XLIX. *An Account of Obſervations made on the Mountain* Schehallien *for finding its Attraction. By the Rev.* Nevil Maſkelyne, *B. D. F. R. S. and Aſtronomer Royal.*

Redde, July 6, 1775.

IN the year 1772, I preſented the foregoing propoſal, for meaſuring the attraction of ſome hill in this kingdom by aſtronomical obſervations, to the Royal Society; who, ever inclined to promote uſeful obſervations which may enlarge our views of nature, honoured it with their approbation. A committee was in conſequence appointed, of which number I was one, to conſider of a proper hill whereon to try the experiment, and to prepare every thing neceſſary for carrying the deſign into execution. The Society was already provided with a ten-feet zenith ſector made by Mr. SISSON, furniſhed with an achromatic object glaſs, the principal inſtrument requiſite for this experiment, the ſame which I took with me to St. Helena in the year 1761; which wanted nothing to make it an excellent inſtrument but to have the plumb-line made adjuſtable, ſo as to paſs before and biſect a fine point at the centre of the inſtrument. This was ordered to be done, and a new wooden ſtand provided for it, capable of procuring a motion of the ſector about a vertical axis, by means of which it could be more eaſily brought into the plane of the meridian,

ridian, or turned half round for repeating the obſervations with the plane of the inſtrument placed the contrary way, in order to find the error of the line of collimation. A large parallelopiped tent, $15\frac{1}{2}$ feet ſquare and 17 feet high, was alſo provided for ſheltering both the inſtrument and the obſerver who ſhould uſe it, compoſed of joices of wood well framed together, and covered with painted canvas. The Society was likewiſe poſſeſſed of moſt of the other inſtruments requiſite for this experiment; as an aſtronomical quadrant and tranſit inſtrument made by Mr. BIRD, and an aſtronomical clock by SHELTON, which had all been provided on occaſion of the obſervations of the tranſit of Venus in 1761 or 1769. A theodolite of the beſt ſort was wanting, a neceſſary inſtrument for obtaining the figure and dimenſions of the hill. One of Mr. RAMSDEN's conſtruction of 9 inches diameter, was thought the fitteſt for the purpoſe, on account of the excellence of the plan on which it was made, and the number of its adjuſtments, being capable of meaſuring angles for the moſt part to the exactneſs of a ſingle minute. The other inſtruments prepared for this buſineſs were, two barometers of M. DE LUC's conſtruction, made by Mr. NAIRNE; a common Gunter's chain; a roll of painted tape three poles long, having feet and inches marked upon it; two fir poles of 20 feet each, and four wooden ſtands, for ſupporting them when uſed in meaſuring the baſes, and a braſs ſtandard of five feet for adjuſting them. The poles and ſtands were provided on the ſpot.

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Although accounts had been received from various perſons of ſeveral hills ſuppoſed proper for the intended purpoſe, ſome better and ſome worſe authenticated; yet, in order to be ſure of finding the beſt hill for the experiment, it was determined to ſend a perſon furniſhed with proper inſtruments, to make ſuch obſervations on various hills in England and Scotland, as might enable us to chooſe the fitteſt for the purpoſe. Accordingly Mr. CHARLES MASON, who had been employed on ſeveral aſtronomical occaſions by the Royal Society, was appointed to make a tour through the Highlands of Scotland in the ſummer of the year 1773, taking notice of the principal hills in England which lay in his route either in his going or in his return. It appeared from his obſervations, that ſcarce any hill was ſo well adapted to the purpoſe as our ſanguine hopes had led us to expect; for either they were not high enough, or not ſufficiently detached from other hills, or their greateſt length fell in a wrong direction, too near the meridian, inſtead of lying nearly Eaſt and Weſt, which is a circumſtance requiſite to make a hill of a given height afford the greateſt effect of attraction. In particular, the hills on the confines of Yorkſhire and Lancaſhire, mentioned in the foregoing propoſal, were found not to anſwer the deſcription that had been given of them. Fortunately, however, Perthſhire afforded us a remarkable hill, nearly in the centre of Scotland, of ſufficient height, tolerably detached from other hills, and conſiderably larger from Eaſt to Weſt than from North to South, called by the people of the

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low country Maiden-pap, but by the neighbouring inhabitants, Schehallien; which, I have ſince been informed, ſignifies in the Erſe language, Conſtant Storm: a name well adapted to the appearance which it ſo frequently exhibits to thoſe who live near it, by the clouds and miſts which uſually crown its ſummit. It had, moreover, the advantage, by its ſteepneſs, of having but a ſmall baſe from North to South; which circumſtance, at the ſame time that it increaſes the effect of attraction, brings the two ſtations on the North and South ſides of the hill, at which the ſum of the two contrary attractions is to be found by the experiment, nearer together; ſo that the neceſſary allowance of the number of ſeconds, for the difference of latitude due to the meaſured horizontal diſtance of the two ſtations in the direction of the meridian, would be very ſmall, and conſequently not ſubject to ſenſible error from any probable uncertainty of the length of a degree of latitude in this parallel. For theſe reaſons the mountain Schehallien was choſen, in preference to all others, for the ſcene of the intended operations; and it was concluded to make the experiment in the ſummer of the year 1774.

It was foreſeen, that this experiment would be attended with conſiderable expence, and ſuch as might eaſily have exceeded the common funds of the Royal Society, without ſome extraordinary aſſiſtance. The bounty of his Majeſty, our Patron, happily removed this difficulty. At the humble requeſt of the Society, his Majeſty had been graciouſly pleaſed to grant a very ample

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ſum to their diſpoſal, for defraying the expences of the obſervations of the late tranſit of Venus in 1769, as his Majeſty had before done with reſpect to the former tranſit of Venus in 1761. Out of this benefaction, after all expences had been paid, there was a conſiderable remainder, and, the Society humbly requeſting to know his Majeſty's pleaſure about the diſpoſal of it, he was graciouſly pleaſed to direct them, to *lay it out in ſuch manner as they thought proper, and was moſt agreeable to the end of their inſtitution.* As this bounty of his Majeſty had been originally granted for an aſtronomical purpoſe, the Society thought they could not diſpoſe of it on any more important object, or in any manner more conſiſtent with the intentions of their Royal Patron and Benefactor, than by expending it on this aſtronomical experiment of the attraction of a mountain, as what could hardly fail of throwing light on the principle of univerſal gravitation, and was likely to lead to new diſcoveries concerning the conſtitution of this earth which we inhabit, particularly with reſpect to the denſity of its internal parts.

The experiment being thus reſolved upon, the next next thing to be done was to fix on a proper perſon to carry it into execution. Numerous and intereſting as my literary engagements are at the Royal Obſervatory, I had no thoughts of undertaking this care and labour myſelf, till the Council of the Royal Society were pleaſed to do me the honour to think my aſſiſtance neceſſary to inſure the ſucceſs of ſo important and delicate an experiment. Their thinking ſo, was a ſufficient motive with me to encounter

encounter whatever difficulties and fatigues might attend operations carried on in ſo inconvenient and inclement a ſituation. But it was requiſite I ſhould alſo have his Majeſty's permiſſion for abſenting myſelf ſo long from my duty at the Royal Obſervatory. This his Majeſty was graciouſly pleaſed to grant; and to allow me to ſtay as long as I thought neceſſary, to complete *my very important obſervations*.

Such were the motives for undertaking this experiment, and the preparations made for putting it in execution. I am now to give an account of the operations themſelves.

The quantity of attraction of the hill, the grand point to be determined, is meaſured by the deviation of the plumb-line from the perpendicular, occaſioned by the attraction of the hill, or by the angle contained between the actual perpendicular and that which would have obtained if the hill had been away. The meridian zenith diſtances of fixed ſtars, near the zenith, taken with a zenith ſector, being of all obſervations hitherto deviſed capable of the greateſt accuracy, ought by all means to be made uſe of on this occaſion: and it is evident, that the zenith inſtrument ſhould be placed directly to the North or South of the centre of the hill, or nearly ſo. In obſervations taken in this manner, the zenith diſtances of the ſtars, or the apparent latitude of the ſtation, will be found as they are affected by the attraction of the hill. If then we could by any means know what the zenith diſtances of the ſame ſtars, or what the latitude of the place would have been, if the hill had

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been away, we ſhould be able to decide upon the effect of attraction. This will be found, by repeating the obſervations of the ſtars at the Eaſt or Weſt end of the hill, where the attraction of the hill, acting in the direction of the prime vertical, hath no effect on the plumb-line in the direction of the meridian, nor conſequently on the apparent zenith diſtances of the ſtars; the differences of the zenith diſtances of the ſtars taken on the North or South ſide of the hill, and thoſe obſerved at the Eaſt or Weſt end of it, after allowing for the difference of latitude anſwering to the diſtance of the parallels of latitude paſſing through the two ſtations, will ſhew the quantity of the attraction at the North or South ſtation. But the experiment may be made to more advantage on a hill like Schehallion, which is ſteep both on the North and South ſides, by making the two obſervations of the ſtars on both ſides; for the plumb-line being attracted contrary ways at the two ſtations, the apparent zenith diſtances of ſtars will be affected contrary ways; thoſe which were increaſed at the one ſtation being diminiſhed at the other, and conſequently their difference will be affected by the ſum of the two contrary attractions of the hill. On the South ſide of the hill, the plumb-line being carried Northward at its lower extremity, will occaſion the apparent zenith, which is in the direction of the plumb-line continued backwards, to be carried Southward, and conſequently to approach the equator; and therefore, the latitude of the place will appear too ſmall by the quantity of the attraction; the diſtance of the equator from the

the zenith being equal to the latitude of the place. The contrary happens on the North ſide of the hill; the lower extremity of the plumb-line being there carried Southward will occaſion the apparent zenith to be carried Northward or from the equator; and the latitude of the place will appear too great by the quantity of the attraction. Thus the leſſer latitude appearing too ſmall by the attraction on the South ſide, and the greater latitude appearing too great by the attraction on the North ſide, the difference of the latitudes will appear too great by the ſum of the two contrary attractions; if therefore there is an attraction of the hill, the difference of latitude by the celeſtial obſervations ought to come out greater than what anſwers to the diſtance of the two ſtations meaſured trigonometrically according to the length of a degree of latitude in that parallel, and the obſerved difference of latitude ſubtracted from the difference of latitude inferred from the terreſtrial operations, will give the ſum of the two contrary attractions of the hill. To aſcertain the diſtance between the parallels of latitude paſſing through the two ſtations on contrary ſides of the hill, a baſe muſt be meaſured in ſome level ſpot near the hill, and connected with the two ſtations by a chain of triangles, the direction of whoſe ſides, with reſpect to the meridian, ſhould be ſettled by aſtronomical obſervations.

If it be required, as it ought to be, not only to know the attraction of the hill, but alſo from thence the proportion of the denſity of the matter of the hill to the mean denſity of the earth; then a ſurvey muſt be made

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of the hill to aſcertain its dimenſions and figure, from whence a calculation may be made, how much the hill ought to attract, if its denſity was equal to the mean denſity of the earth; it is evident, that the proportion of the actual attraction of the hill to that computed in this manner will be the proportion of the denſity of the hill to the mean denſity of the earth.

Thus there were three principal operations requiſite to be formed. 1. To find by celeſtial obſervations the apparent difference of latitude between the two ſtations choſen on the North and South ſides of the hill. 2. To find the diſtance between the parallels of latitude. 3. To determine the figure and dimenſions of the hill.

I arrived at the hill of Schehallien on the laſt day of June, and found the obſervatory and inſtruments there, which had been brought down ſome time before from London to Perth on board a ſhip, and thence conveyed over land to the hill under the care of Mr. REUBEN BURROW, my late aſſiſtant at the Royal Obſervatory. The obſervatory was fixed half-way up the South ſide of the hill, as the place where the effect of the hill's attraction would be at the greateſt, and it was placed in the like manner when it was afterwards removed to the North ſide. A circular wall was raiſed, five feet in diameter, and covered at top with a moveable conical roof for ſheltering the aſtronomical quadrant; and a ſquare tent was put up for receiving the tranſit inſtrument, all near to the obſervatory. A *bothie*, or temporary hut, was alſo made near it, for my reſidence, while I was attending

the aſtronomical obſervations on this ſide of the hill. I firſt put the ſector, nearly in the meridian, by means of the variation compaſs; but, thro' the badneſs of the weather, which was almoſt continually cloudy or miſty, I could not before the middle of July get a ſufficient number of obſervations with the aſtronomical quadrant, to know the ſtate of the clock, in order to draw a meridian line on the floor of the obſervatory, for ſetting the ſector truly in the plane of the meridian. The firſt obſervations which I made with the ſector, after it was put truly in the meridian, were on the 20th of July. Between this time and the end of the month, I obſerved the zenith diſtances of 34 ſtars, ſome to the North and ſome to the South of the zenith; and many of them ſeveral times over, having taken 76 obſervations in all, with the plane of the ſector turned to the Eaſt. On the firſt of Auguſt, I turned the plane of the inſtrument about, to face the Weſt, and ſet it in the meridian again, by means of the meridian line drawn on the floor the 26th of July, and ſecured by picquets driven into the ground; and between that and the 15th of the ſame month, I obſerved 39 ſtars including moſt of thoſe taken in the former poſition of the inſtrument, and took 93 obſervations in all.

And here let me take notice of a method which I fell upon of verifying the poſition of the ſector, with reſpect to the plane of the meridian, which, had I thought of it at firſt, would have ſaved me much trouble; and therefore I will now mention it, as it may be uſeful to future obſervers. It conſiſts in obſerving the tranſits of two

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ſtars, differing conſiderably in declination from one another acroſs the vertical wire of the ſector, and comparing the obſerved difference of their tranſits with the known difference of their right aſcenſions. If they agree, it may be ſafely concluded, that the inſtrument is truly placed in the meridian. If not, by comparing the alteration that would be produced in the difference of the tranſits, by ſuppoſing the inſtrument out of the meridian, by any ſmall quantity, as one degree or ten minutes, with the obſerved error, the deviation of the inſtrument from the meridian may be inferred. In this manner I found, that the inſtrument had been put very exactly in the meridian by means of the meridian line; the difference by the two methods coming out only $2\frac{1}{2}$ minutes of azimuth. As to the continuance of the inſtrument in the plane of the meridian, I had a conſtant proof of it by the ſame means, and likewiſe a farther ſecurity, which I did not fail to attend to, by noting the degree and minute which an index depending on the vertical axis of the inſtrument pointed out on a fixed azimuth circle. Being apprehenſive of error in an inſtrument ſupported on a wooden frame, I frequently examined the paralleliſm of the fore arch to the back arch, by meaſuring their perpendicular diſtances at the two ends with a braſs ſcale, whoſe vernier ſhewed the five hundredth part of an inch, and found it liable to variations of a minute or two, owing probably to the force uſed in ſetting the ſector to different zenith diſtances, and the weakneſs of ſome ſcrews at the top of the frame; which ſmall error I corrected, till I found it liable

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to continual returns: and I ſatisfied myſelf, that the plane of the ſector never deviated above three minutes from the meridian in any of the obſervations taken on the South ſide of the hill, which, it is evident, could not in the leaſt affect the obſerved zenith diſtances of ſtars. I hardly ever obſerved without examining the biſection of the point at the centre of the inſtrument, by the plumb-line; which was abſolutely neceſſary, on account of the gradual changes of the wooden frame. My view in mentioning theſe minute circumſtances is, to caution future obſervers, as well as to confirm my own obſervations. But whoever makes uſe of an inſtrument of this kind, ſupported on a wooden frame, will find the greateſt attention neceſſary to attain the ſame degree of accuracy in his obſervations, as if his inſtrument were fixed to an immoveable wall. In the mean time, by obſervations taken with the quadrant and tranſit inſtrument, I got a meridian line, and planted a pole to preſerve it on the top of the hill, to the South of the inſtrument, and another at the foot of the ſame hill; from whence, by meaſuring off an equal diſtance to the Eaſt (as the South-weſt corner of the obſervatory lay to the Eaſt of the tranſit inſtrument) and ſetting up another pole, another meridian line was got, paſſing through the South-weſt corner of the Southern ſtation of the obſervatory. The reaſon for making the meridian line paſs through the South-weſt corner of the obſervatory rather than through the middle of it was, that this part of it had been taken when

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the obſervatory had been uſed as an object in taking angles by the theodolite, in the ſurvey of the hill.

While I was engaged in theſe aſtronomical obſervations, Mr. BURROW, attended by Mr. WILLIAM MENZIES, a land-ſurveyor living in the neighbourhood, who had been recommended by ſome of the principal gentlemen of the country, as a proper perſon for this work, went out every day that the weather permitted, to take ſections of the hill, and angles between ſeveral objects, for determining the figure and dimenſions of the hill. The method made uſe of was this, which was propoſed by Mr. BURROW, and was well adapted to the purpoſe. A number of ſtation poles were ſet up at convenient diſtances all round the foot of Schehallien; but rather without its baſe, and chiefly on little eminences riſing from the foot of it, which formed a polygon of many ſides, ſurrounding the hill; and when delineated on paper, ſhew very nearly the ſhape of its baſe. At each ſtation, the angular poſition of two or more of the other ſtations being obſerved with the theodolite, and one ſide being determined by means of a meaſured baſe, all the other ſides will be known. From theſe ſtations, ſections of the hill up to the top were taken in the following manner. The theodolite, being placed at any ſtation, was pointed towards the hill; and a labourer was ſent with a number of poles, which he was to plant in the ground truly upright, at regular diſtances and in a vertical plane, according to ſignals which he received from the perſon that ſtood at the theodolite, who alſo took the altitude of the foot of

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each pole, and likewife the horizontal angle contained between the plane of the fection poles and the next ftation pole to the right or left. The theodolite was then removed, and planted directly over the centre of this ftation pole, which was removed for this purpofe; and the horizontal angle taken between a pole now planted at the firft ftation and each of the poles of the fection. The horizontal diftance of the two ftation poles being known, the horizontal diftance of each of the fection poles from the firft ftation, and their refpective perpendicular altitudes above it, or depth below it, will be given.

It is manifeft, that thefe operations, when connected by angles with the two ftations of the obfervatory and the meridian line, would at the fame time give the fhape and dimenfions of the hill, and the diftance of the parallels of latitude paffing through the two ftations of the obfervatory, as well as their refpective elevations above the bafe of the hill. But errors being apt to accumulate in a long chain of triangles; to obviate this danger, as well as to produce a check on any great miftakes, that might happen to be made in reading off, or writing down, the angles, I caufed a heap of ftones, or *carn* as it is called by the people of the country, to be raifed in a circular figure fix feet high, at the higheft point of the ridge of the hill, which is to the Weft of it, as a fignal to be obferved from the feveral angles of the polygon, and as a means of connecting the two ftations of the obfervatory by a fmaller number of triangles. Another *carn* towards the Eaftern end of the ridge of the hill was

afterwards fet up for the like purpofe. I propofed to determine the diftance of the two *carns* by connecting them by angles with a bafe, to be meafured in a level fpot in the vale below the hill, and then to make ufe of the faid diftance as a fecondary bafe for determining the fides of the polygon, and the diftance of the two ftations of the obfervatory. Had the two *carns* been vifible from the two ftations of the obfervatory, two triangles would have fufficed for connecting the two ftations together. But, notwithftanding that this was not the cafe, and that only the two *carns* were vifible from one another, yet all the angles of thefe two triangles were meafured by Mr. BURROW in the following method, fuggefted by himfelf. He went with the theodolite to the neighbouring hill on the South fide of Schehallien, which runs parallel to it; and, by varying his fituation, found a point whence the Weftern *carn* and Southern obfervatory appeared by the theodolite to be in one vertical plane, and removing the theodolite he planted a pole there. In like manner he planted another pole on the fame hill, in a vertical plane with the Southern obfervatory and Eaftern *carn*. Then returning to the obfervatory, he took the horizontal angle contained between the two poles, which it is evident is equal to its oppofite angle, or that contained between the *carns*. And going to the Weft *carn*, he took the angle contained between the Eaft *carn* and the pole planted on the oppofite hill, in a line with the Southern obfervatory and Weft *carn*, which is the fame with the angle between the Eaft *carn* and Southern Obfervatory. And laftly, going to the Eaft *carn*, he took the angle con-

tained between the Weſtern *carn* and the pole placed on the oppoſite hill in a line with the Eaſt *carn* and Southern obſervatory, which is the ſame with the angle contained between the Weſtern *carn* and Southern obſervatory. Thus were the three angles found of the triangle made by the Southern obſervatory and two *carns*. In the like manner were the angles of the triangle made by the Northern obſervatory and two *carns* found afterwards. And, as a proof that the angles of the two triangles were rightly determined, their ſum in the firſt caſe differed from 180° by little more than two minutes; and in the ſecond caſe by only half a minute.

Notwithſtanding the advantages which attended this method of finding the diſtance of the two ſtations of the obſervatory, I thought it proper to make uſe alſo of the other method of doing the ſame thing by a ſmall number of triangles carried directly acroſs the hill, thinking it expedient, in a matter of ſuch conſequence, to rely on no ſingle operation; but, as far as poſſible, to confirm every deduction by another found in an independent manner. I had cauſed two poles to be ſet as far up the hill of Schehallien as they could be placed; one as near the Weſtern, and the other the Eaſtern *carn* as they could be, ſo as to be viſible from the Southern ſtation of the obſervatory: alſo two others in like manner viſible from the North obſervatory; one of which was very near the Eaſt *carn*, and the other only 269 feet diſtant from the Weſternmoſt of the two poles viſible from the South obſervatory; ſo narrow was the ridge of the hill in that part, although it grew wider both to the Weſt and

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Eaſt, but much more towards the latter. With theſe four poles, the Eaſt *carn*, and the two ſtations of the obſervatory, five triangles were formed, connecting the two ſtations of the obſervatory, the relative ſituation of which to each other would be determined as ſoon as the length of any one of the ſides of theſe triangles was known, either by comparing it with a baſe meaſured in the valley below, or with the diſtance of the two *carns* ſettled in that manner.

I had got ſufficient obſervations of zenith diſtances of ſtars with the ſector on the South ſide of the hill by the 15th of Auguſt; I prepared therefore for removing the obſervatory and inſtruments to the new ſtation on the North ſide. This was a work of great labour and difficulty, as every thing was carried over the ridge of the hill on men's ſhoulders, and ſome of the packages were very weighty; it employed the labour of twelve men for a week, and was compleated on the 26th. A large level area had been cut away, with great labour, here, in the ſide of the hill, for receiving the obſervatory, as had before been done on the South ſide of the hill. A new *bothie* was alſo erected, and places for holding the quadrant and tranſit inſtrument, as before, adjoining to the obſervatory.

The badneſs of the weather prevented me from beginning my obſervations with the ſector till the 4th of September; but, that being a clear night, I had a fair opportunity of putting in practice the method of bringing the inſtrument into the meridian by the tranſits of the

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ſtars acroſs the plane of the ſector, which was mentioned before. The ſector being put up with its plane facing the Weſt, and ſet near the meridian by the variation compaſs, allowing for the variation, I found, by the tranſit of o Draconis, on the North ſide of the zenith, compared with thoſe of κ, ι and θ Cygni on the South ſide, that the inſtrument deviated $49\frac{1}{2}$ minutes to the Weſt of the South in azimuth; which being corrected, by turning the inſtrument about on its vertical axis, towards the Eaſt, by the help of the diviſions on the azimuth circle; I then found, by the tranſit of η Cephei, on the North ſide of the zenith compared with that of π Cygni on the South ſide, that the inſtrument deviated ſeven minutes to the Eaſt of the South in azimuth, which I corrected accordingly. And ſo near was it brought to the meridian in this manner, that by the moſt exact compariſon of the tranſits of ſeveral ſtars on the 7th and 8th inſtant, it appeared to be only two minutes out of the meridian, and that to the Eaſt of the South; which ſmall error I alſo attempted to correct; but the inſtrument reſted one minute out of the poſition which I intended to give it, owing to the difficulty of turning it about to ſuch great nicety, and ſo I let it remain.

It was indeed a moſt fortunate circumſtance, that I thus got the inſtrument ſo near the meridian by the very firſt night's obſervations, thoſe of September 4th; for the badneſs of the weather in the day prevented me from getting a meridian line by the Sun till the 15th. Had I therefore been obliged to wait for ſetting the inſtrument right by the Sun, I ſhould

I ſhould have loſt four good days obſervations, which were two-thirds of thoſe I took on this ſide of the hill with the plane of the inſtrument turned to the Weſt, and been retarded near three weeks in my obſervations; and, as the opportunities of weather fit for obſerving at all were but very rare, I might have been thereby thrown back into the winter, and defeated of making ſo complete a ſet of obſervations on the North ſide of the hill as I had got on the South ſide, whoſe correſpondence would thereby have been rendered leſs perfect. I had the ſatisfaction, however, when I drew the meridian line on the floor of the obſervatory by the equal altitudes of the Sun taken on the 15th, to find it agree perfectly, even to the ſame minute, with the poſition of the inſtrument, as determined by the tranſits of the ſtars. But no one will doubt of the ſuperior eaſe and readineſs afforded by the latter method, in preference to the other.

On the 20th of September I compleated the obſervations with the plane of the ſector turned to the Weſt, having obſerved 32 ſtars, and taken 68 obſervations in all. On the 22d, I turned it about with the plane to face the Eaſt, and ſet it again in the meridian, by putting it parallel to its former poſition, by means of the meridian line ſecured by marks made on picquets let into the ground perpendicularly below the plane of the inſtrument, before it was turned. Between this time and the 24th of October, I obſerved 37 ſtars, and took 100 obſervations in all, with the plane of the inſtrument facing the eaſt:

and

and thus I compleated my whole ſeries of obſervations with the ſector, having obſerved 43 different ſtars in all, on both ſides of the hill, and taken 337 obſervations.

As a few obſervations, taken with ſo excellent an inſtrument as this zenith ſector, would have been ſufficient to determine the apparent difference of latitude of the two ſtations of the obſervatory, to a ſecond or two; I am apprehenſive I may be thought by many to have multiplied obſervations unneceſſarily. However that may be, I apprehend, that doubling the obſervations in each ſtation of the obſervatory, by taking them with the plane of the inſtrument alternately facing the Eaſt and Weſt, will be allowed to be a proper ſtep, as the line of collimation of the inſtrument is hereby ſeparately determined at each ſtation, and thereby all danger of any alteration happening in the ſame, in its removal from one ſide of the hill to the other, is intirely obviated. I had, indeed, all the reaſon in the world to think, that the ſector was carried from one ſtation to the other without the leaſt accident; but ſtill it was proper to guard againſt what was poſſible to happen.

But I had reaſons alſo for multiplying the obſervations made in the ſame poſition of the inſtrument. It was important to demonſtrate the exactneſs of the inſtrument from the near agreement of a number of obſervations taken with it, as its excellence was not to be intirely preſumed, unleſs this proof could be ſhewn in its favour. Beſides, it might be expected, that ſome unſteadineſs or warp-

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ing of the wooden ſtand on which it was ſupported, might affect the accuracy of the obſervations; or there might be variable and diſcordant refractions, even near the zenith, on the ſide of ſo ſteep a hill, more than are found in lower ſituations. Add to this, that when I began my obſervations on the South ſide of the hill, having a proſpect of bad weather before me, and not knowing how few obſervations I might be able to get on either ſide of the hill, I thought it prudent to endeavour to obſerve moſt of the ſtars in the Britiſh catalogue, which came within the reach of the inſtrument, that I might be ſure of being provided with obſervations of ſome at leaſt of the ſame ſtars, which I might afterwards obſerve when I ſhould be removed to the North ſide of the hill; where, after an interval of perhaps ſome months, many ſtars, that before paſſed the meridian in the night, would paſs it in the day, and conſequently be either inviſible through the teleſcope of the ſector, or more precarious of being ſeen.

Although a meridian line had been found by the tranſit inſtrument at the South obſervatory, whereby the relative ſituation of the two ſtations of the obſervatory, as well as of the other points of the hill, with reſpect to the meridian, might be determined; yet I judged it would be more ſatisfactory to confirm this by another meridian line drawn at the Northern obſervatory. This I found, as I had done the former, by ſetting the tranſit inſtrument to agree with the pole-ſtar at the computed time of its paſſing the meridian, and confirmed it by comparing

the difference of the tranſits of the pole-ſtar and of α Pegaſi, α Andromedæ, and γ Pegaſi, with their difference of right aſcenſion, in the ſame manner by which I had put the ſector in the plane of the meridian, and found it to agree with the former meridian line within two minutes.

It remains to give an account of the manner in which the two baſes were meaſured; one in a level ſpot at the foot of the hill, to the Southward; and the other at the diſtance of about $2\frac{1}{2}$ miles from the hill to the North-weſt, in the plain of Rannock. I cauſed two meaſuring poles to be made of ſtrait-grained well-ſeaſoned fir, in the form of ſquare tubes, 3 inches ſquare and 20 feet long, and ſtrengthened with ſquare pieces withinſide at ſeveral diſtances. Theſe were carefully compared with the braſs ſtandard made by Mr. BIRD, the ſame which was uſed in the meaſure of the degree at Penſylvania, immediately before they were applied to the meaſure of the baſes, and the height of the thermometer noted at the time in order to make allowance for the expanſion or contraction of the braſs ſtandard by heat or cold. Four wooden ſtands were provided for ſupporting the poles; each having a triangular baſe with three iron ſpikes beneath, at each of the angles. An upright pole, ſix feet high, roſe from the middle of one ſide of the triangle, and two ſhort braces were joined to it from the two ends of this ſide, and a long ſlant pole from the oppoſite angle. Two ſliding arms were put upon the upright pole, capable of being raiſed or depreſſed, one above and the other below the

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place where the ſlant pole was faſtened to the upright pole, for ſupporting the meaſuring poles at a convenient height above the ground. In meaſuring the baſe, one end of a pole was ſupported on one of the ſtands, and the other end on another ſtand; and it was ſet horizontal by means of a ſpirit level laid on it about the middle, and by raiſing or depreſſing the arm on which it reſted at one or the other end. The other pole was then, in like manner, ſupported on the two other ſtands truly level, and in the ſame vertical plane with the former pole, namely, that of the intended baſe, without regarding whether they were exactly of the ſame height, and with ſome ſmall horizontal interval between their ends. This interval was meaſured by laying one leg of a braſs rectangle, which was divided into inches and tenths, along one pole, while the other, or vertical leg, touched the end of the other pole: for it was not thought adviſeable, to bring the ends of the poles to touch exactly, as that would have taken up a great deal of time, and might have endangered the altering the poſition of the hindermoſt pole, if it ſhould chance to receive any ſhock by laying down the foremoſt pole. It is evident, that the inches and tenths given by the diviſions of the braſs rectangle are to be added into one ſum together with the poles, in computing the length of the baſe. When the foremoſt pole was truly placed, and the interval between them had been meaſured by the divided ſide of the braſs rectangle, the hindermoſt pole was taken up, and the ſtands on which it had reſted were advanced forwards, and the pole again laid

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on them, truly level, and in the true direction of the base. In order to set the poles continually in the proper direction of the base, the following method was used. The theodolite was first set up at one end of the base, and an upright pole at the other, and another in the middle, and a third was from time to time advanced to a little distance forward; and the measuring poles were sometimes placed in the proper direction by the eye, looking along the lengths of both poles together to the upright pole before them, and sometimes by the help of the theodolite. In this manner, about the middle of September, a base was measured by Mr. BURROW and Mr. MENZIES of 3012 feet, in the valley at the foot of the hill to the South-west; but not so accurately as this method is capable of, owing to the stands being very unsteady, through the looseness of the spikes in the feet and other faults, during the measuring the first quarter of the base, though they were mended before the mensuration of the remainder of it. The mensuration of another base of the length of 5897 feet, in the meadow of Rannock, about 2½ miles to the South-west of the centre of the hill, which I attended myself, was performed with the greatest accuracy, according to the same method, on the 10th, 11th, and 12th of October, with new stands, more substantial and firm than the former.

The extreme badness of the weather no less retarded the operations of the survey than the celestial observations; for there was almost constant rain, mist, or high wind, to obstruct the use of the theodolite: indeed

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all the people of the country agreed, it was the worſt ſeaſon that had ever been known. So that it was not till the 20th of October that the ſections had been carried all round the hill. Nor would this work have been ſo much forwarded as it was, had it not been for the uſe of an additional theodolite of the ſame conſtruction, and by the ſame maker, as the former, which was lent me, upon my requeſt, by the right honourable JAMES STUART MACKENZIE, lord privy ſeal for Scotland; who, having long cultivated a diſtinguiſhed taſte for aſtronomy, was pleaſed to honour the experiment of attraction with every aſſiſtance, which his intereſt or recommendation could procure. I am particularly to acknowledge the favour he conferred upon me by introducing me to the acquaintance of Sir ROBERT MENZIES, baronet, his brother-in-law, a gentleman converſant in mathematical and philoſophical learning, who honoured me with his friendſhip during my reſidence in the country; and, beſides many perſonal civilities ſhewn to myſelf, rendered many material aſſiſtances to the main purpoſe of carrying on the experiment. It is with pleaſure alſo, that I acknowledge the civilities of all the neighbouring gentlemen, who often paid me viſits on the hill, and gave me the fulleſt conviction that their country is with juſtice celebrated for its hoſpitality and attention to ſtrangers. I was honoured alſo by viſits from many learned gentlemen who came from a great diſtance; particularly the lord privy ſeal, Dr. WILSON, profeſſor of aſtronomy at Glaſgow, and his

ſon, and Dr. REID, profeſſor of moral philoſophy, and Mr. ANDERSON, profeſſor of natural philoſophy, alſo at GLASGOW, lord POLWARTH, Mr. RAMSAY, profeſſor of natural hiſtory at Edinburgh, Mr. Commiſſioner MENZIES of the Cuſtoms at Edinburgh, Mr. COPLAND and Mr. PLAYFORE, of the univerſity of Aberdeen, the rev. Mr. BRICE, and my eſteemed friend Col. ROY, who had been my companion in the journey as far as Edinburgh. So great a noiſe had the attempt of this uncommon experiment made in the country, and ſo many friends did it meet with intereſted in the ſucceſs of it!

The uſe of the two theodolites at once, as mentioned above, much forwarded the compleating of the ſections all the month of October; Mr. MENZIES obſerving the bearings at one ſtation with one theodolite, while Mr. BURROW obſerved the altitudes or depreſſions with the other theodolite at the other ſtation; and the labourer, who uſed to plant the poles in the hill, taking only one pole with him, and fixing it up at one place to be obſerved at both theodolites, and then removing it to the next ſtation for the like purpoſe. Notwithſtanding which, the weather grew at length ſo bad, by the early coming in of froſt and ſnow in the beginning of November, when the ſurvey was near compleated, as to render it impoſſible to do any thing more that ſeaſon. It became therefore neceſſary to finiſh this aſtronomical campaign, leaving the theodolite in the care of Mr. MENZIES, to complete what little remained to be done the next ſeaſon.

I have thus deſcribed the plan which was adopted for the operations on Schehallien, and the manner in which it was carried into execution; it only remains to give the reſult of computations made upon thoſe operations for deducing the effect of the attraction of the mountain. The operations themſelves at large ſhall be communicated another opportunity.

I had cauſed the arch of the ſector to be divided by fine points, according to a new and arbitrary diviſion adapted to the method of continual biſection. One-eighth part of the radius of the inſtrument was found by three biſections, and applied as a chord to the arch from the middle on each ſide, intercepting each way 7° 9′ 59″,917. Theſe ſpaces were each divided by points into 128 parts, by continual biſection; therefore one diviſion will contain 3′ 21″,561854. Hence the number of degrees and minutes anſwering to any number of diviſions may eaſily be found. Twenty-four additional parts were alſo ſet off taken from the former, and added at the 128th diviſion on each ſide, to fill up the whole extent of the arch, which thus conſiſted of 152 diviſions on each ſide of the centre, anſwering to an angle of 8° 30′ 37″,4, which was therefore the greateſt angle the inſtrument was capable of meaſuring. To find the value of the parts of the micrometer in ſeconds, I meaſured the diſtance of the points on the limb, by five at a time, by means of the plumb-line, in parts of the micrometer from

from o to 128 on each ſide of the middle or point marked o upon the arch. By a mean of all theſe meaſures, one diviſion of the arch or 3′ 21″,562 came out equal to four revolutions and 34,8272 parts of the micrometer, 41 of which make one revolution; and therefore one part is equal to 1″,0137545, and 41 or one revolution is equal to 41″,5639345. Hence the value of any number of revolutions and parts of the micrometer may be eaſily found. At all obſervations of the ſame ſtar, whether on the North or South ſide of the hill, I brought the ſame point of the arch, namely, that which agreed neareſt with the zenith diſtance of the ſtar, under the plumb-line, ſo that the difference of the apparent zenith diſtances of the ſame ſtar on contrary ſides of the hill is given in parts of the micrometer, and has no reference to the diviſions of the inſtrument, whether they be equal or unequal; and, the parts of the micrometer ſcrew being perfectly equal, as I had formerly ſatisfied myſelf by meaſuring the interval of two given points on the arch with different parts of the ſcrew, that difference of apparent zenith diſtances may be perfectly relied on, as far as depends upon the inſtrument, provided the biſection of the ſtar by the wire in the teleſcope, and that of the point on the arch by the plumb-line, were accurately performed. As the plane of the inſtrument was placed both Eaſt and Weſt, at both ſtations of the obſervatory, the difference of the latitude of the two ſtations may be found

as well from the obſervations made in one poſition of the inſtrument as the other. If the inſtrument had ſuffered no change by being carried over the hill, that is to ſay, if the line of collimation was not altered thereby, the reſults ſhould come out equally true from the obſervations taken in both poſitions of the inſtrument. On the contrary, if the line of collimation ſhould, by any means, have ſuffered any alteration between the obſervations made at the two ſtations, this would cauſe the difference of latitude to appear too ſmall, by the obſervations made in one poſition of the inſtrument, by the quantity of the alteration, and as much too great, in the other poſition of the inſtrument. But ſtill the mean between the two reſults, deduced from the obſervations taken in the two different poſitions of the inſtrument, would give the true difference of latitude; and that equally, whether the line of collimation had ſuffered any change or not. Therefore this will be the beſt method of comparing the obſervations together, and I ſhall take a mean of all the reſults, deduced from the obſervations taken in each poſition of the inſtrument ſeparately, and then a mean of thoſe means for the true difference of latitude. By ſingle obſervations of ten ſtars; *viz.* β, α, and η Caſſiepeæ, and ι, η, β, 39, 45, 46, and 53 Draconis, made on both ſides of the hill, with the plane of the ſector facing the Weſt, after making the proper allowance for preceſſion, aberration, and deviation, and ſemi-annual ſolar nutation of the earth's axis (ſee my tables annexed to my Obſervations made at the Royal Obſervatory), the apparent difference

ference of latitude between the two ſtations of the obſervatory, comes out 54″,1, 54″,7, 54″,0, 55″,4, 55″,0, 55″,0, 52″,2, 54″,0, 54″,3, 53″,1, reſpectively; the mean of all which is 54″,2; the greateſt difference of any one reſult from the mean being only 2″. In like manner, by ſingle obſervations of as many ſtars; *viz.* β and α Caſſiepeæ; ε Urſæ majoris; β, 39, 46, o, 49, and 53, Draconis; and 23 Cygni; made on both ſides of the hill, with the plane of the ſector facing the Eaſt; after making all the allowances as before, the apparent difference of latitude comes out 54″,5, 52″,3, 56″,8, 53″,5, 54″,5, 57″,2, 56″,1, 55″,3, 54″,1, 55″,1, reſpectively; the mean of all which is 55″; the greateſt difference of any one reſult from the mean being 2″. The two means 54″,2 and 55″,0 differ only 0″,8, which ſhould argue only an alteration of 0″,4 in the line of collimation; but this is too ſmall a quantity to be depended on; and therefore it is moſt probable, that the ſtate of the inſtrument remained unvaried. However, whether it did or no, the mean of the two means, or 54″,6, is to be eſteemed the apparent difference of latitude between the two ſtations of the obſervatory, and, when compared with the difference of latitude which ſhould reſult from the trigonometrical meaſures, will give the ſum of the two contrary attractions of the hill.

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It muſt be owned, that this point will be ſettled with rather more certainty when all the obſervations made with the ſector, which exceed 300, ſhall have been computed; but, as from the agreement of theſe reſults together, as well as from the ſmall differences that are uſually found in obſervations made within a few days of one another, one may preſume, that the reſult from the whole will not differ materially from that deduced above from 40 obſervations, I thought I had better take this opportunity of gratifying the impatience of the Society in preſenting them with theſe my firſt computations before their ſummer receſs, than delay giving them any account at all of this experiment, till I had leiſure to complete the whole of my calculations.

I am now to ſhew, what the diſtance is between the parallels of latitude paſſing through the two ſtations of the obſervatory in feet, according to the trinogometrical menſuration; and thence, what the difference of latitude ought to have been, if the hill had been away, or had exerted no ſenſible attraction. This depends on the enumeration of ſeveral particulars.

The length of the baſe meaſured in the meadow of Rannoch was 5897,119 feet, according to the ſtate of the braſs ſtandard when the thermometer was at 40°; but, to reduce it to anſwer to the ſtate of the braſs ſtandard in the heat of 62°, we muſt ſubtract 16,721 feet; we ſhould alſo ſubtract farther 0,327, for the diminution which the braſs ſtandard has ſuffered by wear, and there remains 5880,071 feet for the true length of the

the baſe in Rannoch. See Phil. Tranſ. vol. LVIII. p. 313. 324. and 326. Hence, with the help of the angles taken with the theodolite at the ends of the baſe in Rannoch, and at the Weſt *carn*, the horizontal diſtance between the Eaſt and Weſt *carns* comes out 4047,4 feet. Nearly the ſame reſult comes out from the baſe meaſured on the South ſide of the hill, though with leſs exactneſs; this, when all corrections are made, is 3011,684 feet, whence the diſtance of the two *carns* ſhould come out 4058 feet, or about ten feet longer than reſults from the baſe in Rannoch. But I prefer the deduction from the baſe in Rannoch as moſt to be depended on. Hence, by the calculation of the two triangles formed by the two *carns* and the two ſtations of the obſervatory, the diſtance between the parallels of latitude paſſing through the two ſtations comes out 4364,4 feet, which, according to M. BOUGUER's table of the length of a degree in this latitude of 56° 40′, at the rate of 101,64 Engliſh feet to one ſecond, anſwers to an arc of the meridian of 42″,94. The other ſeries of triangles carried acroſs the hill, gives the ſame diſtance of the parallels only 10 feet leſs, and conſequently the arc of the meridian only $\frac{1}{10}$th of a ſecond leſs. Thus the difference of latitude found by the aſtronomical obſervations, comes out greater than the difference of latitude anſwering to the diſtance of the parallels, the former being 54″,6, the latter only 42″,94. The difference 11″,6 is to be attributed to the ſum of the two contrary attractions of the hill.

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The attraction of the hill, computed in a rough manner, on ſuppoſition of its denſity being equal to the mean denſity of the earth, and the force of attraction being inverſely as the ſquares of the diſtances, comes out about double this. Whence it ſhould follow, that the denſity of the hill is about half the mean denſity of the earth. But this point cannot be properly ſettled till the figure and dimenſions of the hill have been calculated from the ſurvey, and thence the attraction of the hill, found from the calculation of ſeveral ſeparate parts of it, into which it is to be divided, which will be a work of much time and labour; the reſult of which, will be communicated at ſome future opportunity.

Having thus come to a happy end of this experiment, we may now conſider ſeveral conſequences flowing from it, tending to illuſtrate ſome important queſtions in natural philoſophy.

1. It appears from this experiment, that the mountain Schehallien exerts a ſenſible attraction; therefore, from the rules of philoſophiſing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the ſame property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverſe *ratio* of the ſquares of the diſtances, as laid down by Sir ISAAC NEWTON, is alſo confirmed by this experiment. For, if the force of attraction of the hill had been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increaſed by the near ap-

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proach to its centre, the attraction thereof muſt have been wholly inſenſible. But now, by only ſuppoſing the mean denſity of the earth to be double to that of the hill, which ſeems very probable from other conſiderations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverſe duplicate *ratio* of the diſtances, as deduced by Sir ISAAC NEWTON from the compariſon of the motion of the heavenly bodies with the force of gravity at the ſurface of the earth; and the analogy of nature will be preſerved.

3. We may now, therefore, be allowed to admit this law; and to acknowledge, that the mean denſity of the earth is at leaſt double of that at the ſurface, and conſequently, that the denſity of the internal parts of the earth is much greater than near the ſurface. Hence alſo, the whole quantity of matter in the earth will be at leaſt as great again as if it had been all compoſed of matter of the ſame denſity with that at the ſurface; or will be about four or five times as great as if it were all compoſed of water. The idea thus afforded us, from this experiment, of the great denſity of the internal parts of the earth, is totally contrary to the hypotheſis of ſome naturaliſts, who ſuppoſe the earth to be only a great hollow ſhell of matter; ſupporting itſelf from the property of an arch, with an immenſe vacuity in the midſt of it. But, were that the caſe, the attraction of mountains, and even ſmaller inequalities in the earth's ſurface, would be very great, contrary to experiment, and would affect the

meaſures

meaſures of the degrees of the meridian much more than we find they do; and the variation of gravity in different latitudes in going from the equator to the poles, as found by pendulums, would not be near ſo regular as it has been found by experiment to be.

4. The denſity of the ſuperficial parts of the earth, being, however, ſufficient to produce ſenſible deflections in the plumb-lines of aſtronomical inſtruments, will thereby cauſe apparent inequalities in the menſurations of degrees in the meridian; and therefore it becomes a matter of great importance to chuſe thoſe places for meaſuring degrees, where the irregular attractions of the elevated parts may be ſmall, or in ſome meaſure compenſate one another; or elſe it will be neceſſary to make allowance for their effects, which cannot but be a work of great difficulty, and perhaps liable to great uncertainty.

After all, it is to be wiſhed, that other experiments of the like kind with this were made in various places, attended with different circumſtances. We ſeldom acquire full ſatisfaction from a ſingle experiment on any ſubject. Some may doubt, whether the denſity of the matter near the ſurface of the earth may not be ſubject to conſiderable variation; though perhaps, taking large maſſes together, the denſity may be more uniform than is commonly imagined, except in hills that have been volcanos. The mountain Schehallien, however, bears not any appearance of having ever been in that ſtate; it being extremely ſolid and denſe, and ſeemingly compoſed of an intire

rock.

rock. New obſervations on the attraction of other hills, would tend to procure us ſatisfaction in theſe points. But whatever experiments of this kind be made hereafter, let it be always gratefully remembered, that the world is indebted for the firſt ſatisfactory one to the learned zeal of the Royal Society, ſupported by the munificence of GEORGE THE THIRD.

Obſervations of Stars made with the zenith ſector on the mountain Schehallien, for diſcovering its attraction.

		Reſult of Obſervations on the South-ſide of Schehallien.										
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Eaſt.					Limb of the Sector turned to the Weſt.					
α Caſſiopeæ	24S	July 28. S 1.35,4::					Aug. 4. S 2.7,1	7. 5,5				
η	1S						Aug. 7. N 10,4	10. 12,1	15. 12,7			
β Urſæ Major.	16N						Aug. 8. N 2.23,1					
γ	31S	July 29. N 2.16,9					Aug. 8. N 2.5,7 :					
ε	9N	July 28 N 2.23,6										
ζ	10S	July 28. N 1.11,5										
η	111S	July 22. N 19,8	28. 22,6									
ι Draconis	55N	July 26. N 2.26,0	28. 26,0				Aug. 4. N 2.14,0					
θ	45N	July 26. N 4,2					Aug. 4. S 7,5					
η	96N	July 25. N 6,2	26. 5,4				Aug. 2. S 6,5	4. 5,4				
β	75S	July 21. N 1.37,4	24. 37,0	25. 36,1			Aug. 1. N 1.27,4	2. 24,7 :	4. 24,4	5. 26,9	6. 26,4	15. 28,7
ξ	4N	July 20. N 3.4,5	21. 4,3	25. 5,4	26. 6,4		Aug. 1. S 9,2	15. 7,9				
γ	92S	July 20. N 1.32,0:	25. 32,3				Aug. 5. N 1.20,7					
39	36N	July 24. N 19,1	26. 19,5	29. 19,8			Aug. 1. N 7,9	4. 11,3	15. 11,1			

		Result of Obfervations on the South-fide of Schehallien.									
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Eaft.					Limb of the Sector turned to the Weft.				
45 Draconis.	4N	July 21. N 1,0	29. 2,8				Aug. 1. S 9,4	4. 11,2	15. 5,6		
46	24S	July 24. N 23,9	26. 24,7				Aug. 4. N 13,6	10. 16,4 :	15. 16,6		
ο	44N	July 21. S 4,2	25. 2,7	26. 2,2	28. 2,0	29. 1,0	Aug. 1. S 12,7	4. 12,4			
48	15N						Aug. 11 N 2.5,1	15. 6,0			
49	24S	July 29. N 2.28,0					Aug. 1. N 2.16,4	4. 17,6			
53	3S	July 26. S 22,2	28. 21,5				Aug. 1. S 32,0	4. 31,5	15. 28,4		
54	12N	July 29. S 18,4					Aug. 15. S 25,5				
κ Cygni	66S	July 21. S 0,1	26. N 0,3								
ι	97S	July 21. N 2.36,0	28. 35,0	29. 35,8	31. 36,5		Aug. 1. N 2.23,5	4. 22,8			
θ	124S	July 26. S 19,6	31. 17,5				Aug. 1. S 31,4				
23	5N	July 24. N 8,5	29. 8,7	31. 11,0			Aug. 1. S 2,7	4. 2,4	7. 1,5	15. N 1,1	
33	14S	July 24. N 1.0,2	26. 0,5	29. 1,3	31. 3,1		Aug. 1. N 30,4	4. 30,7	7. 31,6	15. 34,9	
η Cephei	77N	July 23. S 5,5	26. 2,4	29. 1,2	31. 1,6		Aug. 1. S 12,5	4. 11,2	7. 10,5	10. 9,8	15. 7,5
α	89N	July 23. S 25,0	26. 22,0	28. 20,8	29. 21,0	31. 20,0	Aug. 1. S 33,9	4. 32,8	7. 32,8	10. 31,7	15. 28,5
π Cygni	116S	July 29. N 12,8					Aug. 7. 0,0 :	10. N 1,9			

Names of the Stars.	Point on the limb.	Result of Obſervations on the South-ſide of Schehallien.										
		Limb of the Sector turned to the Eaſt.					Limb of the Sector turned to the Weſt.					
10 Cephei	61N						Aug. 4. N 28,8					
12	44N						Aug. 12. N 21,9					
13 μ	20S	July 26. N 34,0	29. 34,2				Aug. 7. N 24,7 :	10. 27,1	15. 28,3			
14	4N	July 26. N 2.38,5					Aug. 4. N 2.27,7	10 31,5				
15 ν	37N						Aug. 12. S 24,3					
21 ζ	8N	July 29. S 1.4,7	31. 2,9				Aug. 4. S 1 14,6	10. 11,5	15. 11,0			
	13S	July 23. S 27,8	26. 26,7				Aug. 4. S 36,0	10. 33,8				
27 δ	11N	July 26. S 37,0	30. 36,9				Aug. 4. S 1.6,7	7. 6,7	9. 6,0	10. 5,5	15. 3,3	
	149N						Aug. 9. N 1.3,5					
1 Caſſiopeæ	27N	July 29. N 2.29,9	31. 30,3				Aug. 4. N 2.20,7	15. 22,9				
2	26N	July 31. S 13,0					Aug. 4. S 24,9	9. 23,5	15. 21,4			
5 τ	13N						Aug. 10. N 16,2	15. 17,5				
7 ρ	8S						Aug. 10. S 2.14,4	15. 13,6				
β	22N	July 26. N 39,2					Aug. 4. N 29,0	7. 29,5	15. 31,9			

Names of the Stars.	Point on the limb.	Refult of Obfervations on the North-fide of Schehallien. Limb of the Sector turned to the Weft.				Limb of the Sector turned to the Eaft.					
α Caffio-peæ.	24S	Sept. 8. S 3.9,0	15. 7,4			Oct. 2. S 2.27,5:	3. 25,3	5. 26,8	24. 22,3		
η	1S	Sept. 7. S 33,8	8. 33,5	19. 30,3		Oct. 2. S 13,4	3. 11,8	9. 11,0	24. 7,2		
β Urfæ Major.	16N					Oct. 6. N 1.3,0					
γ	31S					Oct. 14. N 29,0	15. 25,5	18. 26,5	22. 26,4		
ε	9N					Oct. 3. N 35,5	4. 35,6	15. 30,0	17. 33,1		
ζ	10S	Sept. 15. S 24,2				Oct. 16. S 19,6	22. 22,0				
η	111S					Oct. 17. S 1.6,3	18. 6,6	22. 6,9			
ι Draconis	55N	Sept. 20, N 37,6				Oct. 17. N 1.6,1	22. 1,4				
θ	45N					Oct. 17. S 1.13,7	23. 16,4				
η	96N	Sept. 7. S 1.18,0	20. 19,4			Oct. 23. S 1.12,3					
β	75S	Sept. 7. N 17,3	15. 17,5			Oct. 7. N 28,8	17. 27,2	23. 26,1			
ξ	4N	Sept. 15. N 1.29,5				Oct. 23. N 1.39,9					
γ	92S	Sept. 18. N 13,6	20. 13,4								
39	36N	Sept. 7. S 36,8	20. 38,9			Oct. 3. S 25.4	17. 26,5				
45	4N	Sept. 7. S 1.14,0				Oct. 17. S 1.1,1	23. 2,0				

		Refult of Obfervations on the North-fide of Schehallien.									
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Weft.				Limb of the Sector turned to the Eaft.					
46 Draconis.	24S	Sept. 7. S 33,0				Oct. 3. S 20,9	17. 18,7	23 19,4			
o	44N	Sept. 18. S 1.16,5				Oct. 3. S 1.4,8	18. 3,3	22. 3,6			
49	24S	Sept. 20 N 1.12,2				Oct. 3. N 1.25,5	22. 26,5:	23. 26,3			
53	3S	Sept. 7. S 1.35,0				Oct. 3. S 1.21,6	17. 20,6				
54	12N					Oct. 17. S 1.17,5	22. 18,0				
κ Cygni	66S					Oct. 18. S 40,0					
ι	97S	Sept. 7. N 1.20,0	18. 20,7	20. 20,5		Oct. 3. N 1.36,0	15. 35,9	18. 36,7			
θ	124S	Sept. 7. S 1.34,6	20 33,7			Oct. 6. S 1.17,0:	22. 17,6:				
23	5N	Sept. 7. S 1.5,9	18. 3,9	20. 3,0		Oct. 3. S 29,3	15. 28,3	18. 28,6			
33	14S	Sept. 7. S 13,1	18. 10,3	20. 10,8		Oct. 3. N 4,0	6. 5,8	7. 5,8			
η Cephei	77N	Sept. 7. S 1.12,5	8. 13,1	18. 9,3	20. 10,6	Oct. 3. S 36,9	6. 35,1	7. 35,9			
α	89N	Sept. 7. S 1.33,6	8. 35,0	20. 31,9		Oct. 3. S 1.13,5	6. 12,0	23. 9,7			
π Cygni	116S	Sept. 7. S 1.1,1	8. 1,4			Oct. 6. S 21,5	9. 21,4				
10 Cephei	61N	Sept. 20. S 10,0									
13 μ	20S	Sept. 7. S 17,5	18. 14,9			Oct. 3. N 1,5	7. 1,7	9. 3,5			

		Result of Obfervations on the North-fide of Schehallien.									
Names of the Stars	Point on the limb.	Limb of the Sector turned to the Weft.				Limb of the Sector turned to the Eaft.					
14 Cephei	4N					Oct. 9. N 2.9,8					
21 ζ	8N	Sept. 7. S 2.16,0	18. 11,7	20. 11,2:		Oct. 3. S 1.35.6	6. 35,3	9. 33,5			
ι	13S	Sept. 7. S 1.38,0	8. 37,3			Oct. 3. S 1.17,5	9. 15,0	23. 13,3			
27 δ	11N	Sept. 8. S 2.8,0				Oct. 3. S 1.30,6	6. 26,5	9. 25,5	22. 22,6	23. 22,6	24. 23,4
ι	149N	Sept 8. N 1,7	15. 3,8								
1 Caffiopeæ.	27N	Sept. 7. N 1.17,4	13. 21,2	15. 20,4		Oct. 3. N 1.39,0	6. 39,9 :	9. 2.0,0			
2	26N	Sept. 7. S 1.27,0	8. 26,0	15. 23,5		Oct. 3. S 1.4,5	6. 5,4	9. 3,9			
5 τ	13N	Sept. 7. S 29,4	8. 28,1	13. 26,4		Oct. 2. S 7,8	3. 7,8	9. 5,1			
7 ρ	8S	Sept. 7. S 3.18,7	8. 18,5	15. 18,3		Oct. 3. N 1.38,0	6. 36,0	9. 40,5	24 2.4,6		
β	22N	Sept. 7. S 14,0	8. 13,6	13. 11,7	15. 11,7	Oct. 3. N 6,9	6. 7,9	9. 8,9			

N. B. The figures fet down under the days of the month are the revolutions and parts of the micrometer, by which the ftar is North or South of the divifion fhewn in the fecond column. The revolution, though not repeated, is fuppofed to continue the fame as in the firft ob-

obſervation of the ſtar. One part of the micrometer is $= 1{,}0137545''$, and one revolution or 41 parts is $= 41{,}56393''$. 128 diviſions of the arc have the chord $= \frac{1}{8}$th of the radius.

PRE-