weſtern limb, with ſeveral ſtars, with reſpect to right aſcenſion, at the little obſervatory. The four ſtars, with which the moon was compared, are diſtinguiſhed by letters, according to the order of their right aſcenſion.

1ſt Wire

	1st Wire	2d Wire	3d Wire	Stars North or South of ☽'s centre	☽'s Limb at Mid. Wire
					Apparent Time
	H ′ ″	′ ″	′ ″	′	H ′ ″
a	2 48 30—	48 45	49 0	13½ N.	
d	Clouds	55 53+	55 48+	4½ N.	
e	5 56 10+	56 25+	Clouds	15½ N.	
☽	2 57 3	57 18	Clouds		8 41 58
a	3 16 32	17	17 22+	14½ N.	
b	3 19 12—	19 27	19 42+	22 N.	
c	3 22 38	22 53+	Clouds	13½ N.	
☽	3 26 16	26 32	26 47½		9 11 8
a	4 22 38—	22 53—	23 8—	12 N.	
b	4 24 57	25 13—	25 27½	19 N.	
c	4 Clouds	28 39	28 54	11¼ N.	
e	4 30 18	Clouds	30 48—	12½ N.	
☽	4 33 55	34 11½	34 27		10 18 38
a	4 52 44—	52 59	53 14		
b	4 Clouds	55 19	55 34	*b* 7′ N. of *a*	
☽	5 4 54:	5 9½:	Clouds		10 49 31½
e	5 15 9	Clouds	15 39½		
☽	5 20 4½	20 20	20 35½		11 4 40
✱	5 30 52	31 7½	31 22½		
a	5 33 15	33 30	33 45	20 circ. of S. ✱	
a	5 57 39	57 54+	58 9+	6½ N.	
b	5 59 59+	0 14½		13½ N.	
c	6 3 25½	3 40½	3 55½	6½ N.	
e	6 5 20	5 35	5 50	$7\frac{7}{15}$ N.	
☽	6 11 44	12 0—	12 15		11 56 12
✱	6 18 18½	18 34	18 49		
a	6 20 43	20 58	21 13	20 cir. S. of ✱	
b	6 23 2	Clouds	23 32½	13½ S. of ditto	
c	6 26 28+	26 43½	26 59	21½ S. of ditto	
e	6 28 23	28 38+	28 53—	19½ S. of ditto	
e	7 55 56	56 11½	56 26+	10 S.	
☽	8 5 56	6 12	6 27		13 50 9

♄ January

♄ January 9. Compared ☽'s weſtern limb with three ſtars. Adjuſted the directing wire by a bright point in the moon.

	1ſt Wire	2d Wire	3d Wire	Stars N. or S. of ☽'s centre	☽'s Limb at Mid. Wire — Apparent Time
	H ′ ″	′ ″	′ ″	′	H ′ ″
☽ L.	3 59 42	59 58	0 13½		9 41 8
1 *	4	17 cir.		10 cir. S.	
2 *	4	19 42		1 N.	
3 *	4 23 10	23 26—	23 40½	11 ½ S.	

January 9. from equal altitudes, the ſun paſſed the meridian at 18 H. 17 M. 29 S. and January 10, at 18 H. 20 M. 50,6 S. per clock, which loſes 59 S. upon the rate of ſidereal time in one revolution of the fixed ſtars: therefore the ſun may be computed to have paſt the meridian, January 8, at 18 H. 14 M. 7 S.

☿	February 3				
a	4 49 31	49 45½	50 0	The minute not noted, but from following obſervations muſt have been 50 M. to middle wire.	
b	4 50	17	32		
☽	4 53 23½	53 59	53 54—		7 44 28
b	5 0 6½	0 21	0 36—		
☽	5 3 42	3 57	4 12		7 54 44

♀	February 5				
☽	5 0 0½	0 16	0 31½		7 42 52
*	5		10 13	6 N.	
☽	5 44 12½	44 28½	44 44—		8 26 56
*	5 52 40	52 55	53 10	5 N.	
☽	5 57 7—	57 23—	57 38		8 39 48
*	6 5 12+	5 27½	5 43		
☽	6 22 24	22 39	22 55		9 5 0
*	6	30 3	30 18		

	1st Wire	2d Wire	3d Wire	Stars N. or S. of ☽'s centre	☽'s Limb at Mid. Wire Apparent Time
	H ′ ″	′ ″	′ ″	′	H ′ ″
☉	February 7				
☽	6 36 5	36 20	36 35		9 10 29
	6	51 ½ cir.			
g	6 58 48		59 17½	7 N.	
i	6 59 49+	0 . 4		12 N.	
l	7 1 24			12 S.	
☽	7 24 4—	24 19	24 34		9 58 20
	7	39 cir.		13 S.	
l	7 48 2	48 17	48 32	3 ⅓ S.	
q	8 2 7	2 22	2 36+	5 ¾ S.	
☽	9 7 10	7 25	7 40		11 41 8
a	9		16 1+	10 S.	
b	9		18 20		
c			18 32		
h	9 26 3	26 17½	26 32	7 ½ S.	
k	9		27 46		
l	9	28 38—	28 52½	8 8/10 N.	
m	9	32 15			
n	9 32 18	32 32	32 46½		
o	9 36 42	36 57+	37 11+	6 S.	
p	9 38 29	38 43½	38 58+	6 N.	
q	9 42 27+	42 42	42 56½	5 ¾ N.	
☽	10 9 38½	9 53½	10 9—		12 43 26
c	10 18 51	19 6	19 20+	14 N.	
f	10	26 32		0	
k	10	28 21			
l	10	29 27		22 N.	
q	10 43 16	43 31	43 46	18 ⅔ N.	
☽	10 51 34½	51 49½	52 4+		13 25 15
a	10 57 8—	57 23		13 ¾ N.	
f	11	7 17+	7 32	9 ½ N.	

	1 Wire	2 Wire	3 Wire	Stars N. or S. of ☽'s centre	☽'s Limb at Mid. Wire
					Apparent Time
	H ′ ″	′ ″	′ ″	′	H ′ ″
☽	12 22 10	22 25+	22 40+		14 55 36
d	12 29 16	29 31—	29 45+	12 $\frac{1}{10}$ S.	
☽	12 36 17+	36 32½	36 48—		15 9 44
d	12 42 54½	43 9	43 24—	9 $\frac{1}{10}$ S.	
e	12	44 9—	44 23½	12 S.	
☽	12 53 10	53 26—	53 40+		15 26 31
d	12 59 13—	59 27	59 41½		
e	13 0 12	0 27—	0 41		
☽	13 7 27	7 42	7 57		15 40 45
d	13 12 58½	13 13	13 28		
☽	13 21 32	21 48—	22 3		15 54 48
e	13	27 48+		3 S.	
♂	February 9				
☽	3 59 42	59 58	0 13½		6 26 26
	4	17 cir.		11 S.	
	4	19 42		1 N.	
	4 23 10	23 26—	23 40½	11 S.	

The obfervations from February 3[d] were made with my own clock, with which Mr. Dixon returned from the Cape of Good Hope December 30[th], 1761, after examining the going of it there. He found it to get there 36,6 feconds upon fidereal time in one revolution of the fixed ftars, or 29,3 feconds *per* day more than it got at St. Helena with the fame length of pendulum: but I propofe to give a more particular account of thefe, and fome other experiments then made by Mr. Dixon at the Cape, fome other opportunity.

From equal altitudes, the fun paffed the meridian, January 30[th], at 20[h] 51′ 8″ ¼; February 5[th], at 21[h] 16′ 5″,6; and February 7[th], at 21[h] 24′ 17″,7. Hence the clock appears to

 have

have got at the rate of 6″,3 upon ſidereal time in 24 hours. By the ſetting of four ſtars behind the hill, obſerved with the teleſcope of the equal altitude inſtrument, January 29th, and again February 7th and 8th (after the manner deſcribed by Mr. Maſon in his account of the going of Mr. Ellicott's clock determined by him and myſelf in this manner, Phil. Tranſ. Vol. LII. Part II. Page 534.) the clock appeared to get 6″,25 upon ſidereal time in one revolution of the ſtars, which agrees exactly with the former determination by the ſun's equal altitudes. In like manner, I always found the going of the clock, determined by theſe two different methods, would come out as nearly the ſame as the equal altitudes of the ſun could be depended upon, that is to ſay, to a ſecond, even from the obſervations of two ſucceſſive days.

I muſt not paſs by this occaſion, without taking notice of ſome remarks, which Mr. Short paſſes on my method of examining the going of the clock, by obſerving ſtars ſetting behind a hill, with the teleſcope of the equal altitude inſtrument; (vide Mr. Short's account of Mr. Maſon's paper concerning the going of Mr. Ellicott's clock at St. Helena. Phil. Tranſ. Vol. LII. Part II. Page 540). Mr. Short repreſents Mr. Maſon, as ſaying in his paper, that I propoſed making uſe of the equal altitude inſtrument to determine the regularity of the motion of Mr. Ellicott's clock, by obſerving the vaniſhing of the ſtars out of the field of the teleſcope, an expreſſion not contained in Mr. Maſon's paper, who is only ſpeaking of our obſerving ſtars ſetting behind a hill, at the diſtance of a quarter of a mile, in the ſame part of the field of the equal altitude inſtrument. Had we proceeded in the method ſuppoſed in the remarks, no doubt the obſervations would have been liable to conſiderable inaccuracy: but as we uſed the teleſcope of the equal altitude inſtrument, only to aſſiſt the ſight in obſerving the ſtars ſetting behind the hill, we were liable to no other error than what might ariſe from the ſmall alterations of the inſtrument, ariſing from the changes of heat and cold, moiſture and drineſs, ſeen from the diſtance of the top of the hill, which will eaſily be allowed to be quite inſenſible. And, indeed, how otherwiſe could the obſervations, contained in Mr. Maſon's paper, agree ſo well together as they do? A circumſtance alone ſufficient to create a ſuſpicion of the objection being ill grounded. The reaſon of Mr. Maſon and myſelf always obſerving the ſtars to vaniſh behind the hill, in the ſame part of the field of the teleſcope (that

is,

is, very near its centre) was, in order to keep the object glaſs at the ſame height; though this being leſs than an inch in diameter, and conſequently ſubtending leſs than 13″ from the top of the hill, there could not have been a ſecond of time difference, whether the ſtars had been obſerved to vaniſh behind the hill, either in the upper or lower part of the field of view.

Mr. Short alſo remarks, that no inference can be formed with reſpect to the different forces of gravity, in different latitudes, from experiments made with clocks, becauſe the ſame clock, ſet up on different ſides of the ſame room, will be found to differ conſiderably from itſelf. I readily allow that, if clocks are fixed up in a ſlight manner, or againſt common wainſcots, the experiments made with them cannot be depended upon. Nevertheleſs it does not appear, but that when they are fixed in a firmer manner, they may be depended upon near enough to be of a conſiderable uſe in phyſical enquiries: which I have reaſon to think from the many experiments I have tried with the Royal Society's clock, made by Mr. John Shelton, which I propoſe to give a particular account of at ſome other oportunity.

Obſervations of the Sun's ſetting in the Sea.

At the Obſervatory at the Alarum-Houſe, which, by careful menſuration, I found to be elevated 1983 feet above the level of the ſea. Therefore the height of the eye is 1988 feet.

1761	Appᵗ Time H ′ ″	
June 14	5 39 5½	☉'s upper limb ſet in the ſea.
June 16	5. 39. 1:	☉'s upper limb ſet. A little cloudy.
July 18	5 39 34	☉'s L. L. ſet in the ſea, certain to 2 or 3 S.
	5 42 8	☉'s U. L. ſet in the ſea, certain to ½ S.
July 30	5 44 21:	☉'s U. L. ſet in the ſea. A little cloudy.

The

The like Obſervations made in James's-Valley, near the Sea-ſide.

1761	Appt Time H ′ ″		
Nov. 16	6 23 42	⊙'s L. L. ſet	Height of eye above the ſea 16 feet.
	6 25 22	⊙'s centre ſet	
	6 26 21	⊙'s U. L. ſet	
Dec. 15	6 29 24	⊙'s L. L. ſet	Height of eye above the ſea 15 feet.
	6 32 10	⊙'s U. L. ſet	
Dec. 16	6 32 11½	⊙'s U. L. ſet, thro' a ſmall apperture in a cloud. The height of eye above the ſea 15 feet.	
Dec. 17	6 29 24	⊙'s L. L. ſet	Height of eye above the ſea 15 feet.
	6 30 52½	⊙'s centre ſet	
	6 32 20½	⊙'s U. L. ſet	

☿ Auguſt 5th
Appt Time

H ′ ″	
5. 6. 7	A ſtar 6th magn. in Piſces, at centre of equal altitude inſtrument.
5. 6. 32	Jupiter's centre at the horizontal wire.
5 7 8	Jupiter's centre at the vertical wire.

The teleſcope remained fixed during theſe obſervations.

Again.

17 31 30	The ſtar at the vertical wire.
17 31 46	The ſtar at the horizontal wire.
17 32 11	Jupiter's centre at the horizontal wire.
17 32 37	Jupiter's centre at the vertical wire.

Here alſo the teleſcope remained fixed during the obſervations.

☿ Auguſt 19th. Found the little ſtar, which is 14″ N. of β Scorpii, to precede it one ſecond of time, by my parallactic wires, with my watch, which makes four beats in a ſecond of time. If any thing the difference was ſomething more than a ſecond of time: the little ſtar may therefore be ſuppoſed to precede β Scorpii 17″ in right aſcenſion. By obſervations made with the 10 foot ſector on ſeveral nights, while β Scorpii and the little ſtar were paſſing the field of the teleſcope, I found the little ſtar to be exactly 14″ N. of β Scorpii in declination. For June 24 the difference was 12″,5. July 21, 13″,6. July 22, 14″,7. July 23, 15″,3: Auguſt 14, 12″,9. Auguſt 17, 14″,8.

The bright ſtar in the foot of the Centaur, marked α in the catalogues, when viewed through a teleſcope, becomes divided into two ſtars, one of which is about the ſecond, and the other about the fourth magnitude. They were both obſerved by the Abbé de la Caille. I found their diſtance, by the divided object glaſs micrometer fitted to the reflecting teleſcope, to be 15″ or 16″. But it is, in a manner, impoſſible to meaſure the diſtance of two ſtars very accurately with this micrometer, for being ſimilar lucid objects, when they are brought very near each other, their light will be confounded together before they exactly coincide.

The larger Magellanic cloud, viewed through a teleſcope, exhibits a few ſtars which then appear ſeparated to a conſiderable diſtance from each other. Their being ſo few in number, and ſo thinly ſcattered, is the reaſon of this phænomenon appearing ſo very faint.

The leſs Magellanic cloud, viewed through a teleſcope, exhibits a very remarkable lucid nebula, with ſome tolerable bright ſtars ſurrounding it. The nebula appears nearly circular, about 3′ in diameter.

Tranſits of ſtars, with which the moon was compared, February 7, 1762, at St. Helena, taken upon the meridian, at the Royal Obſervatory, in the beginning of the year 1763, for aſcertaining the exact difference of their right aſcenſion. Vide page 363.

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire	
		′ ″	′ ″	H ′ ″	′ ″	′ ″	
January 16	*a*		52 52	8 53 31	54 21		
	c	54 41	55 20+	8 55 59+			
	d		57 17	57 56	58 35	59 14	ξ of Cancer
	e	57 38	58 16½	58 56	59 35	0 13½	
	h	2 42::	3 21::	9 4 1:			
	f					4 44	
	l		5 40+	6 20	6 59	7 38½	
	o			9 13 39:			
	q	19 5	19 44+	9 20 23½	21 3	21 14½	λ of the Lion

Note, the clock gets five ſeconds per day upon ſidereal time.

March 6	*c*		52 39	8 53 18	53 57	54 36
	h			9 1 19	1 58½	
	i				2 42	
	l				4 18	4 57
	m				7 34	8 14
	q			17 43—	18 22	19 1

Clock loſes one ſecond and an half per day.

March 7	*a*		50 10	8 50 49+	51 29	
	c	51 59	51 37+	53 17	53 56+	
	d		54 34½	55 13½	55 52½	56 31
	e		55 33½	56 14	56 52½	57 31
	h			9 1 17½		
	l		2 58	3 37	4 17	4 55½
	q		17 2	17 41	18 21	19 0

March 8	*a*			8	51 27	
	c	51 57	52 37	53 15		
	d	53 54	54 32½	55 12	55 51	56 29½
	e	54 54	55 33	56 12	56 51	57 29½
	i	0 41		9 2 0	2 39	3 18
	l				4 15+	4 53
	q	16 21	17 0	17 39½	18 19	18 57½

Date	Star	1 Wire (′ ″)	2 Wire (′ ″)	3 Wire (H ′ ″)	4 Wire (′ ″)	5 Wire (′ ″)
March 9	*a*	49 28	50 7	8 50 45½	51 25	
	c	51 55+	52 34	53 13—		
	d	53 52½	54 31	55 10	55 49	56 27
	e	54 52½	55 31	56 10	56 49	57 28
	f			9 0 41		
	h			1 14+	1 55:	
	l		2 54½	3 34	4 13½	4 52
	n			7 30		
	o			10 54:	11 34:	
	q	16 20—	16 59	17 38	18 17+	18 56
March 12	*a*			8	51 52	52 31
	c	52 23	53 2	8 53 41	54 20½	
	d		54 59	55 38	56 16½	
	e		55 59	56 39	57 17	57 55
	g			9	2 5	
	l					5 19
	q		17 26—	9 18 6	18 45	19 24
March 13	*a*		50 33—	8 51 12	51 51+	52 30½
	c	52 22	53 0.·.	53 40		
	d	54 19	54 57+	55 36½	56 15½	
	e	55 19	55 57	56 37½	57 15+	57 54
March 15	*a*			8 51 11	51 50	52 29
	c	52 21	53 0+	53 39		
	d		55 57	56 36		
	e		56 57	57 36½		
	q	16 46	17 25	9 18 4+	18 44	19 22+

Clock loses $\frac{5}{6}$ second per day.

Date	Star	1 Wire (′ ″)	2 Wire (′ ″)	3 Wire (H ′ ″)	4 Wire (′ ″)	5 Wire (′ ″)
April 6	*a*			8 50 54	51 33	
	c	52 5	52 43	53 22		
	d	54 1	54 40	55 19	55 58	
	e	55 1	55 40	56 19	56 58	
	g		59 25	9 1 7		
	i	0 48½::	1 27	2 7		
	q	16 28	17 7+	9 17 47	18 26	19 5½

Clock loses $\frac{3}{4}$ second per day.

N. B. By a mean of ſeveral tranſits of ſtars obſerved about this time, as well as by the foregoing obſervations, it appears that the intervals of the four firſt wires are exactly equal, and that the interval of the two laſt wires is ſmaller than the others by [illegible]th part, which anſwers to $\frac{4}{10}$th of a ſecond of time in the tranſits of the above ſtars.

By feveral tranfits over the meridian, obferved at the Royal Obfervatory, at the latter end of the year 1762 and beginning of 1763, the firft or the preceding ftar of the three ψ's of Aquarius (with all which I compared the moon, December 4, 1761, at St. Helena) preceded the fecond or fubfequent one 2′.7″,11 of time in right afcenfion, and the third or laft 3′.9″,70 and the fecond preceded the third 1.′2″,59 all according to the time of a clock regulated to agree with the diurnal revolution of the ftars.

LVIII. *An Account of an extraordinary Difeafe among the* Indians, *in the Iflands of* Nantucket *and* Marthu's Vineyard, *in* New England. *In a Letter from* Andrew Oliver, *Efq; Secretary of his Majefty's Province of* Maffachufett's Bay, *to* Ifrael Mauduit, *Efq; F. R. S.*

Bofton, 26 Oct. 1764.

SIR,

Read Dec. 20, 1764.

CONSIDERING your connexions, both as a member of the Royal Society, and of the Society for propagating the gofpel among the Indians, I tranfmit you an account of an uncommon ficknefs, which prevailed the laft year at the iflands of Nantucket and Martha's Vineyard, which lie about fix or feven leagues from each other, and the latter about four or five leagues diftant from the Indian plantation at Mafhpee on the Continent, where it did not make its appearance at all. As I had my account from the Englifh minifter, and from the phyfician at Nantucket, and from the fociety's mif-fionary